Milton Friedman and Economic Debate in the United States, 1973–2006
Chapters 1 to 10

Edward Nelson[1]

September 25, 2023

[1] The views expressed in this study are those of the author alone and should not be interpreted as those of the Federal Reserve or the Board of Governors.

Milton Friedman and Economic Debate in the United States, 1973–2006

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Milton Friedman and Economic Debate in the United States, 1973–2006
Introduction

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Federal Reserve Board
August 15, 2023

The book is a sequel to the two-volume study *Milton Friedman and Economic Debate in the United States, 1932–1972* (Nelson, 2020a, 2020b). The objective of this follow-up work is to provide an account of Milton Friedman's role in a succession of major economic debates that took place in the United States from the start of 1973 through his death in November 2006. The debates considered cover both those that were largely carried out in the economic-research literature and those that primarily proceeded in the media or in policy forums.

The latter forums have an even more considerable role to play in this book's account than in they did in the previous volumes' coverage of the period from 1932 to 1972. Friedman did generate a large body of research articles in the 1973–2006 period, in addition to co-producing the 1982 *Monetary Trends* book with Anna Schwartz. Nevertheless, from the early 1970s onward, it was increasingly the case that he primarily participated in economic debates in the public-policy outlets of media or discussions of current policy. His *Free To Choose* television series in 1980 was the most prominent example. As part of that program's episodes, Friedman was involved in something that recurred from the 1970s to the 2000s: televised debates, with economists and other social scientists, on national economic issues. Over the same period, however, he also produced a large volume of popular writings, including the *Free To Choose* companion book written with Rose Friedman, other books, and many op-eds.

Some of the controversies for which Friedman was best known were ones that he deliberately launched via volleys fired out through his public-policy writings. In particular, advocacy of drug legalization, already a part of Friedman's public-policy prescriptions in the early 1970s, was something that he pressed in the 1980s and 1990s in books, op-eds, and television appearances. The issue of drugs is discussed in Chapter 16 of this book.

A different instance of a Friedman-related controversy that arose from his activities outside

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research concerned Chile. The controversy materialized in the mid-1970s following his visit to the country and meetings with members of its military dictatorship. This was a controversy whose scale he did not anticipate. His March 1975 visit to Chile visit had received some expressions of disapproval when it became publicly known that he had agreed to make the trip. Post-trip, however, it became the most controversial part of his career, especially on account of his meeting with Pinochet during the trip and the association of his economic-policy prescriptions with Pinochet's dictatorship. The matters under debate in this controversy encompassed both whether Friedman should have refrained from visiting Chile and arguments and whether policy prescriptions associated with Friedman could be implemented in a democracy. This controversy is covered in this book, in Chapter 7, with the focus being on the element of the debate taking place in the United States.

Of a far smaller order of magnitude as a controversy, but one highly germane to Friedman's own specialization in research, was the debate in policy and research circles in the United States from 1982 onward about whether monetary aggregates had outlived their usefulness in monetary analysis and policy. It will be shown that, to the end of his days, Friedman remained a monetarist. He did not repudiate his previous emphasis on monetary aggregates or his prescription of constant monetary growth. However, over the quarter century from 1982 to 2006 he made important acknowledgments of major forecasting errors that he had made using monetary aggregates, of shifts in M2 velocity, and of how Federal Reserve countercyclical stabilization policy had been of a higher quality since the mid-1980s than he had thought likely.

Approach

As in Nelson (2020a, 2020b), the perspective provided in this book is that of an author who specializes in the same field of research that Friedman did: that is, monetary economics. The chapters that follow therefore represent a monetary economist's attempt to provide an analytical narrative of Friedman's career from 1973 to 2006, with the narrative organized primarily in terms of key economic debates.

The section that follows titled "Conventions Used in This Book" gives a summary of the way in which the book is organized. The format is the same as in the previous book, with one exception. In the previous book, the chronological approach of the book was interrupted for five chapters (5 to 9) in order to lay out Friedman's economic framework, which had crystalized by 1951. In contrast, because Friedman largely continued to use that framework from 1973 to 2006, the present book consists entirely of analysis of (aspects of) blocks of years—although, in the

course of this analysis, revisions that Friedman made during 1973–2006 to his views (such as with regard to antitrust) are discussed, including the reasons for this change.

The period 1973–2006 was one in which the monetarist counter-revolution in economics, which formed a common thread in much of the narrative of volume 2 of *Milton Friedman and Economic Debate in the United States, 1932–1972*, had largely been completed, at least as far as Friedman's research contributions are concerned. The narrative is consequently more disparate—covering Friedman's efforts to advance his views on monetary policy and numerous aspects of his free-market positions, in the public square, as well as the continuing resistance in the 1970s to his views on inflation that was articulated in policy circles and by leading U.S. Keynesians. The coverage also includes much on the advent of floating exchange rates and Friedman's interventions regarding international and domestic economic policy in the wake of the oil shock. The 1970s and 1980s also saw Friedman's efforts, mostly unsuccessful, to have rules on fiscal policy conduct—in particular, limitations on spending—instituted by constitutional amendment. Reflecting the variety of topics covered in the book, Chapter 1 serves as the book's launchpad, using two snapshots taken from years (1977 and 2004) covered in the subsequent narrative. It also attempts to provide an overview and summary of what might otherwise seem highly diffuse subject matter of chapters 2 to 18 by highlighting various themes and debates that are covered in the course of this book.

Acknowledgments

The author is grateful to a number of people for providing comments on drafts of this book. In many cases, the comments pertained to drafts of specific chapters. Accordingly, each of the individual chapters of this book contains an acknowledgments paragraph recognizing feedback received on earlier versions of those chapters.

In the years prior to starting this book, the author benefited from extensive discussions, listed in Nelson (2020a), with a number of individuals concerning Friedman's place in monetary economics. For research assistance on various matters that were part of the coverage of Nelson (2020a, 2020b) as well as, in a number of cases, of the present book, the author is grateful to Miguel Acosta, George Fenton, Christine Garnier, William Gamber, and Andrew Giffin. For help in finding and obtaining archival material and related information, the author is indebted to Riccardo DiCecio, Andrew Ewing, Johanna Francis, Daniel Hammond, Stephen Kirchner, Levis Kochin, Özer Karagedikli, Eric Monnet, Charles Palm, Jeremy Piger, Marcel Priebsch, Hugh Rockoff, Glenn Rudebusch, Christopher Sims, Tara Sinclair, the late David Small, Katrina

Stierholz, Paolo Surico, George Tavlas, Gloria Valentine, Mark Wynne, and the staffs of the libraries of Duke University, the Federal Reserve Board, the Federal Reserve Bank of Dallas, the Federal Reserve Bank of San Francisco, and the Federal Reserve Bank of St. Louis. In addition, the following individuals kindly shared material from their own collections: the late Douglas Adie, James Bullard, Nigel Duck, Claire Friedland, John Greenwood, Christopher Gust, Rudolf Hauser, R.W. (Rik) Hafer, Robert Hall, James Heckman, Douglas Irwin, Michael Keran, David Laidler, Leo Melamed, Ann-Marie Meulendyke, Michael Mork, Charles Nelson, Gerald O'Driscoll, Pascal Salin, Roger Sandilands, Larry Schembri, the late Anna Schwartz, Christopher Sims, Stephen Stigler, and Lester Telser. Other individuals are thanked in the initial footnotes to specific chapters of this book. The author extends sincere apologies to any individuals who also provided help for this project but who have inadvertently not been mentioned in the preceding acknowledgments.

Notwithstanding the acknowledgments given above, the views and conclusions expressed in this study are the author's alone, and the author is solely responsible for errors in this study. In addition, the views expressed in this book should not be interpreted as those of the Federal Reserve or the Board of Governors.

Interviews

Many people kindly made themselves available for interviews with the author for this project. The interviews are listed below. In many cases, the coverage of the interview principally involved topics that were, in the event, covered in the previous volume or that are planned to be in the subject matter of the companion project that concerns Friedman and economic debate in the United Kingdom. However, even those interviews not explicitly cited in later chapters proved extremely helpful in shaping the present book.

Andrew Abel	October 14, 2014
Joshua Aizenman	June 30, 2016
Roger Alford	January 23, 2014
Robert Aliber	May 1, 2013 and May 3, 2013
William A. Allen	December 13, 2013
William R. Allen	March 14, 2014
Christopher Allsopp	December 9, 2013 and April 10, 2015
Richard Anderson	November 14, 2013
Kenneth Arrow	December 7, 2013

Alan J. Auerbach	May 18, 2015
Kathy Axilrod	April 25, 2013 and June 26, 2014
Stephen Axilrod	April 24, 2013
David Backus	April 16, 2014
Robert Barro	June 4, 2013
Roy Batchelor	November 8, 2013
Francis Bator	January 6, 2015 and March 16, 2015
William Baumol	January 23, 2014
Gary Becker	December 13, 2013
Michael Beenstock	September 26, 2013
Geoffrey Bell	November 5, 2013
Esra Bennathan	April 1, 2015
Ben Bernanke	February 19, 2014
Alan Blinder	December 6, 2013
Christopher Bliss	January 2, 2015
Ronald Bodkin	November 17, 2015
Michael Bordo	July 24, 2013
Michael Boskin	July 3, 2013
William Brainard	March 5, 2014
Donald Brash	July 9, 2013
Arturo Brillembourg	September 30, 2015
Samuel Brittan	April 18, 2013
Charles H. Brunie	July 15, 2013
Sir Alan Budd	October 7 and 8, 2013
Edwin Burmeister	November 20, 2014
Lord (Terence) Burns	September 18, 2013
Joseph Burns	September 12, 2013
Guillermo A. Calvo	April 1, 2014
Thomas Campbell	August 19, 2015
Michael Canes	November 7, 2013
Victor Canto	September 11, 2015
Thomas Cargill	April 17, 2015
Jack Carr	July 29, 2013
Victoria Chick	January 13, 2015
A. Lawrence (Lawry) Chickering	March 24 and 27, 2015
Robert Chitester	July 9, 2013

Gregory Chow	July 1, 2013
Carl Christ	May 1, 2013 and August 15, 2015
Lars Christensen	August 9, 2013
Richard Clarida	April 3, 2021 and February 10, 2023
Kenneth Clements	September 26, 2013
Warren Coats	October 21, 2013
Tim Congdon	November 25, 2013
Michael Connolly	May 13, 2015
J. Phillip Cooper	September 17, 2015
Francis Cripps	January 22, 2015
Dewey Daane	May 8, 2015
Michael Darby	October 15, 2013
Sir Partha Dasgupta	February 27, 2014
James Davidson	February 12, 2015
Paul Davidson	May 3, 2013
Sir Roderick Deane	August 14, 2013
Lord (Meghnad) Desai	January 9, 2015
William Dewald	April 25, 2013
Peter Diamond	November 19, 2014
Avinash Dixit	June 10, 2015
Lord (Bernard) Donoughue	February 3, 2015
William Dougan	September 19, 2013
Gerald Dwyer	August 20, 2013
Lord (John) Eatwell	January 4, 2015
Benjamin Eden	March 14, 2014
Barry Eichengreen	April 3, 2014
Kenneth Elzinga	March 10, 2015
Paul Evans	February 26, 2013
Eugene Fama	September 11, 2013
Martin Feldstein	November 21, 2013
Christopher Fildes	December 9, 2015
Stanley Fischer	August 30, 2013
Franklin Fisher	January 16 and 21, 2015
Duncan Foley	October 2, 2014
Claire Friedland	October 27, 2014
Benjamin Friedman	May 10, 2013 and July 23, 2013

Roman Frydman	March 2, 2015
Mark Gertler	September 26, 2014
William Gibson	March 6, 2013
Christopher Gilbert	November 21, 2014
Claudia Goldin	September 20, 2013
Charles Goodhart	July 3, 2013
Robert Gordon	March 21, 2013
John P. (Jack) Gould	March 20, 2015
Lyle Gramley	April 2, 2013; April 10, 2013; June 24, 2013
Jo Anna Gray	August 8, 2013
Alan Greenspan	August 19, 2013
Arthur Grimes	September 17, 2013
Lord (Brian) Griffiths	September 23 and October 7, 2013
Herbert Grubel	May 19, 2015
Graham Hacche	November 14 and December 9, 2014
R.W. (Rik) Hafer	August 29, 2013
Robert Hall	May 31, 2013
Lars Peter Hansen	March 11, 2014
Arnold Harberger	April 12, 2013; May 2, 2013; December 9, 2013; September 12, 2014
Geoffrey Harcourt	July 7, 2014
Laurence Harris	October 30, 2015
Oliver Hart	December 29, 2014
Andrew Harvey	December 18, 2014
Rudolf Hauser	June 22, 2012 and July 15, 2013
Robert Heller	September 9, 2013
Dieter Helm	January 14, 2015
Robert Hodrick	January 23, 2016
Lord (David) Howell	September 4, 2015
Thomas M. Humphrey	July 10, 2013
Otmar Issing	October 11, 2013
Richard Jackman	November 4, 2014
Sir Antony Jay	May 29, 2013
Peter Jay	May 8, 2013
Glenn P. Jenkins	June 17, 2019
Peter Jonson	November 14, 2013

Lars Jonung	September 8, 2014
Jerry Jordan	June 5, 2013
Thomas (Tom) Jordan	June 24, 2013
Dale Jorgenson	September 12, 2014
Peter Jovanovich	March 24, 2015
Edward Kane	June 24, 2015
George Kaufman	November 12, 2013
Henry Kaufman	October 14, 2014 and January 22, 2015
William Keegan	January 9, 2014
Michael Keran	March 7, 2013
Mohsin Khan	December 18, 2015
Lord (Mervyn) King	August 15 and 16, 2016
Benjamin Klein	March 4, 2013
Levis Kochin	April 23, 2013
Laurence Kotlikoff	May 26, 2015
Arthur Laffer	June 10, 2013 and August 11, 2014
David Laidler	June 3 and 19, 2013; November 6, 2014
Lord (Richard) Layard	February 7, 2014
Eugene Lerner	July 29, 2016
Fred Levin	March 10, 2014
Peter Lilley	October 28, 2014
David Lindsey	May 2, 2013
Richard Lipsey	June 17, 2015
Rachel Lomax	March 25, 2014
James Lothian	October 24, 2013
Robert Lucas	March 12, 2013
George Macesich	May 28, 2013
Gregory Mankiw	September 24, 2013
Henry G. Manne	April 30, 2014
Harry Markowitz	February 23, 2016
Thomas Mayer	October 16, 2013
Deirdre McCloskey	August 21, 2013 and August 17, 2015
Rachel McCulloch	October 4, 2013
Bennett McCallum	June 13, 2013
Ronald McKinnon	January 23, 2014
Sir Christopher (Kit) McMahon	February 14, 2014

Philip Meguire	November 19, 2013
David Meiselman	April 30, 2013 and July 16, 2014
Leo Melamed	June 19, 2013
Allan Meltzer	April 21, 2013 and April 8, 2015
Ann-Marie Meulendyke	April 29, 2013
Marc A. Miles	February 20, 2014
Murray Milgate	January 22, 2015
Marcus Miller	April 16, 2014
Patrick Minford	October 4, 2013 and March 27, 2015
Jeffrey Miron	June 20, 2013
James Mirrlees	January 6, 2015
Frederic Mishkin	June 18, 2013
John H. Moore	April 29, 2014
Michael Mork	June 5, 2013
Richard Muth	May 20, 2015
Robert Neild	November 6, 2013
Charles Nelson	September 9, 2013
Marc Nerlove	September 13, 18, and 26, 2013
David Newbery	October 10, 2014
Maurice Newman	September 18, 2013
Stephen Nickell	December 6, 2013
Robert Nobay	December 3, 2013
Coleman Nutter	April 18, 2014
Jane Nutter	April 23, 2014
Gerald O'Driscoll	April 24, 2015
Lawrence Officer	January 10, 2016
Lee Ohanian	September 26, 2013
Peter Oppenheimer	December 29, 2014
Athanasios Orphanides	June 27, 2014
Adrian Pagan	January 8, 2015
Michael Parkin	May 29, 2013
Sam Peltzman	March 1, 2013 and June 22, 2015
John Pencavel	May 12, 2014
Gordon Pepper	October 21, 2013
Edmund Phelps	May 16, 2013
Charles Plosser	April 2, 2015

William Poole	March 25, 2013 and April 30, 2013
Richard Posner	October 27, 2014
Edward C. Prescott	February 16, 2016
Lionel Price	December 12, 2014
R. David Ranson	April 30, 2014 and September 22, 2014
Robert Rasche	May 7, 2013
Brian Reading	November 28, 2013
Hugh Rockoff	August 29, 2013
Harold Rose	October 11, 2013
Julio Rotemberg	September 5, 2014
Robert Rowthorn	December 17, 2014
John Rutledge	November 14, 2014
Michael Salemi	November 12, 2014
Pascal Salin	November 4, 2015
J.R. (Dick) Sargent	April 15, 2015
Thomas Sargent	January 24, 2014 and March 26, 2014
Thomas Saving	June 9, 2014
Jose Scheinkman	March 13, 2014
Charles Schultze	July 9, 2013
Matthew Shapiro	November 14, 2013
Robert Shiller	September 26, 2014
George Shultz	May 22, 2013
Jeremy Siegel	September 17, 2013
William E. (Bill) Simon, Jr.	September 25, 2015
Christopher Sims	March 15, 2013 and September 20, 2013
Thomas Simpson	May 9, 2013
Allen Sinai	May 7, 2015
Peter Sinclair	November 13, 2014
Lord (Robert) Skidelsky	November 27, 2013
Robert Solow	December 2 and 31, 2013; July 7, 2014; April 3, 2015
Aris Spanos	March 26, 2014
Ben Stein	March 18, 2015
Charles Steindel	December 3, 2015
Arlie Sterling	October 9, 2015
Max Steuer	December 8, 2015

Stephen Stigler	November 6, 2013
Houston Stokes	May 12, 2015
Lawrence Summers	November 22, 2013
Alexander Swoboda	September 10, 2014
Vito Tanzi	April 15, 2014
George Tauchen	November 13, 2014
John Taylor	July 2, 2013
Lester Telser	October 8, 2013; November 18, 2014
Niels Thygesen	February 10, 2015
A.P. (Tony) Thirlwall	October 1, 2013
Jim Thomas	January 7, 2015
Nicolaus Tideman	May 15, 2015
Richard Timberlake	September 10, 2014
George Tolley	November 14, 2014
Juan J. Toribio	November 14, 2013
Robert Townsend	November 14, 2014
Stephen Turnovsky	April 28, 2014
Charles Upton	January 8, 2015
Gloria Valentine	April 1, 2013, May 8, 2013, and December 5, 2013
Paul Volcker	October 16, 2013
Michael Walker	June 21, 2013
Neil Wallace	March 15, 2013
T. Dudley Wallace	July 20, 2015
Kenneth Wallis	January 29, 2015
Glen Weyl	June 17, 2015
William R. (Bill) White	May 5, 2015
Marina v.n. Whitman	May 1, 2019
Donald Winch	September 22, 2014
Paul Wonnacott	May 12, 2014
Geoffrey Wood	November 14, 2013 and September 8, 2014
Clifford Wymer	April 17, 2014
Leland Yeager	August 8, 2013
Richard Zecher	September 3, 2013

In addition to the above, the author interviewed Milton Friedman, Phillip Cagan, and Sir Alan

Walters (all in 1992), Anna Schwartz (in 1992 and 2003), and Alvin Marty (in 2008).

Conventions used in this book

The chapters in this book that cover blocks of years—that is, Chapters 2–18—are divided into sections titled "Events and Activities," "Issues," and "Personalities" (with the latter two sections in turn broken into subsections). The "Events and Activities" section covers some of Friedman's main engagements in economic debate over the years considered in the chapter; this section, however, omits those topics subsequently covered in the "Issues" and "Personalities" sections. The "Issues" section covers major policy or research matters in which Friedman was involved during the years in question. The "Personalities" section is of the same format as the "Issues" section, except that it is more closely focused on an individual with whom Friedman interacted (or to whom Friedman reacted) in the years covered in the chapter. In each case, no attempt is made to provide a complete picture of the work of the individual considered in the "Personalities" section. The aim of the discussion is, instead, to bring out the activities and work of Friedman that reflected his overlap of interests with the individual in question.

The motivation for the "Events and Activities"/"Issues"/"Personalities" division of each chapter is that Friedman's activities covered several different areas in each block of years considered. Consequently, an explicit demarcation of each chapter by topic seemed preferable to a strictly chronological format.

References are described in the past tense ("Romer and Romer (2002a) argued…") for publications that appeared during (or prior to) Friedman's lifetime, and in the present tense ("Romer and Romer (2013a) argue…") for post-2006 articles. An exception to the latter practice is made for cases in which items published after 2006 were by authors who are now deceased (for example, Anna Schwartz and Gary Becker). In those cases, even post-2006 articles by the authors are referred to in the past tense.

Except when quoting others, or when using standard terminology (for example, "the Chicago School"), the term "Chicago," appearing by itself, refers to the city of Chicago. It is not used as shorthand for the University of Chicago.

Articles cited in this book that appeared in newspapers or news or public-affairs periodicals are referenced in the main text or footnotes by their publication title and date (for example, "*New York Times*, January 25, 1970"). Fuller bibliographical details for these articles (including article title and, where given, article author, as well as page number, where known) appear in Section I of the Bibliography, in which the news articles are listed in chronological order. (Section II of

the Bibliography covers books, as well as articles that were published in research journals. This section of the Bibliography gives articles in alphabetical order, arranged by author.)

To limit the extent to which the flow of sentences in the main text is interrupted by bibliographical references, and to contain the number of times that the word "Friedman" appears in any sentence in the main text, citations of Friedman's writings appear in footnotes rather than in the main text. Accordingly, it is in the text of footnotes that one will find citation of the Friedman items to which reference is made in the made text (with such footnotes typically reading "See Friedman (1973a, 1973b)…").

Interviews conducted specifically for this book are indicated in the main text or footnotes by the name of the interview subject and the date of the interview. Interviews quoted or cited in the main text or footnotes that appeared in research journals are cited using the name of the interviewer (not the interviewee).[1] Thus, John Taylor's interview with Milton Friedman, published in 2001 in *Macroeconomic Dynamics*, is cited as Taylor (2001) and not as a Friedman-authored article.

[1] An exception is made in the small number of cases, which include Friedman (1973c), in which there was no credited interviewer and in which Friedman was listed in the publication as the article's author, even though the article was published in question-and-answer format.

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 1: 1977 and 2004

Edward Nelson[4]
Federal Reserve Board
July 28, 2023

I. 1977

As of 1960, the Hoover Institution, formed at Stanford University in 1919, had only three or four full-time appointed researchers.[5] It was in the 1960s that a substantial rise in the number of Hoover Institution researchers would begin. Even after this expansion was well underway, however, the institution was known for being mainly concerned with issues other than economics. It was particularly disposed toward foreign-policy and national-security topics—an orientation accurately conveyed by its full name, the Hoover Institution on War, Revolution, and Peace.

Against this backdrop, in the mid-1960s, members of Stanford University's economics department tended to hold the Hoover Institution in little regard. Their low evaluation was based in part on the institution's lack of credentials in economics but also on the perception that the organization—founded by Herbert Hoover prior to his period as U.S. president—valued Republican-party-oriented public-policy work over fundamental research of the kind associated with universities. "The Hoover Institution was quite a bit to our right," Marc Nerlove—a member of the economics department at the time, and a one-time student of Milton Friedman's—recalled (interview, September 18, 2013). "Literally to our right, if you stood there," Nerlove added—referencing the institution's physical location, *vis a vis* the university's economics department, on Stanford University's campus.

The Hoover Institution did hire more economists from the mid-1960s to the mid-1970s. But one

[4] Email: Edward.Nelson@frb.gov. The views expressed in this paper are those of the author alone and do not necessarily reflect the views of the Board of Governors of the Federal Reserve System or its staff. The author is grateful to George Tavlas for help in obtaining materials used in the research underlying this chapter. The author regrets to note that, in the period since the research underlying this chapter began, eight of the individuals whose interviews with the author are quoted below—Kenneth Arrow, Gary Becker, Martin Feldstein, David Meiselman, Allan Meltzer, Robert Neild, George Tolley, and Leland Yeager—have passed away.

[5] See Campbell (2000, pp. 7–8).

of the economist hires of the 1960s—Milton Friedman's brother-in-law, Aaron Director—was bound to do little to expand the institution's national profile, as Director had already established himself, during his long period as a teacher at the University of Chicago, as someone who produced exceedingly little written work.

A hire in the 1970s—Martin Anderson—was, in contrast to Director, high-profile and prolific. He was also 35 years younger than Director, with Anderson having been born in 1936. But although he had certainly been part of the U.S. economic-research world in the 1960s—graduating with a Ph.D. from MIT in 1962 and holding positions as an assistant and subsequently associate professor in Columbia University's business school—Anderson had largely left that world since then. In his entry in the 1970 edition of the directory of American Economic Association (AEA) members, Anderson listed his area of occupation as "government," his title as "special assistant to the president," and his address as "The White House, Washington D.C."[6] In the 1974 AEA directory, there was no Martin Anderson entry at all.[7] Furthermore, and notwithstanding his forthrightness—both in public debate, and in policy advice given in private to politicians—on macroeconomic matters in the mid- and late 1970s, the research credentials that Anderson had established in the 1960s had been in microeconomics. All told, appointments like those of Director and Anderson therefore did little to tilt the Hoover Institution's research profile away from the international-affairs area and toward national economic issues.[8]

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[6] See American Economic Association (1970, p. 10).
[7] Anderson would become a heavy critic of formal, journal-oriented economic research, and although in doing so he perhaps did not elevate the supply-side economics movement (whose members largely worked outside the research world) quite as much as characterized in Krugman's (1995, p. 85) critical discussion of Anderson (1992), it is certainly true that the 1992 book was caustic about what Anderson perceived as the non-policy-oriented focus of economic research. In addition, in Anderson (1988) he made several negative remarks about his former teachers, Paul Samuelson and Robert Solow. Anderson's targets in the economics profession were not limited to Keynesians or Democratic-candidate-affiliated economists, however. Anderson's (1988) account of the Reagan Administration was highly critical of Anderson's former Nixon Administration colleague Herbert Stein—who, like Friedman and several others, in the 1980s served as a member of Ronald Reagan's outside advisory group, the PEPAB (a group coordinated by Anderson; Friedman's participation in this group is discussed in Chapters 12 and 14 below). The antipathy of Anderson toward Herbert Stein was reciprocated: "I mean, my father was not a fan of Martin Anderson, and that would be putting it mildly—that would be putting it very mildly," Ben Stein observed (interview, March 18, 2015). Friedman, who was a friend of both Anderson and Herbert Stein, would have been well advised to stay out of the feud in his memoirs, but he instead favorably quoted Anderson's (1988, pp. 266–267) trenchant criticism of Stein (see Friedman and Friedman, 1998, p. 392). Stein was, as Anderson perceived matters, not sufficiently supportive of the Reagan Administration's approach to fiscal policy (and, in particular, of its resistance to proposals for tax increases, in the years after its flagship 1981 tax cut had been passed).
[8] Geoffrey Moore was made a senior research fellow of the Hoover Institution in the first half of the 1970s. But Moore retained a full-time position as deputy head of the very East Coast-concentrated National Bureau of Economic Research. Consequently, his Hoover Institution appointment did not, in practice, imply a sustained on-campus presence.

The perception that the Hoover Institution's main agenda items concerned geopolitical matters was also seemingly confirmed by much of the book product issued directly by the organization in the late 1970s, through its Hoover Institution Press publishing arm. The Hoover Institution Press' output included, in 1977, Hugh Seton-Watson's *The Imperialist Revolutionaries: Trends in World Communism in the 1960s and 1970s*, and, in 1978, Robert Wesson's *Lenin's Legacy: The Story of the CPSU*.[9] Wesson also held a conference at the Hoover Institution on September 13–15, 1978, on the subject of the future of the Soviet Union.[10]

Nevertheless, the Hoover Institution did have a Domestic Studies Program, in addition to its International Studies Program and its National Security Affairs Program (Campbell, 2000, p. 8). "The Domestic Studies Program… has undergone major expansion," the institution's director noted in early 1977.[11] In particular, over the course of the 1970s the institution made further, major efforts to beef up its credentials in economics, particularly on the macroeconomic side. One of the key strides in this direction was to participate in joint appointments to the economics department and the Hoover Institution—Michael Boskin and Robert Hall being two major (and, in part because each of their joint appointments began when they were under the age of 40, enduring) cases of individuals who had dual economics-department professor/Hoover Institution fellow affiliations by the end of the 1970s.[12] Separate from this effort, however, a spectacular increase in the visibility of the Hoover Institution was achieved by the appointment of Milton Friedman as a senior research fellow.[13]

W. Glenn Campbell, the director of the Hoover Institution at the time of Friedman's

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[9] See Seton-Watson (1977) and Wesson (1978). CPSU stood for Communist Party of the Soviet Union. Chapters 10, 11, and 14 below will discuss books released by Hoover Institution Press after 1978 on international affairs (some of which may have informed Friedman's sporadic public comments on foreign and defense policy) and one on conscription (which recorded the proceedings of a conference in which Friedman participated).
[10] The proceedings of this conference were published as Wesson (1980).
[11] Remarks of W. Glenn Campbell in Hoover Institution (1977, p. 13). In these remarks, Campbell also indicated that George Stigler headed an advisory group on the expansion of the program. Stigler would have longstanding connections with the Hoover Institution but, in contrast to Friedman, who wholly left the city of Chicago and the university's economics department as part of his move to the Hoover Institution, Stigler would remain based at the University of Chicago through his death in 1991. See Chapter 14 below.
[12] Hall received a joint appointment, of economics-department professor and Hoover Institution senior fellow, that became effective in the 1978/1979 academic year. At that time, Boskin was an economics-department professor and had a courtesy appointment as a Hoover Institution senior fellow, later made more permanent and formal. See Hoover Institution (1979, p. 14).
[13] In connection with the appointment, Martin Feldstein brought up a "good story, [though] I only know it as hearsay." As part of the formalities of the Friedman appointment, letters of recommendation had to be provided. "And so one of the people asked to write a letter on behalf of the appointment of Friedman was George Stigler, and Stigler wrote the following letter: 'Milton Friedman is the best economist in a bad century.'" (Martin Feldstein, interview, November 21, 2013.)

appointment, would subsequently highlight Friedman as "undoubtedly the best-known economist in the world."[14] As it happened, this description—although probably valid as a description of the situation prevailing over the first half of the 1980s—was likely not accurate either when Campbell made it in 2000 (by which point Federal Reserve Chairman Alan Greenspan's fame had certainly eclipsed Friedman's) or when Friedman joined the Hoover Institution in 1977 (when John Kenneth Galbraith—who already had had a television series of his own and had produced numerous bestselling books—was undoubtedly better known worldwide than Friedman). On the criterion of economic-research credentials, however, Friedman had long surpassed Galbraith, Greenspan, and many others. This point was underscored by the fact that when, in early November 1976, Friedman's forthcoming move to the Hoover Institution from the University of Chicago was announced publicly, he was already the winner of a Nobel prize in economics—having been given the award three weeks earlier.

Friedman actually joined the Hoover Institution in the fall of 1977, a little short of a year after this announcement of his appointment and in the wake of his and Rose Friedman's move at the start of the calendar year from the city of Chicago to California's Bay Area.[15] Milton Friedman's affiliation with the Hoover Institution therefore started with the 1977/1978 U.S. academic year. Concurrently, the institution proceeded with other aspects of its ongoing process of improving its profile in the area of economics. As part of these endeavors, a series of research events in macroeconomics was organized, taking place in that 1977/1978 academic year.

An invited speaker at one of these events, and presenting some of his dissertation work, was Olivier Blanchard, 28 years old and recently appointed to Harvard University's economics department after his completion, in the previous May, of his MIT doctorate. Blanchard's research for his Ph.D. had been supervised by Stanley Fischer—who was himself an MIT graduate but who had also, from 1969 to 1973, been a colleague of Milton Friedman's in the Department of Economics of the University of Chicago. The mutual friend in Fischer was the main connection between Blanchard and Friedman at that point—although Blanchard also knew several of the numerous older MIT and Harvard University economists who had debated Friedman over the years. At the time of his paying a visit to the Hoover Institution, Blanchard had never himself met Friedman—and did not anticipate doing so in the near future.

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[14] Campbell (2000, p. 228).

[15] Writing in March 1977, W. Glenn Campbell (in Hoover Institution, 1977, p. 12) stated that Friedman "became a senior research fellow on December 1, 1976." This may have been Friedman's official start date. But the Friedmans did not relocate to California until the start of 1977 and, although he spoke at Hoover Institution events in January and April 1977, he did not move into his Hoover Institution office until after the summer of 1977. See Chapters 8 and 9 below.

As the Hoover Institution event that featured Blanchard's presentation got underway, however, it turned out that his first meeting with Friedman was actually taking place. "My first conference presentation after my Ph.D. was at the Hoover Institution," Blanchard recalled (personal communication, June 2, 2014). "When I came in the room, I saw Milton in the front row. And I literally froze." Standing at the lectern, and expected by the audience to commence his presentation, Blanchard was stunned: "I could not talk for a couple of minutes (infinity as these things go). I thought this was the end of my career."[16]

The natural rate hypothesis and the evolving consensus on inflation

Blanchard's immobilized reaction was an illustration of the fact that, although Friedman might have been behind one or two other economists in name recognition among the American public at large, by 1977 he was second to none in fame in the economics profession on the matter of research on inflation.

The 128-page dissertation by Olivier Blanchard that he drew upon in his Hoover Institution talk was heavily concerned with a development in macroeconomics with which Friedman was closely associated: the natural rate hypothesis (NRH).[17] According to this hypothesis, there might well be a short-run inverse relationship between inflation and the unemployment rate, but no long-run relationship existed.

The 1977/1978 academic year in which Blanchard was presenting this work was the tenth anniversary of the 1967/1968 year that had seen Friedman give (in December 1967) and publish (in March 1968) his American Economic Association presidential address, "The Role of Monetary Policy."[18] Looking back on this work a further 25 years on, Friedman would suggest that, as a research contribution, it ranked below his development of the permanent income hypothesis. "It's *The Theory of the Consumption Function*" that he was most proud of, he explained, his grounds being that his work on "the natural rate had more influence on the

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[16] Blanchard recalls the event at which he presented as being a conference. The Hoover Institution did have a conference on income distribution in October 1977, and a wide interpretation of the subject matter may have permitted the inclusion of Blanchard's paper. It is more likely, however, that Blanchard's presentation was part of the special workshop series in the field of monetary economics that the Hoover Institution convened over the academic year 1977/1978 and that is discussed below.

[17] Blanchard's dissertation had two chapters, both of them concerned with the expectations-augmented Phillips curve: "The behavior of the economy under predictions formed using macro-econometric models" and "Wage indexing in the short and the long run." The second of these chapters was published in revised form as Blanchard (1979). Blanchard recalls that his Hoover Institution presentation in the fall of 1977 was primarily of the first, never-published, chapter.

[18] Friedman (1968a).

world—but, as a consistent program, carried through, it doesn't seem to me [that] it was in the same class." (*Sunday Times* (London), September 8, 2002.)

Not that Friedman denied himself a considerable amount of credit for the Phillips-curve work: in late 1985, he observed, "I was myself more or less the originator of the natural rate hypothesis." In the same remarks, he described the NRH as "so familiar a part of my own intellectual equipment."[19] Friedman had, indeed, been publicly articulating the NRH since the early 1950s, starting soon after his views on monetary and business-cycle matters crystalized.[20]

Despite his early start as an exponent of the hypothesis, however, Friedman was not a prime mover when it came to key aspects of the post-1968 development of the NRH—and particular the areas of empirical testing and theoretical formalization that, together, would help win many economists over to it. On the empirical side, neither in the 1960s nor later did Friedman produce a great deal in the way of empirical testing of the natural-rate-restriction—the accelerationist or verticality condition that put inflation expectations in empirical Phillips curve with a unit coefficient.[21] Nor did he regularly produce his own estimates of the natural rate of unemployment—although he did publicly discuss reasons why it might be rising during the 1970s, and in doing so he clearly drew on others' estimates of the natural rate.[22] Likewise, Friedman was largely in the passive role of a reader, sometimes belatedly, of others' empirical work on the NRH, as estimates of Phillips curves increasingly incorporated the long-run verticality restriction associated with the natural rate hypothesis, rather than an active participant in the many of the journal and conference forums in which these contributions appeared.

On the theoretical side, Friedman's verbal exposition of the NRH in his 1968 article contrasted with the formal modeling of it done by Edmund Phelps both in the 1960s and 1970s. In addition, because it was expectations-oriented and related to intertemporal optimizing behavior, the NRH fired up a movement in 1970s economic research of which Friedman was not part: the theoretical literature on rational expectations and the microeconomic foundations of macroeconomics, including the macroeconomic analysis of labor markets. In the early 1970s, this literature

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[19] Milton Friedman, letter to Nigel Duck, December 18, 1985. The author is grateful to Nigel Duck for providing a copy of this correspondence.
[20] See Nelson (2020a, Chapter 7).
[21] See Nelson (2020a, Chapter 7; 2020b, Chapter 14) and Chapter 13 below for further discussion.
[22] Chapter 5 below discusses Friedman's analysis during the 1970s. With regard to Friedman's hesitancy to produce estimates of the natural rate unemployment, see the discussion in Nelson (2020a, Chapter 7), as well as Friedman's remark in Snowdon, Vane, and Wynarczyk (1994, p. 177): "I do not have any numerical estimate of the rate of unemployment that currently corresponds to the natural rate in the USA."

featured major contributions regarding the NRH on the part of Robert Lucas, Thomas Sargent, and Dale Mortensen, among others.

That others had done much of the running in the economics profession's development and absorption of the NRH was manifested in the fact that Phelps, Lucas, Sargent, and Mortensen all later won Nobel awards in economics (the first two in Friedman's lifetime, the latter two after his death in the decade after his death in 2006). It was also registered in the fact that Blanchard's 1977 dissertation—despite its concentration on natural-rate and Phillips-curve matters—did not cite, or even mention, Friedman (or Phelps).

Friedman had, nevertheless, played a central role in catalyzing the post-1967 work on the NRH, including Lucas', that Blanchard did cite. Reflecting this reality, Gelting (1974, p. 67) had observed that Friedman's 1968 article "certainly greatly stimulated" the theoretical discussion of the inflation/unemployment tradeoff. The importance of Friedman's own analytical contribution on the matter of the Phillips curve was signified by it being part of the basis for his own Nobel award in 1976, and its significance as a reason for his receipt of the award was reflected in Friedman himself devoting his Nobel lecture, "Inflation and Unemployment," delivered in December 1976 and published in 1977, to the distinction between the short-run and long-run Phillips-curve tradeoffs.[23]

Recidivism in inflation analysis during the 1970s

In a retrospective, over 25 years after Friedman's Nobel award but still within his lifetime, Laurence Ball (2005, p. 263) underlined the point made by Gelting (1974) but went further by describing the 1960s and 1970s as featuring a state of affairs in which many U.S. economists subscribed to fallacious views on inflation—a state eventually fixed once "a genius arrived on the scene: Milton Friedman." With regard to inflation, Ball argued, "Friedman cleared away confusion. He repeatedly explained to us that 'inflation is always and everywhere a monetary phenomenon.' In 1968, he explained that the output-inflation tradeoff exists only in the short run; in the long run, unemployment must gravitate to its natural rate. Friedman's views were controversial at first, but they were soon absorbed into mainstream thinking. By 1979, U.S. policymakers had learned Friedman's main lessons."

[23] Chapter 6 of this book discusses the contributions that were embedded in Friedman's 1976 Nobel prize as well as the circumstances of his learning of the prize and the content of his Nobel lecture (Friedman, 1977c). Chapter 7 then provides further details regarding Friedman's week in Stockholm in December 1976, in which he attended events related to his award.

Ball's retrospective provided an accurate characterization: the natural rate hypothesis and—a Friedman position related to and highly consistent with the EAPC, but not actually implying nor implied by it—the dictum that inflation was a monetary phenomenon did become hallmarks of mainstream U.S. economics.[24] But—as might be guessed on the basis of the gap between the 1968 and 1979 dates that Ball cited—the process of absorption of these ideas into the mainstream involved important delays and bumps along the way. As this book will discuss, in important respects, the "soon" that Ball portrayed as part of the process of assimilation spanned nearly the whole decade of the 1970s.

As far as policymaking was concerned, it was unambiguously the case that between 1970 and 1979 the Federal Reserve leadership eschewed Friedman's basic positions on inflation. The conversion to a more monetary policy-centered view of inflation never got a foothold at the Federal Reserve between 1972 and 1978 in the first place. Arthur Burns became a proponent of nonmonetary views of inflation soon after he became Federal Reserve chair in 1970.[25] And he did not repudiate those views over the rest of his tenure as the Federal Reserve's chair, including during his second term as chair from early 1974 to early 1978.[26]

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[24] As was stressed in the discussion of inflation and the Phillips curve in Nelson (2020b, Chapter 7), Friedman's proposition that inflation was a monetary phenomenon and that a long-run vertical expectations-augmented Phillips curve were not advanced as rival propositions but rather were compatible. That discussion cited several references that have recognized the compatibility of the two propositions (see also Hoover, 2008, for another forceful statement to this effect). Failure to see their compatibility has, however, also lingered in the economic literature through the years and has, in part, reflected a misconception that the notion that inflation is a monetary phenomenon implies a structural equation linking price changes to money or other monetary policy variables. Even Steindl (2004)—a study mostly concerned with the relationship between monetary policy, prices, and output—and with Friedman's view on their linkages—erroneously stated that a Phillips-curve framework "has no role for the quantity of money in affecting prices" (p. 113).

[25] Lawrence Christiano stated (in Usabiaga-Ibánez, 1999, p. 89): "Arthur Burns was a card-carrying monetarist." This characterization was not correct. Burns did not consider himself a monetarist, and his various remarks during the 1970s on the role that monetary policy could play in removing inflation did not correspond to the monetarist position. It is worth stressing that statements to the effect that the achievement of long-run achievement of price stability requires, *among other things*, that monetary growth should be at rates consistent with nominal GDP growth not appreciably exceeding potential real growth do not capture the monetarist position on inflation.

[26] Chapters 3 to 8 below contain extensive analysis of Burns' views and policy decisions over the period from 1973 to 1979 and discusses the case for the interpretation of Burns' views taken here. A key point that is developed in this analysis—building on DiCecio and Nelson (2013)—is that such developments as the outbreak of inflation in 1973, the troubles with and expiration of wage and price controls in 1973−1974, and the introduction of monetary targeting in 1975, did not shake the nonmonetary perspective on inflation that he had articulated in 1971−1972. An early indication that this was the case was in a memorandum of April 13, 1973, that Burns wrote to President Nixon. In it, Burns wrote that the anti-inflation "program I have in mind would consist of a package of reforms, both legislative and administrative, designed to reduce substantially the existing abuses of economic power by the labor unions and corporate giants." It also proposed that the "formation of a Wage and Price Review Board, operating on a voluntary basis to achieve moderation in wage and price behavior, would be a part of the reform package." Pending such a package becoming feasible, the president "might well consider an early return to a mandatory program of prenotification [of planned wage and price increases] for large firms and employee groups. The Cost of

Burns' attitude to the United States' renewed inflation problem became clear in his response to the change in economic fortunes that occurred near the start of the final year of his first term. Reflecting the misplaced euphoria prevailing at that time, a Reuters analysis stated at the end of 1972: "Having tackled its inflation so much more successfully than Britain and continental Europe, the U.S. does not need their degree of dear money and still less any replica of the 1966 and 1969 credit crunches." (*South China Morning Post* (Hong Kong), December 31, 1972.) The subsequent eruption of U.S. inflation—which essentially began at the turn of 1972/1973 (see Chapter 2)—dispelled this optimism. To many, including Friedman, the inflation surge also demonstrated the extent to which strong inflationary pressure in 1971−1972 had been masked by those years' price controls. But although Arthur Burns recognized that there was an excess-demand element in 1973's inflation breakout—and, in the event, did exercise a substantial monetary tightening in response, building on 1972's increases in the federal funds rate—he predominantly denied Federal Reserve responsibility for high inflation and pointed to the nonmonetary sphere in discussing both the explanation of, and the remedy for, inflation.

G. William Miller took Burns' place, in what was perceived at the time as a hostile takeover. But, in his short-lived tenure as Federal Reserve Chair (of slightly less than eighteen months in 1978−1979), Miller propounded views on inflation that were largely interchangeable with those subscribed to by his predecessor.[27]

Both Burns and Miller placed great emphasis on incomes policy as the instrument to be deployed against inflation, on cost-and-price pressures unrelated to the aggregate demand/aggregate supply balance as a major and systematic source of inflation in the modern era, and on the marginal contribution (one essentially limited to *not adding upward pressure* on inflation, rather than being the means of restoring price stability) that could be contributed by monetary policy. As Chapter 10 of this book will document in detail, Paul Volcker made the difference: when he came to office as Federal Reserve chairs, he quickly eschewed nonmonetary views of inflation.

Outside the Federal Reserve, too, cost-push views continued to thrive in public discourse over much of the 1970s. It might be expected that the fact that the Nixon wage and price controls failed to prevent an outbreak of inflation in 1973, and then expired in 1974, would have had the effect of creating a dissipation of interest in approaches to the analysis and control of inflation

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Living Council could then be empowered to hold up specific increases in both wages and prices until it is satisfied that appropriate rules of behavior are being met." See Burns (1973e, p. 2).

[27] See Chapters 8 and 10 below.

that did not focus on the state of aggregate demand.[28] This dissipation did not, however, take place in the years from 1974 to 1978. A key reason why it did not was the phenomenon of renewed and intensified stagflation.

Stagflation had helped give rise to the 1972 acceptance of the NRH in the empirical Phillips-curve literature. But stagflation was always susceptible to an alternative interpretation. The NRH had suggested that inflation reacted to demand-and-supply forces and that they did so in a manner that implied that the inverse long-run inflation/unemployment relationship faded as time passed. The alternative account of stagflation suggested, in contrast, that inflation did not depend on demand-and-supply factors in the first place.[29] In this nonmonetary perspective on inflation, inflation and unemployment had no tendency to be inversely related even in the short run, and the inflation process was dominated by cost-push forces. The appeal of this nonmonetary explanation was why the conversion to a more monetary policy-centered view of inflation did not get a foothold at the Federal Reserve in the 1970s, as Arthur Burns quickly latched on to cost-push interpretation of developments when stagflation appeared in 1970.

But the public-policy world outside the Federal Reserve—such as commentaries on the economy by members of past administrations—embraced this interpretation, too. And it did so very strongly in the post-price control environment of 1974–1975, when the United States' stagflation phenomenon was much worse than it had been in 1970. The perception that a large amount of economic slack was coexisting was high inflation was exacerbated by the real-time data and estimates used at the time, which seemed to suggest that the double-digit inflation of 1975 was being accompanied by a double-digit sized percentage shortfall of output below its potential level (Orphanides, 2003, Figure 2, p. 645). As it happened, one feature that was widely predicted as the next phase of the United States' stagflation problem—a wage explosion to follow the price outburst—did not occur: nominal wage growth turned out to be quite restrained *vis a vis* prior and contemporaneous inflation in 1974 and 1975 (see Chapter 2 and 5 below). In popular discourse, however, profit-push explanations filled the void left by the wage-push explanation, with historian Arthur Schlesinger stepping into economic discourse to suggest that prices were being set by firms independently of demand and supply.

Arthur Schlesinger's intervention prompted a withering public reply on Friedman's part. But the views that Schlesinger advanced were not, in fact, far removed from the perspective on inflation

[28] Chapter 2 below discusses the 1973 eruption of inflation and the 1974 end of the Nixon wage/price controls.
[29] It, in particular, suggested that this was the case when output fell short of potential output—that is, in circumstances in which the output gap was negative.

taken within the Ford Administration. Federal government economic official Albert Rees, a former University of Chicago colleague of Friedman's, himself gave credence to administered-price and other nonmonetary explanations of inflation when he headed the executive branch of government's Council on Wage and Price Stability.[30] Indeed, despite certainly having some members in senior positions who were sympathetic to Friedman's point of view on the monetary origin of inflation, the Ford Administration pursued a number of nonmonetary schemes against inflation—schemes that were less binding on the national economy than the Nixon wage-price controls, but that prominently, and infamously, included Ford's high-profile "Whip Inflation Now" campaign, intended to rally members of the general public into fighting inflation through their personal attitudes and initiatives.[31]

The Whip Inflation Now campaign, which quickly fizzled out, did not obtain significant support from the U.S. economics profession, whereas the Nixon controls had done so for the first eighteen months to two years of their three years in force. That WIN was ridiculed in 1974–1975 suggested that Friedman's position that measures other than aggregate demand control could not hope to cure inflation had made at least some progress over the first half of the 1970s in influencing opinion in policy circles and in the general public—as well as in helping shape the views of the proverbial median economist. Nevertheless, it would not be accurate to suggest that the Friedman perspective on inflation, including the NRH, had prevailed as the consensus view on inflation by the time of the Ford years or in the early years of the Carter Administration that took office in January 1977.[32] At the time when Blanchard was presenting his dissertation work at the Hoover Institution event, the macroeconomics world in the United States was characterized by a considerable schism—with those doing theoretical work much more receptive to the ideas associated with Friedman than were many policy-oriented economists.

The natural rate hypothesis had, indeed, become the benchmark hypothesis in front-line technical economic research, and, in particular, in research involving formal theoretical modeling, by the mid-1970s. It was in reference largely to the theoretical work that developed and fortified the NRH that Cochrane (2023, p. 2) observes, "The 1970s were a golden decade for macroeconomic

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[30] See Chapter 2 below.
[31] Again, see Chapter 2.
[32] For example, in testimony given on January 27, 1977 (a week into the Carter Administration), the head of the Council of Economic Advisers, Charles Schultze, stated that the present "kind of inflation has not been and will not be cured by a policy of sluggish recovery, high unemployment, and idle plant capacity" (Committee on the Budget, U.S. House of Representatives, 1977a, p. 211). For a detailed discussion of anti-inflation policy in this period, see Chapters 8 to 10 below.

research, as much as they were a miserable decade for the economy." In the wake of this literature, the presumption that the NRH was correct as a long-run proposition, and that inflation/unemployment dynamics should be interpreted in that light, permeated Blanchard's (1977) dissertation, which was not an empirical study but instead was concerned with deriving dynamics in a theoretical context. In that theoretical analysis, he noted, "the tradeoff is only between unexpected inflation and output" (Blanchard, 1977, p. i).

It was a different story, however, among many other economic researchers in the mid- and late 1970s—particularly those who regularly made interventions in public debate. At first, matters in the empirically-oriented U.S. economic-research world did, like the theoretical literature, seem to be settling in favor of acceptance of the NRH. This was in large part thanks to the econometric postmortems carried out on the U.S. Phillips-curve relationship. Empirical studies of inflation had been faced with the fact that U.S. inflation in the very early 1970s was much higher than would have been predicted on the basis of its relationship with the unemployment rate. The accelerationist restriction associated with the NRH was found helpful in explaining recent data. As a result, the no-long-run-tradeoff view embedded in the NRH had wide and growing acceptance among U.S. economists in academia and at think-tanks (like the Brookings Institution) by 1972.[33]

But, having reached this state, economic thinking in the United States did not rest there. It would, consequently, not be correct to characterize the debate on the NRH, and on the accompanying judgment on aggregate demand policy's role in controlling inflation, as settling down permanently in 1972 in U.S. economics circles as a whole. It would turn out that the period from 1972 to 1978 was not, in fact, a straightforward one of consolidation in the acceptance, in practical circles, of NRH ideas and of inflation as a monetary phenomenon. Rather, there was a considerable amount of new doubt thrown on the link between aggregate demand and inflation, even when this was viewed through the NRH-based Phillips-curve lens.

More specifically—and as subsequent chapters of this book that cover this time span in detail will elaborate: From 1972 to 1978, there was considerable recidivism among U.S. economists with respect to inflation, particularly on the matter of whether the expectations-augmented Phillips curve should be regarded as the new benchmark means of understanding the inflation process. Paradoxically, therefore, the same period in which Friedman won the Nobel prize in

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[33] See Gordon (1976a, p. 193) and Blinder (1981c, p. 95) for discussion, as well as the retrospectives in Nelson (2020b, Chapter 14).

economics partly on the basis of his Phillips-curve work, and published his *Journal of Political Economy* article on inflation and unemployment, saw fresh impetus in the United States against his ideas about inflation. This reaction amounted to two-pronged backlash. Both his view of how the Phillips curve worked and his position that monetary restriction had to be the means of eliminating inflation encountered fresh and prominent opposition.

In this recidivism regarding the analysis of inflation, a leading figure was Arthur Okun. In 1975, Okun had declared that all economists were accelerationists now. But his conversion was extremely short-lived—as in the late 1970s he was, once again, strongly espousing the view that inflation in the 1970s was resistant to demand restraint. It would transpire that Okun's revised opinion was that he was glad to accept the Friedman-Phelps story as valid and useful in understanding the developments in U.S. data seen over the 1960s—but not those recorded since 1970.[34] In interpreting post-1970 developments, Okun did not revert to a belief in an old-style Phillips curve. But he now rejected the augmented Phillips curve, too—in favor of a cost-push view of inflation. Okun was evidently impressed by the phenomenon noted above: the apparent simultaneous occurrence in 1974−1975 of very large amounts of slack and of inflation close to double digits. The NRH suggested that such a coincidence could occur temporarily, but not as a persistent phenomenon, as slack would pull inflation down. In contrast to this prediction, however, a large-slack/high-inflation combination seemed to be present in the United States seemed, to many, to be prevalent over much of the period from 1974 to 1977. For example, from early 1977 inflation started picking up from its mid-decade decline, at a time when official estimates of the output gap were still sharply negative, standing at about −5 percent late in the year (Orphanides, 2003, Figure 2, p. 645; 2004, Figure 3, p. 160).

Retrospective accounts would establish that there was actually little in inflation's behavior in 1975−1978 that was out of the ordinary from the perspective of a long-run vertical, expectational Phillips curve (see, for example, Rudebusch and Svensson, 1999, Roberts, 2005, and the discussion in Chapters 5 and 10 below). It was certainly the case that simultaneous slack and high inflation occurred in 1975, which saw the decade's peak rate of unemployment. But inflation in that year was exhibiting a steep decline (albeit with bumps)—a decline that intensified in 1976 and saw the twelve-month rate slip below 5 percent. This outcome was consistent with the emergence of economic slack producing downward pressure on inflation. The simultaneous improvement in the rates of inflation and unemployment of the kind seen in

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[34] See Chapter 5 below. Gordon (1983, p. 26) correspondingly associated Okun with the view that the inflation of the 1970s could not be understood using any kind of Phillips-curve framework.

1976 (and again from 1983 to 1986) was consistent with the NRH—indeed, Friedman had opined even in 1965 that once inflation was brought down, it was possible for a noninflationary recovery to ensue. Conversely, when inflation began its fresh surge at the turn of 1976/1977, it was in conditions in which the output gap was crossing from negative to positive values.

In contrast, as already noted, the output-gap estimates at the time pointed to a different conclusion, because they registered continued considerable slack throughout 1977, even as inflation was rising. The seemingly recurring experience of inflation being resilient in the face of slack—inflation behavior that had already ended Arthur Okun's brief conversion to the accelerationist Phillips curve—seemed by 1978 to be creating deep doubts about the susceptibility of the inflation problem to a monetary-policy-based solution of a similar kind even in Alan Greenspan, formerly CEA chairman under Ford. In 1978, Greenspan, apparently accepting the readings on output gaps produced at the time, professed to see simultaneous large slack and high inflation as having become a persistent part of the U.S. economic landscape and, in reaction, he showed signs of succumbing to the view that the modern experience was confounding explanations of inflation that were centered on demand and supply.[35]

Among academic economists, a prominent and jarring case of the resurgence in nonmonetary views of inflation came in the form of Rudiger Dornbusch. Dornbusch had been a graduate student in economics at the University of Chicago through 1970.[36] He would briefly (in 1974/1975) be a teacher at the university's business school. But his lasting appointment was at the Massachusetts Institute of Technology (MIT) economics department from 1975 onward, as an associate professor and then professor (American Economic Association, 1981, p. 124). Specializing in open-economy analysis, Dornbusch in his time as a student at the University of Chicago had mostly moved in circles different from Friedman's. Consistent with this, Dornbusch's pathbreaking paper on exchange-rate overshooting, published in the *Journal of Political Economy* in 1976, was notable for using an old-fashioned—both permanently-nonvertical and expectations-*un*augmented—Phillips curve as part of its theoretical model.[37] But, although he was a late convert, Dornbusch endorsed the NRH in his teachings at MIT in the mid-

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[35] See Romer and Romer (2004, p. 158). See also the discussion in Chapter 10 below.
[36] Robert Gordon recalled (interview, March 21, 2013) the "wave [of graduate students] that came through Chicago that was there in 1968 when I arrived [as an assistant professor]. Because of my MIT-slanted education, they knew a lot more about some things than I did... And they included people like Jacob Frenkel, Rudi Dornbusch, Mike Mussa, Rachel McCulloch, Claudia Goldin. Also Mike Darby. They were really an extremely impressive group of people. [In doctoral supervision, Robert] Mundell tended to get the best students, including Dornbusch and Frenkel."
[37] See Dornbusch (1976). Other aspects of Dornbusch's open-economy analysis are discussed later in this chapter, while Chapter 15 provides a more extensive discussion.

to late 1970s—a position reflected in his 1978 undergraduate macroeconomics textbook with Stanley Fischer.[38] At the point when this textbook was completed, Dornbusch seemed to have taken, if with a slight lag, the course that many economists of his generation had followed during the 1970s: moving from an initial reaction of resistance to the NRH, to embrace of it.

Things then took a sharp zigzag in March 1978, at a time when the writing of the textbook had been completed. Early in that month, the 35-year-old Dornbusch testified to Congress in order to provide an analysis of the current U.S. economic situation and his own policy prescriptions. His analysis of inflation was jarringly different from the Dornbusch-Fischer textbook's treatment and much more like the accounts given in the second half of the 1970s by Arthur Okun. Weeks later, in May 1978 at MIT, Dornbusch would give departmental approval (providing a formal supervisor's signing-off on the document, in Fischer's temporary absence) of Blanchard's NRH-oriented doctoral dissertation. But Dornbusch's March 1978 testimony indicated that he was, in fact, now gripped by skepticism about the Friedman position on inflation.

In a sharp backtracking from his textbook analysis, Dornbusch's testimony provided an account of the 1970s record, and related policy recommendations for 1978, that suggested that aggregate demand restriction did not have a role to play in removing U.S. inflation. CPI inflation in the year to January 1978 had been 6.8 percent, but Dornbusch's testimony suggested that "sustained real growth considerably above the trend is required for two or more years."[39] He added: "I don't really see that monetary policy has by any means been very expansionary… we should have easier monetary policy to accommodate the growth of investment."[40] Dornbusch suggested the deployment of nonmonetary measures against inflation—that is, devices designed to affect specific prices and costs—in the context of expanding aggregate spending via monetary and fiscal steps.[41] His submission accompanying his testimony insisted: "The lessons of the last few years have forcefully established the point that low aggregate demand is a very poor instrument

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[38] On the relation of this textbook to Friedman's positions, see Nelson and Schwartz (2008, p. 848) and the discussion of late-1970s teaching at MIT in Chapter 8 below. Robert Gordon (interview, March 21, 2013) dated Dornbusch's expounding of the NRH to the mid-1970s in the period Dornbusch slightly before he joined MIT. "What those books achieved [Dornbusch and Fischer, 1978, and Gordon, 1978] was they both developed—and I certainly give him credit—the first diagram of what I always call the SP-DG [sticky-prices/demand-growth] model of inflation dynamics. Apparently, it was [originally] a [University of] Chicago business-school handout by Rudi Dornbusch somewhere, back in 1975. And it's always been in my textbook. You've got nominal GDP growth that determines the demand curve and you've got the Phillips curve that moves up and down with expectations. So this is the inflation-[expectations]-augmented Phillips curve."

[39] Testimony of March 7, 1978, in Committee on Banking, Finance, and Urban Affairs, U.S. House of Representatives, 1978, p. 42).

[40] In Committee on Banking, Finance, and Urban Affairs, U.S. House of Representatives (1978, pp. 43, 46).

[41] In Committee on Banking, Finance, and Urban Affairs, U.S. House of Representatives (1978, p. 43).

with which to attack the inflation problem."[42]

This presentation represented a striking change in the direction in which Dornbusch's thinking on inflation had been going in recent years. But it was symptomatic of wavering by economists in the years from 1976 to 1978 on the matter of whether they had converged to the expectations-augmented Phillips curve as their framework for analyzing inflation and on demand restraint as the means of addressing inflationary pressure. Dornbusch, Okun, and Otto Eckstein were all examples of prominent economists whose analysis in the late 1970s implied an acceptance that 1960s-style inflation/unemployment tradeoff ideas should be eschewed, *but* that the analysis of inflation to be used as a replacement was not the NRH but, instead, a framework in which there were powerful forces propelling inflation that were separate from the balance of aggregate demand and supply.[43]

In the executive branch of government, too, a nonmonetary approach to inflation was evident in the Carter Administration from its inception. This administration accepted that older Phillips-curve ideas had been shown wanting, but it also expounded the position that the era of stagflation showed that inflation was demand-resistant and that most of inflation was not due to excess demand in the first place. The state of inflation analysis in 1978 was, therefore, little different in the U.S. administration and among many leading U.S. macroeconomists from the mindset that had led to the Nixon wage/price controls in 1971. Truly statutory controls remained out of vogue, as they had been in the very late Nixon period and under Ford. But the Carter Administration actively tried to introduce a legislative wage-price control mechanism in the form of tax penalties for wage and price increases deemed excessive—a variant of the tax-based incomes policy (TIP) advanced in public forums by Okun and correspondingly condemned by Friedman as amounting to wage and price controls—being not an alternative to such controls but simply a scheme that used more elaborate methods for penalizing control violations.[44]

In 1979, the situation was transformed. A reassessment by the U.S. government of the country's aggregate-supply performance in the 1970s greatly reduced the amount of estimated slack through 1978. This made it a much more straightforward matter for analysts using these estimates, which were publicly available, to interpret the decade's developments in terms of

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[42] In Committee on Banking, Finance, and Urban Affairs, U.S. House of Representatives (1978, p. 50). See Chapter 8 for further analysis of this Dornbusch testimony.
[43] On Eckstein's analysis in this period, see Chapter 13 below.
[44] On the campaigning for TIP by two of its main proponents, Arthur Okun and Henry Wallich, and Friedman's reactions, see Chapters 5 and 11 below. As already indicated, the Carter Administration's nonmonetary approach to addressing inflation is primarily discussed in Chapters 8 and 10.

excess demand.[45] In government, the Carter Administration continued to adhere to a nonmonetary view of inflation but—as the new estimates of the output gap showed output near, or crossing, potential—was, in notable contrast to its first eighteen months in office, supportive of a tightening of aggregate demand (including via monetary policy) in order to forestall an overshooting of full employment. In the Federal Reserve, the shift in view was more fundamental: with the ascendancy of Paul Volcker, a decided move in the thinking on the part of the Federal Open Market Committee and the Federal Reserve Board occurred during 1979 toward a monetary view of inflation. These policymakers correspondingly perceived the creation of economic slack as being a necessary part of the disinflation process.

As for Rudiger Dornbusch, in 1979 he underwent a striking and rapid reconversion to subscribing to the combination of the two positions that Milton Friedman stressed—the NRH-based view of the expectations-augmented Phillips curve and the notion that inflation was a monetary phenomenon. In a paper delivered at a conference held on June 17−19, 1979, Dornbusch and Fischer (1981a, p. 319) opened by noting: "The view that inflation is always and everywhere a monetary phenomenon is widely accepted as an explanation of at least long-run inflation." Their subsequent analysis had the "inflation rate… determined by the interaction of aggregate demand and supply" (p. 321).

Dornbusch also made clear in post-1979 accounts that he believed that the high U.S. inflation of the 1970s had, indeed, been due primarily to the monetary policy followed under Arthur Burns' Federal Reserve.[46] It was now not difficult for him to see excess demand as having been present in 1978 even though, at the time, he had perceived there to be considerable slack. In his March 1978 testimony, Dornbusch had conditioned on an assumed full-employment or natural rate of unemployment of 5 to 5.5 percent and contrasted this with the 4 percent benchmark rate of the 1960s (Committee on Banking, Finance, and Urban Affairs, U.S. House of Representatives, 1978, p. 49). In the event, however, an assumed natural rate of 6 percent over the 1970s was

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[45] The 1979 revision to the Council of Economic Advisers' estimates of potential output went strongly in what modern accounts concur was the correct—downward—direction. The resulting estimates still overstated potential output and consequently were out of line with later assessments that the output gap was positive in the late 1970s. But other implicit and explicit estimates by government officials in the late 1970s and early 1980s correctly gauged that excess demand was emerging in this period (see, for example, Taylor, 1979b; Cagan, 1986c, pp. 254−255, 272; Orphanides and Williams, 2005), and the Volcker Federal Reserve's actions in 1979 were consistent with this assessment.

[46] See Chapter 8. In addition, Blanchard and Dornbusch (1992, p. 124) plotted the U.S. output gap as being positive from 1972 to 1974 and from 1978 to 1980, while also noting that "in 1978−79 overheating of the economy gave rise to double-digit inflation."

more realistic and had been adopted in many retrospectives on the decade's experience.[47]

Dornbusch's position from 1979 onward reflected the emergence of solid and durable consensus in the U.S. economics profession, one that started in that year. This consensus endorsed a monetary view of inflation, with monetary policy a key factor driving demand pressures. The consensus further accepted that these pressures were, in turn, transmitted to inflation via an expectations-augmented Phillips curve featuring a unit coefficient on inflation expectations.[48] At the culmination of the first—1979 to 1988—decade in which this consensus prevailed, DeLong and Summers (1988, p. 433) observed: "The natural rate hypothesis, with its corollary that demand management policies cannot affect an economy's long-run average level of unemployment or output, has come to be widely accepted even by Keynesian economists. This view is enshrined in standard textbooks."[49]

For his part, Friedman had been able to maintain a consistent view on inflation's drivers during the 1970s, including during the 1972–1978 period in which many colleagues in the profession wavered in the degree to which they were willing to connect inflation's source to monetary expansion and its cure to monetary restraint. One of his most notable correct predictions of inflation reflected this consistency. Many were caught off guard by the return of double-digit inflation in 1979–1980. But Friedman was not.[50] By mid-1977, he had already made a prediction of renewed double-digit inflation, to materialize at the end of the 1970s. He was able to do so while sidestepping the matter of appropriate estimates of economic slack by predicting inflation's rise on the basis of nominal variables—in particular, the rapid monetary growth rates

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[47] See Orphanides and Williams (2005) and the discussion in Chapters 5 and 10 below. The 6 percent ballpark estimate of the natural rate of unemployment (regarded as applicable to the perceived conditions of the 1980s and also seen as the best retrospective estimate for the 1970s) was apparent in Nordhaus' (1983, p. 9) remark: "I am aware of no careful analyst who today argues that the inflation-threshold rate is markedly below 6 percent." Dornbusch and Fischer (1987, pp. 552–553) refrained from using a specific number, but they employed a band of estimates (5.5 to 6.5 percent) centered on 6 percent for the period since 1975, and they noted that U.S. government sources used 6 percent as the current value of the natural rate of unemployment (p. 552).
[48] Even Otto Eckstein, who had been one of the strongest proponents in the United States of the notion that inflation was difficult to fight via the restriction of aggregate demand (see Chapter 14 below), was willing by 1983 to concede the possibility that inflation "may, in fact, be highly responsive to demand" (Eckstein, 1983, p. 14). He was also willing to grant an important contribution of monetary policy in past high inflation, observing (Eckstein, 1983, p. 21): "The negative real interest rates created by excessive monetary expansion in some past years were ultimately very destructive."
[49] The impact on undergraduate textbooks of the natural rate hypothesis and other Friedman-connected ideas is considered in Chapter 6's discussion of leading undergraduate textbook author Campbell McConnell.
[50] When Lawrence R. Klein won the economics Nobel in 1980, one commentator observed that in 1976 Klein's "model and others failed to foresee the spiral in the inflation rate that occurred two years in 1978." (*Kansas City Star* (Missouri), October 17, 1980.) Klein was certainly not alone in not predicting a major post-1976 rise in inflation, but Friedman was almost alone in forecasting that rise. For a detailed discussion, see Chapters 8 and 10 below.

seen during Arthur Burns' second term as head of the Federal Reserve. Friedman also accurately predicted that the surge in inflation would in due course be associated with a period of stagflation. "Nearly a dozen years ago, I warned of an inflationary recession… We have since then had three… and a fourth is almost surely on the way." (*Newsweek*, April 24, 1978.)

Friedman was able to rationalize the coexistence of rising inflation and rising unemployment on three key grounds—the expectational dynamics associated with the NRH; a rising natural rate of unemployment; and lags of inflation behind nominal aggregate demand—and he appealed to all of these three grounds in explaining the U.S. inflation experience of the 1970s.

Friedman remained, as in previous decades, extremely ill-disposed toward the principal alternative way in which to explain simultaneous high inflation and weakness of output: the cost-push view of inflation. He did not deny that the factor cited by proponents of cost-push monopoly power existed among goods producers and labor suppliers. Indeed, the fact that Friedman was in favor for several decades of antitrust legislation in the United States, and expressed some support for its extension to the area of labor unions, was testament to his acknowledgment of the existence of monopoly power in the United States. And even after he became disaffected with antitrust law and moved (in 1973−1974, as discussed in Chapter 4 below) toward favoring the repeal of much of it, he continued to advocate the undermining of monopoly power by other means, including through reducing barriers to entry into lines of work and into particular areas of production and via the removal of restrictions on international trade.

Friedman's rejection of the cost-push interpretation of inflation did not arise out of a perception that the U.S. economy was perfectly competitive—he most certainly did not subscribe to that view—but because he rejected the premise that monopoly power gave rise to inflationary pressure. As far as he was concerned, this premise, like other cost-push theories of inflation, was long refuted, notwithstanding its perennial popularity in popular explanations and the emergence on occasion of new proponents of it in the economics profession.

Friedman had, in particular, put stress on the point that monopoly power in a sector did not mean a sector or price that was immune to market forces of demand and supply: the normal circumstance would, instead, be that a monopoly would, in response to variations in market conditions, adjust the price that it charged for its product. In making this point, as he had in many discussions in which he had been involved since 1950 on the monopoly/inflation link, Friedman was echoing the position voiced by a prior generation of University of Chicago economists—including Henry Simons, who had remarked in a 1938 radio broadcast: "it is

important to recognize that the producer has some limits upon the prices that he can charge—his price policy is controlled by the demand conditions for his product, that is to say, by the competition of other products."[51]

This constraint on the within-sector power of a monopolist limited the ability of monopolists to affect aggregate inflation, too. But, as Friedman stressed, there were other economy-wide constraints on the impact of monopolies on inflation. If a monopolist tried to set their prices without any regard to market conditions, it would not set off an inflationary spiral but instead generate forces that tended to limit the effects on inflation. In particular, behaving in this manner would generate increases in their own product's prices but, over time, these price actions would leave a mark on relative prices but not the aggregate price level. And if the degree of monopoly power exercised in the economy did experience an increase over a period, this would still not imply inflation but, instead, an adjustment of the aggregate price level to a higher value.[52] To suggest otherwise, Friedman and Schwartz observed in their *Monetary History*, reflected "the confusion between 'high' and 'rising' that is so common a fallacy in reasoning about economic matters."[53]

At a distance from the research world

Owing to his constant involvement in public-policy debate, Friedman was conscious of the ongoing heavy adherence in the media to nonmonetary views of inflation. He also would have had some awareness of the comeback of these views among some economists, not least because he was asked about the TIP idea (see Chapter 5). Friedman's appreciation of the resilience of cost-push views in the analysis of U.S. inflation was evident in the spring of 1978, when he

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[51] NBC (1938, p. 4).

[52] In the standard framework for analyzing inflation, the degree of price increase would be governed by the extent to which the increase in monopoly power: see, for example, Batten (1981, p. 21) and McCallum (1990, p. 965). In terms of the expectational Phillips curve, therefore, one could allow for monopoly to exert an influence on inflation by affecting log potential output y_t^* in the equation $\pi_t = \pi_t^e + \alpha\ (y_t - y_t^*) + u_t$ (π_t^e being inflation expectations and u_t the cost-push shock in period t) with higher monopoly lowering y^* and affecting inflation in the period during which the authorities do not allow real aggregate demand y_t to decline commensurately with the fall in y_t^*. Of course, cost-push proponents went further, including by arguing that cost-push forces affected inflation by creating systematically positive values of u_t. Friedman argued implicitly that u_t was zero-mean and tended to be serially uncorrelated. He therefore essentially denied that this shock term, which captured cost-push forces, figured importantly in the explanation of extended periods of rising prices. The main empirical qualification that Friedman made that he was prepared to grant that the early New Deal years did see a sequence of positive u_t shocks in the United States (see Friedman and Schwartz, 1963a, pp. 498–499, and the further references and analysis provided in Nelson, 2020a, Chapter 2). This period saw, according to the *Monetary History*, "explicit measures to raise prices and wages" on a wide scale and with government backing (Friedman and Schwartz, 1963a, p. 498).

[53] Friedman and Schwartz (1963a, p. 498).

referred to “the many factors other than money that politicians, economists, and journalists write about” in trying to account for the inflation experienced over the past decade (*Newsweek*, April 24, 1978).

Nevertheless, there were reasons for believing that Friedman was largely oblivious to the setbacks occurring in the acceptance of the EAPC in research circles, and of the continuing reluctance by many economists to tracing the inflation of the 1970s to the behavior of monetary policy. Many of the comments that he made in the late 1970s about economists’ disagreement on inflation focused on narrower possible areas of difference—such as divergent evaluations of the discount factor, and so of whether a demand restriction that produced short-run output costs was worthwhile—and largely took for granted the existence of a common analytical framework among U.S. economists.

The reason for Friedman’s apparent underappreciation of the swing back to cost-push views among major U.S. economists was that, in the second half of the 1970s it became unusual for him to attend research conferences. He was present at the 1977 Blanchard presentation at the Hoover Institution—at which, as it happened, Blanchard was accepting Friedman’s premises regarding inflation. But the circumstances of that occasion well illustrated the conditions under which he was most likely to be found at a research-related get-togethers after the mid-1970s. He was now predominantly only attending events if they were held at, or near, his Bay Area location.

Consequently, although the Hoover Institution’s head W. Glenn Campbell was certainly making a reasonable claim when, in March 1978, he described Friedman as “the nation’s leading monetary economist" (Hoover Institution, 1978, p. 12), Friedman was now seen infrequently at the kind of economic-research events that had established his reputation in monetary economics. His increased concentration on public policy distanced himself from research activity, and he now had both less time and less motivation to attend research conferences. In contrast to his jostling of public-policy and research engagements in the final decade or so of his years at the University of Chicago, from 1977 onward public-policy work would be Friedman’s main reason to undertake work-related travel.

There continued to be exceptions to this rule, of course, and, at the very end of 1977, an important instance of Friedman going out of town in order to attend an economic-research event was his going to the Ninetieth Annual Meeting of the American Economic Association, held in New York City. The occasion for his attendance, however, was a ceremony marking his past

research contributions. Specifically, he was the center of an AEA luncheon on December 29, 1977, honoring him as 1976's Nobel laureate in economics.[54] After that, it was rare for Friedman to be at the AEA annual meetings, even though he was a former president of the AEA. He was an attendee-presenter at the December 1983 and January 1996 meetings, but in both cases no travel on his part was required, as these were occasions when the AEA meetings were held in San Francisco.

The pattern of Friedman largely being absent from the research world was set when the AEA held its next, 91st, annual meeting, held in his former home city of Chicago in August 1978. Friedman, now deeply involved in making a television series, did not attend.

Two notable features of the 91st AEA meeting illustrated a pattern that would emerge when it came to how his name and work were recalled in economic writings in the era after he had largely left research.

One session showed how Friedman's past work was being built upon by others. The August 1978 AEA meeting session titled "Wages and Employment" consisted of papers by Blanchard, Guillermo Calvo, and John Taylor, with Taylor presenting "Staggered Wage Setting in a Macro Model" which would be published in the resulting issue of the *American Economic Review* (Taylor, 1979a) and that would use the staggered-nominal-contracts framework that he would lay out more fully in Taylor (1980). The session therefore was an early example of the New Keynesian work that would continue over the subsequent decades, including notable work by Calvo. As with Friedman's work, this approach would combine a focus on monetary policy's influence on output and inflation. It would provide a concrete Phillips curve, in the context of microeconomic foundations and rational expectations, that embedded the dynamics stressed in Friedman's and Phelps' work. And—in contrast to earlier rational expectations macroeconomic models but in common with Friedman and Phelps—it would provide for the existence of substantial short-run nonneutrality of monetary policy, with this nonneutrality resulting primarily from sticky (though, ultimately, fully adjustable) nominal goods prices.[55]

Another session at the 1978 AEA meetings had a paper that was another case of developing Friedman's work but was more overt in making a connection to his work: John H. Makin And

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[54] See American Economic Association (1977, p. 797) and Brunner and Meltzer (1993, p. 345). Friedman was also a discussant at one session of this AEA meeting, as discussed below.
[55] See American Economic Association (1978a, pp. 483–484). The research developing the New Keynesian Phillips curve is described in further detail in Chapters 11 and 18 below.

Maurice D. Levi, both of Seattle's University of Washington, had a paper on the session titled "Fisher, Phillips, Friedman, and the Measured Impact of Inflation on Interest" (American Economic Association, 1978a, p. 484). A related paper by the authors published in the *American Economic Review* at the end of the year (Levi and Makin, 1978) did not have Friedman's name in the title but recent work of his was cited: the Nobel lecture published in June 1977. The case of the Levi-Makin *AER* article illustrated a second feature of the manner in which Friedman would be cited after 1976: although he produced some new research, most notably *Monetary Trends* in 1982 with Schwartz, it would tend to be the older work that dominated his bibliographical citations. Most of the monetary work of Friedman's that would be cited consisted of pre-1970 contributions.[56] The Nobel lecture was the main exception: it would pick up numerous citations. It had already been cited in the *AER* earlier in 1978, with Katona (1978, p. 75), in a written version of remarks made at the December 1977 AEA meeting, having referred to propositions advanced by "Milton Friedman in his recent Nobel lecture." But, reflecting the reduced amount of material that Friedman put in research journals after 1975, and the usually muted impact of his post-1975 research, the Nobel lecture would be a late-dated example of there being really highly cited Friedman research.[57] He would continue to be cited profusely in economic research appearing from the late 1970s onward, but the bulk of those citations would be of pre-1975 work.

The tendency for Friedman's older writings to be those that received citation would prevail not only in macroeconomics but also in the areas of social policy when—as occurred increasingly from the 1970s onward—economic-research papers considered ideas that he had floated primarily in public-policy, rather than research, outlets: the negative income tax and education vouchers. For example, in another *American Economic Review* paper, Keeley, Robins, Spiegelman, and West (1978, p. 886), 1962's *Capitalism and Freedom* was cited as the basis for the authors' statement (p. 873), "Milton Friedman is usually credited with developing the concept of a negative income tax," and when the NBER issued a conference volume *The*

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[56] Interpretation of Friedman's citations is complicated by the fact that a non-macroeconomic article, his 1970 popular piece on the social responsibility of business, would actually become his most cited piece of writing. This outcome largely reflected the fact that the article generated interest across different social-science disciplines. In addition, it attracted the interest of many individuals who were at the intersection of research and the non-research private sector. See Chapter 4 below for a detailed discussion.

[57] In the monetary area, popular writings like the Friedmans' *Free To Choose* book, as well as later essentially popular work on monetary policy like Friedman's 1992 book *Money Mischief*, did ultimately cumulate considerable citations in the economic-research literature. Friedman's interventions in public debate on monetary policy also would occasionally be cited. For example, Craine, Havenner, and Berry (1978, p. 782) cited Friedman's Congressional testimony of November 6, 1975 (Committee on Banking, Housing and Urban Affairs, U.S. Senate (1975a), while many U.K. research studies would cite a 1980 submission that Friedman made to a House of Commons inquiry on monetary policy (Friedman, 1980a).

Economics of School Choice, Hoxby (2003, p. 1) cited Friedman's 1955 essay on vouchers when noting that "Milton Friedman is generally credited with spurring modern interest in school choice."[58] Hoxby also cited Milton and Rose Friedman's *Free To Choose* book in this connection. But it remained the case that it was Milton Friedman's initial work in the area—the 1955 piece—that tended to get cited. This was so despite the fact that he remained active, through public speaking, op-eds, and direct campaigning, on the subject of education vouchers right through to 2006.[59] Indeed, after 1977, and especially after 1992, Friedman had a much more vibrant and continuous degree of involvement in public-policy forums advocating vouchers than in research forums dealing with monetary analysis.

At the University of Chicago from 1977, a research forum in the monetary area that Friedman had created proceeded without him: the economics department's money-and-banking workshop that he had started in the early 1950s. In Friedman's later years running the workshop, it had already heavily featured rational expectations work. That situation continued after Robert Lucas took over the oversight of the workshop at the start of 1977. Consistent with Sheffrin's (1996, p. 25) observation that in light of the emphasis on expectations in the NRH and the role of the NRH in the 1970s rational expectations literature, "Any discussion of the rational expectations hypothesis in macroeconomics must therefore begin with the Phelps-Friedman 'natural-rate' hypothesis," the NRH figured heavily in the 1977 proceedings of the workshop (see Chapter 7 below). Indeed, Blanchard was one of those presenting such material in his workshop, with chapter 2 of his thesis, "Wage indexing in the short and long run," being given on February 2, 1977—just a few weeks into the workshop's post-Friedman era.[60] But the University of Chicago money workshop gradually moved away from themes that Friedman had emphasized and, in particular, its focus shifted to flexible-price models that left little role for a link between monetary policy and business cycles.

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[58] That is, Hoxby cited Friedman (1955d). Friedman's advocacy of school vouchers is discussed in Chapter 18 below.

[59] He had not made it an important part of his teaching or research. Thomas Simpson, a graduate student in the economics department in the middle and late 1960s, observed that "it wasn't as if, for example, when it came to vouchers that he would have a seminar on vouchers or some kind of opportunity for people to come and interact [with him] and discuss those. There were certain students who were attracted to Chicago because of Friedman and Friedman's so-called 'classical liberal' views or libertarian views. And they found occasions to stop by his office or somehow find the time and opportunity to talk to him about these kinds of things. But, you know, I don't really recall there being events that were open to everybody in the department, where we got a chance to sit around and talk about those kinds of things. I mean, at that point, *Capitalism and Freedom* had been published, and all of us, at one point or another, had our copies and had read through it, and were very much aware, and would talk about, a lot of the issues that were covered in that. But it wasn't from direct interaction with Friedman." (Thomas Simpson, interview, May 29, 2013.) Friedman's free-market activity at universities predominantly occurred elsewhere, as he would occasionally make trips off-campus to give talks to free-market student groups at other universities.

[60] The date of the workshop session was obtained from the records of the library of the University of Chicago.

As discussed below, work on monetary policy's real effects did proceed elsewhere. But Friedman was far from active in this ongoing dialogue.

One might get a contrary impression from the Hoover Institution's annual report for 1977 (which appeared in the spring of 1978). On the basis of its description, one might believe that Friedman had got back right into the money-workshop routine upon joining the institution. The report gave him as one of the organizers of a "year-long seminar in monetary economics with participants from around the country" held at the Hoover Institution in 1977/1978 (Hoover Institution, 1978, p. 45).

But the truth was that Friedman was hardly at these events, let alone organizing them. The year 1977/1978 was, as noted, one in which the Hoover Institution was trying to expand its macroeconomic coverage, and this effort was reflected in several monetary economists visiting on a temporary basis in the same period in which Friedman was starting. "The year Friedman [first] went to the Hoover Institution, it was sort of the macro year there, and Barro was there, I was there, Meltzer and Brunner," Michael Darby recalled (interview, October 15, 2013). Karl Brunner, Allan Meltzer, Robert Barro, and Darby—each of them visiting the institution for the year—were those ho really ran and regularly attended the monetary seminar that was held at the Hoover Institution in that year.[61] Friedman appeared occasionally at the sessions of this seminar. But he was so often away from the Hoover Institution over the course of the 1977/1978 academic year that Meltzer's recollection of his own time as a visitor was "Milton wasn't there" (Allan Meltzer, interview, April 21, 2013).

The Hoover Institution had foreshadowed that their new hire would continue his monetary research: "Milton Friedman, who was appointed a senior research fellow, will continue his research on monetary economic policy."[62] This was an accurate outline of his activities insofar as he and Schwartz were finishing *Monetary Trends.* But completing that book was essentially a writing-and-proofreading task undertaken in 1980 and 1981, once Friedman's television work was finished. When *Trends* was released, he did not set out to champion the book's results in speaking forums. The contrast was significant with how he behaved after co-producing major monetary research in the 1960s. Whereas, in the period surrounding the completion of the 1963 Friedman-Meiselman study of monetary versus fiscal policy and the 1963 Friedman-Schwartz studies of money and output, he had given seminars and conferences expounding and defending

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[61] The first three presentations in the workshop series were successively given by Robert Hall, Michael Darby, and Stanley Fischer (Hoover Institution, 1978, pp. 45–46).
[62] Hoover Institution (1977, p. 12).

the studies' findings, by the time *Monetary Trends* was finalized in 1981 Friedman was no longer doing this sort of activity. Over the 1980s, he was known to give, albeit infrequently, keynote speeches at prestigious research conferences. Primarily, however, from the late 1970s onward, his travel for talks was mostly in order to appear at non-research forums: remunerated speeches at events organized by business organizations and free-market institutions, or participation in the basically public-policy and philosophy-focused (and not research-oriented) meetings of the Mont Pelerin Society. The pattern seen in the 1960s of Friedman attending a large number of economic-research conferences was now gone.

In particular, although the National Bureau of Economic Research's practice of producing research conferences (and thence proceedings volumes) actually increased, Friedman very rarely participated in the conferences. A sign of this came in the organization's October 13−14, 1978 conference on rational expectations and economic policy, held in Melvin Village, New Hampshire (*NBER Reporter*, Winter 1978, p. 15). Among the attendees were three professors of economics who had been Ph.D. students at the University of Chicago during the Friedman years. One of them—Phillip Cagan—had, over 20 years earlier, had his dissertation supervised by Friedman. Another, William Poole, had not had Friedman as a Ph.D. adviser but attended the money workshop and would later refer to "[m]y teacher and mentor, Milton Friedman."[63] A third, Robert Lucas, had been a class member in Friedman's Price Theory course and, well after finishing his Ph.D., became closely associated with Friedman's body of work via his own fundamental developments of the NRH.[64] The October 1978 NBER conference also featured two contemporaries of Friedman who had participated in the Keynesian-monetarist debates: fellow monetarist Karl Brunner and longtime opposite number Paul Samuelson. But Friedman himself did not attend.

Subsequently, Friedman attended a total of four NBER conferences in the 1980s. One—barely a research conference, as the commissioned papers served as background for the main discussion by figures prominent in public policy—was held in Florida at the very start of the 1980s, and his participation was made possible by the fact that its timing dovetailed with Milton and Rose Friedman's East Coast publicity tour for the release of the *Free To Choose* book and television series. Another Friedman participation was as a lunch speaker at an NBER conference, held in May 1988 in Washington, D.C., that marked the fiftieth anniversary of the founding of the

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[63] From Poole's remarks of January 26, 2011, in Committee on Financial Services, U.S. House of Representatives (2011, p. 23).
[64] See Nelson (2020b, Chapter 15) for a full discussion of Friedman/Lucas connections in the 1960s and 1970s. Some further material on this subject is provided in Chapter 16 below.

Conference on Research in Income and Wealth.[65]

The remaining two Friedman-attended NBER events were more fully-fledged research conferences, and Friedman's participation in both of them reflected his sentiment: "Anna's a wonderful woman, isn't she?"[66] Anna Schwartz's co-running of a spring 1982 NBER conference on the Gold Standard's historical performance prompted him to attend as a discussant, while in October 1987 he attended, and spoke at, an NBER conference held in New York City specifically honoring Schwartz.

The tendency for the NBER's many macroeconomic conferences held on the East Coast location was certainly a deterrent to Friedman's participation. But it need not have been decisive: until his heart attack in September 1984, Friedman did tend to visit the East Coast quite frequently. His non-participation in NBER events was a reflection of the winding down of his research activity and his heavier post-1976 orientation toward public-policy activity.

In the period after he left the University of Chicago, Friedman also had occasion to alienate fellow researchers, particularly those of the younger generation when he suggested that the federal government's research-sponsoring body, the National Science Foundation (NSF), be discontinued. "I personally happen to think there is no justification for the National Science Foundation," he remarked in early 1977, while adding that he had never asked for a grant from the NSF.[67] When the issue of NSF funding of economic research heated up as a national issue in 1981, Friedman not only supported cuts to research funding but also took the opportunity to criticize the rise of mathematical work in economics. Although he had a current job title that included "research" in it—and was constantly being described as a professor in the media—Friedman was expressing a position on funding of economic studies that was at variance with that of most in the economic-research world—a situation underlined when Robert Lucas spoke out publicly on the opposite side of Friedman on the matter.

The incident regarding funding helped reinforce the sense of a severing of ties between Friedman and more active economic researchers. In 1981, the judgment that Friedman was now largely out of the research world undoubtedly underlay the decision by Martin Feldstein, head of the NBER, to relieve Friedman of his longtime position as one of the Bureau's research fellows—a

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[65] See Friedman (1991c).
[66] Milton Friedman, interview, January 22, 1992.
[67] Friedman (1978b, p. 5).

departure that, although an employer initiative, was publicly presented as a retirement.[68]

Separate from Stanford University's economics scene

Although it received some news coverage when it occurred, the fact of Friedman's move to the West Coast took a long time to sink in with much of the general public and even with the economics profession. The association of Friedman with the University of Chicago continued, with Wonnacott and Wonnacott's (1979) undergraduate textbook referring (p. 279) to "Milton Friedman of Chicago." The perception was reinforced when a good deal of the *Free To Choose* series was made on the University of Chicago's campus.[69]

From early 1980 onward, when his regular television work was largely completed, Friedman was less often away from his new location of the Bay Area. As it happened, however, his activities were dissimilar to those in his University of Chicago years. Indeed, aside from his use of an on-campus office, they were frequently not very university-oriented at all. Although in principle Friedman had now more opportunity to do so than had been the case during 1977−1980, Friedman did not really take up roots in the economic-research scene of Stanford University and Greater San Francisco. He had already given up teaching, so there was no connection to the university through that line of work. And Friedman's additional move away from university-related activity—namely, his exit, in considerable measure, from research circles—was confirmed by his post-1980, post-*Free To Choose* choice of activities.

At the end of the 1970s, he did develop a habit—which he maintained for over a quarter of a century—of attending the Federal Reserve Bank of San Francisco's major research conferences on domestic macroeconomics, and in the 1980s and 1990s Friedman also made numerous appearances at the Western Economic Association's annual meetings, which were often held conveniently near his San Francisco home.[70] Friedman also, despite his departure as an affiliate,

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[68] On the events described in this and the previous paragraph in the text, see Chapter 11 below.
[69] See Chapters 10 and 11.
[70] Friedman had a largely honorific title of president of the Western Economic Association (WEA) in 1985 and produced a presidential address for it, far more discursive and public-policy oriented than his 1967 AEA address. See Chapter 14. Earlier, on July 4, 1981, Friedman had given a more substantive lecture (albeit one mainly addressed to current issues) to the annual WEA meetings, held in San Francisco. Part of the reason why this talk eventually saw print as a journal article was that Friedman was not required to produce a full written draft and was able to speak extemporaneously. William Dewald recalled (interview, April 25, 2013): "He'd already moved to San Francisco at that time, so I agreed to have the *Journal of Money, Credit and Banking* lecture presented at the Western Economic Association meetings in San Francisco, since he was there. I wanted to make it easy on him, so I hired a stenotypist…. Friedman presents this lecture, without [complete] notes, at the Western Economic Association meetings, and I have every word recorded by the stenotypist. We get it typed up, and I send it to him,

sporadically appeared at the NBER's Bay Area meetings from the early 1980s to the mid-2000s.[71]

Tellingly, however, he did not become engaged in the economics world of the university with which he was now affiliated, Stanford University.

As head of the Hoover Institution, W. Glenn Campbell wrote publicly of "the Institution's many interrelationships with Stanford University" (Hoover Institution, 1977, p. 12). Privately, however, W Glenn Campbell was unhappy that the university was reluctant to agree to joint appointments at the Hoover Institution and at individual Stanford University departments (Campbell, 2000, pp. 9–10). This situation was beginning to change substantially in the late 1970s, as noted above.[72] But Friedman was not part of this movement, and he did not want to be. Although Campbell (2000, p. 10) apparently interpreted Friedman's lack of post-1976 teaching as reflecting a veto by the economics-teaching bodies of Stanford University, in fact the last thing he wanted was to teach regular classes. Friedman also did not take part in the economics schools' departmental seminars at Stanford University.

In fact, when he was in New York City for the December 1977 AEA meeting and served as a discussant at a session on negative income tax, he likely had more interaction with, and exposure to the work of, non-Hoover Institution Stanford University economists, than he typically did in the periods when he was present on the university's campus.[73] Seven of these economists were authors of papers presented at the session, which was held on December 27, 1977.[74]

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and essentially, he made virtually no changes in what he had spoken… [H]e was such an effective oral communicator, beyond that of any other economist that I have ever encountered. He could say what was in his mind clearly and understandably and effectively… [T]he published *Journal of Money, Credit and Banking* lecture that Milton Friedman gave… was essentially word-for-word as it was spoken." Friedman did make some changes: Having been furnished with the transcribed version, he produced a draft that included footnotes and references, and he circulated this version for comment before producing the final version (published as Friedman, 1982a).

[71] This was mostly simply as a participant rather than being part of the program, although he was a discussant at one event in 1983, discussing a paper by Robert Lucas (see Chapter 4), and he submitted a paper to a 2000 NBER meeting, ultimately accepting the role of a lunchtime speaker for the event (see Chapter 18). One of the final occasions on which Friedman attended the various NBER events held in the Bay Area was his attendance of the meeting of the NBER Program on Economic Fluctuations and Growth, convened in San Francisco on February 6, 2004.

[72] See also the 1984 quotation from the head of Stanford University, Donald Kennedy, in Campbell (2000, p. 319).

[73] Friedman's participation in this session was one of many cases in which he continued to discuss the negative income tax (NIT) after the early 1970s. As discussed in Nelson (2020b, Chapter 13), these cases contradict suggestions that he abandoned the NIT idea in the early 1970s. Indeed, he was advocating NIT a quarter-century after this period (*Barron's*, August 24, 1998, p. 30).

[74] See American Economic Association (1977, p. 789). The AEA listing for this session gave Friedman's affiliation as "University of Chicago and Hoover Institution." The correct affiliation, of course, would have been "University of Chicago and Hoover Institution, Stanford University."

Friedman's Hahn problem

The case of Frank Hahn provides a prime illustration of Friedman's tendency to absent himself after 1976 from many of the ongoing debates on matters concerning monetarism in the research literature—and particularly of Friedman's lack of engagement in economic-debate forums taking place on the Stanford University campus after he formally became affiliated with the university.

In the 1970s, Hahn was an unusual case of an active, prominent U.K.-based economist who wrote on aggregate output and price fluctuations and who was routinely cited on these matters by researchers in the United Kingdom and the United States alike. In this connection, it is notable that, among the new U.S.-based economists emerging in the late 1970s, Olivier Blanchard was one of those citing Hahn. A paper that Olivier Blanchard produced (but in the event did not publish), several months after his appearance at the Hoover Institution, was titled "The Solution of Linear Difference Models Under Rational Expectations: Its Application to the Hahn Problem."[75]

The "Hahn problem" discussed in the title of Blanchard's paper referred to work by Hahn—a 1966 article on the dynamic stability of models that had multiple financial assets—that Friedman likely knew next to nothing about.[76] Despite his own occasional statements that he would himself write a book on capital theory, Friedman was not a participant in the large theoretical debate on capital, even though a number of his Keynesian opponents were. Friedman hardly ever referred to Hahn in his published research, and so he was certainly not a contributor to the body of work to which Dasgupta and Heal (1979, p. 254) referred when they observed: "The literature built around Hahn's [1966] article is simply huge."[77]

Nevertheless, the title of Blanchard's paper, if Friedman ever came across it, would have struck a chord, thanks to its reference to "the Hahn problem." In the 1970s, Friedman had a Hahn problem, in much the same way as he had a Tobin problem, a Samuelson problem, and a Heller problem. Hahn—who, as discussed below, had a semi-regular link with Stanford University's economics scene—had become one of the various prominent economists in academia arrayed

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[75] See Blanchard (1978).
[76] See Hahn (1966) and Burmeister (1980, pp. 7, 266). As it happened, Blanchard's (1978) introduction linked Hahn's result to work that Friedman knew very well: the Cagan (1956) study of hyperinflation that was Friedman-supervised work and that was published in a volume that Friedman edited. Blanchard's basis for connecting the analysis of inflation in Cagan (1956) and the Hahn problem as advanced in Hahn (1966) was that both were concerned with the existence of dynamic stability (that is, convergence to a steady state).
[77] Confusingly, another contribution by Hahn, this one more solidly in the field of monetary economics, later *also* came to receive the label of the "Hahn problem." See Walsh (1998, p. 41).

against monetarism. Although he produced much general-equilibrium work (essentially in the same line of the 1960s research that Blanchard had cited) that had nothing directly to do with the debates in which Friedman was engaged, Hahn in the 1970s and 1980s also wrote much material—some highly technical, some intended to be accessible to those outside the general-equilibrium field—that was more clearly targeted against monetarism.

Friedman had described himself in mid-1970 as "blessed with critics" in the economics profession as a result of his monetarist work.[78] Shortly after Friedman made this characterization, Frank Hahn became an additional, and prominent, critic of monetarism. Hahn had written negatively about Friedman's monetary work in book reviews appearing in the 1950s.[79] But this criticism had not been followed by direct exchanges between the two of them in print in the 1950s and the 1960s. Hahn (who in 1960 began a long affiliation with Cambridge University) and Friedman maintained friendly relations. The scope for conflict between them was restricted by the fact that Hahn—being a general-equilibrium theorist—worked in more abstract and mathematically focused areas than Friedman typically did.[80]

A turning point in their relations came in 1971 when Hahn published an article-length review of Friedman's book *The Optimum Quantity of Money and Other Essays*. Friedman was likely not too unsettled by Hahn's criticisms of the opening chapter of the book that had formed the basis of the book's title. Despite the deflation rule derived in "The Optimum Quantity of Money," neither in that article, nor subsequently, did Friedman advocate deflation as a policy prescription.[81] But, as the title of Hahn's (1971) review, "Professor Friedman's Views on

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[78] Friedman (1970h, p. 326).
[79] See Hahn (1954, 1958).
[80] Friedman had not worked in the general-equilibrium area of mathematical economics that was Hahn's specialty. Nevertheless, as someone who was involved in more mathematical economics (of a type that overlapped with statistics—not a Hahn area) early in his career—in the 1930s and 1940s—Friedman occasionally displayed some proprietorial feelings about how such methods, as well as statistics and econometrics, should be used in economic research. One manifestation of these feelings was his apparent plan to produce a treatise that laid out how to use mathematics in economics. Gary Becker observed (interview, December 13, 2013): "In fact, people used to report that Friedman was thinking of writing a book on the mathematics of economics, though he never mentioned that to me." Another example was when Friedman objected (at a session of his money workshop on January 28, 1975) to the use of the term "marginal significance level" (which John Geweke had used in his presentation) on the grounds that the term "marginal" already had a well-established technical usage in economics as referring to a partial derivative (John Geweke, personal communication, October 30, 2014).
[81] In particular, it continued to be the case that Friedman opposed steady-state deflation as a practical policy prescription. This was despite the fact that, particularly from the 1980s onward, researchers often presented his 1969 theoretical paper on the benefits to household liquidity of steady-state deflation in terms that implied that Friedman actually advocated that policy—one prominent example being Lucas and Stokey's (1983, p. 82) statement, "This is the conclusion Friedman (1969[a]) reached." Furthermore, a policy of deflation along these lines would often tend to be referred to as the "Friedman rule." When Meltzer (1983, p. 25) mentioned "Milton Friedman's monetary rule," most readers at the time would have correctly taken Meltzer to be talking about the constant-

Money," indicated, the piece also questioned the quality of the other chapters in the Friedman book—which had included several of his major solo- or coauthored articles in 1952–1968 advancing monetarism.

At the personal level, Friedman's reaction included removing Hahn from those to whom the Friedmans sent Christmas cards each year. At the professional level, Hahn's reaction to monetarism essentially put him in the same camp—against Milton Friedman, particularly on matters related to inflation and its cure—as other senior figures at Cambridge University. On the subject of Friedman, therefore, Hahn was four square with figures with whom he had great differences on other economic matters. Mervyn King recalled (August 16, 2016): "He represented what we would now call the neoclassical group at Cambridge, mathematical economics, general equilibrium theory." As Hahn's work with Kenneth Arrow—and his resulting connection to Stanford University discussed below—attested, Hahn was often associated with mainstream U.S. economics rather than U.K. Keynesianism. Kenneth Arrow

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monetary-growth, price-stability-seeking, rule. In other work that started appearing around 1983, however, the term "Friedman rule" came to be used repeatedly in reference to the deflation rule studied in Friedman (1969a). The upshot is that the nine-line index entry on "Friedman rule" in Walsh (1998, p. 514) was entirely in reference to the policy derived in the 1969 paper.

As already indicated, however, Friedman did not advocate a policy of deflation, and he did not recommend it for the United States in his 1969 paper. Indeed, Bryant (1980, p. 251) listed Friedman (1969a) as one of the places in which Friedman affirmed his support for a constant-monetary-growth rule designed to deliver price stability, accurately citing pages 47–48 of the 1969 paper in this connection.

The term "Friedman's zero interest rate rule" (Ghironi. 2003) is something of an improvement on "Friedman rule" when referring to the policy that Friedman derived in 1969. Although this label is certainly capable of carrying the incorrect imputation that Friedman advocated the rule, it at least has the advantage of recognizing that the default meaning of the term "Friedman rule" should be as a synonym for the constant-monetary-growth rule, not the deflation rule.

The fact that Friedman opposed deflation and eschewed the deflation rule that he had analyzed in his 1969 paper was stressed in Nelson (2020a, Chapter 8) and is discussed further in Chapters 3 and 18 of this book. It is true that there was a degree of ambivalence in Friedman's other discussions of deflation. This ambivalence reflected the fact he saw deflation as having a mixture of implications for the economy—some adverse, some not. One reason for this position was the expected-deflation/unexpected-deflation distinction. Romer and Romer (2013) argue that Friedman and Schwartz (1963a) implicitly regarded expected deflation as being something that hurt the cyclical behavior of output, on account of the upward pressure that expected deflation places on real interest rates. And, certainly, there is support in Friedman and Schwartz's writings for Romer and Romer's interpretation (see Nelson, 2020a, Chapter 2). In contrast, in the case of unexpected deflation or a one-time price-level decline, there is some scope for stimulation of the economy, as real interest rates are not raised and may actually decline, while conversely the purchasing power of nominal money balances is increased. On a related point, Friedman and Schwartz (1982, p. 403) stressed that, provided that nominal income could be taken to be unchanged by the price decline, real output would be stimulated by a percentage amount roughly equal to that decline. (Of course, other means of distinguishing between "good" and "bad" deflation" are possible: see, in particular, Bordo, Lane, and Redish, 2009. See also Kaufmann, 2020, who suggests that the U.K. and U.S. empirical evidence heavily favors the judgment that deflation is costly.)

Another reason for Friedman's ambivalence in discussing the implications of deflation was the short-run/long-run distinction. Although he portrayed deflation as potentially disruptive for the business cycle, he had confidence in the economy's capacity to adjust in the long run (that is, return to potential output) in the face of steady, low-single-digit deflation.

recalled (interview, December 7, 2013): "Look, I'm sure he would tell you he was a Keynesian when it came to policy. But, on the other hand, he certainly had very difficult times with the Old Guard—especially Joan Robinson, Richard Kahn—that regarded his interest in, you know, neoclassical economics as abhorrent."[82] By the early 1970s—during which Hahn returned to Cambridge University, having been at the London School of Economics from 1967 to 1972 (Blaug and Sturges, 1982, p. 150)—Hahn was very much on the same side as these Cambridge University economists, from Friedman's viewpoint—owing to their all disliking monetarism (and, furthermore, having a lot of common ground on why they disliked it).[83]

The tendency for Hahn to have considerable solidarity with Keynesianism, particularly the U.K. variety, was magnified by his dislike of the U.S. rational expectations work of the 1970s. In contrast to Hahn's highly abstract work, much of this literature obtained concrete dynamic models out of general equilibrium analysis. And the results that emerged from these analyses were often favorable to Friedman's views on monetary policy and inflation. But Hahn's reaction was not to see Friedman's monetary positions as more compatible with general equilibrium than he had previously thought. Rather, as Goodfriend (1984, p. 384) noted, in looking at the 1970s rational expectations literature, Hahn was "inclined to view that class of models as simply wrong." Crucially, Hahn objected to Lucas (1972)-type models not only on the grounds that they were flexible-price models but also on the grounds that they established that inflation was controllable through monetary policy.

"Frank Hahn and James Meade were in the mainstream of economics, and it was the post-Keynesians at Cambridge who were not," Mervyn King observed. "... [But] Frank Hahn eventually came to be very anti-rational expectations and anti- much of the policies that emanated from what you might call the growing mainstream in the United States." (Mervyn King, interview, August 16, 2016.)

In the early 1980s, Hahn was a critic of monetarism in U.K. public debate and research forums alike. During this period, Friedman did make some responses in the U.K. media to Hahn's

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[82] Although the focus of the present book is on Friedman and U.S. economic debate, there will be occasion to discuss some of Friedman's interactions with Cambridge University in Chapters 2, 4, and 9.
[83] This contrasted with the so-called Cambridge-versus-Cambridge debate on capital theory, in which Hahn sided with the U.S. Cambridge (Massachusetts Institute of Technology) side of the debate. The aforementioned Hahn (1966) article appeared in an issue of the *Quarterly Journal of Economics* that opened with a symposium specifically concerned with this debate, although it appeared separately from that issue's symposium. Some of the issues at stake in this debate were connected with the debates in which Friedman himself was involved: see Chapter 9 below.

criticism of monetarism.[84] But he showed no interest in rising to the challenge offered in Hahn's research pieces—even when the opportunity to do so arose at Stanford University. This fact, along with Friedman's disengagement from Stanford University's economics community, was brought out in Hahn's repeated visits to Stanford University on occasion during the first half of the 1980s, arising from the activities organized by Hahn's longtime associate and past coauthor, Kenneth Arrow.

Among these trips was one that Hahn made to Stanford University in the summer of 1980, with his visit coinciding with a multi-week session of the Institute for Mathematical Studies in the Social Science, for which Arrow was the economics director.[85] Hahn, a frequent participant in these sessions, presented a paper bearing the broad title "On Inflation" at a session workshop on August 21, 1980 (Hahn, 1981, p. 1). In contrast to the occasion in 1977 when Olivier Blanchard found that Friedman was unexpectedly an audience member at his presentation, Friedman was nowhere to be found at Hahn's workshop talk.[86]

No doubt, the fact that Hahn's criticisms were in the area of rarefied theory was a deterrent to Friedman becoming engaged. But it was also clear that Friedman by 1980 was simply not inclined to engage in debate in journals or research forums with critics of monetarism as he had been through 1972. The bar that had to be reached before he responded to academic critiques of his work, already high, had been raised. This disinclination covered both the realm of print and in-person events, even those for which it would be quite easy for Friedman to attend. It was becoming evident that he had no interest in interposing himself into debates on macroeconomic and monetary matters at events at Stanford University that were outside his home turf of the Hoover Institution.[87]

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[84] One of these exchanges is discussed in Nelson (2020a, Chapter 7). An extended discussion is planned for inclusion in the author's projected study of Friedman and U.K. economic debate.

[85] Kenneth Arrow recalled (December 7, 2013) that at Stanford University in the 1980s there was regularly "a summer session... [of] the Institute for Mathematical Studies in the Social Sciences—[specifically] an economics wing to that, which I directed for a while. Mordecai Kurz [subsequently] directed it. And for many years—it would have been through [the funding of] the NSF—we had summer sessions, for which there were a number of people who came regularly from other countries. It [the economics wing of the institute] was one of the most active of all of them."

[86] From 1981 onward, Friedman's absence was made still more likely by the fact that the Friedmans' annual routine was to spend much of their summer in their second home in California's Sea Ranch, about 2½ hours' drive from the Stanford metropolitan area.

[87] As discussed in Chapter 14 below, Friedman in the 1980s took the same position that he had voiced in Friedman (1970h, p. 326): so many research papers were written challenging his work on monetarism that he did not have the time to answer them all—and so had adopted a default position of answering none. As discussed in later chapters, he in practice deviated from this rule on several occasions. It is true, however, that critiques of Friedman's work were plentiful in the research literature in the 1970s and 1980s and that Friedman usually did not attempt to produce rebuttals. The number of published critiques is an understatement of how many critiques appeared, as some did not

Consequently, Hahn's August 1980 presentation, like so many critiques of Friedman's views on inflation, was one that took place in Friedman's absence. In the written version, which was subsequently released in the mathematical institute's working-paper series, Hahn opened by speaking of a whole set of writings on inflation: "Reading them is only marginally instructive and rarely a pleasure."[88] The writings in question were not identified, but Hahn removed doubt about who was his main target by immediately used two direct quotations of Friedman. "They have announced that inflation is 'always and everywhere a monetary phenomenon' and that an increasing quantity of money is a 'necessary and sufficient' cause of inflation. The first proposition, if not banal, is hard to interpret[,] while the second seems unproven."[89]

Notwithstanding the weight that Hahn carried in U.K. and U.S. economics circles, his critique of monetarism made little impact—and, in particular, did not prevent the Friedman position on inflation from prevailing in monetary analysis. "Hahn has done some of the best work in monetary theory," Marvin Goodfriend (a researcher at the Federal Reserve Bank of Richmond) wrote when one item in the large early-1980s outpouring of Hahn writings critical of monetarism appeared. But, Goodfriend added, Hahn lacked the "empirical, historical, or institutional material that is helpful for studying monetary economies in general."[90]

Goodfriend noted that, in addition to criticizing monetarism, Hahn was trying to offer an alternative to the flexible-price rational expectations models of the type associated with Robert Lucas. In the event, much practical U.S. macroeconomics did judge against those models. But the resulting literature—New Keynesian economics—did not embrace the Hahn alternative but, instead, a sticky-price rational expectations framework. In particular, whereas, in the middle 1970s, there had been a swing back among practically oriented U.S. economists toward nonmonetary views on inflation, in the 1980s what Mervyn King in the above quotation called "the growing mainstream in the United States" experienced a period of consolidation: as already indicated, inflation was seen as a monetary phenomenon, while the natural rate hypothesis

reach print (and so, in this pre-electronic age, largely disappeared). For example, Clower (1971, p. 24) cited as a 1970 Purdue University mimeographed paper a study by Patrick Hendershott and George Horwich, "The Monetary-Interest Rate Mechanism and Its Policy Implications: A Critique of Milton Friedman." The study was never published. Similarly, in 1984 the working-paper series of the economics department of London's Queen Mary College included Drobny and Wriglesworth (1984) whose title indicated that the study was a "critique of Milton Friedman's natural rate hypothesis." The paper never appeared in print.

[88] Hahn (1981, p. 1).

[89] Hahn (1981, p. 1). Hahn (1981) was apparently never published, although an article of the same title appeared as Hahn (1990).

[90] Goodfriend (1984, pp. 381, 384).

(another idea that Hahn had criticized) embedded in the same consensus.[91]

The ground won by Friedman among economists would have come home to Hahn in the late 1980s, when he was involved, as coeditor, in putting together the two-volume *Handbook of Monetary Economics*. The chapter on inflation in the handbook was by Bennett McCallum, who wrote (1990, p. 965): "it would be useful to address at the outset… how much validity should be assigned to Milton Friedman's famous dictum that 'Inflation is always and everywhere a monetary phenomenon'… That particular statement has been strongly disputed by leading economists, including one of the editors of this *Handbook*... I would suggest, nevertheless, that there is in fact little professional disagreement with Friedman's position…"

Hahn's approach had been to isolate special cases in which theoretical models delivered inflation in the absence of monetary expansion as his basis for refuting the proposition that such expansion was necessary for inflation. This was an approach unlikely to impress Friedman, who eschewed the concentration on exceptional theoretical possibilities as the basis for arriving at propositions.[92] Paralleling Friedman's attitude, McCallum (1990, pp. 965–966) defended the view of inflation as a monetary phenomenon. McCallum put it forward as having been intended as a proposition about the *empirical regularities* associated with inflations, and he rejected the notion that *theoretical counterexamples* should be grounds for rejecting the proposition, as these might amount to unrepresentative special cases.[93]

McCallum further made the case that inflation was a monetary phenomenon by using what Hahn (1981, p. 1) had indicated, in broad terms, should be the basis for the discussion: "a model of a

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[91] Hahn was critical both of Phillips-curve analysis, including its augmented version (Hahn, 1981, p. 1; 1990, pp. 21–23) and of the concept of the natural rate of unemployment (see the discussion of his criticism of this concept in Nelson, 2020a, Chapter 9). "Principally, Hahn's point was that the economy is not self-equilibrating in general. And that would be extended, probably, to the natural rate [hypothesis]," his longtime colleague Robert Neild observed (November 6, 2013). In a discussion that had implied that Hahn's criticism of U.S. macroeconomics may have been overstated, Gale (1986, p. 91) nevertheless apparently supported Hahn against Friedman, referring to "Milton Friedman's [1968b] ill-judged remark about the natural level of unemployment being 'ground out by the Walrasian system of general-equilibrium equations.'" As discussed in Nelson (2020b, Chapter 9), however, modern dynamic stochastic general equilibrium analysis has provided support for Friedman's notion that a Walrasian system (defined concretely as the flexible-price version of the model) can generate a well-defined natural rate of unemployment.

[92] See Nelson (2020a, Chapter 9).

[93] In the course of this discussion, McCallum (1990, p. 965) noted that it was likely common ground among Friedman and his critics that his proposition that inflation was always and everywhere a phenomenon had not been advanced simply as a statement of the truism that inflation corresponded to a decline in the purchasing power of money. Friedman certainly did not intend the proposition to correspond to this truism. Some critics (including Hahn, as indicated in the above quotation from Hahn, 1981), however, professed not to know whether Friedman meant the proposition as something other than the truism. See Nelson (2020a, Chapter 7) for a further discussion.

monetary economy based properly on the choice of agents"—that is, a dynamic general equilibrium model included money. In particular, McCallum used the Sidrauski (1967a) model. This model came out of dissertation work by a University of Chicago economics student in the mid-1960s. After Sidrauski's premature death, the model had been developed further and become the baseline setup used in general-equilibrium monetary analysis.[94] McCallum showed that, in this model, "in a steady state… the inflation rate will equal the (per capita) money growth rate" (p. 970): that is, there was a fixed, one-for-one relationship between the steady-state growth rates of nominal money and the price level.[95] McCallum stressed that this continued to be so if the model was expanded to include trend growth in output or the presence of factors, such as payments technology, that would put a trend in velocity.[96]

On this basis, McCallum argued that Friedman's proposition about inflation as a monetary phenomenon was a consensus position in the economics profession, rather than one associated with one side of a debate. Friedman, too, not long afterward expressed a certain amount of satisfaction with the state of thinking about inflation in "American monetary economics" (letter to the author, November 12, 1991).

The monetary policy debates of the 1980s in U.S. research circles

Had the debate been settled in the manner that Friedman and McCallum indicated? A broader look at developments in mainstream U.S. macroeconomics suggests that it indeed had. Friedman was likely not ideally situated to assess developments in American monetary economics, in light of the distance he had from research circles by the early 1990s. On the surface, it might also appear that there could be grounds for discounting McCallum's characterization of the U.S. economics profession as having accepted Friedman's views on inflation. McCallum was not

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[94] A key part of the development of the Sidrauski model was by Brock (1974).

[95] As McCallum made explicit in a later application of this model (1990, p. 978), the relationship between monetary growth and price-level increases extended to the case of deflation. This was, in effect, a demonstration of the point (stressed in Nelson, 2020a, pp. 277–278) that Friedman's proposition about inflation extended to negative inflation rates. As Buiter (2003, p. 55) correctly noted, Friedman's stance was that "inflation (and by implication deflation) is always and everywhere a monetary phenomenon." Friedman (1987a, p. 18) explicitly noted: "Deflation can be prevented if and only if the quantity of money per unit of output can be kept from decreasing appreciably." As indicated above, he also believed that deflation *should* be prevented. He was confident that it *would* be prevented: "Central banks have learnt that the way to avoid deflation is to print money." (*Sunday Times* (London), September 8, 2002.)

[96] The Sidrauski model, as adjusted to a discrete-time setting in such work as Brock (1974), was infinite horizon. Because of this feature, and because it therefore embedded a more standard role for money than that of overlapping-generation models, it became a more standard baseline general-equilibrium model for monetary analysis than the kind of model used by Lucas (1972).

situated in an economics department in the "Top 20" American universities.[97] In addition, McCallum often described himself as a semi-monetarist, and so he could be expected to be sympathetic to the Friedman perspective on inflation. Yet the fact was that McCallum (1990) was indeed portraying the modern consensus on inflation accurately.[98] In the 1960s and into the 1970s, leading economics departments on the East Coast of the United States had been the center of Keynesian academic opposition to Friedman's monetary work.[99] But in the 1980s, the prevalent attitude among the younger generation of the members of these departments, notably those in the emerging New Keynesian movement, was one that accepted key elements of Friedman's monetary analysis—notably including its coverage of inflation's causes and dynamics.

This reality was clouded by the fact that Friedman himself was under siege for a protracted period in the 1980s, having to account for errors that he had made in predicting inflation. In debates in the mid-1980s that he mainly participated in via media and non-journal outlets, Friedman was on the defensive about monetarism: the velocity of M1 (the monetary aggregate on which Friedman was putting the most emphasis in this period) exhibited a sharp break in trend; Friedman erroneously predicted an inflation breakout on the basis of high growth in M1; and the Federal Reserve correspondingly proved to be wise in discounting the signals provided by M1 by mostly deemphasizing M1 in policymaking after 1982. In the media, the juxtaposition of M1 velocity breaking from its long-run trends, Friedman's prediction errors, and the Federal Reserve's reduced focus on monetary targets formed the basis for declaring the "death" or "demise" of monetarism.

But, in the 1980s and into the start of the 1990s, these debates on monetary aggregates' current empirical status did not concern most of the economists who, in effect, constituted the next generation (born in the 1940s and 1950s—about 30, 40, or more years after Friedman and his contemporaries) of monetary economists at major universities. To be sure, one of these researchers, Friedman's part-namesake (though no relation to him), Benjamin Friedman, of

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[97] He was instead a professor at a business school at a university (Carnegie Mellon University) that was predominantly classified being outside the "Top 20." The business school did not formally have a self-contained economics department, although it had professors of economics among its senior staff membership.
[98] McCallum, in particular, did not focus on degenerate special cases, to which Hahn had attached importance. See also Grossman (1984) for a critical perspective on Hahn's focus on such cases.
[99] Nelson (2020b) covered debates that Friedman had with various U.S. Keynesians through 1972, while this book will further discuss Friedman's debates and interactions with Franco Modigliani of MIT (Chapter 8), James Duesenberry and Otto Eckstein of Harvard University (Chapters 8 and 13, respectively), as well as the evolution of his debates with James Tobin. Chapters 3 and 11 will discuss occasions during the 1970s and 1980s on which Charles Kindleberger of MIT criticized Friedman on international matters.

Harvard University, was a prominent exception: he concentrated on the empirical arguments against monetary aggregates, and he highlighted Milton Friedman's serious overpredictions of mid-1980s U.S. inflation rates.[100] Referring to current research, Benjamin Friedman deplored the fact that "much work in this area… represents actual monetary policy outcomes by the growth rate of the money stock."[101] But the preoccupation of the emerging New Keynesian literature being produced in that decade was, instead, primarily the analytical one of constructing a rational expectations model suitable for the analysis of stabilization policy. At this point in the evolution of the New Keynesian literature, monetary aggregates had a prominent role and were coming out favorably.

Specifically, the policy-oriented part of the theoretical literature on stabilization policy in the United States in the 1980s overwhelmingly accepted monetary aggregates as a measure of monetary policy, both by postulating that the central bank's policy instrument was the money stock and by specifying an economic structure in which there were close links between (real) spending and (real) money. Indeed, anticipating this trend, Olivier Blanchard's dissertation (1977, p. 9) specified real aggregate demand as depending on real money balances, with the two series having a close bivariate relationship, disturbed only by a serially correlated exogenous shock. Blanchard noted that such a simple specification of aggregate demand behavior allowed the analysis to "concentrate… on the aggregate supply curve," and the New Keynesian literature of the 1980s took a similar perspective.[102] The choice of specification was therefore, in part, a pragmatic one. The fact, however, that this literature defaulted to a simple relationship between money and spending contrasted with the skepticism about this relationship in Keynesian critiques of Friedman in the 1960s.

And the New Keynesian literature's agreement with Friedman went beyond the aggregate

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[100] See Chapter 13.
[101] B.M. Friedman (1991, p. 82). In this particular discussion, Benjamin Friedman went on to argue (p. 82) that to "rely on the growth of some monetary aggregate to measure the stance of monetary policy" was at cross purposes with the likelihood that it was no longer the case that "the Federal Reserve places much emphasis on money growth targets in planning and carrying out U.S. monetary policy." This specific criticism (unlike other empirical criticisms of money that Benjamin Friedman advanced) was of questionable validity. After all, the original development of the monetarist literature had made the case that monetary aggregates had merit as a measure of policy stance even in periods when actual policy disregarded such aggregates.

In order to put things in perspective, it should be stressed that the notion that if a variable *X* is not used as a policy instrument or target by policymakers, *X* is *ipso facto* an invalid indicator of policy stance is a common, if flawed, argument. For example, Friedman's former student Robert Laurent (1995, p. 3) argued that "the monetary base is not likely to be a good indicator of policy" on the grounds that the Federal Reserve had not used the base as an instrument. Valid arguments can be advanced for regarding the base as an invalid policy indicator, but what Laurent cited was not really one of them.
[102] The quotation is again from Blanchard (1977, p. 9).

demand side. On the supply side, this literature, in common with Friedman, specified a Phillips curve that was vertical in the long run but was, on account of the presence of nominal rigidity, nonvertical in the short run. This was a contrast with the flexible-price rational expectations, or New Classical, literature of the 1970s. That literature had a Phillips curve that was nonvertical only momentarily: unanticipated monetary policy actions were the only monetary source of output variations, and the associated deviations of output from its natural level were highly transitory. That literature was dissipating in the early 1980s but was soon succeeded by the even harder-line version of flexible-price models: real business cycle (RBC) models, in which monetary policy actions did not matter at all for the behavior of output. The sticky-price rational expectations models developed in the late 1970s were developed in the New Keynesian literature and used as the basis for taking the fight in the 1980s to the RBC literature and making the case that, in fact, monetary policy mattered heavily for output variations in the short run.

As stressed above, the notion that a long-run vertical Phillips curve could be blended with a short-run nonvertical Phillips curve and rationalized on the basis of nominal rigidity lined up with Friedman's own championing of the natural rate hypothesis. But in the 1980s the argument for this type of specification of the Phillips curve was mainly being made by New Keynesian economists at U.S. East Coast universities. Macroeconomists at the University of Chicago now tended to be strongly associated with flexible-price models. Most notably, although he was consistent across the 1970s and 1980s in advocating the assumption of flexible prices, Robert Lucas had seemingly undergone a change in viewpoint. He had moved away from his 1970s position that there were important, if short-lived, effects of monetary policy on output (as reflected in the monetary-surprise models that he advanced during the 1970s) and toward associating himself with the RBC models of the 1980s. In particular, Lucas (1987) had seemed to accept the premise that the U.S. postwar business cycle was well described by the RBC model, and he regarded important connections between money and output as mainly prevailing in notable, but now-long-past, episodes like the Great Depression.

Reservations about the direction in which University of Chicago macroeconomics had gone in the post-Friedman era were openly expressed by Phillip Cagan in the mid-1980s. Cagan (1986b, p. 32) observed that the University of Chicago economics world of the 1950s—in which he had been a graduate student and, later, an assistant professor—was known for its tendency to apply to practical problems the assumption of frictionless perfect competition to a far greater extent than was typical in much economic analysis taking place outside the university. But, he insisted, the 1950s-era University of Chicago analysts also knew when not to apply it—with one area in

which it was not applicable being that of money and the business cycle.[103] Cagan called for a return of "price inflexibility and long lags." He added reproachfully: "These are not Keynesian concoctions… They were an accepted view before Keynes. Not all older views are Keynesian."[104]

At the University of Chicago itself, however, advocacy of nominal rigidity had become uncommon. But a rare proponent of those rigidities—and a critic of RBC models on grounds similar to those articulated by New Keynesians and by older monetarists—was Michael Mussa, an economist at the business school. Although he had many areas of common interest with colleagues at the university, Mussa in his studies of macroeconomic and financial phenomena expounded positions that were associated with Friedman but that were little heard at the University of Chicago after 1976: a belief that the assumption of rational expectations should be pursued without also postulating full price flexibility; a firm view that short-run price stickiness should not be dismissed as amounting to an assumption that private-sector agents had money illusion; and an assessment that monetary policy exerted powerful effects on the business cycle. Richard Clarida, who worked with Mussa when both joined the Reagan Administration in 1986, recalled Mussa's reaction to the RBC research: "'Well, you know Rich, there's some pretty good stuff in there, but you just can't take real business-cycle models seriously,' he said, "because after all, if we really believed in real business-cycle models, we shouldn't call it the Great Depression. We should call it the Great Vacation." (Richard Clarida, interview, February 10, 2023.) In contrast to Lucas, who, as indicated above, was coming to view the Friedman-Schwartz account of the cycle as valid for depressions but not for milder U.S. economic downturns, Mussa (1994, p. 85) affirmed the importance of Federal Reserve policy in the generation of all post-Korean War recessions, while also suggesting that monetary policy had been a key influence on the course of the economy during postwar economic expansions.[105]

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[103] Cagan was making this criticism in the context of discussing 1970s-style New Classical models. He subsequently registered a similar judgment, however, about the real business cycle literature. "This real business cycle theory," he suggested, was "sort of a complete turnaround" when compared with Keynesian criticisms of monetarism—by accepting fully monetarism's account of inflation's sources but rejecting any role for money in cyclical output fluctuations. "So there's a little flurry of activity going on there, among a small group. But that'll eventually pass away. Because it's just a group around Rochester and Minneapolis that hold that view. And some of the things that they say—that there could be important technological changes in the production function which you want to take account of—are O.K. But this extreme view that prices adjust right away is all right in order to understand how the economy works in a theoretical way. But it doesn't really help to understand business cycles." (Phillip Cagan, interview, January 13, 1992.)

[104] Cagan (1986, p. 32). Cagan was not alone in stressing that the assumption of sticky prices was common ground among Keynes and earlier writers. On this matter, Blanchard and Fischer (1989a, p. 372) observed that, although "the sticky wage and price assumption is now identified mainly with Keynesians," it was also the case that the assumption "can be found in the writings of most analysts of the aggregate economy, certainly in Hume (1752) and more recently in Irving Fisher and Milton Friedman."

[105] Further discussion of Mussa's monetary analysis appears in Chapter 7.

The direction of public debate on monetary policy

The debate on RBC ideas, although heated, was largely confined to U.S. research forums. Among more practically-minded academic monetary economists in the 1980s, there was solid support for sticky-price models—a situation reflected in Ferguson and Hart's (1985, p. 1133) observation that work using these models "continues to dominate analysis of macroeconomic fluctuations." Indeed, the more that one concentrated one's attention on practical and policy-oriented economic analyses in the 1980s, the greater one saw the support among economists for flexible-price models thin out. Correspondingly, the main academic macroeconomists who went to serve in the Reagan Administration in the second half of the 1980s included Mussa, Friedman's former student Michael Darby and monetarist Allan Meltzer—all of them critics of RBC and other flexible-price models.

Likewise, in public debate on stabilization policy in the 1980s, the RBC work and other flexible-price rational expectations models were absent. The applied policy work that academics and others brought to this debate was overwhelmingly based on a presumption of sticky prices. Against this background, Tobin (1986, p. 351) observed: "Today there is a big gulf between academic macroeconomics and the macroeconomics oriented to contemporary events and policies." Proceeding from this observation, Tobin suggested that the likes "of old-fashioned IS/LM and of structural econometric models built in its spirit" were the tools prevalent in policy-oriented analysis, including that done by economists working for the U.S. Congress, and he suggested that this was a basis for "hope for the future of Keynesian economics," in both academia and policymaking.[106]

These Tobin remarks, however, should be interpreted in light of Phillip Cagan's observation of the same vintage, quoted above, that not all pre-1970s economic ideas about output and price dynamics corresponded to Keynesian ideas. Tobin's 1986 remarks implied that in the 1980s Keynesian economics of the kind that he had argued for in past decades was what had prevailed in the practical analysis of economic policy. But this implication was only partially accurate. The implicit or explicit models being used in practical policy analysis in the 1980s did eschew the flexible-price thinking of the RBC approach or of the earlier New Classical/monetary-surprises literature. But, as will now be discussed briefly, these models also tended to incorporate ideas that had at one time been considered un-Keynesian and anti-Keynesian—and

[106] Tobin (1986, p. 351).

that Friedman had propounded in the 1950s and 1960s.[107]

For one thing, the 1980s policy-oriented models to which Tobin referred included, outside the IS/LM segment of those models, a specification of the Phillips curve that (like the emerging New Keynesian literature in academia) was based on the expectations-augmented, long-run vertical variant of the Phillips curve.[108] Even Tobin by the end of the 1970s had appeared to accept this variant as the appropriate one (see Chapter 8 below). During the 1980s and 1990s, he showed signs of withdrawing this concession—and of returning to a belief in a permanently-nonvertical curve.[109] Among other economists, however, the post-1979 acceptance of the natural rate hypothesis was far more categorical. Data outcomes in the 1980s and 1990s in the United States largely added to the evidence accumulated over the 1970s in favor of that hypothesis. In particular, the notions that the removal of inflation required a temporary negative gap and not a permanent one, and that, once inflation was beaten, a noninflationary price stability could ensue, had foundation in the Friedman-Phelps Phillips curve and were inconsistent with the two alternatives that had been: a pure cost-push view of inflation and the old-fashioned, long-run nonvertical Phillips curve. Friedman had in the 1960s offered 1961−1965 as an example of a noninflationary recovery that had been able to proceed against a background of low inflation expectations.[110] The U.S. economic expansion of 1982−1990, following the early-1980s

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[107] More extended discussions of this point appear in Chapters 14 and 18 below.

[108] Correspondingly, it was this version of the Phillips curve that Cagan (1986c, p. 269) referred to as "the standard Phillips curve."

[109] See, for example, Tobin (1991, p. 348). Tobin here was referring to hysteresis theories of unemployment. A discussion of whether these theories should be considered a refutation of the natural rate hypothesis appears in Chapter 14 below. In addition, Tobin (1994, p. 152) argued that stopping the 1979−1982 disinflation short of reaching full price stability had given the U.S. economy "trillions of dollars of GNP—yes, real GNP."

[110] See Nelson (2020a, Chapter 10; 2020b, Chapter 1). Failure to recognize that Friedman's monetarist framework is consistent with a variety of patterns of behavior of inflation in recovery conditions was a flaw in the analysis of Steindl (2004). Steindl erroneously associated with Friedman the position that the "normal course" of economic recovery would have prices rising (p. 114) and incorrectly claimed that it was a quantity-theory position that prices always rose in response to monetary expansion (p. 53).

In a similarly misconceived vein, Steindl (2004, pp. 52−53) singled out the course of the U.S. price level and of output in the period from May 1938 to August 1940 period as inconsistent with Friedman's position on price-and-output dynamics. This was a period, Steindl suggested, in which prices mostly fell while money rose—supposedly a "seemingly perverse" situation (p. 53). He alleged that Friedman and Schwartz "do not even comment on" the price behavior in the period (p. 112) and that they may not have even noticed the episode despite the associated movement in the price data being displayed in their plots (Steindl, 2004, p. 53). In making this allegation, Steindl seemingly overlooked the fact that the *Monetary History* covered the 1939−1941 period both in Chapter 8 (on which Steindl focused) and in Chapter 9. The latter chapter specifically granted that U.S. prices in aggregate did not start rising until August 1940 onward (Friedman and Schwartz, 1963a, pp. 551−552). Friedman and Schwartz explained the initial lack of inflation after the breakout of world war by using Phillips-curve reasoning (p. 550): "Because of the large absolute amount of unemployment and unused industrial capacity, wholesale prices at first remained stable, starting to rise only in the fall of 1940." Nonseasonally adjusted monthly CPI data (available at *https://fred.stlouisfed.org/series/CPIAUCNS*) support Friedman and Schwartz's characterization of the 1939−1940 period as price stability (rather than falling prices), as the price level was largely unchanged, on net: the declines in

disinflation, would provide a further example.[111] The Friedman-Phelps Phillips curve turned out to be very applicable to the 1980s disinflation/recovery episode.[112] Its success on this score was already being remarked on in the mid-1980s by David Lindsey, an economist at the Federal Reserve Board who was heavily involved on the staff side of the monetary policy process. Lindsey remarked that "the familiar Milton Friedman (1968[b])-Edmund Phelps... process fits the data on postwar U.S. aggregate prices and wages very well."[113]

And in the IS-LM portion, or aggregate demand specification, of policy-oriented models to which Tobin made explicit reference, some version of Friedman's permanent income hypothesis was typically incorporated.[114] Indeed, if anything, something like the infinite-horizon version of this hypothesis gained ground over the 1980s. This version implied a sharper distinction of the permanent income hypothesis both from old-style consumption functions and from the life-cycle consumption function.[115]

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the CPI were concentrated in the period from October 1939 to January 1940, and they only partly offset the preceding shift upward (which had occurred in September 1939). Friedman did happen to comment on this decline when he observed: "my recollection is that all commodity prices almost fell immediately on the outbreak of war in September 1939" (Instructional Dynamics Economics Cassette Tape 151, August 7, 1974). He interpreted the commodity price decline as resulting from a desire to become liquid at the outbreak of war. The notion underlying this interpretation—that a sharp movement in basic commodity prices had an effect on the short-run behavior of inflation but that aggregate economic conditions guided the long-run behavior of inflation—would also mark Friedman's interpretation of the first oil shock in 1973−1974. See Chapter 3 below.

[111] Early in this expansion, Friedman's capacity to appreciate its similarity to the noninflationary expansion of the early 1960s, and to see it as a validation of his views on Phillips-curve dynamics, were obscured by his erroneous assessment in 1983−1984 that the Federal Reserve had already eased excessively—and that the economy would very shortly overshoot potential output again and experience revived inflation. Chapter 13 discusses in detail Friedman's contemporaneous errors in evaluating monetary and economic developments in the mid-1980s.

[112] In contrast, Otto Eckstein—in common with many of those who saw inflation largely in cost-push terms—had a baseline view of inflation behavior in which the change in, not the level of, the output gap was the primary demand variable driving inflation. According to this view, the disinflation associated with recessions tended to be temporary. Along these lines, Eckstein (1983, p. 11) wrote of the early-1980s U.S. disinflation: "Actual wage increases fell a lot, but much of this reduction was due to the substantial rise in unemployment, and [it] will be partly lost as the labor market returns to normal."

[113] Lindsey (1986, p. 197). See also Stockman and Glassman (1987) and the more recent references already cited. On the Federal Reserve's endorsement and usage of expectations-augmented Phillips curve ideas in the Paul Volcker era and subsequently, see also Chapters 10, 13, and 18 below.

[114] Blinder (1981c, p. 95) argued that, as the permanent income hypothesis and the expectations-augmented Phillips curve incorporated into Keynesian models by 1979, these ideas should now be considered Keynesian. As matters transpired, some better clarity in terminology was established in the 1980s by the practice of referring to models that incorporated these ideas (as well as rational expectations and optimizing behavior) as New Keynesian.

[115] The forward-looking version of the permanent income hypothesis implies a sharp distinction between the permanent-income and life-cycle hypotheses, as this version of the permanent income hypothesis is inconsistent with the notion that making consumption depend on permanent income simply amounts to adding lagged income to a static consumption equation. Such a notion was based on a literal interpretation of Friedman's (1957a) empirical (adaptive-expectations-based) implementation of the permanent income hypothesis in time-series work. The notion was stressed in 1970s discussions that minimized the significance of the PIH (see Nelson, 2020a, p. 203) as well as some later discussions that endeavored to suggest that the permanent-income and life-cycle versions of the consumption function were observationally equivalent (see Modigliani and Sterling 1986; and the references in

The forward-looking version, in particular, implied the result, associated with the Ricardian equivalence hypothesis, that government borrowing through the issue of securities to the private sector tended not to raise real interest rates. Friedman was himself a strong proponent of the notion that budget deficits raised real rates, as well as the related idea of crowding out, but changed his view in the late 1970s and lined up more closely with the Ricardian characterization of household behavior.[116] The conviction that budget deficits and public debt raised real interest rates did have formidable strength in public discourse, as evidenced by its influence over the economic debates associated with the presidential election campaigns from 1984 through 1992 and on the budget-deficit reduction measures enacted under various administrations after 1981.[117] At an empirical level, however, the evidence of the 1980s and 1990s tended to suggest only a weak influence of public debt on real interest rates. This was consistent with limiting cases of the permanent income hypothesis but was inconsistent with the emphasis on strong crowding-out effects of the Reagan-era budget deficits, as prominently advanced by Tobin and other key Keynesians and by leading financial-market economist Henry Kaufman.

What was called "the Kaufman phenomenon" (*Financial Times* (London), October 22, 1981) highlighted a way in which the work of Friedman and other monetarists had influenced the terms of practical debate on stabilization policy. It was clear in the 1980s that debate on fiscal policy—exemplified in financial markets and the media by Henry Kaufman's widely read analyses—centered on budget deficits' effect on real interest rates, not on their effect on economic activity. An element common to financial market commentary and U.S. academic macroeconomics was that, with respect to aggregate demand policy—specifically, the matter of what instrument should be regarded as setting the course of nominal spending and the short-run behavior of real spending—the main focus was on monetary policy.

Talk of fiscal policy's role as an active economic stabilizer—so prominent in both U.S. government-produced analyses and outside discussions during the 1960s—dissipated. The outcome of the debate among researchers had already moved in the direction of monetary policy triumphing over fiscal policy, reflected in Phillip Cagan observed in the early 1970s that "monetary policy is recognized and treated as a major determinant of economic activity" (*The*

Chao, 2003, p. 101). It is stressed in Nelson (2020a, Chapter 5) that Friedman did not regard adaptive expectations as an inherent part of the permanent income hypothesis.

[116] See Chapter 7 below. See also Nelson (2020b, Chapter 13).

[117] In this vein, and referring primarily to the decades of the 1980s and 1990s, Blinder (2002, p. 393) noted that the proposition that "fiscal deficits raise long-term interest rates" was something that "seemingly all policymakers and market participants believe," and he implied that this belief pertained to a crowding-out channel that operated on real interest rates.

Commercial and Financial Chronicle (New York), May 6, 1971, p. 1), while in mid-decade he noted the contrast with earlier periods "when fiscal measures monopolized discussions of macro-policy" and judged the debate between monetary and fiscal policy saw "monetarism today is relatively triumphant," the remaining matter how much to rank it above "dethroned fiscal policy" as an influence on aggregate demand (Cagan, 1976, p. 318). In the policy world, however, the reflex action of the Ford Administration in 1975 in response to recession was to secure the introduction of a tax rebate. Nevertheless, with a lag, the unfavorable judgment on the merits of countercyclical fiscal policy moved from research to the policy world over the following six years.

An early setback for the presumption in favor of fiscal measures was reflected in a decision by President Carter's decision, applauded by Friedman, in April 1977: the withdrawal of a proposed tax rebate, which the administration had previously advanced as a stimulus measure (see Chapter 8). Although the president did secure subsequent fiscal-stimulus measures in 1977 and 1978, an economic recovery program that he proposed in response to the 1980 recession was never legislated.[118] And, as the Friedmans stressed (see Chapter 14), although a stimulus bill did emerge in response to the 1981/1982 recession and had President Reagan's support, its dollar amount was small. The debate on monetary policy versus fiscal policy had faded significantly, in favor of the former.

In this environment, Kaufman's esteem stemmed in good part from his reputation as one of the nation's leading "Fed watchers." Although he inevitably had to provide comments at a high frequency on unfolding monetary, economic, and financial events, Kaufman was known for the amount of study underlying his analyses: "when he says something, there is a lot of research behind it," an academic admirer of Kaufman's—Burton G. Malkiel, of Yale University's business school—observed.[119] Notably, Kaufman's focus on scrutinizing Federal Reserve-issued data, documents, and statements as a means of gleaning monetary policy stance and policymakers' thinking had a certain lineage with the work with historical materials that Friedman and Schwartz had done in their past study of the Federal Reserve. Friedman and

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[118] See Chapter 11 on the later Carter period.

[119] *New York Times*, January 6, 1982. These remarks appeared in a newspaper profile of Kaufman's that, like many others, noted the scope for his comments to produce moves in financial market prices—making him, in the words of the article, "something approaching a cult figure and a walking news event." In the same article, Kaufman was quoted saying, "I never known what statements will have [a] market impact and which ones will not." This observation paralleled Friedman's perspective on the impact on economics of his own research: "I think it's almost impossible to predict what [writings] will be influential… It's an accident what happens to get picked up and what doesn't." (In Taylor, 2001, p. 329.) Chapter 12 below discusses Friedman's and Kaufman's perspectives on one another.

Schwartz were writing at a time when interest in the Federal Reserve and ratings of its effects on the economy were much lower than they were in the 1980s. Partly as a result of their work, by the 1980s monetary policy's effects were rated as high. One effect of this in financial markets was that Fed watching became something of a mini-profession—with Kaufman having a long spell occupying the premier spot in it. The advent of professional Fed watching did not altogether meet with Friedman's approval, as it reflected the fact that policymakers retained judgmental freedom over policymaking and were not subject to an automatic rule. But the prevalence of Fed watching was testament to markets' agreement with his contention that, of the tools available to the government, those associated with monetary policy were the most decisive in shaping the course of aggregate demand and inflation over time.

In academia, too, the Friedman-Schwartz tradition of analyzing Federal Reserve written materials received a major fillip in the late 1980s with the appearance of Romer and Romer (1989). Introducing the issue of *NBER Macroeconomics Annual* in which this article appeared, Blanchard and Fischer (1989b, p. 1) noted that "David and Christina Romer reexamine and extend Friedman and Schwartz's evidence on the relation between money and output" and that in doing so they were "deal[ing] with perennial issues in macroeconomics." At the close of the 1980s, therefore, real effects of monetary policy at the business-cycle frequency continued to be one of the perennial research issues, notwithstanding the zero-ing out of these effects in the RBC literature that had emerged during the decade.

Blanchard and Friedman

To return to the event—the 1977 presentation at the Hoover Institution—discussed at the outset of this chapter: After being initially thunderstruck at seeing Friedman in the audience, Olivier Blanchard did regain his rhythm, and his talk proceeded. "After the talk, Milton came to me, and, very kindly, reassured me that I still had a future," Blanchard recalled (personal communication, June 2, 2014). Indeed, he did: twenty-seven years later, Blanchard was associate editor of the *Journal of Economic Perspectives* and was thanked by Friedman in print for comments on the preliminary version of Friedman's article in the Fall 2005 issue of the journal. The published piece—another study of the perennial issue of the money/output relation—proved to be the final journal article that Friedman ever produced.[120]

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[120] See Friedman (2005a). Well after Friedman's death, a notable connection between Blanchard's thinking and Friedman's with regard to how, as an empirical matter, to interpret the implications for U.S. fiscal policy and monetary policy of the government's intertemporal budget constraint. See Chapter 12 below.

II. 2004

That 2005 article was part of a late-in-life spurt in Friedman's research efforts. It was something of a break with his typical behavior over the prior two decades, in which his focus had continued to be very much on public-policy interventions rather than research. Friedman had confirmed publicly in 1986 that he had consciously moved away from research—"people's capacities to do scientific work decline with age" (*Los Angeles Times*, December 14, 1986, p. 14). Consistent with this posture, Friedman's book *Money Mischief* in 1992, although containing some reworking of material that he had published in research outlets, had been a popular book.

As well as that book and the previous books with Rose Friedman, speeches, and other public events, Friedman's public-policy activity included many media contributions. And in light of the gradual drying up of his contributions to journals and other economic-research outlets, it was largely via the media that Friedman's own debates on macroeconomics with colleagues in the profession continued. In a poignant indication of his transition from a research focus to a media focus, Friedman's final direct exchange in the research literature with his most prominent Keynesian opponent, James Tobin—that is, consisting of contributions of both of them—appeared in print in 1976.[121] In April 1977, Friedman was back to debating Tobin—but on television, in a discussion on a news program of appropriate policy against inflation (see Chapter 8).

A longer-standing case in which Friedman's debates with academic Keynesians primarily took place in the public square rather than research outlets pertained to Paul Samuelson. Samuelson had had little direct debate with Friedman in research forums, but he had long been Friedman's opposite number in alternating *Newsweek* columns as well as panels. The *Newsweek* connection ended when Samuelson left his column in 1981, although the two did have some rematches in television debates in the late 1980s and early 1990s.[122]

In addition, Friedman's career was notable for the degree to which he debated not only economists, like Samuelson and Tobin, who were close to his own age but also many who were of different generations. Friedman's move toward concentrating on public policy increased this tendency. The next-generation economists were researchers whom Friedman was unlikely to encounter in journals, as they were active in article publishing and he was not. But some of these

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[121] See Friedman (1976a) and Tobin (1976), as well as the discussion in Chapter 2.
[122] On these later exchanges, see Nelson (2013) and Chapter 17 below.

economists, having started to publish in journals in the 1970s, were becoming involved in public policy debates in the 1980s. In the course of this, some of them wound up debating Friedman. Princeton University's Alan Blinder had remarked of Friedman, "I don't think of him as an adversary because he is so much older than I am."[123] The contrast in ages was undeniable: Blinder, born in 1945, was 36 when he made this remark in July 1982 while Friedman was about to turn 70. But Blinder, who followed Friedman and Samuelson into the vocation of producing newspaper op-eds and magazine columns, eventually did confront Friedman directly in a public-policy setting, as the two had a debate on fiscal policy held at University of California, Davis, on October 5, 1984.[124]

Twenty years later, in October 2004, one of the very last debates between Friedman and a next-generation economist took place when he appeared on San Francisco television alongside the University of California, Berkeley's Alan J. Auerbach (born in 1951).[125] By 2004, Auerbach had been located in the Bay Area for ten years. But, reflecting Friedman's only occasional participation in local research events, "the only time we had a substantive interaction" was when both appeared, on opposite sides, on local public television in 2004 (Alan Auerbach, interview, May 18, 2015).

Auerbach elaborated (interview, May 18, 2015): "It was during the 2004 election campaign. And I was working in the Kerry campaign at the time. And [public-television station] KQED had a series of live panels—really, debates—on a variety of different issues." The first of these, to be broadcast live on October 16, would concern the U.S. economy and the economic policies of the rival presidential campaigns—those of Democratic party nominee John Kerry and the Republican party's George W. Bush, who was running for reelection.

The format was planned as consisting of an economist making the case for each candidate,

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[123] Klamer (1983, p. 160). In these remarks, Blinder indicated that, as far as economic research was concerned, he believed that there was a higher priority on challenging New Classical economics than on challenging monetarism (though he also indicated was opposed to both). By the early 1980s, as the contents of Tobin (1980) and Tobin's remarks in Klamer (1983, pp. 106–112) made clear, this was James Tobin's priority, too. Nevertheless, Tobin's writings in the 1980s and 1990s contained abundant criticism of Milton Friedman, including of Friedman's views on monetary and fiscal policy. They also criticized Friedman for championing the NRH—a concept to which, as noted, above Tobin did have a lasting reconciliation. One of the final Tobin critiques of the NRH appeared in a book on the hypothesis that Friedman did not contribute to but which he read. In the Friedmans' memoirs, he referred to Tobin's (1995) contribution to this book, in which Tobin suggested that policymaker embrace of the NRH had lastingly worsened real performance in the United States, but in which he also stressed protracted economic weakness in Japan and Western Europe, "all because of the dreadful influence of my article!," Friedman summarized sarcastically (Friedman and Friedman, 1998, p. 231).

[124] See Chapter 14 below.

[125] Auerbach's year of birth appeared in American Economic Association (1981, p. 46).

alongside two other non-academic commentators on local and national economic issues. In the weeks approaching the broadcast, the program's makers found that there was a shortage of Bay Area-located academic economists who were willing to defend Bush and the Republican side on television. Auerbach recalled that he was consequently in contact with the program's makers: "there was a producer trying to put the program together and frantically trying to populate it. And I agreed, I think at the outset, to do it. And he was looking for somebody from the Bush camp. And I remember I suggested a couple of people who were involved in Republican circles, you know, down at the Hoover Institution—I think Mike Boskin and John Cogan. And he was not able to get anybody—he was sort of frantic that they weren't going to be able to pull it off. And he called me back maybe a day or two before [the broadcast]. And I assume this was, you know—as is the case on these sorts of things—probably a relatively young person doing the job as the producer—[because] he called me back and said, 'Well, we got this guy Friedman.' And he seemed very much to feel that it was, you know, not nearly as good as the people he'd tried before, but at least they had found somebody who would do it! (*laughter*) Which I guess was a sobering thought—that our reputation's fleeting." (Alan Auerbach, interview, May 18, 2015.)

Administrations from the 1970s to the 2000s

At the start of the 1970s, a *New York Times* profile (January 25, 1970, p. 80) observed: "Milton Friedman has inevitably been considered a 'right-wing' economist, an impression seemingly confirmed by his association with [Senator Barry] Goldwater in 1964." A decade later, another newspaper article noted "his enemies, and there are many of them, … accuse Friedman of ruthless, soulless, 'only money matters' philosophies" (*Daily Mail* (London), March 18, 1980). "State pensions, health plans for the poor, compulsory schooling: in the name of liberty, Friedman would abolish them all," said a caption appearing alongside a photograph of Friedman in a newspaper interview, whose title referred to him as a "demon king" (*Independent on Sunday* (London), July 26, 1992).

That Friedman wanted to scale back the role of government drastically was not in doubt. But, as critics of Friedman from a libertarian perspective were quick to point out, his was not a no-government solution. Furthermore, Friedman's proposals did not really correspond to the list attributed to him in the 1992 piece. His plans involved a minimum income for every member of the population—and so did not really entail outright abolition of government-provided retirement income. Relatedly, he saw the negative income tax as the means of delivering funds for health provision to the poor. And his favorable attitude toward the principle of making schooling voluntary went alongside the practical policy prescription—backed up, as already noted, by

much policy activism through 2006 on his and Rose Friedman's part—that education should be freely and universally provided, via school vouchers.

Though he certainly failed the test of being a pure libertarian, Friedman certainly did want to go further in reducing government spending than any major national Republican party candidate suggested. Nevertheless, he did publicly support successive Republican presidential candidates. "I'm a libertarian in philosophy, but, as I [often] say, I'm a libertarian with a small 'ℓ' and a Republican with a capital '*R*.'' (CSPAN, November 20, 1994, p. 14 of transcript.) By 2004, Friedman's support for the Republican side had included public endorsements of successive candidates over the prior forty years and also occasions on which he had a formal relationship with the campaign—most actively in the 1964 Goldwater campaign, with smaller but recurring advisory roles in the 1968 Nixon and 1980 Reagan campaigns.

On prominent social issues, however, Friedman broke sharply from the position taken in successive Republican party platform positions. "I remember there was a situation where Mrs. Reagan, in particular, tried to get Professor Friedman to come out against abortion. And that was a no-go, an absolute no-go," Friedman's long-time assistant, Gloria Valentine recalled (interview, May 7, 2013.)[126] And on an issue that Nancy Reagan focused on while she was First Lady—narcotic drugs—Friedman was, when it came to whether usage by adults should be legal, taking a stand opposite to that made by Nancy Reagan and by a whole succession of Republican candidates and presidents. In October 1969, he had testified that he thought it would be a good idea for Congress to resolve not to legislate against the generation of products for which consumers had a demand but that he was not recommending, at this hearing, that heroin production be legal.[127] But he soon did so—advocating drug legalization in a 1970 speech and a 1972 *Newsweek* column. Although Friedman continued to press this case in the Reagan years, the national attention on his advocacy of drug legalization crested in the George H.W. Bush presidency. Indeed, the only reference to Friedman that ever occurred in a national network presidential debate was a question to Bush and then-Governor Bill Clinton that a reporter made in one of the 1992 debates, and Friedman was mentioned because of his position on drugs.[128]

Friedman's thinking on policy issues also deviated from strict partisan lines because of his

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[126] With regard to Rose Friedman, Gloria Valentine added that she was "practically standing at the phone when Mrs. Reagan was talking to him—practically signaling don't you say yes. They both agreed on that. That's why I said it was a no-go." (Gloria Valentine, interview, May 7, 2013.) Friedman also made public his opposition to a prohibition on abortion (see Chapter 16 below).
[127] See Friedman's testimony of October 6, 1969, in Joint Economic Committee, U.S. Congress (1970a, p. 818).
[128] See Chapter 16.

conviction that trends in thinking about the role of government tended to influence both political parties.[129] Indeed, at the onset of the Democratic administration of Jimmy Carter, Friedman believed that the president might respond to the emerging mood in favor of limited government by reducing the role of the public sector in the economy. In the event, Friedman was disappointed on this score, with the exception of moves that Carter made toward deregulation of certain industries.

Indeed, from 1971 to 1981, the usual feeling that Friedman had about presidents' economics was disappointment, irrespective of who was in the White House. By the end of Richard Nixon's first term as president, he had already been disillusioned by the president's move toward wage/price controls and expansionary fiscal policy. In the second Nixon term and the Ford years, Friedman had good relations with and much agreement on the principles of domestic and international economic policy with Nixon's Secretary of the Treasury George Shultz, Shultz's successor William Simon, and Ford's head of the Council of Economic Advisers, CEA head Greenspan. But Friedman nevertheless experienced considerable disappointment with the direction of economic policy over 1973−1977. As already noted, the federal government's continuing espousal, in the late Nixon period and during the Ford years, of nonmonetary approaches to inflation, despite the demise of wage/price controls, was a sore spot. So, too, was a surge in the government-spending share of output in 1973−1974 that, Friedman believed, Ford showed little interest in winding back.[130]

After the first oil shock of 1973−1974, a lasting grievance on Friedman's part—one that he held against the Nixon, Ford, and Carter Administrations—was their unwillingness to embrace steps that allowed U.S. oil and gasoline prices to reflect the rise in world prices. Although Friedman's case for prompt price decontrol would receive wide support in retrospective accounts, at the time he and other proponents of decontrol, like Kenneth Arrow, were unable to generate a consensus in favor of this among major U.S. economists. During the 1970s, such economists as Paul Samuelson, William Nordhaus, and Nicholas Georgescu-Roegen were opposed to proposals to deregulate domestic oil prices, while another economist—former monetary analyst, James Schlesinger—proposed, as an energy official in the early Carter years, an elaborate system under which pre-tax price controls on oil would be made permanent. It was only in the second half of Carter's term that the administration moved to decontrol oil prices—in a move whose details

[129] This thinking was reinforced by his embrace of public-choice theory in the 1970s. Various aspects of Friedman's attitude toward public-choice theory are analyzed in Chapters 4, 6, 11, 13, and 14 below. See also Nelson (2020a, Chapter 9) for an evaluation that Friedman's application of the theory to monetary policy probably had adverse effects on the accuracy of his analysis.
[130] See Chapters 2 and 3.

Friedman criticized, in part because the decontrol program was phased, rather than instantaneous.[131]

Ronald Reagan quickly fully deregulated the U.S. oil price soon after becoming U.S. president in 1981. This action presented a clear-cut case of a Reagan outcome that Friedman could cite as an achievement. So too was the administration's tax-cut package passed by Congress later in 1981. But after initial enthusiasm about the Reagan economic program unveiled that year, Friedman was, over much of the remainder of Reagan's first term and into his second term, frequently critical of the administration's domestic record, and occasionally of the president himself, due to the lack of progress in reducing in the share of federal government outlays in U.S. output. "There is emerging a change in basic philosophy," Friedman had suggested in early 1977.[132] He saw the Reagan victory as an electoral confirmation of the philosophical change and expected that the change would be manifested under Reagan in a very sizable reduction in the government-expenditure share. In practice, it would emerge that the political consensus had moved much more decisively against high marginal income tax rates than it had against rising government spending—a situation reflected in Bordo and Landau's (1986, p. 100) decidedly ambivalent reference to "[t]he recent turn towards support for smaller government (if it has taken place)."

Friedman did in 1982 credit Reagan with having changed the terms of debate in such a way that major new domestic spending programs were not central to political discourse. And by the time of their 1984 book *Tyranny of the Status Quo*, he and Rose Friedman discerned the possibility of a slowdown in the rate of growth of federal spending under Reagan.[133] This slowdown became clearer in the post-1984 data, although the restraint in federal spending proved much more modest than what Friedman had hoped for when Reagan was elected in 1980.[134]

Reagan was succeeded in office in 1989 by his vice president George H.W. Bush. But Friedman before long judged that this appearance of continuity had not been matched by an adherence to Reagan-style economic policy. "Why did you once say that the worst mistake Ronald Reagan ever made was selecting George Bush as his vice presidential running mate?," Friedman would be asked in a television interview. "Because it was," Friedman replied simply (*The Charlie Rose Show*, PBS, December 26, 2005, p. 10 of transcript).

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[131] See Chapters 3, 6, 7, 9, and 10 for the evolution of the energy-crisis debate in the United States from 1973 to 1978.
[132] Friedman (1977f, p. 10)
[133] See Chapter 14.
[134] See Chapter 16.

Federal government "spending as a fraction of income didn't decline as much as he would have liked to see under Reagan, [but] at least it didn't increase[,] as it has under Bush," a summary of a January 1991 Friedman talk reported him as saying (Oppenheimer and Company, 1991, p. 8). "Government spending has been going up like mad under Bush," Friedman remarked three years into the president's single term (Trebach and Zeese, 1992, p. 73). As it turned out, he was underappreciating the extent to which a slowdown in the underlying growth of domestic federal spending—a slowing that began under Reagan—was continuing under Bush, particularly after 1990.

That tendency for growth in federal spending to be slow, not rapid, continued—despite Friedman's initial fears—under President Bill Clinton. Friedman was sparing in the personal credit that he gave to Clinton on what turned out to be an economic record of strong expansion in output and a declining federal-government spending share.[135] But, especially after Clinton's first two years in office, Friedman generally refrained from very sharp criticism of the president, and he seemed taken aback when, in the course of a joint interview that they gave in late 1998, Rose Friedman remarked: "Clinton, as governor of Arkansas, was for vouchers—until he became president. And now he's in bed with the teachers' unions—as well as everybody else." "That's a nasty crack!" was Milton Friedman's startled reaction (*The American Enterprise*, January/February 1999, p. 21).

The 2004 television program

The planned live broadcast went ahead on October 16, 2004, with the panel consisting of Friedman, Auerbach, *San Francisco Chronicle* business columnist David Lazarus, and Stephen Levy, head of a center concerned with the local economy. Unlike, perhaps, the program's producer, its host, Cynthia Gorney, was clearly aware of the novelty of having Friedman as part of the panel. After the panel had moved into a rhythm of go-round discussion of topics, Gorney veered into a question directed solely at Friedman. Gorney noted, and Friedman confirmed, that Friedman had no formal affiliation at all with the Bush campaign. But Gorney pressed the matter. He was an "extremely influential" economist, she observed. "How closely do you think your views may parallel those of people who are directly advising the president at this time?"

Friedman in past decades had been widely remarked on for his habit—appreciated by some, grating on others—of smiling broadly when delivering answers in interviews. In latter-day

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[135] See Chapter 18.

appearances such as in this 2004 television debate, however, it was rare for him to smile—a change that perhaps reflected an increased need on his part to concentrate when delivering an answer. Gorney's question, nevertheless, did prompt Friedman to smile, as well as generating an empathetic grin on Auerbach's part. "I'm not going to try to make any estimate on that," Friedman replied to Gorney, while exhibiting another trademark interview response—a shaking of his head. "You're putting me in an impossible situation." Friedman then immediately moved the subject of the discussion from himself to an economic topic previously covered by the other panelists. (*Election 2004: The Economy*, KQED, October 15, 2004.)

Productivity and the output gap

One of the central questions covered in the 2004 program was a central one about whether the U.S. economy was "in trouble," and, in particular, whether it was still in a protracted state of underemployment three years after the 2001 recession.

Friedman rejected the notion that the U.S. economy was currently soft. The other panelists, including Auerbach, cited the weakness registered in recent years in payroll employment data. Friedman cited the strength of the alternative, survey, data on employment, which had been stronger, and went on to declare: "we are in a period of unusual prosperity right now… We would be very fortunate to have this position continue indefinitely."

This evaluation of the current state of the economy, although at variance with the judgments of the other panelists, would be borne out. It would turn out that the quarter in which this discussion took place, 2004:Q4, was a period in which the negative output gap opened up by the 2001 recession was in the process of closing altogether. Retrospective estimates would put the U.S.s output gap, whose most recent trough had been −2.7 percent in the fourth quarter of 2002, was −0.2 percent in 2004:Q4, before moving to 0.3 percent in the first quarter of 2005.[136]

It was actually a matter on which Auerbach and Friedman agreed—productivity growth—that was a precarious part of their current assessment. Friedman remarked that the 1990s had seen "tremendous technological progress," and Auerbach observed, "we have continued to have very, very rapid growth in productivity."

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[136] See "100*(Real Gross Domestic Product-Real Potential Gross Domestic Product)/Real Potential Gross Domestic Product" (generated using Bureau of Economic Analysis data on quarterly real GDP and Congressional Budget Office estimates of potential output), available on the Federal Reserve Bank of St. Louis' FRED portal at *https://fred.stlouisfed.org/graph/?g=f1cZ*.

The prior decade had witnessed a notable contribution to a longer-term advance in technology that had occurred in Friedman's lifetime—an advance reflected in the remark he once made that, when he started college in 1928, television had been a futuristic dream (*Australian Business Monthly*, October 1993, p. 52).[137] In the decade after World War II, television had become a standard household product, while in more recent decades, desktop computers had become prevalent in both homes and workplaces. In 1991, Friedman had remarked on the accompanying change in the ordinary usage of words when he noted that when John Maynard Keynes deployed the word "computers" in 1939, it was the case that "Keynes meant human computers" (that is, research staff assigned to make computations by hand) and not the "electronic marvels" of the computer age.[138] Friedman had had the opportunity to access university-based computers in the mid-1940s but he had himself got into the habit of using "computers" to mean people, and then to mean handheld calculators, before moving more regularly into the modern usage of "computers" when he and Anna Schwartz had time-share access to computers in making regression estimates as part of their *Trends* project in the 1970s. He subsequently owned a home computer from the late 1970s and had one in his office, too.[139]

The spread of online technology in the 1990s was credited with a U.S. productivity pickup that started in mid-decade. It was this same higher trend rate of productivity that Auerbach and Friedman were referring to in their October 2004 discussion. But, in precisely this period, the benefits to U.S. productivity growth of the 1990s' culmination of the computer revolution seemed to dissipate. From late 2004, U.S. productivity grew at a much slower rate (Fernald, 2016).[140]

Government spending, taxes, and deficits

An undercurrent of Friedman's thinking was that the U.S. economy was capable of doing still better than it had since 1995 on the productivity-growth front. He believed that free-market-oriented policy reforms had the ability to raise the productivity growth rate associated with a given amount of technological knowledge as well as encourage the development of new technology. This was part of the reason why he had been supportive of the tax cuts seen in the Reagan years and the more recent tax cuts passed in 2001 and 2003 under President George W. Bush.

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[137] Of course, as of 1928, prototypes of television had been developed, but television broadcasts remained far off.
[138] Friedman (1991d, p. 36).
[139] See Chapter 18.
[140] For further discussion, see Chapter 18.

Friedman had, for a while, been seen as the leader, or at least the co-leader, of the national (albeit initially concentrated at state level) tax revolt of the second half of the 1970s that culminated in the 1981 Reagan tax cut. Well before the start of the Reagan years, however, he had been superseded as the face of the tax revolt by economists, such as Arthur Laffer, and politicians, like Jack Kemp, who were more firmly associated with support for tax cuts, and particularly with a belief in productivity-boosting and tax-revenue-enhancing effects of tax cuts.[141] These supply-siders had substantial agreement with Friedman on general fiscal policy prescriptions but had major disagreement with him on the quantitative effects of tax cuts and on whether government spending reduction was a prime goal. Most of all, many supply-siders came to regard Friedman as an adversary because they tended to disagree with him so much on domestic and international monetary policy issues. To some extent, the feeling was mutual. Even Lawrence Kudlow—at various times a Reagan administration official, a financial market economist, and a television broadcaster—although he was a friend of Friedman's and had sometimes been described as a monetarist in the media, was regarded by Friedman with some wariness. Asked in 2001 if he would "care to choose between Larry Kudlow and Alan Greenspan" (who were offering different evaluations of current conditions), Friedman replied immediately: "[I] have no difficulty at all. I'll take Alan any time." (*Uncommon Knowledge*, September 25, 2001, p. 2 of transcript.)

This remark notwithstanding, frictions between Friedman and supply-siders had subsided considerably by the early 2000s, and they were in harmony in supporting the tax cuts passed under President George W. Bush in 2001 and 2003. Friedman's remarks on taxes in the October 2004 television appearance were largely ones that Arthur Laffer and other supply-siders could applaud. Friedman used the discussion of the Kerry campaign's proposal to remove some of the recent years' cuts in marginal income tax rates to question the very notion of making the tax system more graduated: "this is a demagogic move. Do we really want a society in which we say: If you get above a certain level, you mustn't work—you don't try anymore, because you're going to get a very high tax? Do we really want a society in which we say there's a ceiling on how much you can earn?… Look, the people who earn more than 200,000 dollars, some of them earn 'em for one year and drop earlier, that's not their lifetime earnings. They are the people in general who have been most productive, who have contributed most… It's not a disincentive to them alone, it's a disincentive to every working person. Everybody who is working would like to be in that 5 percent. And now you tell 'em: If you get into that 5 percent, we're going to whop you over the head."

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[141] Friedman's differences with these economists included the fact, although he welcomed tax cuts, he particularly stressed constitutional limits on government spending. See Chapters 5, 6, 9, 10, and 14 below.

Friedman continued: "I think most economists have agreed for a long time that it would be desirable to have a tax system which interfered *less* with all sorts of incentives. That it would be desirable to have a flat-rate tax system, with the same rate applied to everybody, except for those at the very bottom." (*Election 2004: The Economy*, KQED, October 15, 2004.)

To this, Gorney correctly conjectured that Auerbach would probably differ with Friedman's characterization of the views of "most economists." Auerbach indeed disagreed. Nevertheless, Auerbach recalled of the occasion that "for anyone who knew Friedman and his work, he was, very much, consistent…. He was saying the same things he would have said 50 years earlier." (Alan Auerbach, May 18, 2015.)

This was true: Friedman had advocated a flat-rate tax in *Capitalism and Freedom*. It was also true that Friedman was likely incorrect when co-opting the majority of the economics into support of a single-rate tax regime. His reference to "most" economists was a case of Friedman's tendency to overestimate the extent of professional agreement with himself and so to give hard-to-justify characterizations of the majority viewpoint.[142] It was, however, true that, since the early 1960s, the idea of flattening the U.S. tax-rate schedule in exchange for removal of tax deductions had acquired wide professional support. In fact, the major actual move of the federal tax system in this direction in 1986 had reflected the fact that such a move had secured the support of economists and politicians of different ideological persuasions. Of this 1986 move, Campbell (2000, pp. 194–195) would write: "The radical revision of the tax code under President Reagan could be traced to the writings of Milton Friedman almost [sic; more than] two decades earlier…" This was accurate, in the sense that Friedman had been a champion of moving to a lower-marginal-rate/fewer-deductions swap. But—as Friedman acknowledged, so, for decades, had longtime adviser to Democratic politicians, Joseph Pechman of the Brookings Institution.[143] Pechman, who died in 1989, lived long enough to see the act, passed in 1986, come fully into effect in 1988 as an almost two-rate federal income-tax system. The partial inspiration that Friedman provided for the tax reform was obscured by his generally negative remarks prior to its passage. He was highly skeptical that any meaningful tax reform would be achieved and extremely critical of the respects in which Reagan's and Congress' tax-reform proposals stopped short of a true flat tax. Consequently, he made it hard for others to see the

[142] This tendency would be magnified in the 1970s by his enthusiasm about the public-choice approach to modeling policymaking. In particular, Friedman would tend to discount differences about energy and inflation policy among economists: he would frequently treat his own views as widely shared, with variation in policy prescriptions across economists then attributed mainly by him to divergent assessments of what was politically feasible.
[143] On Pechman's work, which Friedman acknowledged in *Playboy* (February 1973, p. 62) (reprinted in Friedman, 1975a, p. 25; 1983b, p. 41), see Chapter 14 below.

extent to which the 1986 act moved the income-tax system much closer to the arrangements that he favored.

Friedman praised the 1986 tax act once it was passed—though he remained wary, fearing that over time the U.S. political system would largely restore federal income taxes to their prior level of complexity. His fear would largely be borne out, as many new tax deductions were added to the system after 1986. Another move away from the 1986 reform came from 1990 onward with legislated increases in marginal income tax rates signed by George H.W. Bush and Clinton. Friedman opposed the increases in taxes enacted in the 1990s. When, correspondingly, President George W. Bush advanced tax cuts in the 2000s, Friedman supported the proposals. [144] Stimulus to the supply side was a major reason he cited for his support. A source of irritation to him in 2001 was the extent to which the case for a tax cut was presented by the Bush side in Keynesian, demand-stimulating terms.[145]

On the 2004 television program, Friedman included a second reason he favored tax cuts beyond their supply-side rationale. This second reason was one that was unambiguously associated with himself, although the label often used for it, "starve the beast," was not one he typically used. Friedman argued, as he often had in the past, that the revenue loss and associated deficits associated with tax cuts would put downward pressure on government spending and have supply-side benefits through that channel.[146] Auerbach knew this was familiar ground but recalled that he thought that viewers might not think so: "I remember thinking at the time that, for a Bay Area audience, they might think he was, because of his age, he maybe was, you know, saying things that were a little erratic." (Alan Auerbach, interview, May 18, 2015.) The matter was clarified when another panelist on the program, the reporter David Lazarus, identified the

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[144] Friedman's participation in the public debates on each of these sets of tax cuts was not very substantial, although he was highlighted as a prominent supporter of one aspect of the 2001 tax cuts—the suspension/repeal of the federal estate tax. The London *Financial Times* (May 15, 2001) included a (1970s-vintage) photograph of Friedman in a news item on the public debate on the estate tax, on account of Friedman having recently co-signed with 278 other economists a public letter that concluded: "Death should not be a taxable event. The estate tax should be repealed.'"
[145] See Chapter 18. The George W. Bush Administration advanced this argument even though opinion in the economics profession had, as discussed below, reached a consensus against the active use of fiscal policy in addressing business-cycle fluctuations. For example, Feldstein (2002, p. 152) referred to "the general presumption against discretionary 'countercyclical' fiscal policy" and noted (p. 151): "Even economists who did not consider themselves to be monetarists came to this conclusion."
[146] These starve-the-beast views were adopted by Friedman in the early 1960s (see Nelson, 2020a, pp. 6, 384, 389; 2020b, pp. 16, 362n4; and the analysis in Chapter 9 below). Thomas Simpson recalled hearing at first hand, during the 1965−1968 period, "Friedman's very strong belief that, to use his expression, "*T* determines *G*," that taxes determine government spending… from the perspective of the *political* process… And so he made those statements very, very often in courses that he was teaching and workshops, and things like that, so you know, we were very much exposed to that brand of thought." (Thomas Simpson, interview, May 29, 2013.)

argument as a longstanding Friedman position and even praised its internal logic. But, Lazarus insisted, starve-the-beast mechanisms had not proved to operate in the United States in practice.

In fact, the record was mixed. In particular, the Reagan deficits had eventually given rise to a legislated spending-restraint mechanism in the second half of the 1980s under the Gramm-Rudman-Hollings law, and Blanchard and Dornbusch (1992, p. 116) had ventured the judgment: "But, in fact, Gramm-Rudman has worked." More recently, however, the George W. Bush tax cuts had been associated initially with rapid increases in domestic federal spending. Friedman replied to Lazarus by acknowledging that the administration, in "this term, has been very bad on the spending [side]. I don't deny it. I think it'll be different. I think the deficit that just emerges, has emerged, it's going to produce pressure on the Congress." In the event, federal spending's share of GDP fell in only one year of Bush's second term, fiscal-year 2007 (Council of Economic Advisers, 2022, Table B–46, p. 411).

Toward the end of the program, Friedman raised an issue that had not so far been covered: healthcare. Healthcare had been one of his research interests in the prewar period. At that time, his focus had been on the supply side. His return to the issue in the past couple of decades had, however, seen him concentrate on the demand side. Friedman had since the early 1980s joined those who viewed with alarm the very steep rise in the U.S. medical spending share of GDP since the mid-1960s. By 2004, the share substantially exceeded that in other advanced countries, and this anomaly was often cited as a product of the fact that the United States' medical services were more private-sector oriented than in many other countries. Friedman's perspective was different: he stressed the increase in the federal government's spending on healthcare as a driver of the overall medical-expenditure share. There was certainly substance in the point that the U.S. government's role in medicine had increased substantially. That it had done so dramatically in the second half of the 1960s and over the 1970s was a point stressed by Friedmans in their *Free To Choose* book but also, around the same time, by Democratic politician Sam Nunn, who had noted in a speech to the Senate chamber in September 1979: "in fiscal year 1965, federal budget outlays for health were $1.7 billion; however, by 1978 expenditures for health had risen to $43 billion, a percentage increase in health of 2,463 percent."[147]

This increase occurred as part of a broader rise in expenditures on hospitals and other health expenditures by all tiers of government in the United States—from $5.244 billion in fiscal-year

[147] From Nunn's speech in the U.S. Senate, September 18, 1979, quoted in *Congressional Digest*, November 1979, p. 284.

1960 to $63.731 billion in fiscal-year 1985 (Tax Foundation, 1988, p. 9, Table A7). The rise in the U.S. general price level in the quarter century from 1960—so important a focus in Friedman's monetary work—only went a small way in accounting for this increase: the 12,152 percent increase in nominal government spending on health from 1960 to 1985 contrasted with a rise in the U.S. consumer price index of roughly 375 percent over the same period.
It was also clear, however, that private-sector spending on healthcare had also increased explosively. Friedman's late-in-life analysis of the healthcare industry did not exempt private-sector arrangements from criticism. Nor, indeed, was his proposed solution—in the form of a revamped insurance system—altogether *laissez-faire* in its character, as it would have increased the government's role in some respects and decreased it in others.[148]

In the October 2004 PBS television program, he could only hint at his views on the matter by endorsing a Bush proposal to introduce individual medical accounts—a move that Friedman believed would help in dismantling what he regarded as one of the flaws in the U.S. system—employer-provided medical insurance—and introduce a more market-oriented and price-guided medical system.

Gorney forestalled the discussion of healthcare, however. She thanked Friedman for raising the topic, but only because it allowed her to highlight the fact that the following week's program would be on healthcare. Friedman's views on healthcare would not receive an airing on that next program, as it would consist of a different set of panelists. The current program on the economy had run out of time (*Election 2004: The Economy*, KQED, October 15, 2004).

III. The shift in canvas

The 2004 television discussion was concerned with economic matters most closely associated with presidential politics. It covered monetary policy only when Friedman explained why he thought that 2001 had been associated with "one of the briefest recessions in history." He did not think that the brevity of that recession was a presidential achievement: "I don't think the credit [for] that goes to Bush. I think the credit… goes to Alan Greenspan—to the Federal Reserve for the policies which it followed."[149]

Singling out the Federal Reserve for praise was not something for which Friedman had become

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[148] Friedman's views on healthcare are discussed in Chapters 10 and 18.
[149] In these remarks, Friedman made points that he would elaborate in Friedman (2005a).

known. On the contrary, Greenspan (2010, p. 237) would recall that Milton Friedman was "historically the Federal Reserve's severest critic." With regard to Greenspan's predecessors as heads of the Federal Reserve, Friedman had not refrained from criticism even in the friendly setting of the Mayo Institute when, on May 19, 1978, William McChesney Martin, Jr., head of the Federal Reserve from 1951 to 1970, had turned the lectern over to Friedman, whom he had introduced as the day's guest speaker. "I have to admit [that] I have been a steady critic of many of the policies and practices of the Federal Reserve," Friedman remarked. He had recently walked away from a role as a consultant to the Federal Reserve Board—a role that amounted to meeting the Chair and the other governors for a half-day once or twice a year to discuss the current economy. This role had started for Friedman in 1965 under Martin. Friedman recalled in his open remarks at the Mayo event: "I enjoyed being associated with Bill over many years at the Federal Reserve. I have nothing but respect and admiration for the public spirit and enterprise and initiative which he showed in that post." He then added the sting in the tail: "Even if he did the wrong things."

Something that Bill Martin said about himself in his introductory remarks would become increasingly applicable to Friedman a generation later: "I had a fellow run up to me in the hotel lobby and say, 'My goodness, are you still around?'"

The "you still around?" reaction, although seldom voiced to him directly, was a common one toward latter-day Friedman appearances. In March 2001, on return to London after co-presenting a paper at a Federal Reserve Bank of San Francisco conference, Nicoletta Batini remarked to colleagues at the Bank of England, "*Milton Friedman* was there!," her tone of voice registering her continued incredulity. Likewise, in hosting the 2004 television panel, Cynthia Gorney likewise still seemed to be adjusting to the reality that Friedman was a panelist. A surprised tone entered her voice on the occasions when, in the course of the hour, she read his name out among when listing the four guests for the viewers' benefit.

Friedman, already a four-decades-plus veteran of the economics scene in 1977, was still part of it when Alan Greenspan became Federal Reserve chair in 1987 and was "still around," and writing and speaking about economic issues, when the Greenspan era ended in 2006. A repercussion for economic debate of Friedman's longevity was that, as already alluded to, he lasted long enough to face multiple generations of debating opponents: senior figures born in the nineteenth century like A.C. Pigou in the 1930s and Dennis Robertson in the 1950s, and from the 1950s onward a stream of U.S. Keynesian opponents who were roughly contemporaries of his (but who tended to be some years younger than Friedman, predominantly having birthdates in the 1910s than

himself) like James Duesenberry, James Tobin, Paul Samuelson, and Franco Modigliani. There were also many figures whom he debated who were born in the 1920s and emerged starting in the early postwar years—Kenneth Arrow, Don Patinkin, and Robert Solow among them. But a considerable number of those with whom Friedman would eventually cross swords were born still later—some well after Friedman started publishing in 1935, including numerous economists, like Blinder and Auerbach, who were born some time after the end of World War II.

Owing to his engagement in debate, particularly in the media, with younger figures, many of those who were Friedman's interlocutors in conference or media forums during the 1970s (or earlier) were still alive in the early to mid-2020s, over fifteen years after Friedman's death—including Solow, Peter Jay, William Nordhaus, and Marina Whitman and, among those who were not economic specialists but whom Friedman nevertheless jostled with on economic matters, Phil Donahue, Ralph Nader, and Francis Fox Piven.

The multitude of Friedman's debates had also, however, seen him debate numerous figures who were younger than himself but whose lives were cut short.

One case in point was Warren Smith. Just a few years younger than Friedman, Smith made a fairly late start, only beginning to publish work on monetary economics in the mid-1950s. But over the subsequent decade and a half, he would be a steady generator of hardline Keynesian product.[150] Donald Kohn, a student at the University of Michigan in the 1960s, took Smith's graduate class: "I received a very Keynesian education… In the end, it was pretty clear what Warren Smith's views were." Kohn recalled that, during his period as a student, he had been present at a campus "debate between Friedman and Warren Smith. The debate took place in the undergraduate library. It was a good lesson in economics, because they ended up arguing about the size of coefficients in certain equations and what the implications of that were. I went expecting to see something like *Firing Line,* but it was really two economists arguing very specific things and the implications for how they saw the system working. It was a really good education. It wasn't 'He said, she said.' [Instead,] it was, "Let's talk about the specifics of how these things fit together.'"[151]

Smith did not turn out to be one of Friedman's debating opponents of the 1970s, as he died at age 58, having suffered a stroke while he was teaching a class on April 20, 1972 (Hymans, 1972;

[150] See also Chapter 5 below.
[151] In Small, Lindsey, and Clouse (2010, pp. 4–5).

Ironwood Daily Globe (Michigan), April 24, 1972).

Another premature passing was that of Eli Goldston, an executive who debated Friedman publicly on the issue of the social responsibility of business but who died suddenly at age 54 in 1974, not long after he had stepped down from his business role in order to play a larger part in public debate. In early 1976, an academic economist Vivian Henderson debated Friedman on income-distribution policy. But Henderson died at age 52 between the taping and broadcast of their televised debate. In 1980, Arthur Okun, a recurring part of national economic discussions over the prior two decades and a longtime Keynesian sparring partner, died suddenly, also at age 52. In 1984, another recurrent Keynesian opposite number of Friedman's, Otto Eckstein, died at age 57.

Furthermore, the length of Friedman's career meant not only that he was around long enough to spar with multiple generations of interlocutors but also that he lived to see very considerable change in economic policy. These included some changes in the monetary realm that were in the directions he had recommended and that, by the 2000s, implied that he had been as big an influence on monetary policy as any living researcher had been.

In this connection, Friedman's longtime Hoover Institution employer Glenn Campbell (2000, p. 194): "Friedman had impressed on the minds of policymakers the critical role of monetary policy in determining the inflation rate. His teachings and writings contributed greatly to the… significant shift in monetary policy in 1979, which produced the first sustained period of noninflationary growth in two decades." Campbell's assessment was that of a non-expert, as he was altogether outside monetary economics. But it was substantially correct. The 1980s were not, for Friedman, a period of great satisfaction because, as already indicated, he was very much on the defensive regarding the merits of looking at monetary aggregates. It was also one in which Friedman was seemingly becoming less, not more, influential on monetary policy, because the Federal Reserve veered away from being concerned with monetary aggregates.[152]

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[152] As discussed in Chapters 14 and 16, Friedman remained predominantly supportive of monetary aggregates' merits over the course of the 1980s, although he did make a notable shift from emphasizing M1 to emphasizing M2. After 1992, he acknowledged major changes in the relationship between M2 and the economy. By this point, Phillip Cagan, although still on the monetarist side, had concluded (largely on the basis of the M1/income relationship) that there had been substantial alterations in the relationship between monetary aggregates and the economy: "money as an indicator has lost some of its previous attraction… [T]hese institutional changes have occurred, so things are not so simple as they used to be." Asked about Anna Schwartz's reliance on the modern M2 series, he remarked, "she probably has a little more confidence in it [M2] than I do." (Phillip Cagan, interview, January 13, 1992.) This was an understatement, as Cagan placed little weight on the modern M2 definition and did not feel that it was an aggregate whose predictive qualities were likely to weather ongoing institutional changes.

Ultimately, it continued this move away from monetary aggregates over the Greenspan tenure. But the 1980s and beyond saw the consolidation of the view that monetary policy should shoulder the responsibility for controlling inflation—and that no other policy was capable of successfully meeting that responsibility. This view, consistently held in policy circles from 1979, was confirmed a quarter century on when Alan Greenspan (2004, p. 33) remarked: "I have little doubt that an unrelenting focus of monetary policy on achieving price stability has been the principal contributor to disinflation. Indeed, the notion, advanced by Milton Friedman more than 30 years ago, that inflation is everywhere and always a monetary phenomenon, is no longer a controversial proposition in the profession."

By 2004, this view of inflation was also held by monetary authorities abroad. In some cases (such as in Switzerland and Germany), a monetary perspective on inflation had been prevalent in officialdom before it took hold in the United States, in other countries (like the United Kingdom) the official conversion took place in the same timeframe of the late 1970s as it did in the United States, and while in other cases (like Australia and New Zealand) the change took place only later, from the mid-1980s to the early 1990s.

The overall shift in viewpoint on the causes of, and responsibility for, inflation was also reflected in the publications of international policy agencies. The International Monetary Fund's 1970 annual report had contended that "the control of inflation is rendered particularly difficult by strong cost-push forces, with wage settlements substantially in excess of normal productivity growth," while the 1971 report had stated along similar lines that "cost-push forces are still very strong in the industrial countries even though excess demand has now clearly been eliminated."[153] In contrast, the 1981 report observed: "Moderation of inflation in the industrial countries is unlikely to occur without a reduction in rates of growth of aggregate nominal demand that is sufficiently marked and prolonged to break inflationary expectations."[154] That 1981 report also noted: "During the 1970s, most attempts to establish formal incomes policies encountered failure."[155]

Friedman had kept the monetary view of inflation alive during long stretches of time when it was out of favor within officialdom. He had publicly criticized price controls in World War II after he left government service in Washington, D.C.[156] During the era of the Korean War price

[153] International Monetary Fund (1970, p. 5; 1971, p. 7).
[154] International Monetary Fund (1981a, p. 37).
[155] International Monetary Fund (1981a, p. 37).
[156] See Nelson (2020a, Chapter 3).

controls, at a college symposium held in Ohio on "Inflation and Price Controls," Friedman debated both current price-control administrator Michael V. DiSalle and the World War II-era controls administrator John Kenneth Galbraith—who, in the postwar period, had remained viscerally supportive of price controls, including in peacetime. "I challenge anyone to find any example in which price and wage controls stopped inflation when there was any strong inflationary pressure on the economy," Friedman told the symposium (*Cleveland Plain Dealer* (Ohio), March 14, 1952). He had subsequently been a leading critic of the creeping move toward wage-price jawboning in the later Eisenhower years, of the far more explicit jawboning and wage-price guideposts under Kennedy and Johnson, and of the wage-price controls under Nixon. And, as indicated above, Friedman articulated strong opposition to the various post-1974 incomes-policy official efforts at, and outside-government proposals regarding, incomes policy advanced from 1974 to 1981 under Ford and Carter.

The official support for nonmonetary measures in the executive branch ended with the Reagan Administration's winding up of the Council for Wage and Price Stability in 1981.[157] But, just as Friedman had been a longtime critic of incomes policy in the era when it enjoyed support in officialdom, it was natural to expect that, in the post-1979 period, U.S. Keynesians would continue to make the case for incomes policy against inflation. This advocacy of incomes policy did take place, but it petered out quickly. A Washington, D.C., group, the Center for Democratic Policy, that featured members of the former Carter Administration heavily in its organization, put out a pro-incomes-policy technical pamphlet, *Controlling Inflation: Studies in Wage/Price Policy*, early in the Reagan years (Mitchell, Bosworth, and Seidman, 1981), and Congressional hearings about the promise of incomes policy were held in 1982 (see Chapter 14). But this facet of Keynesian economic prescriptions faded after the early 1980s. When Solow and Tobin (1988, p. 15)—in discussing what they still regarded as "the endemic problem of achieving full employment without inflation"—stated that "there is still little reason to believe that it can be done without incomes policy," they were really referring to the instrument assignments in their ideal world, rather than making a prescription for current policy.

Fifteen years later, Friedman could reasonably characterize the modern consensus as believing that to see inflation as "produced primarily by pressures on cost that could best be restrained by direct controls" was to subscribe to "a wrong theory" (*Wall Street Journal*, August 19, 2003).

[157] This shift in policymaker emphasis was foreshadowed by the greater responsibility assumed for curbing inflation by the Paul Volcker Federal Reserve in 1979, although Volcker refrained for the most part from explicit criticism of the Carter Administration's variant of wage-price guideposts.

As his 1988 remarks made with Solow indicated, Tobin was not prepared to subscribe to this consensus himself. He nevertheless granted in the spring of 1985 that it was "widely understood and accepted" that central banks had a "duty to hold in check" inflation. He even remarked: "Monetarism… has greatly influenced economic thought and government policy… Much of its influence is durable." (*The Economist* (London), April 27, 1985, p. 26.)

How the consensus was changed

Friedman remarked in December 1980 that, in the course of his career, "I have been an outcast for a very long time from many different groups." Yet he also observed that "outcasts often become the majority because they are the ones who have what is needed to solve a problem."[158]

In the area of monetary policy, Friedman's views did move from being outcast views to being incorporated into official, and majority, thinking. And this remained the case on fundamental matters related to the understanding and control of inflation, even after the—certainly important, to him and other monetarists—reserves-control and monetary-aggregate-target-focused aspects of his policy views had reached their peak level of official acceptance in the early 1980s and had once again reverted to being out of favor with policymakers and most monetary economists.

How did Friedman achieve this degree of acceptance? Notwithstanding the fact that he taught at a leading graduate school in economics from 1946 to 1976, and the media's routine references to former students of his as Friedman's "disciples," his success in getting his views accepted did not come from formal training of future monetarists. Despite Friedman's creation of the money workshop, the spread of his ideas was not really attributable to any process of his University of Chicago students graduating and moving to influential positions elsewhere in the American economic profession. Certainly, there were some workshop members who went on to have careers in which they expounded monetarism in research publications—including members of U.S. academia like Michael Bordo, James Lothian, and William Poole. And, of course, numerous aspects of Friedman's monetary work were developed by Robert Lucas, who had taken Friedman's Price Theory class. But the dearth of University of Chicago students closely associated with Friedman who went on to have senior positions at major East Coast universities was striking.

Phillip Cagan might appear to have been well placed to be an exception. One of the students

[158] Friedman (1981c, p. 152).

most closely associated with Friedman's monetarist movement, he became a full professor at Columbia University, with which he was affiliated from the mid-1960s.[159] Cagan did not shirk the application of the label "monetarist" to himself. He routinely described himself as a monetarist. In addition, Cagan's Ph.D. dissertation was a highly uncommon case of a Friedman-supervised dissertation in the monetary area making a very major impact on economic research.[160] And his credentials as a monetarist in print went back even before the Cagan (1956) study of hyperinflation. They began with a 1955 book review in the *Journal of Political Economy* in which he observed of increases in the money stock, "No substantial, permanent rise in prices has so far occurred without them."[161]

But Cagan's instincts in his professional career were decidedly middle-of-the-road. His inclination was to build bridges. Consequently, in confronting Keynesians, he did not have the smiling wrecking-ball approach of Friedman or the take-all-comers aggressiveness of Brunner and Meltzer.[162] Cagan's tendency toward moderation served him well in expressing opposition to the extremes—as he did in criticizing hardline Keynesianism and also in resisting, as already noted, the considerable popularity in U.S. academic macroeconomic circles in the 1980s of flexible-price monetary analysis. But the same outlook made him poorly situated to make the case for some of the monetarist positions, such as the case for policy rules and the concentration on small models—matters on which monetarists faced more moderately minded opponents. Consequently, Cagan was not particularly well situated to contribute importantly to altering the consensus of the U.S. economics profession.[163]

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[159] Another member of the 1950s money workshop to have a top-rank university appointment was Gregory Chow, long of Princeton University. Chow's research, however, mostly stayed away from the Keynesian-monetarist debates.

[160] Cagan remarked to the present author that "when young people come along, they have to find some way to make an impression." (Phillip Cagan, interview, January 13, 1992.) This was a somewhat jaded remark, made in reference to the modern-day operation of economic research. Nearly forty years earlier, however, Cagan himself had been a quintessential example of a young economist making an impression, with Cagan (1956) appearing in print when he was 29. Most of his later work did not receive a comparable degree of acclaim. When the present author mentioned having read Cagan (1989a), Cagan replied (interview, January 13, 1992): "Oh—glad you did. That hasn't gotten much attention."

[161] Cagan (1955).

[162] Brunner and Meltzer were also to mark themselves out by working in areas of monetarism in which Friedman's own contributions were more sparse. See Nelson (2020b, Chapter 13) on this matter as well as the discussion of Allan Meltzer in Chapter 17 below.

[163] Among the features that exhibited Cagan's middle-of-the-road outlook compared with Friedman was his willingness to use the term "macroeconomics" and his readiness, versus Friedman's reluctance, to decompose the behavior of inflation in terms of the behavior of cost increases, such as in Cagan (1984, p. 25; 1986c)—a readiness also reflected in assertions like "Wages are a good indicator of the underlying rate of inflation" (Cagan, 1986c, p. 270). In the 1970s and 1980s, he also eschewed stands that Friedman embraced, as when he rejected the notion that Ricardian equivalence might have empirical relevance (Cagan, 1976a, p. 319) and when he contended that monetary policy's role in the 1980 recession "appears limited" (Cagan, 1984, p. 28).

Still other Friedman graduate students who were of roughly the same 1950s vintage as Cagan were supportive of monetarism in their post-1950s careers but were located at less prominent institutions than Cagan or Friedman. Indeed, In the case of students who focused on monetary issues a recurrent frustration (evidently voiced by David Fand and Richard Selden, for example) that Friedman's ubiquity in 1960s Keynesian-monetarist debates made it more difficult for his 1950s-era students to make a strong, lasting impression after graduation in their own advocacy of the quantity theory of money.[164]

Friedman had also produced quite a few students who had been in his money workshop or Price Theory class who became critics of his monetary framework. The most prominent of these was Neil Wallace, but more comprehensive and hardline critics of Friedman were, in their different ways, Boris Pesek and Leonard Rapping.[165] Another renegade workshop member—both as a graduate student and a subsequent member of the University of Chicago economics department—was Lester Telser, who strongly disliked monetarism, as his occasional research contributions on monetary matters made clear.

Relatedly, as stressed in Nelson (2020b, Chapter 12), over his years at the University of Chicago Friedman did not have a critical mass of fellow proponents of monetarism among his departmental colleagues.[166] Allies on free-market economics like Gary Becker and George

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[164] With regard to Fand, David Meiselman observed (interview, April 30, 2013): "Milton had a house in Chicago on Kenwood Avenue. And the house had three floors in it. Milton's family lived on the first two floors, then there was a third floor—there was an apartment on the third floor, and David Fand and his wife lived up there. David was always attached to Milton. But, for many years, he was very angry at Milton—for many years—because he felt like he [Friedman] did not give him enough acknowledgments and that he never gave him enough help." With respect to Selden, his former University of Virginia colleague Leland Yeager remarked (interview, August 8, 2013): "Selden made the remark—now, these words are mine, I don't have an exact quotation—that he felt that Friedman had double-crossed him by going [permanently] into monetary economics. He was under the impression that Friedman would stick to price theory, leaving the field of monetary [economics] open to be dominated by himself, Richard Selden."

[165] In the case of Wallace and Pesek (as with Frank Hahn), one of the Friedman contributions that they criticized was actually one from which Friedman, in his discussions of practical monetary economics, largely detached himself—namely, the optimal-monetary-growth or deflation rule that he derived in his 1969 book chapter.

[166] The previous volume discussed Keynesians who were Friedman's colleagues in his years at the University of Chicago, including Martin Bailey and Robert Gordon. Another example was Donald Tucker, an MIT graduate who was a member of the department in the 1960s. Tucker made multiple presentations to the money workshop, including two occasions when Friedman was present: October 18, 1966, and April 23, 1968. Thomas Simpson, a graduate student in this period, recalled (interview, May 29, 2013): "Tucker did some interesting stuff. But, you know, Friedman had very, very strong views on monetary economics—extremely strong views. And Tucker was kind of orthogonal to those views. From the standpoint of others, particularly the students who were exposed to some of Tucker's work, we thought it as interesting stuff and worthwhile stuff… [But] Friedman just dumped all over him. And he just didn't like that stuff at all, because he [Tucker] was using more of a, for want of a better term, IS-LM kind of approach to monetary economics, and that wasn't Friedman's approach… And so anyone who had looked at it in a different way, he was very skeptical of, and, oftentimes, wasn't terribly polite. (*laughter*)."

Stigler were researchers who worked in areas very separate from monetary analysis.[167] Friedman did have, in his closing years at the university, a strong fellow adherent to the NRH in Robert Lucas. But the opposite situation had prevailed in an earlier period, with his colleague Albert Rees being a proponent of a nonvertical Phillips curve—and, in his years after leaving the University of Chicago, a critic of the NRH as well as a supporter of cost-push views.

All told, then, it was simply not the case that there emerged a critical mass of highly influential economists trained by, or working nearby, Friedman who practiced monetary economics and systematically spread his views. Clearly, Friedman's influence on economists and his success in changing economic thinking did not occur primarily by his own direct training or teaching of next-generation economists or through an influence on University of Chicago colleagues.

Outside the university, too, Friedman's influence did not arise in a clear-cut, visible manner: there were very few prominent, acknowledged conversions to monetarism by Keynesian academics at other U.S. academic institutions.[168] There were also not many unambiguous cases of eminent converts to Friedman's views in the non-academic economic world of media and financial-market analysts of the economy. Indeed, over the 1980s, some of the journalists who had been sympathetic to Friedman in the 1970s became critics of his monetary analysis—a situation that reflected in part the problems encountered with monetary aggregates and in part the backlash against monetarism in the heavily media-oriented supply-side movement. A central example consisted of the unsigned *Wall Street Journal*'s editorials, which became strongly anti-monetarist from the mid-1970s onward. In addition, op-ed writers or economics columnists in the U.S. press like Alan Murray, Warren Brookes, and Louis Rukeyser started out being receptive toward Friedman's economics but became more critical of monetarism over time.

Among journalists outside the financial area exposure to Friedman's economics and to the man himself provoked a variety of reactions. The perspectives of different individuals associated with him on the *Free To Choose* television series exemplified this point. Longtime U.K. journalist Anthony Jay found him persuasive—so much so that, on the basis of Friedman's

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[167] For further discussion, see Chapters 3 and 14 below.

[168] Thomas Mayer was possibly the most prominent instance. In addition, the decade of the 1970s saw notable cases of authors who had published prominent Keynesian work becoming supporters of, and contributors to, that decade's rational-expectations monetary, or New Classical, literature: Robert Barro, Herschel Grossman, and John Kareken. That literature was, in the 1970s, generally supportive of Friedman on several counts. Kareken's writings, however, drew on the rational expectations literature to stress his own disagreements with Friedman (see Nelson, 2020b, Chapter 15). In contrast, Robert Barro enjoyed warm relations with Friedman and over the years, and he highlighted areas in which they agreed, although Barro did not share Friedman's belief in price stickiness. Grossman, too, defended monetarism on occasion, including against Frank Hahn's criticisms (see Grossman, 1984).

writings and meetings with him, he helped set up the series. But political scientist and veteran broadcaster Robert McKenzie, who hosted debates made as part of the series, became disillusioned with Friedman. McKenzie, affronted by Friedman's lack of interest in continuing their exchanges off-camera, denounced him publicly as being "as dogmatic in advancing [his] ideas as any Marxist" (*The Listener* (London), May 9, 1980).[169]

From the above discussion, it is clear that the shift in the U.S. economics world's discussion of monetary matters from the 1960s to the 1980s in favor of views that Friedman expounded had little to do with his students getting major posts at major universities or other economics centers—as so few did so—or with major members of the economics explicitly indicating that they had changed their thinking in Friedman's direction—as American academia, in particular, did not see a major trend in which economists went from calling themselves Keynesians to describing themselves as monetarists or as Friedmanites.

Instead, the growth of Friedman's influence took place through the positions advanced in his work coming to permeate the views of the profession and to alter its consensus. Individual pieces of his monetary thinking—such as the Friedman-Schwartz emphasis on money and the business cycle, and the natural rate hypothesis—came to be widely acknowledged as having made an imprint. Other pieces that tended to be less associated with specific cited contributions, such as his work on lags and on inflation's monetary character, also came to be embedded in the consensus.

The possibility that Friedman's monetary positions would be assimilated into mainstream opinion, but that the implied shift in the consensus would not receive much explicit acknowledgment, was something that he felt was already being realized during the late 1960s and early 1970s. The examples that he gave at that time were in the context of an ongoing debate between Keynesians and monetarists: the former group, he suggested, were accepting monetarist positions and then distinguishing their views from his own by caricaturing his position.[170]

Some of the change in thinking toward acceptance of Friedman's monetary views did indeed take place in the gradual and unacknowledged fashion that he sketched—as the usurpation, by the mid-1980s, of monetary policy by fiscal policy in mainstream economic discussions of stabilization policy attested. This usurpation was discussed above. But a separate factor in

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[169] See Chapter 11 for a fuller discussion.
[170] See his remarks in *Time* magazine, December 19, 1969, and in Friedman (1970c, p. 22; p. 14 of 1991 reprint).

operation that went in the direction of promoting the acceptance of Friedman's views was something that has already been stressed: a generational move. Many self-identified younger Keynesians of the 1980s, while eschewing the flexible-price approach of the 1970s rational expectations literature, accepted key views on output and price dynamics espoused by Friedman. In so doing, they helped finalize the transition of these views into mainstream positions. Balls (1998, p. 118) recalled: "in my first week at the Harvard economics department, Professor Greg Mankiw—doyen of the young New Keynesians—eulogized Milton Friedman to the new graduate class." And in an interview given at his location of MIT in October 1997, Olivier Blanchard had observed, "the main message of monetarism has become part of the common wisdom and, as such, nearly invisible."[171] And sometimes Blanchard made this message more visible in his own work, by directly citing Friedman. As one of the younger contributors to the Frank Hahn-coedited *Handbook of Monetary Economics* that was noted above, Blanchard (1990, p. 791) had written: "The event studies provided by the U.K. and U.S. disinflations of the early 1980s, in addition to those described by Friedman and Schwartz, strongly support the view that monetary policy affects output."[172]

Changing professional views on appropriate monetary policy

One reason why many of these New Keynesians did not classify themselves as monetarists, despite considerable common ground with Friedman on the appropriate specification of the dependence of economic activity and inflation on monetary policy, was that they diverged from his specific policy prescription.

Even in the mid-1980s, when Friedman's emphasis on the money stock continued to be largely accepted by them, contributors to the New Keynesian literature tended not to be as rules-oriented as he was.[173] It is true that Friedman—together with other monetarists, as well as various figures

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[171] Snowdon and Vane (1997, p. 232). Blanchard had moved to MIT's economics department in 1983 (Blanchard, 1997, p. iv).
[172] A little after the Blanchard article appeared, new disinflations in the United States and the United Kingdom provided further evidence on this score. During the U.S. part of this early-1990s disinflation (which is discussed in Chapter 16 below), Phillip Cagan remarked: "If you're going to bring the inflation rate down, you've just got to go through a tough period… you've got to slow the economy down. And you can do this by raising interest rates if you know exactly [by] how much to raise them; or by a monetary target… You have to slow the economy down and wait for [increases in] prices to come down. Look at the U.S. now. We have just come out of a recession. Growth is very sluggish. And people are talking about [rises in] prices coming down but if you look at the actual index, the CPI, it hasn't come down very much. It's been a slow, painful process… everybody finds their costs going up by the same amount, so they raise their prices. And you've got to cut into that momentum—which means that, where you cut, you create a lot of pain." (Phillip Cagan, interview, January 13, 1992.)
[173] The subsequent enhanced professional attention to interest-rate rules will be considered later.

in the rational expectations literature of the 1970s, such as Lucas, Thomas Sargent, and John Taylor—did succeed in having analytical research cast alternative policies in terms of alternative policy rules. But even in the context of describing monetary policy in terms of rules, a key difference can remain on the matter of policy activism—and especially on whether an explicitly countercyclical policy response is called for.

Friedman remained a proponent of a non-activist rule—that is, one that did not explicitly set out to respond to deviations of employment or output from policymaker objectives.[174] "In your review of economist Paul Krugman's book *Peddling Prosperity*," Friedman wrote to *Newsweek* (April 25, 1994), "you refer to 'Milton Friedman's theory that the Federal Reserve could manage the economy by focusing on money-supply measures.' This is almost the opposite of what I have in fact argued." The day when a monetary policy rule was imposed in place of an activist policy of "managing the economy" was, Friedman indicated, the day he lived for. Indeed, he continued through the rest of his life to favor a constant-monetary-growth rule.[175]

In contrast, both the New Keynesian literature of the 1980s (which considered policy rules mainly in terms of money-stock rules) and of the 1990s and 2000s (when attention was focused on interest-rate rules) typically had responses to real economic activity. James Tobin had been borne out in suggesting that a central-bank orientation toward an inflation objective (*The Economist* (London), April 27, 1985, p. 26) "definitely does not exclude countercyclical demand management."

But, *in responding to output*, the rules studied certainly paid heed to Friedman's suggestion that monetary policy should be directed at "the ultimate end of achieving a reasonably stable price level."[176] They did this via the specification of the real variable in the rules. Phillip Cagan had already noted that a monetary policy rule that respected the NRH was capable in principle of stabilizing both a real-economic-activity variable and inflation over time.[177] The policy rules studied in the New Keynesian literature did this. They embedded the natural rate hypothesis by

[174] Constant monetary growth would constitute both a rule and a non-activist policy when expressed as a reaction function for money. It could be regarded as an activist rule in terms of the implied reaction function for the short-term interest rate.
[175] See *Wall Street Journal*, August 19, 2003, and the discussion in Chapter 18.
[176] Friedman (1960a, p. 88).
[177] Cagan (1986a, p. 34), for example, had stated: "Monetary growth can be targeted to achieve price stability in the long run and yet allow discretionary deviations within limits devoted to short-run objectives." This perspective was in line with the later monetary policy literature on "constrained discretion." (See Chapter 18. Both Cagan and the later literature were using "discretion" to refer to what more precisely should be called an activist policy rule or reaction.)

specifying that the real variable to be stabilized was the difference between the actual and natural rates of unemployment or output.

As well as the response to the output gap, the rules under study also included a price-level or inflation target. In so doing, they reflected the common ground that had materialized in academia and policymaking by the 1980s that monetary policy should be centered on, and be assigned responsibility for, low inflation. The influence of Friedman in forming this modern consensus was implied by the fact that in October 1997, it was after making his already-quoted remarks about monetarism that Olivier Blanchard remarked (Snowdon and Vane, 1999, p. 232): "I am struck by how stable monetary policy and low inflation are now considered to be an absolute necessity for macroeconomic stability, especially in Europe."

As for Friedman himself, in the 1990s and 2000s, the constant-monetary-growth rule that he favored continued to be one that secured zero inflation—although he indicated that he would be happy as a matter of practice if U.S. inflation averaged 2 percent.[178]

The global dimension—international economic policy

In order for the discussion of policy rules to be focused on the stabilization of domestic variables was that an underlying premise, when openness of the economy was allowed for, that the exchange-rate system was floating. It was that situation that allowed a country to experience situation of high capital mobility yet not be in danger of having international payments flows overwhelm its own attempt to set values for key monetary instruments and ultimately of economy-wide totals. "Let me emphasize: Any goal about price levels or about monetary growth implies a willingness to allow the exchange rate to vary. You cannot simultaneously peg the exchange rate," Friedman once remarked (*The Age* (Melbourne), April 11, 1975).

By the late 1960s, the case for floating exchange rates that Friedman had championed since the early 1950s had attained wide support among U.S. economic researchers—so much so that Marcus Fleming (1968, p. 4) referred to "an academic revolt" against the fixed-rate system. Subsequently, the floating-rate system became a reality. The Bretton Woods system ended in 1971, as far as Friedman was concerned (see Nelson, 2020b, Chapter 15). But the actual move to lasting floating occurred in March 1973 and is discussed in Chapter 3 below.

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[178] See Chapter 18.

By late 1976, in the wake of Friedman's Nobel, *Business Week* had observed (November 1, 1976, p. 73) that his stance had "suddenly become part of the international codes … [of] economic policy." In particular, floating exchange rates' existence had been ratified by multiple governments in a January 1976 Jamaica IMF conference that made an after-the-fact "decision" to validate the floating regime—and, more importantly, support its continued operation.[179] Friedman was highly cautious in attributing the move to float as a victory for his intellectual case for flexible exchange rates, and, in the mid-1980s, he and Anna Schwartz contended: "Decades of academic argument in favor of… adopting flexible exchange rates had little or no impact on institutional arrangements until crises made major changes inevitable."[180]

Friedman and Schwartz's formulation did not actually amount to a denial of an influence on policymaking and outcomes of Friedman's interventions making the case for flexible rates. Rather, it was a reference to what created the point of inflection between fixed exchange rates and floating exchange rates. The advocates of floating on analytical grounds had already gained the upper hand in the research world by the late 1960s, as indicated above. The actual move to flexible rates was undoubtedly driven in part by the great practical difficulty of maintaining fixed exchange rates and, in part, by the emergence of crises when governments made hurried moves to shore up the existing system. But the actual move by some countries to floating in 1971−1973 was in part based on policy-autonomy arguments of the kind that Friedman had advanced. And the acceptance of floating rates in 1974−1977 was, certainly, heavily shaped by those arguments.[181]

The U.S. government in effect accepted floating as a permanent part of the landscape by removing foreign exchange controls in 1974. Other governments later followed suit by liberalizing their own regulations in this sphere.[182] And although exchange-market intervention was frequently made by central banks from 1973 onward, this occurred against a background of an acceptance of the default international monetary system being one of indefinitely proceeding floating rates. The implied system was much closer to a floating world than what Friedman had envisioned up to 1971. He had then thought that it was more likely that fixed exchange rates would continue in a decentralized form, with smaller countries regularly going through extended periods of pegging their exchange rates to the U.S. dollar.[183]

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[179] See Chapter 7.
[180] Friedman and Schwartz (1986, p. 60).
[181] For further discussion, see Chapters 7 and 9.
[182] For further discussion, see Chapter 3.
[183] See Nelson (2020b, Chapter 15) and Chapter 3 below.

Although the U.S. research consensus in favor of floating hardened over the 1970s, there was at the start of the decade, and remained thereafter, a vociferous pro-fixed rate line of thinking, among some policy practitioners, journalists, bankers, financial market participants, and economists. Another advocate of floating rates, Fritz Machlup (1966, p. 173) had recalled that making the case for flexibility routinely generated "angry comments" on the part of these figures. This continued to be the case after floating became a reality.

One of those angry voices belonged to one of the leading open-economy macroeconomists, Robert Mundell. In the years leading up to the introduction of floating rates, Mundell had been a notable defection from those favoring flexibility: "Mundell has become an ardent exponent of fixed exchange rates recently," Harry Johnson (1973, p. 85) observed.[184]

Mundell, a University of Chicago colleague of Friedman's until 1971, had not been an overt proponent of floating exchange rates. But his pioneering research in the early 1960s on the monetary policy/fiscal policy mix in an open economy was seen as supportive of monetary-autonomy arguments of the kind that Friedman had stressed in his 1953 paper on flexible rates. This close association was brought out in 1978 by former University of Chicago student Marc Miles. Miles published an *American Economic Review* challenging the argument that flexible exchange rates brought "monetary independence"—that is, that floating rates imbued a country's authorities with the scope to carry out a monetary policy, and generate an inflation rate, distinct from what was prevailing abroad. As the references in the economic literature that had advanced the monetary-autonomy argument, Miles cited Friedman's 1953 paper on floating rates and Mundell's (1968a) book that consolidated his early-1960s work that had provided analytical support for floating.[185]

By the time of Miles' study, however, and indeed by the time of the publication of the 1968 book, Mundell had switched to the fixed-rates side. But, although Mundell would receive high-profile support among some politicians and journalists, the Friedman view on floating continued to have wide support in U.S. academia in the 1960s and—particularly after the 1972 departure of John Connally from the post of Secretary of the Treasury—among successive policymakers, including Connally's immediate successors, George Shultz and William Simon.

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[184] Thomas Simpson (interview, May 29, 2013) observed of Mundell, "Harry Johnson thought he was one of the most brilliant economists in the world, and so it was really Harry Johnson who brought him there," that is, to the economics department of the University of Chicago in the mid-1960s. Johnson's (1973) description of Mundell's fixed-rate advocacy of "ardent" perhaps suggested that Johnson did not find that Mundell's change in views was analytically sound.
[185] Miles (1978, p. 428).

In academia, too, unhappiness about the major post-1973 swings in exchange rates did not lead to large-scale support for fixed exchange rates. Mundell's early-1960s research continued to be widely used in research and teaching. But the Mundell change to support of fixed exchange rates after 1965 was not based on analytical research comparable to his earlier open-economy work, and his post-1965 campaigning for fixed exchange rates did not generate major interest among researchers. The generational shift in the U.S. research community—important in spreading support for Friedman's views on national monetary issues—was also crucial in bringing out new supporters of floating rates. One of the most notable cases was Mundell's former student, Rudiger Dornbusch. Although he had Mundell as his dissertation adviser, Dornbusch drew much more inspiration from the earlier Mundell research than he did from the sometimes hard-to-follow positions that Mundell espoused in the later 1960s and early 1970s.[186]

As an eminent open-economy macroeconomist in the 1970s, Dornbusch was not won over by the new Mundell enthusiasm for fixed exchange rates. He associated Mundell in the 1970s with a "hard-core view [that] is no longer very fashionable"—one in which inflation and the exchange rate moved so closely together that exchange-rate depreciations failed to generate the real adjustments associated with them in Friedman's 1953 analysis.[187] Dornbusch also testified to Congress in 1978, "I don't see an advantage in using monetary policy to stabilize the dollar."[188] In saying this, Rudiger Dornbusch, one of the leading open-economy macroeconomists emerging in the 1970s, associated himself quite closely with Friedman's pro-float position.

By the time of Dornbusch's testimony, the floating exchange rate regime had already passed a major test: the recycling of OPEC export revenues to oil-importing countries after the 1973–1974 oil shock. The floating-rate system continued in the United States in the 1980s, and efforts to create exchange-rate bands among major currencies in 1985–1988 petered out.[189] In

[186] Although Dornbusch (2001) gave a glamorized retrospective account of his experience with Mundell as a teacher-adviser, Stanley Fischer told the present author that this article's positivity did not gel well with Dornbusch's feelings on the matter in the 1970s. It is also worth emphasizing that Paul Krugman (1995, p. 88), presumably in part on the basis of his discussions of the matter with Dornbusch, gave the distinct impression that Mundell's economic analysis underwent a discontinuous shift in quality and content "around 1970."

[187] See Dornbusch (1978, p. 92).

[188] Testimony of March 7, 1978, in Committee on Banking, Finance, and Urban Affairs, U.S. House of Representatives (1978, p. 46). As already indicated, Dornbusch's domestic policy prescription was in a different direction from Friedman's at the time. Even at this stage, however, common ground among them was that a float should be permitted in order to allow monetary policy to concentrate on domestic objectives.

[189] In its second term, there was considerable dissension within the Reagan Administration on the matter of whether exchange-rate management was necessary, and there remained a considerable pro-floating faction, including among numerous members of or officials in the Council of Economic Advisers. One second-term CEA member, Michael Mussa, had no illusions about floating rates being volatile (see Mussa, 1986b), but he concluded (see Mussa, 1988, p. 30) that "the swings in [the] foreign exchange value of the U.S. dollar should not be viewed as negative or destabilizing developments." For further discussion of this period, see Chapter 17.

particular, this step did not lead to the post-float fixed-rate system that critics of Friedman's views on floating rates, including Mundell and Congressman Jack Kemp, had pushed for. As Friedman frequently noted, dissatisfaction with flexible exchange rates' performance did not result in the emergence of a proposed alternative system that could command strong support among economists or policymakers.

The main exception to the trend toward floating rates was the creation in 1999 of the euro area, with European monetary union proceeding despite Friedman's assessment that it should not happen and his prediction that it would not happen. In other parts of the world, however, a consensus in favor of floating cemented.

In one important case—the United Kingdom—support for floating, strong in the late 1970s, had by 1990 sufficiently dissipated that the pound sterling was fixed against the mark. But U.K. support for floating then became as firm as ever once the fixed exchange rate was dropped, in crisis circumstances, in September 1992.

The fact of a permanently floating pound also left an imprint on the presentation of the country's statistics on international payments flows. Hahn (1954, p. 400) had mocked Friedman for suggesting in his 1953 article that a float would cure the United Kingdom's balance-of-payments problem.[190] But all Friedman had meant was the analytically correct point that a balance-of-payments disequilibrium (an overall outflow or inflow of funds) cannot occur under a float.[191] Indeed, the series recording this disequilibrium (the so-called balance for official financing or settlements) had so faded in relevance in global economic discussions during the floating-rate era that it was eventually dropped from the U.K. balance-of-payments data tables.[192]

In another case, that of Australia, a central bank official, W.E. Norton, noted at a 1989 conference with regard to the prior five-and-a-half years' experience of a floating Australian dollar: "All told, in present circumstances, the floating rate regime has done much… to encourage both stability and efficiency."[193] That remained a widely held view in Australia. In Sydney in 1993, as the tenth anniversary of a floating national currency approached, Reserve

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[190] Hahn also incorrectly stated that Friedman's statements regarding the United Kingdom did not use "the slightest use of analysis or facts" (p. 400).
[191] In particular, he did not claim that a float would produce a zero current account balance. On Friedman's views on the current account balance, see Chapters 7 and 15 below.
[192] See Nickell (2006). See also the further development of the balance-of-payments tabulations for the United Kingdom given in Office for National Statistics (2014).
[193] Norton (1990, p. 167).

Bank of Australia economists were astonished when meeting the visiting Mundell to find him urging that the country fix its exchange rate against the U.S. dollar. The reception that Mundell's suggestion got was testament to the strong pro-float mindset in Australia, whose currency was of the last to move into fully flexible rates after the Bretton Woods era but, by the second half of 2023, had been floating without interruption for forty years.

The floating-rate system as it operated in practice defied suggestions that flexible exchange rates would give rise to slow growth in world trade or to increases in trade barriers. The case of Australia provided a particularly striking counterexample to such suggestions, as its floating-rate era saw substantial reductions in tariffs and other forms of protection. These reductions largely took the form of unilateral reductions. In other regions too, rates of protection also fell, although a frustration for Friedman was that unilateral removal of protection was rarely a favored course. His longstanding recommendation was for the United States to phase out its trade barriers unilaterally, and he disliked the notion that reductions in protection should or must come from multilateral agreements.

The international dimension—economics and geopolitics

Friedman's influence on international exchange-rate arrangements, and the prominence of his name in domestic economic policy debates abroad, were registered in William McChesney Martin's observation, in his already-discussed 1978 introductory remarks, that Friedman "is certainly a world figure, not just a national figure."

Friedman's status as a world figure did not mean he had a command over a wide range of aspects of world affairs. In fact, he did not. Because his name became so associated with Chile, on account of his 1975 trip there after the 1973 coup and also on account of the place of University of Chicago-trained economists in the Pinochet junta regime, Friedman could not escape discussing matters involving foreign policy.[194] And he sometimes brought up U.S. foreign policy and defense policy matters himself, including in 1975−1977 when he penned some *Newsweek* columns on geopolitical subjects.[195] But the fact was that foreign policy was not Friedman's forte. He had a very incomplete knowledge of historical experiences abroad. In addition, as a conceptual matter, foreign policy was not something with which he had an affinity. The economic perspective on international relations tended to focus on the scope for kinds of

[194] On this controversy, see Chapters 7 and 14 below.
[195] See Chapter 7.

cooperation between countries that could be delivered impersonally, and without central direction, by market forces' equilibration of demand and supply. The spheres of foreign policy and defense instead often involved focusing on different countries' areas of conflict and competing interests—and, in cases in which resolutions of disputes occurred peacefully, the process of negotiation was often stressed. Correspondingly, media accounts of foreign policy often highlighting the importance of particular personalities in international politics. This focus on personalities was the antithesis of economics' concept of the invisible hand.

Friedman was far more at home when discussing developments occurring abroad or U.S. relations with another country when these matters prominently involved economic issues. Of the various topics that came up in the post-World War II environment, a recurrent one was a major issue facing the postwar world and intersecting economics, ideology, and international politics. This issue was the choice of the national (and world) economic system that characterized the postwar contest between market arrangements and a Communist economic regime.

In the first forty-five years after World War II, the dispute over the desirability of market economies and a Communist system was a central element in the East/West geopolitical contest. But in the 1970s, at least, the dispute formed a nonnegligible part of academic debate on domestic economic arrangements in the United States. Early in the decade, the U.S. economics profession included a vocal minority—one gaining some ground during the campus protest era—that felt that economic teaching and research had paid insufficient attention to Karl Marx's economic writings and should embrace those writings as a means of analyzing the working of the modern-day American economy. It was during the height of this radical economics that the *New York Times* (January 25, 1970, p. 80) referred to "the Marxian or the Keynesian variety" of government intervention in the economy, as though both varieties were vibrant components of U.S. economic debate.

Friedman was not one of those who regarded Marxian thought as a worthwhile part of technical economic analysis. He did devote a small part of his Price Theory lectures in the 1950s (subsequently published in textbook form in 1962) to Marx's critique of the market system. But this discussion quickly concluded that "the Marxian theory of exploitation is logically fallacious."[196] In 1962, he referred to the "lack of validity of Marx's theory" as well as remarking

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[196] Friedman (1962c, p. 197). Friedman's original University of Chicago Price Theory class in 1946/1947 had the issue of Marx's analysis looming over it owing in part to the opening-up of the Cold War at the time but also to the fact that two of the class's students were enthusiasts for the Marxist critique of capitalism and were producing a newsletter at the time (George Tolley interview, November 14, 2014).

that "his scientific analysis and predictions have all been contradicted by experience."[197] Almost a quarter-century later, Friedman observed of Marx that "almost all his major predictions were falsified" and maintained: "he has contributed essentially zero to economics as a science. Marx was simply a follower of David Ricardo." (*American Banker*, April 30, 1986, p. 20.)

Friedman's long-established negative view of Marxian analysis meant that he was not impressed when fellow economists saw value in it. He got on well with Hirofima Uzawa, a departmental colleague of his from the late 1950s to the late 1960s. But he deprecated Uzawa's adherence to Marxian economics and confronted Uzawa in person about what Friedman perceived were flaws in Uzawa's published analyses. At a dinner, after "the pleasantries were over, all of a sudden Milton unloads and has all of these articles that Uzawa had written… with all these falsities" (Arthur Laffer, interview, June 10, 2013).[198]

Another clash with an economics professor who saw merit in Marx's analysis—this time one of a younger generation—occurred when Friedman was on Stanford University's campus in 1976 for an event alongside Duncan Foley. Mark Gertler recalled (interview, September 26, 2014): "I remember him giving, you know, a debate with my advisor, Duncan Foley. And, as much as I like Duncan, it was just no contest, because Friedman just had this disarming style—which was, you know, 'How can such a brilliant person like you think this way?' It's pretty hard to respond to that." After the debate, Friedman approached Foley and told him that he was wasting his time working on radical economics. A positive aspect of this that Foley noted was that, in the course of their debate, he had impressed Friedman enough for him to ask himself why Foley was taking the side he did. Foley was, however, also struck by the fact that, although Friedman himself had been in a small minority on key issues in economics in the past, he was discouraging Foley from carrying out his own attempt to change thinking on economics (Duncan Foley, interview, October 2, 2014).

In fact, as already indicated, Friedman did not regard the Marxist critique of orthodox economic analysis as productive. The one major item of Marx's economics that he accepted as an agreeable addition was terminological: for the most part, he was receptive to the word "capitalism" as a label for the market system, and he famously spread the usage of the term himself by deploying it in a book title.[199]

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[197] Friedman (1962d, p. 5).
[198] Laffer recalled the occasion for this exchange as being a dinner held at Mundell's house, with Uzawa, Mundell, and Arthur Laffer present, and taking place after Uzawa had left the University of Chicago but was making a visit.
[199] Chapter 9 discusses Friedman's usage of the term "capitalism."

Away from the debate on whether Marxism should play a significant part in U.S. academic economics, Friedman also made numerous interventions on the largely separate, and much grander, conflict concerning Marxism. This conflict spanned geopolitics and economics and was manifested in what he called the existence of "fundamentally different economic systems"—market economies in many parts of the world, and Communist regimes in the Soviet-dominated East bloc as well as in China.[200]

In the initial decades after World War II, especially in the late 1950s and early 1960s, there was a widespread perception of the Communist bloc, and the USSR in particular, as an economic powerhouse. The Soviet Union's rapid growth rates were seen by many as largely the reflection of productivity advances well in excess of those being achieved in the United States. The USSR was portrayed as set to catch up with and surpass per-capita U.S. income levels. Friedman was a consistent and withering dissenter from this perception (see Nelson, 2023). His assessment, a minority one at the time, would become the conventional wisdom after others' detailed economic analyses, appearing in the mid-1960s and later, revealed the very limited degree of the Soviet Union's catch-up. And, starting from a comparatively low level of per-capita output, the USSR's own economic-growth performance deteriorated in the 1960s and 1970s. In the mid-1980s, Vigor (1986, p. 120) was relaying a widely-accepted fact when he observed: "the West, and especially the USA, is far superior to the USSR in technological inventiveness and in the bringing of it to fruition in the factory."

To Friedman, the differences in technology, productivity, and living standards were traceable to the choice of an economic system. Centralized economic arrangements had locked the USSR and other Communist economies into a low-income state. "That is why those countries have such low standards of living; that is why they are so inefficient," he remarked in March 1981 of the Soviet Union and China.[201] Friedman believed that U.S. and U.K. historical evidence confirmed that there was a strong link between the presence of free markets and the technological innovation associated with productivity growth (see Chapter 6 below).

The Berlin Wall symbolized, for Friedman and many others, both the economic and political differences between the Communist and market systems. "I wanted to show you how much difference it [the difference in systems] makes by letting you see how the people live on the other side of that Berlin Wall," he told viewers of the *Free To Choose* series in a segment filmed

[200] The quotation is from *Free To Choose* (U.S. television version), PBS, Episode 9, "How To Cure Inflation," March 7, 1980, p. 6 of transcript.
[201] Friedman (1984h, p. 8).

in the Federal Republic of Germany. "But the East German authorities wouldn't let us." Pointing to the other side of the Berlin Wall, he observed: "The people over there speak the same language as the people over here. They have the same culture. They have the same forebears. They are the same people. Yet you don't need me to tell you how differently they live."[202] Subsequently, the Berlin Wall's fall in 1989 symbolized the breakup of the USSR-led European Communist bloc.

Later still, Friedman would be credited with being prescient about the dissolution of the Communist bloc: "Through the 1980s[,] he predicted the collapse of Communism," one account claimed (*Sunday Times* (London), September 8, 2002). This attribution was, however, an exaggeration. As already indicated, Friedman was indeed consistently among those who doubted the East bloc's capacity to make economic advances. But he was not in the front line in predicting the actual breakup of the Soviet bloc or the collapse of the USSR. In mid-1983, Sovietologist Robert F. Byrnes, an Indiana University historian, was quoted remarking, "We don't see any collapse in the Soviet system or any weakening," other than ongoing economic lethargy (*San Antonio Light* (Texas), June 14, 1983). This mainstream judgment largely corresponded to Friedman's perspective, too. He saw it as quite likely that the system would soldier on, in a low-income state, with rule over the satellite states, and over the USSR itself, maintained by the Soviet state's military strength and its other coercive powers.[203]

The Soviet Union's accumulation of military forces, together with the ideology of the regime, made Friedman take the USSR seriously as a geopolitical opponent, despite his low evaluation of its economic system. He had considerable concern about the prospects of a USSR-investigated world revolution or an upsetting of the balance of power in Europe and in other regions of the world in favor of the Soviet Union. Fears of such eventualities, along with the Soviet military buildup, eventually made Friedman largely side from 1972 onward with the hawkish critics of the defense and foreign policies of the Nixon Administration and, especially, of the Ford Administration (see Chapter 7).

But, although Friedman shared hardline defense figures' critique of the 1970s policy of U.S./USSR détente, he had already split with them on many issues that had a strong economic dimension. The most prominent of these was conscription: Numerous hardliners in the 1970s

[202] *Free To Choose* (U.S. television version), PBS, Episode 9, "How To Cure Inflation," March 7, 1980, p. 6 of transcript.
[203] President Ronald Reagan himself wrote in notes in September 1984 (Anderson and Anderson, 2009, p. 165): "There are differences between our two political and economic systems[,] and I don't think either one of us will change." See also Chapter 15 below.

and early 1980s, including the writers of the *Wall Street Journal* editorial page, wanted a return of the draft. But Friedman believed that his own longstanding opposition to conscription, voiced often during the Vietnam War (see Chapter 4), remained sound. Against the opposition of many military officials, he and others had succeeded in securing the end of the draft in 1973. Despite the later criticism, the decision was maintained. Asked in the weeks after the attacks of September 11, 2001, whether the United States would resume conscription, Secretary of Defense Donald Rumsfeld observed: "I cannot at the moment foresee that… I was one of the original supporters and promoters of the all-volunteer army, along with a couple of strange bedfellows, Milton Friedman and Norman Thomas, back in the 1960s. I think it was the right decision. We've got a great set of volunteers who get up every morning and voluntarily put their lives at risk." (*Meet the Press*, NBC, September 30, 2001, p. 8 of transcript.)

To Friedman, conscription should be seen in economic terms—as a tax. Friedman was scornful of defense hardliners for not seeing it in these terms and felt that, being steeped in the world of the armed forces, they were too inclined to see the United States' strength in terms of the state's command over resources. In this connection, Friedman believed that the consumer sovereignty of the U.S. and other market economies was a source of strength over the Eastern bloc, not a source of weakness, because it added to overall output and so made any allocation of the nation's resources to defense less of a burden on the overall economy than it would otherwise be. His perception that hawks did not have a good command of economic issues was reinforced by the fact that one of the leading hardline forces to emerge in politics, public-policy debate, and journalism in the 1970s—the neoconservative movement—had as a prominent proponent Senator Henry Jackson. Jackson was an interventionist in domestic issues, both on macroeconomic management and on U.S. oil pricing (see Chapter 6).

Senator Jackson and others were also much more well-disposed than was Friedman toward using trade measures, including restrictions on access to the U.S. market and on the sale of U.S. products, as a weapon against the Soviet bloc. Friedman was generally against economic sanctions, and he was highly critical of the grain embargo imposed on the USSR by President Carter in 1980 (see Chapter 10). As indicated, Friedman judged the USSR economy to be inherently weak, on account of its internal economic policy, and believed that trade restrictions would only make a marginal difference to Soviet economic performance.

In office, the Reagan Administration did not, in fact, pursue a concerted policy based on trade restrictions against the USSR. Anderson and Anderson (2009, p. 86) quoted Reagan as writing in his diary in December 1981 that he "would quarantine the Soviets and Poland with no trade."

But Reagan, in the event, did not pursue such a policy. As Friedman's former coauthor and colleague, Allen Wallis, an official in the U.S. Department of State under Reagan, noted in public remarks, the administration did not have a policy of an economic war against the USSR.[204] The administration did, it is true, have a sporadically pursued policy of restricting strategic exports to the USSR. These strategic exports were likely defined more broadly than Friedman (who *was* agreeable to the restriction on export to the Communist world of high-technology U.S. products) would have liked. But the administration was criticized for applying these restrictions more strictly to its allies than to itself, and the policy was, eventually, largely dropped (see Elliott, 1988, p. 81).

The administration followed a more consistent line in pursuing a policy that Friedman strongly supported: a continuation and expansion of the U.S. defense buildup begun under Carter. This policy, along with the—partly related—weakening of the USSR economy, would produce dividends in the late 1980s in the concessions made in U.S./USSR negotiations by Mikhail Gorbachev's Soviet Union. At a November 1973 conference, a U.S. defense expert had observed: "it is hard to imagine that the Soviet Union would reduce significantly its MR/IRBMs [medium-range/intermediate-range missiles]… unless these reductions were paralleled by some constraints on British and French strategic nuclear delivery vehicles" (Coffey, 1974, p. 127). In the event, Reagan did secure, in 1987, agreement on the Soviet Union's part to eliminate its IRBM fleet (and without constraints on the French or U.K. forces featuring as a *quid pro quo*). This agreement occurred after the United States had, earlier in the decade, implemented a plan to place its own intermediate-range nuclear missile fleet in Western Europe, including U.S. missiles placed in the Federal Republic of Germany in 1983 when Arthur Burns was the American ambassador to that country.[205]

The installation of missiles represented the kind of emphasis on supplies of military hardware—rather than reliance on overseas deployment of troops—that the non-expert, but opinionated, Friedman felt was the direction in which the United States should go in its contribution to the Western alliance. Indeed, while Rayack (1987, p. 176) asserted that Friedman "clearly… would support the Reagan military posture," Rayack neglected public comments that Friedman made during the Reagan years that confirmed that his position was less straightforward and more idiosyncratic. In contrast to the position taken by successive administrations, including Reagan's, Friedman's position, as related in early 1982, was that the United States should have

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[204] See Chapter 15.
[205] Again, see Chapter 15.

military alliances, but should not have troops deployed in Western Europe, whose countries he felt should be relied on to assemble their own adequate level of armed personnel.

Friedman related this position in an interview with conservative journalist William F. Buckley, Jr., who, being a journalist-commentator who covered a wide range of subjects, was accustomed—in contrast to Friedman—to writing on a routine basis about *both* the subjects of economics and foreign policy. Buckley and Friedman were longtime friends. But their views by no means fully overlapped—with Buckley being a more straightforward Republican party conservative and far less libertarian than Friedman, as well as being (at least until the Nixon price-controls experience disillusioned him) very inclined to embrace wage-push interpretations of inflation. But, both being national columnists who were on television often, as well as occasional confidants of Ronald Reagan, Buckley and Friedman tended to be treated as subscribers to the same line of thinking.

Reflecting this situation, after a Reagan-supporting, pro-free-market high school student, played by Michael J. Fox, was introduced as one of the central characters in the series *Family Ties* in the 1982/1983 U.S. television season, Fox explained of the character, "He's an admirer of William Buckley and Milton Friedman," while making clear to the interviewer that he did not share the views of the character (Alex Keaton) he was playing nor Keaton's interest in economics (*The Sunday Herald and Review* (Decatur, Illinois), January 23, 1983).[206]

The public celebrity

The fact that Fox mentioned Friedman and Buckley in the same breath reflected the status of both as celebrity commentators. In Friedman's case, his celebrity had emerged in the middle 1960s as his monetary policy work made more public impact and his public-policy activity intensified via the 1964 Goldwater campaign and then, on a sustained basis, via his *Newsweek* column that started in September 1966. Eighteen months after he started the column, a profile of Friedman, whom it referred to as "This little giant," noted of him, "His name is constantly before the public." (*Chicago Daily News*, March 26, 1968, p. 37.) That would remain the case in the 1970s, a decade in which Friedman was a multimedia presence. Notably, although his *Free To Choose* television series was seen, with some justification, as something for which the ground

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[206] Including his middle initial, the character's name was Alex P. Keaton. This longer name, which was used frequently in the series, likely was intended to reflect the frequent invocation of the middle initial that took place in public discussions of William F. Buckley. The emphasis on the middle initial marked, however, a difference in the character both from Ronald Reagan (who had a middle name but who tended to avoid using his middle name or initial) and from Friedman (who had no middle name).

had been laid by John Kenneth Galbraith's 1977 television series *The Age of Uncertainty*, Galbraith himself remarked when his own series began broadcasting that Friedman had (through many interviews on news programs) had had ample television exposure and now it was Galbraith's turn (*The Guardian* (London), January 8, 1977).[207]

Friedman's own *Free To Choose* series was a bigger success than Galbraith's. But although its U.S. viewership was high by the standards of the public television stations on which it was broadcast, it was not a high-rating or mass-viewer series by the standards of the major American television networks. In contrast, Milton and Rose Friedman's tie-in *Free To Choose* book was an unambiguous national phenomenon, being on the U.S. bestseller lists in 1980–1981.

The success of the *Free To Choose* project further increased Milton Friedman's national profile. For Rose Friedman, this development had a negative side, associated with the personal danger to which celebrities were exposed. Gloria Valentine recalled the "only time I saw Mrs. Friedman disagree with him, and it was on a small thing. A gift came at Christmas, and he opened it, and there were cans of peanuts. And he opened one and started eating, and she said, 'You don't know who that was from,' and so there was a big argument, sort of heated, with her—and I thought she was going to hit him. She tried to get him to stop eating the peanuts, because she didn't know. I knew, but, of course, I couldn't get into the conversation except to say the man has sent gifts before. That's all I could say. I can't remember the name of the person, but each year, he did send peanuts. They were usually [addressed to] the office. That time, I guess he got the home address and sent them to the home. And Mrs. Friedman was very upset because, you know, there were cranks around, and threats to his life. So I can understand where she was coming from. The only thing was I knew that this man had been doing this for five or six years, and she didn't know that." (Gloria Valentine, interview, May 7, 2013.)

Despite the boost to his profile arising from *Free To Choose*, Friedman implied that both John Kenneth Galbraith and John Maynard Keynes, who had each preceded him as a celebrity economist, had achieved greater fame than he had (see Chapter 11). But although Friedman was willing to put Galbraith and Keynes on the same plane as each other as celebrities, there was a world of difference between them, in his view, in terms of their contributions as economists. Despite their personal friendship and his respect for Galbraith's writing ability, he did not regard Galbraith as influential on technical economics. To Friedman and many other economists,

[207] Galbraith may have been referring specifically to a spate of media appearances that Friedman made, particularly on U.K. television, in late 1976 (see Chapter 9).

Galbraith's status as a professor of economics at Harvard University and his media profile conveyed an image of a leading economic researcher that did not meet the reality.[208]

In marked contrast, Friedman observed of Keynes: "Now, I believe Keynes was a great man. He was a great economist. But I think his theory is wrong."[209] "I know Milton Friedman was always very antagonistic to the Keynesian system," Phillip Cagan, who had known Friedman virtually from the time of his becoming a monetarist, remarked (interview, January 13, 1992). But as Friedman's University of Chicago colleague George Tolley observed of him (interview, November 14, 2014): "he said, 'Even though I don't believe [in] it, every graduate student should know about Keynesian economics.'"

As has been stressed above, Friedman made a lot of ground in replacing Keynesian economics. He credited his success on this score to what he at least regarded as "the obvious conflict of experience with the Keynesian view and the consistency [with] the monetarist view" (*American Banker*, April 30, 1986, p. 20). Notably, however, the aspects of his views most closely concerning monetary aggregates reached a peak of acceptance—in the economics profession and in policy circles—during the early 1980s. They then lost support, among both camps, over the remaining quarter-century of his life.

Some of the backlash against monetary quantities that was seen in the 1982−2006 quarter century involved a rethinking of the relative merits of monetary aggregates-based and interest rates-based approaches to the analysis and formulation of monetary policy. As this rethinking resulted in an increased focus on interest rates, it was arguably a throwback to Keynes. It is notable that Patinkin (1984, p. 98) argued: "what Keynes meant by monetary policy is not what today's monetarists mean by it. For what Keynes always meant by this term was not a policy of controlling the quantity of money, but one of controlling the rate of interest."

Patinkin was oversimplifying, as some analysts viewed Keynes as too willing on occasion, notably in the *General Theory*, to model the monetary authority as setting the stock of money rather than an interest rate.[210] But Patinkin was essentially correct that Keynes often treated the central bank as setting the short-term interest rate and made many recommendations that involved the pursuit of an interest-rate policy.

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[208] The U.S. economics profession reinforced this image by having Galbraith as the president of the American Economic Association in 1972.
[209] CSPAN, November 20, 1994, p. 14 of transcript.
[210] See, for example, Flemming (1993). In the same vein, Cagan (1990, p. 696) observed: "Keynes views on money supply… turn out to be mixed."

For his part, Friedman did not actually disagree that central banks were prone to manage interest rates. But, in contrast to Keynes, he regarded it as a bad thing—as his and Rose Friedman's reference in 1980 to "the Fed's obsession with interest rates" attested.[211] His contention was not that actual monetary policy targeted series such as the monetary base or total bank reserves. Instead, a key objection to central banks' practices was precisely that they did *not* do so. And he insisted that they should.[212]

By the early 1970s, Friedman had largely prevailed in the argument about monetary indicators. He and other monetarists had suggested that the difficulty of distinguishing between real and nominal interest rates, as well as fluctuations in the natural values of real interest rates, meant that the behavior of monetary aggregates provided a more reliable indication of monetary policy stance than did interest rates.[213] The beginning of U.S. monetary targeting in 1975 (see Chapter 5) was a reflection of monetarists' considerable success in changing the balance of opinion on monetary indicators.

By the end of the 1970s, monetarists also appeared to have been victorious in the debate on whether the means of targeting monetary aggregates should be by control of a reserves quantity. In practice, however, the conversion of U.S. policymakers to this position pleased Friedman little, in part because the reserves aggregate that they focused on was not his usual recommended policy instruments (see Chapter 10).

Furthermore, the Federal Reserve's conversion to a reserves-oriented operating procedure proved short-lived, ending in 1982. By the mid-1990s, there was openness on the part of central bankers—in the United States and elsewhere—about their use of a short-term interest rate as an instrument, and there was considerable approval of this practice (though not by Friedman) on the part of economists. In addition, monetary-growth targets had been essentially abandoned by this point. It was clearly evident that monetary targeting, and reserves or base control as the means of targeting monetary growth, were ideas whose professional and policymaker acceptance had peaked then declined as Friedman aged from his late sixties to his mid-nineties.

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[211] Friedman and Friedman (1980, p. 267).
[212] For further discussion, see Nelson (2020b, Chapter 9) and Chapters 5 and 10 below.
[213] See Chapter 18 for further discussion. The monetarist position was consequently that interest-rate rules aimed at achieving price stability were, in theory and in principle, attainable but that successful versions of these rules were not likely to be attainable in practice. A late expression of this view by a monetarist was Cagan's (1989b, p. 167) statement that "a procedure of automatically putting the interest rate up or down by a certain amount whenever a selected price index moved so many points away from stability… would be unsatisfactory, because whatever magnitude and frequency of adjustments in the interest rate were prescribed for a given change in the price index would not be appropriate under all circumstances."

Was the victory of short-term interest rate policies a victory of Keynes over Friedman? Only in limited senses. The short-term interest rate was certainly playing a role in an overall activist policy arrangement—one that was in part geared to real economic stabilization. In these respects, Friedman's ideas had been rebuffed. But the interest-rate policies being pursued, and those analyzed by researchers including John Taylor (1993), in the 1990s and 2000s were predicated on the notion that monetary policy actions provided a necessary and sufficient condition for controlling inflation.[214] In contrast, Keynes had by the mid-1930s come to a largely nonmonetary view of inflation.[215] And, as far as stabilization of real output was concerned, saw the contribution that monetary policy could make as corresponding to what Hansen (1938, p. 27) called the "relentless maintenance" of fixed interest rates across the maturity structure—a very different interest-rate rule from those that would be explored in the 1990s by Taylor and others.

Friedman had common ground with the New Keynesian literature's approach to monetary policy in a further respect. Unlike Henry Simons in the previous generation, and unlike also his former student Phillip Cagan, Friedman did not have a negative perspective toward financial deregulation. He also had no fundamental fear that financial deregulation or financial innovation undermined monetary policy.[216] In contrast, Cagan despaired at the degree to which new financial instruments were proliferating: "I really do not see attractive alternatives to some kind of regulation" in order to stem the tide, Cagan (1986b, p. 54) argued, while stressing that he shared Henry Simons' view that financial innovation would critically undermine monetary control. "He would be appalled at what he would see today!," Cagan (1989b, p. 171) remarked of Simons, underlining the lineage between their views. And with respect to conditions in which a new set of extensive financial regulations was not imposed, the events of the 1980s shook Cagan's confidence in the constant-monetary-growth rule. Cagan (1990, p. 697) declared that "there are few proponents of strict monetary growth rules anymore." He later elaborated (interview, January 13, 1992): "The old Friedman prescription, … the old idea of constant

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[214] Chapter 18 below discusses the Taylor rule.

[215] As was indicated above, this was also the conclusion being advanced by Frank Hahn in his attempts in the early 1980s to develop a theoretically rigorous critique of monetarism's account of inflation. The New Keynesian literature did not share this conclusion. Notably, Woodford (1995, p. 40) referred to "the importance of the service that quantity theorists such as Milton Friedman have rendered to contemporary discussion of issues of macroeconomic policy"—with Woodford citing such service as having included a stress on the dependence of inflation on monetary policy and on the importance of stable policy rules. Perhaps alluding to the earlier Hahn-type critiques of monetarism, Woodford added: "Criticisms of… quantity-theoretic reasoning have sometimes been supposed to cast doubt upon these more fundamental commitments of the quantity theorists as well, but nothing that I have said here should be taken in that light."

[216] Friedman did believe that some aspects of U.S. financial innovation had taken place in an unnecessarily disorderly way because banks had been restricted by regulation from taking the lead in promoting new financial products, and because the course of financial innovation had been heavily shaped by the country's experience of high inflation.

growth in the money supply, while it might not be *too* bad, isn't going to be as good as originally thought. So people who were never enamored with the idea in the first place can say, oh, that idea's no good."[217]

Friedman's view was very different. He was confident that an effective monetary policy could take place in a deregulated world. He welcomed financial deregulation—such as the removal of many of the Regulation Q ceilings on retail-deposit interest rates in the 1980s. He also did not think that the emergence of a deregulated world could preclude quantity-focused monetary policy operating procedures and monetary targeting—although the 1990s did see, as already indicated, acknowledgment on his part that the relationship between M2 and the economy had shifted since 1992. He and the New Keynesian literature had parted ways on the merits of monetary aggregates. But there was fundamental common ground among New Keynesians and himself that central banks' monetary policy powers did not derive from, or rest on, the existence of financial regulation and that monetary policy determined inflation whether the financial system was regulated or deregulated.

This common ground stemmed from the New Keynesian literature's continuing agreement—albeit, owing to that literature's focus on interest rates, largely unspoken—with Friedman's contention that "high-powered money is the key monetary tool available to governments."[218] The monopoly power on the supply of reserves to the banking system underlay central banks' influence on interest rates, monetary aggregates, and national spending and price aggregates.[219] Friedman held that such monopoly power was not seriously threatened in a deregulated world. And he believed this to be a desirable situation. Here he disagreed with fellow free-marketer Friedrich Hayek, who wanted the private sector to become a supplier of base money. Friedman was loath to get into a public argument with Hayek. But he made clear his disagreement by publishing, in the mid-1980s, a series of skeptical remarks about Hayek's arguments, including

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[217] Cagan did not, however, repudiate the logic of money-multiplier analysis. He was critical of the tendency of some work by academics and central banks that seemed to imply that central banks had no control, at any time frequency, of the quantity of reserves or that reserves were irrelevant to commercial banks' aggregate balance sheet decisions. Of some of these writings, Cagan observed (interview, January 13, 1992): "You can make sense of what they're saying, I suppose—it's just that they're bending over backwards to take [that is, express their position as] a very anti-monetarist view." See Cagan (1990, 1993) for his relevant later writings.
[218] Friedman (1988, p. xxiv).
[219] Friedman was emphatic on the point that the demand for reserves would be positive and would be related to bank deposits even when deregulation included zero reserve requirements. Hence his statement (Friedman, 1960, p. 88) that the stock of money was a variable "that the monetary authorities can effectively control and for which they have primary responsibility" did not depend on the existence of reserve requirements. For further discussion, see Nelson (2020a, Chapter 4; 2020b, Chapter 14). López-Salido and Vissing-Jorgensen (2023) have recently found a continued dependence of U.S. banks' demand for reserve balances on their issuance of deposit liabilities.

in a paper with Anna Schwartz that affirmed the public sector's role in managing money.[220]

Bequeathing ideas

Friedman frequently indicated that he would regard his career as having been a success if the ideas that he had developed were remembered among rank-and-file members of later generations of the economics profession. He cited this criterion repeatedly in 1976–1977 in the aftermath of his Nobel award. And in April 1986, in an interview that consisted largely of a career retrospective, he reaffirmed (*American Banker*, April 30, 1986, p. 20): "It is a matter of how useful the work I have done turns out to be future generations of economists."

A further near-decade on, in November 1994, another career-focused interview seemed to provide a negative verdict with respect to this criterion. The interviewer told Friedman that he had recently talked to someone who had received an MBA degree at Stanford University's Graduate School of Business had not heard Friedman mentioned once in the degreee's courses in the late 1980s and early 1990s. "I can believe that," Friedman replied matter-of-factly. He then went on to explain the outcome by suggesting that U.S. academia was inclined to downplay his contributions because of his free-market views.[221]

This response contrasted with other occasions in the 1990s in which Friedman expressed pleasure at the survival of his views in monetary economics. His resigned response at hearing about the Stanford business school's alleged neglect of his work may have reflected an instinctive reaction, in light of the ups and downs in the relationship between Hoover Institution and Stanford University. It may also have reflected the lack of affinity that Friedman had with some of the material taught at business schools, as he had little appreciation for the field of finance.[222]

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[220] See Chapter 14. Friedman's reluctance to become very overt in his criticism of Hayek in this area was doubtless shaped by his deep respect for Hayek's writings on other matters concerning the market system. His esteem for Hayek's writing on markets covered Hayek's (1945) paper as well as Hayek's popular writings. In connection with the former, Friedman observed, in a March 1981 talk, when touching on the information-signaling feature of prices: "This is a function whose crucial importance tended to be neglected until that great article which Fritz Hayek wrote shortly after the end of World War II in 1946 or 1947 or something on prices as a means of transmitting information." In the published version, this was corrected to: "The crucial importance of this function tended to be neglected until Friedrich Hayek published his great article on 'The Use of Knowledge in Society' in the *American Economic Review* in 1945." (Friedman, 1984h, p. 9.) But the fact that Friedman initially was vague on the date may have been a result of a strong memory of reading and being impressed by the article in the 1940s (a period that, by the early 1980s, probably survived in Friedman's memory only in the form of snapshots and lasting impressions).
[221] CSPAN, November 20, 1994, p. 10 of transcript.
[222] On Friedman's attitude to the field of finance, see Chapter 4.

It is plausible enough that the macroeconomics courses of Stanford University's business school did not refer to Friedman by name by the late 1980s. Standard macroeconomics courses routinely used concepts that Friedman had advanced—like permanent income and the natural rate of unemployment—but his name was not built into these concepts as, for example, A.W. Phillips' name was in the case of the Phillips curve.

It is, unlikely, however, that Friedman went altogether unmentioned in the business school's courses on corporate practices. Although it was a popular work, Friedman's 1970 piece on the social responsibility of business became essentially a permanent part of readings in business courses, albeit usually because it articulated a view that the courses came out against. Friedman largely pulled out after 1973 of the heavy debate that took place among executives and business researchers with regard to Friedman's views on corporate social responsibility.[223] One rare latter-day occasion on which he participated in these discussions was actually at Stanford University's business school. He appeared on a panel on "An Ethic for the Global Economy," held on May 20, 2000, as part of the business school's seventy-fifth anniversary celebrations.

Even aside from his influence on the content of business-school and macroeconomics courses, Stanford University's business school seems in retrospect a particularly poor example of an institution to point toward as one that did not refer to Friedman. For it proved to be the case that Nobel Prize-winning economic research emanated from Stanford business school that was explicitly presented as building on Friedman and Schwartz's work in the *Monetary History*. Specifically, Ben Bernanke was at the business school in the early 1980s when he proposed a bank-loans channel as an addition to Friedman and Schwartz's monetary-contraction channel in the propagation of the forces generating the Great Depression.[224] This research was presented in the post-Friedman money workshop at the University of Chicago in 1982 and was published in the *American Economic Review* in 1983. Later, it would be the main basis for Bernanke's Nobel award in economics in 2022.[225]

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[223] See Chapter 4.
[224] Temin (1989, p. 161) took Bernanke's (1983) work as one in which it was "not the growth of the money stock" that mattered for the Great Depression but, instead, the behavior of bank loans. In fact, Bernanke explicitly advanced his loans-based account as a complement to, rather than a replacement for, the Friedman-Schwartz money-focused explanation. Steindl (2004, p. 44) correctly cited the related work of Bernanke (1995) as having stressed the importance of money in the Great Depression. For example, Bernanke (1995, p. 25) stated that "the evidence for monetary contraction as an important cause of the Depression, for monetary reflation as a leading component of recovery, has been greatly strengthened." But Bernanke also emphasized money's importance for the understanding of developments from 1929 to 1933 in numerous writings from 1982 to 1992, and he did so again in 2002: see Chapter 18 below.
[225] Bernanke gave a talk titled "The Financial Collapse As a Direct Cause of the Great Depression" to the money workshop on February 23, 1982 (University of Chicago library records).

In the 1990s, Bernanke's research focused on the formulation of monetary policy in modern-day circumstances. It was after this body of work appeared that he switched from being a researcher to being a practitioner, becoming a Federal Reserve Board member in 2002. Bernanke then succeeded Greenspan as Federal Reserve chair in Friedman's last year of life, in 2006. Before that, in his years as Board governor, Bernanke had given speeches at two separate conferences (in 2002 and 2003) held specifically in Friedman's honor.[226] In each case, Bernanke speech discussed the impact of Friedman's monetary work on the thinking of researchers and of figures in the policy world. Friedman appreciated the speeches but did not make detailed comments on them—reflecting his view that, when it came to assessing what his legacy was, "I really think that's for somebody else to say." (*Pittsburgh Tribune-Review*, April 7, 2001.)

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[226] Again, see Chapter 18.

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 2: Monetary Policy and Public Policy Debates, 1973 to 1974

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Federal Reserve Board
July 28, 2023

I. EVENTS AND ACTIVITIES RELATED TO MONETARY POLICY AND PUBLIC POLICY DEBATES, 1973–1974

Milton Friedman had two long absences from the University of Chicago during 1972 and early 1973: January-September 1972, and the first half of December 1972 through mid-February 1973.

The first of these absences involved teaching at the University of Hawaii in the early months of the year, then other activities in the Pacific, as well as guest lectures in Israel, given in April, on monetary topics. Several months of further engagements, primarily in North America, followed, from the base of the Friedmans' Vermont summer home.[2] Friedman also attended, and presented at, the Mont Pelerin Society meetings in Switzerland in September.

On his return to the University of Chicago after these activities, Friedman resumed hosting his Workshop on Money and Banking. He did so as his recent debate with fellow monetary economists was being unveiled to the public, appearing as a "Symposium on Friedman's Theoretical Framework" that made up much of the *Journal of Political Economy*'s Special Issue on Monetary Theory (September/October 1972).

As it happened, just as the special issue saw print, one of Friedman's sparring partners in the symposium—Don Patinkin—was on hand to attend Friedman's workshop. Patinkin served at the University of Chicago during the Fall Quarter of 1972, as Ford Foundation Visiting Research Professor of Economics (Patinkin, 1973, p. 787). A graduate student who was also visiting the

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[1] Email: Edward.Nelson@frb.gov. The author is grateful to George Tavlas for comments on an earlier version of this chapter, as well as for the feedback given by participants in a seminar at the University of California, Berkeley, at which some of the material in this chapter was presented. See the Introduction in Nelson (2020a) for a full list of acknowledgments. The views expressed here are the author's alone and should not be interpreted as those of the Federal Reserve or the Board of Governors.
[2] That is, the Friedmans' second home in Ely, Vermont. (Although Leeson, 2000, p. 741, described this as Friedman's "holiday home," it was hardly that, as Friedman by his own account did much of his research work there.)

University of Chicago at this time, John Rutledge, himself attended the workshop and was struck by the stellar quality of Friedman and the other participants. "What I remember is that fall, him having just had his sixtieth birthday [on July 31]. And I think that he had heart surgery just after that; not while I was there, but after I had gone. [In the Fall semester] it was that incredible group of people. This was the money and banking workshop that met Tuesday afternoons. And I was, of course, a kid. (*laughter*) And in that room, Friedman was at the head of the table. In terms of just some of the guys in the room, [there was] Don Patinkin, Robert Gordon, Stanley Fischer, Robert Barro…" Others Rutledge remembered among the attendees included Fischer Black, Don McCloskey (later Deirdre McCloskey), and Jeremy Siegel. "And there were several more, but it was all around one table… So it was an extraordinary roomful of people."

One of the papers presented at the Fall 1972 workshop sessions was by two economists from the business school, John Gould and Charles Nelson (the latter a fixture at the workshop during the first half of the 1970s). The bottom line of this paper—titled "The Stochastic Structure of the Velocity of Money"—was that the time-series behavior of the log of U.S. M2 velocity resembled a random walk. The Gould-Nelson paper had been drafted during the summer of 1972 and sent to Friedman at that time. From Vermont, Friedman had composed a highly skeptical response in the form of a letter to Gould, which his secretary, Gloria Valentine, typed up in Chicago on August 7.[3]

It was against the background of this correspondence that, prior to the workshop presentation of the paper, Gould spotted Friedman and Fischer in a campus cafeteria. "It was during a summer period that some of that correspondence had gone on… Now it was the Fall quarter, and the university was kind of gearing up; Friedman was back and Fischer was around, and I happened to be having lunch… I happened to look over and see Friedman and Fischer having lunch together at another table. So, when we were finished with lunch, I went over to say "Hello, welcome back," and everything else, and we actually fell into a discussion about the paper—about the velocity paper, and the possibility that velocity was a random walk." When Fischer suggested that velocity could not be a random walk, Gould was surprised to find Friedman defend the other side of the argument—that velocity might be a random walk. "And so Friedman immediately turns on Fischer and says, 'Oh no, there are quite a number of reasons it could be a random walk,' and he proceeded to give the argument on the other side. And that was Friedman—the quintessence of Friedman: It was economics he was interested in, and he could

[3] A copy of the letter was kindly supplied to the author by John P. Gould and Charles Nelson.

go either side of the argument, and really get some insight out of each." (John P. Gould, interview, March 20, 2015.)

Friedman's thinking on velocity during 1972 was shaped by his finding with Schwartz—reported by them in that year's NBER *Annual Report*—that twentieth-century movements in U.S. M2 velocity often mirrored those of broad-money velocity in the United Kingdom.[4] Friedman and Schwartz took the similarity across the two economies of annual fluctuations in velocity as implying that the factors driving velocity in each country also moved together. They were therefore fortified in their view that velocity's behavior was traceable to economic variables. Friedman articulated this interpretation of U.S. velocity behavior in his August 1972 correspondence with Gould, in which Friedman mainly accepted the premise—common at the time—that a finding that a macroeconomic series was a random walk made the series economically uninterpretable. However, even this letter allowed for the possibility that random-walk behavior of the logarithm of velocity could be consistent with it being an economically-meaningful series, provided that the economic drivers of velocity included random walks. This possibility was also likely what Friedman emphasized when he acknowledged the economic plausibility of velocity being a random walk in his Fall 1972 discussion with Gould and Fischer.

In retrospect, this discussion also points up the fact—noted by Stock (1988) in reference to the case of the consumption function—that Friedman grasped the essential concept of cointegration well before that concept was formalized by Engle and Granger (1987). In particular, Friedman realized that it was possible for the logarithm of M2 velocity to be a random walk, yet for the money demand relationship, expressed in levels form, to have long-run stability; that is, for the nonstationarity (stochastic trends) to cancel in the overall relationship linking log money (log M2), log prices (log P), log real income (log y) and other variables. More specifically, under the setting of a unitary income elasticity of real money demand—a parameterization that Friedman found empirically and analytically attractive by the early 1970s—the logs of real money and log real income were part of a cointegrating vector with coefficient 1.0 on log real income, and log velocity, $\log P + \log y - \log \text{M2}$, could depend on an opportunity-cost variable (usually an interest rate) that was a random walk.[5]

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[4] See Friedman and Schwartz (1972).

[5] McCallum's (1993) defense of the quantity theory of money considered another possibility—that financial innovation produces a difference-stationary money demand shock. On this interpretation, the absence of a cointegrating levels relationship between money, income, and opportunity-cost variables like interest rates is consistent with the quantity theory of money, which McCallum suggested should be thought of as primarily offering propositions concerning relationships between growth rates series.

This possibility was also allowed for in the published version of Gould and Nelson (1974), which discussed how a random-walk process for the log of M2 velocity could be consistent with the existence of the monetary relations that Friedman and Schwartz espoused, while also considering situations in which a random-walk finding for velocity could be inconsistent with those relationships. Likewise, Anna Schwartz's (1975, p. 154) consideration of Gould and Nelson's (1974) findings highlighted an interpretation of Gould and Nelson's results in which nonstationary velocity behavior occurred in the context of a stable money demand relationship (that is, a constant-parameter long-run money demand relationship with a stationary error term), so long as "the determinants of velocity… that we have isolated are also random walks."

But, although Friedman accepted the possibility that log M2 velocity was a random walk, and while he and Schwartz were able to reconcile it with their own positions, he did not in fact believe that the random-walk characterization was accurate. Indeed, in *Monetary Trends* in 1982 Friedman and Schwartz expressed the view that their own results for the United States, covering the period through 1975, offered decisive evidence against Gould and Nelson (1974).[6] In their results, Friedman and Schwartz established that log real money balances were systematically related to log real income and an interest-rate variable. Such a finding, as noted, is not in itself inconsistent with velocity being nonstationary, provided that the interest rate is too. However, following the Klein (1970) dissertation work that he supervised (see also Klein, 1974), Friedman was increasingly attracted to the hypothesis that the "own rate" (the interest rates on deposits in M2, weighted by the deposits' share of the aggregate), while below that on market rates, kept in step over time with market interest rates. In such a situation, the possibility of nonstationarity in velocity arising from interest-rate variation was closed off. *Monetary Trends* opted for the Klein-style specification of the opportunity-cost variable and so used a money demand function that implied that interest rates, even if nonstationary, did not produce nonstationarity in interest rates.[7]

That said, in their work on money demand in the early 1970s Friedman and Schwartz still had the level of market interest rates appearing directly in the estimated function, rather than as a spread over the own rate (Schwartz, 1975, p. 147). Thus, though Friedman's contention that M2

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[6] Friedman and Schwartz (1982, p. 209).

[7] Both in Schwartz (1975) and Friedman and Schwartz (1982), Friedman and Schwartz's money demand function had nominal income growth as another driver of money demand. Provided that nominal income growth was mean-reverting, this term too could not be a source of nonstationarity in velocity. In particular, in the sample period covered by Gould and Nelson (1974), nominal income growth repeatedly returned to its mean and recorded a low rate in the final year of their sample, 1970. And, although nominal income growth was higher on average in the 1970s than in previous decades, it was only in the later years of the decade that it was high throughout; in contrast, the initial rise in nominal income growth in 1971–1973 was largely wound back in 1974–1975.

velocity was stationary was one he held both in the early 1970s and in *Monetary Trends*, his *initial* rejection of the alternative hypothesis of a random-walk velocity at least partly reflected the fact that—like many macroeconomists before the appearance of the Nelson and Plosser (1982) study—Friedman found it difficult to accept that short-term market interest rates were a random walk, at least when rate were measured at monthly, quarterly, or lower frequencies.

In an analysis for the February 1973 issue of *Morgan Guaranty Survey*, Friedman considered the postwar velocity of M2 and emphasized that in the previous decade it had "displayed no appreciable trend… It has been extraordinarily stable."[8] He conceded that velocity had had "a moderate upward trend before 1962"—one that, in the event, the Federal Reserve Board's later (1980) broadening of the M2 definition would largely eliminate. As Friedman's velocity figure went back to 1948, it also included the sharp move up in velocity in 1950 that Friedman and Schwartz's *Monetary History* had suggested (and Friedman's *Morgan Guaranty Survey* reaffirmed) was a panic move out of money when the Korean War broke out, reflecting fears (in the event largely unrealized) of a repeat of a World War II-style inflation, in which inflationary pressure was suppressed by controls and so led to shortages.[9]

This retrospective analysis of the postwar period was therefore one that judged modern conditions as featuring covariance-stationary M2 velocity behavior.[10] Friedman, indeed, was extremely comfortable with the notion of M2 velocity stationarity, because he believed it was consistent with the analysis of the *Monetary History*, which had suggested that the rise in velocity seen in the early postwar years would not be repeated in the 1960s.[11] However, the *Monetary History* had actually predicted that velocity would *decline* in the 1960s, as Friedman

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[8] Friedman (1973a, p. 7). In keeping with his practice since the late 1960s, Friedman's 1973 analyses excluded large negotiable certificates of deposit from the definition of M2 (see, for example, Friedman and Schwartz, 1973, p. 50). The Federal Reserve Board also followed this practice, both for the M2 series it had published since 1971 and for its revised M2 definition in 1980. See Nelson (2020b, Chapter 14) for a detailed discussion.
[9] Friedman (1973a, pp. 8–9).
[10] Friedman's assessment in 1973 of a stationary M2 velocity was related to Friedman and Schwartz's revised analysis earlier in the decade of historical velocity data. As discussed in Nelson (2020b, Chapter 13), this assessment had the effect of leading them to regard the real income elasticity of U.S. M2 demand as approximately unity. They judged that their earlier estimates of the income elasticity had been biased upward by inclusion in their sample of the period in the late nineteenth century of rapid growth in U.S. commercial banking. Thus, while Gould and Nelson (1974) had been taking Friedman and Schwartz (1963a) as contending that M2 velocity since 1869 was trend-stationary, by the time the Gould-Nelson paper appeared Friedman and Schwartz had actually shifted to the position that velocity since 1903 was covariance stationary (and trend-stationary before 1903).
[11] In particular, in Instructional Dynamics Economics Cassette Tape 113 (January 17, 1973), Friedman observed, "I have more confidence in that stability than I otherwise would, because I predicted that it would happen," and he pointed to Chapter 12 of Friedman and Schwartz (1963a) in this connection. However, as indicated below, it is not completely accurate to suggest that Friedman and Schwartz's account predicted stability in M2 velocity.

and Schwartz believed that real money balances constituted a luxury good.[12] As already discussed, it was only in the early 1970s that Friedman switched to a belief in an income elasticity of money demand of roughly unity. With an income elasticity of unity, it made more sense that, as Friedman put it, the velocity of M2 was "almost precisely stable for that dozen year period" after 1960 (Instructional Dynamics Economics Cassette Tape 113, January 17, 1973).

Ironically, however, having just arrived at this judgment, Friedman came to believe that a permanent shock to the velocity of U.S. M2 was in progress during 1973, one not unlike that seen at the start of the Korean War.

As will be seen below, against the background of surging inflation during 1973 and 1974, Friedman reaffirmed that aggregate demand behavior be put at the forefront of the analysis of inflation. But, for much of 1973, he was more well-disposed than usual toward viewing the strength of aggregate demand, both nominal and real, as a reflection of forces *other* than those represented by monetary growth. For example, he initially viewed the increase in U.S. aggregate nominal spending in the first quarter of 1973 as *substantially beyond* what would be expected by monetary growth.[13] In particular, Friedman was impressed by what seemed to be a sharp increase in M2 velocity and was prepared to ascribe some of the boom in aggregate demand in process that year to an autonomous increase in velocity—in which households' demand for money shifted in response to a fear of shortages, as they had in 1950 (Instructional Dynamics Economics Cassette Tape 119, April 25, 1973; see also Tape 120, May 11, 1973, and Tape 124, July 4, 1973).[14]

But as more data accrued, it would turn out that such an appeal to factors driving velocity was not necessary for the analysis of aggregate demand in 1973; indeed, by mid-1974, Friedman was able to observe that the relationship between money (in particular, M2) per unit of output and

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[12] Friedman and Schwartz (1963a) had suggested that the increase in M2 velocity in the postwar years through 1960 reflected steps up in the U.S. private sector's willingness to economize on bank deposits, as their confidence in national economic stability increased, and they argued that this process of successive rises in (old) M2 velocity would cease, as confidence stabilized at a higher level. This prediction concerning U.S. broad money velocity—that it would cease rising—was indeed borne out by 1961–1972 developments. But, as indicated, Friedman and Schwartz in the early 1960s assumed an income elasticity estimate of well above unity (perhaps about 1.65 or 1.8) for the U.S. money demand function. The therefore saw rising real national income as likely to put an overall downward trend into the behavior of M2 velocity for the years after 1960. See, for example, Friedman and Schwartz (1963a, pp. 654, 675), as well as Friedman's (1961b, p. 263) conjecture that "the factors accounting for the postwar decline in the money-income ratio have spent their force, and the ratio may be expected to resume its long-period upward trend." The predicted decline in velocity did not occur and, as indicated, Friedman and Schwartz in the period after 1963 revised down their estimate of the income elasticity of money demand.

[13] See his testimony of June 21, 1973, in Joint Economic Committee (1973a, p. 130).

[14] See also Nelson (2020a, Chapter 4).

prices (another way of looking at the money/nominal income link) had been closer for the period since 1962 than the average historical relationship between the two series (*Newsweek*, June 24, 1974).[15] The result that the relationship between money and the economy in 1973 was closer than was thought at the time had a parallel with 1971−1972. In that earlier period, it was nonmonetarists who had rashly declared money/income relationships to have shifted (see, for example, Mitchell, 1972, as well as the discussion in Nelson, 2020b, Chapter 15). In 1973, Friedman himself had been trying to explain what proved to be, at best, an ephemeral discrepancy between the courses of money and nominal income.

Indeed, the accumulation of more data for the year as well as statistical revisions would make it clear that the relationship between income and money was, in fact, close over the course of 1973. As in the study of earlier periods, allowing for lags in the relationship between money and income is important in removing apparent divergences between the two series. Notable insight in this connection is provided by a concept that Friedman advanced in the *Morgan Guaranty* piece: something he would come to call "leading velocity"—a velocity series defined as nominal income divided by money two quarters earlier. This concept—for which Friedman promulgated the "leading velocity" terminology in the *Wall Street Journal* of September 1, 1983—amounts to a formula for velocity that recognizes, albeit roughly, the lag between the two series and in particular the tendency of movements in monetary growth to precede movements in nominal income growth. Friedman and Schwartz together promoted the leading-velocity concept (which they, and Friedman's *Morgan Guaranty* article, called "adjusted velocity") when they provided an update on their research in the NBER's *Annual Report* for 1973 and showed how, on data through the end of 1972, the associated adjustment generated "smoother short-period behavior" of velocity, while leaving trend behavior unchanged.[16]

The Friedman and Friedman-Schwartz presentations of the leading-velocity concept crystalized their earlier work on lags and included Friedman's recent refinement of focusing on comparisons of growth rates of money and nominal income when ascertaining lags.[17] John Rutledge, who gave a few presentations at the money workshop, recalled of his first presentation (given on October 24, 1972) in the fall semester, "after the workshop, everyone left and he [Friedman]

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[15] David Meiselman published a similar chart to Friedman's (covering 1960 to 1974) in the *Wall Street Journal* of September 13, 1974—using the chart to support the position that 1973's inflation predominantly reflected monetary excess "rather than from declines in output stemming from such oft-cited events as the disappearance of anchovies off the coast of Peru or the operations of the OPEC [Organization of Petroleum Exporting Countries] cartel." (On such nonmonetary factors and their use in explaining inflation, see the discussion below and that in the next chapter.)
[16] See Friedman and Schwartz (1973, pp. 51−52).
[17] For discussion of this refinement, see Nelson (2020b, Chapter 15, Section I).

stayed there with me. And he was talking about the lags in monetary policy. And he had two rolls of tracing paper." On one roll, M2 growth was plotted; the second roll showed nominal GNP growth. "And we went over to the window. We held up the tracing paper against the glass, so it was see-through with the sunshine coming through it. And we moved the first roll of tracing paper left and right until it looked like it was sitting right on top of the other. The *M* line was sitting right on top of the GNP line—which was a way of measuring the average lag of monetary policy. And then he said: 'You know, this is really a pretty good way to do it, because you and I can see things that sometimes regression programs miss.'" (John Rutledge, interview, November 14, 2014.)[18]

Calculating leading velocity, instead of actual velocity is a way of transferring such matching-up of money and income variations into velocity space. Specifically, the use of leading velocity has the effect of moderating or even removing the spike (up or down) in velocity that is observed when a money movement occurs that only later shows up in nominal spending.[19] Indeed, for 1973—after account is also taken of post-1973 revisions and redefinitions of money and income—most of the rise in velocity is absent from the leading-velocity series. With modern data on nominal GDP and the modern M2 definition, the rise in velocity as ordinarily measured is 3.9% in the four quarters to 1973, but the rise in leading M2 velocity for the same period is less than 0.1%, with the four-quarter rise in nominal GDP of 11.1 percent to 1973:Q4 matching the growth rate of M2 to 1973:Q2 of 11.1 percent.

Over a longer period, the leading-velocity/current-velocity distinction diminishes in importance. Consequently, reliance on conventionally-measured velocity is sufficient to bring out the extent to which nominal income and money moved in line with one another over the period covered in this and the next few chapters. U.S. nominal GDP in the fourth quarter of 1976 was over 45 percent higher than its value in the fourth quarter of 1972; in contrast, in 1976:Q4 the ratio of nominal GDP to M2—that is, the standard velocity calculation—was only about 1.6 percent higher than its value in 1972:Q4.

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[18] The date of this workshop was obtained from University of Chicago Library records.

[19] See Nelson (2020a, Chapter 10) for more discussion of this concept, to which Friedman had alluded in 1970 (see Nelson, 2020a, Chapter 6) and which had also been invoked in Wicksell (1935, p. 142). The concept was later used in Federal Reserve Board staff research (Axilrod, 1983, pp. 4−6; Porter and Small, 1989, pp. 244−245) as well as in Mankiw and Summers (1986, p. 420). The fact that Friedman first plotted a leading-velocity series in an outlet other than *Newsweek* or a research journal may have slowed down recognition of his use of the concept. For, in a Federal Reserve Board staff memo titled "Friedman's Redefinition of Velocity," from James Pierce to David Lindsey dated June 19, 1975, Pierce recalled learning of Friedman's use of the concept in conversation with Friedman "at the Academic Consultants Meeting six months ago" (the meeting of December 12, 1974). (Federal Reserve Board records.)

As the picture became clearer, Friedman realized that an appeal to a velocity shift was hardly needed to explain either the boom in 1973 or the leadup to the boom; almost the whole of the observed expansion in nominal aggregate demand had a counterpart in money growth. Support for his position came in May 1975, when the U.S. Department of Commerce added the money stock (albeit the M1 monetary aggregate that was not Friedman's preferred series), expressed in real terms, into its index of leading indicators.[20] And Feldstein and Stock (1994, pp. 24–30) would find that equations linking nominal GDP growth to prior growth in (modern) M2 (as well as to an error-correction term embedding the long-term proportionality between M2 and GDP) remained stable—and the money terms retained statistical significance—as the 1970s were added to the sample period.

Illness and recovery

Friedman's 1973 analyses of velocity came in the months following his second absence during 1972–1973 from the University of Chicago. This absence was for health reasons: his heart surgery in Rochester, Minnesota in December 1972, and his subsequent period of recuperation from late December 1972 through mid-February 1973.

In recording his biweekly economic commentary on December 15, 1972, Paul Samuelson observed: "The first order of business today is to wish good luck to my colleague, Professor Milton Friedman of the University of Chicago. According to what I read yesterday in the *Washington Post* and the *New York Times* and also this morning in the *Boston Globe*, Professor Friedman just today is to undergo open-heart surgery at the Mayo Clinic. It's rare that an economist has in his own lifetime a significant influence on the history of ideas and on the passing scene of policy. But let me say it, since I have not always been in perfect agreement with Professor Friedman—that Milton Friedman has had this unusual influence. Not only has been an adviser of presidents and to the advisers of presidents, but [also] he has influenced two generations of students."[21] Samuelson added, in remarks that underlined the gravity of the

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[20] See Laurent (1993, p. 11). The basis for money being expressed in real terms in the index lay in the fact that the leading indicators were primarily designed to project real economic activity, rather than nominal spending.

[21] Although its articulation on this occasion was obviously prompted by Friedman's health crisis, Samuelson's friendly posture toward Friedman, particularly in his public statements, was not new. In discussing his interaction with Nicholas Kaldor, Friedman claimed that their great differences in views "never affected our personal relations" (Friedman and Friedman, 1998, p. 247). This was a glamorized account—the relationship between Friedman and Kaldor really was, in fact, acrimonious (or nonexistent) by the mid-1970s—but was much truer as a description of the Friedman-Samuelson relationship. Samuelson was harshly critical of Friedman in conversations with colleagues, but he would implore tell them not to repeat these criticisms to others (see Nelson 2020a, Chapter 4). In particular, Samuelson explained to MIT colleagues that the poisoned discourse among turn-of-the-century Swedish economists of rival schools had helped motivate him to keep good personal relations with Friedman (Stanley

procedure facing Friedman: "I've been seeing quite a number of economists in the last two days, and it's been amazing what a groundswell of appreciation has been expressed in these last hours." (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 117, December 15, 1972).[22]

As already indicated, Friedman's operation was on December 15, 1972. In 1983, Friedman would be quoted recalling: "They did a bypass operation, as a preventive measure, which has been just incredibly successful. I have never had a problem since. Everything has been fine."[23] The initial aftermath of the surgery gave little indication that this outcome was likely. Marc Nerlove recalled that "he later described the recovery from that to me, in some detail. It was very hard to recover, but he obviously did—he lived many years longer without problems. But [before that] he'd never been ill in his life." (Marc Nerlove, interview, September 26, 2013.) Anna Schwartz, in conversation with the author 35 years after Friedman's surgery, noted that Friedman's condition in the initial weeks after the operation was so frail that those around him had doubts about a full recovery. "Who would have thought, at that time, that Friedman would live on to ninety-four?"[24] Similarly, Rose Friedman's correspondence with Arthur Burns in the days following his operation expressed discouragement and distress at her husband's weak state.[25]

Fischer, personal communication, June 1, 2017). Indeed, in an appearance at MIT with Friedman in 1969, Samuelson held up their relationship as a contrast with the "bad blood" between Gustav Cassel and Knut Wicksell, adding that the latter provided a warning against letting "scientific differences or political differences cloud… personal friendship" (*The Great Economics Debate*, WGBH, May 22, 1969). In the same vein, Samuelson (1981, p. 356) noted that "the force of this [Swedish] example has been important in my own life. Over what will soon be five decades, Milton Friedman and I have had some occasions to disagree on policy choices and doctrines, without that affecting our friendly relations."

[22] In addition to the newspapers Samuelson cited, Friedman's upcoming operation received widespread coverage in other city papers; among these, the *Chicago Daily News* made it a front-page item (December 13, 1972). In Friedman and Friedman (1998, pp. 420–421), Rose Friedman pointed to Milton Friedman's appearance before the media after arriving in Rochester, Minnesota, in the leadup to his operation, as what set off the national press coverage of his imminent surgery. Nonetheless, the widespread reporting of Friedman's medical treatment did not really start until, as just indicated, December 13, while Friedman's local media appearance was some days earlier (see *Rochester Post-Bulletin* (Minnesota), December 9, 1972). Even ahead of this, a public reference to Friedman's heart problems had appeared in the *Pittsburgh Post-Gazette*, December 7, 1972, when cardiac complications were cited as the reason a visit that Friedman had been scheduled to make to Pittsburgh in that week had been canceled. As discussed in Nelson (2020b, Chapter 15), the state of Friedman's health seems to have been a major concern among family and friends since the spring/summer 1971 period. He had, on doctor's orders, arranged that he would only participate in the August 1971 Mont Pelerin Society meeting by videolink from Vermont, despite currently serving as the society's president (*Register-Republic* (Rockford, Illinois), August 6, 1971). Anna Schwartz had corresponded with Friedman in relation to his insurance for, and his convalescence after, a medical procedure that he had during the week of July 19–23, 1971 (letters from Anna Schwartz to Milton Friedman, July 14 and 27, 1971, Anna Schwartz papers).

[23] In Martin (1983, p. 57).

[24] Schwartz's remarks were made on May 27, 2008 (see Nelson, 2009a, p. 489).

[25] December 1972 correspondence in the Arthur Burns papers, Gerald Ford Presidential Library.

This initially precarious recovery during December 1972 was followed by a more robust condition. Friedman was discharged from St. Mary's Hospital on the morning of December 26 (*Rochester Post-Bulletin* (Minnesota), December 26, 1972; *Kansas City Times* (Missouri), December 27, 1972). Friedman spent January 1973 convalescing in Palm Springs, California.[26] The surgery had a lasting effect on Friedman's intellectual activity, by giving him a permanent interest—albeit as a hobby rather than as a research topic—in the state of knowledge regarding treatment of heart conditions (*Newsweek*, January 11, 1982). During 1973 he also had a surge of renewed interest in shaking up the medical profession, devoting much of his first post-surgery cassette commentary (taped from Palm Springs) to a restatement of the case against barriers to entry to medical practice and recalling *Income from Independent Professional Practice* in the process (Instructional Dynamics Economics Cassette No. 113, January 17, 1973).[27]

Thus, during January Friedman was back commenting on current events. And late in that month, while in Palm Springs, he was meeting with officials from the California government about budget control.[28] It also transpired that Friedman's period of illness and recuperation did little to lower his public profile during 1973. Interviews he had given before his surgery appeared in print during his convalescence. The *National Journal* (January 13, 1973) reported on an interview Friedman had given by telephone from his hospital room just before the surgery, and the February 1973 issue of *Playboy* published a long interview that the magazine had conducted with him in late 1972. The new edition of Paul Samuelson's economics textbook, published on March 12, included newly added discussion of Friedman, in which readers were urged to study Friedman's works for their articulation of monetarist and free-market views.[29] The *Encyclopaedia Britannica*'s *Book of the Year* for 1973—its record of events in 1972—gave a mention to the *Journal of Political Economy* symposium on Friedman's framework (Encyclopaedia Britannica, 1973, p. 252). Friedman's own tendency to produce at least one book per year was not uninterrupted in 1973, as that year would see publication of his April 1972 lectures in Israel.[30] A few years after its publication, this slim book would even receive the (undeserved) description, in a biographical sketch of Friedman, of being among his "best-known

[26] See, for example, Friedman and Friedman (1998, p. 389).
[27] That is, Friedman and Kuznets (1945). Friedman also gave an interview on the subject in *Medical Economics* (April 16, 1973). A *Newsweek* column (January 8, 1973) considered medical drug regulation, although it was probably written before Friedman's surgery. His first column written after the surgery was likely that in *Newsweek*, January 29, 1973. Friedman also recorded two more cassette commentaries from Palm Springs: those of January 31 (Tape 114) and February 14 (Tape 115).
[28] See Friedman and Friedman (1998, p. 422) as well as Chapter 5 below.
[29] See Samuelson (1973a, pp. 847–848).
[30] See Friedman (1973b).

works."[31]

Due to his absence from the city of Chicago, Friedman would miss appearing in person at an awards ceremony at the Chicago Press Club on January 20, at which he would be named Chicagoan of the Year (*Chicago Tribune*, January 15, 1973).[32] About a month later, however, the *Chicago Tribune* reported (February 14, 1973) that Friedman would be returning to Chicago that week. The Friedmans flew from Palm Springs to Chicago on Saturday, February 17.[33] Consequently, by the end of February 1973, Friedman was back in the city of Chicago and again keeping close tabs on economic developments. Benjamin Eden, a University of Chicago graduate student over this period, recalled: "At the time, everybody was worried, but then he came back." (Benjamin Eden, interview, March 14, 2014.)

On his return, Friedman would find that the ground had moved under his feet. His period of convalescence had coincided with profound changes in the U.S. economic landscape.

Inflation erupts in the United States

At the end of 1972, in a report on the U.S. economic outlook, the Nixon Administration's Council of Economic Advisers stated that "the prospects are good for another year combining rapid expansion and a reduced rate of inflation" (quoted in *Chicago Daily News*, December 27, 1972). In a similar vein, in an article published in March 1973, the Deputy Secretary of the Treasury, William E. Simon, could still claim success and express optimism concerning the administration's previously-expressed objectives: "Over the past four years… we have succeeded in cutting inflation almost in half—from about 6 percent to almost 3 percent. We now have the lowest rate of inflation and the highest rate of real growth of any developed nation in the world… Our goals for 1973 are to continue to move toward full employment and to cut the rate of inflation to 2½ percent by the end of the year. These are ambitious goals—but attainable."[34] (*Bankers Monthly*, March 15, 1973.)

But by the time Simon's article was published, major blemishes on the picture had materialized, and a *Wall Street Journal* front-page article observed that in a matter of weeks the U.S. economy had transformed from "a glistening Cinderella's coach" to a "pumpkin" (*Wall Street Journal*,

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[31] In Friedman (1976b, p. iv).

[32] Friedman accepted the award by making a long-distance call into the awards ceremony (Instructional Dynamics Economics Cassette Tape 116, March 2, 1973).

[33] See Friedman and Friedman (1998, p. 422). This return date was planned well in advance, being mentioned in Friedman's letter to Arthur Burns (January 29, 1973). (Burns papers, Gerald Ford Presidential Library.)

[34] Quotation from page 16 of the article.

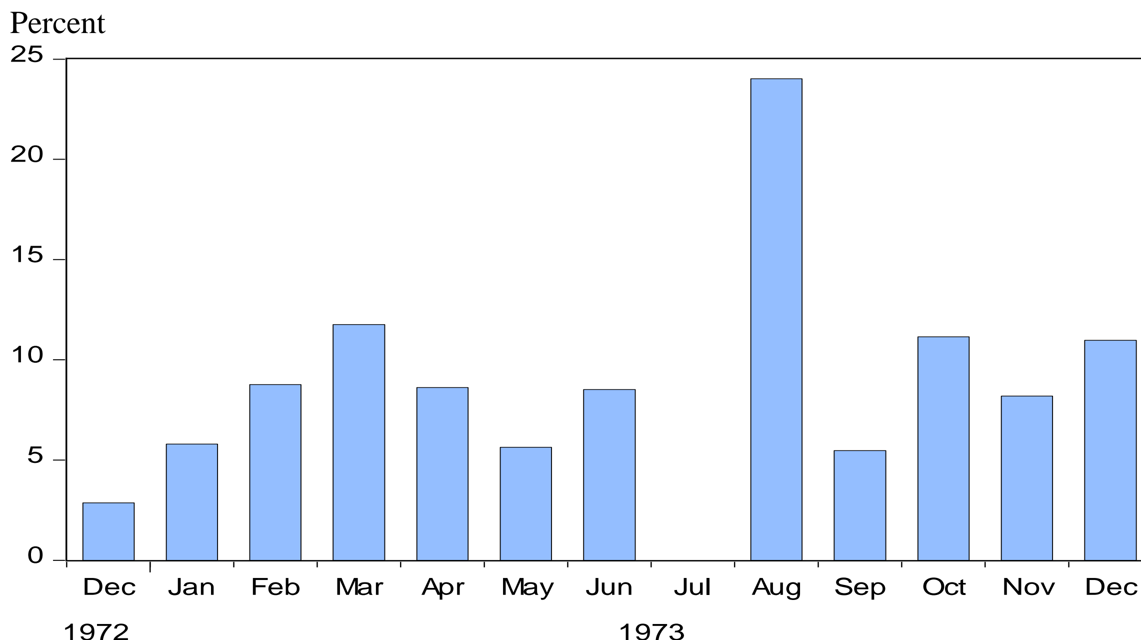

Figure 1. Annualized monthly rates of CPI inflation, December 1972–December 1973.
Source: Computed from CPI data in the Federal Reserve Bank of St. Louis' FRED portal.

February 27, 1973). By then, the surge of the Consumer Price Index (CPI) in January 1973, shown in Figure 1, had been reported. The United States had entered what Modigliani (1978, p. 195) labeled "the three very disturbed years" following 1972.

Even by the time of his forced absence, Friedman was convinced that the U.S. economic developments demanded a course correction on the part of policymakers. In his interview before his operation (*National Journal*, January 13, 1973) he stated: "There is a difference in what is right when you're in an expansion, and you have unused resources to employ, and what's right otherwise." He expressed much the same sentiment in a letter at the end of 1972—dated December 27, 1972, but written prior to his operation—to President Richard Nixon, with Federal Reserve Chairman Arthur Burns copied into the correspondence. What Friedman failed to appreciate, however, was that economic overheating had already set in and that this excess demand was poised to spill over to inflation, even in the presence of mandatory price controls.

The end of the period of apparently benign behavior coincided closely with the end of calendar-year 1972. Friedman realized that the tranquility of 1972 was precarious. That had been a message of his December letter to Nixon.[35] His letter had stressed the need for a prompt

[35] A copy of the letter was, as noted, also forwarded to Arthur Burns, and that copy is held in the Burns papers at the Gerald Ford Presidential Library. Notwithstanding the December 27 date on the letter, the accompanying cover

tightening in the settings of aggregate demand policy. Friedman affirmed this posture in January 1973, when he observed: "The Fed must be just about as distressed as I am over this explosion in money." (Instructional Dynamics Economics Cassette Tape 113, January 17, 1973.)

But though he was warning about problems in store if current policy settings were continued, Friedman did not grasp was the extent to which the past two years' policy settings had already put in place economic overheating and sharply higher inflation for 1973. As of late 1972, he favored aggregate demand settings that would make output grow about 1 percent below potential for a period, in order to reduce inflation from 3 percent to close to zero after 1973 (*Rochester Post-Bulletin* (Minnesota), December 9, 1972).[36] That recommendation indicated his view that the 1969–1970 demand restriction had not been great enough to remove inflation altogether. But it did not indicate an appreciation that the subsequent, 1970–1972, aggregate demand policies had actually more than reversed the disinflationary pressure delivered by the earlier period of restraint.

For inflation in 1973 itself, Friedman had predicted an increase, but not a dramatic one—to a rate somewhere above 3 percent (*Rochester Post-Bulletin* (Minnesota), December 9, 1972) or perhaps above 5 percent at the outside (from his recollection of his 1972 forecast in Instructional Dynamics Economics Cassette Tape 136, December 13, 1973).

This contrasted starkly with the actual outcome: a rate of CPI inflation for the year to December 1973 of 8.9 percent. The corresponding rate for December 1972 had been 3.1 percent. The twelve-month inflation rate moved up swiftly during 1973 as high monthly readings for early in the year (shown in Figure 1) entered the twelve-month calculation. The twelve-month rate was already above 4 percent in March 1973, and it never went below 4 percent for the rest of the 1970s.

When pressing in late 1972 and very early 1973 for a tightening of policy settings, Friedman premised his case on an acceptance that the authorities were satisfied to get 2–3 percent inflation and on the belief (in retrospect, very likely incorrect) that output remained slightly below potential. Thus, he saw the requisite tightening as one that lowered nominal income growth to 5 to 7 percent from mid-1973 onward.[37] From this starting point, Friedman was caught off-guard

letter from Rose Friedman to Burns confirmed that, as already noted, the letter was drafted prior to Friedman's operation and that Friedman was not well enough to work further on the letter during the two weeks following his operation.

[36] See also Friedman (1973a, p. 5).

[37] See Friedman (1973a, p. 5).

by the sharp upturn in inflation and nominal income growth that quickly emerged at the start of 1973: as earlier noted, nominal income growth by the end of 1973 was well above 10 percent, and inflation itself was approaching double digits.[38] A host of other economic indicators would also deteriorate in the course of that year, as discussed later.

Wage and price controls: intensification and strains

As recounted in detail in Chapter 15 of the previous volume, Friedman's negative posture toward wage and price controls was shared by the Nixon Administration in its early years. But this attitude encountered considerable opposition in public discourse and was eventually eschewed by President Nixon himself. Nixon's embrace of controls in August 1971 received wide support; Friedman, who would acknowledge that Nixon's move was backed by "a widespread national consensus," was criticized for his continuing opposition to controls and to nonmonetary approaches to the analysis of inflation.[39] For example, a column in the *Chicago Daily News* at the end of August 1971 contended that Nixon's switch occurred "with the nation on the verge of economic calamity," and looked askance at Friedman's reaction: "Undaunted and unashamed, Friedman now tells us that Mr. Nixon's wage-price-rent freeze is a 'lamentable mistake' that 'will end… in utter failure and the emergence into the open of the suppressed inflation.'" (*Chicago Daily News*, August 31, 1971.) And in November 1972, as President Nixon approached reelection fifteen months into his controls program, economics columnist Hobart Rowen noted Friedman's opposition and concluded: "So, controls have been tried—and they have worked…" (*Boston Globe*, November 5, 1972.)

The years 1973 and 1974 would, however, see general acceptance that Friedman's skepticism about the Nixon wage-price controls had been justified—although it would take several more years before his comprehensively negative posture toward nonmonetary approaches to controlling inflation became widely shared.

On January 11, 1973, President Nixon announced Phase III of his wage-price controls. This new setup was widely seen as amounting a major relaxation of the controls and as setting the stage for a transition to a largely voluntary incomes policy.[40] Friedman himself took Nixon as having largely abandoned controls by taking this step and praised the president accordingly (*Newsweek*,

[38] In 1974, Friedman observed (Instructional Dynamics Economics Cassette Tape 156, October 23, 1974) that his and others' inflation projections had been "very bad" over the past few years.
[39] The Friedman quotation is from *Fortune* (July 1974) (reprinted in Friedman, 1975a, p. 152).
[40] The Associated Press report on Nixon's announcement of Phase III said that it "scrapped most firm wage and price controls" (*Arizona Republic*, January 12, 1973).

January 29, 1973). However, the fact that the CPI then registered such a large rise in January produced a backlash against Nixon's relaxation of controls—prompting William Simon to acknowledge, in the aforementioned March 1973 article, that "Phase III has not been well received."

When the analysis in Friedman's January 29 *Newsweek* column is viewed in the light of the January CPI data and subsequent data releases, it becomes evident that he had been underestimating the degree of excess spending—and corresponding inflationary momentum—already prevailing in the U.S. economy by the end of 1972. The column was, however, accurate in forcefully arguing that the basic course of inflation during 1973 would reflect the behavior of "total demand," and not what was done by the U.S. authorities in the area of wage and price controls. In particular, Friedman stressed that those controls would not be able to contain inflationary pressure created by great excessive demand. In the same vein, he would see much of the inflation recorded during the Phase III period as reflecting the unveiling of price increases, themselves largely driven by demand pressure, that were suppressed during the earlier, more restrictive phases of price control. To him, the surge in U.S. inflation during 1973 confirmed that controls had done nothing to remove the forces producing inflationary pressure. If anything, he said, they had increased the scale of these forces, by encouraging the authorities to carry out expansionary policies.

Events during 1973 would bear out observations Friedman had made in November 1971: "The longer-run danger is that, under the cover of price and wage control—which conceals but does not reduce inflation—inflation pressure will build up, [coming] from fiscal and monetary policy," in which case "the prospect is that the controls will collapse sometime in 1973, open inflation will resume, and we shall have to face the [three] alternatives of producing a severe recession to halt inflation, permitting inflation to proceed openly, or clamping on much more widespread and stringent controls." (*Daily News* (New York), November 11, 1971.) In the event, the year 1973 saw a combination of these alternatives being pursued.

The magnitude of the inflation that appeared from 1973 onward would also mean that the traditional demarcation between wartime and peacetime experiences in the United States would need to be reassessed. In 1970, Friedman had described the price rise during the late 1960s as the most severe of "any peacetime period in American history," while acknowledging the difficulty in classifying it as a "peacetime" inflation in light of the ongoing U.S. involvement in

the Vietnam War (*New York Times*, December 20, 1970).[41] The price-level burst that started in 1973 rendered this classification issue moot, for the U.S. inflation rates recorded in 1973 and 1974 far exceeded those seen in the late 1960s. And the 1973−1974 price rises were assuredly "peacetime" inflation, occurring predominantly after the U.S. had ceased active combat in Vietnam in January 1973—and certainly well after U.S. combat had peaked.[42]

In March 1973, Friedman engaged in his first overseas travel since his operation. In a lecture given at the National Bank of Yugoslavia on March 20, Friedman harked back to the first famous failure of price controls under Diocletian and drew parallels with the United States' "attempts to stop inflation by Phases I, II, or III."[43] Friedman realized, however, that his view that the controls had only suppressed inflation and not removed them was not the popular perception. On this occasion, he hedged his bets about whether U.S. policymakers yet grasped that controls were only cosmetic: he noted that officials could embrace controls either to give the appearance of fighting inflation or because they were "[b]elievers in 'cost-push."[44] In retrospect, as indicated later in this chapter, it was the second motivation that remained the principal impetus for the use or advocacy of controls and other incomes policies among U.S. policymakers in 1973−1974. But, irrespective of the motivation for controls, it was clear to Friedman, as he noted in April, that the advent of controls had encouraged "the miseducation of the public" about how to deal with inflation (Instructional Dynamics Economics Cassette Tape 119, April 25, 1973).

A symptom of this miseducation was the aforementioned backlash against Phase III, which led President Nixon into a major revision of the controls in June 1973. The June 1973 measures—discussed further in the discussion titled "Watergate" later in this chapter—included a new price freeze. As shown in Figure 1, this freeze was quickly felt in July 1973's near-cessation of

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[41] Friedman was disinclined to attribute much of the rise in inflation in the second half of the 1960s to the scaling-up of the United States' military expenditures. This reflected his skepticism about fiscal expansion, if unmonetized, as a source of upward pressure on aggregate demand and inflation, together with his position that monetary policy need not, and often did not, move in step with fiscal policy. He did acknowledge in one retrospective (Friedman, 1982a, p. 105) that, in the presence of the Federal Reserve's emphasis on interest-rate stabilization, "external pressure arising from the Vietnam War" was a factor putting upward pressure on interest rates in the mid-1960s and tending to lead, under the monetary policy arrangements in force, to excessive expansion in the money stock. In Instructional Dynamics Economics Cassette Tapes 28 (June 12, 1969) and 29 (June 30, 1969) he had identified 1965 specifically as the year in which he believed that Federal Reserve actions largely held interest rates down in response to upward pressure arising from the step-up in Vietnam War-related federal government expenditures.
[42] True, the early-1973 upsurge partly reflected unveiling of inflation that had been suppressed in the stringent control period of 1971−1972. But it is still implausible to regard 1973−1974 U.S. inflation rates as a ramification of the United States being on a war footing. Defense spending peaked as a share of U.S. output in real terms in fiscal year 1968 (Council of Economic Advisers, 2018, Table B−18, p. 553). This timing contrasts with the much later peak, in the 1972−1973 period, of monetary growth and nominal income growth.
[43] Friedman (1973d, p. 6).
[44] Friedman (1973d, pp. 5−6; quotation from p. 5).

recorded price increases.

The June 1973 revision of the controls was variously dubbed "Phase IV" (for example, *Yorkshire Post*, June 15, 1973) and "Phase 3½" (for example, *Daily News* (New York), June 15, 1973). Friedman, in keeping with the administration's own numbering system, reserved the "Phase IV" terminology for what would *follow* the new freeze (Instructional Dynamics Economics Cassette Tape 123, June 23, 1973).[45] He characterized the freeze as motivated by cost-push ideas about inflation (Instructional Dynamics Economics Cassette Tape 125, July 18, 1973). "It's treating the façade of inflation rather [than] getting to the guts of it," Friedman remarked when some details regarding the next, post-freeze, controls phase were announced (*Newsday*, July 19, 1973).

Challenging cost-push views in 1973–1974

As Friedman's remarks in July indicated, the developments in inflation and anti-inflation policy over the first half of 1973 had made it imperative for him to redouble his efforts to articulate the monetary view of inflation and highlight the flaws of cost-push views of inflation.

This situation had emerged even before the first OPEC oil shock of 1973–1974, which began unfolding in October 1973. The discussion that follows highlights key aspects of Friedman's critiques of cost-push views in 1973–1974; most of the coverage of his reaction to the oil shock, including its implications for inflation is, however, reserved for the next chapter.

In 1983, Friedman reflected: "It became increasingly clear that excessive money growth was a major culprit as inflation accelerated in the 1970s."[46] However, the transition during which the acceptance that inflation was a monetary phenomenon became truly widespread among economists lasted over much of the 1970s, being very incomplete by 1973. And the acceptance among those *in policy circles* of this interpretation—reversing their renewed enthusiasm for cost-push views at the start of the decade—was, if anything, even slower, stretching into 1978 and 1979.

Friedman's aforementioned March 1973 lecture, while reaffirming his own stand that "what is called 'cost-push' is a symptom and not a cause of inflation," acknowledged that this perspective had not become the consensus. Instead, there remained "wide disagreement about both the

[45] He also tentatively used the "Phase 3½" label for the new freeze: see his testimony of June 21, 1973, in Joint Economic Committee (1973a, p. 132).
[46] Friedman (1983a, p. 6).

causes of inflation and the remedies for inflation."[47] Furthermore, within months of his lecture, the tightening of price controls under Nixon, alongside Arthur Burns' continued support for incomes-policy approaches, pointed up the fact that the cost-push perspective still prevailed in U.S. official circles.

It was against this backdrop that Friedman made some of his strongest and most emphatic rejections yet of cost-push theories of inflation. "I believe that what is called 'cost-push' is a symptom and not a cause of inflation," he declared in his March lecture, while restating his long-held position that a demand-generated inflation may exhibit the features commonly said to characterize cost inflation.[48] "So much of the discussion of inflation is *wrong* on this point," Friedman would complain in August 1973. "It confuses the arithmetic with the economics." (Instructional Dynamics Economics Cassette Tape 127, August 15, 1973.)[49] He specifically challenged the importance of a number of special factors cited during the year as causes of inflation. In contrast, Arthur Burns—whose analysis of 1973 inflation developments is considered further below—stated in September 1973 that the rise in food prices "accounts for much of the overall price increase thus far this year."[50]

For Friedman, the ground had already been laid for a general price rise—having been provided by earlier years' monetary policy. That being the case, the increase in food price rises simply became "the form which a [general] price increase has taken which might have taken other forms." (Instructional Dynamics Economics Cassette Tape 130, September 26, 1973.) As for the dollar's devaluation, that, Friedman said, "must be ruled out entirely" as a source of inflation. With respect to viewing inflation in terms of nominal wage growth in excess of productivity growth, Friedman observed: "That's arithmetically correct. Economically, it's nonsense." (Instructional Dynamics Economics Cassette Tape 127, August 15, 1973.) And, as discussed in

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[47] Friedman (1973d, p. 5).
[48] Friedman (1973d, p. 5–6; quotation from page 5).
[49] See also American Enterprise Institute (1974, p. 38). Criticism of cost-push inflation views for their propensity to explain inflation in terms of the components of the price index was common in the monetarist literature, especially in the United Kingdom—a country in which, during the 1970s, cost-push views were even more entrenched than in the United States. For example, Laidler (1976a, p. 495; 1978a, p. 72), criticized "cost-accounting exercise[s] masquerading as an explanation of inflation," R.J. Ball and T. Burns (1977, p. 16) wrote of "the bookkeeping approach to inflation," and Capie and Wood (1989, p. 96) derided the use of "adding-up economics" as the basis for the analysis of inflation. It should be acknowledged that, as emphasized in Walters (1987) and Spanos (2012), Friedman was himself not opposed to the use of identities or tautologies for providing a decomposition with which to analyze the behavior of economic variables; it was instead the practice of substituting that analysis for a deeper framework to which he took exception. In particular, in the case of cost-push analysis, he criticized the treatment of large segments of costs or the price index as exogenous.
[50] From Burns' testimony of September 12, 1973, in Committee on Banking and Currency, U.S. House of Representatives (1973, p. 336).

Section III of the next chapter, quite aside from the analytical validity of tracing the behavior of inflation to variations in unit labor cost, Friedman was skeptical of the empirical reliability of the link between these two series.

Starting in earnest later in the year and continuing for much of the subsequent decade, Friedman also rejected energy price rises as a major source of inflation (see the next chapter). In July 1973, he summed up his opposition to the special-factors explanations: "there is no doctrine in economics that is more widely believed and more fallacious than the doctrine that costs determine prices—rather than that prices determine costs." (Instructional Dynamics Economics Cassette Tape 125, July 18, 1973.)[51]

Even critics of cost-push views of inflation might balk at Friedman's proposed alternative formulation here. Certainly, in tracing the behavior of inflation to monetary policy, a rejection of the notion that (all) production costs are exogenous is vital. But it is the endogenous behavior of both prices and costs—together with their joint dependence on monetary behavior—that typically features in monetary critiques of cost-push views. Friedman's statement that "prices determine costs" would seem to clash with the notion that both series are endogenous.[52] The interpretation offered here is that Friedman used this formulation not to claim a formal causal ordering, but to underscore two points: one concerning the determination of real costs and one concerning the behavior of nominal costs. Per standard price theory, Friedman regarded the configuration of relative prices as a reflection of consumer preferences.[53] Economic efficiency produced a relationship between real costs of producing each good and that good's price.[54] With

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[51] Likewise, later, in Instructional Dynamics Economics Cassette Tape 168 (June 1975, Part 1), Friedman criticized the "basic idea that everybody has that costs determine prices." See also Friedman and Friedman (1980, p. 262).
[52] One interpretation of Friedman's remark is that the causality he was talking about pertained to the contemporaneous causal ordering of variables, *a la* Sims (1980), and that he had in mind the notion that, in any given period, goods prices might be predetermined in relation to nominal wages. This interpretation of Friedman's remark can be ruled out by the fact that the opposite position—i.e., that wages in period *t* are predetermined while prices can react to period-*t* shocks—is an important part of his aggregate supply theory (see Friedman, 1968a, pp. 9−10, and the analyses in Nelson, 2020a, Chapter 7; 2020a).
[53] That the consumer-sovereignty view of relative price determination underlay Friedman's characterization of the cost/price nexus is underscored by an observation on one of the cassettes already cited (Instructional Dynamics Economics Cassette Tape 125, July 18, 1973). He took the message of economic theory on this matter as that "things that cost absolutely nothing may still have very high prices, and that no matter how much you spend on producing something that has no use or no value to anyone, its price will be zero." (The reference to items that cost nothing to produce but command high prices may be primarily to fiat money. Friedman may also have had in mind aspects of human capital with which people are born, but which can generate high-price services. In Friedman, Porter, Gruen, and Stammer, 1981, p. 33, for example, he mentioned Frank Sinatra's voice as an example of this phenomenon.)
[54] This also reflects Friedman's critique of older economic theories, prevalent especially before the advent of marginal-utility analysis, to the effect that products or inputs had an inherent value to them aside from that assigned by consumer preferences. For example, Friedman (1955a, p. 902) characterized the development of marginal-utility

regard to nominal costs, they, being nominal series, eventually had to stay in step with price-level trends—with those trends, in turn, flowing ultimately from monetary policy actions.

Thus, Friedman regarded the economic process as more naturally viewed in terms of a dependence of costs on prices, even though both costs and prices were endogenous variables determined ultimately by exogenous shocks, economic policy decisions, and fundamental parameters such as those governing consumer preferences.

Friedman did concede *some* validity to special factors in analyzing inflation. For one thing, some of the special factors that were being cited had effects on potential output and so altered the price level consistent with a given quantity of money. But a permanent shift in potential output, once it had occurred and been felt in an adjustment of the price level, could not (for given monetary growth) alter the future inflation rate. Consequently, special factors did not explain longer-term movements in inflation even when they mattered for the level of potential output.

Furthermore, movements in food or other price categories were frequently being cited as significant drivers of inflation simply on the grounds that they were items that entered the consumer price index. Friedman strongly objected to this practice: one "cannot," he argued, simply aggregate individual price changes "and, from that, determine what inflation is going to be" (Instructional Dynamics Economics Cassette Tape 127, August 15, 1973). "On the contrary, we do better by looking at it the other way." The underlying basis for viewing inflation as a reflection of individual prices' behavior was, Friedman said, a notion that the price movement of goods in each sector is "a law unto itself, and you add the total up" to obtain the inflation rate (Instructional Dynamics Economics Cassette Tape 130, September 26, 1973). His monetary perspective on the matter suggested, in contrast, that economy-wide conditions fixed the pace of increase in the aggregate price level; with the national inflation rate so determined, higher price rises for one class of goods reduced the scope for increases in prices of other goods. Accordingly, Friedman noted during 1973 that the "incredibly rapid rise in food prices has, I believe, meant a slower rise in other prices than would have occurred." (Instructional Dynamics Economics Cassette Tape 130, September 26, 1973).[55]

The primary concession Friedman made was that over very brief periods—say, for three to four months—variations in the individual components of the price index do drive inflation, because a

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analysis as allowing "demand to be assigned its proper role and the shackles of the cost of production or, even worse, labor theory of value to be overthrown."

[55] See also the discussion of the first oil shock in Section II of the next chapter.

price increase concentrated in one sector could dominate the movement of the index. For example, and as discussed below, Friedman conceded that special factors had raised the 1973 U.S. inflation rate by 2 percentage points. But over longer horizons, as other prices in the aggregate index adjusted, the sector-specific price increase became a relative-price phenomenon only. Thus, in Friedman's view, over horizons stretching from 6 months to 2−3 years, aggregate forces came into their own in the determination of inflation, providing offsetting downward pressure on the inflation rate (Instructional Dynamics Economics Cassette Tape 130, September 26, 1973).

Renewing criticism of Chairman Arthur Burns

As inflation came out into the open in 1973, Friedman's warnings in 1971 about the dangers of the high monetary growth then occurring also came to be widely seen as having been vindicated. Even many commentators who stressed cost-push factors both in 1969−1971 and during 1973−1974 tended to acknowledge an important role played by excess demand in the latter period of renewed high inflation.

Friedman's analysis of the inflation process meant that he laid the blame for it at the door of the Federal Reserve—and, in particular, the Federal Open Market Committee (FOMC) headed by Chairman Arthur Burns. Friedman's criticisms of the record of the Burns FOMC had largely been in abeyance since his *Newsweek* columns "Irresponsible Monetary Policy" and "The Case for a Monetary Rule" in early 1972 and had even given way to strong praise late in that year (see Nelson, 2020b, Chapter 15). But, as inflation took off during 1973, Friedman revived and buttressed his earlier criticisms.

Friedman continued to be irked by Chairman Burns' acceptance or promotion of a variety of nonprice mechanisms for organizing allocations in markets. Burns did make a move in the direction of financial deregulation when the Federal Reserve Board removed the interest-rate ceiling on large certificates of deposit in May 1973 (Meltzer, 2009b, p. 868). The same month, however, on May 22, Burns introduced a new, informal control over commercial banks when he wrote to Federal Reserve member banks urging them to limit lending (Meltzer, 2009b, p. 868). Friedman's declared: "I may say I think it is a disgraceful letter," adding that the request it relayed was "extralegal" because it did not rest on formal Federal Reserve powers (Instructional Dynamics Economics Cassette Tape 123, June 23, 1973).

Shortly afterwards, during a week in which Burns was on the program of an event in Scotland commemorating Adam Smith (see Burns, 1973a), Friedman remarked that this was "is in some ways amusing," as Burns' advocacy of incomes policy had led the United States away from the reliance on market mechanisms that Smith had advocated. However, Friedman added, it was "in some ways appropriate," as Smith had happened to depart from market principles by supporting state action against high interest rates or usury, and Burns' role in recent years heading the interest-and-dividend-monitoring committee—which ran concurrently with the Nixon compulsory wage-price controls—had made the Federal Reserve Chairman, in effect, a modern proponent of interest-rate ceilings (notwithstanding his recent relaxation of CD rate ceilings).[56]

Likewise, against the spirit of financial liberalization, in mid-1973 the Federal Reserve Board introduced a marginal reserve requirement (Meltzer, 2009b, p. 868; Burger and Rasche, 1977, p. 20), applied to increases in certain categories of banks' wholesale deposit and nondeposit liability instruments. The marginal reserve requirement was increased in September 1973; it was then lifted in December 1974, only to serve as a prototype for new marginal reserve requirements imposed on bank wholesale liabilities in late 1979 (Romer and Romer, 1993, pp. 80–81).

Marginal reserve requirements were among the most distortionary of the Federal Reserve's control instruments, because agents had strong incentives to find ways of bypassing the requirements.[57] Furthermore, to Friedman, marginal reserve requirements made no substantial contribution to monetary control: open market operations could achieve the same aim as changes in reserve requirements, whether of the ordinary or marginal type.[58] Furthermore, measures directed specifically at restricting banks' issuance of wholesale deposits were not powers that Friedman considered necessary, as he largely excluded banks' wholesale liabilities from his preferred definition of money.[59]

Friedman's criticisms of Burns' financial-regulation measures were, however, skirmishes when compared with his case against Burns' monetary policy record and the FOMC's strategy. In

[56] This Friedman observation was in Instructional Dynamics Economics Cassette Tape 122 (June 6, 1973).
[57] Earlier, Goldenweiser (1949, p. 15) had put marginal reserve requirements in a favorable light but had noted that they had not been deployed in the United States to date. In 1952, when he and Friedman were co-contributors to a joint statement by U.S. economists' on using monetary policy against inflation, Goldenweiser had been part of the majority opinion urging the introduction of marginal reserve requirements in the United States, while Friedman had dissented from this recommendation (see Samuelson and others, 1952, pp. 387, 390).
[58] This point, already articulated at length in Friedman (1960a, pp. 30–35), was something Friedman reiterated in mid-1973: "It's worth emphasizing that there is absolutely nothing that can be done by changes in reserve requirements that cannot be done by open market operations." (Instructional Dynamics Economics Cassette Tape 124, July 4, 1973.)
[59] See Nelson (2020b, Chapter 14).

1973, Burns' conduct of Federal Reserve open market policy since 1970, and especially since 1971, was the subject of a renewed and intensified indictment from Friedman.[60] Burns' occasional protests notwithstanding, Friedman saw these actions as responsible for the rapid growth of the M1 and M2 monetary aggregates since 1971 and the outbreak of inflation in 1973. It was on this subject that Friedman became, in 1973–1974, involved in what proved to be his most prominent public confrontations with Burns.

Friedman articulated a critique of Burns' record on monetary policy in June 1973, when he gave his first in-person Congressional testimony in eighteen months. The testimony was, first and foremost, on the working of the new system of floating exchange rates (discussed in the next chapter). But the testimony came in the wake of what Friedman had called "incredible" increases in the U.S. wholesale-goods price index (Instructional Dynamics Economics Cassette Tape 119, April 25, 1973) and of Friedman's declaration that there had been an "enormous step up in the rate of price increases" in the year so far (Instructional Dynamics Economics Cassette Tape 120, May 11, 1973). Not surprisingly, therefore, the question-and-answer session of the testimony moved onto the topic of U.S. monetary policy setting.

In this session, Friedman declared: "Some of my best friends are at the Fed." He added, alluding to Burns: "And that is a literal statement." But he went on to say that the prior eighteen months' monetary growth had been "decidedly too high" and that the Federal Reserve "must bear a great deal of responsibility for an economic climate which underlies the rapid price explosion in the first few months of this year."[61]

Having thus foreshadowed that he would resume hostilities with Burns, Friedman did so in earnest in August 1973. For the past year, Robert Barro had been a new colleague of his in the University of Chicago's Department of Economics. And—apparently somewhat to Friedman's irritation—Barro did not hesitate to remind him of the elation Friedman had expressed publicly when Burns became Chairman in early 1970 (Jeremy Siegel, interview, September 17, 2013).[62] Against this background, in a cassette commentary early in August Friedman said of Burns' record (Instructional Dynamics Economics Cassette Tape 126, August 2, 1973): "I must confess that I am baffled, because I did not expect it." He recalled that, in the knowledge of Burns' record as an economist and government official prior to 1970, he had "more or less settled back"

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[60] In addition to what follows, see the discussion in Nelson (2007, 2016).
[61] From Friedman's testimony of June 21, 1973, in Joint Economic Committee (1973a, p. 130).
[62] Barro joined the Department of Economics during the first half of the calendar 1972 (Barro, 1972, p. 598; American Economic Association, 1974, p. 21). Subsequently, he was, as already noted, a presence in the money workshop when Friedman resumed his hosting of that workshop in the second half of 1972.

upon Burns' appointment as Chairman, expecting monetary growth to proceed henceforth at a rate that was noninflationary and fairly steady, only to find himself being "disappointed in both respects."[63]

Friedman then lashed out, berating Burns for the "utterly unjustified and irresponsible policy of the Fed in increasing the money supply so rapidly over the past few years" (Instructional Dynamics Economics Cassette Tape 126, August 2, 1973). Soon thereafter, Friedman provided a sustained critique of Burns' record in a *Newsweek* column titled "The Inflationary Fed" (*Newsweek*, August 27, 1973). This column reiterated Friedman's formulation of the matter—"inflation is made in Washington"—and stated that his hopes in January 1970 when the Federal Reserve ostensibly shifted focus from interest rates to monetary growth "have been shattered. Monetary growth has been both higher and more variable in the past three and a half years than in any other postwar period of equal length."

Burns did not read Friedman's column on a regular basis.[64] Furthermore, he was not well disposed toward responding to Friedman's criticisms; indeed, it had become rare for him to refer to Friedman publicly.[65] But in September 1973, Burns was pressed in Congressional questioning about the column and so submitted a written reply to it. This submission—which in the event did not mention Friedman at all—argued that "Federal Reserve policy has been to resist expansionary forces," with reference being made to the fact that M1 growth had been below nominal GNP growth in 1972–1973.[66]

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[63] The early-1970 praise for Burns was documented in detail in Nelson (2016; 2020b, Chapter 15). A more cautious note had been voiced by Friedman in December 1969, when he had stated (*Los Angeles Times*, December 7, 1969) that there was a 50 percent chance that even with the switch to the Burns regime the average rate of inflation in the next five years will be same as previous five, because of overreaction to recession. However, even this remark had not envisioned the scenario actually realized—of inflation being much higher in 1970–1974 than in 1965–1969.
[64] During 1972, Board staff provided a file of Friedman's columns for the first half of the year after Burns expressed curiosity about their content. Later, Friedman tipped Burns off about his August 27, 1973 column, by sending him a pre-publication copy. (Information from Burns papers, Gerald Ford Presidential Library.)
[65] This reticence contrasted with that of Burns' fellow Board Governor, Jeffrey M. Bucher, who delivered a speech in April 1973 ostentatiously titled "Why Is Friedman Like Freud?" (Bucher, 1973). This was apparently the only speech by a Board Governor to mention Friedman in its title until Ben Bernanke's (2002, 2004a) two speeches concerning Friedman. The content of Bucher's speech was more standard than the title. The speech did contain a reaffirmation of the point, stressed by policymakers in 1970–1971 (see Nelson, 2020b, Chapter 15), that the FOMC's 1970 change in procedures had not constituted an endorsement of monetarism. Bucher observed (p. 5): "But we do not regard the monetary aggregates as the only important star in the firmament. That is to say that I believe there is little, if any, tendency at the Federal Reserve Board to mistake the monetarist view of economic behavior as a viable substitute for the whole of economic thought." (As stated, this was not a position from which Friedman would be likely to dissent, as he did not see monetarism as pertaining to all economic thought; in particular, he did not see monetarism as seeking to displace or challenge mainstream microeconomics.)
[66] In Committee on Banking and Currency, U.S. House of Representatives (1973, p. 336).

From the monetarist perspective, this did not constitute a satisfactory response: M1 growth had been high in absolute terms; the fact of a spread of nominal GDP growth above M1 growth was not evidence of easy monetary policy. Indeed, as Friedman had discussed in his *Morgan Guaranty* piece early in 1973, M1 velocity had exhibited an upward trend over the postwar period, as the community economized on currency and demand deposits, which did not bear interest. In the case of M1 growth, the problem as Friedman saw it was that in 1971−1973 it had been allowed to be 8 percent, instead of being kept at 4 percent; in the latter case, he contended, average nominal income growth would have been commensurately lower.[67]

In another respect, Burns was aided by the form of the question asked of him at the hearing, as the coverage of the question was confined to monetary policy in 1972−1973, while Friedman's column had criticized overall monetary policy since 1970. Burns' response stated: "If money supply growth had been somewhat slower in 1972, it is doubtful that price behavior in the first half of 1973 would have been appreciably better."[68] Even Friedman could agree with this observation, in light of his contention that the effects of monetary policy on inflation made themselves most felt with an 18–24 month lag. But, ahead of making this observation, Burns made a remark with which Friedman could not agree: "The recent rate of inflation we have been experiencing cannot be attributed in any significant degree to monetary policy."[69] Burns instead cited "special factors that have tended powerfully to raise prices this year," including food price increases, bottlenecks in availability of industrial materials, and U.S. dollar devaluation.[70]

The citation of special factors as causes of inflation had, of course, underpinned Burns' original advocacy of wage-price guidelines in 1970 and his endorsement of the 1971 imposition of wage-price controls. His position on controls did evolve somewhat during 1973, as will be seen below. However, that year most definitely saw Burns continue to appeal to special factors when analyzing inflation.

Burns' cost-push views figured prominently again in the next run-in with Friedman, which was also in the context of a reply to a member of Congress, this time a senator. With Friedman's criticisms of U.S. monetary policy gaining ground, Chairman Burns wrote a letter to Senator William Proxmire in November 1973 (subsequently published in Federal Reserve district banks'

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[67] See Friedman's testimony of June 21, 1973, in Joint Economic Committee (1973a, p. 130).
[68] In Committee on Banking and Currency, U.S. House of Representatives (1973a, p. 337).
[69] In Committee on Banking and Currency, U.S. House of Representatives (1973a, p. 337).
[70] In Committee on Banking and Currency, U.S. House of Representatives (1973a, pp. 336–337); quotation from page 336.

research periodicals and in the Board's *Federal Reserve Bulletin*: one such printing was Burns, 1973b) that apparently rejected the notion of Federal Reserve responsibility for inflation while also, seemingly, denying much linkage between Federal Reserve actions and monetary growth.

To the previous list of world economic developments, devaluation, and commodity price changes, Burns added environmental controls as a source of recent inflation.[71] Burns' analysis prompted Friedman himself to let fly in a rebuttal, which also took the form of a letter to Proxmire, in early 1974.[72] This rebuttal made headlines (for example, *New York Times*, February 26, 1974) and would see print as an article in a number of Federal Reserve Bank bulletins, including the *Federal Reserve Bank of St. Louis Review*.[73]

In this rebuttal, Friedman seized on Burns' (1973b, p. 21) statement, "The severe rate of inflation that we have experienced in 1973 cannot responsibly be attributed to monetary management..." "As written," Friedman wrote acidly, "this sentence is unexceptionable. Delete the word 'severe,' and the sentence is indefensible."[74] Indeed, because he judged that the statement's construction, by conveying a different impression from the actual content of the statement, made it "about as misleading a statement as anybody can make." (Instructional Dynamics Economics Cassette Tape 135, December 4, 1973.)

Commodity price increases and other one-time events, Friedman said, might explain why U.S. inflation in 1973 reached 8 percent instead of 6 percent, but not why inflation could reach 6 percent in the first place.[75] Furthermore, the longer the period one considered, the less valid it

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[71] On later occasions, along with the OPEC actions, other items Burns would add to the causes of inflation were U.S. grain sales to the Soviet Union (discussed in Chapter 7) and (in Burns' testimony of August 6, 1974, in Joint Economic Committee, 1974b, p. 268) increases in public utility prices, which would "release new inflationary forces."

[72] Although the published version in Friedman (1974d) is undated, the copy of the letter in the Federal Reserve Board's files is dated February 20, 1974; likewise, the statement was published in Joint Economic Committee (1974a, p. 740–744) with a cover letter (p. 739) dated February 20, 1974. However, both the published and unpublished versions of the letter contain a footnote indicating that the letter was largely drafted before January 31, 1974 (see Friedman, 1974d, p. 22, for the footnote) and a second-draft version of the letter, closely resembling the final version, was dated January 28, 1974 (Anna Schwartz papers, Duke University).

[73] See, for example, Friedman (1974d, 1974e). Friedman's request that it be published in the *Federal Reserve Bulletin* was turned down. (Burns papers, Gerald Ford Presidential Library.) Friedman had earlier reacted to the Burns letter in his cassette commentary series (Instructional Dynamics Economics Cassette Tape 135, December 4, 1973). Karl Brunner also wrote a detailed critique of Burns' letter (Brunner, 1974), although this critique was somewhat overshadowed by Friedman's much more well-known rebuttal.

[74] Friedman (1974c, p. 20). Although Friedman did not note it, Burns' use of the word "severe" made the Burns statement a more qualified one than Burns' September *de facto* reply to Friedman. Thus, by Friedman's lights, that earlier statement was indeed "indefensible."

[75] Friedman's concession that 2 percentage points to 1973's inflation rate could be due to essentially nonmonetary factors appeared in both Instructional Dynamics Economics Cassette Tape 135 (December 4, 1973) and Friedman

was to invoke nonmonetary events to explain inflation. Consequently, "the Fed's long-run policies have played a major role in producing our present inflation."[76] Friedman also discounted Burns' stress on the looseness of the links between Federal Reserve actions and monetary growth, and between monetary growth and the course of total spending. As he often did, Friedman presumed that Burns and he largely shared the same analytical framework, especially regarding the determination of inflation.[77] Friedman interpreted Burns' verbal formulations as evasions that failed to face up to the implications of that framework.

In retrospect, however, it appears appropriate instead to view Burns' views of the determination of the money supply, aggregate spending, and—especially—inflation as fundamentally different from Friedman's, in a direction that emphasized nonmonetary factors and the ineffectiveness of monetary policy tools.[78]

This interpretation is underscored by the further analysis Burns provided when, once again, Congressional questioning forced the Chairman to offer a reply to Friedman's letter in the week that it appeared. Burns appeared irritated that Friedman had written a reply to Burns' letter to Proxmire. That letter, he told Proxmire, was in response to Proxmire's questions—it was "not addressed to Mr. Friedman's questions." Burns added: "Mr. Friedman is a very dear friend of mine," Burns added. "I don't wish to engage in any debate with him or with any other economist."[79] Burns cited the "extraordinary" U.S. budget deficits from 1971 to 1973; these had had "an enormous influence on the rate of inflation in this country," including, he implied, by

(1974d, p. 20). Later, Friedman and Schwartz (1982, p. 104) granted that, for the United States, 1.5 percentage points of (output-deflator) inflation resulted from the first oil shock of 1973−1974 (see the next chapter). But this assignment should be regarded as pertaining essentially to the calendar-1974 U.S. inflation rate; especially when annual-average data are used, it is the case that much of the OPEC oil shock's direct impact on U.S. final-goods prices was in 1974 rather than 1973. These Friedman discussions of the 1973−1974 U.S. inflation may have been the basis for a later textbook's generalization (without citation of a specific paper by Friedman or by other monetarists) that monetarists assigned 2 percentage points of inflation to the first oil shock (Collins and others, 1984, p. 214).

[76] Friedman (1974d, p. 21).

[77] With regard to spending determination, Friedman recognized that Burns viewed fiscal policy as an important influence in its own right, and that Burns did not restrict its influence (as Friedman largely did) to operating via the reaction of the monetary authorities; thus, Friedman affirmed that Burns "would give much greater emphasis to fiscal effects than I would" (Instructional Dynamics Economics Cassette Tape 126, August 2, 1973). Burns' emphasis on nonmonetary determinants of spending was stressed in Hetzel's (1998) characterization of Burns' views. It also was evident in the text of Burns (1973b) and in Burns' impatient reaction, when confronted in Congressional testimony with Friedman's analysis: "one can talk about money supply from now until doomsday" (February 26, 1974, testimony, in Joint Economic Committee, 1974a, p. 748).

[78] Friedman occasionally noted that Burns had changed his position from the 1960s on inflation (Instructional Dynamics Economics Cassette Tape 126, August 2, 1973) yet, as noted in the text, Friedman's (1974d) dissection of Burns' analysis reads as though Friedman cannot quite believe that Burns' views are so different from his own.

[79] From Burns' February 26, 1974, testimony, in Joint Economic Committee (1974a, p. 745). The *New York Times* report (February 27, 1974) on the testimony instead rendered Burns as referring to Friedman as his "old friend."

forcing higher rates of monetary growth.[80] Burns saw this as a key weakness of "Mr. Friedman's very interesting letter to you, [in which] you will find not even one word about fiscal policy."[81]

Although, as discussed later in this section, Friedman was sympathetic in general terms to the notion that the growth of government had been an impetus for the advent of peacetime inflation in the United States in the postwar period, he believed that the scale of deficits experienced in the 1970s was not large enough to have prevented Burns from pursuing a noninflationary monetary policy if he had pressed ahead with doing so. Indeed, neither the data on budget deficits nor on real interest rates in 1971−1973 squared with Burns' suggestion that deficits were a source of upward pressure on monetary growth (see Hetzel, 1998, and Nelson, 2005). By the standards of recent prior years, deficits were indeed high—but they were well below levels that had proven consistent with low monetary growth in many countries. Furthermore, the most standard case in which a central bank might respond to higher deficits by raising money growth is when it is preventing real interest rates from rising or in limiting their rise. But real interest rates actually fell in the 1970s—a development that suggests that the easy stance of monetary policy in that decade went far beyond what could plausibly be ascribed to a response to deficits.

In another area, Burns was more sanguine than Friedman. Burns quoted a sentence from Friedman's letter, "There is literally no way to end inflation that will not involve a temporary, though perhaps fairly protracted, period of low economic growth and relatively high unemployment."[82] Burns countered: "I think that we can end inflation over the next 2 or 3 years without going through a period of heavy unemployment."[83] Although he was a gradualist with regard to how to carry out disinflation, Friedman did regard a period of slow expansion of economic activity as an inevitable part of a move to lower inflation. On his reasoning, a stretch of time in which output was below potential was required, in order to generate the requisite disinflationary pressure. Burns, in contrast, saw a great deal of existing U.S. inflation as not originating in excess demand and hence as removable via nonmonetary measures.

However, with regard to the specifics of the nonmonetary measures, Burns during 1973 did

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[80] From Burns' testimony of February 26, 1974, in Joint Economic Committee (1974a, pp. 745−746).
[81] From Burns' testimony of February 26, 1974, in Joint Economic Committee (1974a, p. 745). This remark was superficially similar to the criticism that Brunner and Meltzer's (1972, p. 842) made of Friedman in the *JPE* symposium, for "neglecting fiscal policy." However, when it came to the inflation process, their own assessment downplayed fiscal influences (for given monetary growth) and so was similar to Friedman's and dissimilar to Burns'.
[82] Friedman (1974d, p. 23), quoted in Burns' testimony of February 26, 1974, in Joint Economic Committee (1974a, p. 747).
[83] From Burns' testimony of February 26, 1974, in Joint Economic Committee (1974a, p. 748).

move somewhat away from his pro-controls position. In February 1973, he suggested that some compulsory wage and price controls were needed, perhaps targeting firms and unions with economic power (see *Kansas City Star* (Missouri), February 16, 1973, and Nelson, 2005) and in June 1973 he was still endorsing controls (*Chicago Tribune*, June 10, 1973). However, Burns was publicly critical of the new Nixon price freeze. This response to the freeze was a watershed, as Burns two years earlier had pressed Nixon to include a price freeze in his anti-inflation arsenal.[84] By early August 1973, Burns was expressing sympathy with the position that, in future, the United States should shift away from reliance on controls, and toward a greater emphasis on monetary and fiscal policies, in fighting inflation (*Kansas City Times* (Missouri), August 4, 1973). He reaffirmed this view later in the year, in Burns (1973b, p. 21), suggesting on this occasion that "controls on wages and prices cannot be expected to yield the benefits they did in 1971 and 1972." But Burns, notwithstanding his changed views after mid-1973 regarding compulsory wage and price controls, continued to advocate incomes policies against inflation and to express skepticism about what monetary policy alone could do. This aspect of Burns' 1973−1978 perspective is documented in detail in DiCecio and Nelson (2013). Burns thus continued to be an advocate of other types of incomes policy after the controls expired in 1974.

Monetary policy/fiscal policy interactions and sources of high monetary growth

Friedman's March 1973 lecture noted that, upon acceptance of his monetary view of inflation, it remained to be explained *why* monetary growth was excessive.[85] This issue had increased in importance because the fact of excessive monetary growth in the early 1970s had, by 1973−1974, become less a matter of dispute.

Chairman Burns himself in late 1973 and early 1974 publicly accepted that monetary policy had been too loose in 1972 (see Burns, 1973b, p. 21, and his testimony of February 26, 1974, in Joint Economic Committee, 1974a, pp. 719, 745−746). Friedman was aware of both these statements and indeed quoted each of them (the latter in *Wall Street Journal*, August 21, 1975). But in the

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[84] In Congressional testimony of June 27, 1973, Burns said (Joint Economic Committee, 1973a, p. 181): "A price freeze is a dangerous thing, and I hope that it will not last, as I indicated earlier, 60 days. It should be terminated, in my judgment, much sooner if at all possible." In contrast, just over two years earlier, in a memorandum to President Nixon of June 22, 1971, Burns had recommended that a six-month wage and price freeze be implemented by January 1972 if a less strict national incomes policy was insufficiently successful (Burns, 1971a, p. 8). (This memorandum, which has been available for the past several years on the Federal Reserve Bank of St. Louis' website, was earlier quoted in Meltzer, 2005, p. 169, and was also described in Meltzer, 2009b, p. 751. It has also been reproduced as an appendix in Shultz and Taylor, 2020, pp. 73−79.)

[85] Friedman (1973b, p. 6).

1980s he and Rose Friedman would insist that they did not know of any official statement by a member of the Federal Reserve Board that admitted a policy error.[86]

It is interesting to speculate about why Burns' 1973–1974 statements were apparently not counted as being such admissions. One reason may be that Burns' (1973b) admission was highly qualified: it referred to the rapid M1 growth of 1972, rather than to the high M2 growth (in both 1971 and 1972) on which Friedman focused. Furthermore, Burns (1973b, p. 21) suggested that the state of excessive policy ease was only something that "may" be evident "[i]n retrospect," and so not amounting to a clear-cut policy error.[87]

There was another reason why Friedman did not see Burns' assessment as implying true contrition. This, of course, was the fact that Burns attributed a good part of monetary growth, even when facilitated by Federal Reserve actions, to larger federal budget deficits. Burns' position on this matter was echoed by Robert Holland, who became a new Federal Reserve Board Governor in 1973 and attributed 1972's high monetary growth largely to fiscal deficits (*Kansas City Times* (Missouri), October 18, 1973).

For his own part, Friedman did perceive the expanded size of government since World War II as conducive to higher monetary growth. He cited this factor in an analysis in late 1974 (*Wall Street Journal*, November 23, 1974) and, earlier in the year, he suggested that legislated deficits had helped promote inflation.[88] These analyses lined up with those he had given in prior decades: the notion that the need to finance an upward trend in government spending might lead to greater use of money creation had featured in his 1954 talk on the danger of a secular peacetime inflation and in his 1963 lecture on inflation.[89] Friedman was, nevertheless, very skeptical of the near-automatic relationship between deficits and monetary growth posited by Burns. Friedman saw what relationship there was between deficits and money creation as reflecting a voluntary monetary policy decision—acquiescence on the part of the Federal Reserve—rather than any automatic responsibility or obligation by the central bank to accommodate fiscal deficits.

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[86] Friedman and Friedman (1984, p. 96; 1985, p. 94). See also *Newsweek*, July 24, 1978.
[87] Similarly, in testimony of June 27 and August 3, 1973 (Joint Economic Committee, 1973a, p. 184; 1973b, p. 231) as well as February 26, 1974 (Joint Economic Committee, 1974a, p. 719), Burns said that judgments that a move to restriction should have started sooner, or that monetary policy should have been less expansive, were those he likely agreed with only "[i]n retrospect." (The second of these quotations, that of August 1973, was also given in Meltzer, 2009b, p. 838, albeit using a secondary source that had somewhat misquoted the Burns statement.) These statements did amount to qualified admissions of error. But they also sent a message to Burns' interlocutors that the best analysis available at the time did, in fact, justify the policy actions taken by the Federal Open Market Committee.
[88] Friedman (1974h, p. 20). See also Friedman and Friedman's (1985, p. 107) observation that the "rising and variable rate of inflation is… itself primarily a result of the growing role of government…"
[89] Friedman (1954a, 1963). See also Nelson (2020a) for discussion.

Furthermore, as already indicated, the scale of monetary growth in 1971 and 1972 exceeded what could plausibly be attributed to accommodation of fiscal policy. Accordingly, in 1974 Friedman would state that the present inflation owed a great deal to Federal Reserve behavior that was separate from President Nixon's choices regarding economic policy (Instructional Dynamics Economics Cassette Tape 151, August 7, 1974).

Friedman did, however, accept that a tighter fiscal policy would make it easier to pursue monetary restraint. Fiscal policy developments in 1973 and 1974 would alarm him on this score. The Friedmans would later suggest that "from about 1973 or 1974 onward" government spending "rose without checks."[90] Statistics on U.S. government spending do suggest that these years were something of a turning point. Barro (1978, p. 579) would suggest that there was little evidence that the relative size of the U.S. public sector increased from 1968 to 1976. But this judgment rested on defining government spending narrowly as purchases—instead of overall outlays—and Friedman was among those urging that outlays were the more relevant measure of public expenditure. U.S. federal outlays moved up 2.5 percentage points of GDP in fiscal year 1975 to 20.7 percent, the first time in the postwar period that that share had exceeded 20 percent (Council of Economic Advisers, 2018, Table B−18, p. 553). And general (that is, across branches of the public sector) U.S. government outlays rose from 30.6 to 34.6 percent of GDP in the United States in the calendar years 1973 to 1975 (*OECD Economic Outlook*, June 1989, Table R−14, p. 185).

Friedman himself, using a narrower national-income concept as the denominator, suggested that the government-spending share had already been around 40 percent prior to the recent surge.[91] He believed the share was excessive at that level: "I doubt that anyone is bold enough to say that 40 percent of all the good things in life come from the government" (*Chicago Today*, October 26, 1972). Reflecting this judgment, as well as his acknowledgment that fiscal restraint would reduce pressure for monetary growth, Friedman in December 1974 called for a 10 percent across-the-board cut in federal spending, accompanied by a reform of welfare arrangements in order to direct funds to the neediest individuals (*Daily Courier*, December 7, 1974). A couple of months earlier, he had remarked (*Chicago Tribune*, October 9, 1974): "The question is, will we have the courage to stick to a policy of fiscal and monetary restraint in order to lick inflation for good, or will we be tempted to move in the other direction again?"

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[90] Friedman and Friedman (1984, pp. 25−26; 1985, p. 31).
[91] In *Newsweek*, August 5, 1974, Friedman gave the government-spending share as 39.1 percent in 1970. In *Los Angeles Times* (June 8, 1973) he gave the share as 43 percent.

But, as suggested above, although he saw fiscal restraint as an appropriate part of a disinflationary package, Friedman did believe that, even in the absence of fiscal restriction, it was possible and desirable for the Federal Reserve to achieve a policy that ended inflation. The FOMC had, of course, engaged in monetary restraint for stretches of time in the 1960s—and would again in 1973−1974—but Friedman cited aspects of the Federal Reserve's reaction function that led monetary restriction to be abandoned too soon and for excessive monetary ease to be the predominant problem since 1960. One was already discussed above: the conviction by the Federal Reserve authorities—most notably, Burns—that, in large part, inflation control should appropriately be assigned to nonmonetary instruments.

Another problem was that, since the mid-1950s, it was rarely in doubt in the United States that monetary policy had sizable effects on output in the short run. Thus, as Friedman acknowledged, the skepticism among 1970s policymakers that aggregate demand restriction would reduce inflation was accompanied by their acceptance that such restriction would produce higher unemployment, at least in the short run.[92] This meant that even if monetary restriction was seen to be a necessary part of reducing inflation, the prospect of the real costs of disinflation might discourage the authorities from enacting such restriction—and encourage them to overreact when recessions did occur.[93] These obstacles motivated Friedman's advocacy of indexation in 1973−1974—a campaign discussed in Section II below.

The reluctance to accept monetary restraint was reinforced by the United States' commitment to a full-employment policy, whose pursuit Friedman saw as having the effect of pressuring the Federal Reserve to pursue expansionary policies.[94] Thus he saw the New Economists of the 1960s, by making the full-employment goal even more central to U.S. economic management, as having helped "put us into the pickle we're in now" (Instructional Dynamics Economics Cassette Tape 153, September 6, 1974).

This critique of full-employment policy allowed for the possibility that the full-employment goals were too ambitious: that is, that potential output was overestimated when policymakers sought a zero output gap. Orphanides (2003) forcefully advanced the argument that such overestimation is quantitatively very important for the understanding of U.S. monetary policy mistakes in the 1970s and that it was intensified by delayed recognition of the slowdown in

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[92] See Friedman (1973b, p. 6).

[93] See, for example, Friedman's discussion in *Fortune* (July 1974), as reprinted in Friedman (1975a, pp. 151−152).

[94] See, for example, his discussions in Friedman (1974h, p. 20; 1975a, p. 60) of the pressures on the Federal Reserve.

productivity growth that occurred around the end of 1973 (a slowdown discussed in later chapters of this book).

Once inflation burst out in 1973, some of those pointing to excess demand as the culprit did suggest that it was likely that the economy's potential had been badly overstated. Friedman was not at the forefront of these voices. He did insist that the United States' inflationary policy had had its counterpart a period of "high fever" in which output boomed above potential.[95] But he was not among those analysts closely scrutinizing official potential-output estimates and suggesting improvements; and, as discussed later in this book, he was not ahead of the pack in noting the post-1973 growth slowdown. Nonetheless, as stressed by Orphanides and Williams (2013) and documented in detail in Nelson (2020a, Chapter 8), Friedman eschewed policy prescriptions that were geared to the levels of unemployment or output or to estimates of the full-employment values of these level series. Because, as Orphanides (2003) showed, output-gap measurement errors during the 1970s dominated the levels of real-time gap series but not their corresponding growth rates, Friedman's policy prescriptions and monetary analysis during the decade were considerably less vulnerable to errors in estimates of potential output than were those made by many in economic commentary policy circles in the same period.

Interest rates

In 1972, Friedman's fellow *Newsweek* economics columnist Henry Wallich had ventured to observe (Wallich and Sandberg, 1972, p. 3): "We can have, by 1977, short- and long-term rates well below today's, provided inflation, and the expectation of it, [have] been reduced to the level prevailing in the early 1960s… Our guess as to the rate of inflation that the authorities will cause or permit to exist within the economy is in the 2–3 percent range. Adding our inflationary expectation to our forecast of the real interest rate, we arrive at a nominal rate in the range of 6–7 percent."

Some indications of more near-term prospects for interest rates were provided in a newspaper survey in early 1973. One of the forecasting firms surveyed—White, Weld and Company—suggested: "We believe that the rate of inflation has moved downward more than generally realized. Long-term interest rates will decline in 1973." (*Kansas City Star* (Missouri), January

[95] Friedman (1974a, p. 15). Later in 1974, Friedman described the U.S. as experiencing excessive money (that is, nominal) demand (*Inflation*: *The Money Merry Go Round*, PBS, October 7, 1974). This description may have been in recognition of the fact that much of the excess spending had, by this stage, been transmitted to inflation expectations and was accordingly making itself felt less than previously in a response of real output.

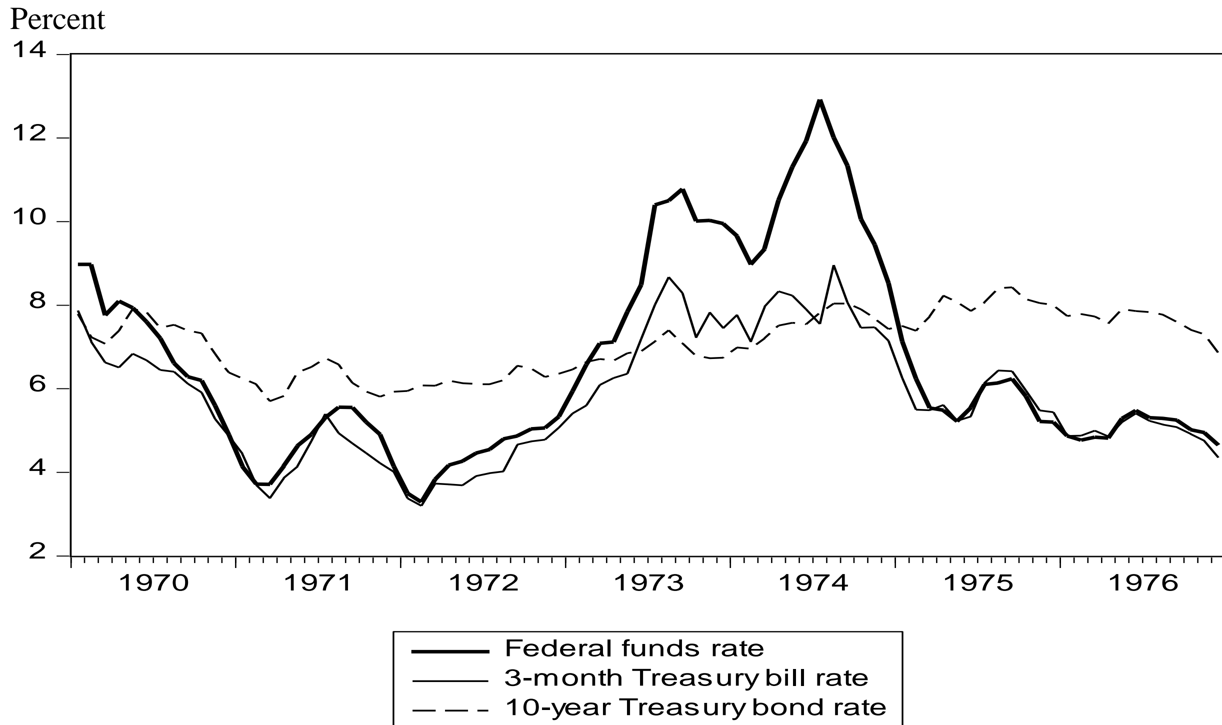

Figure 2. Short- and long-term interest rates, 1970–1976.
Source: Monthly data in the Federal Reserve Bank of St. Louis' FRED portal on the federal funds rate, the three-month rate (secondary) on Treasury bills, and the ten-year constant-maturity Treasury rate.

14, 1973.)

Actual interest rate behavior (shown in Figure 2), like that of inflation, defied these predictions. In December 1972, the federal funds rate averaged 5.33 percent, the three-month Treasury bill rate 5.07 percent, and the ten-year Treasury rate 6.36 percent. Not until the final months of 1976 would the two short-term rates take values as low as this, and in the intervening years—as part of what Friedman described (in Instructional Dynamics Economics Cassette Tape 183, January 1976, Part 1) as the "much greater variability of interest rates over the past few years"—the federal funds rate had stood at 12.92 percent and the Treasury bill rate near 9 percent, with both peaks being reached in the summer of 1974.

And longer-term interest rates, far from being well below their 1972 levels by 1977, never returned to 1972 levels over the rest of the decade. Contrary to Arthur Burns' observation in October 1972 that there were "no basic economic reasons why there should be any strong upward pressure on these long-term interest rates" (quoted in *Evening Sun*, October 13, 1972), the 10-year Treasury rate rose during 1973 and peaked at an 8.04 percent monthly average in August-September 1974. The period through early April 1974 would lead a financial reporter to

see "inflation and continually soaring interest rates [as] vying with each other as the [U.S.] market's major concern" (*Financial Times* (London), April 13, 1974), while events later in the year would be remembered by a financial columnist as "those dismal days of late 1974 when interest rates were going through the roof and it looked as though the world was coming to an end" (*Kansas City Star* (Missouri), October 17, 1979).

Likewise, Burns had predicted in March 1970 that the mortgage rate in 1972 would be lower than it was at the time he was speaking.[96] So it was: the mortgage rate was 9.29 percent in March 1970 and 7.57 percent in December 1972—but the mortgage rate then rose during 1973 and in May 1974 exceeded its March 1970 value. It would stand in double digits in August-October 1974, before moving down to 9.51 percent in December.[97]

The upshot of the developments just described was that, after 1972, there was a drastic alteration in the time-series pattern of nominal variables. The U.S. inflation rate had already shifted, over the period from the mid-1960s to the early 1970s, from being a modestly serially correlated series to one with autocorrelations above 0.7 in annual data (Klein, 1975a, p. 134; 1975b, p. 467).[98] But once data from the early 1970s were included, a unit root process became a good approximation of U.S. inflation behavior (King, Plosser, Stock, and Watson, 1991, p. 824).[99] Indeed, for an unsettling stretch of time in the mid-1970s, it looked as though the behavior of inflation had shifted even beyond unit-root characteristics into a pattern that economists seldom need to consider in analyzing modern advanced economies: a strictly explosive process for inflation. McCallum (1994, p. 235) found that, as the four quarters of 1974 were added to the sample period for an autoregression for inflation, the coefficient sum on lagged inflation rose

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[96] See his testimony of March 18, 1970, in Committee on Banking and Currency, U.S. Senate (1970a, p. 10).
[97] See the FRED series on "30-Year FHA Mortgage Rate: Secondary Market," available at *http://research.stlouisfed.org/fred2/data/FHA30.txt*.
[98] It was this shift up that contributed to the failure to reject the natural rate hypothesis in Gordon (1972), a situation analyzed in Nelson (2020b, Chapter 14). Nelson and Schwert (1977)—a paper that would be cited by Friedman and Schwartz, 1982, p. 479)—argued for treating CPI inflation as difference-stationary even for the period January 1953-July 1971 (see Nelson and Schwert, 1977, p. 481). In retrospect, however, this can be seen as only a rough approximation, whereas data from later in the 1970s pointed more strongly to a unit-root inflation process (if the choice was restricted to being between a stationary autoregressive process and a unit-root process).
[99] Levin and Piger (2004, p. 6) cited Nelson and Plosser (1982) as finding that "postwar U.S. inflation exhibits very high persistence, approaching that of a random-walk process." In fact, however, Nelson and Plosser argued that inflation was stationary—perhaps not a surprising position on their part, as they used a long annual-data sample, ending in 1970, during which inflation showed a decided tendency to go back to its mean value. Specifically, Nelson and Plosser (1982, p. 148) found a first-order autocorrelation for consumer price inflation of 0.58 for 1860−1970 and a first-order autocorrelation for GNP deflator inflation of 0.43 for 1889−1970. In contrast, King, Plosser, Stock, and Watson (1991) found U.S. inflation was $I(1)$ when a long postwar sample of 1954:Q1−1988:Q4 was used.

from 0.879 to 1.016—a point estimate implying an explosive time-series process for inflation.[100] A parallel development took place with respect to short-term nominal interest rates. In a Ph.D. dissertation that came to Friedman's attention, Shiller (1972, p. 49) observed that "the time series of short rates has shown no tendency to diverge…"[101] Within a couple of years, Shiller's comment became obsolete: a first-order autoregression for the federal funds rate for the twenty-year sample period August 1954-July 1974 yields an autoregressive coefficient of 1.012, implying an explosive interest-rate process—the "tendency to diverge" that Shiller had seen as alien to interest-rate behavior in the United States.

These signs of strictly explosive behavior receded before long: in the case of interest rates, due to what was called a "dramatic downturn in short-term money market rates" in the second half of 1974 (*Bankers Monthly*, October 15, 1974); in the case of inflation, thanks to the sharp falling-off in inflation over 1975 and 1976. But interest rates and inflation had, as already noted, reached heights during the 1973–1976 period well beyond those in the earlier period, and, in the case of inflation and longer-term interest rates, ended 1976 above their starting levels at the end of 1972.

In his April 1972 lectures in Israel, Friedman had observed: "By now, one lesson that Irving Fisher tried to spread some 75 years ago has been learned. There is an important difference between nominal and real interest rates."[102] At the time, he could look back at the concurrent rises in interest rates and inflation in the 1960s and their falling-off together in 1970 and into 1971. Therefore, as of early 1972, the most recent evidence in favor of the Fisher effect had been the fact of jointly *declining* rates of interest and inflation. But these declines (which in any event were partly illusory in the case of inflation, being a reflection of wage and price controls) proved to be ephemeral. Friedman had warned in 1970 that if monetary restraint was not abandoned, "interest rates over the next decade will be decidedly lower than they are now" (Instructional Dynamics Economics Cassette Tape 45, February 26, 1970). But he had also cautioned that "the prudent man will still have to reckon with a very considerable probability that, over the next five or ten years, the average rate of inflation will not be kept below 3 percent and in that case interest rates will not be as low as I have said." (Instructional Dynamics

[100] McCallum's regression was for GNP deflator inflation. Data on CPI inflation tell the same story. For example, using the CPI inflation data in the 1975 *Economic Report of the President* (Council of Economic Advisers, 1975, Table C–48, p. 304), a first-order autoregression for the estimation period 1955–1974 delivers a coefficient on lagged inflation of 1.086.
[101] Shiller's dissertation, written at the Massachusetts Institute of Technology, was cited in Friedman and Schwartz (1982, p. 478).
[102] Friedman (1973b, p. 60).

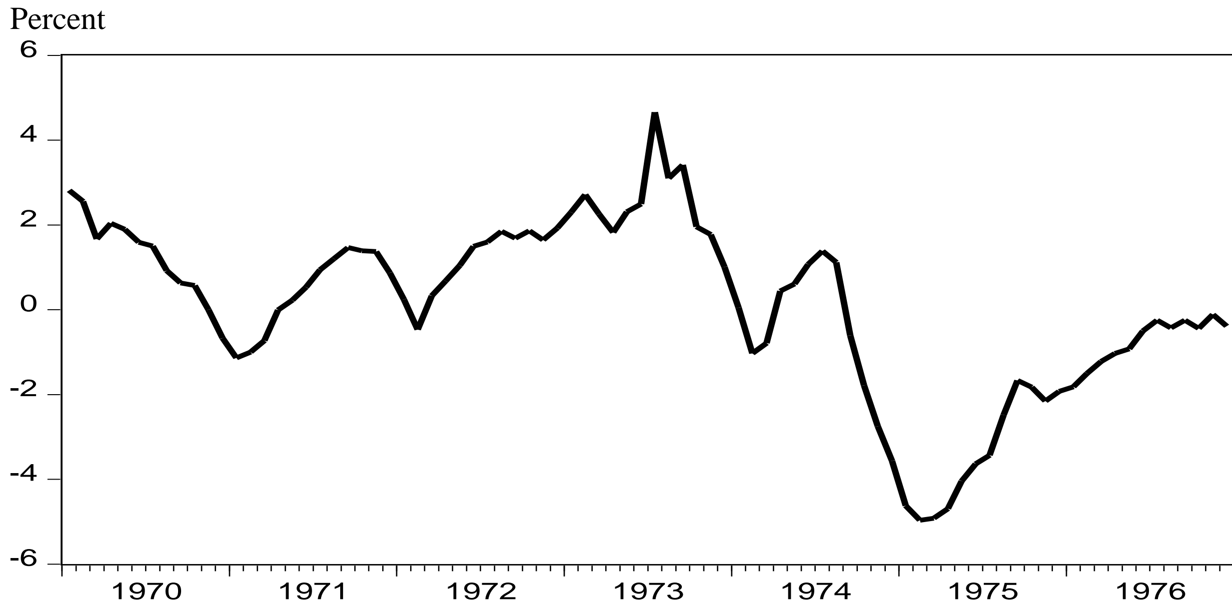

Figure 3. Real federal funds rate, monthly, 1970 to 1976.
Source: Calculated as the monthly average of the federal funds rate minus the twelve-month percentage rise in the CPI. Both series are obtained from the Federal Reserve Bank of St. Louis' FRED portal.

Economics Cassette Tape 56, August 6, 1970.) This second scenario for the 1970s contemplated by Friedman in 1970 proved to be the one that was, in fact, realized.

The years 1973 and 1974 saw both inflation and nominal interest rates reach new peaks, increasing the correlation between the two series. The correlation between nominal interest rates and inflation was so strikingly clear in light of this experience—as well as another stretch of simultaneously falling interest rates and inflation, emerging in 1975—that Friedman and Schwartz published a note in 1976 on the comovement of the commercial bill rate and CPI inflation.[103]

By mid-1973, the run-up of nominal interest rates had sufficiently impressed Friedman that he mused that it had become more difficult to detect the initial liquidity effect (a downward movement in yields) associated with a shift to high monetary growth: interest rates and rates of increase in nominal money were positively correlated in the 1970s (*Los Angeles Times*, July 30, 1973). For long-term rates, this observation was consistent with his observation that investors had likely become more sophisticated regarding economics and so had gotten better at embedding expectations of inflation into interest rates, as well as looking to monetary growth for insight into the future behavior of inflation.

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[103] See Friedman and Schwartz (1976).

With regard to short-term interest rates, however, Friedman's reference to the elusiveness of the liquidity effect was at cross-purposes with his continued acceptance that the Federal Reserve exerted considerable influence on short-term interest rates via that effect. Already in mid-1973 he reaffirmed that efforts to reduce monetary growth would in the first instance tend to raise short-term interest rates (Instructional Dynamics Economics Cassette Tape 125, July 18, 1973). Looking back on the experience of the early 1970s, Friedman would further reconcile the joint presence of the liquidity effect and Fisher effect as factors bearing on short-term interest rates. The authorities had let short-term rates move up with expected inflation, he suggested, but had done so slowly and incompletely, thereby keeping the real interest rate down and even promoting a real-rate decline. Appeals to the Fisher effect aided greatly in understanding interest-rate behavior in the 1970s. But the Fisher effect by no means provided a complete account of the course of rates. The liquidity effect, through which easier monetary policy put downward pressure on nominal interest rates, continued to be part of the short-run behavior of U.S. yields, and the Federal Reserve had exploited its ability to generate liquidity effects as a means of keeping the short-term real interest rate negative for much of the period through 1980. As Friedman put it in 1976: "Adjusted for inflation, interest rates have been abnormally low."[104] This narrative was consistent with Evans' (1984, p. 221) observation that, as "inflation became chronic by 1979 because interest rates had been kept below their natural rates in the 1960s and 1970s, interest rates had to rise for disinflation to occur."[105]

The U.S. authorities' pre-1979 actions likely did not reflect a wish on their part to generate the abnormally low values real interest rates of the kind that were realized in the 1970s. On the contrary, Arthur Burns stated in September 1974: "Interest rates throughout the world and throughout history have followed the rate of inflation, and for a very good reason."[106] Rather, they likely reflected a belief that nonmonetary measures could so reduce U.S. inflation that the nominal interest-rate choices made by the Federal Reserve would be consistent with positive and normal levels of real interest rates. In the event, the ineffectiveness, except for short periods, of

[104] *Newsweek*, August 23, 1976. The attributions made to Friedman in the preceding sentences are based on the material in this column and those in *Newsweek*, March 10, 1975, *Newsweek*, December 29, 1980, and Friedman (1983a, p. 3; 1984a, p. 27).

[105] See also Dornbusch and Fischer (1978, p. 517). The notion that the FOMC was slow in allowing real interest rates to adjust was consistent with Clarida, Galí, and Gertler's (2000) estimates of the reaction function for the federal funds rate. (However, contrary to the Friedman and Evans narratives, their theoretical discussion took for granted that the FOMC during the 1960s and 1970s allowed the real federal funds rate to equal its natural rate, on average.)

[106] From Burns' testimony of September 25, 1974, in Committee on the Budget, U.S. House of Representatives (1974, p. 134). In addition, earlier the same year, the then Secretary of the Treasury, George Shultz stated: "I would rather have lower interest rates, which translates as I wish we had a lower rate of inflation." (Testimony of February 8, 1974, in Joint Economic Committee, 1974d, p. 105.)

nonmonetary devices against inflation meant that they were not reliable instruments for helping deliver normal real interest rates. As Figure 3—which depicts a measure of the short-term real interest rate—shows that the temporary intensification of price controls in 1973 gave rise to an ephemeral period of real interest rates of 4 percent, one rapidly succeeded by negative real interest rates.[107] At the end of the 1970s, a renewed determination to use monetary policy to achieve disinflation, and corresponding disillusionment with nonmonetary devices for this purpose, created conditions for the real interest rate to be made positive on a sustained basis.

In the meantime, and in contrast to the new peaks attained by nominal interest rates in the 1973−1976 period, the real interest rate plunged from 1974 onward, most clearly so for short-term real rates, as shown in Figure 3. Indeed, this is a factor explaining why the Wallich prediction regarding interest rates, quoted earlier, was not as far off as one might expect. For although neither Wallich nor many others contemplated inflation being as high it was from 1972 to 1977, neither could they imagine real interest rates becoming so low. Longer-term interest rates could plausibly be regarded as still implying positive real yields, but only if, as of the mid-1970s, investors expected inflation to recede to well below 6 percent. Indeed, Wallich—having changed his role from commentator to policymaker, thanks to his recent switch from *Newsweek* columnist to Federal Reserve Board Governor—observed in April 1974 (Wallich, 1974a, p. 9) that "investors do not expect inflation over the longer run to persist at rates comparable to those we have experienced in recent months."

In the same vein, Friedman would note in 1976, after inflation had come off its 1973−1975 highs: "The market is acting as if it expected inflation to decline still further over the coming years." (*Newsweek*, August 23, 1976.) Alan Greenspan would later contrast the mid-1970s peak of inflation, when longer-term interest rates failed to incorporate an expectation of substantial secular inflation, with the period 1979−1980, when longer-term interest rates moved into a new range that well and truly incorporated high inflation expectations.[108]

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[107] This is not to imply that the *ex post* real interest rate was an infallible indicator of monetary policy stance over this period. Indeed, as discussed below, during late 1974 negative *ex post* short-term real interest rates proved consistent with a tightening of U.S. monetary policy.

[108] Greenspan observed that, until the late 1970s, "the markets assumed that a noninflationary environment was always just beyond the horizon…. [E]ven during the 1973−74 price explosion, long-term U.S. government bond yields rarely exceeded 8½ percent. This implied a long-term inflation forecast of 6 percent or less. In fact, with double-digit inflation in 1974, the forecast of a 6 percent rate of inflation over a 15−20 year period meant that the market expected an early return to previous inflation levels and a rate of price increase perhaps quite below 6 percent in the outlying years." (From Greenspan's testimony of January 22, 1981, in Joint Economic Committee, 1981, p. 12.)

Monetary policy operating procedures

In 1963, Friedman and Schwartz's *Monetary History* had pointed to the tendency for defects in monetary tools—that is, in the operational or implementational aspects of monetary policy, as distinct from policymakers' macroeconomic strategy and doctrinal framework for understanding the links between monetary actions and the economy—to enhance "mistakes in policy arising from erroneous analysis."[109] The Federal Reserve's behavior in the 1970s would reinforce this observation. Doctrinal errors made the FOMC less well-disposed than it should have been to deploy monetary policy against inflation. But even in periods like 1972–1973 when the Committee actually sought to restrain monetary growth, elements of their operating procedures hindered the achievement of the desired restraint.

As Friedman acknowledged, monetary-growth targets had been instituted by the FOMC beginning in 1970, although they would not become part of its statutory responsibilities until the second half of the decade.[110] In early 1972, furthermore, the FOMC had ostensibly adopted "reserves against private deposits" (RPD) as its instrument, thereby appearing to accept monetarists' recommendation of a move from an interest rate to a reserves aggregate as its operating target. Some months later, the *New York Times* printed a lengthy article about this change, asking: "How 'Friedmanite' Is the Fed?" The article observed: "The Federal Reserve System, the nation's central bank, has adopted this year a significant, though not widely known, change in its operating techniques that appears, at least, to bring it much closer to the prescriptions of Professor Milton Friedman and his 'monetarist' school of economics."

However, reason for skepticism about the significance of this change lay in what the FOMC did not do. It did not move to the total-reserves or aggregate-monetary-base instrument for which monetarists had argued. And it did not rescind the move to lagged reserve requirements, whose introduction in 1968 hindered short-run FOMC control of bank reserves.

What is more, in the 1950s the FOMC had already shown that it was perfectly capable of, in effect, managing short-term interest rates while at the same time framing its short-term decisions in terms of choices of some kind of reserves aggregate. A thoroughgoing move to a reserves aggregate as an instrument, in contrast, would mean eschewing interest-rate management. A major indication that no such move occurred was given in Chairman Burns' speech of May 12,

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[109] Friedman and Schwartz (1963a, p. 531).
[110] See Nelson (2020b, Chapter 15) for an analysis of Friedman's discussions during 1970−1972 of the FOMC's monetary-aggregate targets.

1972, in which he characterized the previous December's Smithsonian agreement on international monetary arrangements as likely to help in "giving monetary authorities somewhat more scope to pursue different interest-rate policies" (Burns, 1972, p. 77). In mid-1973, Burns referred to the "federal funds rate, which we control in large part," and he further noted: "While setting target ranges for RPD at each meeting, the Federal Open Market Committee also establishes ranges of tolerance for the federal funds rate…"[111]

As discussed in the previous volume, as late as October 1972 Friedman was taking the FOMC's adoption of RPD as a very serious change in operating procedures. He would eventually judge, however, that the FOMC made no material change in its procedures between 1952 and 1979.[112] Consistent with this judgment, Friedman's initial (early 1972) skepticism that RPD was such a change resurfaced in 1973. This was evident in the explanation he offered early in 1973 for why monetary conditions had been allowed to become so relaxed in 1972: "the Fed became worried about rising interest rates" (*Los Angeles Times*, February 18, 1973, p. 1).[113] Indeed, as the switch to the RPD regime increasingly revealed itself not to be an authentic and major change in procedures, the acrimony that often characterized the debate on Federal Reserve operating procedures resurfaced—and became more public than ever on Friedman's part.

Friedman concluded (Instructional Dynamics Economics Cassette Tape 127, August 29, 1973) that the Federal Reserve's misses of its monetary-growth targets could not be explained wholly by technical problems in controlling money. As before, he maintained that the Federal Reserve could greatly improve monetary control if it focused on that goal using a *bona fide* commitment to a reserves instrument. But, when it was faced with more stable short-run behavior of interest rates and more stable monetary growth, he believed that the Federal Reserve preferred the former. He had previously found that this was the case with Chairman Martin. Interaction with Arthur Burns convinced him that it continued to be the case under Martin's successor (Instructional Dynamics Economics Cassette Tape 116, March 2, 1973).[114]

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[111] From Burns' testimony in Joint Economic Committee (1973a, pp. 185, 193).
[112] Friedman (1982a, p. 103).
[113] See also Friedman's remark (*Newsweek*, August 6, 1973) that the Federal Reserve Board had wished "to avoid the wrath of Congressman Patman." Although Friedman did not spell it out on this occasion (or in another reference to Patman in Friedman, 1975a, p. 59), Patman (Chairman of the House of Representatives Committee on Banking and Currency Committee from 1963 to 1974) was a longtime critic of the Federal Reserve and in particular of policies that involved raising nominal interest rates (see Meltzer, 2009a, 2009b). Note, however, that, as stressed in Nelson (2020b, Chapter 15), nominal interest rates were allowed to rise somewhat in 1972, and Burns' cost-push views allowed him to see price controls as helping to reduce the need to set nominal interest rates still higher.
[114] Consistent with this characterization of his views, Burns himself would publicly say that "our research indicates that short-run fluctuations in the money supply have little or no significant impact on the economy" (Joint Economic Committee, 1973a, p. 192).

The attempt to hold down interest rates also hindered control of monetary growth even over longer-term horizons, Friedman argued, because it promoted serial correlation in deviations of monetary growth from its target rate.[115] Thus in a retrospective on the inflation observed earlier in the decade, Friedman would later note in 1978 that pressures "to keep interest rates low are a major reason for high monetary growth" (*Newsweek*, April 24, 1978).[116] Friedman emphasized that the authorities' unwillingness to allow nominal interest rates to vary more, and to exceed inflation, was inhibiting not just short-run monetary control but also long-run price stability, and that only a period of disinflation could permanently deliver low nominal interest rates. Hence his recommendation: "The best way to hold rates down in the long run is for the Fed to raise them temporarily." (*Los Angeles Times*, February 18, 1973).

Although, as will be seen, the Federal Reserve did tighten policy during 1973, this was too late to prevent its expansionary period of 1971−1972 from being felt dramatically in inflation in 1973 and 1974. Consequently, Friedman observed at the end of the latter year of the experience in the United States and elsewhere: "In the process of trying to hold down interest rates, they have produced inflation, with the end result that interest rates went up to far higher levels than they would have if the central banks had followed an appropriate monetary policy."[117]

A policy of interest-rate stabilization also produced potential problems in the event of major shocks that put downward pressure on aggregate demand. Although, in the late summer of 1974 (Instructional Dynamics Economics Cassette Tape 152, August 21, 1974), Friedman remarked that he could not see any possibility of the FOMC taking actions that would convert the slowdown into a *serious* recession, he later regarded the Committee as having done precisely that by not letting market interest rates *fall* as much, and as rapidly, as was needed to prevent a sharp further step-down in monetary growth in 1974–1975. This move to an overly-restrictive policy is discussed in the account that follows of the behavior of aggregate output in the 1973−1974 period.

Nominal income, output, and unemployment

The deterioration in economic performance evident in inflation and interest rates in 1973 and 1974 was also felt in the behavior of aggregate economic activity.

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[115] See especially Friedman's *Newsweek* column of December 8, 1975, as well as his more detailed written remarks on the matter in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1975a, pp. 45–46).
[116] See also Friedman (1977a, p. 24).
[117] Friedman (1975b, p. 702 of 1979 reprint).

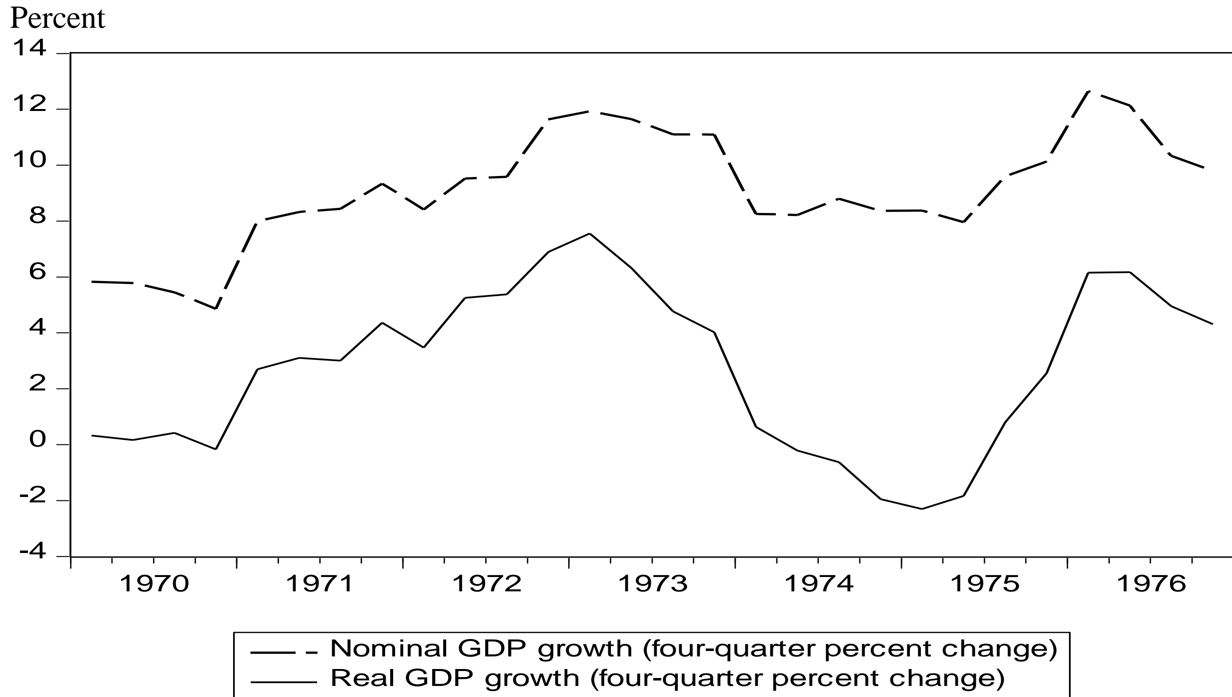

Figure 4. U.S. nominal and real GDP growth, four-quarter growth rates, 1970–1976.
Source: Computed as percentage changes from the Federal Reserve Bank of St. Louis' FRED portal.

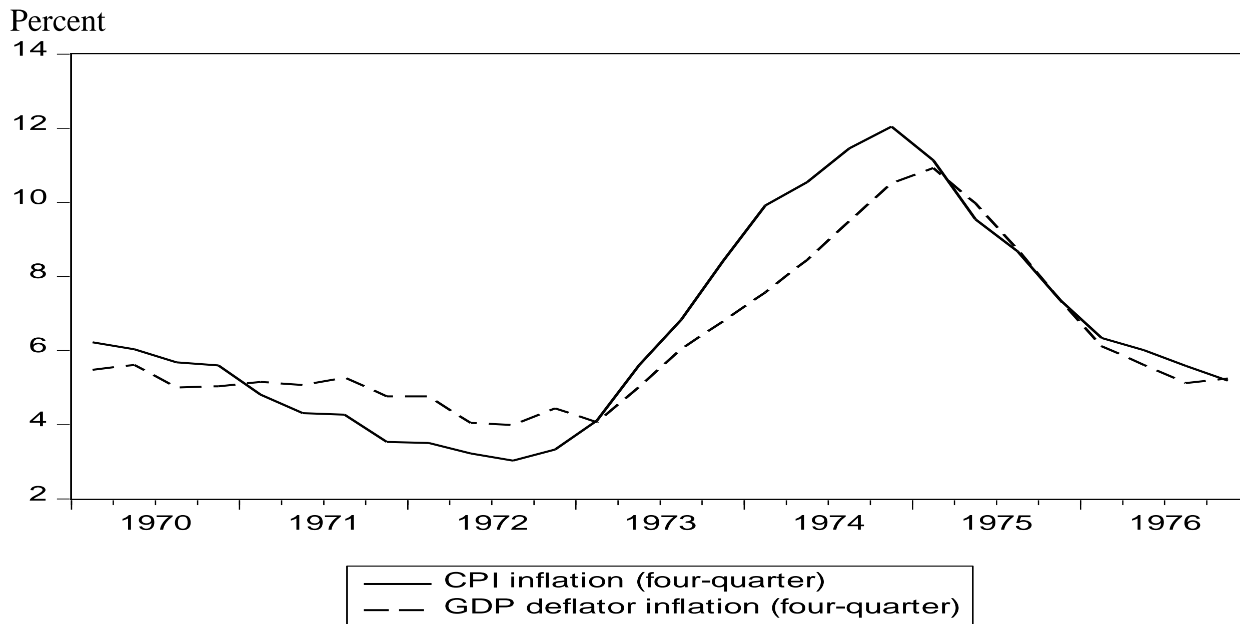

Figure 5. CPI and GDP deflator four-quarter inflation rates, 1970–1976.
Source: CPI inflation is the four-quarter percentage change in the quarterly average of the CPI; GDP deflator inflation is the four-quarter percentage change in the GDP deflator (2012 prices). The level of each series was obtained from the Federal Reserve Bank of St. Louis' FRED portal.

Speaking in late 1971, Friedman had acknowledged that—notwithstanding his complaints about stop-go policies—the post-World War II period had, on the whole, been a great success for the United States in terms of aggregate economic stability (Instructional Dynamics Economics Cassette Tape 89, December 26, 1971). This was a common assessment—expressed, for example, in the upbeat evaluation of Boughton and Wicker (1975, p. 468): "Monetary policy has improved greatly since the end of World War II…" But the degree of economic fluctuations observed from 1973 onward meant that such judgments demanded reappraisal. U.S. output variations continued to be small in relation to those in the interwar period, but they were markedly more intense than those typically observed for much of the period since the early 1950s. Figure 4 shows that, in the period from 1970 to 1976, aggregate demand instability, whether measured by growth in nominal GDP or in real GDP growth, rose sharply—alongside the increased rates of inflation seen in that period (and shown in Figure 5).

Like Friedman, the Federal Reserve saw an excess-demand situation as prevailing during 1973 (see, for example, Meltzer 2009b, pp. 823, 869 on the FOMC's discussions). On inflation, too, although during 1973 Chairman Burns attributed far more of the rise in inflation to special factors than did Friedman, he did grant that demand pressure had played a part.[118] By the time of the Federal Reserve chair's major acknowledgment on this score in late 1973, Burns' FOMC had tightened monetary policy considerably. Indeed, monetary policy tightening showed up in 1973 on a host of criteria: slower growth in the monetary base, M1 and M2; higher short-term nominal interest rates—with the federal funds rate continuing the rise that had started in 1972; and for a time, and as already noted, higher real interest rates.[119]

Even before the tightening showed up in monetary aggregates, the *Wall Street Journal* reported in early 1973 that the extent of economic overheating that had emerged made a monetary policy tightening likely. This, it said, had led many economists to predict "a sharper-than-expected slowdown… late this year or in 1974," although it added that hardly anyone was predicting a recession (*Wall Street Journal*, February 27, 1973). This was a rash assessment, however: not long thereafter another newspaper report contended that *many* economists were predicting a deep recession by year's end (*Kansas City Times* (Missouri), March 7, 1973).

Back in 1971, Friedman himself had warned that an excessive monetary expansion in 1971−1972

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[118] See the Burns (1973b) publicly-released letter, discussed above.
[119] Specifically, the quarterly average of the real interest rate is positive throughout 1973, then becomes negative for one quarter in 1974:Q1, and is continually negative from 1974:Q4 through 1978:Q1.

might well lead to a subsequent monetary contraction and a recession.[120] During the course of 1973, he believed he was seeing this prediction being realized. In August, he stated that the U.S. economy might already be in recession (*Wall Street Journal*, August 3, 1973). In September, more cautiously, he observed that U.S. real output had been slowing down for some months (Instructional Dynamics Economics Cassette Tape 130, September 26, 1973).[121] The step-down in monetary growth that had taken place over the year so far led Friedman in October 1973 to predict a recession, one in which "we might be in the early stages" already (*The Evening Bulletin* (Philadelphia), October 12, 1973; see also *Chicago Tribune*, October 13, 1973).

Importantly, Friedman believed that a recession was necessary. "You can only go on a drinking bout for so long," Friedman observed in August, employing one of the analogies to alcoholism that he increasingly employed in his discussions of the macroeconomy. "Sooner or later there's going to be a hangover." (*Kansas City Star* (Missouri), August 12, 1973.) In November, he called for a mild recession over the next two to three years to eliminate inflation: "It is far better to have a mild recession for the next few years—to take the cure and pay the price—than to go off again to the races." (*Chicago Tribune*, November 21, 1973.)

Into early 1974, Federal Reserve officials articulated a diametrically opposed view on both the likelihood of and the need for recession. Board Governor Holland observed in October 1973 that there were no flashing warning signs of recession (*Kansas City Star* (Missouri), October 18, 1973). As already noted, in his February 1974 statements in reaction to Friedman's public letter, Chairman Burns took issue with Friedman's position that a period of lower output was needed to get inflation down. And still another Board governor made one of the most outspoken public criticisms made by officials of reliance on monetary restriction to eliminate inflation, one directed specifically at Friedman. In what the *Wall Street Journal* called an "unusually blunt counterattack against critics of the Fed's policies," Governor John Sheehan denied that inflation was primarily due to the Federal Reserve and that using monetary policy against inflation would generate 15 percent unemployment and a popular uprising: "Milton could go to his farm [that is, Friedman's Vermont summer home] and sit this out, but when he comes back, he will find the

[120] See the discussion in Nelson (2020b, Chapter 15).
[121] The fact that the United States underwent a pronounced slowdown in 1973 has led some to argue that the recession of 1973–1975 should be dated as having begun early in 1973. McNees (1978) proposed March 1973, and Paul Samuelson (*Newsweek*, February 18, 1974) argued that the United States was in a growth recession ever since the spring of 1973. Dornbusch and Fischer (1978, p. 542) argued that the growth recession began in the second quarter of 1973 and suggested that this was obscured at the time by the fact that initial real GNP estimates for that quarter took higher values than in later revisions, and by the continued placid behavior of the unemployment rate over the course of 1973. Of course, a growth recession is highly distinct from an actual recession, and the definition of a growth recession depends on the assumed growth rate of potential output.

cities burned down and the University of Chicago along with them." (*Wall Street Journal*, March 29, 1974.)[122]

In the event, a substantial rise in unemployment—to 9 percent in May 1975, from 4.6 percent in October 1973—was not avoided. Indeed, as that increase in unemployment attests, the mid-1970s recession turned out to be severe. It also featured a 2.75 percent decline in real GDP on modern data.[123] Friedman regarded contraction and unemployment of these magnitudes as unnecessarily severe and avoidable by alternative, smoother monetary policies. In particular, as will be discussed below, Friedman laid much of the blame for the severity on the recession on the Federal Reserve's conduct of monetary policy during 1974, which did not turn out to be gradualist.

By the time of Friedman's November recession prediction, the Middle East war of 1973 had occurred, and OPEC had launched a series of oil supply cutbacks and price increases that became the first oil shock. The first proposed price increase, announced on the eve of the war, was 66 percent (*The Guardian*, October 6, 1973), although in the event this proved only a fraction of the overall oil price hike induced by OPEC in 1973−1974. As the scale of OPEC's actions became apparent, other economists joined in forecasting or declaring recession, and a brief item on *NBC Evening News* in mid-December 1973 reported that both Friedman and Paul Samuelson were foreseeing a U.S. recession of some degree in 1974 (NBC, December 13, 1973).

The National Bureau of Economic Research later dated the 1973−1975 recession as having begun in November 1973.[124] This dating underscores the fact that the recession predictions that Friedman made in late 1973 amounted to another case (paralleling his 1971 prediction of a 1973 breakout in inflation) in which he should have adhered unwaveringly to a forecast. But—just as he had with his 1971 warnings of severe inflation in 1973—Friedman *did* waver. After a decline in the fourth quarter of 1973, real GNP grew in the first quarter of 1974, and in August 1974 Friedman said he did not see the Federal Reserve converting the slowdown into a serious recession; he added that there might well be a "minor recession" ahead, but he did not see the U.S. economy as currently being in recession (Instructional Dynamics Economics Cassette Tape 152, August 21, 1974). When a sharp economic downturn did emerge, Friedman took flak for

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[122] Sheehan's statement was subsequently quoted in *Wall Street Journal* (April 1, 1974), Schiff (1977, p. 9), and Bordo and Istrefi (2018, p. 13).
[123] However, with Sheehan's declaration likely in his mind, Friedman would note that this rise in unemployment had not led to mass riots (*Newsweek*, August 4, 1975). Friedman's analysis of the unemployment rate in the 1970s is discussed in Chapters 4 and 6 below.
[124] See "U.S. Business Cycle Expansions and Contractions" at ***https://www.nber.org/cycles.html***.

having been too sanguine about the economy. For example, *Washington Post* economics columnist Hobart Rowen would (in *Washington Post*, October 15, 1976) point to Friedman's remark at the White House's general summit (of September 28, 1974) on inflation, "We are not, and I emphasize not, in danger of a major depression or even a severe recession," as an example of the fallibility of Friedman's analysis. Likewise, Gordon (1976b, p. 55) quoted Friedman's *Newsweek* column of September 23, 1974, which stated that "recent rates of monetary growth are not too low," as an indication that Friedman had little inkling of the major economic downturn that was in process.

The fact of a recession became widely known and accepted around October 1974, and its severe character became clear later in the year.[125] There is no doubt that, as Rowen and Gordon implied, the severity of the U.S. output decline in late 1974 caught Friedman off-guard. But it would be a mistake to suggest that this revealed a basic flaw in his monetary analysis of the time. Rather, two factors seem to have been important.

First, like many others, Friedman simply underestimated the extent to which inflation would take off in 1974. At a Business Council conference in October 1973, Friedman stated: "The consumer price index is much more likely to hit a 6 percent or 7 percent increase than to fall from this year's 5.7 percent to the standard expectation of 5 percent in 1974." (*American Banker*, October 15, 1973.) The consumer price index would, in the event, rise by over 10 percent in 1974.

Friedman acknowledged that he had, like others, underpredicted the year's inflation.[126] In his case, this underprediction meant that his forecast of high nominal income growth (made on the basis of the behavior of high monetary growth) did not map into positive real output growth. As noted above, the nominal money/nominal income relationship exhibited very little disruption over the years from 1973 to 1976 as a whole, at least if an M2-type definition is used. However, *within* the aggregate growth of nominal income, the amount of increase that took the form of inflation in 1974, as distinct from output growth, exceeded what Friedman had anticipated.[127]

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[125] Romer and Romer (1994a, p. 27) noted that the Federal Reserve's first major public acknowledgment of a protracted decline in economic activity was in the official record of the October 14 FOMC meeting. Also in officialdom, Secretary of the Treasury William Simon in a public statement in October 1974 described the U.S. economy as "technically" in recession (*Daily News* (New York), October 25, 1974). The seriousness of the downturn had become clearer by late November 1974 (see, for example, *Detroit Free Press*, November 27, 1974, and *Daily News* (New York), November 28, 1974).

[126] See his acknowledgments of this in Instructional Dynamics Economics Cassette Tape 159 (late November/early December 1974) and Friedman (1975c, p. 178), for example.

[127] In *Chicago Tribune*, November 21, 1973, he had seen a 7-percent-plus inflation rate as being likely in 1974.

Table 1. Behavior of U.S. monetary growth, 1971 to 1975

Period	Average quarterly annualized rate of growth			
	Old M1	Old M2	Modern M1	Modern M2
1971:Q1 through 1973:Q2	7.5	10.9	7.3	12.1
1973:Q3 through 1974:Q1	5.7	9.2	5.5	6.2
1974:Q2 through 1975:Q1	3.7	6.6	3.7	5.8
1975:Q2 through 1975:Q3	7.2	10.4	7.0	15.4

Source: In the case of modern M1 and M2, growth rates are derived from quarterly averages of the monthly data in the Federal Reserve Bank of St. Louis' FRED portal. The tabulations given in Lothian, Cassese, and Nowak (1983) are used to obtain quarterly levels data on the old (pre-1980) U.S. M1 and M2 series.

This underestimate largely reflected the combination of the end of wage-price controls and the advent of the food and oil price shocks. As discussed above, as well as in the next chapter, these were events thar Friedman granted had a sizable short-term positive effect on measured inflation. It deserves emphasis that he saw their effects as mainly mattering for the time pattern of inflation, rather than for its average rate over a period of years. Indeed, in *Newsweek* (April 24, 1978), Friedman pointed to the striking agreement in the United States during the 1970s between averages of the inflation rate and those of prior monetary growth, so he did not regard overall inflation performance as out of line with what had happened to the monetary aggregates (and particularly M2).

A second important factor underlying Friedman's failure to foresee the severity of the approaching recession was the considerable further tightening of monetary policy that occurred during 1974. This tightening rendered his earlier comments on the economic outlook obsolete. Although, as already noted, the short-term real interest rate was very low in 1974–1975, Friedman maintained that monetary policy was tight on average over that period, particularly in mid-1974 through early 1975. This interpretation requires there to have been considerable fluctuations in the natural real rate of interest that involved the natural rate taking unusually low values in mid-decade. But, as will be seen in the discussion that follows of monetary developments, it is an interpretation that is backed up by the weakness of monetary growth.[128]

In June 1974, Friedman was quoted saying he so far saw no evidence of tight money (*Business*

[128] Some evidence that the fluctuations in the natural rate intensified during the 1970s and that the natural rate troughed in 1975 is provided in the estimate of Laubach and Williams (2003, Figure 1, p. 1066). In addition, at the time, the *Journal of Commerce* (March 31, 1975) noted: "Today, everybody and his brother want their funds employed at short term. There is little interest in having funds tied up for seven to ten years." Such a pattern of behavior would tend to reduce the short-term natural interest rate.

Week, June 15, 1974, p. 80) and this assessment was followed by his September 1974 remarks, quoted above, in which he suggested that monetary policy was not tight and there was no danger of a severe recession. But these commentaries were largely based on behavior of the money stock during the first half of the year. The course of the downturn in monetary growth for different parts of 1974 is shown in Table 1. The dates chosen for periods of monetary growth in the table correspond to quarterly counterparts of the monthly periods used by Friedman in his *Wall Street Journal* op-ed of August 21, 1975. As the table indicates, basically the same story emerges regardless of whether one uses the old definitions of M1 and M2 (which Friedman used in his op-ed) or the modern definitions that expanded the range of deposits included in M1 and, to a far greater extent, in M2.

The table shows that monetary growth, after falling in 1973 and early 1974, declined still further over much of 1974. Friedman's analysis of monetary growth behavior led him to identify a "sharp further tightening in mid-1974" (*Wall Street Journal*, August 21, 1975), an analysis basically consistent with chronologies arising from accounts of Federal Reserve deliberations—with Romer and Romer (1989, p. 141) dating a notable monetary tightening to April 1974.[129] Even when speaking in early December 1974, Friedman called the recession "mild" (quoted in *Pittsburgh Post Gazette*, December 6, 1974). What was requited, he reaffirmed at this time, was to "stick with the policy of monetary restraint for a two- or three-year period." If that happened, benefits would eventually flow on both the price and output fronts: "We can kill inflation, get to a zero rate of inflation, and [then] have a healthy noninflationary expansion."[130]

Friedman had, however, already raised concern that, since June 1974, monetary growth had fallen too much (Instructional Dynamics Economics Cassette Tape 156, October 23, 1974). And, as the response of the economy started to emerge in earnest at the end of 1974, Friedman's analysis of the mid-1970s downturn took a consistent complexion (Instructional Dynamics Economics Cassette Tape 160, December 19, 1974). In his narrative, the Federal Reserve had made a material monetary tightening in 1973 that was in the appropriate direction but should have been enacted much earlier.[131] But, taking the leveling-off of interest rates as itself a sign that tightening had stopped, the central bank had continued and overdone the actual policy

[129] Like Friedman, Romer and Romer saw this tightening as coming on top of an earlier tightening in 1973 (see Romer and Romer, 1993, pp. 79–80). Bernanke, Gertler, and Watson (1997, p. 121) also stressed that U.S. monetary policy began tightening ahead of the 1974 downturn and before the oil-price surge of 1973–1974. They characterized it as a response by the Federal Reserve to the non-oil commodity price rise. However, it may be more appropriate to view the tightening as, instead, having resulted primarily from the FOMC's recognition in 1973 of an excess-demand situation, as noted above.
[130] Friedman (1975b, p. 707).
[131] Again, see in particular his commentary in *Wall Street Journal*, August 21, 1975.

tightening. Specifically, according to this account, the FOMC had permitted a further decline in monetary growth because it did not lower the federal funds rate target rapidly enough during the course of 1974. It had thereby "deepened the recession" (*Newsweek*, March 10, 1975). A minor recession had turned into one that was large by postwar U.S. standards.[132] And, as Friedman would indicate in his retrospectives on the period, the scale of this recession prompted an overreaction by the FOMC in favor of expansionary moves that ultimately reversed the decline in inflation that occurred in 1975−1976.

Friedman's suggestion that monetary policy tightened in late 1974 seems valid.[133] It does not appear appropriate to regard monetary policy as actually easing in the second half of 1974 after the federal funds rate peak. Commentary at the time suggested otherwise: for example, a September 1974 press report stated that "the Federal Reserve shift actually began earlier this summer[,] with the Fed cautiously encouraging a gentle decline in the sensitive federal funds interest rate" (*Kansas City Star* (Missouri), September 13, 1974), and an October 1974 report suggested that since mid-August, "in a series of moves, the central bank has signaled its somewhat easier stance on credit by steadily lowering the target range for fed funds" (*Kansas City Star* (Missouri), October 21, 1974). But the behavior of monetary aggregates records a further tightening of the screw during the later part of 1974.

And the course of economic activity in late 1974 is consistent with a reaction to the monetary tightening. In particular, the fall in monetary growth was followed by a severe contraction in real output in the second half of 1974. On modern real GDP data, the output contraction in 1974:Q3 (of −3.7 percent at an annualized rate) is worse than that of 1974:Q4 (of −1.5 percent at an annualized percentage rate).[134] The data at the time, however, concentrated the output contraction in the fourth quarter. The headline of the *Washington Star-News* (Washington, D.C.) (January 16, 1975) in the wake of the release the Commerce Department's initial data for 1974:Q4 came out summed up the dismal situation: "GNP Drops 9.1 Percent: Slump Deepens, Inflation Soars."

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[132] Among Friedman's many statements of this point, see Friedman (1975d), his written and in-person testimony in November 1975 in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1975a), his *Newsweek* columns of, August 25, 1975, October 3, 1977, and February 19, 1979, and Instructional Dynamics Economics Cassette Tapes 163 (February 1975, Part 1) and 183 (January 1976, Part 1).

[133] There is a parallel between monetary policy being tight in late 1974—notwithstanding real short-term interest rates being low—and Bernanke's (1999, pp. 11−12) argument that the Bank of Japan's monetary policy was tight even though real interest rates were low, as well as that monetary policy was exerting a restrictive influence on nominal and real aggregate demand via effects operating on private-sector balance sheets.

[134] Neither decline is as severe as the subsequent move, in 1975:Q1, of real GDP of −4.8 percent at an annualized rate. It was that quarter's decline that, on the basis of the vintage of real GNP data prevailing in the early 1980s, prompted Hamilton (1983, p. 229) to refer to "the dramatic output drop of 1975:[Q]1."

This output contraction was associated with a decline in consumer spending that was hard to explain using what—despite some modifications under the influence of Friedman's and others' research contributions—were still largely old-style Keynesian consumption functions. Mishkin (1977, p. 126), for example, characterized the period as witnessing a collapse in consumption, and Blinder (1979, p. 57) referred to "the astounding $13.7 billion decrease in consumer spending in 1974:[Q]4." The shift to much lower rates of money creation during 1974 likely offers insight into the contraction in U.S. consumption observed late in the 1973–1975 recession. The squeeze on real money balances engendered by the monetary contraction may have put downward pressure on asset prices, as well as lowered medium-term expected future income. The combination of lower asset prices and lower expectations of income may, in turn, have played an important part in promoting the decline in U.S. households' spending.[135]

The fact that monetary growth continued to contract in late 1974 when nominal and real interest rates were declining raises the question of whether the Federal Reserve could, using open market purchases, actually have prevented the decline in money growth. The answer to this would seem to be that they, indeed, could have done so. This affirmative answer supports Friedman's characterization that monetary policy in this period featured avoidable protracted periods of too-low monetary growth as well as other periods of too-high growth in money. Short-term nominal interest rates, while off their peaks, remained well above—indeed, decidedly above—their lower bound of zero, so the option remained available to the authorities to stimulate monetary growth simply by engaging in larger conventional open market purchases. Indeed, policymakers at the time conceded that they were capable of raising monetary growth. They rejected this policy option because of the further decline in nominal interest rates it might generate, not on grounds of feasibility (Wallich, 1975a, pp. 5–7).[136]

Nor is there good reason to doubt that open market purchases would have generated a prompt

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[135] Abel and Bernanke (1992, pp. 172–173) pointed out that consumption fell more in the 1973–1975 recession than in the 1981–1982 recession, and they appealed to the permanent-income effects of the first oil shock as the reason for the difference. This explanation for consumption behavior in the mid-1970s is complementary to the appeal to monetary factors given here. The permanent income hypothesis (either in Friedman's form or in modern versions) implies that permanent real events matter for permanent income and consumption. But it also implies that monetary policy actions can, by affecting real interest rates relevant for spending, affect consumption for given expected future consumption or income, and it is also consistent with monetary policy having effects on the trajectory, though not the long-run level, of real permanent income.

[136] Wallich took this position even though he had joined the Federal Reserve Board with an inclination to focus on money, stating; "It's a mistake to watch the short jiggles in the interest-rate trend and not observe more closely the money supply." (*Kansas City Star* (Missouri), September 20, 1974.) (Wallich had made this statement before the further step-down in monetary growth shown in Table 1 had become apparent.) In the late 1970s and in 1980, Wallich—although generally supportive of targeting monetary growth—would become more emphatic about the superiority of interest rates to monetary aggregates as indicators.

uptick in monetary growth. Although Federal Reserve officials sometimes wrote as though the money stock did not respond to open market operations until some quarters after short-term interest rates responded, there are grounds for believing that, during the 1970s, a substantial same-quarter response of monetary growth to open market operations.[137] Various studies of money supply determination in that period, cited in Nelson (2020b, Chapter 14), pointed to that conclusion. So did Christiano, Eichenbaum, and Evans' (2005, p. 6) Figure 1, which showed that the response of M2 growth to Federal Reserve policy shocks followed a pattern that, while building over time, included an immediate—that is, same-quarter—reaction. Thus, the decline in monetary growth in late 1974 appears both undesirable and eminently avoidable, and Dornbusch and Fischer's (1978, p. 543) observation seems appropriate: "there is little justification for the reduction of monetary growth in mid-1974."

Monetary economics at the University of Chicago and conferences

The monetary economics scene at the University of Chicago experienced numerous comings and goings during 1973 and 1974. Among these was Robert Gordon's move to Northwestern University at the start of the 1973/1974 academic year. Lingering manifestations of Gordon's former affiliation with the University of Chicago were his continued attendance of the University of Chicago's money workshop during his first year in his new position, as well as in the 1974 publication, by the University of Chicago Press, of the Gordon-edited *Milton Friedman's Monetary Framework: A Debate With His Critics*—a record of the 1970–1972 contributions by Friedman and his critics, alongside some new material by the contributors (though none of the new material was by Friedman).[138] As discussed in the previous volume, Friedman's opening contribution to this book—espousing a theoretical framework—was widely criticized. But the ongoing interest in his macroeconomic views, together with the false hope that Gordon (1974) contained a definitive statement of them, meant that *Milton Friedman's Monetary Framework* stayed in print over Friedman's lifetime and beyond.[139]

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[137] See especially Davis (1973, pp. 180–181). Friedman did not explicitly take issue with the Davis lag estimates; instead, he implied that they might well be substantially accurate when the federal funds rate was the FOMC's operating instrument (see, for example, Instructional Dynamics Economics Cassette Tape 127, August 29, 1973). In contrast, Friedman said that if an instrument for reserves or for the monetary base were employed to target money, it was "literally inconceivable" that over a seven-month period deviations of money growth from target would be large (*Newsweek*, March 10, 1975). It is likely, however, that monetary growth actually responded to contemporaneous FOMC actions even in periods when the FOMC used the federal funds rate as its instrument.

[138] Some of Friedman's newly-written research published during 1974 is considered in the next two chapters.

[139] The book was initially released in hardback, with a paperback version materializing later and being the version that was kept in print. Until soon before publication, the planned title of the book had been *A Symposium on Friedman's Theoretical Framework* (see University of Chicago Press, 1974, p. 672).

Stanley Fischer also left the University of Chicago in mid-1973—in his case, for the Massachusetts Institute of Technology. Before his move, he was witness to what proved to be a momentous decision on appointments to the Department of Economics. "I was at the faculty meeting at which Bob Lucas was brought to Chicago as a visitor, with a clear intent of looking at him as one of the most important people of the next generation," Fischer recalled (interview, August 30, 2013). Although Lucas would give his affiliation as being with the University of Chicago in print only in the second half of 1975, after his appointment had become permanent (Lucas, 1975, p. 890), he was working and teaching at the university in the 1974/1975 academic year. Seven years later, Lucas would be described, somewhat belatedly, by *Business Week* (September 14, 1981) as an "economist at the University of Chicago and heir apparent to Nobelist Milton Friedman," and a motivation for the 1973 invitation to Lucas was that Friedman's move away from producing research in monetary economics was creating a vacuum: Fischer, while noting of Friedman that "he had done a *lifetime's* work by that stage," said of his departmental colleagues that "they wouldn't have been thinking of the next generation if they had thought that Milton was operating at full bore."

Friedman's departure from the University of Chicago was also a looming prospect, notwithstanding his recovery from his 1972 operation. In a sign of what was to come, the Friedmans spent the lead-up to Christmas 1974 in San Francisco (Instructional Dynamics Economics Cassette Tape 160, December 19, 1974).[140]

The late-1974 period did see Friedman discussing monetary topics at a succession of research conferences. One of these was the Conference on Monetarism at Brown University, Rhode Island, on November 2, 1974.[141] Attendees included Anna Schwartz and Friedman's former departmental colleagues Fischer, Gordon, and Carl Christ, and Friedman's former dissertation students Phillip Cagan, Michael Darby and John Scadding. In addition, Karl Brunner and Allan Meltzer were on hand to expound their version of monetarism. Several prominent Keynesian opponents of monetarism were also present: Franco Modigliani, Albert Ando, Lawrence Klein, Robert Solow, and James Tobin.

Friedman's principal function at the conference was to serve as one of the discussants of Tobin's paper (coauthored with Willem Buiter).[142] About three pages of Friedman's 7½-page comment

[140] This was ahead of the American Economic Association meetings a week later in the same city.
[141] See Brunner (1976a, p. 27) for the date of the conference.
[142] See Tobin and Buiter (1976) and Friedman (1976a). Much of Friedman's (1976a) discussion was analyzed in Nelson (2020a, Chapter 5).

amounted to an amplification of the defense he had given in his 1972 *Journal of Political Economy* of comments he had made in a *Newsweek* column (January 23, 1967). As conference attendee Benjamin Friedman—himself soon to emerge as one of the leading critics of monetarism—recalled, Milton Friedman's focus on this column's material flowed from exchanges in the conference's morning session. The conference, despite its sweeping title, focused almost entirely on the "monetary policy versus fiscal policy" debate that had occupied so much journal space in the 1960s. It was, therefore, geared toward a discussion of aggregate demand determination, rather than the broader Keynesian-monetarist debate.

The morning's floor sessions had seen Tobin characterize the Milton Friedman view of fiscal policy. "Milton kept saying, 'No, no, no, that's not what I said, I didn't ever say that,'" Benjamin Friedman recalled. "And Tobin got very upset, and we all went off for lunch, and then when we came back from lunch, Tobin had gone off and Xeroxed one of Milton's *Newsweek* columns in which he had said that. And then there was a debate, part of which took the form of Milton saying, 'Well, you can't criticize me in an academic setting for something I said in *Newsweek*!' You know, that sort of thing. So people thought of Milton as a very, as I say, elusive, deliberately elusive, debater." (Benjamin Friedman, interview, May 30, 2013.)[143] Milton Friedman did, however, defend what he had said in the *Newsweek* column (which had argued that, for given monetary policy, a tax increase would only have a temporary and minor effect on inflation) and, as he had in his 1972 *JPE* reply to Tobin, elaborated on the modeling assumptions that would roughly justify the results predicted in the *Newsweek* column.

Friedman's defense of his *Newsweek* column at the Brown University conference reflected his conviction that, while his interventions in public-policy debates were made for a less technically-oriented audience than were his research contributions, both sets of contributions were based on the same economic framework. The public-policy writings lacked the methodical analysis expected of a research paper—"I would not defend in detail statements I might have made in more or less popular discussions," Friedman would observe to the present author in 1991.[144] But they typically included features that made the popular analyses line up with those Friedman made in his research papers. As Robert Gordon pointed out in the proceedings volume for the Brown University conference, Friedman's "*Newsweek* pieces on fiscal policy contain almost exactly the same description [of the limited effect on nominal income on unmonetized fiscal

[143] In the published version of Tobin and Buiter (1976, p. 274), the *Newsweek* column was quoted, albeit indirectly, by citing Friedman's (1972a, p. 274) quotation from the column.
[144] Letter from Milton Friedman to the author, July 16, 1991. See also his remarks in Hammond (1992, p. 97).

actions]… as the journal articles" that Friedman published in the 1960s and 1970s.[145] Benjamin Friedman, too, would note that Milton Friedman was among those academic economists who, when writing an article for the general public, made sure "it's cleverly worded so that a professional peer who disagrees sees that there's something there that's actually an 'out.'" (Benjamin Friedman, interview, May 30, 2013.)

In his comment on Tobin and Buiter, Milton Friedman argued that the qualifications were a key part of the analysis: "such 'hedges' are highly relevant to both the theory that we find useful and the policy measures that we recommend."[146] In particular, the proposition that unmonetized (or "pure") fiscal actions little affected aggregate nominal income might be a useful approximation even if it did not hold exactly in the data.

The following month, Friedman attended the annual meetings of the American Economic Association, held in San Francisco on December 27–30, 1974. Friedman was a discussant at a conference session whose ostensible focus was the twenty-fifth anniversary of the "rediscovery of money" in the late 1940s and early 1950s. However, Friedman, like the other participants, focused on much more recent developments. Consumer-price data released in March 1974 showed that the twelve-month CPI inflation rate had reached 10 percent in February—the first occasion since 1948 on which the United States had had a double-digit rate (*Star-News* (Pasadena, California), March 22, 1974). The twelve-month rate had then stayed in double digits over 1974, with the end-of-year readings at a little over 12 percent. Matters had come a long way a long way since November 1972, when the then Secretary of the Treasury, George Shultz, had indicated that he was still regarded it as "quite possible" that inflation in the year to December 1972 would come inside President Nixon's 2 to 3 percent objective for the year (*Morning Advocate* (Baton Rouge, Louisiana), November 6, 1972).

"Double-digit inflation and double-digit interest rates, not the elegance of theoretical reasoning or the overwhelming persuasiveness of serried masses of statistics massaged through modern computers, explain the rediscovery of money," Friedman declared in the American Economic Association meeting session.[147]

Friedman's contribution to the session affirmed his pride in being one of the early-1950s

[145] Gordon (1976b, p. 55). See also Nelson (2004b) for a detailed comparison between Friedman's *Newsweek* columns and his research writings.
[146] Friedman (1976a, p. 312).
[147] Friedman (1975c, p. 176).

advocates of money's importance. But it was only much more recently, as he saw it, that the economics profession had been reaching an adequate qualitative appreciation of the effects of monetary policy, particularly regarding inflation. Friedman saw this as the profession coming round, in large part, to his own view. A few weeks after Friedman's San Francisco remarks, columnist William Safire wrote (*New York Times*, January 20, 1975): "Long ago, when even conservative economist Milton Friedman asserted, 'we are all Keynesians now,' the general theory put forward by Lord Keynes was, by and large, universally accepted." But Friedman did not see the previous quarter-century that way at all. On the contrary, as he had written in 1968—when criticizing misinterpretations of the quotation from him that Safire would use—that there had been a long-running "fight in economics by some of us against entrenched Keynesianism."[148]

Friedman took advantage of his remarks at the 1974 American Economic Association meetings to note one matter on which he had gained ground in the profession: attitudes to his monetary view of inflation. "The rediscovery reflects primarily the impact of experience. How can any teacher of elementary economics today stand in front of a class and begin his exposition of what is inelegantly called 'macroeconomics' with the statement: 'throughout this course we shall treat the price level as fixed'?"[149]

II. ISSUES RELATED TO MONETARY POLICY AND PUBLIC POLICY DEBATES, 1973–1974

INDEXATION

In 1973 and 1974, in personal appearances as well as in his *Newsweek* columns and other public writings—most notably an article in *Fortune* (July 1974)—Friedman made a prominent case for widespread indexation to inflation of contracts in the U.S. economy. This activity on Friedman's part and the debate it sparked are recounted and analyzed in detail in Nelson (2018). The discussion of that debate in what follows is, consequently, limited to a summary and expansion of the main points.

With regard to the public sector's practices, Friedman called for indexation to be made routine in the area of taxes. He wanted tax schedules to be indexed. Such a move would have the effect of

[148] Friedman (1968b, p. 5).
[149] Friedman (1975c, p. 176).

forestalling increases in average tax rates that would otherwise occur when taxpayers' nominal personal and corporate incomes merely kept in step with the price level. In *Newsweek* (May 18, 1974), Friedman pointed to various countries, most recently Canada, that had introduced tax indexation. Other countries would follow later in the 1970s, including Australia. However, tax indexation did not prove an altogether enduring reform—it would eventually be abolished in both Australia and Canada, for example—partly because governments, when returning inflation-induced revenue, preferred to do so via reductions in tax rates rather than by adjustments to income-tax rate thresholds. Indexation would, however, be introduced permanently into the United States federal income tax schedule in the 1980s.

Friedman also advocated the issuance of indexed long-term government bonds. His support for this measure went back to the 1940s. It was a reform that the United States would make in the 1990s although—as in other instances abroad—indexed bonds were issued in parallel with, rather than as a full replacement for, marketable nominal long-term Treasury securities.

The most controversial aspect of Friedman's indexation proposals pertained to the private sector, for which he recommended widespread wage indexation. Five aspects of this proposal are worth noting.

First, Friedman did not wish indexation of nominal wages to the price level to preclude adjustments of the real wage to real shocks. Rather, he saw indexation as aiding the process in which real wages and relative prices adjusted appropriately to developments in the real economy. Indeed, he supervised in 1974−1976 dissertation research by his student, Jo Anna Gray, whose work underlined the need for real wages to adjust to real shocks (Gray, 1976a, 1976b). Nevertheless, Gray and others felt that Friedman's public expositions of indexation gave insufficient emphasis to the ongoing need for appropriate readjustment of the relative-price structure in response to real shocks, including the then-new OPEC oil shock of 1973−1974.

Second, many misinterpreted Friedman's advocacy of indexation as amounting to giving up hope that price stability would be achieved. Friedman, in contrast, viewed wage indexation both as a means by which the private sector could protect itself against inflation when it was present and as creating conditions that made restoration of price stability more likely. He basis for believing that disinflationary policies would be made more possible stemmed from the fact that he tended to emphasize—indeed, overemphasize—incentives that governments had to choose to inflate (incentives that stemmed partly from non-indexation of incomes in the short run) as well as the

fact that Friedman thought, as discussed presently, that indexation would limit the short-term damage to output and employment of a disinflationary policy.

In a move that was likely intended to highlight Friedman's view that indexation would promote disinflation, a rewrite of his *Fortune* article for a U.K. readership was subtitled *A Proposal for Escalator Clauses To Reduce the Costs of Ending Inflation*.[150] Nevertheless, the perception that Friedman's advocacy of indexation was a sign of acquiescence to permanent inflation died hard, with *Euromoney* magazine (April 1975, p. 21) asserting: "Even such an intrepid inflation fighter as Milton Friedman has practically thrown in the towel and advocated linking just about everything under the sun to the cost-of-living index, citing Brazil's positive experience with 'indexation.'"

Third, much initial reaction to Friedman's proposals took him as advocating compulsory indexation of wages. But Friedman in fact insisted that such indexation be voluntary. This aspect of his recommendation was explicit in his *Fortune* piece, but he underlined it in a September 1974 talk in London, in which he declared: "What we ought to do is to encourage private indexing but not require it."[151] Likewise, in 1978 he stated of indexation clauses: "Such contractual arrangements should be on a strictly voluntary basis for private transactions but should be legislated for governmental transactions." (*Newsweek*, May 29, 1978.) The misperception that he favored compulsory wage indexation likely stemmed from Friedman's use of the example of Brazil. In Brazil, which Friedman had visited in late 1973, widespread indexation of private-sector contracts—an arrangement known as "monetary correction"—was indeed compulsory.[152] During 1974, Friedman clarified that his advocacy of indexation did not hinge on the Brazilian case and that his belief in indexation predated his exposure to Brazil's experience (Instructional Dynamics Economics Cassette Tape 149, June 26, 1974). Working at cross-purposes with this clarification, however, the publishers of the U.K. version of Friedman's indexation article issued it as a pamphlet with the main title *Monetary Correction*.[153]

Fourth, Friedman saw indexation as a means of *helping* (in a manner described presently) to fight inflation but not, of course, as the means of *actually fighting* (and eliminating) inflation. He continued to insist that the means of fighting inflation was monetary restriction. As he put it: "Indexation is a bandage, not an antibiotic." (*The Herald* (Melbourne), April 10, 1975.)

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[150] See Friedman (1974b).
[151] Friedman (1974c, p. 77).
[152] See Boianovsky (2020) for a detailed analysis of Brazil's experience and Friedman's commentary on it.
[153] Friedman (1974b); see also the subsequent U.S. reprint, Friedman (1974c).

Fifth, Friedman hoped that indexation would align the actual-inflation and expected-inflation terms in the expectational Phillips curve. Therefore, starting from high inflation, the interim costs in terms of lost output and employment of a tighter monetary policy, designed to restore price stability, would be reduced. As Friedman put it: "It is the delay in the adjustment of anticipations that prevents inflation from being halted without a temporary period of unemployment."[154] The research literature in the 1970s and 1980s offered only mixed support for this contention. On the one hand, results showed that a credible disinflation—one in which nominal contracts rapidly recognized the new, lower long-run inflation rate implied by a firmer monetary policy—would have little real cost. Such findings lent weight to the introduction of arrangements that allowed speedier adjustment of contracts to perceived developments in expected inflation.[155] On the other hand, it was also pointed out that contracts that indexed wages to recent rates of inflation—which seemed to be how widespread indexation might be implemented in practice—would stretch out the reaction of inflation and real variables to monetary policy developments and to various real shocks, rather than producing a quick closure of the output gap in response to such developments. Friedman's post-1974 writings indicated that he was taking heed of such results; in a 1977 paper, for example, he conceded that indexation need not be a reliable means of promptly aligning expected and actual inflation.[156]

Giannoni and Woodford's (2005) analysis of the effects of indexation in a modern New Keynesian model underlined the mixed implications of indexation. They found that indexation, when added to an environment of sticky prices, does reduce the distortion to relative prices arising from inflation; in so doing, indexation improves welfare. However, indexation means more persistence in the output gap and inflation. It therefore implies more persistent deviations than otherwise of aggregate output from its no-nominal-rigidities baseline.

WATERGATE

A passing remark by Rose Friedman in a dialogue with her husband for his cassette commentary series possesses, in retrospect, an eerie quality. In a session recorded on June 14, 1972, she indicated that the discussion would turn to longer-term topics "in view of the absence of really hot immediate issues" (Instructional Dynamics Economics Cassette Tape 101, June 14, 1972).

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[154] Friedman (1974g, p. 64).
[155] Although these results came from the rational expectations literature, they lined up with Friedman's (1969, p. 45) observation that "anticipated inflations or deflations involve no tradeoffs between inflation and employment."
[156] Friedman (1977a, p. 412). This contrasted with Friedman's position on indexation ("escalator clauses") in Friedman (1958a, p. 252; p. 183 of 1969 reprint).

Just three days later, the break-in at the Watergate hotel took place in Washington, D.C. The subsequent tracing-back of the burglars' activities to the instructions of White House personnel gathered steam over the rest of 1972; in 1973 and 1974, this process transformed into what would be described on the back cover of Bob Woodward and Carl Bernstein's book *All the President's Men* as "THE BIGGEST STORY"—the Watergate scandal.

As of March 1973, the Watergate coverup was gaining headlines but President Nixon, having won his landslide reelection only four months earlier, was still in a strong political position. As noted in Section I, Nixon had felt able to relax the wage-price controls considerably (via Phase III) and it was against that background that Friedman offered the previously-quoted praise for the president in January 1973 in *Newsweek*. The same perspective underlay remarks Friedman wrote about two months later: "I honor the president's courage and good sense…" (*Newsweek*, April 2, 1973). This picture of a firm drive on Nixon's part to liberalize the controls system of his first term would, however, prove premature: six months later, Paul Samuelson (in *Newsweek*, September 24, 1973) would look back on the president's "on-again, off-again support for controls" over the course of 1973.

By late April 1973, the economic picture had deteriorated with the price-level surge; furthermore, President Nixon's political security had been greatly reduced by Watergate revelations that had implicated his closest domestic aides. At the end of the month, a financial columnist reported (*Manchester Union Leader*, April 29, 1973) that, in this environment, "[e]conomists at the giant commercial banks see pressures mounting for an almost-complete reversion to more rigid wage and price controls." Friedman shared concerns about this eventuality. Events moved in this direction in early May, when Nixon tightened the Phase III price controls. Nixon's new measures included a requirement that major firms give a month's notice if they planned to set prices more than 1.5 percent above their January 10 levels, as well as an announcement of investigations into these firms' pricing practices. Friedman commented that Nixon's latest move was "plain public relations gimmickry, which would not have the slightest effect on the course of inflation." He interpreted the president as endeavoring to relieve the public pressure on himself concerning inflation, at a time when Watergate strains were intensifying (*Daily News* (New York), May 3, 1973). Nixon's adjustments to Phase III had indeed come just two days after he dismissed his senior advisers H.R. Haldeman and John Ehrlichman in the wake of the Watergate disclosures.

Friedman commented further on Watergate in June. By this time, Paul Samuelson had publicly observed that the Watergate revelations had "irretrievably marred" the president's reputation

(*Newsweek*, June 11, 1973). In his own comments, Friedman stressed that he lacked any "special competence or knowledge on the political side," adding: "I don't know whether President Nixon will be forced to resign. I hope not." But, Friedman went on, the "odds certainly are increasing that way [resignation] every day." He judged that, in the face of such threats to his tenure, Nixon was now more likely to acquiesce to pressure for the federal government to undertake greater economic intervention (Instructional Dynamics Economics Cassette Tape 122, June 6, 1973).

That is indeed what happened, with the intensification of price controls announced by Nixon on June 13, 1973, with the new controls phase being, as noted above, unofficially labeled "Phase 3½" or "Phase IV." Another name given for the new phase was "Freeze II" (see, for example, Blinder, 1979, p. 109). For, like the original August 1971 proclamation of controls, the June 1973 measures froze consumer prices. One of the distinguishing features of what Darby (1976, p. 144) called "the fiasco which was Freeze II" was that the freeze, announced as applying for sixty days, applied only to final-goods prices and not to wages (*Daily News* (New York), June 14, 1973).[157] This aspect of the freeze gave Friedman some wry amusement. As already noted, he saw the new measures, like their predecessors, as driven by a cost-push view of inflation. But, unlike the 1971 freeze, the June 1973 freeze did not apply to wages; consequently, it was inconsistent even with the wage-push rationale often invoked in 1971 for the imposition of wage and price controls (Instructional Dynamics Economics Cassette Tape 123, June 23, 1973).

Friedman was away from the city of Chicago during the announcement of the June 1973 measures. In his absence, the *Chicago Tribune* contacted George Stigler for a local response. His reported reaction (*Chicago Tribune*, June 15, 1973) reflected Stigler's own disillusionment with the Nixon Administration's approach to economic policy. Stigler had been involved in the new administration's policy formation by accepting an invitation to head a Task Force on Productivity and Competition, which reviewed antitrust policy and reported to President Nixon in February 1969.[158] In 1973, however, Stigler remarked acidly on having had "the good luck to miss" the telecast of Nixon's announcement of Freeze II. Of the new measures, Stigler remarked: "This is lousy. We're just going back to the original [1971] freeze. The last time, we

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[157] In Freeze II, as well as the Phase IV program that followed in August 1973, there had not been a return to the stricter wage controls that prevailed in earlier years' phases; instead, the liberalization of wage limits introduced in January 1973 under Phase III had been permitted to continue (Darby, 1976, p. 144). (In contrast, Schuettinger and Butler, 1979, p. 108, stated, in a brief summary of the June 1973 package, that it included a freeze on wages—a puzzling contention in view of the fact that, as Blinder, 1979, p. 109, stressed, it was the absence of a corresponding wage freeze that made the new freeze historically anomalous.) In addition, agricultural prices were exempt from the June 1973 price freeze.

[158] See Committee on the Judiciary, U.S. House of Representatives (1974, p. 4).

quit when the controls began to fall apart. Maybe this time, we'll stay with them until their dismal failure—and finally learn our lesson."

As for Friedman himself, a few weeks before the announcement of the new price freeze, he had (in Instructional Dynamics Economics Cassette Tape 121, May 24, 1973) lamented the way the Nixon approach to inflation of recent years had both "miseducated the public and… introduced price control as a permanent weapon in the armory of economic policymakers." When Freeze II was announced, Friedman lashed out. His remarks in New York City the day after the freeze, at the *Institutional Investor*-sponsored second trust management conference, marked his most decisive break with the Nixon Administration (*American Banker*, June 15, 1973). Nixon's controls, he said, were the "worst mistake in American economic policy that has been made by an American president in the last 40 years" (*Oakland Tribune*, June 15, 1973), and they amounted to a policy that "has no end except ever-widening control over every facet of your life and my life" (*American Banker*, June 15, 1973.) "Once you start, where do you stop?," Friedman asked. "…You go from price controls to allocation controls to production controls… to rationing." He added that price controls had never worked over the previous two thousand years of history (*The Sun* (Baltimore), June 15, 1973).[159] A month later, Friedman remarked that he saw U.S. price control continuing indefinitely and declared this situation a disaster (*Bennington Banner* (Vermont), July 19, 1973).

The new price freeze ended on August 11, whereupon a more flexible Phase IV (genuinely bearing the name that some had given to the preceding Freeze II) governed until the end of the controls period (Darby, 1976, p. 145). As mentioned above, wage-price controls had been initially relaxed considerably in very early 1973, only to be intensified with the midyear freeze. The continued controls regime did not prevent a significant rise in inflation; and by late in the year, as discussed in Section I, even Arthur Burns, who had so promoted the 1971 proclamation of controls, favored letting U.S. wage and price controls end. Public opinion also shifted against controls during 1973, partly in response to the increased occurrence of goods shortages (Darby, 1976, pp. 143–144; Brittan and Lilley, 1977, p. 145). The popular backlash against controls that George Stigler hoped would occur had indeed materialized. In January 1974, John Dunlop, Director of the Cost of Living Council, confirmed: "The notion of Phase V is not a viable notion." (*Dallas Morning News*, January 17, 1974.)

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[159] Some studies that appeared in the 1970s suggested that the unsuccessful record of price controls actually stretched back forty centuries (Schuettinger and Butler, 1979—a book cited by Friedman and Friedman, 1980) or even fifty centuries (Schuettinger, 1976).

The 1970 law that permitted the controls was allowed to expire on April 30, 1974, and with it, the controls themselves lapsed (Darby, 1976, pp. 138, 145; Blinder, 1979, p. 110). Friedman's distaste at the experience and his wary attitude toward the president were highlighted by his reaction to a Nixon economic statement three months after the end of controls: "I especially welcomed the news Nixon will avoid wage-price controls. I hope this time he sticks to it." (*Times-Bulletin* (Van Wert, Ohio), July 27, 1974.)

The jaded tone in that remark likely reflected not only Friedman's disappointment with Nixon's 1971 U-turn but also the reality that, by late July 1974, there was abundant evidence that Nixon himself had misled the country about Watergate. The president's situation had already deteriorated after the first eight months of 1973 to such an extent that the *New York Times* (September 5, 1973) asked Friedman whether he regretted publicly supporting Nixon in 1972.[160] Friedman replied that he did not. "I do not condone the Watergate business. I think it's disgraceful… But Watergate does not alter what a disastrous choice McGovern would have been." Indeed, the fact that McGovern was the Democratic candidate in 1972 underscored for Friedman the lack of political imperative for Nixon's imposition of controls and his other 1971 changes to domestic economic policy. Friedman reasoned that, insofar as these measures bought short-term political benefits, those benefits had been unnecessary for Nixon's reelection, as George McGovern would likely have been the opposing party's presidential nominee regardless of whether controls had been imposed, and McGovern's candidacy itself ensured Nixon's reelection (Instructional Dynamics Economics Cassette Tape 119, April 25, 1973).[161] Furthermore, the longer-term costs of the New Economic Policy meant that its domestic aspects had not been politically beneficial beyond the short run—something Friedman was later pleased to see Nixon acknowledge in his presidential memoirs (see his column of *Newsweek*, October 16, 1978, referring to Nixon, 1978).

Even though Watergate often dominated economic developments in news coverage during 1973, Paul Samuelson observed in his column (*Newsweek*, September 24, 1973) that "inflation is the No. 1 political issue today—not Watergate, not détente with Russia and China and not even the rise in unemployment that now seems on its way." Friedman shared this view, and during 1974 he expressed tentative hope that political pressure like that Nixon had faced in 1970–1971 for

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[160] Friedman and several other academics were asked in the article whether they regretted including their names in full-page *New York Times* advertisements in October 1972 endorsing Nixon. In contrast to Friedman, Phillip Cagan and Friedman's University of Chicago colleague Lester Telser indicated that they regretted endorsing Nixon.
[161] However, in Congressional testimony on June 21, 1973, Friedman indicated that it was not until September 1972 that McGovern was no longer regarded as having a serious chance of winning the election (see Joint Economic Committee, 1973a, p. 145).

over-expansionary policies was becoming less likely to recur.[162] However, he also recognized that an abatement of the inflation rate from double-digit levels, if it occurred in conditions of economic sluggishness, might revive pressure for an urgent stimulation of demand, in which case the improvement in inflation would largely be temporary (*Newsweek*, June 24, 1974).[163] As discussed in later chapters, this scenario would be realized in 1975 and in the early years of the subsequent economic recovery. It had been made more likely, as discussed earlier and further in Section III below, by the fact that the United States' experience of the mid-1970s did little to discourage a nonmonetary approach to inflation analysis among policymakers, even though it did rule out mandatory controls as a viable anti-inflation weapon.

As far as the economic repercussions of Watergate were concerned, Paul Samuelson observed in a column (*Newsweek*, August 16, 1974) that blaming economic problems on Watergate was widespread but erroneous. This judgment lined up with sentiments Friedman had expressed the previous year, when he had said that Watergate had had essentially no direct effect on economic activity (Instructional Dynamics Economics Cassette Tape 122, June 6, 1973).[164]

As for indirect economic effects of Watergate, Friedman commented (in Instructional Dynamics Economics Cassette Tape 132, October 24, 1973) that the "only good thing I can see" from Watergate was that, as it so occupied Congress' time, fewer laws would likely be passed in the short run. But, as already noted, he saw Nixon's authority as weakened by Watergate, so the scandal tilted national policy in favor of the still-more interventionist economic policies favored by Nixon's political opponents. Friedman ventured to suggest that this did not have to be the case: several months into the post-election Watergate revelations, Friedman contended that Nixon's popularity could not go much lower—though it did—so "now is the time for him to do unpopular things." (Instructional Dynamics Economics Cassette Tape 23, June 23, 1973). But Friedman realized that this was not how events were proceeding in practice.[165] Indeed, he alluded

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[162] See, for example, Friedman (1975b, p. 709).
[163] See also Friedman (1975b, pp. 707–708).
[164] Nor did he think that Watergate would have repercussions for the world economy. Friedman judged that other countries' reaction to the Watergate scandal would be: "This is a nice show but I'm going to go about my business." (*Chicago Tribune*, October 23, 1973.)
[165] See especially Friedman's testimony of June 21, 1973, in Joint Economic Committee, U.S. Congress (1973a, pp. 144–145). In a similar vein, Paul Samuelson (in *Financial Times* (London), August 6, 1974) would conclude that "the true economic importance of President Nixon's Watergate troubles" was the loss of the clout needed to implement unpopular economic measures. Set against this is the fact that Samuelson was mainly referring to fiscal policy. The Federal Reserve did tighten monetary policy over much of the Watergate period, and Watergate arguably facilitated a monetary policy tightening by taking public attention away from economic policy. (On this score, Watergate did not have the effect Friedman feared in his June 1973 testimony, in which he had expressed concern about both monetary policy and fiscal policy remaining lax.)

to the leadership vacuum left by Nixon's predicament when, in complaining about the Federal Reserve's performance, he observed in early 1974: "If it does not take the lead in imposing the temporarily unpopular measures required, who will?"[166]

By early 1974, although the records of Nixon's tape-recorded White House conversations remained unreleased, and the matter of whether the president had himself participated in the Watergate coverup was still a matter of conjecture, enough of the White House officials' activities in relation to Watergate had come onto the public record for Friedman to indicate that "I'm not for a moment condoning any of those abuses" (Instructional Dynamics Economics Cassette Tape 138, January 16, 1974). What Friedman would call the "growing nightmare of Watergate" proceeded further during 1974, as Nixon's personal culpability in the affair became clearer.[167] White House conversation transcripts that Nixon released (doing so, as it happened, in the same week as the end of wage-price controls) were highly damaging to the president's case. An effort by the House Judiciary Committee to obtain more detailed and more accurate transcriptions of key conversations led to a new set of releases in July; these contained further evidence of Nixon's immersion in the coverup. In the first week of August, the president himself released a new transcript of a damning exchange from June 23, 1972—what came to be called the "smoking gun" conversation. In the wake of this disclosure, Missouri's newspaper the *Kansas City Times* editorialized (August 7, 1974): "Mr. Nixon has lied repeatedly to the American people."

Nixon resigned on August 9, 1974. That day, Friedman was quoted referring to Nixon's "misdeeds" in Watergate (*New York Times*, August 9, 1974). This example underlines the fact that *New York Times*' economics writer Leonard Silk was in error in subsequently claiming that Friedman refrained from public criticism of Nixon's conduct in connection with Watergate.[168] Furthermore, in November 1974 Friedman called Nixon's conduct "reprehensible" (Instructional Dynamics Economics Cassette Tape 157, November 6, 1974), and, two years later, he reflected more generally (*Chicago Tribune*, November 28, 1976): "I think he [Nixon] is a very complicated person. He operates on different levels. I do not approve of many things he did." In particular, he affirmed around the same time that Nixon's behavior in Watergate was "so reprehensible" (Instructional Dynamics Economics Cassette Tape 202, November 1976, Part 1). But Friedman also expressed some qualms about the way Nixon had been treated: he felt that the pressure for the president to resign had been precipitate, and that the appropriate procedure by

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[166] Friedman (1974d, p. 23).
[167] The quotation is from Friedman (1975a, p. 41).
[168] See Silk (1976, p. 90).

which Nixon might leave office before his second term expired would instead entail impeachment by the House of Representatives, followed by conviction in a trial in the Senate (Instructional Dynamics Economics Cassette Tape 156, October 23, 1974; see also Instructional Dynamics Economics Cassette Tape 138, January 16, 1974). In addition—referring, as he had previously, to what he perceived as the misdeeds of former president, Lyndon Johnson—Friedman claimed that Nixon's misconduct had not been greater than that of some of his predecessors.[169]

Although, on the whole, Friedman's judgment was that Nixon had been "a terrible president" (quoted in *Reason*, June 1995, p. 33), he was also capable of giving a more itemized appraisal.

With respect to Nixon's economic record, Friedman observed at the time of the president's resignation (Instructional Dynamics Economics Cassette Tape 151, August 7, 1974) that Nixon's international policy was good since 1971 and bad before 1971, with the reverse true for domestic policy. And for domestic policy, it remained the resort to wage-price controls that figured most heavily in Friedman's assessment of Nixon's economic legacy. Friedman was not always clear whether he believed Nixon's move in 1971 reflected a genuine but drastic switch in thinking toward Keynesianism or simply constituted political expediency. Either way, he saw it as showing a lack of principle—or not sticking to one's principles—on Nixon's part. It undermined Friedman's earlier belief that Nixon was principled in the area of domestic policy (expressed in Instructional Dynamics Economics Cassette Tape 17, March 1969). In the years after Nixon's resignation, Friedman often drew parallels between the president's economic-policy switch and Watergate, as they both exemplified the insufficient weight that Nixon had put on adherence to principle (*Chicago Tribune*, November 28, 1976; *The Power of Choice*, PBS, 2007). "I think the greatest disappointment to myself and many others has been what the Nixon regime turned into," Friedman observed when Nixon had been out of office for about three years (*Reason* magazine, August 1977, p. 29).

And as between Watergate and wage-price controls, Friedman was emphatic about which of them he regarded as the more damaging of Nixon's actions. In late 1974, Friedman observed (*Miami Herald*, November 24, 1974): "I hold that against him more than all that Watergate business. Watergate was trivial compared to the damage done by the imposition of wage and price controls."[170] "There never was a more clear, sharper, stronger statement of opposition to

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[169] In Feldberg, Jowell, and Mulholland (1976, p. 43).

[170] For a similar remark, see Instructional Dynamics Economics Cassette Tape 157 (November 6, 1974).

price and wage control than Mr. Nixon made for two years straight in 1969 and 1970," Friedman observed in a 1977 television appearance, "and [then] he went back on all of us by putting in price and wage control[s]…"[171] Later in the year, he repeated his view that the controls were "far more harmful than the much more publicized Watergate scandals," and a quarter-century later, in 2002, he declared that the imposition of controls was the "worst decision he [Nixon] ever made."[172]

III. PERSONALITIES IN MONETARY POLICY AND PUBLIC POLICY DEBATES, 1973–1974

ALBERT REES

Against a background of many discouraging developments, one change occurring in the 1973–1974 period that Friedman strongly applauded was the removal of U.S. wage and price controls. Their abolition was effective as of May 1974; three months later, Friedman took the ascension of Gerald Ford to the presidency as increasing the likelihood that controls had been ended permanently. From his interaction with Ford when Ford was a leading Congressman, he believed that Ford had economic principles. In addition, Friedman conjectured, Ford "will sacrifice those principles for political expediency less often than Mr. Nixon did. In retrospect, it's obvious that Nixon's biggest flaw was a willingness to sacrifice principle for political expediency. The impression I have of Jerry over the years is that he is less likely to do so."[173]

With regard to compulsory controls, Ford gave an early signal that reinforced Friedman's confidence. Ford told a press conference on August 28: "I foresee no circumstances under which I can see the reimposition of wage and price controls… [I]t means wage and price controls are out, period."[174]

But, even in these early stages of its life, the Ford Administration was giving mixed signals about

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[171] *MacNeil-Lehrer Report*, PBS, April 18, 1977; also transcribed in *American Banker*, April 21, 1977.
[172] The quotations are respectively from *Milton Friedman Speaks*, Episode 12, "Who Protects the Consumer?," taped September 12, 1977, p. 26 of transcript, and *A Conversation with Milton Friedman*, 2002. See also Friedman's remarks in the following: *New York Times*, August 9, 1974, and April 3, 1984; Instructional Dynamics Economics Cassette Tape 202 (November, 1976, Part 1); *Milton Friedman Speaks*, Episode 15, "The Future of Our Free Society," taped February 21, 1978, p. 11 of transcript; Friedman (1984b, p. 43); and his testimony of May 17, 1979, in Committee on the Judiciary, U.S. House of Representatives (1980, p. 155).
[173] *National Journal Reports*, August 17, 1974, p. 1224.
[174] From Ford's remarks at a press conference of August 28, 1974, at *https://www.presidency.ucsb.edu/documents/the-presidents-news-conference-74*.

whether and to what extent incomes policy would figure in the federal government's anti-inflation armory following the demise of formal controls. And, as we shall see, by the end of 1974, Friedman was expressing fears that Ford would eventually reimpose compulsory controls.

One of the manifestations of the Ford Administration's ambivalence toward incomes policy was in the appointments to senior economic positions. Ford kept as his Secretary of the Treasury William Simon and he secured the confirmation of Alan Greenspan, Nixon's nominee to be Chairman of the Council of Economic Advisers. Both these figures were strong opponents of incomes policy and remained so as members of the new administration. Furthermore, Greenspan in particular was an outspoken proponent of the monetary view of inflation, and in September 1974 he testified before the Joint Economic Committee that in "the longer term… the general price level is essentially a financial phenomenon which largely reflects changes in unit money supply."[175]

But, in strong contrast, Ford also brought into his economic team a major advocate of incomes policy and of cost-push views of inflation. Ironically, in view of the widespread tendency to regard the Chicago school as synonymous with Friedman's monetarism, the new appointment—Albert Rees—had nearly two decades of experience at the University of Chicago and had been a departmental colleague of Friedman over that whole period.

Rees' appointment was announced at a conference on inflation that the administration convened at the Hilton Hotel, Washington, D.C., on September 27–28, 1974. This summit was the culmination of a series of more specialized conferences on inflation held over the previous weeks. Two of these (on September 5 and 23) had been economists' conferences; Friedman attended both of the economists' conferences as well as the eventual summit, which contained a spectrum of participants of different specialties, as well as business and labor representatives. CEA Chairman Greenspan had hosted the first of these conferences at the White House, and President Ford himself had attended the first and final sessions of that conference. The final session consisted of summary statements to Ford from several of the leading attendees, including Friedman, Samuelson, John Kenneth Galbraith, former Secretary of the Treasury Shultz, and three past CEA chairs. In the session, Friedman observed: "There is one, and only one, way to cure the disease, to slow down the rate of increase of total dollar spending, and only the federal government can effect that cure." Friedman concluded his remarks by directly addressing Ford, urging him "to tell the public the hard truth and to persuade the public that the sooner we bite the

[175] From Greenspan's testimony of September 26, 1974, in Joint Economic Committee (1974c, p. 8).

bullet and take the cure, the better."[176]

Ford received the remarks politely: "Thank you very much, Milton."[177] But already Ford was making recourse to nonmonetary approaches to controlling inflation, and this trend would continue. Days before his resignation, President Nixon had sent legislation to Congress requesting the revival of the controls-era Cost of Living Council, and early in his presidency Ford renewed this request (*National Journal Reports*, August 17, 1974). The consequence of this proposal was the passing of a law creating the Council on Wage and Price Stability. At the September 28 proceeding of the general conference on inflation, Ford announced: "The Council on Wage and Price Stability, recently established by Congress at my request and with my deep appreciation, is another arm I will use in the fight on inflation. I have asked Dr. Albert Rees, a distinguished economist and professor of economics at Princeton, to direct the Council's work. We are fortunate to have Dr. Rees [here] with us."[178]

For most of his adult life, Rees, who was born in 1921, had known Friedman. They had first met in the mid-1940s. Rees had received a Ph.D. from the University of Chicago in 1950, his graduate studies having overlapped with Friedman's early years as a member of the Department of Economics. Even prior to completing his doctorate, Rees was deeply embedded in the working of the department: he had been made an assistant professor in 1948 (American Economic Association, 1974, p. 332) and was less formally a departmental member still earlier—as evidenced by his retrospective description of Henry Simons, who died in 1946, as among his "former colleagues" (in Selden, 1975, p. 83). Rees was promoted to associate professor in 1954 and professor in 1961, before leaving for Princeton University in 1966 (American Economic Association, 1974, p. 332). After Rees' departure from the University of Chicago, he and Friedman had kept in touch, and Rees was a participant in the conference held in Friedman's honor at the University of Virginia in October 1972.

Friedman was a great admirer of Rees' research in the area of the macroeconomics of labor markets—an attitude evidenced by Friedman's commissioning of Rees' (1962) book *The Economics of Trade Unions* for the Cambridge Economic Handbook series. Rees and Friedman had much common ground, their degree of accord being reflected in the label that Rees would receive of "Milton Friedman's Secretary of Labor" among people who knew them both (see Nelson, 2020b). In particular, and contrary to the analysis of labor unions made by a range of

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[176] In Council of Economic Advisers (1974, pp. 123, 124).
[177] In Council of Economic Advisers (1974, p. 125).
[178] In White House (1974, p. 292).

economists, Friedman and Rees both articulated the view that the prevalence of organized labor did not make nominal wages an exogenous variable insensitive to economic slack. Rees was also a strong supporter of Friedman's proposal for a negative income tax (see Selden, 1975, p. 183).

However, Friedman and Rees differed on an area highly relevant to Rees' new post in the Ford Administration. Rees was very much a subscriber to a traditional, permanently downward-sloping Phillips curve. This led him to articulate the view in the 1960s that higher inflation was acceptable in exchange for getting unemployment down. Strikingly, as the natural rate hypothesis emerged in the late 1960s, Rees would reaffirm his belief in a long-run nonvertical Phillips curve. In 1969, in a consultant's memorandum for the Federal Reserve he matter-of-factly took the unemployment/inflation tradeoff as a permanent reality.[179] As Rees explained in a lecture on "The Phillips Curve as a Menu for Policy Choice" that he gave in London in January 1970, he was aware of the challenge of the natural rate hypothesis—or, as he put it, "the 'expectations' approach, whose strongest proponents have been Professors Phelps and Friedman."[180] But, as he went on to explain, he did not find the hypothesis plausible: he argued instead that the long-run Phillips curve, though steeper than the short-run curve, was not vertical.[181] In September 1974, Friedman himself gave a lecture in London on the Phillips curve; the footnotes to the published version of this talk cited Rees (1970)—an *Economica* article consisting of Rees' lecture on the Phillips curve—as an example of the resistance that the natural rate hypothesis had faced.[182] Rees' opposition to the natural rate hypothesis had continued in the intervening years, and in 1973 he had again disputed Friedman's contention that the long-run curve was vertical (see Nelson, 2020b, Chapter 13).

Furthermore, like many believers in the original Phillips curve, Rees also took a "partial cost-push" view of inflation.[183] That is, while Rees believed that excess demand made inflation higher than otherwise and economic slack made inflation lower than otherwise, he regarded inflation as having an inherent tendency to occur even when demand was not excessive. According to this view, at full employment there was a tendency for nominal wages and prices to rise, due to cost-push factors having a positive mean.

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[179] See Nelson (2020b, Chapter 13).
[180] Rees (1970, p. 227).
[181] Rees (1970, p. 237).
[182] See Friedman (1975f, p. 18; 1976b, p. 221).
[183] That this was an important part of the view of inflation advanced by proponents of the Phillips curve during the 1950s and 1960s was emphasized, with documentation, in DiCecio and Nelson (2013), Nelson (2020a, Chapter 10), and Schwarzer (2018).

This perspective provided a rationale for direct government intervention in wage- and price-setting even in circumstances in which it was accepted that aggregate demand policies could influence inflation, as government intervention might improve the tradeoff (both short- and long-run) between inflation and unemployment. Rees' sympathy to incomes policy was reflected in his taking, during his time in the United Kingdom in 1969–1970, a visiting position in the Wilson Government's Prices and Incomes Board. When the new Heath Government abolished the Board, Friedman expressed delight at its demise in a letter to Rees—and gave Rees some friendly chiding about the time he had spent at the Board.[184]

The satisfaction that Friedman could take at the turn of events in the United Kingdom proved fleeting. By late 1974, official incomes policies had been back in force in the United Kingdom for two years, while various instrumentalities designed to monitor and regulate price-setting had been instituted by the U.K. government. And in the United States, Rees himself was in charge of the latest prices- and incomes-monitoring board. It did not have mandatory control powers. But, as Friedman's reactions to Arthur Burns' statements over the previous few years had indicated, Friedman did not believe this was a crucial distinction. Indeed, in his March 1973 lecture in Yugoslavia Friedman had remarked that "an 'incomes policy'" against inflation simply meant "controls on wages and prices."[185]

And when his appointment was announced, Rees himself indicated that he expected his agency's price and wage recommendations to be followed, even though they would not be compulsory edicts. He cited his own 1969–1970 experience with the U.K. Prices and Incomes Board, which likewise had lacked statutory enforcement powers, as providing a precedent: "By and large, its recommendations were widely accepted." (*The Daily Princetonian*, September 30, 1974.)

Congressional testimony that Rees gave in February 1975 explaining the functions of the Council on Wage and Price Stability confirmed that intervention would be vigorous: "In the months ahead, we plan to continue an active voluntary wage-price policy."[186] Rees also indicated that he believed that there was widespread wage-push pressure that he had a remit to fight: "Most of the wage increases that might contribute substantially to inflation are those reached through collective bargaining."[187]

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[184] Letter to Rees of November 9, 1970, from Friedman's papers in the Hoover Institution archives.
[185] Friedman (1973d, p. 6). See also Friedman (1976b, p. 233) for a similar remark.
[186] From Rees' testimony of February 5, 1975, in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1975b, p. 14).
[187] From Rees' testimony of February 5, 1975, in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1975b, p. 15).

"*Whip Inflation Now*"

By the time of Rees' testimony, one of the Ford Administration most-remembered, but also most desultory, initiatives against inflation had come and gone.

Notwithstanding the Federal Reserve's tightening phase, and the presence of figures in the Ford Administration like Greenspan and Simon, Friedman remained irritated by the proliferation of false cures for inflation. Following his attendance of Ford's general conference on inflation, Friedman lamented that the participants at the conference had offered a "bewildering variety of proposals for governmental action," other than action on aggregate demand (*Newsweek*, October 14, 1974). Following the conference, President Ford himself gave further credence to the notion that inflation could and should be eliminated using measures other than aggregate demand restraint. He announced a WIN ("Whip Inflation Now") campaign, in which members of the public were encouraged—via the wearing of WIN badges, ownership of other WIN memorabilia, or the signing of a WIN form—to identify themselves as inflation-fighters.

Friedman's reaction to the WIN campaign was derisive. It was a "bunch of hogwash," he remarked (*Chicago Tribune*, October 18, 1974; *Chicago Sun-Times*, October 27, 1974, p. 85).[188] Friedman also described himself as "very much disappointed" that, having rejected controls, the Ford Administration could then advance irrelevancies like the WIN initiative (*Chicago Sun-Times*, October 27, 1974, p. 85).

WIN was not well received by the public, and Ford Administration officials themselves quickly lost enthusiasm for the WIN program. By February 1975, it was clear that the WIN campaign had essentially been abandoned (*Washington Star-News* (Washington, D.C.), February 12, 1975). The "silly" WIN program, Friedman would note, had been "hastily buried" soon after its launch.[189]

Rees and administered price inflation

The demise of WIN did not, however, mean the end of nonmonetary measures against inflation by the Ford Administration.

In April 1975, Albert Rees appeared before Congress to recount the activities in which the

[188] See also Friedman (1974h, p. 2).
[189] Friedman (1976c, p. 10).

Council on Wage and Price Stability had been engaged in since its creation the previous year. In the 1950s, when theories of administered price inflation were gaining ground, he and Friedman had been in the vanguard of the counter-movement. They had stressed that even firms and workers that treated their product's price as a choice variable were ultimately beholden, in their choices, to the forces of demand and supply. But now Rees suggested that the possibility of administered inflation remained a live issue. "On April 14th, the Council had a conference on 'Concentration, Administered Prices and Inflation,'" he recalled. "The conferees represented all sides of the debate on the question of the effect on prices of a small number of producers with market power… One near-consensus of the meeting was that these 'administered prices' did not necessarily react quickly to changes in supply and demand. The debate centered around the questions of whether or not administered prices, over time, are in fact inflationary..."[190]

Rees also reaffirmed that the Council perceived wage-push forces that it would aim to counter: "We expect to become more active in monitoring wage negotiations, since we believe that wage increases will become a more important element in the inflationary process than they have been in the recent past…"[191] The following June, Rees cited reasons the Council needed to monitor wage and price increases: "there are noncompetitive forces in the private sector, in corporations, in strong trade unions, and in some agricultural cooperatives, and I believe that all of these organizations with market power have to be watched."[192]

In contrast, at the same June hearing a former Friedman student articulated the opposite position. Phillip Cagan testified: "I believe that the only way to control inflation is to see to it that aggregate demand, reflected in the budget and monetary policy, grows at an even pace… I think the Council is an exercise in futility and is a waste of the taxpayers' money."[193]

Friedman judged in October 1974 that there was a 50 percent chance that wage-price controls would be reintroduced by Ford before November 1976 (*Chicago Tribune*, October 18, 1974). There remained encouraging signs that this would not happen: for example, Secretary of the Treasury Simon observed in October 1974: "I thought we had learned our lesson [from wage-price controls]. It doesn't work." (*Salt Lake Tribune*, October 28, 1974.) But near the end of

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[190] From Rees' written answer in Committee on Appropriations, U.S. Senate (1975, p. 1829).
[191] From Rees' submission for a hearing held on April 23, 1975, in Committee on Appropriations, United States Senate (1975, p. 1821).
[192] From Rees' testimony of June 17, 1975, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1975a, p. 33).
[193] From Cagan's remarks on June 17, 1975, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1975a, p. 33).

1974 Friedman noted that controls seemed to be rating highly again in opinion polls—reflecting the fact, he suggested, that public understanding of inflation's causes remained poor.[194] He speculated that Ford might reinstitute compulsory controls: this time, he nominated August 1976 as a date by which controls might be revived.[195]

Developments during December 1974 seemed consistent with Ford going in this direction. In that month, Rees, at the president's request, urged that the steel industry rescind part of its announced price increases. Rees would testify the following June that the fact "we rolled back prices in the steel industry in December" was an achievement of his Council.[196] But the attitude underlying this policy move—that the control of inflation required that the private sector not be free to make decisions on price-setting—had been the basis for the controls programs under Nixon. It also provided Friedman with an unhappy reminder of President John F. Kennedy's 1962 intervention in the setting of steel prices: "It is really depressing in the extreme to see Mr. Ford proceeding on exactly the same path Mr. Kennedy proceeded on." (Instructional Dynamics Economics Cassette Tape 160, December 19, 1974.)

ARTHUR SCHLESINGER

In the course of a talk he gave in mid-1982, Friedman took issue with recent proclamations on the U.S. economy by "Arthur Schlesinger... [who] made his reputation as a historian but, apparently, is not content with that modest state."[197] These remarks amounted to a return bout with Schlesinger. Friedman had previously challenged the eminent historian in an exchange in the U.S. press in late 1974.

Arthur Schlesinger had an arrangement with the *Wall Street Journal* under which he supplied

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[194] Friedman (1975b, p. 707). At other times in 1973−1974, Friedman gave formulations that seemed to imply that the public understood that inflation was a monetary phenomenon. For example, in Friedman (1973d, p. 6) he said that "despite their complaints, people like inflation." By this, he meant that the expansionary policies that ultimately produce inflation initially had favorable effects on output and perceived real wages, and conversely that the public did not like the short-run real effects of disinflationary policies. These Friedman formulations elided the difference between public demands for overexpansionary policies, on the one hand, and public realization of or acquiescence in the inflationary results of those policies, on the other. When he did consider the latter more specifically, Friedman acknowledged that inflation *per se* was "a policy that nobody wants" (see his June 21, 1973, testimony, in Joint Economic Committee, 1973a, p. 135).

[195] Friedman (1975b, p. 707). See also Friedman's remarks in Instructional Dynamics Economics Cassette Tape 159 (late November/early December 1974).

[196] From Rees' testimony of June 17, 1975, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1975a, p. 33).

[197] Friedman (1982b, p. 54).

occasional op-eds. It was in this capacity that he wrote an op-ed for the edition of October 23, 1974, titled "How About Taking Inflation Seriously?" Schlesinger acknowledged that his op-eds to date had "stayed a good distance away from economics," but he declared that this practice now had to stop. His new op-ed turned out not just to be on economics, but to be directly concerned with a topic in Friedman's area of specialty of economics. Schlesinger dealt with the causes of, and appropriate response to, the phenomenon of an economy being struck by "deepening recession as well as by spiraling inflation." In short—although he did not use the term in his op-ed—Schlesinger was concerned with "stagflation."

The term "stagflation" had been used in U.K. policy debates by Conservative party politician Iain Macleod in 1965 (see Nelson and Nikolov, 2004).[198] It had not caught on then, but after Macleod prominently reused the term shortly before his death in 1970, "stagflation" recurred in U.K. economic discussions. Indeed, Friedman heard the term during his September 1970 U.K. visit (Instructional Dynamics Economics Cassette Tape 58, October 4, 1970).[199]

In U.S. outlets, the term was also picked up to a limited extent during 1970–1971. For example, it appeared in U.S. press reports on U.K. developments (for example, *New York Times*, July 22 and September 6, 1970; *Time*, September 14, 1970), while the term was used in U.S. economic discussions in Fall 1970 by Herbert Stein (*Manchester Union-Leader* (New Hampshire), October 6, 1970) and Senator William Proxmire (*New York Times*, October 18, 1970).[200] Martin Bronfenbrenner of Carnegie Mellon University also used the term both in the title and the text of a lecture, "A Reconsideration of 'Nixonomics' and 'Stagflation,'" in a speech he gave in South Korea that was issued by the Bank of Korea on September 28, 1970, and in a revised discussion titled "Nixonomics and Stagflation Reconsidered" delivered in Kalamazoo, Michigan, on February 24, 1971, and published later in 1971.[201] In 1972, the word "stagflation" appeared as an entry in a U.K. business dictionary (Davis, 1972, p. 176), while the United States' Merriam Company indicated it would be including the word in the next major revision of the Merriam Webster dictionary (*The Crescent-News* (Defiance, Ohio), February 4, 1972; F.E. Compton Co., 1972, p. 532). In late 1972, with the U.S. economy expanding and recorded inflation lower than

[198] In its obituary of Clark Warburton, the *Washington Star* (September 21, 1979) claimed inaccurately that Warburton coined the term during the 1960s.
[199] Friedman did not use the term when he inaugurated the Institute of Economic Affairs/University of London lecture series during his visit (Friedman, 1970c). However, a year later, James Meade used it when he gave the second lecture in the same series (see Meade, 1971).
[200] Early usages of the term in the London press included *Financial Times*, July 8, 1970, and *Daily Mail*, July 16, 1970. See also Boianovsky (2021, p. 11) on the subsequent usage of the term in Continental Europe.
[201] See Bronfenbrenner (1970, 1971). The first research-journal article mention of "stagflation" appears to have been Bell (1971, p. 22) and the first journal article to use the term in the title was Truu (1971).

in earlier in the decade, columnist Hobart Rowen drew a contrast with "the 'stagflation' policy of the early Nixon years" (*Boston Globe*, November 5, 1972). Friedman himself favored the policy of the early Nixon years and saw a continuation of that policy as more likely to set the economy up for a durable situation of noninflationary economic expansion. But he did acknowledge that the pre-controls Nixon policy largely proceeded against a background of " 'stagflation,' as it's been called—we had it in this country in 1970" (Instructional Dynamics Economics Cassette Tape 95, March 22, 1972).

The word became much more widely used when Paul Samuelson—who had deployed the term in a panel alongside Friedman in early 1972 (*Newsweek*, January 31, 1972, p. 75)—reused it on multiple occasions the following year. In particular, the new edition of his textbook, appearing in March, not only used "stagflation" but gave it an index entry, and Samuelson also used the term in his *Newsweek* column of March 19, 1973.[202]

It may have been because he had just read this column that Friedman himself used the term in his lecture in Yugoslavia in March 1973.[203] Over subsequent years, he would describe the terminology as "unlovely" and refer to "that miserable, unpleasant word," apparently registering his distaste both for the word itself and the condition it summarized.[204]

U.S. media commentary on stagflation's causes and implications was abundant in 1973−1974. As Dornbusch and Fischer (1994, pp. 484–485) would recall, "during periods of stagflation, such as 1973−1974… there are articles in the newspapers that the laws of economics are not working as they should because inflation is high or rising even as output is falling." Schlesinger's *Wall Street Journal* piece was a perfect example of such an article, as it declared that the phenomenon of stagflation was testament to the fact that "in the commanding heights prices are set, not by laws of supply and demand operating in a free market, but by private market power."

Assertions such as Schlesinger's appeared notwithstanding the fact that, as Dornbusch and Fischer noted, stagflation was reconcilable with demand/supply analysis. They noted two phenomena that provided such a reconciliation, both of them relevant to analysis of 1973−1974.

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[202] The column would be reproduced in Samuelson (1973b), as was a January 1973 essay in which Samuelson also used the word "stagflation" (see p. 218). For his textbook's references to "stagflation," see Samuelson (1973a, pp. 825, 829, 833). Samuelson's other uses of the word around this time included his commentaries on his own Instructional Dynamics Economics Cassette series of November 3 and 17, 1972 and of January 12 and February 8, 1973 (Tapes 114, 115, 119, and 121 in the Samuelson commentaries) as well as Samuelson (1973c, p. 226).
[203] Friedman (1973d, p. 8).
[204] On "unlovely," see Friedman (1977c, p. 455) and Friedman and Schwartz (1982, p. 442); the "miserable…" remark appeared in Friedman (1983c, p. 48).

First, there had been supply shocks, such as the first OPEC oil price increase, that lowered potential output. Second, for given potential output, stagflation was explicable using an expectations-augmented Phillips curve: An adjustment upward in inflation expectations implied a leftward shift of the aggregate supply curve, and so a deterioration in the inflation/unemployment combinations available to the economy in the short run.

The latter explanation of stagflation—in which the coexistence of inflation and above-normal unemployment is a part of the endogenous dynamics of the response of the two series to monetary policy actions—had, of course, been something Friedman had stressed when advancing the expectations-augmented Phillips curve in the 1960s.

Stagflation was difficult to reconcile with a traditional Phillips curve that lacked a prominent expectations term. Hence the phenomenon was puzzling to subscribers to that version of the Phillips curve. Paul Samuelson, for example, accepted that the recent boom of 1973 had generated inflation, but he added that "at this time, and as far that one can look ahead, the country definitely faces a problem of wage-push." (*Financial Times* (London), August 6, 1974.) Similarly, during the 1973–1975 recession Albert Rees remarked that "it is unusual to have inflation and recession simultaneously."[205]

Friedman, in contrast, noted that the emerging inflation and unemployment outcomes were vindicating his own version of the Phillips curve. By early 1973, with the global experience of stagflation experienced to date, alongside developments in the research literature favoring the Friedman-Phelps perspective, Friedman was able to observe that "it is now being increasingly recognized" that there is "no tradeoff except as a very temporary phenomenon."[206]

In the months ahead of the appearance of Schlesinger's op-ed, Friedman made several more public affirmations of the success of the natural rate hypothesis. In May 1974, he observed: "Recent stagflation, etc.—in this country, in Britain, and elsewhere—has just put to rest the idea which Bill Phillips is his original article produced on the basis of evidence of a century in Britain."[207] Similarly, in his lecture in London in September 1974, Friedman noted that the "emergence of 'stagflation'… [has] rendered somewhat ludicrous the confident statements that

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[205] From Rees' testimony of June 17, 1975, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1975, p. 32).
[206] Friedman (1973d, p. 7).
[207] Instructional Dynamics Economics Cassette Tape 145 (May 1, 1974). Similarly, in *The Economist* (September 28, 1974), Friedman referred to "A.W. Phillips' incorrect interpretation."

many economists had made about 'tradeoffs,' based on empirically-fitted Phillips curves."[208]

At the same time, however, a strand of Keynesian economics could legitimately argue that stagflation was reconcilable with Keynesianism, even without oil shocks. Thirlwall (2018, p. 14), for example, has observed that it is inaccurate to suppose that "Keynes's macro-model cannot explain stagflation," because Keynes' analysis allowed for exogenous wage-push inflation that made both unemployment and inflation worse. Though the Phillips-curve literature had stressed the endogeneity of inflation, arguments that inflation was almost wholly cost-push had long been dominant in U.K. policy circles, and it was the consistency of stagflation with the cost-push view that had prompted Arthur Burns and other key figures in the United States to embrace cost-push views from 1970 onward.[209]

Arthur Schlesinger took the cost-push explanation of inflation as accurate. Hence, Schlesinger argued, what economic analysis needed to do was go back to cost-push theories of inflation as espoused in the United States in past decades by figures such as Galbraith and Gardiner Means. Schlesinger made next to no distinction among the monetarist position, older Phillips-curve views, and the attitude of the Ford Administration. Rather, he seemingly took for granted that they all used a permanently-nonvertical Phillips curve as their understanding of inflation and as the basis for using aggregate demand restriction against inflation. Therefore—although he would later offer qualified praise for Friedman by labeling him "the ablest conservative economist of our own day" (*The Sunday Plain Dealer* (Cleveland, Ohio), June 22, 1975)—Schlesinger provided little acknowledgment either of the fact that proponents of the natural rate hypothesis rejected a long-run nonvertical Phillips curve, or of the fact that policymakers like Burns had in recent years themselves embraced cost-push views. Indeed, as already noted, during the mid-1970s a Ford Administration policymaker, Albert Rees, buttressed official support for cost-push theories by promoting the administered-inflation idea that profit-push

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[208] Friedman (1975f, p. 19; 1976b, p. 221). Friedman made a related remark, further using the term "stagflation," later in the lecture (1975f, p. 21; 1976b, p. 226). In this second passage of the lecture, Friedman took stagflation as so far having shifted general opinion among economists in favor of an expectations-augmented Phillips curve but not necessarily one that implied a long-run absence of a tradeoff (that is, one with a unitary coefficient on the expected-inflation term). This characterization, as discussed in Nelson (2020b, Chapter 14), may have inadequately recognized the degree of U.S. academic conversion to the natural rate hypothesis by 1974. However, the acceptance of a long-run vertical Phillips curve for the understanding of inflation/unemployment patterns remained uncommon in 1974 in U.S. media and policy circles, as well as in academic economics outside the United States.

[209] Over the same period, cost-push views were also prevalent in much of continental Europe. For example, in June 1975, Jelle Zijlstra, head of The Netherlands' central bank, stated: "Taking a longer view than just the past year, inflation has accelerated regardless of the economic weather… [T]he power of particular groups and interests to push incomes and prices up has exceeded the power of governments to stop them from doing so." (Quoted in *Kansas City Star* (Missouri), August 14, 1975.)

forces drove up inflation.

A key reason why Schlesinger likely saw little similarity between his own views and those of the Ford Administration likely lay in the fact that that he favored, and officials did not, a return to widespread compulsory price controls. Much like Friedman did at the time, Schlesinger speculated that the Ford Administration might ultimately reimpose controls. But, in contrast to Friedman, Schlesinger saw such a return as desirable: "the time has come to substitute price-fixing in the public interest for price-fixing in the interest of private profit" (*Wall Street Journal*, October 30, 1974).

Friedman was galvanized by Schlesinger's op-ed into writing a letter to the editor, published in the *Wall Street Journal* about three weeks after Schlesinger's article appeared. "Since Mr. Schlesinger ventures into economics," Friedman wrote, "perhaps I may venture into history."

Friedman had long had serious doubts about historians' coverage of U.S. economic development. Speaking at Rockford College, Illinois, on December 6, 1974, a couple of weeks after his reply to Schlesinger appeared, Friedman would recall that when he and Schwartz were writing their monetary history, "I read a great many of the general histories of the nineteenth century," and he had found himself "simply appalled by the level of ignorance of economic matters that was displayed in those history books…"[210]

This dissatisfaction extended to historians' accounts of the twentieth century, as Friedman's rebuttal to Schlesinger's letter indicated. The historical record, Friedman argued in his *Wall Street Journal* letter, suggested that large firms' power had not increased substantially over time. This observation buttressed a point Friedman had made eighteen months earlier, when the Nixon Administration had oriented price controls toward large firms. On that occasion, he had pointed out that this made little sense even from an arithmetic point of view, as the largest price increases were then coming from small firms and agricultural prices (*Daily News* (New York), May 3, 1973). By late 1974, with larger firms accounting arithmetically for more price rises, the idea of administered inflation had, on the surface, acquired enhanced plausibility. But, Friedman stressed in his letter, the United States had acquired a tendency for inflation in the postwar period even though it was not obvious that firm power had increased over that period. In contrast, he added, the economic role of government had increased over the same period, and that role was a

[210] Friedman (1976e, p. 6). See also Friedman (1983b, p. 65).

more plausible source of the emergence of sustained inflation.[211]

Friedman concluded: "Mr. Schlesinger's prescription for inflation—increased direct control over prices by the government—is like prescribing a bottle of Scotch as a cure for alcoholism." (*Wall Street Journal*, November 23, 1974.)

This was a poignant analogy. Over 1973–1974, Friedman repeatedly used analogies linking inflation or inflationary booms with alcoholism.[212] "There is a very close relation between the problem of inflation and the problem of drinking," he remarked in mid-1973 (*Chicago Daily News*, June 14, 1973). Friedman would stick with this analogy in later years.[213] It is likely that his judgment that the analogy was fitting arose largely from interactions with particular colleagues at the University of Chicago. Among many of the university's economists, there was a culture of heavy drinking. Although not part of this scene, Friedman saw it at close hand. In time, Friedman's advocacy of drug legalization would lead to accusations that he had a carefree attitude toward substance abuse.[214] But in fact Friedman, who drank only sparingly, was all too aware of the self-destructiveness arising from alcoholism and other addictions. And his own heart problems in the early 1970s would only have magnified his awareness of the health dangers that could arise from undue alcohol consumption. When, in early 1974, while Friedman was attending a conference in Paris, fellow conference attendee Peter Jonson suggested they go over to a conference event at which drinks were "free," Friedman replied: "Perhaps free for you, young man, but not for me." (Peter Jonson, personal communication, December 28, 2018.)[215]

As for the actual cure, Friedman did share the position of many traditional Phillips-curve proponents that it involved demand restriction and abnormally high unemployment. The distinguishing feature of his position was that this period of economic slack would only be temporary; the fall in inflation would, in contrast, be permanent. In a February 1974 radio broadcast, Friedman had noted that "given that we have started on an inflationary course, there is

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[211] Recall from Section I that, although he was unsympathetic with ideas that monetary policy was forced to accommodate fiscal policy actions, Friedman saw monetary policy in the postwar period as having been inflationary in part by taking actions intended to stabilize the economy and interest rates.
[212] See, for example, Friedman (1973c, p. 31; 1973d, p. 8; 1975b, p. 702), as well as the examples given in Section I of this chapter and in the discussion that follows.
[213] Among many examples, see Friedman (1975e, p. 18; 1992a, pp. 214–215), Feldberg, Jowell, and Mulholland (1976, p. 17), and Friedman and Friedman (1980, pp. 270–276). See also *The Listener* (London), April 24, 1980.
[214] See Chapter 11 below on 1987–1992 for further discussion.
[215] As indicated, however, this posture did not imply teetotaling behavior on Friedman's part. Later in the year, an interviewer reported him "sipping a Bloody Mary" after playing tennis (*Chicago Sun-Times*, October 27, 1974, p. 85). In 1986, he was reported drinking a Scotch and soda at a reception (*Los Angeles Times*, December 14, 1986 p. 12).

no way of going from that position to a position of no inflation without passing through a period of unemployment."[216] Furthermore, failing to restrict demand would not maintain the lowering of unemployment initially associated with an expansionary policy, as that policy had pushed unemployment below its natural rate. Rather, the expectations argument, as he stressed at the December 1974 American Economic Association meetings, implied not only that unemployment fell in the early stages of a shift to a more inflationary monetary policy regime, but rose later on as it returned to its natural rate—the "stagflation stage" of the dynamic adjustment of the economy to the new inflation rate.[217]

What a disinflationary policy could achieve was restoration of the original, lower, inflation rate, at the cost of experiencing an interim period in which unemployment rose *above* its natural rate. Once inflation expectations fell back into equality with the inflation rate, unemployment would revert to its natural rate. This was a point that Friedman, several weeks before his American Economic Association remarks, had illustrated by again deploying an analogy to drinking. "Inflation is like alcoholism. The good effects come first with the first few drinks, but the bad effects are felt the next morning with the hangover. If you go on the wagon, the bad effects from the attempt of a cure are felt first, but the good effects come later when the cure takes hold." (*Chicago Tribune*, December 1, 1974.)

What Friedman would see soon after he made these remarks was that the "bad effects" of monetary restriction—higher unemployment—were being made worse than necessary by a lurch by the Federal Reserve in the direction of severe tightening. This shift in monetary policy would dominate Friedman's commentary on the U.S. economy during much of 1975.

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[216] *University of Chicago Magazine*, August 1974, p. 13. See also Section I above.
[217] Friedman (1975c, p. 178).

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 3: Debates on International Economic Policy and Geopolitical Developments, 1973 to 1974

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July 30, 2023

I. EVENTS AND ACTIVITIES RELATED TO DEBATES ON INTERNATIONAL ECONOMIC POLICY AND GEOPOLITICAL DEVELOPMENTS, 1973–1974

"Saturday Signing Brings Peace!" declared the front-page headline of *The Desert Sun* for January 26, 1973. This was how the local newspaper of Palm Springs—the city in which Friedman was recuperating from his heart surgery—announced what was believed to be the end to the Vietnam War. The *Desert Sun* report quoted U.S. Secretary of State William Rogers saying of the ceasefire arrangement coming into force that weekend: "we hope that this does usher in a generation of peace."

The ceasefire agreement—embedded in what were known as the Paris Accords—followed a period in which President Nixon had intensified U.S. forces' bombing campaign against North Vietnam. That intensification may have played a role in Friedman telling Nixon in their December 1972 telephone conversation that the president was doing a fine job.[2] For, prior to this conversation, in his occasional public comments on the topic, Friedman had suggested that air attacks should play a larger role in the United States' prosecution of the war, and that undue emphasis had been put on troop-intensive ground combat.

This viewpoint was similar to that of Senator Barry Goldwater, the 1964 U.S. presidential candidate whom Friedman had served as an economic adviser. Goldwater—who was a far closer observer of defense and foreign-policy matters than Friedman was—had strongly approved of Nixon's intensification of air attacks, and he saw the Paris Accords as flowing from that move (*Arizona Republic*, January 25, 1973). This judgment was reflected in Goldwater's euphoric

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[1] Email: Edward.Nelson@frb.gov. The author is grateful to George Tavlas for comments on a previous draft of this chapter and for help in providing useful information. See the Introduction in Nelson (2020a) for a full list of acknowledgments. The views expressed here are the author's alone and should not be interpreted as those of the Federal Reserve or the Board of Governors.
[2] See Nelson (2020b, Chapter 15). However, the Friedman/Nixon conversation preceded by several days the period regarded as featuring the heaviest intensification of U.S. air attacks.

reaction to the January announcement. "The ceasefire-and-peace agreement in Vietnam marks one of the most important victories the United States has ever scored over Communist aggression," Goldwater contended. "…[I]t is a monumental achievement for the President of the United States, attesting at one and the same time to Richard Nixon's vision, courage, and ability. What the accord means is that a strong president, willing to take bold action, literally snatched victory from the jaws of defeat…" (*Arizona Republic*, January 26, 1973.)[3]

However, even in the period of 1972−1973 when it was possible to believe, as Goldwater suggested, that the United States was achieving something approximating a military victory, Friedman implied that lasting damage to the United States' domestic scene had been created by the Vietnam War. In October 1972, he had observed that the "history of the last ten years would have been wholly different … [without] this terrible bypass via Vietnam." The war had "altered the intellectual atmosphere" in a way that, in Friedman's estimation, derailed the momentum for free-market thinking that he had perceived when he wrote *Capitalism and Freedom* (Selden, 1975, p. 51). The war and its prosecution had generated antagonism toward U.S. government decisions from university students and academics—in part because of the conscription policy that Friedman himself strongly opposed—and had reduced the confidence by the U.S. populace in the rule of government.[4] However, very much in contrast to Friedman's attitude, the leading critique of the government that had developed in U.S. academia as a result of antiwar sentiment supported more, not less, public-sector intervention in the domestic economy.

In the event, the "peace" to which Goldwater and Rogers referred in their January 1973 remarks did not materialize. Fighting continued after the withdrawal of large-scale U.S. forces. In late April, Friedman remarked that it was doubtful whether one could refer to the Vietnam War as having ended, in view of the fact that the "whole thing is coming unstuck right now" (Instructional Dynamics Economics Cassette Tape 119, April 25, 1973). Some weeks later, Paul Samuelson astutely noted in his *Newsweek* column (of June 11, 1973) that it was not yet clear whether the January 1973 settlement would be regarded as a historic Nixon achievement. During the early Nixon years, Samuelson had expressed the judgment that the president realized he could not win the war (Instructional Dynamics Economics Cassette Tape 30 (Paul Samuelson series), July 1969). Consequently, Samuelson was skeptical that Nixon in 1972−1973 had so

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[3] The Goldwater commentaries reported in the *Arizona Republic* on January 25 and 26 consisted, respectively, of remarks in an interview he gave on January 24 (following Nixon's national address, on the evening of January 23, announcing the agreement), and of a syndicated op-ed Goldwater subsequently penned on the matter.

[4] For example, in his Congressional testimony of June 21, 1973, Friedman remarked that "the moral climate… suffered very greatly from the divisiveness over the Vietnam issue in the past ten years" (in Joint Economic Committee, 1973a, p. 137).

turned the tide that he had secured a sustainable settlement that would preserve South Vietnam as a noncommunist state. South Vietnam went on to fall to Communist forces in April 1975. In light of this outcome, Milton and Rose Friedman would, years further on, have occasion to refer to the sequence of events that "condemned us to defeat in Vietnam."[5]

The fact that the Vietnam War outcome eventually came to be judged as ending in defeat for the United States opened up the question about whether the 1973 settlement was known by its signatories to be unsustainable. Answering this question is well beyond the scope of this book, but it is a question that, roughly speaking, has generated two diametrically opposed answers. On one view—prominently advanced by Nixon in his memoirs (1978, p. 889)—the 1973 settlement could have been preserved had the United States both provided military support and imposed economic sanctions when treaty violations occurred during the period from 1973 to 1975. On the other view, the U.S. leaders knew even in 1973 that the settlement would not be sustained and that South Vietnam's defeat was therefore a near-inevitable future step.[6]

Those followers of foreign policy to whom Friedman himself was close did not have a unanimous position on which of these interpretations was more valid. Among them, Friedman's friend Ben Stein—a White House aide until Nixon's departure in August 1974—was a proponent of the view that the 1973 settlement could have been sustained. This contrasted with the ultimate conclusion of Friedman's first dissertation student, Warren Nutter. Nutter, who had served in a senior position in the U.S. Department of Defense during Nixon's first term, initially defended the Paris Accords, describing them in a February 1973 television appearance as delivering "an honorable settlement."[7] But Nutter later came to doubt whether the administration had been sincere regarding its stated intention to act to enforce the Accords.[8] He would become an outspoken public critic of Henry Kissinger, who was Nixon's national security adviser at the time of the Paris Accords and who would succeed Rogers as U.S. Secretary of State in August 1973.[9]

When looking back on the war, Milton and Rose Friedman would imply that it had been lost well before 1973. They judged that the diminution in U.S. public support for the fighting meant that

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[5] Friedman and Friedman (1985, p. 80).
[6] See Garthoff (1994, pp. 293, 491). This second interpretation is partly supported by Dallek's (2007, p. 454) subsequent analysis of White House conversations at the time of the settlement.
[7] In Kaplan and others (1973, p. 61).
[8] See Palmer (1984, pp. 141−142).
[9] For further discussion, see Chapter 7.

the country was unwilling to allocate the resources needed for victory.[10] They also believed that, as "the Vietnam War helped to undermine belief in the beneficence of government," a long-term effect of the war, including America's defeat, was to boost support for free-market views.[11]

Research output in 1973 and 1974

With the U.S. withdrawal from Vietnam leading to the end of conscription in 1973, Friedman in 1973–1974 confined his writings on international matters primarily to the topics of other countries' economic experiences and of international economic policy. Some of this activity was manifested in his research publications and projects over this period.

In the second half of 1973, Friedman's book *Money and Economic Development* (some of which was discussed in the previous chapter) appeared. Friedman gave the book a bit of advance publicity in his June 1973 Congressional testimony, telling members of the Joint Economic Committee that it would soon be released.[12] In that testimony, Friedman observed that in *Money and Economic Development* he "recommended as probably the optimum policy under current conditions for a developing country that it peg its exchange rate to its major trading partner…"[13] The book's remarks, together with those Friedman made elsewhere, have been seized upon by critics of floating exchange rates—an early example being Bergsten (1975, p. 456), who declared that "further reflection has led even the most ardent supporters of flexible rates (e.g., Friedman) to admit that fixed rates were superior for some countries"—who have interpreted Friedman as seeing his case for floating exchange rates as having limited applicability.[14] According to their interpretation, Friedman was a supporter of fixed exchange rates for small and highly open economies, or indifferent on those countries' choice between fixed and floating rates (see, for example, Hanke, 2008).

But the remarks in the 1973 book, alongside Friedman's other writings, do not sustain this interpretation. They suggest instead that he saw the countries for which fixed exchange rates

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[10] Friedman and Friedman (1985, p. 80).
[11] Friedman and Friedman (1988, p. 484).
[12] See Friedman's testimony of June 21, 1973, in Joint Economic Committee, U.S. Congress (1973a, p. 115).
[13] From Friedman's testimony of June 21, 1973, in Joint Economic Committee, U.S. Congress (1973a, p. 115).
[14] Another early example was Charles Kindleberger, who, in a paper for the November 1974 Carnegie-Rochester Conference, remarked (on the basis of their June 1973 Congressional testimony) that "Friedman and Meltzer believe that small countries should not have independent monetary policies, but tie to a larger unit and take their money supply and interest rates from it." (Kindleberger, 1976, p. 72.)

were preferable to floating rates as falling into a very narrow category.[15] His remarks implied that fixed rates were likely to be valid only for some developing economies and for *very* small city-state economies, such as pre-1997 Hong Kong.[16]

It is also important to note that Friedman's favorable attitude toward a fixed parity in the case of small economies was not a new position on his part in the early 1970s.[17] For example, in early 1968 he observed that in conditions in which the United States followed liberal international arrangements alongside an aggregate demand policy directed at internal stabilization, "many another country would be well advised to link its currency with ours" (*Newsweek*, January 29, 1968).[18]

As *Money and Economic Development* underlined, Friedman's favorable remarks about non-floating regimes were primarily in reference to the case of a currency union between a developing economy and a larger economy, and not to fixed rates that went short of such a union.[19] "I regard a floating rate as second best for such a country," Friedman stated. His basis for this belief that a floating rate gave a developing economy the leeway to take recourse to high inflation as a source of revenue, an opportunity it might well seize, especially if it lacked reliable means of obtaining regular forms of taxes on a larger scale. Large-scale recourse to the inflation tax, he believed, would lower the real output of the economy, both because of the adverse influence of inflation on economic activity and because higher revenues for the public sector would encourage public spending over private spending.[20]

Consistent with his belief in the wide applicability of flexible rates, Friedman during visits to

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[15] See his testimony of June 21, 1973, in Joint Economic Committee (1973a, pp. 127−128) and the discussion that follows.

[16] For example, in September 1965 (as recorded in American Enterprise Institute, 1966, p. 109), Friedman stated: "I think it would be most unwise at the present moment, for example, for Hong King to give up its present fixed rates..." In 1983, Friedman would make informal contributions to the discussions that led to the introduction of Hong Kong's currency board, based on a close link to the U.S. dollar (Friedman and Friedman, 1998, p. 326).

[17] For a contrary interpretation, see Edwards (2021). In the present author's view, that contrary interpretation is not sustainable in the face of Friedman statements that appeared on the record before the early 1970s, including Friedman and Roosa (1967, p. 121) and other items highlighted here.

[18] Similarly, in 1973 Friedman indicated that his position was not newly arrived at: "I continue to believe that, so far as international monetary arrangements are concerned, the best solution would be… [a floating system], with various minor countries, if they wished, tying their currencies to one of major countries." (Instructional Dynamics Economics Cassette Tape 126, August 2, 1973.)

[19] A confusion is that here, as in similar remarks in Friedman and Roosa (1967, p. 121), Friedman insisted on calling a fixed exchange rate between two currencies a "peg" and that a truly fixed rate came only with a currency union. In Friedman (1973d, p. 12), in contrast, he called a peg a "'dirty' fixed rates" system, with a currency union being "clean" fixed rates. See also Friedman (1982a, p. 99).

[20] Friedman (1973b, p. 64). He included among the cost of inflations the distortions arising from attempts to suppress it via price and other direct controls: see Friedman (1974i, pp. 273−277).

Australia in 1975 and 1981 urged that that country float its exchange rate. Furthermore, during his period (1988–2003) as Governor of the Reserve Bank of New Zealand, Don Brash wrote to Friedman, asking whether he believed fixed exchange rates, or even membership of a currency union, were appropriate for New Zealand—which under Brash had a floating rate alongside an inflation-targeting domestic monetary policy strategy. Friedman, Brash recalled, "wrote back and said, 'No, no, I don't think you should do that at all.'… And he made it very clear that we should not depart from our current arrangements." (Don Brash, interview, July 9, 2013.) Likewise, in 1998, Friedman characterized the economies of Australia and New Zealand, which had floating rates, as having weathered the Asian financial crisis better than those economies in the region that had fixed their exchange rates (*The Times*, October 12, 1998).

Money and Economic Development also offered a seemingly strong empirical demonstration of the validity of the quantity theory of money. Friedman presented a scatter plot depicting country averages of monetary growth per unit of output and of inflation for forty countries. What emerged was a visually impressive cluster around a 45-degree-line.[21] This result seemed to line up with the quantity theory's notion of a one-to-one long-run relationship between monetary growth per unit of output and inflation. Although cross-country-average plots of this nature had appeared occasionally in research studies and other commentaries up to this point, it was only after 1973 that evidence based on cross-country averages became very prominent in the literature on the quantity theory on money. Such evidence received particular impetus from McCandless and Weber (1995), a study that used cross-country-average data in plots and regressions alike.

However, the sort of cross-country evidence that Friedman presented in 1973, and that much later research emulated, was something of a trap. Crucially, it was a procedure neglected *time-series* patterns *within* each country. It is perfectly possible for it to be the case that, in each individual country, the time series of monetary growth and inflation are highly correlated—perhaps after allowance for lags—yet that no (or a weak) relationship between monetary growth and inflation appears in a cross-country scatter plot using averages of the two series over the sample period. This could be so even when allowance is made for different values of real output growth across countries, by transforming the monetary growth series to growth in money per unit of output. The reason for this shortcoming of cross-country evidence is that individual countries may have *country-specific velocity trends*, arising, for example, from different rates of technological progress in the banking and payments systems of each country. Consequently, cross-country evidence is not a valid test of the quantity theory relationships because they

[21] Friedman (1973b, pp. 17–18).

impose a common velocity trend across countries (Nelson, 2003, pp. 1036–1037; McCallum and Nelson, 2011, p. 111).[22]

By 1983, Friedman was sufficiently conscious of the empirical importance of this point that he was moved to observe that the monetary growth rate consistent with long-run price stability "will depend on the country and the circumstances," and he also stressed his view that Japan's M2 velocity in the postwar period had a downward trend that partly arose from developments in the country's financial structure.[23]

1974 saw the publication of a brief Friedman article called "Monetary Policy in Developing Countries." As would be expected from its title, this chapter overlapped heavily in content with the *Money and Economic Development* book. Indeed, it was largely an early version of some of the material in that book. Reflecting its earlier origin, the chapter gave Friedman's affiliation as "University of Chicago and the University of Hawaii," in recognition of the fact that it was largely written during his early-1972 spell in Honolulu.[24]

The chapter is particularly notable because of two aspects of its condensed account of Friedman's monetary policy views. First, it opened by reaffirming that "monetary policy is a poor instrument for [active stabilization]… thanks to the length and variability of the lag in the effects of monetary policy [and] the limitations of our knowledge about the factors responsible for such lags."[25] This passage, alongside much other evidence, highlights the fact that Friedman's emphasis on model uncertainty figured in his thinking about monetary policy in the 1970s; contrary to some interpretations, it was not merely an "old Friedman" view characteristic of his work only up to the mid-1960s.[26] Second, a section in the chapter titled "The Rate of Money Creation" provided a rare post-1969 Friedman citation of his 1969 paper "The Optimum

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[22] The cross-country evidence does appear impressive taken as a whole, as the chart in Friedman (1973b) and regressions in McCandless and Webber (1995) demonstrated. But this largely reflects the fact that the country-specific velocity trends described above tend to be numerically small compared with the overall movements of monetary growth and inflation, *provided* that countries with very high average inflation are included among the countries included. When attention is directed to low- or moderate-inflation countries, evidence is mixed on whether monetary growth and inflation have a strong (and near-unitary) relationship when cross-country averages are used. In this connection, De Grauwe and Polan (2005) took the looseness of the monetary-growth/inflation relationship in cross-country averages for such countries that inflation was not a monetary phenomenon. But this conclusion neglected the velocity issue described in the references noted in the text above.
[23] Friedman (1983a, p. 4); see also his related remarks in *Newsweek*, September 4, 1978. For a discussion of Friedman and Schwartz's increasing acceptance of the relationship between financial structure and velocity behavior, see Nelson (2020a, Chapter 8).
[24] Friedman (1974i, p. 265).
[25] Friedman (1974i, p. 265).
[26] See Nelson (2020a, Chapter 7) for an extensive discussion.

Quantity of Money." Here, he characterized the 1969 analysis as consistent with recommending "a roughly stable or slowly declining level of final products"—and thereby greatly downplayed the perceived support of the 1969 analysis for a policy of deflation.[27] Similarly, the following section in Friedman's 1974 chapter, titled "Prescription," endorsed policies of steady monetary growth that kept down, but did not eliminate the private sector's opportunity cost of holding money balances. As with his bottom line in the 1969 paper, therefore, he favored price stability, not deflation.[28] Friedman had progressed from a 1968 remark that the deflation rule "might be better yet" than gearing monetary growth toward price stability, to his remarks in print in 1969, and later, that downplayed the desirability and empirical applicability of the deflation rule.[29]

Friedman and Schwartz reported in 1973 referred to "the draft of the manuscript we are readying on *Monetary Trends in the United States and the United Kingdom*."[30] Their work on this sequel to *Monetary History* and *Monetary Statistics* continued over 1973 and 1974, although progress continued to be hindered by the fact that Friedman was occupied with many other activities. Friedman did find time to give a presentation on the project's work on the United Kingdom at a seminar at the University of Minnesota in October 1974 (see Nelson, 2020b, Chapter 15).

Also over 1973−1974, the *Encyclopaedia Britannica*—which, despite its name, was now being both published and organized principally from the city of Chicago—issued a new set of volumes. A *Newsweek* report (January 21, 1974) on the new edition noted that "a number of big names" had contributed new entries, including Friedman and Nobel Prize-winning chemist Linus Pauling (who, in 1976, would become a prominent critic of Friedman's receipt of the economics Nobel).[31] Although Friedman's entry, titled "Money," attributed authorship merely to "M.Fr.," this was hardly an unsigned article and, as the *Newsweek* coverage indicates, it was always recognized as Friedman's work. Friedman himself acknowledged his authorship of the piece on many occasions—including in the *Financial Times* (January 6, 1977) and in the bibliography his

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[27] Friedman (1974i, p. 276), discussing Friedman (1969a).

[28] See Friedman (1974i, pp. 276–278). See also Friedman (1969a, pp. 47−48) and the discussion in Nelson (2020a, Chapter 7).

[29] The quotation is from Friedman (1968a, p. 16). Woodford's (2009, pp. 38, 39) references to findings of "the desirability of mild deflation (to reduce the opportunity cost of holding money to zero, as called for by Friedman 1969[a])" and to "deflation at the rate called for by Friedman" really refer to the formal analysis presented in Friedman (1969a) rather than to the monetary policy recommendation Friedman actually settled on at the end of that paper. (A rare case of an article in the literature that used the "Friedman rule" terminology yet that explicitly acknowledged that Friedman's post-1969 writings confirmed that he was not actualy an advocate of the deflation rule was Velde and Weber (2000). See their page 1228, in which Velde and Weber noted Friedman's (1990b, 1990c) criticism of the (low-single-digit-average) deflation that the United States experienced during a stretch of the pre-World War I gold standard.

[30] Friedman and Schwartz (1973, p. 50).

[31] See Chapter 7.

office provided to Thygesen (1977, p. 96)—and in Martin (1983, p. 61), he recommended the entry (again identifying himself as its author) as an introductory treatment of monetary issues.

The entry's discussion of the "monetarist" view (a term he used freely in the article) overlapped considerably with text from his published writings from earlier in the decade. A notable feature of the entry was its coverage of international issues. Friedman's discussion treated the exchange-rate system in force in countries other than the United States as one of adjustable but pegged exchange rates, and the examples he cited of exchange-rate adjustment were from 1969.[32] A lot had happened between the time at which the entry was written and its 1974 publication.[33] When he revised the entry for 1986 publication, Friedman was able to include a paragraph on the shift since 1971 from the "dollar standard" to a system of floating exchange rates.[34] The latter stages of this shift are analyzed in Sections II and III of this chapter.

An article titled "A Bias in Current Measures of Economic Growth" in the March/April 1974 issue of the *Journal of Political Economy* proved to be the final article Friedman published in that journal over those years in which he was based at the University of Chicago. One prominent aspect of this article was its discussion of immigration, which Friedman numbered among the United States' "great economic achievements."[35] That description was one of many discussions of immigration that appeared in Friedman's writings and other public statements: for example, on the occasion of formally receiving his Nobel prize in 1976, Friedman remarked, "It should be emphasized that we've [that is, Americans] led [in winning] these awards over the years because of our open immigration policy. [But] now, that's changed—it isn't what it should be." (*Las Vegas Review-Journal*, December 12, 1976.)

The existence of this multitude of discussions of immigration is not what one might be inferred from the consideration of Friedman's views on this matter in Ruger (2011, pp. 136−139). Ruger correctly notes that Friedman indicated over time that he favored free immigration in principle but not in conditions of a welfare state. However, Ruger's earliest Friedman reference to immigration is from 1977, and Ruger implies that, other than in the same 1977 item, Friedman did not discuss the welfare state/migration linkage in public statements prior to 1998. In fact, however, Friedman discussed immigration on numerous occasions before 1977 and also gave

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[32] Friedman (1974f, pp. 351−352).

[33] In *Reason* (June 1975, p. 91), Friedman stated that he had written the entry a "few years" earlier. The most recent reference that the entry cited was Friedman (1971a), and this citation may have been a late addition, as an earlier version of Friedman (1971a), namely Friedman (1970a), was cited on another page. See Friedman (1974f, pp. 355−356).

[34] Friedman (1986a, p. 325).

[35] Friedman (1974g, p. 432).

multiple commentaries before 1998 on the immigration/welfare-state connection.

With regard to his support for immigration, there were many Friedman discussions that highlighted the benefits accrued to the United States from the large-scale migration associated with pre-1914 arrangements—and the benefits to migrants of those arrangements. The 1974 *JPE* discussion was one example. Discussions earlier than this included a column of October 28, 1968, in *Newsweek*, in which Friedman cited migration arrangements prevailing during the period between the Civil War and 1914 as one basis for characterizing that interval as one in which the United States "came about as close to a *laissez-faire*, free-enterprise society as one could hope to observe." Another was his February 1973 *Playboy* interview, in which Friedman said of the period in question, "millions of penniless immigrants came in from abroad, with nothing but their hands, and enjoyed an enormous rise in their standard of living."[36]

Ruger (2011, p. 136) claims that "immigration is basically ignored in his two major works on freedom" (that is, *Capitalism and Freedom* and *Free To Choose*). But in fact the Friedmans' book version of *Free To Choose* opened with three paragraphs on the role of migrants in helping create the "economic miracle" of the United States.[37] And in the television version of *Free To Choose*, Friedman, in the filmed portion of its first episode, likened modern-day immigrants to "the early settlers—they want to better their lot and are prepared to work hard to do so."[38]

In the same year as the broadcast of *Free To Choose*, Friedman noted: "I am in favor of freer migration as I am of freer trade."[39] However, a caveat to Friedman's support for freer migration was that already noted: he regarded fully free migration as untenable in the modern world. The different welfare states of various nations were what made him stop short of advocating wholly free movement of individuals across countries. This was a fairly standard position of many libertarians. But it is not the case, as Ruger (2011) implies, that Friedman before 1998 hardly ever indicated that it was his own position. He articulated and endorsed the position on multiple

[36] See Friedman (1975a, p. 8; 1983b, p. 19). Related Friedman remarks in, for example, Friedman (1976a, pp. 5–6; see also Friedman, 1983b, pp. 62–63), Friedman and Friedman (1980, p. 52), and Instructional Dynamics Economics Cassette Tapes 118 (April 13, 1973) and 214 (May 1977, Part 2). In addition, Friedman (1976b, pp. 162, 237) discussed pre-1914 immigration arrangements, in primarily a positive rather than a normative context, while Friedman (1962a, p. 96) had discussed how, in the nineteenth-century era of very high migration, public education likely was helpful, in promoting common values and a common language.
[37] Friedman and Friedman (1980, p. 1).
[38] *Free To Choose* (U.S. television version), PBS, Episode 1, "The Power of the Market," January 12, 1980, p. 1 of transcript.
[39] Friedman (1981a, p. 25). See also Friedman's September 1979 remarks quoted in Nelson (2009, p. 72).

occasions before 1998—not just in the 1977 talk that Ruger cites.[40] In particular, Friedman explicitly did so in the 1980 *Free To Choose* television series.[41]

Friedman would reaffirm this position on various occasions between 1980 and 1998. For example, in Hinshaw (1988, pp. 107–108), Friedman remarked: "Personally, I believe in free migration, but I believe that you can only have free migration effectively in a world in which you don't have some other things that you do have in the present world; I mean that there's a fundamental conflict between a welfare state and free migration. And so I have to choose a second-best at that point." And in 1996, he stated: "In a welfare state you cannot have, unfortunately, free immigration. I'd like to have free immigration. I am the son of immigrants, of course... I don't have any good way to answer this question... I can't really come out and say that we should have completely free and open immigration."[42]

II. ISSUES RELATED TO DEBATES ON INTERNATIONAL ECONOMIC POLICY AND GEOPOLITICAL DEVELOPMENTS, 1973–1974

THE FIRST OIL SHOCK

Taping an economic commentary on October 4, 1973, Paul Samuelson proclaimed that the runup in commodity prices seen over the previous year had now gone into reverse. "Basic commodity prices have in many cases declined substantially," Samuelson remarked, and he conjectured that, if a general index of these commodity prices existed, that index would register a peak during August and a percent decline of about ten percent since then (Instructional Dynamics Economics Cassette Tape 138 (Paul Samuelson series), October 4, 1973).

Just two days after Samuelson's remarks, however, the die was cast for an event that would tower over the commodity price increases observed in the first eight months of 1973. On

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[40] Ruger (2011, pp. 158, 198) cites the 1977 talk in question, "What Is America?," as though it was unearthed by a 2008 blog. But, although not cited by Ruger as such, "What Is America?" was actually Episode 1 (taped October 3, 1977) of *Milton Friedman Speaks*, a series of Friedman talks in the late 1970s put on limited videotape release in the early 1980s and available commercially in the twenty-first century as a DVD series. The talk in question was identified as being episode 1 of *Milton Friedman Speaks* by Friedman and Friedman (1998, p. 604) and was quoted and cited in Nelson (2009, p. 30). The relevant material on migration appeared early in the *Milton Friedman Speaks* episode in question (pp. 5–7 of transcript).

[41] See *Free To Choose* (U.S. television version), PBS, debate portion of Episode 8, "Who Protects the Worker?," March 1, 1980 (p. 8 of transcript). Reder (1982, pp. 30–31) also noted Friedman's remarks on the matter in this debate.

[42] CSPAN, April 18, 1996 (a talk by Friedman at Claremont College). See also Friedman's remarks in *Forbes*, December 29, 1997, p. 55.

October 6, war broke out between Israel and an alliance of Egypt and Syria. Even in that day's newspapers, published before the war began, it was reported that the oil producers represented by the Organization of Petroleum Exporting Countries (OPEC) were pushing for a 66 percent oil price increase.[43] Subsequently, however, as part of its response to the war, OPEC would preside over the first oil shock—which would, in total, amount to a roughly fourfold increase in the world price of oil in 1973−1974.

This shock set the oil price apart from other commodity prices. For, as Samuelson correctly observed, by October 1973 certain commodity prices were retracing some of their increases from the earlier year. Although they would show strength during 1974, prices for commodities other than energy would record an absolute decline as the 1973−1975 recession proceeded.[44] An International Monetary Fund series on nonfuel commodity prices in industrial countries registered an index value of 40.7 in 1972, 58.1 in 1973, and 75.8 in 1974, but a fall to 67.8 in 1975 (International Monetary Fund, 1986, p. 173). In contrast, the world oil price rose every year, averaging $1.90 per barrel in 1972, $2.70 in 1973, $9.76 in 1974, and $10.72 in 1975 (International Monetary Fund, 1986, p. 171).[45]

This behavior was testament to the fact that the oil price increase proved permanent—remaining in force for years after the weeks-long 1973 Middle East war had ended. The second oil shock of the late 1970s would then build on the permanent oil price increase of the first oil shock and lead to an average oil price in 1980 of $28.67.

The 1973−1974 OPEC oil shock features very prominently in many narratives of U.S. macroeconomic developments in the first half of the 1970s. For Milton Friedman, however, the emphasis placed on the shock was largely misplaced. He did increasingly recognize the importance of the oil shock as a major event that altered the economy's supply potential. But Friedman deplored the tendency for the OPEC shock to become a near-catch-all explanation for the deterioration in macroeconomic performance from the 1960s to the 1970s.

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[43] See the previous chapter.
[44] A general index of commodity prices, the U.S. producer price index for all commodities (available in the Federal Reserve Bank of St. Louis' FRED at *https://fred.stlouisfed.org/series/PPIACO*), registered an increase of only 4.2 percent in the year to August 1972 but an increase of 18.5 percent in the year to August 1973. Reflecting the softening to which Samuelson referred, the twelve-month rate of increase then fell to 15.4 percent in the year to November 1973, but, with the impetus of higher oil prices, it rose to 23.4 percent in the year to November 1974.
[45] The reported price refers to Saudi Arabian oil. The spot price of West Texas Intermediate oil (available in the Federal Reserve Bank of St. Louis' FRED portal at *https://fred.stlouisfed.org/series/WTISPLC*) shows a higher initial price as well as a less steep initial increase. Its annual averages are $3.56 in 1972, $3.87 in 1973, $10.37 in 1974, and $11.16 in 1975.

Friedman's analysis of the oil shock can be partitioned into (*i*) his analysis of the origin of the shock, in particular the power and role of the cartel; (*ii*) what he regarded as the shock's implications for output and potential output; (*iii*) his analysis of its effect on inflation; and (*iv*) his view on how U.S. energy policy responded to the shock. Items (*i*) to (*iii*) are analyzed in this section. Item (*iv*) is discussed in the discussion titled "William Nordhaus" in Section III below, as well as in later chapters.

Friedman's analysis of the OPEC cartel and the origin of the oil shock

Increases in the price of energy and talk of an "energy crisis" had taken place over the course of 1973 even before OPEC's October moves.[46] Early in the third quarter of 1973, Friedman referred to what he called the "bubble in raw materials prices," including the oil price (Instructional Dynamics Economics Cassette Tape 125, July 18, 1973). The oil price increase that had been recorded was, he felt, of a piece with the rises in the prices of other commodities, including gold. In his view, these increases partly reflected the world boom and other fundamental market conditions but were considerably reinforced by "speculative fever" (*Newsweek*, September 17, 1973). However, even at this stage, Friedman did cite a role for OPEC in producing the increase. "At the moment," he testified in June 1973, "the reason why the oil price is so high is because, with the assistance of unwise policies on the part of the United States, there has been formed an international selling cartel, the OPEC countries…"[47]

The increases in energy prices that occurred before the fourth quarter of 1973 would, however, appear minor when judged against the steep rise imposed by OPEC in its actions from October 1973 onward. Friedman, like Samuelson, was caught off-guard by the increase: for example, in June 1973 he had suggested that oil purchasers in Japan were being imprudent in making long-term contracts on the basis of the present oil price.[48] In the event, of course, contracts that locked in pre-October prices proved highly beneficial to the purchasers.

The energy price increases associated with the first oil shock occurred against the background of continuing U.S. wage-price controls, including on retail gasoline prices. The controls did not

[46] For example, on May 17, 1973, the Hong Kong newspaper *South China Morning Post* editorialized that "fears of an energy crisis have made headlines in the American press for some months." It added prophetically that "the world has up to now taken lightly the prospects of an oil famine on any major scale" and noted that "there is an increasing tendency among the Arab oil producers to exploit their stranglehold of the market as a political weapon against Israel and those countries which support her."
[47] From Friedman's testimony of June 21, 1973, in Joint Economic Committee (1973a, p. 139).
[48] Again, see Friedman's testimony of June 21, 1973, in Joint Economic Committee (1973a, p. 139).

prevent a sizable pass-through of the higher oil prices to prices at the pump. But a complete pass-through from world oil prices to U.S. retail prices was prevented by the general wage-price controls and then, in 1974–1981, by price controls applying specifically to retail gasoline prices.

The U.S. policy reaction to the higher oil price would generate trenchant criticism from Friedman for the rest of the 1970s. At the same time, however, his reaction to the OPEC measures helped establish his reputation as someone who downplayed the implications, in terms of both scale and duration, of the oil shock. The appropriate response, he said in November 1973, was to drop the price controls and "simply allow prices to rise… A 10 to 15 percent [energy] price rise will eliminate the shortage as a serious problem." (*Lima News*, November 21, 1973.)[49]

As the OPEC shock in fact led to an enduring manifold increase in energy prices, such proclamations appear wildly off-base with the benefit of hindsight. The late-1973 statements by Friedman are not, however, quite as indefensible as might be thought, because a distinction has to be made between the Arab oil producers' *embargo* and the OPEC price rise. The valid element of Friedman's analysis was that the embargo—a sellers' embargo, with key Middle East oil producers ruling out export of its product to the United States and some other countries—that was superimposed on the oil price hike did not provide lasting obstacles to oil supplies in the United States. Oil could find its way to the United States not only from non-OPEC countries but also by rerouting of oil from countries (such as the United Kingdom) not subject to the embargo. Indeed, the embargo was lifted at the end of the first quarter of 1974 (*Federal Reserve Bulletin*, April 1974, p. 240), in part because OPEC recognized it had been ineffective in blocking oil supplies to the United States. Friedman was thus not wrong to suggest that a shortage—in the sense of unavailability of petroleum products to U.S. customers—might not be a major problem and might be eliminated by modest price adjustment.

But the short-lived nature of the Arab oil producers' embargo would only underscore the fact that a far more important aspect of the OPEC-related measures was the price increase that the oil-exporting countries achieved via their restriction on amounts offered to the world market. It was on this price increase that Friedman's record as a commentator and forecaster was truly checkered. He expected OPEC to crumble—and to do so quite quickly—and over the mid-1970s he accumulated a catalogue of failed predictions to this effect.

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[49] Similar observations by Friedman appeared in *Newsweek*, November 19, 1973, and in Instructional Dynamics Economics Cassette Tape 134 (November 21, 1973).

A sampling of predictions Friedman made in this vein in late 1973 through Spring 1975:

- In his June 1973 Congressional testimony, in response to the early OPEC price increases, Friedman said: "The cartel has fixed a price which is a multiple of the cost of producing oil in their countries. As a result, there is going to be tremendous pressure for that cartel to fall apart."[50]

- In November 1973, Friedman stated categorically (Instructional Dynamics Economics Cassette Tape 134, November 21, 1973): "I predict without fear of successful contradiction that within three years—no more—we're going to have oil coming out of our ears, we're going to have a collapse in the world price of oil, and I may say it couldn't happen to a better bunch of people [OPEC]."[51]

- In February 1974, he stated that the "oil price cannot stay at $10 a barrel; it will drop drastically within the next six or nine months" (*University of Chicago Magazine*, Autumn 1974, p. 12).[52]

- In March 1974, Friedman said that—just as the surge of meat prices in early 1973 had now faded from the public's memory—so in a year's time the talk of the oil crisis would be a thing of the past.[53]

- In his cassette commentary the following month (Instructional Dynamics Economics Cassette Tape 143, April 3, 1974), Friedman predicted that the approaching period would be one in which oil price "comes sharply down."

- In September 1974 at the White House Economists' Conference on Inflation, Friedman again predicted OPEC's collapse (Council of Economic Advisers, 1974, p. 182).

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[50] From Friedman's testimony of June 21, 1973, in Joint Economic Committee (1973a, p. 139).
[51] Other statements from around this time in which Friedman also predicted oil coming "out of our ears" were in *Philadelphia Evening Bulletin*, November 9, 1973, and *Chicago Tribune*, November 21, 1973. The latter statement gave the timeframe over which OPEC would collapse as three to four years.
[52] In Friedman, Teece, and Griffin (1982, p. 58), Friedman stated: "I was wrong about my prediction in 1974… [but] I did not predict imminent collapse but simply that the OPEC countries would not be able to maintain the cartel for long without specifying what 'long' was." Although this characterization may be correct of Friedman's 1974 *Newsweek* analysis, it does not apply to Friedman's public remarks in 1973 and 1974 *in toto*. Although Friedman was not highly specific regarding the *point* in time at which the cartel would break down—in his January 1974 panel appearance (*Long-Term Energy Solutions*), for example, he remarked, "I don't know when it's going to fall apart"—his observations did specify a *time-frame* over which the cartel would break down.
[53] From Friedman's remarks of March 14, 1974 (Friedman, 1974k, p. 11).

- In an early-1975 column (*Newsweek*, February 17, 1975), Friedman predicted that President Ford would be able to include the collapse of OPEC among the favorable developments that had occurred in his tenure by the time he faced the November 1976 presidential election.[54]

- On Australian television in April 1975, Friedman reaffirmed that OPEC would break down—"we're going to see that happen in the next year." (*Monday Conference*, Australian Broadcasting Commission, April 14, 1975, p. 21 of transcript.)

- In Tokyo a week later, he predicted that this collapse would be accompanied by a fall in the oil price to below its real 1973 level (*The Age* (Melbourne), April 23, 1975).

And, even in early 1976, Friedman was maintaining that the "oil cartel will break pretty soon" (quoted in Feldberg, Jowell, and Mulholland, 1976, p. 46).

In mid-1979, with the original oil price increase of 1973−1974 in the process of being succeeded by a second oil shock, Paul Samuelson (*Newsweek*, July 2, 1979) obviously had Friedman in mind when he wrote: "It is only too easy to poke fun. Nor have economists failed to provide their own quota of laughs. Only remember those pundits who predicted the imminent collapse of the OPEC monopoly on the basis of no more than the syllogism that all past cartels have been mortal." In light of remarks like Samuelson's, the London *Financial Times* editorialized in late 1985 (December 14, 1985) that Friedman "has had to put up a great deal of ribbing from his academic colleagues over the years" about his predictions concerning OPEC.[55] The ribbing extended beyond academia: in the mid-1970s, Friedman received a spoof award from the Promotion for Humor in International Affairs for his erroneous predictions concerning oil prices and the durability of OPEC (as he would recount in his columns in *Newsweek* of September 15, 1980, and March 21, 1983).

Friedman had indeed cited this property of cartels as one reason for his confidence that OPEC would collapse: "OPEC is a cartel, and cartels inevitably break up sooner or later," Friedman

[54] Bruno and Sachs (1985, p. 195) cited this column and suggested that Friedman's prediction was influenced by the "temporary and wholly reversed" non-oil commodity price boom of 1973−1975. However, as we have seen, Friedman's predictions of OPEC's collapse predated the winding-back of the non-oil commodity price boom—and, indeed, preceded the first oil shock period of October 1973 onward.
[55] The editorial made this observation before noting that current oil-price movements seemed to be finally bearing out Friedman's predictions. During the same period in which this *Financial Times* piece appeared—and reflecting the more-protracted publication process of research journals than of newspapers—a contribution to the December 1985 issue of the *American Economic Review* quoted Friedman's *Newsweek* columns of past years as evidence that "Friedman's predictions [have] proven so wide of the mark" with regard to OPEC (Griffin, 1985, p. 962).

had observed at the outset of the first oil shock (*The Evening Bulletin* (Philadelphia), November 9, 1973).[56] On this occasion, he cited the source of the dissolution of a cartel the likelihood that there would be "a chiseler in the bunch" among the carter members. This sentiment echoed Friedman's response to the oil price increases earlier in 1973: "There have been many international cartels… They last for a little while, but sooner or later there gets to be a chiseler, there is always one in every bunch."[57] Over a decade earlier, Friedman had also expressed this sentiment about cartels in *Capitalism and Freedom*.[58]

But contrary to the characterization of them in Samuelson's 1979 remarks, Friedman's predictions had not been based wholly on a generalization about cartels. They had also on rested on what he perceived to features of the world oil market. And for his initial reaction to the OPEC actions, Friedman had actually put heavy weight on the analysis of someone Samuelson acknowledged as an expert on the market: Samuelson's Massachusetts Institute of Technology colleague Morris Adelman. Adelman's analysis emphasized the absence of a physical shortage of oil supplies or reserves in the world, and Friedman relied heavily on this point. Adelman and Friedman also found fault with the United States policy actions for strengthening OPEC and making it an effective cartel.[59] The overlaps and divergences of Friedman's and Adelman's analysis are discussed later in this chapter (in the course of the discussion titled "William Nordhaus" in Section III).

Acknowledging a crisis

Over time, Friedman came to accept the gravity of the problem created by the first oil shock. This acknowledgment was exemplified by his acquiescence to the use of the terms "oil crisis" or "energy crisis." In the first half of November 1973, Friedman tried to avoid referring to an "energy crisis"—maintaining that it was more accurate to refer to "shortages created by the [U.S.] government and exacerbated by the war in the Middle East" (*The Evening Bulletin* (Philadelphia) November 9, 1973). However, he used the term "fuel crisis" on a tape later in the month (Instructional Dynamics Economics Cassette Tape 134, November 21, 1973), while his *Newsweek* column of November 19 referred to the "current oil crisis" and his column of December 10 opened with a reference to "the energy and oil crisis."

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[56] Likewise, on television three years later, Friedman observed, "That cartel has to break, like all other such cartels have broken in the past." (*Wall Street Week*, Maryland Public Television, November 5, 1976, p. 15 of transcript.)
[57] From Friedman's testimony of June 21, 1973, in Joint Economic Committee (1973a, p. 139).
[58] Friedman (1962a, p. 131).
[59] See, for example, Friedman (1977e, p. 8), *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978 (p. 11 of transcript). In the case of Adelman, see, for example, Adelman (1972, p. 254) and Adelman's testimony of January 29, 1975, in Committee on Foreign Relations, U.S. Senate (1975, p. 3).

Nevertheless, Friedman's continuing discomfort with the "crisis" label would be reflected in his remark (in *The Age*, April 23, 1975) that it would be a great mistake to view the world as facing a major energy crisis, as well as in his occasional subsequent indications that he regarded the "crisis" label as applying to the initial 1973–1974 disruptions, including the embargo, rather than describing a continuing state of affairs.[60] But, as time went on, Friedman increasingly embraced both the terms "energy crisis" and "oil crisis."[61] For example, he titled a 1978 talk "The Energy Crisis," using it to refer to the whole post-1973 period.[62]

Later refinements and defenses of Friedman's position

As his emphasis on the ampleness of international oil supplies indicated, Friedman believed that a restriction on the part of the supplier—that is, oil being withheld from sale on the world market—rather than problems of a global physical shortage of oil was what was behind the oil price increase.[63] He would come to see the first oil shock of 1973–1974 as a milestone on the supply side of the oil market—marking the occasion when OPEC's apparatus became effective for the first time, so the "rise in the price of oil [was] engineered by the OPEC cartel."[64] Friedman would trace the strength of OPEC in part to U.S. historical diplomatic decisions that had, as he saw it, ratified the creation of the cartel and strengthened its market power.[65]

The resilience of the cartel and the associated energy-price increase would lead Friedman to modify and qualify his original position on the oil shock.[66] Partially retracting his 1973–1975 predictions that OPEC would not have the discipline to restrict supply for a prolonged period (such as in *The Age* (Melbourne), April 23, 1975, and other items noted above), Friedman would grant that it had done exactly that. Indeed, by early 1975 Friedman was conceding that there would be some merit in imposing a tariff on imported oil to "hasten the disintegration of the

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[60] See Friedman (1977d, p. 17).
[61] See, for example, Friedman (1975a, Chapter 15; 1977c, p. 463) for "oil crisis" and Friedman (1980a, p. 56) (as well as *Newsweek*, July 14, 1975), for "energy crisis."
[62] See *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution" (taped February 10, 1978) and Friedman (1983d).
[63] Barsky and Kilian (2001) would later argue that the OPEC price increase was largely a validation of pressures on the demand side of the market. Friedman was sympathetic in other contexts, such as that of the labor market, to seeing apparent monopolistic power on prices as actually reflecting demand forces. But he did not see the first oil shock as produced by demand pressures. (He also discounted the role of *aggregate* demand in producing the oil price rise: he contended that the oil price rise was far too large in percentage terms for the bulk of it to be accounted for by monetary policy actions in the United States and other countries, and he declared attempts to do so statistically and economically unsound—see *Wall Street Journal*, August 28, 1978.) For a rebuttal to the Barsky-Kilian emphasis on the demand for oil as a prime cause of the first oil shock, see Hamilton (2003).
[64] See Friedman (1990a, p. 51). The quotation is from Friedman and Friedman (1980, p. 19).
[65] As well as the items cited above, see Friedman's *Newsweek* columns of May 2, 1977, and June 18, 1979.
[66] For example, in Friedman (1983d, p. 146).

OPEC cartel" (*Newsweek*, January 27, 1975*a*, p. 24), were it not for the fact that President Ford's proposal for that tariff came as part of a package that included measures that would discourage domestic oil production (*Newsweek*, January 27, 1975*a*; Instructional Dynamics Economics Cassette Tape 168, June 1975, Part 1).

For the most part, however, Friedman was willing to defend his initial economic analysis of the oil shock as sound (see *Newsweek*, September 15, 1980, and March 21, 1983). As his references in 1973 to a breakup occurring within three to four years attest, he did not see the pressures tending to undermine OPEC as acting instantaneously—although he was acknowledging by early 1975 that the forces were acting more slowly than he expected and that he had been wrong about the speed at which OPEC would collapse (Instructional Dynamics Economics Cassette Tape 165, February 1975, Part 3). Rather, he saw them as cumulating over time, in conformity with the property that short-run elasticities of demand and supply were much smaller than long-run elasticities.[67] Thus, "the short-run demand curve facing the OPEC cartel was extremely inelastic," but the long-run demand curve was not.[68] In the case of the response to the oil shock, Friedman saw the forces making for the sending of appropriate price signals to both consumers and producers as having been greatly slowed down by highly counterproductive U.S. government policies regarding energy (see Section III below, as well as the following chapters).

Macroeconomic implications: the business cycle

In the mid-1980s, the Friedmans would cite the first oil shock as a factor that pushed up the unemployment rate.[69] This interpretation of the oil shock was consistent with his original reaction in 1973, when he granted that the OPEC action increased the likelihood of recession (Instructional Dynamics Economics Cassette Tape 134, November 21, 1973). Subsequent evidence by Bernanke, Gertler, and Watson (1997, p. 121) supported Friedman's notion that the recession of 1973−1975 likely would have occurred, due to tight monetary policy, even in the absence of the oil shock. Indeed, these authors suggested that the oil shock by itself would not have produced a recession had monetary policy not tightened in 1973−1974.[70]

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[67] See Friedman (1975a, p. 307) and *Newsweek*, March 31, 1975. See also Friedman (1990a, p. 52).
[68] The quotation is from the discussion in Friedman (1976b, p. 160), which referred to then-current 1975 events.
[69] Friedman and Friedman (1985, p. 106). See also Friedman (1977c, p. 463).
[70] See also Bernanke (2003a). Paul Samuelson was likewise a proponent of the view that U.S. aggregate demand policy had produced the recession, remarking: "I have said again and again that this recession was made in Washington." (Instructional Dynamics Economics Cassette (Paul Samuelson Series), Tape 168, December 2, 1974.)

Macroeconomic implications: potential output

On the economic-modeling front, the first oil shock concentrated the minds of economists in a way that made them bring variations in aggregate supply, or potential output, more routinely into modeling. Paul Samuelson (1974, p. 77) observed at the end of 1973 that "our Fisher-Keynes macro models do not tell us how to handle such a microeconomic restriction on supply and productivity."[71] The stage was therefore set for formal aggregate demand/aggregate supply analysis, as crystallized in macroeconomics texts late in the decade by Dornbusch and Fischer (1978) and Gordon (1978).

Ahead of this formalization, the oil shock was realized from an early stage as a force that reduced potential GDP. For example, testifying in early 1974, Federal Reserve Chairman Arthur Burns, said that "our capacity to produce may actually decline in 1974, or at best rise at an abnormally low rate" and referred to the reduction in production capacity arising from "the sudden and very serious oil shortage."[72]

Notwithstanding this early reaction, official estimates of potential output in the mid-1970s were badly overestimated in the year. One problem was that the pre-oil shock, official estimates of the level of U.S. potential output were highly excessive, as sharp downward revisions in 1977 would make clear (Wonnacott and Wonnacott, 1979, p. 333).[73] A further problem was that, although the 1973−1974 oil shock was recognized early on as a supply shock, it occurred roughly alongside a sharp slowdown in long-term productivity growth. So even estimates that incorporated major downward revisions of the estimated level of U.S. potential output soon implied serious overstatements of aggregate supply, because they missed the emerging decline in the growth rate of potential.

Friedman himself would note the supply-side effects of the oil shock: for example, his Nobel lecture included the observation that the oil crisis "directly disrupted the productive process."[74]

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[71] Likewise, Robert Solow (in Klamer, 1984, p. 134) argued that the economics profession's mainstream model lacked the requisite aggregate-supply analysis at this time, but he contended that it took only a few months over 1974 for the necessary aggregate-supply analysis to become integrated into the mainstream analytical apparatus of economists.
[72] From Burns' February 26, 1974, testimony, in Joint Economic Committee (1974a, pp. 725, 748).
[73] An early judgment that overestimates of U.S. potential output had generated inflationary policy mistakes during the 1970−1973 period was articulated by William C. Freund of the New York Stock Exchange: "Economists share with the federal government responsibility for misjudging our economy's inflation potential. Too much reliance was placed on statistics purporting to show a plentiful supply of industrial capacity and labor. We [now] know how misleading these indicators proved to be..." (*Detroit Free Press*, July 15, 1974, p. 8B.)
[74] Friedman (1977c, p. 463).

In his own quantifying of the importance of the oil shock for the behavior of potential output, Friedman was inclined to view it as a terms-of-trade shock—one that meant more output had to be shipped abroad rather than contribute to meeting U.S. aggregate demand.[75] Accordingly, when he and Anna Schwartz constructed an estimate of the first oil shock on potential output, they arrived at a value of 1.5 percent, which corresponded to the extra share of U.S. aggregate output absorbed by the higher energy cost after the energy price increase.[76]

In late 1973, before the extent of the oil price rise and the U.S. economy's response to it were clear, Friedman had estimated the decline in potential output at about 0.4 percent. He was therefore appalled at what he viewed as a huge overreaction by the stock market to the oil shock. "The stock market has been saying a reduction of four-tenths of 1 percent [of GNP] over the next two or three years reduces the value of American enterprise by 10 percent," Friedman observed. It makes no sense." (*The Evening Bulletin* (Philadelphia), November 20, 1973, p. 22.)[77]

Friedman's assessment that the stock market had grossly overreacted to the oil shock remained even after his estimate of the decline in potential had been raised to 1.5 percent.[78] Together with the behavior of the gold market, the stock market slump in the mid-1970s greatly enhanced his preexisting skepticism about the reliability of the links between economic fundamentals and the pricing of equities and commodities.

Macroeconomic implications: the price level and inflation

The 1.5 percent estimated decrease in potential output, as far as Friedman was concerned, also answered the question of how much the OPEC shock raised the price level. By the time of the

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[75] See, for example, Friedman and Friedman (1980, p. 263) and Instructional Dynamics Economics Cassette Tape 173 (August 1975, Part 1).

[76] Friedman and Schwartz (1982, p. 104) did not source the 1.5 percent number, but Friedman mentioned a range of 1 to 1.5 percent in Instructional Dynamics Economics Cassette Tape 155 (October 10, 1974), and he described 1.5 percent as his estimate in Friedman (1977d, p. 17). The 1.5 percent estimate appeared also in other economists' discussions of the OPEC shock in the mid-1970s. For example, at a March 1975 conference (as recorded in Hinshaw, 1977, p. 102), Wilson E. Schmidt (of Virginia Polytechnic) foreshadowed the Friedman-Schwartz position by stating: "A monetarist would say that there is only one real impact—namely, the adverse effect on the terms of trade. Between October 1973 and October 1974, the terms-of-trade cost for the industrial countries was only about 1.4 percent of real GNP, or something like that; the figure for the United States was 1.5 percent." At the same conference, Robert Solomon considered related reasons for the 1.5 percent number (p. 102).

[77] See also Friedman's remarks on the excessive reaction of equity prices in Instructional Dynamics Economics Cassette Tape 135 (December 4, 1973) and Instructional Dynamics Economics Cassette Tape 137 (June 26, 1974). Friedman also discussed recent years' equity price slump in *Black Enterprise* (June 1974), p. 114.

[78] In retrospect, a major stock-market correction might seem justified by other factors occurring around this time, most notably the post-1973 productivity slowdown. However, it seems implausible to attribute to stock-market participants a sound understanding of this slowdown when policymakers and economic modelers only seem to have truly grasped its existence and scale at a later point—in the late 1970s and early 1980s.

OPEC shock, he had already spent months refuting the idea that various commodity price increases, including those pertaining to meat, cereals, and anchovies, had played a large role in producing inflation. Friedman would reiterate these criticisms in December 1974, when he deplored blaming inflation on natural catastrophes (such as crop failures) that had lowered agricultural production.[79] Indeed, his terms-of-trade-oriented view of commodity shocks' macroeconomic implications led Friedman to suggest that increases in agricultural prices might be *disinflationary* for the United States beyond the short run: as the country was a net exporter of agricultural goods, agricultural price shocks meant the U.S. exports could pay for a higher volume of U.S. imports than previously (Instructional Dynamics Economic Cassette Tape 129, September 13, 1973; *University of Chicago Round Table: The Nation's Economy Out of Control*, PBS, May 1, 1974).[80]

The oil price increase fell into a somewhat different analytical category because, by 1973, the United States was a net oil importer. Therefore, the OPEC oil shock worsened the U.S. terms of trade and so, on Friedman's logic, could make the country's price level higher for a given stock of money.[81] However, as already indicated, Friedman would rate the decline in potential output resulting from the oil shock as 1.5 percent: substantial, but not large in relation to ongoing inflation rates in the 1970s. Through this prism, it followed that the decline in potential output had tended to raise nominal money per unit of output by 1.5 percent and put up prices by the same amount.[82] The oil shock in Friedman's framework therefore had a permanent, but quite limited, long-run effect on the price level. This long-run effect had no implications beyond the

[79] Friedman (1975b, p. 699).
[80] Friedman also made this argument in a letter to Federal Reserve Chairman Burns on October 1, 1973 (Burns Papers, Gerald Ford Presidential Library). He made the further point in that letter that a country under a fixed exchange rate might not see its terms-of-trade improvement realized in a lower price level than otherwise, as that improvement might also be associated with a balance-of-payments surplus and so upward pressure on the country's money stock. With floating rates newly prevalent, a terms-of-trade improvement was, he argued, now more likely to be a source of downward pressure, on net, on a country's price level. The applicability of this analysis to the United States is limited by the insensitivity of U.S. monetary conditions to its balance-of-payments position even under fixed exchange rates; but the analysis is likely highly relevant for other countries. In this connection, it is notable that Gruen and Dwyer (1995) found that terms-of-trade improvements boosted inflation in Australia under fixed exchange rates, but not under floating.
[81] See, for example, *Milton Friedman Speaks*, Episode 6, "Money and Inflation," taped November 7, 1977, p. 36 of transcript.
[82] An assessment based on the same principles was Karnosky (1976, p. 21). Upon finding that as the level of prices in 1974 exceeded the value implied by its estimated historical relationship with the money stock by 4.5 percent, Karnosky suggested that the decline in potential output for 1974 should be inferred as having been 4.5 percent. Karnosky's high estimate of the potential-output decline might, however, have reflected a confounding of two nonmonetary influences on the price level: *bona fide* supply shocks (such as oil) that appreciably lowered potential output and relative-price shocks (food price shifts, for example) that influence the price level in the short run but may not have major ramifications for potential output (and, in Friedman's analysis, might even have had a small positive effect on U.S. potential output).

very short run for the time derivative of the price level. As a corollary, the oil shock, Friedman would note on multiple occasions, "did not produce any longer-lasting effect on the rate of inflation."[83]

A passage on commodity shocks and inflation in a Friedman *Newsweek* column of June 1974 was much cited and quoted in later years.[84] In this discussion, Friedman took issue with the notion that the energy shock meant an upward shift in the ongoing U.S. inflation rate. In challenging such cost-push interpretations, Friedman countered (*Newsweek*, June 24, 1974): "The special conditions that drove up the prices of oil and food required purchasers to spend more on them, leaving less to spend on other items. Did that not force other prices to go down or to rise less rapidly than otherwise? Why should the *average* level of all prices be affected significantly by changes in the prices of some things relative to others?" He immediately added: "Thanks to delays in adjustment, the rapid rises in oil and food prices may have temporarily raised the rate of inflation somewhat."[85] This qualification, however, did not undercut Friedman's bottom line that, for given monetary growth, and taking potential output *growth* as unchanged by the oil shock, the oil shock could not raise the ongoing rate of inflation. Inflation remained a monetary phenomenon when oil shocks were part of the economic environment.[86]

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[83] See Friedman and Friedman (1980, p. 263) and Friedman (1992a, p. 204). This formulation left open the question of how to treat a situation in which a country's terms of trade deteriorates but its volumes of exports and imports remain unchanged. Friedman (1974l, p. 14) himself seemed to recognize this ambiguity when he suggested that an increase in U.S. net exports might be a source of upward pressure on the price level, by reducing the goods available to U.S. residents for a given flow of U.S. aggregate nominal income. One clear-cut way of incorporating the idea that terms-of-trade shocks matter for the price level, without the reaction of net exports having much bearing on the matter, is to put imports into the production function as an intermediate good—in which case terms-of-trade shocks matter directly for the behavior of potential output.

[84] See, for example, Argy (1985, p. 70) and Ball and Mankiw (1995, pp. 161–162). (In many of the citations over the past 25 years, the passage is slightly misquoted and the year and title of the column are given incorrectly.) Of course, this column, though providing a convenient capsulization of Friedman's position, was one of many occasions on which Friedman stressed the notion that, for given monetary policy, a shock to one price tended to provoke downward pressure on other prices. For some examples, see Nelson (2020a) and the previous chapter.

[85] This qualification indicates that the interpretation by Ball and Mankiw (1995, p. 162) of Friedman's column as implicitly assuming a flexible-price view of the world is not appropriate. Indeed, although presented as a *contrast* to Friedman's position on the oil price's implications for inflation, their model can be regarded as a *formalization* of Friedman's notion that the oil shock has a temporary influence on inflation. Note also that while Blinder and Rudd (2013, p. 153) take this qualification in the June 1974 *Newsweek* column as one that previous discussions of Friedman's remarks neglect—and indeed state categorically, but quite incorrectly, that it "is never quoted"—the sentence in question was actually quoted in Humphrey's (1976, p. 83) discussion of the column; this Humphrey article also appeared in the United States monetary literature (see Humphrey, 1977, p. 6; 1986, p. 22) and was included in the collection *Federal Reserve Readings on Inflation* (Federal Reserve Bank of New York, 1979) In addition, Nelson (2005) contained a quotation from and discussion of a passage in Friedman and Friedman (1985, pp. 83–84) that was essentially an elaboration of the points (including the qualifying sentence) expressed in the 1974 Friedman *Newsweek* column.

[86] Among the occasions when Friedman stressed this point were his commentary in Instructional Dynamics Economics Cassette Tape 139 (February 4, 1974), in which he contrasted the situation in the "longer period, [when] these special factors ought to affect the relative prices of food and fuels," in the short run, due to "inertia in other

This position contrasted with that held at the time by many academic economists and economic policymakers. The relative-price/aggregate-price distinction had, of course, been a key part of Friedman's disputation with cost-push arguments over the decades, including in his exchange-rate essay in the 1950s.[87] It had also figured in his 1970−1972 arguments with Burns' conversion to the cost-push view. However, that argument had largely pertained to wage-push explanations of inflation—and so it had centered on the endogeneity of wages and on the relationship between prices and costs, rather than on the relationship between particular prices and the general price level.[88] With the non-oil commodity shocks of early 1973, the last relationship became more prominent in the debate, and this became even more so with the unfolding of the OPEC oil shock from late 1973 onward.

After the OPEC shock, Burns—whose 1973−1974 public sparring with Friedman on the causes of inflation was discussed in the previous chapter—testified: "Rapidly rising prices of food and fuel, in fact, have accounted for a large part of our recent inflationary problem."[89] Burns, like many others, believed that the oil price shock was almost bound to leave a permanent mark on inflation due to the higher cost of living being embedded in wage contracts—a process that (according to cost-push views, with their emphasis on wage-price spirals) would lock in a higher rate of price inflation. This position underlay Burns' remark that a "more fundamental factor affecting the course of inflation in 1974, however, may well be the course of wages and unit labor costs."[90]

From the late 1970s, however, there emerged widespread acceptance of the monetary explanation for inflation in the economics profession and policymaking. This change in thinking led mainstream economic research and analysis in policymaking agencies to adopt frameworks consistent with Friedman's position that oil shocks could not, in fact, give rise to ongoing

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prices," the special factors led both to higher inflation and to a higher absolute price level than would prevail in a flexible-price environment.

[87] See Friedman (1953a, pp. 180−181).

[88] The rejection by exponents of the monetary view of inflation of both the wage-push and the special-factors perspectives on inflation was also emphasized around this time by Anna Schwartz (1973). She argued (p. 262) that "[n]o better response" to such perspectives had been given than by Irving Fisher (1911a), and she quoted him at length to this effect, as well as spotlighting (see Schwartz, 1973, p. 259) his 1911 debate with the University of Chicago's James Laurence Laughlin on the matter (Laughlin, 1911; Fisher, 1911b). Friedman, after being alerted by Schwartz to this 1911 exchange, gave it prominence in Friedman (1972b). Much later, Friedman highlighted Laughlin's cost-push views when he wrote the entry on Laughlin for the *New Palgrave* dictionary of economics (see Friedman, 1987c). (In the period since a version of this chapter was first made public, Dimand, 2020, has also discussed Friedman's coverage of Laughlin. In a highly jarring omission, Dimand does not even mention Friedman's *New Palgrave* article that was concerned specifically with Laughlin.)

[89] From Burns' February 26, 1974, testimony, in Joint Economic Committee (1974a, p. 720).

[90] From Burns' February 26, 1974, testimony, in Joint Economic Committee (1974a, p. 723).

inflation (absent monetary accommodation of the shocks—a topic discussed presently).

Nevertheless, the fact that oil-based explanations of inflation were so prevalent during the 1970s, combined with the continuing visceral appeal of accounts of inflation that refer to specific prices, has meant that even twenty-first century retrospectives on the 1970s inflation have often had to confront the energy-oriented explanations and explain their inferiority to monetary accounts. For example, in opening their classic paper, Clarida, Galí, and Gertler (2000, p. 474) felt obliged to make the point that "while jumps in the price of oil might help explain transitory periods of sharp increases in the general price level, it is not clear how they alone could explain persistent high inflation in the absence of an accommodating monetary policy." This was a point already entrenched in textbook analysis (see, for example, Dornbusch and Fischer, 1994, pp. 234–235, 533–534), but the idea that higher oil prices were bound up with permanently higher inflation died hard.

The durability of oil-push ideas was also highlighted by Bernanke's (2003b) remark, after he had articulated and endorsed the mainstream view that inflation in the 1970s was due to monetary forces: "You may have noticed that I have discussed the Great Inflation of the 1970s with an emphasis on Federal Reserve behavior but without mentioning oil prices." He then spelled out why he had not mentioned them. That Bernanke included this explanation underlined the fact that, while the monetarist rejection of the cost-push view had gone from being a dissenting position in the mid-1970s to conventional wisdom among economists in later decades, even in the later period it still clashed with popular views of what drove inflation.

Reconciling Friedman's monetary explanation and Blinder's "Anatomy"

In his study "The Anatomy of Double-Digit Inflation in the 1970s," Alan Blinder (1982) did not cite Friedman's work. Blinder nevertheless implicitly challenged that work when, in a section of the paper titled "The Inflationary Bulge of 1973–75," he stated (p. 264): "The reasons for this performance can be summarized in three words, none of which is 'money': food, energy, and decontrol." An examination of Blinder's (1982) analysis indicates, however, that it was not really inconsistent with monetary explanation for the 1970s inflation advanced by Friedman and subsequently endorsed by Bernanke. Friedman was perfectly willing to grant that shocks to the prices of food and energy, as well as the removal of the Nixon price controls, played an important role in the *timing* of inflation across quarters. But he insisted that monetary policy dominated the *mean* of inflation. Blinder (1982, p. 281) acknowledged in the case of price controls and their removal that granted that they mainly affected "the time pattern of inflation in

1971–75." Once the same proposition is granted with regard to food and energy shocks, there is no inconsistency between a monetary interpretation of the 1970s inflation and the notion that movements in particular prices dominated price-level behavior in some key parts of the decade.[91]

Appropriate monetary policy and the oil shock

Even among those in the mid-1970s who agreed that the monetary policy response was crucial in determining the implications of the oil shock for inflation, what monetary policy *should* do in response to the oil shock became an area of some discussion. Near-universal agreement, spanning Keynesians and monetarists, pertained to the notion that the authorities should not try to restore the pre-shock price level; that is, it should not reduce the money stock in step with the decline in potential output.[92] However, some economists argued that the monetary growth rate might be temporarily increased, so that the relative-price adjustment set in motion by the oil shock did not entail forcing down the absolute prices of non-energy-intensive products.[93] Friedman saw a case for such a short-term adjustment of monetary growth at the level of principle. But he did not view the case as compelling, especially when judged against the need to contain inflation expectations in the face of the fillip that the initial price-level shock might give these expectations. Consequently, he remained in favor of a nonactivist monetary growth response.

An alternative policy to either a tightening or a temporary loosening of monetary policy in response to the oil shock was to raise monetary growth permanently in reaction to the oil shock. Despite the fact that it was not advanced by economists as the appropriate response, this scenario has become one of the most-discussed ones in the coverage of the monetary policy/oil shock/inflation nexus of the 1970s. One of the reasons for this focus is that monetary accommodation of oil shocks apparently squares the circle and reconciles oil-push and monetary explanations for inflation; as Friedman put it, an account in which "inflation is caused by the

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[91] It should be stressed that, as both Blinder and Friedman were exponents of augmented-Phillips-curve analysis, it was common ground between them that the central bank could ultimately block relative-price increases from permanently raising the price level.

[92] On the Keynesian side, James Tobin (according to Paul Samuelson in *Financial Times* (London), December 31, 1973) stressed the desirability of allowing the aggregate price level to rise on a one-time basis. Among others who endorsed this approach were John Flemming (as attributed to him by Samuel Brittan in *Financial Times* (London), September 9, 1976), Brittan (1983, p. 126), and Brunner and Meltzer (1993, p. 227). For a contrary position, in which it is suggested that "it would be undesirable to accommodate also the first-round effect on the price level," see Svensson (2003a, p. 3).

[93] For example, in April 1975 Michael Mussa noted (see Birnbaum and Laffer, 1976, p. 147) that "you might want to let the prices of those products that are going up in relative price to rise also in money prices, rather than trying to force down the nominal prices of other goods." For a formalization of an argument of this kind, see Aoki (2003).

OPEC cartel may be correct if what is meant is that the OPEC cartel set in motion forces that induced" higher monetary growth.[94]

Another reason for the focus on monetary accommodation lies in the fact that many observers seem to think that a change in the U.S. money stock trend in response to the first OPEC shock is what actually occurred. The fact that inflation's surge and peak in the mid-1970s roughly coincided with the first oil shock may underlie this impression. The interpretation that monetary policy eased in response to the oil shocks became very widespread and appeared even in an early retrospective, made by Warren Nutter in 1976.[95] It continued to be conventional wisdom thereafter, with an op-ed by Martin Feldstein in response to the 1990 oil price rise being titled "The Fed Should Not Accommodate Iraq" (*Wall Street Journal*, August 13, 1990), as though monetary accommodation of oil shocks had been the Federal Reserve's past practice. Furthermore, the explanation for the Great Inflation offered by Chari, Christiano, and Eichenbaum (1998) was premised on the notion that the Federal Reserve raised monetary growth in response to the 1973–1974 oil shock.

These impressions run, however, against two absolutely fundamental aspects of Friedman's account of the 1970s, which have been emphasized earlier in this chapter and which have both received support from subsequent empirical work.

The first aspect is a key fact of timing: the mid-1970s inflation in the United States did not reflect *contemporaneous* monetary ease but instead was largely a delayed reaction to the early 1970s monetary expansion. The U.S. monetary expansion of the early 1970s was so great and so closely related to subsequent inflation that it left comparatively little of the movement in the mean rate of inflation over the period to be attributed to the oil shock. The same was true of numerous other advanced countries, as shown in Darby and Lothian (1983) and Parkin (1980).

The second aspect of the actual 1970s record that deserves emphasis is that the Federal Reserve tightened monetary policy in 1973 and 1974—both before and after the oil shock. This pattern of behavior is at variance with narratives that argue that U.S. inflation and monetary policy developments can be understood in terms of policymaker accommodation of the first oil shock. The first oil shock was not, in fact, accommodated by monetary policy.

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[94] Friedman (1990a, p. 16).

[95] See Nutter's testimony of February 17, 1976, in Committee on Finance, U.S. Senate (1976, p. 66).

INTO THE FLOATING-RATE ERA

In his lecture in Yugoslavia on March 20, 1973, Friedman's discussion of exchange-rate arrangements referred to "the new system that has emerged in the course of [the] past few weeks."[96] Over the previous seven weeks, the attempt to restore a version of the Bretton Woods system via the Smithsonian agreement had broken down. Canada and the United Kingdom already had floating exchange rates by 1973. On February 12, the yen was floated, while the U.S. dollar had another official devaluation, this time of 10 percent (*Daily News* (New York), February 13, 1973). Then on March 11–12, 1973, the dollar-centered fixed exchange rate system of the postwar period ended when six European countries started floating against the U.S. dollar (*Daily News* (New York), March 12, 1973; *Dallas Morning News*, March 13, 1973).

Friedman characterized the new system that had emerged as "'dirty' floating," and he would continue to use that label when that system was still in force nearly thirteen years later (*Wall Street Journal*, December 18, 1985). But at the outset of the March 1973 breakdown of the organized fixed-rates system, he did not see widespread floating as likely to last, even in its "dirty" form (that is, featuring some attempts at exchange-rate management by central banks, which occasionally intervened in the foreign-exchange market to this end).

Instead, Friedman in the early months of floating reiterated his position that numerous countries would likely want to have balance-of-payments surpluses with the U.S. dollar. That is, he saw a continuing form of the "dollar standard," in which accumulation of dollar exchange reserves was an aim of governments (Instructional Dynamics Economics Cassette Tape 120, May 11, 1973). Friedman noted that countries in the rest of the world might want to float or fix their exchange rate against the U.S. dollar. Either situation, he suggested, should be treated with equanimity: "If other countries want to peg [against] the dollar, that's fine, it's their business. If they want to float, that's fine, it's their business too." What was important from the United States' point of view, Friedman insisted, was that it should not have exchange controls nor make efforts to support other countries' exchange-rate targets (*Kansas City Star* (Missouri), March 15, 1973).

However, what emerged was actually far more like true floating. Although it was often called "managed floating," the observed practice did *not* see sustained restoration of exchange-rate targets (*vis a vis* the U.S. dollar) by the United States' large trading partners. In January 1974, Friedman noted that "one has to stress the fact that we are now in a regime of floating exchange

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[96] Friedman (1973d, p. 12).

rates, to all intents and purposes... And, obviously, that's a highly desirable thing."[97] In 1986, while not completely discounting the possibility that the "dollar standard" label still applied to the modern world, Friedman acknowledged that "the world's monetary system has consisted of a collection of national fiat currencies linked by 'floating' exchange rates set in the market.'"[98]

Along with explicit foreign exchange intervention by central banks, there were a couple of other notable respects in which the early years of floating departed from Friedman's prescription for the international monetary system. First, as discussed in Section III below, large-scale swap operations became and remained an important element of governments' activities. Friedman disapproved of these operations, viewing them as a back-door attempt at exchange-rate management. Second, Whitman (1975, pp. 133–134) observed that in 1973–1974 several major countries engaged in a "form of indirect [foreign-exchange market] intervention by which countries may seek to avoid or reduce depreciation of their currencies," namely, "by foreign (as opposed to domestic) borrowing by governmental or government-related authorities." Although not opposed in principle to the notion of the public sector borrowing abroad, Friedman was opposed to such activity being carried out with the express intent of supporting the exchange rate. In a 1976 commentary, he acknowledged that government "borrowing abroad does of course tend to strengthen the currency," but he indicated that this amounted to a situation in which the exchange rate was "artificially held up" (Instructional Dynamics Economics Cassette Tape 185, February 1976, Part 1).

Friedman's role in bringing about the exchange-rate system

The fact that the United States and other major countries adopted floating exchange rates in the early 1970s raises the question of whether this came about to a significant degree because of the influence of Friedman's advocacy, as reflected in his research and other statements. His 1953 paper remained his principal contribution on this matter, but he had consistently been a vocal advocate of floating rates in articles, testimony, and other public statements over the subsequent two decades.[99]

It was not in doubt that Friedman had had an important role in changing opinion among U.S.

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[97] From Friedman's remarks in the panel discussion on *Long-Term Energy Crisis Solutions*, held at the National Conference on Government Responses to the Energy Crisis, January 24, 1974.
[98] Friedman (1986a, p. 325).
[99] See Nelson (2009a, 2017, 2020d) for accounts and analysis of these post-1953 statements. The question of whether Friedman himself had a *personal* role in facilitating floating, via his various direct interactions with U.S. policymakers in the early 1970s, is deferred until the discussion of George Shultz in Section III below.

economists, particularly in academia, on floating. At the time of earlier problems with the Bretton Woods system, Paul Samuelson had been quoted saying (*Chicago Daily News*, March 28, 1968): "If you took a poll of experts in international economics, you would find 80 percent of them favoring floating exchange rates as a result of Friedman's influence."[100] In the early 1970s, there were still some prominent U.S. economists in favor of a fixed-rate system—including Charles Kindleberger and Robert Mundell—but Friedman had the majority of U.S. academics interested on the matter on his side of the argument.

However, recognition that Friedman had prevailed in economists' debate does not mean that the floating era came about because of the influence of his ideas. The general float occurred after the collapse of the 1971−1973 repairs to the Bretton Woods system. It did not arise out of a conscious embrace of floating on the part of policymakers; rather, these policymakers initially presented floating as a temporary measure (though some were privately enthusiastic about floating indefinitely—including George Shultz, as discussed in Section III below). The fact that floating was a default move rather than a concerted policy action in 1974 led Brunner and Meltzer (1976, p. 2) to observe: "The case against fixed exchange rates by Friedman [in his 1953 paper] was not successful for many years and seems to be more of a triumph for events over policymakers than for economists over events." Later, Paul Craig Roberts (in *Wall Street Journal*, December 18, 1985) cast even greater doubt on the importance of Friedman's views on floating when he confronted the notion that "the move to flexible exchange rates was a conscious policy decision, based on the power of contending ideas… It was no such thing." Instead, Roberts suggested, "inflation forced us off the fixed-rate system."

Friedman's position on why floating became prevalent was, however, more nuanced than that of Roberts.[101] He acknowledged that policymakers had accepted a floating-rate system because fixed rates had broken down, rather than as a result of a systematic, orderly deliberation on the merits of fixed-rate versus floating-rate systems. But even in the early years of floating, Friedman allowed for considerable influence of the academic support for floating on how events had proceeded. In May 1974, he cited floating exchange rates as an example of a policy option whose viability had been strengthened by the fact that research and other writings had kept it going as an idea.[102] In that discussion, he noted that his case for floating rates had been an

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[100] Friedman gave a similar estimate of the balance of economists' opinion in Friedman and Roosa (1967, p. 133).
[101] See Nelson (2009, 2017) for further discussion and documentation of Friedman's position on this matter.
[102] Although Burgin (2012, p. 221) relies heavily on Friedman and Friedman (1998) to document the point that Friedman saw this role for his academic work on floating rates, Friedman had actually frequently articulated this view in his earlier discussions of the shift to floating, including the 1974 and 1975 discussions cited in the text of this chapter and in Friedman (1984a, p. 52).

argument against relying on exchange controls instead of floating as a means of securing monetary policy autonomy. Why, he asked, had those advocating the restoration of the fixed-rate system not advanced much stricter foreign exchange controls as a key part of their proposed new system? He suggested that this was because the distortionary effects of foreign exchange controls were now widely accepted (Instructional Dynamics Economics Cassette Tape 146, May 20, 1974). But, of course, this acceptance made a return to fixed rates less likely, as policymakers were loath to surrender national monetary autonomy.

Furthermore, Friedman declared in 1975 that the advent of floating had refuted "the arguments I used to hear 10 and 15 years ago about how it was utterly academic dreaming to suppose you could have a system of flexible exchange rates in the world. The truth is that it is the unrealistic academic who can look at the situation in its broadest context—and get away from the immediate policy position—who is the most realistic."[103]

As the years passed and floating became the permanent system in many parts of the world, Friedman believed that the *retention* of floating rates after 1973 did reflect importantly the force of the intellectual case for floating. The most important argument both in his 1953 article and in the post-1973 debate was that floating provided monetary policy autonomy and fixed rates (absent exchange controls) did not, and that argument proved decisive in the widespread acceptance of floating in many countries (including, by the mid-1990s, the United States, the United Kingdom, Canada, Australia, and New Zealand).

The decisiveness of the monetary-autonomy argument in promoting enduring support for floating exchange rates undermines suggestions (one reflected, for example, in the title of Leeson, 2003a) that the prevalence of floating was a victory of free-market "ideology."[104] The case for floating rates cut across ideological divisions. Paul Samuelson's support, discussed above, for flexible rates attests to this reality. So does the fact that as an economic adviser in the United Kingdom in the mid-1960s, hardline Keynesian Nicholas Kaldor had urged greater flexibility for the sterling exchange rate (for example, Dell, 1990, p. 315). The upshot was that, as Krugman (1989, p. 71) observed, "advocates of flexible rates may be traditional monetarists who want freedom to target their favorite aggregates or Keynesians who want to pursue activist

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[103] *Monday Conference*, Australian Broadcasting Commission, April 14, 1975, p. 25 of transcript.
[104] Similarly, Burgin (2012, p. 204) states that Friedman "abetted the unfolding collapse of the Bretton Woods system," a characterization that (by the choice of the word "abetted") implies that Friedman behaved reprehensively, as well as ideologically, in advocating floating rates.

stabilization policy."[105] And as discussed in the previous volume, Friedman stressed that the U.S. Republican party's thinking was split on fixed versus floating rates, and its traditional inclination was to support a fixed-rates system.

The United States' system of exchange controls (including such measures as the interest equalization tax) was removed in 1973−1974. This process started with an announcement in conjunction with the February 1973 dollar devaluation. At that time, Paul Samuelson remarked that he was "sure that the ideological antipathy towards those controls was an important part of the motivation" for abolishing them (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 122, February 22, 1973). Samuelson's reasoning apparently was that abolition of exchange controls made devaluation more palatable to free-marketeers who were also fixed-rate advocates. Free-market sympathies well have been part of the motivation for the removal of controls. However, once the United States and other countries were no longer floating, the continuing relaxation of foreign exchange controls had a sound, non-ideological, rationale. As indicated above, Friedman had long argued that because countries were unwilling to subordinate macroeconomic management to exchange-rate or balance-of-payments considerations, adoption of fixed exchange rates in the modern era meant that one would institute exchange controls. A corollary was that with floating rates, one did not need exchange controls.

In Congressional testimony on June 26, 1973, Paul Volcker, Under Secretary of the Treasury for Monetary Affairs, endorsed the exchange-rate/exchange-controls link that Friedman had stressed when he observed: "we are certainly aiming at a [global] monetary system which is not dependent upon the widespread use of controls. We do want to get rid of the foreign direct investment controls. That is a major objective. And we are certainly willing to accept an implication that you need more flexible exchange rates as part of the bargain."[106]

The motivation for exchange controls in the pre-1973 system differed somewhat across the United States and other countries. Outside the United States, exchange controls helped deliver monetary policy autonomy alongside fixed exchange rates. In the United States, as Friedman stressed (see the previous volume and the discussion below), the authorities had been able to achieve monetary policy autonomy even when committed to an exchange-rate system, as it was straightforward for them to offset the effect of U.S. balance of payments deficits on American

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[105] A qualification is that in the 1970s and 1980s some Keynesian advocates of flexible rates, such as Kaldor and to some extent James Tobin, became more sympathetic to a return to forms of exchange-rate management.
[106] In Joint Economic Committee (1973a, p. 160).

monetary conditions. But exchange controls did give U.S. policymakers a tool with which they could make the deficit take a lower value than it would otherwise take.

The future role of gold

A United Press International news report on official plans to return to a fixed exchange-rate system was headlined "IMF Experts OK Plan To Dump Gold Standard" (*Detroit Free Press*, January 16, 1974). The article therefore accepted the false premise that the Bretton Woods system had been a Gold Standard. As Friedman reiterated at a November 1974 University of Miami conference on the role of gold, the United States had not had gold standard under Bretton Woods or "anything even approaching" a true gold standard since the Federal Reserve was formed in 1914 (see Manne and Miller, 1975, p. 148).

Indeed, over time Friedman would see Bretton Woods as having been dissimilar to the Gold Standard for other countries as well. During the Bretton Woods era, he and others had largely accepted the narrative that non-U.S. members of the system did not possess a sufficiently broad exchange-control apparatus to secure appreciable monetary autonomy. Consequently, as far as monetary conditions in their domestic economies were concerned, member countries other than the United States were portrayed as being beholden to U.S. monetary policy.[107] Many commentaries on U.K economic policy, in particular, took for granted that the United Kingdom, at least until the 1967 sterling devaluation, had subordinated macroeconomic policy to maintenance of the exchange rate. For example, Marina Whitman, member of President Nixon's Council of Economic Advisers, said on October 27, 1972 that the United Kingdom had "subordinate[d] its domestic economic goals to balance of payments considerations… throughout much of the period since World War II."[108]

Friedman himself subscribed to this interpretation at the time: for example, in September 1970 he remarked that the Bretton Woods system made the United Kingdom "a monetary satellite of Washington" (in *Financial Times* (London), September 18, 1970). But further study brought home to Friedman how much the U.K. authorities had been able to insulate U.K. monetary conditions, and in 1982 he and Anna Schwartz would refer to "obvious primacy of domestic considerations in the formulation of government economic policy in essentially all countries"

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[107] This would remain a standard narrative. For example, Obstfeld and Rogoff (1996, p. 567) characterized the Bretton Woods system as one in which the "United States [was] setting the system's monetary policy."
[108] Quoted in Joint Economic Committee (1973a, p. 207),

after the Second World War.[109] Correspondingly, in the early 2000s Friedman concurred that the United Kingdom had sterilized its balance-of-payments deficits during the Bretton Woods era.[110]

That said, some countries clearly did ultimately find that the pre-1973 arrangements compromised their ability to carry out domestic stabilization objectives. Notably, the 1973 floats of the yen and the Swiss franc took place after both countries found that the many controls that each imposed on foreign investment were unable to withstand the vast capital inflows they were receiving.

It was, however, clear that the Bretton Woods system had, for the most part, not resembled the Gold Standard, especially for deficit countries. Nevertheless, the U.S. government had had some form of commitments involving the gold market over most of the postwar period to date.[111] During much of that same period, Friedman had been a critic of the role of commodity prices in monetary arrangements. Indeed, he noted in May 1973 (Instructional Dynamics Economics Cassette Tape 121, May 24, 1973) that in an article in the early 1960s he had urged that the U.S. authorities abandon their activities in the gold market.[112] He went on to observe that his opposition to commodity currencies predated the 1960s. Indeed, in his 1951 paper on "Commodity Reserve Currency," Friedman had stated: "There is no reason to waste resources in piling up monetary stocks instead of adopting the essentially costless alternative of a fiat standard."[113] The U.S. authorities, he observed, had partly exited activities in the gold market simply by embracing selling arrangements that meant off-loading its gold at what became low

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[109] Friedman and Schwartz (1982, p. 572). See also Chapter 7 for further discussion.

[110] See his observations in Friedman and Mundell (2001, pp. 21–22). The heavy degree of sterilization undertaken by the U.K. authorities in the 1960s was documented by Fischer and Dornbusch (1983a, pp. 417–418). See also Batini and Nelson (2005, p. 18) and Nelson (2012) for discussions of the evidence of U.K. policy autonomy in the area of short-term interest rates.

In a brief and quite muddled passage, Friedman (1992b, p. viii) gave more credence than he had elsewhere both to the Bretton Woods arrangements as a constraint on U.S. monetary growth during the 1960s and to the notion that U.S. "monetary growth was exported" in that decade. With regard to the latter statement, it can perhaps be viewed as a description of the experience of countries like Germany or Japan, whose authorities did indeed permit rapid monetary growth during portions of the decade to 1973 in order to sustain their fixed exchange rates against the U.S. dollar. So interpreted, the statement is consistent with Friedman's recognition (noted above) that other countries, like the United Kingdom, essentially sterilized the effects of international payments flows on their home money stock. The notion that the Bretton Woods arrangements constrained U.S. monetary policy behavior is much harder to reconcile with the general tenor of Friedman's various narratives of monetary developments over the course of the 1950s and the 1960s. But it can be so reconciled if it is interpreted as a reference specifically to 1962—a year in which, Friedman believed, the U.S. monetary authorities allowed international considerations to shape domestic monetary conditions, with the result that U.S. monetary growth during that year was subdued.

[111] Consequently, Friedman was willing to describe August 1971 as being "when the dollar was first cut loose from gold (*Newsweek*, January 30, 1978), even though he did not believe that the gold commitment had meaningfully constrained U.S. monetary policy.

[112] See Friedman (1961a).

[113] Friedman (1951a, p. 232).

prices—that is, $35 per ounce until August 1971 and $38 in 1972–1973 (Instructional Dynamics Economics Cassette Tape 121, May 24, 1973).[114]

But the United States from 1973 onward was involved, initially through Under Secretary Volcker, in trying to work out a viable fixed exchange-rate system to displace the new floating-rate system, and this raised the question of whether it would involve gold in some way, even in a peripheral role like that gold played during the Bretton Woods era. Laffer (1982, p. 164) would later contend that Volcker was sympathetic to an exchange-rate system involving the convertibility of the U.S. dollar into gold. However, Volcker's own statements both in and out of government office provide little support for this interpretation. In particular, in his June 1973 testimony he stated: "We believe the system of the future should not be dependent on gold—gold-based, in the sense that even the Bretton Woods system was."[115] As this statement indicates and as discussed further in Section III, Volcker was indeed interested in moving back to an organized fixed-rate system. But he was opposed to restoring even a minimal role for gold.

The post-1973 framework meant that, even with the longevity of the floating-rate system unresolved, gold was for practical purposes gold gone for good from U.S. and international monetary arrangements. The January 1974 press report mentioned above was a sign of this process, as it referred to a proposal that gold should not be among the media used for official international-payments settlements in a proposed follow-on fixed exchange-rate system. Another sign was a change in U.S. law that allowed U.S. households to possess gold in appreciable quantities after December 31, 1974. The ban on gold ownership had been one of the means by which the United States implemented the peg of the gold price (abandoned in 1971). At the November 1974 conference on gold, Friedman observed that "it is perfectly clear that once we permit private ownership of gold, there will be another nail in the coffin of an official monetary gold standard."[116]

Verdicts on Friedman's case for floating: two misconceptions

Almost as soon as floating rates became widespread, the results they were producing were said to contradict Friedman's predictions about how flexible exchange rates would work. Ahead of discussing some of these critiques, the ground needs to be cleared by considering two arguments

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[114] The behavior of the world gold price after 1973 is considered further in Chapter 7.
[115] Joint Economic Committee (1973a, p. 159).
[116] Manne and Miller (1975, p. 167). For further discussion of this change, see Chapter 7 below.

against Friedman's case for floating rates that are invalid because they attribute to Friedman views that he did not hold.

First, imbalances in both the trade account and current account generally continued to be nonzero under floating rates, and this state of affairs was said to be inconsistent with Friedman's promises regarding what floating rates would bring. Laffer (1973, p. 28) suggested of Friedman's framework: "Trade must, however be balanced because of flexible rates…" Similarly, Gittins (1988, p. 126) contended that "the advantages of floating which had been promised by leading monetarists such as Milton Friedman" included that "current account imbalances… would disappear with floating rates."

These critiques are invalid, as Friedman (accurately) suggested only that the overall balance of payments (the sum of the imbalances in the current and capital accounts) would be zero under floating rates—not that the individual account categories would be in balance. He believed that exchange-rate depreciations typically lower trade and current account deficits. But he did not claim that a floating rate would eliminate the current account deficit. Indeed, as discussed in later chapters and in Nelson (2017), Friedman regarded a nonzero current account balance as a normal state of affairs, and a deficit in the current account as desirable in many circumstances. Early in the floating-rate era, Friedman had occasion to touch on the matter (Instructional Dynamics Economic Cassette Tape 120, May 11, 1973), observing that if "we are able to import more than we export," that was "not a problem. Quite the contrary. If foreign countries like Japan are willing to send us goods and services and take back… interest-bearing IOUs, we have no [cause for] complaint, they're doing us a favor, they're hurting themselves and helping us."

Second, and remarkably, some economists have implicitly or explicitly grouped Friedman with those who advocated floating rates as a means of exploiting a permanently downward-sloping Phillips curve. For example, Argy (1992, p. 154), shortly after citing Friedman's 1953 paper, attributed to floating rates' proponents the position that "exchange rates allowed countries to choose their own combinations of inflation and unemployment."

This attribution flies in the face, of course, of Friedman's longstanding denial of a long-run inflation/unemployment tradeoff.[117] It was not Friedman who tried to motivate floating by the alleged opportunities it offered to choose a permanent inflation/unemployment combination, but

[117] He denied this tradeoff not only in the 1960s and later, but also at the time the Friedman (1953a) article was written. See Nelson (2009, 2020b).

Harry Johnson. In the London *Times* (December 9, 1968), Johnson suggested that floating gave governments "the right to decide what combination of employment and inflation they prefer" and to achieve that combination, and he elaborated upon this argument in Johnson (1969).[118] This argument, though intended to be an improvement and refinement of Friedman's case for floating, would be discredited by the natural rate hypothesis.[119] But Friedman's case for floating emphatically did not appeal to that argument. Although, as late as 1974, some analysts were treating proponents of floating as arguing that it allowed a country to choose the average amount value of the unemployment rate in their economy (see Corden, 1974, p. 140), Friedman was insistent that central banks could not determine the unemployment rate in the long run under any exchange-rate regime.

Early verdicts on floating rates

Several prominent proceedings were held in 1973 and 1974 that tried to make verdicts on the floating-rate system. These included the June 1973 Congressional hearings on "How Well Are Fluctuating Exchange Rates Working?" (Joint Economic Committee, 1973a), which received testimony from Friedman, Volcker, and numerous others; a December 1973 Federal Reserve Bank of Chicago workshop on international inflation that Friedman attended; and a May 1974 conference, held in Williamsburg, Virginia, titled "What Have We Learned from a Year of Greater Flexibility of Exchange Rates?" (see McKinnon, 1976, p. 113). No proceedings volume emerged from the May 1974 conference, but Friedman was an attendee (Instructional Dynamics Economics Cassette Tape 146, May 20, 1974).

In retrospect, these seem to have been absurdly premature occasions on which to make a judgment about floating rates. Alongside the obvious fact that the system had not been operating for long, there are two other compelling reasons for doubting that developments through May 1974 shed much light on floating. First, it was not yet clear that this was not simply a transitional arrangement before a follow-on fixed exchange-rate system—continuously under discussion in these years—was agreed upon. Indeed, Hansen and Hodrick (1983, pp. 120, 122) suggest that it was not until February 1976, when officials from various countries ratified floating, that it was clear that floating was the permanent new system. Their date seems too late—it will be argued in Chapter 7 below that by mid-1975 floating had received *de facto* acceptance as the permanent system—but their point is valid with regard to 1973 and 1974.

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[118] Thirlwall (1980, p. 183) discussed Johnson's articulation of this argument in U.K. economic debate.
[119] See, for example, Niehans (1984, p. 290) and Nelson (2009, p. 70; 2020b) for further discussion.

Second, the first real test of the floating-rate system—the impact of the first oil shock—did not crystalize into its final form until after March 1974, when it became a price increase applied to all countries instead of a price increase applied to some countries and a sellers' boycott applied to other countries.

Friedman's early evaluations of the post-Bretton Woods float were similar to his later ones. In his June 1973 Congressional testimony, his assessment was that the "past few months are a remarkable demonstration of the virtues of floating exchange rates," with Friedman citing the fact that the foreign exchange market and international trade had proceeded in an orderly manner notwithstanding a background of surging commodity prices.[120] At the December 1973 conference referred to above, Friedman remarked: "I think the floating rate system is fine."[121] In March 1974, describing the reaction of the foreign exchange market to the Middle East war, Friedman said that flexible exchange rates had worked the way he had predicted.[122]

In contrast, Charles Kindleberger—in a paper for the November 1974 Carnegie-Rochester Conference, another proceeding devoted to the new floating-rate environment—claimed that the flexible-rate experience had confounded a prediction Friedman's had made concerning exchange rates: specifically, that speculation in the foreign exchange market was stabilizing (Kindleberger, 1976, p. 62). However, the criterion that Kindleberger used to make that judgment was narrow: for him, the fact that exchange-rate variability had gone up since the float was sufficient.[123] Friedman, in his June 1973 testimony, cited instead the criterion of whether private speculation moved the exchange market toward an economically-justified value: "private speculators will have a strong incentive to step in and correct the obviously wrong exchange rate."[124] This position was itself sensitive to judgments about what exchange rate was justified by fundamentals. However, at the May 1974 conference mentioned earlier, Friedman did concede that stabilizing speculation had not yet emerged as a clear feature of the floating experience so far, and he expressed the hope that financial markets would develop instruments that helped

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[120] From Friedman's testimony of June 21, 1973, in Joint Economic Committee (1973a, pp. 116−117; quotation from page 116).
[121] Friedman (1974l, p. 16).
[122] Friedman (1974k, p. 2)
[123] Later, Fischer (1988a, p. 114) contended that the argument that "speculation would ensure substantial exchange rate stability" had been contradicted by the fact that the "the floating-exchange-rate system has seen major movements in exchange rates that have later been reversed." However, more recently, research (such as Chari, Kehoe, and McGrattan, 2002) using optimizing business cycle models has found that these models could produce exchange-rate movements that were persistent but reversed. It is therefore not clear that the specific regularity concerning exchange rates that Fischer cited is in itself actually inconsistent with speculation being a stabilizing force.
[124] From Friedman's testimony of June 21, 1973, in Joint Economic Committee (1973a, p. 118).

promote this feature (Instructional Dynamics Economics Cassette Tape 146, May 20, 1974).

A more clear-cut matter on which Friedman's predictions about floating were confounded pertained to capital flows. "It's only with fixed exchange rates that you have these large short-term capital flows," Friedman had declared late in the Bretton Woods period (Instructional Dynamics Economics Cassette Tape 74, May 20, 1971). That declaration was comprehensively refuted by the experience with floating, both in 1973–1974 and in later years.

But one could question Friedman's position on speculation and reject his prediction concerning capital flows without denying the validity of the central, and for most observers, clinching argument in favor of floating: that it conferred monetary policy autonomy. Indeed, this argument's strength was reinforced as, over the years, economists and policymakers came to accept his monetary diagnosis of the inflation process. In 1974, the International Monetary Fund's Marcus Fleming had claimed that the Bretton Woods system broke down in part because "cost-push factors were becoming more important relative to demand-pull factors in the causation of inflation" (Fleming, 1974, p. 86). If this argument was valid, then inflation had become predominantly a nonmonetary phenomenon, and floating rates offered little extra scope to a country's authorities for controlling inflation. In contrast, if monetary policy was decisive for inflation and cost-push forces only an ephemeral influence on inflation, then the authorities' ability to steer monetary conditions in their country delivered the necessary and sufficient conditions for achieving price stability.

As already noted, the monetary-autonomy argument was of far more relevance to other countries than to the United States, which had monetary sovereignty even in pre-1973 conditions. Federal Reserve Chairman Burns said as much at the dawn of the floating-rate era when he remarked: "What happens to the discount rate happens [in] Washington and not anywhere else." (*Daily News* (New York), March 17, 1973.)

III. PERSONALITIES IN DEBATES ON INTERNATIONAL ECONOMIC POLICY AND GEOPOLITICS, 1973–1974

GEORGE SHULTZ

At the start of 1972, Friedman was very much a critic of the settings of U.S. economic policy. To his great disapproval, the Nixon Administration had embraced fiscal expansion over the past year, instituted wage-price controls in August 1971, and revived fixed exchange rates via the

Smithsonian agreement the following December. Friedman was also extremely unhappy with U.S. internal monetary developments, as reflected in critical correspondence (and a cooling of his personal relations) with Federal Reserve Chairman Arthur Burns and in a long line of public statements criticizing FOMC decisions under Burns, the latest being his column "Irresponsible Monetary Policy" (*Newsweek*, January 10, 1972).

Against this background, a Cabinet member in the Nixon Administration—George Shultz, then Director of the Office of Management and Budget—offered a strong public defense of Friedman. Friedman, Shultz said, had "one of the most extraordinary minds" that Shultz had come across and was, in addition, "a fellow of absolute integrity." Shultz cautioned that as Friedman had studied monetary relations more "than perhaps anyone else in the world," it would be an error just to categorize Friedman's current dissent from U.S. economic policy as reflecting the viewpoint "of some nut" (*Wall Street Journal*, January 7, 1972).

This defense underlined the point that, even when Friedman was at odds with the Nixon Administration and the Federal Reserve, he maintained strong relations with his former University of Chicago colleague Shultz, who would become U.S. Secretary of the Treasury in June 1972.

Over the course of 1972 and into 1973, despite his continuing disagreement with the settings of U.S. economic policy, Friedman had a resumption of generally good relations with the Federal Reserve, which he praised in late 1972, and the Nixon Administration, whose reelection he supported. This rapprochement was, as discussed in the previous chapter, then shattered by the take-off in inflation in 1973 and the administration's continuation of price controls.

It was during this time that Friedman, in a letter to Shultz of May 24, 1973, suggested that Shultz should leave the administration.[125] In the letter, Friedman expressed much the same sentiments that he made in his public commentary at the time (Instructional Dynamics Economics Cassette Tape 121, May 24, 1973) that the deterioration in U.S. inflation alongside the Watergate scandal would put pressure on President Nixon to tighten wage-price controls and increase public-sector intervention in the economy. In this commentary, he cited Shultz as a voice against wage-price controls but pointed to former Treasury Secretary John Connally as a contrary influence on the

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[125] The correspondence is in the Friedman papers, Hoover Institution archives. Meltzer (2009b, p. 787) incorrectly claimed that Shultz did leave the administration around mid-1973 and implied that this was in protest to the continuation of wage/price controls. In fact, Shultz did not leave office until May 1974 and therefore actually outlasted the period of controls.

administration.[126] In Nixon's second-term lineup, Shultz had been given enhanced powers with an office in the White House on top of his Treasury position (*Rochester Post-Bulletin* (Minnesota), December 1, 1972), but Connally had returned to the administration as a counselor to President Nixon (Nixon, 1978, p. 908). Shultz did not follow Friedman's suggestion; he remained Secretary of the Treasury throughout 1973.

Friedman's fears in May 1973 that Shultz's role in the administration would be marginalized initially seemed to be confirmed: although Shultz had said in that month that Watergate was not putting U.S. economic policy off-course (*Evening Star and Daily News* (Washington, D.C.), May 16, 1973), in June Nixon imposed "Freeze II," discussed in the previous chapter. However, in subsequent months, though the economic news got still worse, the administration's economic policy posture became more amenable to Shultz and Friedman in important respects. In the second half of the year (by which time Connally had left the administration), Shultz publicly expressed his unhappiness with the decision to impose Freeze II, while expressing optimism that wage-price controls would be completely gone by mid-1974 (*Washington Star-News* (Washington, D.C.), October 28, 1973). This came to pass: Shultz departed from the Treasury on May 8, 1974; wage-price controls had been abolished a little over a week earlier.

As well as sharing an aversion to wage-price controls, Friedman and Shultz had similar instincts in other areas of domestic macroeconomic policy. Consistent with this shared outlook, Shultz did not voice dissent in February 1974 when a Congressional questioner described him as "George Shultz from the Friedman school of the economic restraint and the University of Chicago."[127] However, as discussed in the previous volume (Nelson, 2020b, Chapter 15), Shultz was not a monetarist, notwithstanding his sympathy with much of Friedman's economic analysis. Shultz himself had noted in his January 1972 observations that he had "always described myself as a friend of Milton Friedman—but not as a Friedmaniac" (*Wall Street Journal*, January 7, 1972).

One difference between them was on fiscal policy. Frequently (for example, in Instructional Dynamics Economics Cassette Tape 114, January 31, 1973), Friedman criticized Nixon Administration personnel for believing that fiscal policy was important in its own right for the

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[126] Friedman had also implicitly contrasted Shultz and Connally, and traced the contrast to Shultz's economics background, when he remarked: "I think a purely political animal is not likely to recognize the economic price he is paying for political advantage, where[as] the economist still may be willing to pay the economic price—but has the advantage of knowing what price he is paying." (*Florence Morning News* (South Carolina), April 16, 1973.)
[127] From Senator William Proxmire's remarks during Shultz's appearance at a hearing of February 8, 1974, in Joint Economic Committee (1974d, p. 102).

behavior of aggregate nominal demand, and not solely via its implications for monetary growth. Shultz was one such believer in fiscal policy's effects. He also saw merit in the use of discretionary fiscal policy measures to stabilize aggregate demand, telling a hearing in early 1974 that the budget deficit in Nixon's budget proposal "will provide a measure of support for the economy." [128] And, as discussed in the previous chapter, Shultz's tenure at the Treasury saw very large increases in federal spending, although Shultz had limited influence on this total.

On inflation, too, Shultz agreed only partly with Friedman's monetary diagnosis and was far more inclined than Friedman was to attribute the inflation occurring during Shultz's years at the Treasury to nonmonetary forces. Friedman had warned Shultz (for example, in a letter of August 5, 1973) against overstating the role of special factors in 1973's high inflation rate. Despite this injunction, in February 1974 Shultz testified that "what the world experienced last year was essentially a commodity price inflation." [129] He added: "Food prices alone may have been responsible for half the increases in consumer prices in the major industrial countries, and[,] toward the end of the year particularly, energy prices also contributed heavily."[130] Shultz traced the commodity price boom in good part to a global economic boom, but even then he emphasized the supply side as the factor driving price increases; furthermore, he expressed concern that the "cost-price screw" might be exacerbated if domestic wages and prices responded positively to energy-price increases.[131] On this occasion, very little of Friedman's position that it was the monetary climate that turned a commodity price increase into a general and ongoing price rise had a parallel in Shultz's analysis.

In contrast, the area of international economic policy was one of much more widespread agreement between Friedman and Shultz. Some months after Shultz left office, Friedman noted that he had devoted his *Newsweek* column of April 1, 1974, to a "tribute to a remarkable man, George Pratt Shultz… a man of principle and an extraordinarily effective administrator and also not incidentally, an economist."[132] In that column, Friedman had characterized Shultz as the primary force among U.S. policymakers in the push for increased exchange-rate flexibility.

In the same column, Friedman suggested that the Federal Reserve had been an obstacle to the transition to a flexible-rate system, by, he claimed, urging intervention in the foreign exchange

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[128] From Shultz's testimony of February 8, 1974, in Joint Economic Committee (1974d, p. 87).
[129] From Shultz's testimony of February 8, 1974, in Joint Economic Committee (1974d, p. 63).
[130] From Shultz's testimony of February 8, 1974, in Joint Economic Committee (1974d, p. 64).
[131] See Shultz's discussion in his testimony of February 8, 1974 (Joint Economic Committee, 1974d, pp. 63−65 and 88); quotation from page 65.
[132] Friedman (1975a, p. 42).

market and supporting the maintenance of exchange controls.[133] This remark prompted, presumably at Burns' instigation, a senior staff member of the Federal Reserve Board to write to Friedman (in a letter dated April 9, 1974) questioning Friedman's statement and asking for evidence.[134] As Friedman referred generally to the Federal Reserve and did not mention Burns by name, his column may have been referring principally to the Federal Reserve Bank of New York rather than the Board. The New York bank's president, Alfred Hayes, was a critic of floating exchange rates.[135]

As for Arthur Burns, he had been privately concerned about the liberalization of exchange-rate arrangements in 1971 (see Meltzer, 2009b, pp. 781–782), and in a public 1972 speech (Burns, 1972, p. 75), he had judged—without referring to Friedman by name—that "last fall's floating rates did not conform to the model usually sketched in academic writings," claiming that it had led to extra restrictions on trade and capital and increased uncertainty in the private sector about the economy and government policy.

However, Burns was far more upbeat about floating in his June 1973 Congressional testimony, stating that events since 1971 had increased his "degree of tolerance" of floating rates.[136] And, as seen above, by 1974 the Federal Reserve Board took objection to suggestions that Burns was hostile to floating. However, Burns' continued wariness about floating rates remained evident in subsequent years, such as in 1977 when he suggested that exchange-rate depreciation was a source of cost-push inflation. In addition, after he left office, Burns clashed with Friedman at a January 1980 conference about whether the floating-rate system had had the effect of increasing the vulnerability of the United States to international developments—with Burns arguing that it had done so (Feldstein, 1980, p. 95).[137]

Friedman, Shultz, and the introduction of floating

Friedman's assessment in March 1974 was: "The free market in exchange rates owes more to George Shultz's influence than to that of any other individual."[138] It would be going too far, however, to suggest that Shultz designed the floating-rate system that emerged in 1973 and that it

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[133] Friedman (1974k, p. 3) made a similar claim.
[134] The letter is in the Arthur Burns papers in the Gerald Ford Presidential Library.
[135] Hayes stepped down as bank president on August 1, 1975 (*https://www.newyorkfed.org/aboutthefed/AHayesbio.html*) and shortly thereafter called for exchange-rate floating to end: see *Daily News* (New York), September 1, 1975.
[136] From Burns' testimony of June 27, 1973, in Joint Economic Committee (1973a, p. 188).
[137] Burns' son, Joseph Burns, was, however, strongly in favor of floating (see Joseph Burns, 1979).
[138] Friedman (1974k, p. 3).

consequently emerged as part of a Shultz plan for international economic policy.

A contrary impression may be gleaned by some discussions of proposals for international monetary reform that Shultz outlined in September 1972. Shultz (2017, p. 283) stated that the 1972 plan "designed a floating exchange rate system in the clothing of a par value system" and Bordo (2018, p. 27) describes related Shultz plans of the period as aiming "to deliver exchange rate flexibility through the back door." In response to interpretations that this was a plan that ushered in the floating-rate system, two things need to be said. First, the plan was a *compromise* between fixed and floating rates, and it was therefore different from the floating-rate system that emerged in 1973. Second, it was a proposal that was well received but never implemented, being superseded by the crisis-induced widespread floating of early 1973.

It is also important not to overstate Friedman's personal role in the 1972−1973 emergence of floating.

It is true that numerous accounts have assigned Friedman a role as a prime mover. For example, Leeson (2003a, p. 174) pointed to accounts that appeared after 1972 that suggested that Friedman had composed the speech Shultz gave in September 1972 outlining his plan and indicated that Friedman in 1999 had denied such close involvement. Suggestions that Friedman was the architect of Shultz's plan were actually made as soon as Shultz announced it, with Arthur Laffer (who had returned to the University of Chicago after working in the Nixon Administration) contending that Shultz had consulted Friedman "every step of the way," with the result that the "proposal is pure Friedman" (*Newsweek*, October 9, 1972).

Friedman,however, immediately poured cold water on this suggestion, objecting that "Laffer grossly exaggerates the role which I personally played." Friedman acknowledged having spoken to Shultz several times and having seen him in Washington, D.C. (Instructional Dynamics Economics Cassette Tape 108, October 5, 1972). In his recent account, Shultz (2017, p. 284) was magnanimous in describing Friedman as a "big contributor" as a consultant (albeit an unpaid and unofficial one) to Shultz in 1972 on international monetary reform.[139] But Shultz also stressed other input he received on his proposal. Furthermore, as already indicated, the 1972 proposal was in fact never implemented.

[139] Shultz and Taylor (2020), which was in part a further account by Shultz of his interactions with Friedman during the Nixon Administration, concentrated on domestic economic policy.

Once—contrary to policymakers' plans—an authentic regime of floating rates materialized in 1973, the U.S. Treasury had in Shultz and his successor William Simon (1974–1977) a head who favored floating exchange rates. However, it would not be accurate to state, as Leeson (2003a, p. 4) did, that it amounted to a simple case in which "Treasury advisers wished to bury Bretton Woods," as this characterization overstates unanimity among senior Treasury figures on the matter. Official United States policy after the beginning of the March 1973 float was to work out a fixed-rate system that could succeed it.[140] Paul Volcker was the Treasury official primarily assigned to work out such a system. Until he left office in 1974, it would seem that Volcker sincerely wanted to replace floating with a new system and was not simply going through the motions in seeking alternatives. Friedman himself in March 1974 hinted that Volcker was an opponent of floating.[141] Likewise, around the time of his departure from the U.S. Treasury, Volcker remarked that the consensus his talks were establishing "certainly does not extend to maintaining a fully floating system permanently," and so did not coincide with the "American academic view" that favored continuation of floating.[142]

Even with his broad satisfaction that floating had been realized, one area in which Friedman was unable to gain ground with Shultz or other policymakers concerned swap agreements (short-term loans by central banks to each other). Friedman argued that the Federal Reserve should be prohibited by law from initiating swap agreements, which he likened to exchange-market intervention (*Newsweek*, April 23, 1973).[143] This recommendation made no headway, and swap agreements proved to be a permanent feature of the floating-rate era.[144]

Floating rates and the first oil shock

During the initial months following the first oil shock, Shultz commented: "There is no international financial arrangement which can offset the real effects of the oil price changes." (*Washington Star-News* (Washington, D.C.), February 15, 1974.)

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[140] See, for example, Shultz's testimony of February 8, 1974, in Joint Economic Committee (1974d, p. 67).
[141] Friedman (1974k, p. 2). The first half of 1974 saw U.S. foreign exchange controls wholly abolished. This was expedited from the original schedule under which controls would be phased out through December 1974 (*The Sun* (Baltimore), February 13, 1973). This was a reform that, in Friedman's view, made the likelihood of a return of fixed exchange rates remote (Instructional Dynamics Economics Cassette Tape 139, February 4, 1974; *Newsweek*, April 1, 1974). However, as indicated by the June 1973 quotation from Paul Volcker given above, Volcker hoped to institute an exchange-rate system that reconciled liberal exchange controls with limits on exchange-rate flexibility.
[142] From Volcker (1976a); quotations from pages 17 and 15, respectively.
[143] See also his testimony of June 21, 1973, in Joint Economic Committee (1973a, p. 117)
[144] Swap arrangements have been praised by such advocates of floating rates as John Taylor (see, for example, CSPAN, October 11, 2010).

The Secretary of the Treasury's judgment lined up with a longstanding Friedman message regarding the properties of different exchange-rate arrangements—and of floating rates in particular. Friedman's own analyses of the repercussions of the oil shock reaffirmed this message. As noted in Section II above, Friedman saw the increased cost of imported oil as bearing negatively on U.S. potential output, so it was a real shock to which the American economy had to adjust. But Friedman also stressed that the *contour* of the adverse reaction of potential output—its initial size and, especially, its magnitude and persistence beyond the period of the price increase—depended on U.S. policymakers' response to the oil shock. Friedman quickly became highly critical of the actual policy response, as the discussion below of his 1974 panel appearance with William Nordhaus will underline. Ahead of that, it is worth dwelling further on the floating-rate/oil-shock connection.

It would be little exaggeration to say that the only positive aspects Friedman saw in the U.S. policy response to the oil shock were in the monetary area: first, U.S. monetary policy did not accommodate the shock (see Section II above), and second, floating rates made advanced economies' adaptation to the shock better than it would otherwise be. With regard to the second aspect, Friedman would be laudatory of the role played by floating exchange rates as a shock absorber in the face of the OPEC price moves and the associated shift in international payments flows.[145]

The distinction between shock-*absorber* and shock-*nullifier* is significant. Over the years, it is true, some commentators would attribute to Friedman the position that a floating-rate system would provide comprehensive insulation and so be a shock-nullifier: that is, it would *cancel out* the effects on the domestic economy of an oil shock. In this characterization of Friedman's view, a floating rate could give an economy insulation with regard to all varieties of shocks originating from abroad, including real shocks such as the OPEC shock.[146]

However, as Friedman clearly indicated, what a floating-rate regime conferred to policymakers was more modest in character than this feature. In his advocacy of floating rates during the 1950s and 1960s, he had repeatedly stressed that a flexible exchange rate did *not* insulate an

[145] See Chapter 7.
[146] For example, Stanley Fischer suggested that the notion that "fiscal policies are transmitted internationally under flexible rates" contradicted the position taken by advocates of floating (Fischer, 1988a, p. 114) and also stated that the virtues of floating rates claimed by their proponents included the property that "flexible rates would insulate countries from foreign shocks" (Fischer, 1988b, p. 12). However, Fischer did not identify Friedman by name with either of these propositions. The association of these propositions with advocates of floating rates may have partly arisen from Johnson's (1969, p. 12) assertion: "The fundamental argument for flexible exchange rates is that they would allow countries autonomy with respect to their use of monetary, fiscal, and other policy instruments."

economy from real international shocks. The property secured by floating rates was the narrower but important property of allowing insulation of a country's *nominal money stock* from international factors.[147] This put the country's monetary authority, via its influence on the money stock—and hence important asset prices in the home economy—in a commanding position to influence the path of the nominal aggregate demand.

Consequently, from Friedman's perspective, a float positioned a country's monetary authorities to set its inflation rate over time differently from that prevailing in the rest of the world.[148] It also permitted the authorities to design a monetary policy that was more stabilizing for real economic activity than would be possible under a fixed-rate regime. In these circumstances, a float offered the opportunity to facilitate an economy's adjustment to a real shock, but it did not deliver complete insulation from that shock.[149]

This analysis implied that, for a country that possessed monetary autonomy—either that arising from a float or, as seemed to be more pertinent to Friedman in the case of the United States, arising from its size and monetary arrangements, whether it floated or not—the oil shock did not have lasting implications for inflation, provided that monetary growth was not affected. But, as Friedman granted that the oil shock affected the level of U.S. potential output, even his analysis implied that this shock had long-run implications for real economic activity. And the longer the higher oil price lasted, the longer-running the implications for the real economy.

In this area, Friedman found fault with U.S. policymakers—not principally with Shultz or Burns, but with the officials and federal agencies delegated the most immediate responsibility for the policy response to the oil shock.

WILLIAM NORDHAUS

Friedman's critique of the policy response to the energy situation would span several years but at an early stage would pit him in debate against Yale University's William Nordhaus. Like

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[147] Along these lines, when referring in their *Monetary History* to the United States' growing autonomy during the nineteenth century with respect to monetary conditions, Friedman and Schwartz (1963a, p. 9) observed: "The weakening of external links offered the possibility of insulating the domestic stock of money from external shocks."
[148] For example, in an appearance in Australia in April 1975, Friedman said of a float (Friedman, 1975g, p. 16): "That doesn't mean that what happens abroad doesn't affect Australia, of course it does…" He gave the OPEC price increase as an example before adding: "Changes that occur abroad will affect your real circumstances of the economy, but nothing that happens abroad will determine your rate of inflation—that will be determined at home."
[149] See Nelson (2020b) for analysis of Friedman's 1950s and 1960s writings on this point, especially Friedman (1953a) and Friedman and Roosa (1967).

Friedman, Nordhaus proved to be a future winner of the Nobel economics award. Unlike Friedman, however, in the 1970s Nordhaus was prepared to offer a partial defense of the national energy policies of which Friedman was so critical.

Friedman's free-market critique of U.S. energy policy

Friedman's indictment of the response of U.S. energy policy to the first oil shock—and, specifically, what he bluntly called the "stupid" federal government regulations on the pricing and allocation of oil and related products in the United States (*The Herald* (Chicago), March 18, 1974)—had discouraged U.S. oil production, held up consumers' demand for petroleum products, and fortified OPEC in its efforts to make the 1973−1974 oil price increase permanent.

Friedman's critique of the U.S. policy response to the oil shock was one he refined over time as he learned more about the energy market and as official policy itself evolved. But the overriding theme that he articulated over the years was consistent: he maintained that the free market was not being permitted to operate in response to the shock.

As already noted, Friedman had already pointed to U.S. government policy as having created energy shortages in the United States during 1973 even before the Middle East war; he had also discussed the oil market at length before 1973, as discussed presently. But once the first oil shock put the energy issue at center stage, the occasions on which Friedman articulated free-market recommendations in this area proliferated. Early instances included his October-November 1973 interventions already discussed in this chapter. Another was a talk on the energy crisis that Friedman gave at Princeton University on December 5, 1973, to a free-market student group, the Undergraduates for a Stable America.[150]

The following month, Friedman appeared at an event in Washington, D.C. on the matter: a National Conference on Government Responses to the Energy Crisis on January 24, 1974. The conference included a panel of economists: Friedman, MIT's Morris Adelman, and Nordhaus. The panel did bring out major areas of agreement on the energy situation. But it also highlighted the fact that, even though Friedman's emphasis on liberalization of the oil market became widely endorsed after U.S. energy price controls were finally removed in 1981, it was far from uniformly accepted among economists in the early years after the oil shock.

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[150] Information from Gloria Valentine, October 6, 2014.

"*Why Now*?"

In the course of a series of engagements in November-December 1973—including a lecture at Vanderbilt University in Nashville, Tennessee, on November 15, his visit to Princeton University on December 5, and an appearance at the National Association of Manufacturers conference in New York City on December 8—Friedman had criticized the federal government's response to the oil shock.[151] He particularly aimed his fire at the prospective next energy policy change, being heavily mooted at the time being, and for whose adoption the Nixon Administration was making preparations: gasoline rationing—that is, coupon-based restriction of the U.S. public's access to the retail supply of fuel. Gasoline rationing would be ridiculous, Friedman remarked in his Vanderbilt University appearance, but "just because something is ridiculous doesn't mean the government won't do it. The government does ridiculous things all the time." (*The Tennessean* (Nashville), November 16, 1973, p. 8.) He raised the prospect of the nation's police forces having to become involved in enforcing this rationing on a disgruntled general public: "Don't kid yourself," he remarked in Princeton, "the ultimate course in rationing—if it is taken seriously—is men with guns telling people what to do." (*Philadelphia Inquirer*, December 7, 1973.) Such, a scenario, he suggested in his New York City appearance, illustrated "why it is that the market, in a political sense, promotes freedom and government control promotes tyranny" (*Democrat and Chronicle* (Rochester, New York), December 9, 1973, p. 1C).

The alternative that Friedman offered, for crude oil and retail gasoline alike, was straightforward and was, of course, the same that he propounded before, during, and after the embargo period: a free price system, effected by the "total dismantlement" of controls on fuel pricing and allocation (*Chicago Tribune*, December 14, 1973). "Higher prices will clear the market," he observed in his New York talk (*Burlington Times-News* (North Carolina), December 26, 1973, page 5B). One benefit of this option, Friedman stressed, was that it would help economic adjustment by encouraging energy conservation in the U.S. population: "we will have 210 million people cooperating in the common interest, led by the price system" (*Philadelphia Inquirer*, December 7, 1973). "In the absence of controls, you can't have a shortage," he noted. "And if you eliminated the controls on oil products, it certainly would eliminate the need for gas[oline] rationing." (*San Diego Union*, December 9, 1973.)

In these interventions, Friedman also put the energy problem in the context of the broader

[151] See *The Tennessean* (Nashville, Tennessee), November 16, 1973; *Philadelphia Inquirer*, December 7, 1973; and *Democrat and Chronicle* (Rochester, New York), December 9, 1973.)

wage/price control system first introduced by President Nixon in August 1971. He acknowledged that the president's dramatic switch on that occasion did amount to a case of exercising leadership—but it was the "wrong kind of leadership," as it amounted to the abandonment of a preexisting economic policy that would have worked (*San Diego Union*, December 9, 1973). Lamenting the 1971 demise of the "steady as you go" policy in favor of a combination of controls plus aggregate-demand stimulation, Friedman observed: "Almost all of our problems stemmed from that move—the widened shortages and the very high inflation, for instance," Friedman remarked. (*Chicago Tribune*, December 14, 1973.)

In his December 1973 remarks to the press, as well as in his end-of-year *Newsweek* column "Why Now?" (December 31, 1973), Friedman cited the controls, not OPEC, as the basic reason why there was a problem of widespread shortages of energy and other products in the United States. His *Newsweek* column provided a precis on how the price system was designed to eliminate shortages while centralized price control tended to create them. More specifically, Friedman remarked to a reporter (*San Diego Union*, December 9, 1973): "The Middle East war, of course, has worsened the situation, but we were heading for exactly the same kind of energy shortage problem before the Middle East war broke out." (*San Diego Union*, December 9, 1973.) It was wrong to suggest, he insisted to the same reporter, that OPEC "decide[d] to create a crisis now. OPEC could not have created a shortage of the kind observed in the United States, including of non-oil energy products (*Boston Herald American*, December 13, 1973).

Friedman's Vanderbilt University remarks in opposition to rationing actually prompted a rebuttal by Nicholas Georgescu-Roegen—who had been a part-arbitrator, part-critic when asked to engage in Friedman's very first economic debate in print (that with A.C. Pigou) back in the mid-1930s.[152] Writing in a local newspaper, the Nashville-based Georgescu-Roegen remarked: "I have a great respect for Professor Milton Friedman's scholarly achievements… But… even Professor Friedman is not without fault." Georgescu-Roegen cited the emergency conditions prevailing as corresponding to a "case of real shortages of commodities which satisfy vital needs" and asserted that in this case "there is no solution other than rationing." (*The Tennessean* (Nashville, Tennessee), December 5, 1973.) For his part, Paul Samuelson contended around the same time that the U.S. public would simply not stand for a free price system for gasoline: "As soon as gasoline got towards 90 cents or $1 [in price] and Exxon started coming in with a tripling of profits, you would have blood in the streets." (*Chicago Daily News*, December 6, 1973.)

[152] For further discussion, see Nelson (2020a, Chapter 2).

But despite the lack of widespread support from other economists for a free-market solution to the energy problem, Friedman's overall perspective on price controls was gaining ground. Of the 1971 Nixon move to price controls, Friedman noted that "people today recognize how absurd it was" (*San Diego Union*, December 9, 1973). There was truth in this assessment: even before the oil crisis broke out, a report observed that Friedman had "a pretty good track record" in his assessments of macroeconomic development (*Kansas City Star*, August 12, 1973). There was much less talk like that seen when in early 1972, when a reporter contended that the "monetarist prescriptions for the economy ended up a bust last year" (*Sunday Times Advertiser* (Trenton, New Jersey), March 5, 1972) and next to no repetition of the proclamations like that columnist Hobart Rowen had made in late 1972, titled "Wage-Price Controls Working," that the practice of controls had produced "a much different picture" from the gloomy outcomes Friedman had predicted (*Boston Globe*, November 5, 1972). That said, the reaction of many economists to the failure of wage and price controls in 1973−1974 was, as discussed in adjacent chapters, not to recommend Friedman's prescriptions but, instead, to propose variants of the nonmonetary remedy to inflation—in particular, other forms of incomes policy that did not entail full-scale mandatory restrictions on wages and prices.

MIT perspectives

In his *Newsweek* column of December 10, 1973, Friedman claimed that there was "wide agreement" that letting "the free market reign" was the most efficient way of responding to the oil shock. In fact, however, as already suggested, the period 1973−1974 saw considerable resistance even among economists to a market-based means of dealing with the oil problem. True, Kenneth Arrow and Friedman came close to putting out a joint statement in late 1973 favoring deregulation of U.S. retail gasoline prices. But the lack of consensus on the matter among leading economists was underscored by Paul Samuelson's declaration in his own *Newsweek* column of November 26, 1973: "Gasoline and fuel oil should definitely be rationed."

Samuelson's position clearly rankled with Friedman. In teaching his Price Theory class at the University of Chicago in 1973/1974 Friedman would point to Samuelson's *Newsweek* analysis of the energy situation as something that could be criticized as faulty positive economics (Charles Plosser, interview, April 2, 2015).

As it happened, the rationing of gasoline that Samuelson recommended was one intervention that was *not* implemented at the national level in the United States in the face of the oil crisis. Although preparations were made for it in 1973, nationwide rationing did not go into effect. So,

in contrast to countries like the United Kingdom, the provision of petroleum to U.S. motorists was not subject to official, comprehensive physical rationing. The fact that rationing was not imposed may have been a factor in Friedman's retrospective judgment (*New York Times Magazine*, December 27, 1987, p. 37) that William Simon, the Nixon Administration's "energy czar" in 1973–1974, made "a bad policy—price controls on oil—run relatively unbadly." Nonetheless, as Friedman observed at the January 1974 panel on energy policy, it was not accurate to suggest that the United States had not responded to the oil price increase with rationing, as the federal government had imposed controls on allocation and production in the wholesale market for oil—controls that continued over the rest of the 1970s.

If Samuelson's response to the oil crisis was discouraging to Friedman, he could take comfort from the posture taken by Samuelson's MIT departmental colleague Morris Adelman, with whom Friedman and Nordhaus shared the stage at the January 1974 conference. In recalling the contributions of Adelman and Friedman to the debate on oil, Adelman's longtime colleague Robert Solow noted: "I think that they were somewhat on the same side." (Robert Solow, interview, December 2, 2013.) Notably, in comments made soon after the outbreak of the Middle East war, Friedman observed that the "person who has taken this position [on oil] most consistently—with a greater degree of expert knowledge than I have; mine is completely superficial knowledge—is a fellow by the name of Morris Adelman, who is a professor at MIT." (Instructional Dynamics Economics Cassette Tape 131, October 10, 1973.) In particular, Friedman's remark on that occasion that "we have enormous reserves of oil" echoed a longstanding Adelman point.[153]

More generally, Adelman's and Friedman's analyses had considerable overlap in stressing weaknesses in the underlying position of OPEC and in being critical of U.S. energy policy.[154] In their January 1974 joint appearance, Adelman expressed numerous observations highly consistent with Friedman's sentiments. *Inter alia*, Adelman remarked that "every fact we know about oil supply [suggests] it's hugely elastic" and "one thing is certain: that, with current prices ruling, oil is going to be coming out from behind the woodwork all over the world."

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[153] See, for example, Adelman (1972) and *Wall Street Journal*, February 9, 1973. Samuelson (in *Newsweek*, November 26, 1973) also referred to and endorsed Adelman's finding that "known oil reserves are up, not down."
[154] Some evidence of Friedman's analysis of the oil market itself having an influence on Adelman came in the fact that Friedman in his January 1974 appearance put the energy crisis in perspective by quoting at length from William Stanley Jevons' (1865) fear—as it turned out, unrealized—regarding an approaching energy shortage (a use of Jevons' work that he would repeat in Friedman, 1983d, pp. 141–142). Adelman (1975) would likewise open with this Jevons (1865) passage.

Consequently, he proclaimed, OPEC "will undoubtedly crumble, as all cartels have done…"[155]

It is therefore not surprising that, during the 1970s, some commentators on the energy debate categorized Friedman and Adelman as members of the same camp. For example, a paper for a conference held on the third anniversary of the oil shock (Issawi, 1977, p. 91) observed scornfully that "many economists of the Morris Adelman-Milton Friedman school are still waiting for it [the oil price] to fall to its marginal cost of production, say 20–30 cents a barrel, which is like waiting for Godot…"[156]

Appearing alongside Friedman and Adelman at the January 1974 symposium, William Nordhaus himself alluded to the similarities between the Adelman and Friedman positions. When he spoke third in their joint appearance on the economists' panel, Nordhaus remarked drily: "Well, we've heard several invigorating lectures today on the miracle of the price system. As the last panelist, it's probably appropriate [for me] to turn to the long-run energy problem."

But Friedman and Adelman were not interchangeable when it came to their views on energy policy. Adelman was less inclined than Friedman to advocate a wholly noninterventionist policy response to the oil shock. In his January 1974 symposium contribution, Adelman suggested that OPEC might be able to maintain discipline for years and that the U.S. government should take active steps to accelerate the group's collapse. These suggestions prompted Friedman to imply that such activism was unnecessary: "I am much more optimistic than Morrie Adelman is about how rapidly the oil cartel is going to find that they are left holding the bag." Also at the January 1974 symposium, Adelman advocated government financing of, or concessions for, research into alternative energy sources, while Friedman in the same discussion rejected both public funding and tax concessions for such research. Furthermore, between mid-1974 to mid-1979 (that is, after the embargo had been lifted but OPEC's oil price increase of 1973–1974 remained), Adelman advocated U.S. government restrictions, in the form of quotas or tariffs, on importation of oil from OPEC countries as a means of squeezing the cartel.[157] Friedman, in contrast, repeatedly opposed such trade restrictions.

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[155] From Adelman's remarks in the symposium *Long-Term Energy Crisis Solutions*, held at the National Conference on Government Responses to the Energy Crisis, January 24, 1974.

[156] Less explicitly—because he did not name names—Paul Samuelson had likewise remarked about two years after the first oil shock: "I'm not one of those economists who are constantly predicting with great confidence that the OPEC monopoly will break down or will break down soon." (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 184, August 1975.)

[157] See, for example, Adelman's testimony (of January 29, 1975) in Committee on Foreign Relations, U.S. Senate (1975, p. 4) and Adelman's remarks in Mitchell (1979, pp. 23–24). See also *The Plain Dealer* (Cleveland, Ohio), February 1, 1975.

Consequently, and especially in light of his high-profile remarks on OPEC, it was principally Friedman who was associated with free-market solutions to the energy crisis, and Friedman who took most of the criticism for having been excessively overoptimistic about an OPEC collapse.

And it was against Friedman rather than Adelman that William Nordhaus was primarily pitted in the January 1974 symposium. As well as representing sharply different positions on the appropriate role of the public sector in response to the oil shock, Nordhaus and Friedman were from different generations. Nordhaus was born in 1941, six years after Friedman started publishing in economics. This generation gap did not prevent Nordhaus—in the course of his lively exchange with Friedman—from remarking that Friedman reminded him of students he had who would not listen properly to what Nordhaus was saying to them.[158]

Friedman's indictment of pre-1973 U.S. energy policy

Although Friedman could not claim to have the depth of knowledge of oil possessed by his fellow panelists in the January 1974 symposium—"I have much less expertise in that area than he does," Friedman noted when he followed Adelman on the panel—he had in fact written on the oil market for over a decade prior to the oil shock. In these earlier discussions, as in 1973, Friedman viewed the operation of the energy market as frustrated by market forces. For example, in *Capitalism and Freedom* he had stated that the United States should not use its government to protect a group of oil producers.[159] His *Newsweek* column of June 26, 1967 on the oil market stated that U.S. oil producers, more than almost any producers, "rely so heavily on special government favors." Likewise, in early 1969, he described oil as a highly sheltered product (Instructional Dynamics Economics Cassette Tape 15, February 1969). Indeed, Friedman and Adelman were both invited to appear at a March 1969 hearings of the U.S. Senate Committee on the Judiciary to make the case for much-reduced government intervention in the oil market, but in the event only Adelman was available to testify.[160]

A number of specific items were included in Friedman's bill of charges in his indictment of U.S. energy policy. Prior to its abolition in April 1973, he repeatedly criticized the quota on oil

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[158] Other statements around this time by Nordhaus about his students were more flattering. Most notably, his 1973 study of U.S. energy resources acknowledged that "Paul Krugman [an undergraduate student at Yale University] provided research assistance extraordinary" (p. 529).

[159] Friedman (1962a, p. 139).

[160] See Friedman's letter (dated February 26, 1969) in Committee on the Judiciary, U.S. Senate (1969, p. 6) as well as Adelman's testimony (of March 11, 1969) beginning on the same page. See also the mention in U.S. senators' discussion (on April 1, 1969) of Friedman and Adelman as advocates of a freer market in oil (see page 305 of the same volume).

imports; this had been imposed by President Eisenhower (citing national-security reasons) in 1959.[161] Then controls on U.S. consumer prices on gasoline and crude oil had been imposed as part of the economy-wide August 1971 wage-price controls. It is against this background that, when the first oil shock arrived, Friedman cited "government price-fixing and government mismanagement" as having created an energy problem before the Middle East war (Instructional Dynamics Economics Cassette Tape 134, November 21, 1973). This characterization contrasted sharply with Nordhaus' (1973, p. 530) contention, expressed at roughly the same time, that U.S. "public policy [has] accepted a *laissez-faire* approach to resource pricing."[162]

Furthermore, when the Nixon general price controls were lifted at the end of April 1974, the controls on prices of petroleum products remained in force (*The Evening Bulletin* (Philadelphia), July 19, 1974).[163] What is more, as already implied, the U.S. government reacted to the OPEC shock by imposing a variety of new controls on prices and quantities in the U.S. wholesale market for oil.[164]

In his contribution to the January 1974 panel, however, William Nordhaus provided a defense of government intervention in the market. "While I generally agree with some of the panelists about the difficulty associated with unrealistic controls," Nordhaus remarked, "I would not care to associate myself with their evangelical zeal for the free market's virtues." However, like Friedman, Nordhaus did warn against government policies that were premised on continuation of present oil prices; indeed, the likely outcome Nordhaus saw was not unlike that sketched by Friedman and Adelman, with demand and supply reactions expected to drive oil prices sharply lower by 1980.[165]

This was a standard view at the time. Many disagreed with Friedman's predictions of a quite-

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[161] On the details of the quota and its rationale, see Rieber (1961). On its abolition, see Bohi and Russell (1977, p. 2) and Friedman (1975a, p. 310). Friedman's criticisms of the quota included those in *Newsweek*, June 26, 1967, *Chicago Daily News*, July 29, 1970, and Friedman (1983d, pp. 147–148). Another development in U.S. energy policy in the 1950s that Friedman criticized was the introduction of controls on natural gas prices. See, for example, *Newsweek*, June 13, 1977, *Milton Friedman Speaks*, Episode 12, "Who Protects the Consumer?," taped September 12, 1977, p. 26 of transcript, and Friedman (1983d, p. 148), as well as Chapter 8 below.

[162] Nordhaus' (1973) paper was evidently completed in late January 1974 (see p. 556).

[163] See also *Milton Friedman Speaks*, Episode 2, "Myths That Conceal Reality," taped October 13, 1977 (pp. 30–31 of transcript), Friedman (1983d, p. 148), and Friedman and Friedman (1980, p. 219).

[164] Friedman's critiques of these controls on the wholesale market (including the "old oil/new oil" distinction introduced by the federal regulators) is analyzed in Chapter 6.

[165] Nordhaus also stressed, as Friedman often did (see Friedman, 1983d) that, beyond the boycott period, physical unavailability of oil was not in prospect for the United States: oil would be available to the country in the amounts it demanded, the question was at what price. However, he parted company with Adelman's and Friedman's predictions about an eventual breakup of OPEC: "I guess I don't share the faith in the powers of competitive forces to erode powerful monopoly positions."

rapid OPEC breakdown. But the basic belief that the oil price would fall back by the end of the decade was widely shared—and contrasted sharply with the eventual outcome, which was a second oil shock in 1979−1980. For example, a few weeks after Nordhaus' January 1974 remarks, Secretary of the Treasury Shultz urged his interlocutors: "ask yourself, is it likely that the price of oil will be higher in 1980 than it is now? And I think the answer to that is very, very clear: No, because of the great surges in supply that are coming from sources of oil and other energy…"[166]

But what should be done in the interim? Nordhaus argued in his remarks that, during the period before oil prices fell back, federally-imposed price and quantity controls on oil "will smooth the transition from one regime to the next." Nordhaus also saw a long-run role for the government in providing stockpiles of oil to protect against future oil shocks. He further insisted that the private sector would not do adequate research-and-development programs for alternative energy sources: "I think substantial federal involvement is called for," Nordhaus contended. The private sector left to itself, he suggested, would devote insufficient resources to the high-risk investments that could generate technological advances. "These are areas where the theorems about the price system that Professor Friedman invokes simply do not apply."

Those advocating a more interventionist policy response could also point out that the free-market positions that Friedman had advocated in the 1960s on oil jarred somewhat with later developments. For example, his June 1967 *Newsweek* column had criticized the national-security basis for the oil cartel and had poured cold water on the notion of an oil embargo: "the Arabian countries themselves cannot refuse to sell for long." In fairness, it needs reiterating that the 1973−1974 oil *embargo* itself did not last long, even though the oil price increase that accompanied it was permanent.[167] But Friedman had in 1967 also not counted on the emergence of OPEC as a cohesive force, as his column had argued that the "world oil industry is highly competitive and far-flung and getting more so."[168] Furthermore, his pre-1973 criticisms of U.S. energy policy—particularly the import quota and the "percentage depletion allowance," a tax deduction for the U.S. oil industry—had made the case that the United States produced too much

[166] From Shultz's remarks of February 8, 1974, in Joint Economic Committee (1974d, p. 75).
[167] This (together with another hint that OPEC would soon collapse) was essentially the basis on which Friedman defended his 1967 analysis when he revisited that analysis in July 1974 (see Friedman, 1975a, p. 310).
[168] Morris Adelman was in the similar position of being critical of pre-1973 energy policy but also surprised by the oil shock. On March 11, 1969, he had testified (Committee on the Judiciary, U.S. Senate, 1969, p. 7): "The basic fact about the world oil industry is that there will be ample supply at less than current prices for 15 or more years." In remarks on January 29, 1975, Adelman acknowledged (Committee on Foreign Relations, U.S. Senate, 1975, p. 18): "I think there is no precedent for the current oil monopoly. Certainly the discrepancy between price and costs... is an order of magnitude greater than anything I have ever seen."

oil and imported too little, and that it should seek the lower petroleum prices available from freer trade.[169] As Herbert Stein was able to point out in a televised debate with Friedman (*University of Chicago Round Table: The Nation's Economy Out of Control*, PBS, May 1, 1974), the pre-1973 control apparatus quite likely put the United States in a better position than otherwise to handle the immediate implications of the OPEC shock, by making the country less reliant on foreign sources of oil.[170]

For Friedman, however, a free market would have been preferable both before and after 1973 because the price system gave the U.S. economy more dexterity in reacting to world developments. It would have been desirable, he contended, for the United States to have benefited fully from the low world energy prices prevailing before 1973.[171] And, in the wake of the OPEC shock, Friedman rejected the Nordhaus position that a public-sector role was needed in the storage of oil.[172] "All experience shows that men love lotteries," Friedman said, a contention that was the basis for his view that incentives existed for private-sector producers to store oil in anticipation of windfalls arising from future price surges.[173] In addition, Friedman insisted that economically beneficial technological advance had predominantly come from the private sector. And, again in contrast to Nordhaus' position, Friedman was adamant that the system of federal controls in force would not aid the United States' adjustment to the OPEC price increase.

Urging decontrol of retail prices

From the onset of the first oil shock, Friedman especially stressed the need for decontrol of retail gasoline prices to send the correct price signal to consumers. He did, however, oppose going further on the discouragement of consumption by increasing federal taxes on gasoline. Friedman differed from Secretary Shultz on this score (Instructional Dynamics Economics Cassette Tape 134, November 21, 1973).[174] A new gasoline tax, Friedman argued, would send signals that were at cross-purposes with one another. The revenue arising from the tax would not go to suppliers,

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[169] See, in particular, his remarks in *Chicago Daily News*, July 29, 1970, and in Friedman (1970b, p. 437).
[170] Similarly, Bohi and Russell (1977, p. 17) argued that the "quota and production controls [in force as of 1973]… saved the United States economy from making even more drastic adjustments to changes in the world price."
[171] See Friedman (1983d, pp. 147−148).
[172] In his Congressional testimony of January 29, 1975, Adelman likewise endorsed the need for a federal agency to be created to stockpile oil (Committee on Foreign Relations, U.S. Senate, 1975, p. 18).
[173] From Friedman's panel appearance of January 24, 1974. Friedman's opposition to public-sector stockpiling programs echoed his discussion in Friedman (1954b) of similar proposals applied to the agricultural sector.
[174] Matusow (1998, p. 266) also quoted what he described as a memorandum (actually, simply a letter—available in the Friedman archives at the Hoover Institution), dated November 16, 1973, in which Friedman laid out to Shultz his opposition to a federal gasoline tax.

so the price rise generated by the tax would discourage demand without boosting energy production (*Newsweek*, May 2, 1977*a*). Instead, he simply favored ending the official controls on energy prices. The resulting price signal, he believed, would result in a substantial shift of demand away from oil.

In time, a very large adjustment in demand would occur: growth in U.S. consumption of petroleum products moved from 4.5 percent per year in 1945−1973 to 0.5 percent per year on average in 1973–2006.[175]

This shift showed little signs of emerging in the initial years following the oil shock. Friedman's explanation for this included, as already noted, the likelihood that the sizable elasticity of demand for energy manifested itself only gradually. But this was only one part of the explanation, and it became less important over time. In 1978, Friedman cited the refusal of the federal authorities to permit the prices of U.S. petroleum products to reflect fully the global rise in oil prices as the central factor behind the absence of an appreciable drop in American energy consumption in the years following 1973—a consumption pattern that contrasted with that in other major countries.[176] The price control, Friedman felt, prevented a sharp reaction of U.S. consumer demand—one that would have put greater pressure on the OPEC cartel.

And on the supply side, Friedman would note that, ultimately, the oil shock "led to a vast increase in oil produced outside of the OPEC countries" (*The MacNeil/Lehrer News Hour*, PBS, April 17, 1987, p. 5 of transcript).[177] But he felt that this supply reaction, as well as the development of substitutes for oil, had been greatly slowed down by the U.S. energy policies of the 1970s.[178]

As for William Nordhaus, his role in energy policy became an official one as a member of the Carter Administration's Council of Economic Advisers in 1977−1979, with his focus being on energy policy issues. Once back outside the official sector, Nordhaus assessed successive U.S. administration's response to the oil shocks and came out negatively. "I think the quality, not of the people, but of the actual decisions that have been made by the federal government over the last seven years is extremely poor," Nordhaus observed in 1980, "and has actually exacerbated

[175] See Alan Greenspan's testimony of June 7, 2006, in Committee on Foreign Relations, U.S. Senate (2007, p. 6).
[176] See Friedman (1983d, pp. 146–147). See also *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, page 14 of transcript.
[177] Similarly, Friedman (2000, p. ix) implied that OPEC was not able to protect itself against competition.
[178] See Chapter 5 and subsequent chapters below.

the problem." Whereas in his 1974 panel appearance with Friedman, Nordhaus had cited insufficient stockpiling as a shortcoming of market mechanisms, his 1980 assessment was that "private institutions find it easier to deal with shortages, stockpiles, and allocations than do public institutions."[179] And, in contrast to his 1974 cautious endorsement of oil price controls, Nordhaus now stated that "decontrol of oil prices is the absolute precondition for rational behavior during emergencies."[180]

Looking back on Friedman's stance on energy policy during the 1970s, Nordhaus would observe: "Basically his was a view that free markets are very powerful and regulation represents capture (which was not quite right on energy). He was right (more than I thought at the time) about the long run, but underestimated the lengths of the lags between prices and outputs. In one sense, the shale revolution today is the result of the deregulation that Milton Friedman pursued [and which was subsequently implemented] in 1981." (William Nordhaus, personal communication, December 8, 2014.)

Nordhaus and debates on inflation and monetary policy in the 1970s

Another activity alongside energy policy in which Nordhaus was highly engaged during the 1970s was the modeling of the behavior of aggregate U.S. prices and costs. His research output advanced criticism of the monetarist view of inflation—a criticism that drew a public response from Friedman.

The paper that gave rise to this response, Nordhaus (1972a), was prepared for the September 14–15, 1972 meeting of the Brookings Institution's panel on economic activity. Nordhaus investigated the surge in nominal wage growth that had occurred across OECD countries in recent years. In judging the validity of this paper's findings, a distinction needs to be drawn between its recording of an empirical regularity, on the one hand, and the conclusions that Nordhaus drew for monetarism from his analysis, on the other.

The *regularity* that there was a surge in nominal wage growth in the early 1970s across advanced economies became widely accepted, as was the label that Nordhaus gave to that regularity in the title of his paper: "The Worldwide Wage Explosion." Nordhaus' documentation of this regularity was an enduring contribution of the paper. Attesting to this, forty-five years after the

[179] From Nordhaus' testimony of November 11, 1980, in Committee on Finance, U.S. Senate (1980, p. 28).
[180] From Nordhaus' testimony of November 11, 1980, in Committee on Finance, U.S. Senate (1980, p. 29).

paper's publication the International Monetary Fund (2017, p. 143) referred to the "uptick [in labor's share of income] around 1970 coinciding with the 'worldwide wage explosion' (Nordhaus 1972[a])."

However, Nordhaus' conclusion in the same paper about the merits of the monetary explanation of inflation has not proved enduring and is contrary to the modern consensus on the matter. Nordhaus (1972a) endeavored to use recent years' international patterns of nominal wage growth to make conclusions about monetarism's proposition about inflation. Nordhaus specifically cited, and took issue with, Friedman's 1966 paper "What Price Guideposts?," which had criticized wage-push views of inflation—and which had been one of the earliest instances in which Friedman used in print his dictum that "inflation is always and everywhere a monetary phenomenon"—a dictum Nordhaus (1972a) quoted.[181]

Nordhaus' empirical approach was based on the contention that this dictum implied a unitary connection between nominal wage growth and monetary growth. And such a connection, Nordhaus suggested, was not present in the data: when estimating nominal-wage or nominal-wage-growth equations for seven countries, Nordhaus (1972a, pp. 437–439) found that nominal money was statistically insignificant in the case of all countries except the United States, and in the U.S. case money had a coefficient of 0.485 rather than being closer to 1.0.[182]
In 1977, Friedman responded to the Nordhaus' (1972a) finding. Friedman's first and primary point—that Nordhaus did "not even consider price inflation but only wage inflation" (*The Times* (London), May 2, 1977)—is considered shortly. Before the discussion of that point, it is worth laying out the reasons why, even when one takes nominal wage growth as informative about inflation, Nordhaus' (1972a) results did not amount to a convincing and valid finding against Friedman's monetary view of inflation.

It should be said first that Nordhaus' equation evaluating Friedman's views, which regressed nominal wage growth directly on nominal monetary growth, constituted a reduced-form specification rather than an attempt to describe the structural equation for wage- or price-setting associated with Friedman's analysis. The Nordhaus equation combined monetary policy's influences on aggregate demand with the response of wages to the output gap, and it is the output-gap/wage link that would be the structural equation in Friedman's work. Nordhaus (1972a), as well as his earlier analysis in Nordhaus' (1972b), did not dispute the influence of the

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[181] See Friedman (1966a) and Nordhaus (1972a, p. 418).
[182] Nordhaus also included productivity or its growth rate in the estimated specifications.

output gap on wage inflation, at least for the United States. More direct investigation of the relationship between resource gaps and wage inflation for the countries in Nordhaus' (1972a) sample found that there was indeed a positive influence of such gaps (see Laidler, 1976b, and, for an extended sample, Grubb, David, Layard, and Symons, 1984).[183]

Once it is granted that aggregate demand matters for wage growth, the question then arises why Nordhaus found that monetary growth and wage growth were generally unrelated. Friedman's contentions that monetary policy was the decisive aggregate-demand tool, and that monetary growth was a useful summary of how monetary policy mattered for aggregate demand, suggests that monetary growth should have been useful in predicting nominal wage growth (especially if nominal wage growth was a reasonable stand-in for the percentage increase in prices). A likely resolution to the puzzle lies in the fact that Nordhaus (1972a) neglected the lag between monetary growth and inflation. He claimed to allow for "the customary six-month lag between monetary impulse and the change in income," by putting six-months-earlier money rather than completely-current-year money on the right-hand-side of his nominal-wage equations (Nordhaus, 1972a, p. 437). But when Friedman wrote of a six-month lag, it was between monetary growth and growth in aggregate (real and nominal) real income. By 1972, Friedman saw the lag between monetary growth and inflation as much longer—about eighteen months to two years, on average.[184] In contrast, by allowing only a six-month lag, Nordhaus was essentially evaluating a static relationship between monetary growth and nominal wage growth.

Consequently, Friedman was able to conclude that Nordhaus (1972a) "in no way contradicts" the proposition that monetary policy affects inflation with a roughly two-year lag. He also noted that Nordhaus' results came from "data for a brief period (1956–1971)" and that longer samples would likely bring out more clearly the validity of the monetarist view of inflation (*The Times* (London), May 2, 1977). Consistent with Friedman's emphasis on both the two-year lag and on the value of longer samples, Gordon (1981, p. 33) observed: "As further support for the monetarist case, the [wage] equations for the United Kingdom clearly indict the Bank of England as the major culprit for the 1974–76 British wage explosion."

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[183] Grubb, Layard, and Symons (1984) estimated an augmented Phillips curve. Laidler (1976b), concentrating on pre-1970 data, did not include an expectations term, but he nevertheless found generally significant gap terms in equations for nominal wage growth. In Nordhaus (1972a, pp. 445–446), the expectational Phillips curve was found to perform badly for France, Germany, and the United Kingdom. In retrospect, however, this finding can be seen as a result of not adequately taking into account rises in the natural rate of unemployment in the late 1960s and early 1970s in the United Kingdom and in Continental Europe.
[184] See Nelson (2020b, Chapter 15).

Monetary policy and the markup

All in all, then, Nordhaus' (1972a, p. 436) contention that the empirical evidence contradicted the notion that "the recent wage explosion should be explicable on monetarist grounds" was not sound. Reasons have been given above why his rejection of Friedman's proposition that "inflation is always and everywhere a monetary phenomenon" was unwarranted. But there was another crucial reason: Friedman's proposition referred to price inflation, not wage inflation. And for Friedman, the connection between variations of nominal wage growth and inflation was by no means as close, automatic, or obvious as Nordhaus' and many other Keynesian analyses supposed.

To be sure, Nordhaus was correct to suggest that Friedman's monetarist framework predicted that nominal wage growth was endogenous—and, in particular, was responsive to monetary policy actions. Furthermore, in Friedman's framework expansions of aggregate demand do, other things equal, give rise to rates of increase in goods prices and in nominal wages (adjusted for productivity) that are equal to each other.[185] But Friedman regarded the short-term relationship between inflation and wage growth as potentially very loose—in contrast to Nordhaus' (1972a, p. 437) attribution to Friedman of the belief that real wage growth followed a smooth trend. And very importantly, Friedman saw monetary policy as exerting reliable influences on inflation *even when* wage growth was exhibiting short-run divergences from nominal wage growth. The way Friedman put it in April 1975 was that "the essence of an inflationary process is that both prices and costs go up"; "sometimes in the process one [costs] goes ahead and at other times [prices] go ahead."[186] When prices "go ahead," in this account, they still reflect monetary influences.

One way of characterizing this line of argument is that—in contrast to static constant-markup models of the kind Nordhaus (1972a) used—Friedman thought of monetary policy as capable of exerting an influence on the markup of prices over marginal cost. In particular, excessive monetary growth could be manifested in higher inflation even if the monetary excess did not initially show up much in higher nominal wage growth: in response to monetary easings, markups could increase or rise at a higher rate. And restrictive monetary policy could reduce inflation or hold it down even when the restriction was not showing up in slower nominal wage growth: markups could fall or increase at a slower rate. An unlikely source of support for this

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[185] See, for example, Friedman (1976b, pp. 222–223).
[186] Friedman (1975h, p. 12).

Friedman position came during 1974 from James Tobin, who observed (Tobin, 1974, p. 228): "If the Fed were willing to starve the economy for liquidity, regardless of the consequences for real output and employment, presumably price indexes could be held down even when unit labor costs are rising..."

The relationship between prices and costs did have an important role in Friedman's aggregate supply framework, with perceptions of prices in relation to labor costs being an inducement for suppliers to produce in that framework, and so being part of the behavior underlying the expectations-augmented Phillips curve that governed the rate of increase in prices.[187] This role underlies the fact that it is perfectly possible for narratives of price behavior that view prices in terms of a—possibly varying—markup over marginal costs to be consistent with an explanation of inflation in terms of monetary variables.[188]

Friedman, however, did not take this route in his empirical work. He was certainly familiar with analyses that focused on markup behavior, especially because of his exposure to the work of Wesley Mitchell.[189] But, as discussed in the previous chapter, in practice Friedman did not find it fruitful to look to cost behavior for understanding the course of aggregate prices. As an analytical matter, he undoubtedly saw a danger that taking such a posture would increase the danger of lapsing into analyses in which the inflation process was portrayed as featuring unidirectional causation from costs to prices. This was certainly how cost-push analysts viewed the process. Seeing things in costs-determine-prices terms was also a trap into which analysts who appropriately recognized the endogeneity of factor prices might also fall.[190]

And, as an empirical matter, Friedman saw the dynamic cost/price relationship as much looser than was usually claimed by those deploying a markup-oriented analytical framework.[191]

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[187] Dimand (2018, p. 7) claims that Friedman' (1968a) Phillips-curve theory "implies countercyclical movement of real wages." But he provides no source in Friedman's writings for this claim, and his characterization flies in the face of Friedman's (1972a, p. 930) explicit discussion of the fact that his framework did not have this specific implication. Friedman stressed that his framework, in which expected real wages matter for workers, can imply that actual real wages can have either a positive or negative relationship with employment.

[188] See Nelson (2003, p. 1040) for further discussion.

[189] Notably, Rotemberg and Woodford (1999, pp. 1053–1054) motivated their work on the markup by pointing to Wesley Mitchell's emphasis on the regularity that costs rose more than prices late in expansions as profit margins narrowed, as well as the implications for aggregate economic fluctuations that Mitchell drew from this regularity. Friedman had discussed the same aspect of Mitchell's thinking in Friedman (1950, pp. 482–483).

[190] As will be seen below, this concern remains valid in modern times. New Keynesian analysis (in which costs and prices are properly viewed as jointly determined) has often been presented in shorthand form as implying that costs determine prices.

[191] Rasche (1976, p. 307): observed: "My impression is that this assumption [a constant markup] is regarded by many monetarists as the ultimate 'Keynesian' heresy…" Not all the inferences Rasche associated with this

Friedman's approach to inflation analysis concentrated on reduced-form relationships between monetary growth and inflation. This provided a way of avoiding structural modeling of the markup relationship, while also embedding his confidence that monetary policy actions showed through in price inflation under a variety of scenarios.

Gordon (1981, p. 4) attempted to encapsulate the monetarist position on inflation by observing that, according to that view, " 'wage-push' by unions... may be able to influence the unemployment rate or the distribution of income, but not the inflation rate." Although this characterization reflected the basic flavor of Friedman's analysis in important respects, it needs to be qualified in two respects.

First, Friedman granted that the labor share of income, and conversely the markup are ultimately insensitive to monetary policy, thanks to the natural rate hypothesis. The markup, being a ratio of one nominal variable to another variable, was a real variable, and so its steady-state value was invariant to monetary actions.[192] Therefore, while the markup could be subject to permanent shifts due to real factors, it was insensitive to monetary policy in the long run.

Second, for given values of the natural rates of output and unemployment, Friedman believed that wage-push, like other cost-push factors, was not a systematic source of upward pressure on prices and costs: apparent upward pressure on wages from wage-push in one sector of the labor market would be offset by downward pressure in other sectors, though the downward pressure might be spread over a number of periods.[193]

Friedman's analysis can be reconciled with New Keynesian models' perspective on the relationship between marginal costs and prices, though it differs from many narratives of New Keynesian analysis in the choice of what reactions should be emphasized. Standard New Keynesian analysis links the current price level to its lagged value and to the expected stream of current and future nominal marginal cost. This in turn implies (see Galí and Gertler, 1999, and Sbordone, 2002) a forward-looking expression for inflation dynamics that can be expressed either as $\pi_t = \alpha \cdot rmct + \beta E_t \pi_{t+1}$ or as $\pi_t = \alpha \cdot E_t \Sigma\, \beta^i rmc_{t+i}$, where $\alpha > 0$, β is a discount factor near

assumption were correct (in particular, contrary to his discussion, a constant-markup view need not imply a wage-push view of inflation; and even monetarist accounts of inflation required a *long-run* proportionality between nominal wages and prices, *ceteris paribus*). But Rasche's basic impression was well founded: in monetarist accounts, pressures on prices arising from monetary policy actions worked partly via the markup.

[192] See Friedman (1970a, p. 211) and Friedman and Schwartz (1982, p. 50).

[193] See Friedman (1951b, 1966) as well as the analysis in Nelson (2020a, Chapters 7 and 10).

1.0, rmc_t is the log of real marginal cost (which in the simplest case corresponds to the log of real unit labor cost), and where any constant terms on both sides are suppressed.[194]

Usually, expositions of New Keynesian analysis take a positive shock to the current labor-cost term $rmct$ as affecting the expected stream $E_t\Sigma\ \beta^i rmc_{t+i}$ in the same direction and so as raising inflation. This perspective perhaps underlay Galí and Gertler's (1999, p. 214) statement—not actually justified by their New Keynesian analysis, in which marginal cost and inflation are determined simultaneously—that "in our model, causation runs from marginal cost [to] inflation."[195]

But Friedman's analysis instead emphasized different notions, which can themselves be reconciled with the New Keynesian inflation equation. In terms of the $\pi_t = \alpha \cdot rmct + \beta E_t\pi_{t+1}$ form of the equation, his notion that markup variation will absorb current-period rises in marginal costs when monetary policy is restrictive can be thought of as saying that monetary policy can hold inflation π_t down by holding down expected-inflation $E_t\pi_{t+1}$ for the period in which (log) real marginal cost rmc_t remains elevated. In terms of the $\pi_t = \alpha \cdot E_t\Sigma\ \beta^i rmc_{t+i}$ version of the equation, it amounts to saying that the monetary restriction in the presence of increased real marginal cost is able to stop the forward-looking sum $\alpha \cdot E_t\Sigma\ \beta^i rmc_{t+i}$ from rising. The insensitivity of this sum to shocks to current marginal cost shows that Friedman believed that transitory, and not just highly persistent, shocks to wages were important.[196]

Friedman's suggestion that the markup rather than current labor cost might, on occasion, be what primarily rises in the short and medium runs in response to monetary policy easings can likewise

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[194] In these equations, s shock term to the inflation equation is assumed absent. This simplification permits the analysis to focus on shocks to labor costs, rather than to prices for a given expected path of marginal cost.

[195] That is, these series are simultaneously determined. The exogenous variables are the shocks in the model, and not either inflation or real (or nominal) marginal cost. Under such circumstances, description of causation running only one way cannot be valid, and a statement that real marginal cost causes inflation may be best interpreted as a statement that the reaction of marginal cost to various shocks (other than direct price shocks—which are assumed absent in the version of the New Keynesian Phillips curve given here) governs how inflation responds to those shocks.

The issue raised is similar to that brought up in some analyses of the inflation/output relation. As discussed in Nelson (2020a, Chapter 7; 2020c), in Friedman's view of the transmission mechanism, as well as in modern New Keynesian models, inflation and output are simultaneously determined: higher demand puts upward pressure on the rate of increase in prices, but also higher prices (in relation to costs) stimulate production. In this setting, it would not be valid to describe causation between inflation and output as running in only one particular direction. It would also not be appropriate to infer (as in Modigliani, 1977, and Laidler, 1990, did, in an interpretation affirmed by Rivot, 2012) that Friedman's various references to a prices-to-output channel signified a belief in flexible prices or a New Classical supply function or that these references were inconsistent with a belief in price stickiness.

[196] Carroll and Crawley (2017, pp. 84–85) argue that Friedman's insistence on the presence of transitory shocks has been inadequately incorporated into the empirical analysis of consumption behavior. The same may be true in the analysis of marginal-cost behavior, particularly with regard to the connections between this behavior and the course of inflation and monetary policy.

be put in New Keynesian terms. In terms of the $\pi_t = \alpha \cdot rmct + \beta E_t\pi_{t+1}$ expression, it means that monetary ease boosts $E_t\pi_{t+1}$ and hence π_t. Indeed, in the 1970s and 1980s Friedman repeatedly cited scenarios in which inflation rose in response to an increase in expected inflation before any reaction of wages.[197] In terms of the $\pi_t = \alpha \cdot E_t\Sigma\, \beta^i rmc_{t+i}$ version of the equation, this amounts to saying that the monetary easing in the presence of unchanged current real marginal cost pushes up the term dated later than t in the forward-looking sum, $\alpha \cdot E_t\Sigma\, \beta^i rmc_{t+i}$.

Friedman's position that monetary policy can affect inflation in the face of considerable looseness in the short-run relationship between wage growth and inflation has gained considerable empirical support. Gowland (1983, p. 114) suggested that "prices rose less than costs" across economies in the mid-1970s because aggregate demand policies became more restrictive during those years, while Laidler (1982, p. 129) judged that "most studies show that the ratio of prices to money wages does seem to vary with market pressures." It is also how officials have characterized how monetary policy affects inflation. For example, in Australia, Grenville (1995, p. 209) suggested that monetary tightenings "reduce [economic] activity and create an 'output gap'… Inflation responds to this output gap, both directly and through the indirect effect on wages."

The notion that nominal wage growth and price inflation might exhibit considerable looseness in their relationship has also received greater acceptance. For example, Obstfeld (2019, p. 2) states that the accrual of U.S. postwar data points to the conclusion that the inflation process has become "ever less dependent on domestic wage inflation." Obstfeld also insists that this change does not compromise "a central bank's ability to control the price level over the long term" (2019, p. 2).

Nordhaus and Cambridge University monetary economics

The public rebuttal, discussed above, that Friedman wrote to Nordhaus' (1972a) challenge to his views on inflation came about because Nordhaus' results had been seized upon by Nicholas Kaldor. In the U.K. press (*The Times*, April 6, 1977), Kaldor had cited Nordhaus' work in support of his own position (already noted in Section II above) of hardline Keynesianism.

What might have made Kaldor think of Nordhaus' work as being more supportive to his case than it actually was the fact that Nordhaus was one of the few U.S. macroeconomists of his generation to establish strong links with macroeconomists at Cambridge University—both

[197] See, for example, Friedman (1976b, p. 228) and Friedman and Schwartz (1982, p. 446).

Keynesians of an older generation like Kaldor and younger, but still hardline, U.K. Keynesians such as Wynne Godley. However, this interaction occurred against a background of Nordhaus' continuing skepticism about the U.K. Keynesian position. Nordhaus recalled: "I visited Cambridge in 1970–71 and had many good friends [holding] the hard-core Keynesian position that money does not matter (such as Kaldor or Godley). I thought they were stuck in the 1930s and hadn't learned the basic propositions of modern monetary economics." (William Nordhaus, personal communication, December 8, 2014.)

During Nordhaus' spell at Cambridge University, he started a project with Godley on the relationship between prices and costs in the United Kingdom. Early output on this research came in the form of Godley and Nordhaus (1972). But some indication of the divergent perspectives of Godley and Nordhaus is indicated by the fact that they were not able to finalize the ultimate output of their project—a book with Kenneth Coutts titled *Industrial Pricing in the United Kingdom*—until June 1977, after more than five years of drafting (Coutts, Godley, and Nordhaus, 1978, p. vii). As the authors put it, their main finding was that "industrial prices (relative to costs) are impervious to aggregate demand… The results indicate that, if demand affects industrial prices, it does so only through factor prices..."[198]

This common stand among the authors on the insensitivity of the markup to aggregate demand contrasted with the dissension among them regarding whether wages were sensitive to the output gap. They disclosed this dissension in the book: "in the opinion of the two British authors [that is, Coutts and Godley], the connection between aggregate demand and the rate at which money wages change in the United Kingdom is at best a weak one" (Coutts, Godley, and Nordhaus, 1978, p. 73).

This passage highlighted the differences among monetarists like Friedman, U.S. Keynesians like Nordhaus, and U.K. Keynesians of the time like Godley. For Friedman, as noted above, aggregate demand affected both the markup and costs. For Nordhaus, it was the case that both in the United Kingdom and in the United States the markup did not respond to aggregate demand, but nominal wages did do so.[199] U.K. Keynesians, in contrast, saw both the markup and nominal wages as insensitive to resource slack and inflation as a pure cost-push phenomenon. Godley expounded this position in work in the 1970s that challenged the monetarist view of inflation

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[198] Coutts, Godley, and Nordhaus (1978, p. 73). See also Nordhaus (1976, p. 60).
[199] For his articulation of this position in the U.S. case, see Nordhaus and Shoven (1974, p. 16).

(Fetherston and Godley, 1978).[200] Though labeled "New Cambridge," this line of work by Godley reaffirmed the older Cambridge Keynesian position that inflation was insensitive to slack and therefore incapable of being controlled by monetary policy.[201] Kaldor likely interpreted both the Godley-Nordhaus research on costs and Nordhaus' (1972a) work on nominal wage growth as consistent with his own pure cost-push views.

Nordhaus, however, did not interpret his work this way and felt that Kaldor and Nordhaus were wasting their abilities by taking an unduly extreme anti-monetarist position. However, as already indicated, he also felt that Friedman's monetarism was itself an undesirable extreme: "In one sense the Cambridge England and Friedman camps deserved each other." (William Nordhaus, personal communication, December 8, 2014.)

Although he believed that monetary policy could affect nominal wage growth and inflation, Nordhaus felt that Friedman unduly discounted both the role of fiscal policy in affecting aggregate demand and the importance of cost-push factors affecting wages and prices.[202] This perspective would be reflected in the twelfth edition (1985) of the Paul Samuelson economics textbook, for which Nordhaus was a coauthor. The inside front cover of the textbook depicted a "family tree of economics," with monetarism (both its Friedman and 1970s rational-expectations variants) categorized as on a different branch from "Modern Mainstream Economics" (see Samuelson and Nordhaus, 1985).

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[200] The book of Coutts, Godley, and Nordhaus (1978) book was itself reasonably neutral in its references to monetarism and to Friedman (other than referring to him simply by his surname in the index: see p. 145). The authors acknowledged (p. 65) the validity of Friedman's (1975f) point that a markup equation could be consistent with expectational Phillips-curve explanations for inflation, and they also granted that a constant markup might be consistent with monetarist explanations for price inflation, provided that wages responded to monetary actions (p. 73).

[201] The New Cambridge view was criticized by Hall (1978a) on this ground.

[202] Nordhaus' position was therefore close to the "partial cost-push" position of U.S. Keynesians described in Nelson (2020a, Chapter 10) and in Chapter 2 above. Nordhaus' belief in both aggregate demand policy and cost shocks as important factors affecting nominal wage growth was evident in Nordhaus (1972a, 1972b), and the weight he gave to fiscal policy, monetary policy, and debt management as influences on aggregate demand was clear from Nordhaus and Wallich (1973).

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 4: Debates on Business, Finance, and Social Policy, 1973 to 1974

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Federal Reserve Board
July 30, 2023

I. EVENTS AND ACTIVITIES RELATED TO DEBATES ON BUSINESS, FINANCE, AND SOCIAL POLICY, 1973–1974

Eli Goldston, president of the Boston-based firm Eastern Gas and Fuel Associates, was an individual who regarded himself as "equally home in both industrial and academic circles."[2] This characterization reflected the fact that, despite being a full-time executive, Goldston—a neighbor of Harvard University's Wassily Leontief—had by the early 1970s long been immersed in the academic world. In addition to following a practice in which he "followed the [economic-research] literature as best he could," Goldston gave frequent speeches at universities and participated in numerous academic events.[3] Among these was the December 1969 American Economic Association meetings, at which he was a discussant at one of the sessions.[4] A version of that Goldston discussion saw print in May 1970, in the proceedings issue of the *American Economic Review*.[5]

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[1] Email: Edward.Nelson@frb.gov. The views expressed here are the author's alone and should not be interpreted as those of the Federal Reserve or the Board of Governors. The author is grateful to Timothy Perri and George Tavlas for comments on an earlier draft of this chapter and received help on accessing archival items drawn upon below from Fabio Ghironi, Daniel Hsu, the late Jay Pasachoff, and Gloria Valentine. See the Introduction in Nelson (2020a) for a full list of acknowledgments. The author regrets to note that, since the research underlying the present chapter started, six individuals who provided the author with recollections that are drawn upon below—Douglas Adie, Joseph Burns, Gary Becker, Harry Markowitz, Ronald McKinnon, and T. Dudley Wallace—have passed away.

[2] The quotation is from his biographical information in the front matter of Goldston (1972). As is often the case with executives, Goldston's formal title as head of his firm varied. In *Boston Herald American* (January 22, 1974, p. 1), he was identified as "chairman and chief executive," while, both in Goldston (1972) and in the introduction given (on November 9, 1971) by Allan Meltzer to the first of Goldston's public lectures at Carnegie Mellon University, his title was given as president.

[3] A couple of Goldston's interventions were included in Samuelson and Skidmore (1970), the sixth edition of the readings collection that accompanied Samuelson's economics textbook. On another of these speaking occasions, Goldston (1968, p. 25476) had occasion to mention Milton Friedman, with words of praise for the latter's proposal of a negative income tax: "Our poverty could be cured by the prescription which Professor Milton Friedman of Chicago has suggested."

[4] See Goldston (1970). The quotation and the information that he was Leontief's neighbor appeared in this article's pages 481 and 479, respectively.

[5] In the discussion, Goldston (1970, p. 481) complained that when he opened the *Quarterly Journal of Economics*, he found that the articles were characterized by an excess of "figures" (by which Goldston evidently meant numerical regression output—not graphs) over words. As it happened, the month in which Goldston's comments

Several months later, Goldston found a new mission. His combination of interests in real-world business and in economic discourse led him to choose a path that so many others in the Boston/Cambridge metropolitan area had embarked on before him: to take on Milton Friedman.

The catalyst for this decision was Friedman's *New York Times Magazine* article of September 13, 1970, which elaborated on its title, "A Friedman Doctrine—The Social Responsibility of Business Is To Increase Its Profits."[6] Goldston, already a longstanding advocate of a broad-based conception of corporate social responsibility, reiterated his stand at an executive retreat held eight days after Friedman's article appeared; and, in the course of the later part of 1970, he also composed a written rebuttal to the Friedman piece.[7]

An opportunity for Goldston to present his counterarguments at a major academic forum came when he was invited to be the speaker for a lecture series held annually at Carnegie Mellon University's business school. Goldston delivered these talks on November 9–11, 1971. Assigned the overall title "The Quantification of Concern: Some Aspects of Social Accounting," and revised in early 1972 for publication as a book, these lectures ranged across various economic issues, including the question of the approach that national economic policy should take toward the stabilization of national income and employment. What Goldston said on this question suggested that he had little time for Friedman's critique of Keynesian economics.[8] And, in any event, he indicated that he regarded the Keynesian-monetarist debate as having been settled in favor of the Keynesian position, "which even President Nixon has recently announced that he has been persuaded to adopt."[9]

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appeared in print saw the appearance of an issue of the *Quarterly Journal of Economics* containing a Friedman piece that was essentially wholly verbal (see Friedman, 1970h).

[6] Prior to its appearance in print, Friedman had presented the essay as a paper in a session on the first day of the Mont Pelerin Society meeting in Munich held from August 31 to September 5, 1970. See *The Spectator* (London), September 12, 1970, as well as the meeting program given at *http://rybn.org/thegreatoffshore/THE%20GREAT%20OFFSHORE/1.ENCYCLOPEDIA/MONT%20PELERIN%20SOCIETY,%201947/DOCS/Inventory%20of%20the%20General%20Meeting%20Files%20(1947-1998).pdf*.

[7] See the Eli Goldston papers (Harvard University library), in particular, *https://hollisarchives.lib.harvard.edu/repositories/11/archival_objects/2405252* and the related audio files. For Goldston's earlier advocacy on the matter, see Goldston (1968) and the materials excerpted and discussed in Samuelson and Skidmore (1970).

[8] A minor—though indirect, and perhaps unconscious—acknowledgment of Friedman's contribution to macroeconomics came in Goldston's (1972, p. 23) statement that modern macroeconomic policy in the United States reflected "the idea of combining the theory of Keynes and the accounts of Kuznets," with Goldston explicitly noting the origins of the latter's work at the National Bureau of Economic Research. Early in his career, Friedman had been closely involved—in a junior, but long-running, role—in the Kuznets/NBER construction of U.S. national accounts.

[9] Eli Goldston, second public lecture at Carnegie Mellon University, November 10, 1971. It happened that the small group of macroeconomists at the business school of Carnegie Mellon University included a number who would hardly be amenable to this conclusion and who were themselves further developing monetarist and natural-rate

Macroeconomics was not, however, the focus of Goldston's lectures. Rather, he contended that, with problems concerning national economic stabilization now largely solved, attention would increasingly be focused on the economy's microeconomic makeup—and in particular, on how U.S. corporations should conduct themselves in a state of reasonably full employment. Here, the social responsibility of business entered as a prominent issue, and, in this connection, Goldston referred to the stance taken by "Milton Friedman… in his well-known Sunday *New York Times Magazine* article."[10] The portion of Goldston's lecture dealing specifically with Friedman played down their differences somewhat.[11] But the fact that, of the Goldston lectures, the one that was specifically concerned with corporate social responsibility concluded with a section titled "Beyond Maximum Net Profit" underlined his disagreement with Friedman's posture.

In his lectures, Goldston also expressed disagreement with the economic stance underlying Friedman's article—a belief in the merits of a market mechanism based on individual private sector entities' optimization. "If you add a whole lot of small decisions, you don't always get a wise social decision," Goldston remarked. "This does… begin to move, out of a free market [and] into a political atmosphere, the making of decisions… [and] like it or not, that seems to me the way we're going."[12]

Two years later, on October 14, 1973, Friedman and Goldston appeared face to face at an executives' conference in New York City, to debate the social responsibility of business.[13] In his remarks, Friedman indicated that he did not oppose the promotion of social causes but suggested

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ideas. These figures included Allan Meltzer, who introduced Goldston's first lecture. Although he was one of Friedman's critics in the *Journal of Political Economy* debate at the time, Meltzer was, of course, a leading figure on the monetarist side of the argument against Keynesianism. Another economics professor at the business school, Robert Lucas, would shortly receive a $63,000 grant from the National Science Foundation to develop his work on the natural rate hypothesis (*Pittsburgh Post-Gazette*, December 25, 1971).

[10] Goldston (1972, p. 44).

[11] In particular, Goldston (1972, pp. 44–45) pointed out that Friedman would likely be receptive to firms voluntarily improving their employees' working conditions (an accurate conjecture, but one that describes a practice that Friedman would likely have regarded as part of the firm's overall maximizing behavior: Friedman had described efforts to maximize profits as involving "offering better working opportunities to people so that it can obtain better people"—see *Kansas City Star* (Missouri), February 9, 1971)—and their making charitable donations. With regard to charitable donations, however, Friedman would eventually elaborate that he believed stockholders should be consulted on, and give consent regarding, the choice of charities that were the recipients (see, for example, Friedman, 1998, and the condensed version of these remarks in *National Post* (Canada), July 5, 1999). Some later research by specialists in the economics of corporations has, in contrast, argued that holders of stock might actually find it useful that they can conduct part of their charitable giving through their ownership of firms and can cede decisions on donations to those firms. See, for example, Bénabou and Tirole (2010).

[12] From Goldston's remarks in the question-and-answer session of the first lecture (November 9, 1971).

[13] The debate was the first session in a three-day conference, held at the New York Hilton hotel and organized by the Urban Research Corporation, titled "National Conference on Corporate Social Responsibility: The Emerging Consensus," and convened on October 14–16, 1973 (as described in *The Carolina Times* (North Carolina), October 13, 1973, and *Fortune*, November 1973).

that it was undesirable that businesspeople had the prerogative to direct toward these causes funds that had been received as profits: "I think social change should take place through individuals pursuing values they believe in—with their *own* money."[14] Friedman also recognized the need for environmental protection. But he argued, as he had before, that such protection should be effected through price signals in the marketplace, including those induced by Pigovian pollution taxes, whose introduction Friedman favored. "The best way corporations can be used to accomplish social goals," he contended, "is (*a*) by forcing them to compete[,] so they don't have these monopolistic profits to throw around and (*b*) by setting up rules[,] so they've got to pay for resources they use."[15]

In his own contribution to the exchange, Goldston reiterated a point he had already made in his Carnegie Mellon University lectures—that firms lacked the information to carry out literal maximization of profits. "It's difficult to know how to maximize profits anyway. In the real business world, you're lucky if you've got a plus or minus 10 percent fix on things."[16] To those who knew Friedman's methodology essay and his work on expected utility, however, this point was not really contrary to his own position. He believed, first, that maximization took place with regard to expected, not actual, values, and second, that maximizing behavior was likely a reasonable approximation, even when it was not a literal description of behavior.[17]

A position more directly contrary to Friedman's was registered in Goldston's remark in the debate, "I question Professor Friedman's assumption [which, in this debate, was really a prescription, rather than an assumption] that corporations should maximize profits."[18] Goldston argued, instead: "A corporation is its people, and they are not detached from society, which has come to expect corporations to do something other than maximize."[19]

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[14] In *Fortune* (November 1973), as reprinted in Bander (1975, p. 45).
[15] In *Fortune* (November 1973), as reprinted in Bander (1975, pp. 44–45). Friedman's reference to having "monopolistic profits to throw around" evoked a sentiment he had put into action when president of the American Economic Association (AEA) in 1967. Friedman motivated his setting up a new journal to be put out by the AEA—the *Journal of Economic Literature*—in part by the fact that it would create a new expense for the association and so limit the accrual of surplus funds that could be deployed in a discretionary manner.
[16] *Fortune* (November 1973), reprinted in Bander (1975, p. 44). See also Goldston (1972, pp. 45–46).
[17] See Friedman (1953b) as well as David Romer's (2006, p. 341) retrospective on the Friedman (1953b) discussion of profit maximization. (For an earlier retrospective, see Lipsey, 1960, p. 513.) Friedman (1982a, p. 116) reaffirmed: "As economists, we treat a business enterprise as if it were solving a large number of complex simultaneous equations even though the persons running that business enterprise never went to school and learned mathematics. We justify that procedure by saying that if we analyze them *as if* they are rationally and knowingly pursuing the maximization of profit, we'll get a good approximation of their behavior."
[18] *Newsday* (Long Island, New York), October 22, 1973.
[19] *Fortune* (November 1973), reprinted in Bander (1975, p. 44).

The clash with Goldston exemplified an aspect of Friedman's behavior that was in defiance of a pervasive caricature made of him. As Rose Friedman would recall, according to his critics Milton Friedman was "the darling of the business community" (*The Observer* (London), February 17, 1980, p. 33). In some quarters, Friedman's position on corporate social responsibility reinforced criticisms of this kind—on the grounds that it appeared to be a call for a self-interested orientation on the part of American businesses. But, in fact, U.S. corporate leaders were largely hostile to Friedman's stand and, reflecting this, he prefaced his remarks at the October 1973 event by observing that he felt like a missionary at a cannibals' convention.[20] It was not only the case that many corporate leaders ill-disposed toward Friedman's conception of a firm's responsibilities. It was also the case that his edict that they should focus exclusively on profit maximization did not sit well with them because it would limit their discretion. As Goldston implied, strict adherence to Friedman's profit-maximization criterion would limit the ability of a "manager to rationalize whichever way it suits him" the appropriate goals of the firm (Goldston, 1972, p. 45).

Indeed, Friedman was arrayed against corporate America on a variety of topics—which as of the early to mid-1970s also included the appropriateness of fixed versus floating exchange rates, the merits of wage controls, and the desirability of abolishing tariffs. Already in late 1972, Friedman had criticized corporate leaders for failing to show resolve, and consistency, in advocating free markets. Developments after 1972 would reinforce this posture on his part. Friedman would describe his participation at a high-level conference in Hot Springs, Virginia, in the first half of October 1973 as a rare occasion on which he had had an opportunity to engage directly with high-flying executives.[21] Friedman's further dialogues with corporate leaders, including those at the aforementioned business conference in New York City (occurring immediately after the Hot Springs conference), as well as later interactions over the mid-1970s, had the cumulative effect of leading him to declare in 1977 that there was never "a meeting at which I hear a big businessman talk, [from] which I don't come away irritated and annoyed… For those of us who want to preserve the free enterprise system... the big businessman is a major part of the problem and not a source of support."[22]

The gulf in perspectives had already been noted in 1968 by an East Coast academic economist, who observed that Friedman's package of recommendations, on the whole, "scares the hell out of

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[20] *Newsday* (Long Island, New York), October 22, 1973.
[21] Letter from Milton Friedman to Anna J. Schwartz, October 29, 1973 (copy received of holding in Friedman office files, 2007).
[22] Instructional Dynamics Economics Cassette Tape 207 (end-January 1977).

businessmen" (*Chicago Daily News*, March 27, 1968, p. 49).

The degree of distance between Friedman and the American corporate world was also manifested in the perspective he took toward the funding of his research and public-policy activities. Friedman certainly was the recipient of such funding. A heavy degree of private-sector funding of U.S. academics' work is an institutional fact, and Friedman was the recipient of many research grants and lecture fees from firms, peak corporate bodies, and private-sector foundations over the years. His published work included explicit acknowledgments of this research funding: for example, in 1982, in the opening of *Monetary Trends*, Friedman and Schwartz expressed their gratitude "to the Ford Foundation and the J. Howard Pew Freedom Trust for grants for the specific purpose of enabling us to complete this work."[23]

Likewise, among his public-policy writings, one could find in the opening of *Capitalism and Freedom* Friedman's acknowledgment that the book had in part sprung from lectures he had given at several university conferences sponsored by the free-market organization, the Volker Fund. In this connection, Burgin (2012, pp. 172–173) focuses on the fact of Volker Fund sponsorship, and—even though Burgin acknowledges in places that Friedman did not alter his views to conform to those of his (numerous) sources of funding—Burgin's coverage of the creation of the *Capitalism and Freedom* often gives the opposite impression: that coverage is included in the index entry titled "Volker Fund, influence of" (p. 302), appears in a chapter titled "The Invention of Milton Friedman" (as though Friedman was insincere or that he was a front-person for unseen interests), and conveys the notion—one not, in fact, justified by the passage of *Capitalism and Freedom* that Burgin quotes—that the materialization of that book was due only to the fact that figures in the Fund applied "pressures" for its appearance.[24] In fact, Friedman prided himself on two key tenets: that his research proper was a breed apart from his public-policy views; and that what he said in both in his research and his public-policy activities did not

[23] Friedman and Schwartz (1982, p. xxxi). Identifying sources of research funding was a practice Friedman continued through the rest of his life. Friedman (2005b) observed, in a preamble endorsing and introducing Miron (2004): "Professor Miron's research for the report was funded by the grants program administered by the Marijuana Policy Project (MPP)."

[24] The passage in question is Friedman's (1962a, p. ii) statement that the academics who hosted the conferences offered "friendly pressure to write them [the lectures] up in tentative form." Burgin quotes this passage (albeit misquoting "pressure" as "pressures") and describes it as "the organizers from the Volker Fund" who "[e]ventually" applied this pressure after the non-appearance of the book (Burgin, 2012, p. 173). In fact, the Friedman passage explicitly distinguished the conference directors—the academics presiding over the conferences and setting up its program—from those who were actually part of the Volker Fund and who provided financing for the conferences. Friedman, in the passage in question, was apparently conveying nothing other than the fact that he received encouragement from fellow academics to write up the lectures that he had given (and which they had hosted) into a book; and, contrary to Burgin's use of the word "eventually," there is nothing in the Friedman passage to suggest that these recommendations occurred after the book had failed to appear after a certain interval.

reflect the influence of his sponsors' funding. As he once put it, "I am speaking for Milton Friedman."[25] "I speak only for myself," Friedman had remarked in a 1970 interview, "and I find it very pleasing to be able to speak only for myself." (*The Listener* (London), February 11, 1971 p. 171.)[26]

As already observed, large-scale private-sector funding of economic research was and is a routine part of the operation of the U.S. academic world. This was true of universities but also organizations like the National Bureau of Economic Research. Anna Schwartz observed (interview, April 21, 2003): "I mean, that's the way the Bureau essentially operated—with grants from foundations." In addition to relying on private-sector funding, the NBER was further linked to the corporate world because it had on its board several directors at large, including eminent business executives. Over the years, Friedman had evidenced that he was not beholden to the views of these corporate figures. Indeed, two of his NBER books (the Friedman-Kuznets *Income* study in 1945 and the Friedman-Schwartz *Monetary History*) had so incensed particular corporate directors that they had insisted on composing comment-rebuttals that appeared inside those same books. In the 1970s, Friedman's public positions continued to be at variance with an NBER director-at-large in the corporate world—in this case, Eli Goldston. Friedman's 1973 debate with Goldston occurred three years after Goldston's name appeared in Friedman's most recent full-length NBER book—when, on an opening page of the Friedman-Schwartz *Monetary Statistics* monograph, the bureau's directors were listed.[27]

In late 1973, Goldston looked poised for an enhanced degree of participation in public debate, as he announced he would be departing Eastern Gas and Fuel Associates, in order to head a consultancy firm. But though the battle lines seemed to be drawn, there would be no repeat encounter between Friedman and Goldston in national discourse. In early 1974—before he

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[25] In Friedman (1982d, p. 42).

[26] In a book recounting the energy debates of the 1970s, Jacobs' (2016, p. 103) account of Friedman's participation in the debates observes that "Friedman was affiliated with one of the institutional centers for conservative economic thought: the American Enterprise Institute [AEI]… with the backing of wealthy donors." This account of the Friedman/AEI connection does not take into account the following facts concerning the years of the energy debates: Friedman's basic posture with regard to the U.S. government's response to the energy crisis—his opposition to price controls—was a reiteration of a stand he had voiced publicly since during World War II; Friedman's three AEI publications in the 1970s (reprinted testimony, a reprinted monograph, and a record of conference participation) were about monetary issues, not the energy crisis; and he resigned from the AEI in 1979 while energy debates were still in progress in the United States. Jacobs also describes Gerald Ford as Friedman's "man in the White House" (p. 166), overlooking the reality that Friedman criticized Ford on energy policy on numerous occasions during his tenure and that this issue led Friedman to suggest that Ford should not receive the Republican party nomination for president in 1976 (see Chapter 5 below).

[27] See Friedman and Schwartz (1970, p. v). See also National Bureau of Economic Research (1974a) on Goldston's service as director (which spanned the period from 1968 to 1974).

could take up his new position, and just under a hundred days after his setpiece debate with Friedman—Goldston died of a heart attack, at age 53 (*Boston Herald*, January 22, 1974; *Honolulu Star-Bulletin*, January 22, 1974).

The fact that Friedman had been able to generate such a concerted effort at rebuttal from figures of Goldston's stature was a testament to the fact that the issue of social responsibility of business had galvanized corporate America. By late 1973, a newsletter, *Business and Society*, subtitled "A Biweekly Report on Business and Social Responsibility" and committed to relaying views on both sides of the debate, was in its sixth year of publication. Its issue of December 21, 1973, noted Friedman's primacy as a protagonist in the debate: "Prof. Friedman… is generally regarded as the arch-enemy of corporate social responsibility." Furthermore, the subject was at the intersection of popular and research discourse, and Friedman's 1970 *New York Times Magazine* piece had accomplished the feat of becoming the standard reference articulating the profit-maximization case in both sets of discussions. This state of affairs was manifested in the appearance, in Spring 1972, of a combined public-policy/research journal—which was still publishing new issues in 2020—titled *Business and Society Review*.

The journal's debut issue featured contributions provided by numerous figures in the corporate and research worlds—including Friedman, Goldston, and Paul Samuelson. Friedman's contribution consisted of a new interview with him. The degree to which he had elicited reaction on the subject of corporate social responsibility was reflected in that interview's main title—"Milton Friedman Responds." That title reflected a presumption of wide knowledge, among researchers and business figures, of Friedman's 1970 piece and the stand taken therein.[28] The title also amounted to an acknowledgment of the backlash the piece had generated in American business opinion.

However, during the same years of the early and mid-1970s in which attention toward corporate social responsibility was surging, it was a subject in which Friedman was losing interest. It was never, of course, a focus of his research proper. But there was a further reason why Friedman's concern—so much on display in 1970—with discouraging firm behavior driven by social responsibility was now fading. He was coming to conclude that such behavior patterns were far less important empirically than he had believed when he penned his *New York Times Magazine*

[28] As it was hard to interpret outside its original context, this main title was dropped when the interview appeared alongside Friedman's *Newsweek* columns in the Friedman (1975a) collection. (Valentine, 1987, p. 540, noted that this reprint consisted of excerpts—see also Friedman, 1975a, p. 240. However, though the reprint omitted the interviewer's introduction, it was otherwise very extensive.) Paul Samuelson's (1972) contribution to the same issue of *Business and Society Review* was reprinted in his own (Samuelson, 1973b) collection.

article. Even in early 1972, in his *Business and Society Review* defense of his 1970 piece, Friedman observed that many of the expenditures that corporations purportedly made in the name of the greater good were actually driven by self-interest. "Most of the time when corporate executives talk about exercising their social responsibility all they are doing is engaging in window dressing. This is why, in fact, there is very little actual corporate social responsibility in a meaningful sense."[29]

Friedman was therefore downplaying the empirical importance of the very phenomenon that he had had in 1970 used a whole article to criticize. Friedman emphatically relayed his skeptical attitude toward the idea that corporations focused on much beyond the bottom line when economist friends held a Festschrift for him in Charlottesville, Virginia, later in 1972. Henry Manne's paper, slotted in the conference program for presentation at 3.15 p.m. on day 1 (Friday, October 21), argued that supposed corporate altruism was motivated by firms' individual optimizing behavior. [30] Manne's paper (published as Manne, 1975) generated ostentatious agreement on Friedman's part at the conference session (see Nelson, 2020b, Chapter 14).

Friedman remained critical of the notion that corporations should take actions driven by social obligations— "Heaven preserve us from corporations which are socially responsible," he observed in 1976."[31] But, having now assessed that such actions were not particularly prevalent, he stepped back from debate about them. His participation in public events on the subject of corporate social responsibility, such as the 1973 exchange with Goldston, became the exception. It would instead be the norm for forums covering the matter to refer to his views but not have him present—a situation already emerging in fall 1972, when Columbia University's Melvin Anshen, the first in a succession of speakers in a lecture series on "Managing the Socially Responsible Corporation," observed: "Professor Friedman is such a sharp debater that one welcomes the opportunity to respond to him in his absence."[32]

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[29] *Business and Society Review* (Spring 1972, p. 7), reprinted in Friedman (1975a, p. 242).
[30] The time slot for the session containing Manne's speech was obtained from the copy of the conference program in the Anna Schwartz papers (subsequently deposited at Duke University).
[31] *The Jay Interview*, ITN, July 17, 1976. This remark echoed one that Friedman had made in a letter of February 8, 1957, to Raymond Saulnier, Council of Economic Advisers: "Heaven preserve us from a world of business men and labor leaders conducting their affairs in terms of 'social' responsibility." In that letter, however, Friedman was responding to the Eisenhower Administration's interest in voluntary wage and price restraint (see S. Gordon, 1974, p. 38), so the specific "social responsibility" in question concerned inflation, rather than the community and environmental issues associated specifically with the later debates on corporate social responsibility.
[32] Anshen (1974a, p. 6). Twelve other speakers contributed to this series of talks on corporate social responsibility, held at Columbia University's Graduate School of Business during the 1972/1973 academic year. Several of these further lectures invoked Friedman's name, generally as the proponent of the position they opposed (see Anshen, 1974b). As it happened, Anshen's remark, quoted above, echoed the sentiment made at New York University by Walter Heller in 1968. Heller—who was steeped in debating Friedman directly, mainly on macroeconomic matters

It also became the norm for those responses to be critical. This was true not only of executives' attitudes to Friedman's arguments, but also of work by microeconomists who specialized in business conduct. Predominantly, the position taken by these specialists—both in the 1970s and in later, more rigorous, treatments—was that Friedman's posture, while true in special cases, was overly simple, and that firms have obligations to shareholders that go beyond pecuniary ones.[33] Oliver Hart, for example, argued that the notion "the only thing firms should be interested in doing is to maximize shareholder wealth" is not accurate when allowance is made for key characteristics of firms. Beyond very simple models of the behavior of a corporation, "it's no longer true that the contribution of the firm is measured by its surplus—and [then,] to maximize surplus is no longer the right objective." (Oliver Hart, interview, December 29, 2014.)

For his part, Friedman wrote very little more on the subject. But his 1970 articulation of a stand that was (especially to noneconomists) contrarian and that seemed jarring, and on a matter that was of interdisciplinary interest, would leave an immense imprint on published research and public-policy writings.[34] The Friedman article on corporate social responsibility would receive massive citations in business and ethics journals. Much of the associated discussion did not put him in a favorable light: for example, a 1991 article in the *Journal of Business Ethics* was titled "Friedman Fallacies."[35] But the perennial; argument about the pros and cons of Friedman's position on corporate social responsibility would have the effect of blowing up his aggregate citation counts. This in turn would prove to be a trap for unwary analysts who mechanically, and mistakenly, sought to interpret Friedman's total citation numbers as a metric that registered his influence on macroeconomics and on monetary economics.[36]

(see Section III below)—had observed: "I fully understand that it's much easier to debate Milton *in absentia* than in person!" See Friedman and Heller (1969, p. 27).

[33] See Hart and Zingales (2017), as well as the account in Nelson (2020b, Chapter 14), for further discussion.

[34] It is, nevertheless, hyperbole, to describe the article as a manifesto launching a, or the, free-market revolution in the United States or as having "changed the course of capitalism." The *New York Times Magazine* (and its electronic counterpart) used these unwarranted descriptions when introducing a special symposium (September 13, 2020) on the occasion of the fiftieth anniversary of the appearance of the Friedman article.

[35] See Grant (1991).

[36] One example of such invalid inferences is Dimand (2018, p. 1), who cites a previous study's finding that "Friedman was the most cited economist in 1981−85[,] with more than 3,500 *Social Science Citation Index* [SSCI] citations" as testament to the influence of the natural rate hypothesis on macroeconomics by the 1980s. The indicated statistic is not, in fact, valid evidence of that influence. Dimand makes no mention of Friedman's 1970 article on social responsibility of business—which, contrary to Dimand's inference, is what made Friedman the most cited economist in the period indicated—or of another interdisciplinary Friedman article, his methodology essay (Friedman, 1953b), that further vastly raised his citations. (In contrast, Mankiw and Reis, 2018, pp. 81−82, citing instead Google Scholar numbers, accurately record that the Friedman *New York Times Magazine* article surpasses any of his macroeconomic writings in recorded citations.) It further deserves emphasis that Friedman was also the most cited economist in 1969−1977, according to SSCI data (see *Milwaukee Sentinel*, March 15, 1979), so Friedman's topping the list of cited economists was not the new, 1980s, development that Dimand takes it to be. In

Friedman's concerns—rapidly receding as the 1970s progressed—that U.S. executives were using their discretionary power over their firms' resource allocation to pursue social goals rather than profit maximization had rested on a deeper worry—that such behavior was contrary to businesses conducting themselves competitively in the marketplace. From his perspective, a situation in which firms focused on social aims raised alarm bells for two interconnected reasons.

First, basic economics suggested it was through a process in which firms competed with one another to satisfy consumer demand that market economic arrangements helped contribute to community welfare. Consequently, Friedman remarked, a firm should ideally feel constrained by market forces that "limit[ed] it to the one function of making a profit, by insisting on competition between firms," in which case the firm would be appropriately preoccupied with "producing a better product at a lower cost" (*Kansas City Star* (Missouri), February 9, 1971).

Second, the fact that firms were seemingly in a position to dispense funds in support of social goals suggested to Friedman that firms had excess profits. "A really competitive enterprise can't sacrifice profit to social goals or else it will go out of business," Friedman remarked in the October 1973 debate with Eli Goldston. "It seems to me that the corporate executive who stands up in public and states, 'My corporation is pursuing socially responsible goals at the expense of profit,' should be a prime candidate for antitrust action, because he cannot do that unless he has some monopolistic power."[37]

Antitrust

This 1973 remark was one of a number Friedman made in the 1970s to the effect that the fact of corporations' social expenditures might be grounds for antitrust measures.[38] These remarks were partly rhetorical in character—after all, Friedman was questioning whether there *was* a fact of such expenditures: the purported social expenditures made by a firm might well, he thought, actually form part of the overall activities it carried out to promote its own commercial position. But the manner in which Friedman invoked a link between corporations' behavior and U.S. Justice Department antitrust responses underwent a revealing change after 1972. In 1972, when

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both the 1970s and the 1980s, citations of the social-responsibility article heavily influenced the outcome that Friedman was the most cited economist in social-science journals.

[37] *Fortune* (November 1973), reprinted in Bander (1975, p. 44).

[38] Most specifically, in *Business and Society Review* (Spring 1972, pp. 6–7; reprinted in Friedman, 1975a, p. 241), Friedman stated: "Any businessman who boasts to the public that he has been using corporate funds to exercise a social responsibility should be regarded as asking for investigation by the Antitrust Division of the Justice Department." This remark would be quoted many times in subsequent years, including in the *Washington Post* (June 27, 1976, p. L2).

referring to the link, Friedman remarked matter-of-factly that if a firm "is a monopoly, it ought to be prosecuted under the antitrust laws."[39] In 1973, as indicated above, he was notably less normative. In 1972 he was saying the finding of monopoly *should* generate antitrust actions, while in 1973 he had shifted to indicating that such a finding *would* generate such actions.

This change in Friedman's construction of the argument signified his evolving perspective on antitrust measures. Although some latter-day writers have often portrayed matters as if there was no doubt about what his views on antitrust were, this was actually a subject on which Friedman wrote very sparingly. Nevertheless, from Friedman's meager amount of public statements concerning antitrust, it is possible to ascertain that his views on the matter altered significantly and permanently during the years 1973−1974. The shift that occurred in these years underlay Friedman's remark a quarter-century later, "My own views about the antitrust laws have changed greatly over time."[40]

As the 1970s dawned, Friedman still adhered to two dicta he had articulated at a conference in mid-1950: that market forces would tend to erode monopolies—by promoting the emergence of substitutes for the monopolist's product and by generating rival sources of supply for the product—so that lasting monopoly power required the existence of actions by the state to frustrate those forces and support the monopoly; and that, equally, even short-run monopoly power was economically costly and, in order to wind back and forestall such temporary monopoly situations, U.S. antitrust measures were desirable.[41] Providing a snapshot of this posture was a profile of Friedman that appeared in *Current Biography Yearbook 1969*, which

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[39] *Business and Society Review* (Spring 1972, p. 6), reprinted in Friedman (1975a, p. 241).

[40] *National Post* (Canada), July 5, 1999, reprinted in Beckner and Gustafson (2000, p. 111), and based on Friedman (1998a). As Friedman often played down alterations in his views that had occurred—in particular, with regard to macroeconomic matters, he frequently described himself as *always* having believed something, when in fact he had believed it since about 1950—occasions when he *did* acknowledge having undergone a change in view deserve particular weight and credence.

[41] Friedman's paper for the May 1950 conference on labor unions included expressions of his belief both in the ephemeral nature of monopoly power, in the absence of government support, and in the desirability (nevertheless) of antitrust measures (see Friedman, 1951b, pp. 233−234). He also laid out the first of these points in his Price Theory lectures of roughly the same vintage (see Friedman, 1951c, pp. 29−32). Nelson (2020a, Chapter 4) contains documentation of Friedman's subsequent reaffirmations of the notion that substitution and competition would erode monopoly positions. As also discussed there, this notion had some antecedents in the writings of earlier University of Chicago figures. One of these, Frank Knight, seemingly articulated alternative positions on the matter: he became associated with the notion that monopoly power had a tendency to increase in a market system (see Patinkin, 1973, and Rayack, 1987, p. 109), but on other occasions he, too, suggested the monopoly power had an automatic tendency to dissipate in periods beyond the short run. See, for example, Knight (1951a, pp. 100−101; 1951b, p. 19). Even in these 1951 writings, however, Knight attributed greater ability than Friedman did to a particular perceived monopoly—labor unions—to influence aggregate market outcomes. See also Tavlas (2023a) for a detailed analysis of the evolving views regarding monopoly held by the University of Chicago economists of this era.

noted—accurately: "His advocacy of free competition is… expressed not only in his opposition to such government functions as farm price supports… but also in his favoring of ruthless application of the antitrust laws…"[42] Friedman would continue to express explicit support for antitrust statutes in his published work and other statements through the early 1970s.[43] But his position would shift during 1973–1974.

The shift flowed from doubts that Friedman had accumulated concerning the U.S. antitrust arsenal. Even in his 1972 collection of *Newsweek* columns, the importance of antitrust policy had been downgraded sufficiently for him to observe, in the introduction to the chapter on monopoly, that "the problem of monopoly, as a matter of policy, is largely a problem not of getting government to enact legislation against monopoly" but of other policy choices.[44]

Some of his reservations concerning antitrust were of long standing.[45] For one thing, Friedman worried about the process of antitrust suits being politicized.[46] Second, he was also increasingly persuaded, from the late 1960s onward, by regulatory-capture arguments.[47] And third, Friedman

[42] Moritz (1970, p. 151). Underlying the Moritz (1970) assessment was Friedman's support (see the next footnote), during the 1950s and 1960s, for antitrust measures.

[43] Retrospectives on Friedman's work have contained confident, but unwarranted, declarations that he had abandoned support for antitrust measures by the early 1950s (Van Horn, 2009) or by the early 1960s (Burgin, 2012). As discussed in Nelson (2020a, Chapter 4), these characterizations are directly contradicted by the record of Friedman's statements (including even those contained in the items cited by the authors in question).

[44] Friedman (1972c, p. 197). (See also Friedman, 1975a, p. 286.) Although the wording of this passage was not especially clear, Friedman's verdict on antitrust at this point seemed to involve ranking it, in terms of its quantitative importance, below other measures that the government could take to promote competition—but not rejecting it as an undesirable policy in its own right.

[45] George Stigler had, earlier than 1972, himself become a critic of antitrust legislation. As always, however, one must be skeptical of viewing Friedman and Stigler's views as having been developed in unison. Indeed, Stephen Stigler, in recalling his father's change in position, did not associate it with Friedman: "His [George Stigler's] views on antitrust evolved over time, from an early view that monopoly was a bad thing, to a later view, not that monopoly was a good thing, but that it wasn't always the problem that people had thought it was earlier. And he did investigations into the effects of regulation and other things that didn't always reverse his earlier view but could change and modify it. Milton, on the other hand, seldom changed his mind, I think I would say." (Stephen Stigler, interview, November 6, 2013.) As it happened, Friedman did change his mind on antitrust—but in a protracted manner that reached its crucial point (of being predominantly critical of antitrust measures) later than George Stigler's shift in position did. In addition, as Friedman was not a microeconomist, his change of mind occurred in a lower-profile and less widely noted way than in the case of Stigler.

[46] Specifically, in Friedman (1976c, p. 10; 1977f, p. 5; 1978b, p. 5), Feldberg, Jowell, and Mulholland (1976, p. 30), and Instructional Dynamics Economics Cassette Tape 213 (mid-May 1977), Friedman expressed the view that, in practice, high-profile figures in U.S. business were constrained in their ability to take issue publicly with specific government economic-policy measures—his reasoning being that they were conscious of the possibility of an antitrust suit being launched against them in retaliation. See also the discussion below of Friedman's concern that fear of antitrust measures hindered firms from making what were, in fact, commercially justifiable decisions to increase the prices of their products.

[47] See Nelson (2020a, Chapter 4). In this connection, it is notable that the discussion of antitrust measures in Friedman and Friedman (1980, p. 53) referred to the Interstate Commerce Commission. This was one of several bodies that Friedman believed had become subject to regulatory capture.

was skeptical about the merits of various specific antitrust provisions. There were provisions he deemed favorable: Friedman took the position that it was good that the public sector had made those private-sector contracts that restrained trade invalid and not legally enforceable. Friedman favorably cited this aspect of the Sherman Antitrust Act in print in 1952; and even after his 1973–1974 change in posture regarding antitrust, he retained his support for this aspect of anti-monopoly legislation.[48] But on the unfavorable side, even in 1965, Friedman was emphasizing "the distinction between the laws which make collusion and restraint of trade illegal, such as the Sherman Antitrust Act, and those which try to control specific business practices. These [latter] controls very often reduce rather than expand competition."[49] He would also later suggest that some provisions of the Sherman Act were themselves undesirable and "have had the effect of increasing monopoly."[50]

But, regardless of his qualms about specific antitrust laws or clauses therein, Friedman remained through the early 1970s essentially supportive of the "trust-busting" actions that are strongly connoted by the term "antitrust"—the breaking-up of large firms into smaller entities to forestall emerging monopoly situations or to pare back existing monopoly power. These key measures would, however, fall into disfavor with Friedman after 1972.

Friedman did not discuss his change of attitude toward the breaking-up of actual or perceived monopolies in his *Newsweek* columns. He may have articulated his newly negative posture toward such actions during his appearances in 1974–1975 on the talk show, *Donahue* (a nationally broadcast NBC television program that at that time was based in the city of Chicago).[51] And he clearly relayed it during this period in the course of making his public, though low-circulation, audiocassette commentaries. Speaking on his cassette series in late 1975, Friedman suggested that the mandated splitting up of large firms may prove counterproductive: "The question is whether the 'cure' of a government-forced breakup is a sensible cure... That's a cure which is likely to do more harm than good." (Instructional Dynamics Economics Cassette Tape 180, November 1975, Part 1.) As a corollary, Friedman later elaborated that corporate mergers played a part in the competitive process: "Mergers

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[48] On these matters, see Friedman (1952, pp. 13, 17), Nelson (2020a, Chapter 4), and Friedman's remark in Rogge (1976, p. 2): "The aspect of governmental intervention which has to do with the contractual agreements that will and will not be enforced in the courts has, I believe, been all for the good."
[49] From Friedman's remarks in Ketchum and Strunk (1965, p. 50).
[50] From Friedman's observations in Rogge (1976, p. 2).
[51] In an appearance on *Donahue* of September 6, 1979, Phil Donahue, whose program began broadcasting from the city of Chicago in 1974, gave the impression of already being aware of Friedman's opposition to the carrying out of antitrust suits by the U.S. Department of Justice. He may have learned it during one of Friedman's numerous previous guest appearances on the program—which occurred when Friedman was still living in the city of Chicago.

promote efficiency by enabling managements that are not using their resources effectively to be replaced. The economy would be far better off in a system where there were fewer restrictions and regulations against the purchase of controlling interests." (*New York Times*, July 18, 1982, p. 56.)

Even though it would never be one of his most well-known economic positions, Friedman's new opposition to antitrust became more embedded in the public record by 1976. He affirmed it in a U.K. television interview given during that year, for example.[52] Correspondingly, a profile of Friedman in the London newspaper *The Observer* noted (December 12, 1976) that among his objections to public-sector interventions, "Friedman is hostile even to government efforts to promote economic competition."

This hostility, of course, arose very much from Friedman's contention that these measures were *intended* to promote competition but need not do so. Friedman continued to favor a situation in which an industry or sector was composed of many small firms, and to be critical of big business, including the private-sector bureaucracy engendered by many large firms. But in contrast to previous years, from the mid-1970s onward antitrust did not number among the recommended weapons when Friedman made prescriptions for fostering competition and reducing market power.[53] This shift was reflected in the way in which he referred over the years to international trade liberalization. In 1948, Friedman had included a list of measures to improve the role of the market and raise potential output both "strengthening of the anti-monopoly legislation" and "drastic reduction of the tariff" (*New York Times*, January 11, 1948). But he would come to assign ranks to these two types of measures. "I have always played a kind of parlor game with people," Friedman remarked in 1976.[54] In this "game," he would challenge interlocutors to name the best single measure to promote competition. Typically, Friedman would get stricter antitrust measures as the response, whereupon he would counter that the

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[52] See Nelson (2020a, Chapter 4).

[53] Therefore, by the time Thorn's (1976, pp. 256−257) textbook profile of Friedman appeared, its statement that Friedman was an advocate of "vigorous enforcement of the antitrust laws" (p. 256)—though accurate as a description of Friedman's views until very recent years—was no longer applicable. By 1976, Friedman wanted many U.S. antitrust laws to be taken off the statute books (and, perforce, no longer enforced). Similarly, though a *New York Post* profile (October 16, 1976) described him as having favored the forced "breakup of monopolies," Friedman had actually moved away from this posture by 1976.

[54] In Rogge (1976, p. 2). In *Speaking Freely* (WNBC, May 4, 1969, p. 35 of transcript), Instructional Dynamics Economics Cassette Tape 101 (June 14, 1972), Friedman (1972c, p. 197; 1975a, p. 286), Friedman and Nader (1976, p. 6), *Milton Friedman Speaks*, Episode 1, "What Is America?," taped October 3, 1977 (pp. 22−23 of transcript), and *Milton Friedman Speaks*, Episode 13, "Who Protects the Worker," taped September 29, 1977 (p. 36 of transcript), he made similar statements. An early case of Friedman making a comparison of antitrust action with trade liberalization, along with the conclusion that the latter would be more powerful in promoting competition, was in *The Great Economics Debate*, WGBH Boston, May 22, 1969.

correct answer was in fact to abolish all tariffs and other restrictions on imports.[55] In this vein, in 1977 Friedman declared: "International competition would be the most effective way to offset local monopoly."[56] He had therefore moved from viewing antitrust and trade liberalization as measures to be taken in unison, to advocacy of the latter to the exclusion of the former.

The *Free To Choose* book, like most of Friedman's written work, covered antitrust-related matters only briefly, but it did convey his latter-day position on antitrust legislation—with these statutes judged as having had "very mixed effects," including "perverse effects" that limited competition.[57] In the U.K. television version of *Free To Choose*, Friedman gave a somewhat more detailed exposition, stating: "I am in favor of anti-monopoly laws which give the consumer greater freedom to choose. The trouble with monopoly is that it limits the choice; and laws which widen the range of consumer choice are all to the consumer's benefit. Therefore, I am in favor not of all the things that are called anti-monopoly laws, because some of the things that are *called* anti-monopoly laws are not. But I am in favor of laws which make contracts in restraint of trade unenforceable in the courts." He added that "the most effective anti-monopoly [measure] is… free trade, international competition."[58]

Friedman's attendance at a key law-and-economics workshop at the University of Chicago likely played a role in fostering and reinforcing his move during the mid-1970s to a negative posture toward antitrust measures. In August 1975, Friedman observed that over the previous forty years, "the University of Chicago Law School has maintained a kind of link with economics… This teaching has occurred particularly in courses on antitrust." (*Hillsdale Collegian*, December 4, 1975 p. 8.) In this connection, Sam Peltzman, a University of Chicago economics graduate who returned to the university in 1973 as a professor in the business school, recalled that there was a close relationship between the school and the department of economics, but, with regard to microeconomics, "it was tripartite. You would have to include the law school in that group, too. For example, the industrial organization workshop met in the law school. That was deliberate." (Sam Peltzman, interview, March 1, 2013.) In the 1974/1975 academic year, as well as the adjacent years, Friedman quite regularly attended the industrial-organization workshop (Kenneth Elzinga, interview, March 10, 2015; Charles Plosser, interview, April 2, 2015).

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[55] Along similar lines, Friedman had earlier suggested that automobile tariffs gave U.S. firms monopoly power (see Ketchum and Strunk, 1965, p. 55).
[56] Friedman (1977n, p. 2; p. 856 of 1978 reprint). For similar observations, see Friedman and Friedman (1980, p. 53) and Friedman's remarks in *Chicago Tribune* (July 20, 1980, p. 21).
[57] Friedman and Friedman (1980, p. 53).
[58] *Free To Choose* (U.K. television version, debate portion), BBC2, episode "Who Protects the Consumer?," March 15, 1980, p. 7 of transcript.

Friedman's attendance of the workshop in this period may have reflected his interest in the emerging public-choice literature as well as its potential applications to U.S. regulatory policy—especially with respect to energy, an area in which Friedman was intensely engaged in the wake of the first OPEC oil-price increase.[59] But exposure to the law-and-economics literature's perspective on antitrust measures—which, broadly speaking, was that these measures might often impede economic mechanisms that were consistent with competition—alongside the public-choice theory's often-negative verdict on business regulation and on regulatory agencies, likely left an imprint on Friedman's own views concerning antitrust.

Charles Plosser, who attended the workshop from 1973/1974 through 1975/1976, recalled of Friedman's participations that "he often had a lot to add" to the workshop's discussion. "Milton would chime in and sometimes disagree. But he would end up kind of deferring a bit to George [Stigler] and Ron [Coase], or Dick Posner, on some of those issues." (Charles Plosser, interview, April 2, 2015.) Friedman likely gained familiarity with the law-and-economics perspective on antitrust and related issues much more by listening to colleagues in these seminars and other forums than he did by actually perusing the literature that they were producing and discussing. He likely read that literature quite sparingly.

Certainly it was not a literature to which he was himself contributing. Friedman had published in the *Journal of Law and Economics* in the 1960s, but his articles for that journal were focused solely on macroeconomics. Neither in that period nor later did he publish in the law-and-economics field.[60] One would get the contrary impression from the fact that the website of the University of Chicago's Becker Friedman Institute has listed for many years (and still does, at the time of writing) a 1979 article titled "Problems of Cross Examination in Labor Arbitration" among Friedman's publications.[61] But this was a mistake, as the article was, in fact, by New York City-based arbitration judge Milton Friedman, one of Friedman's (numerous) namesakes.[62] See Figure 1.

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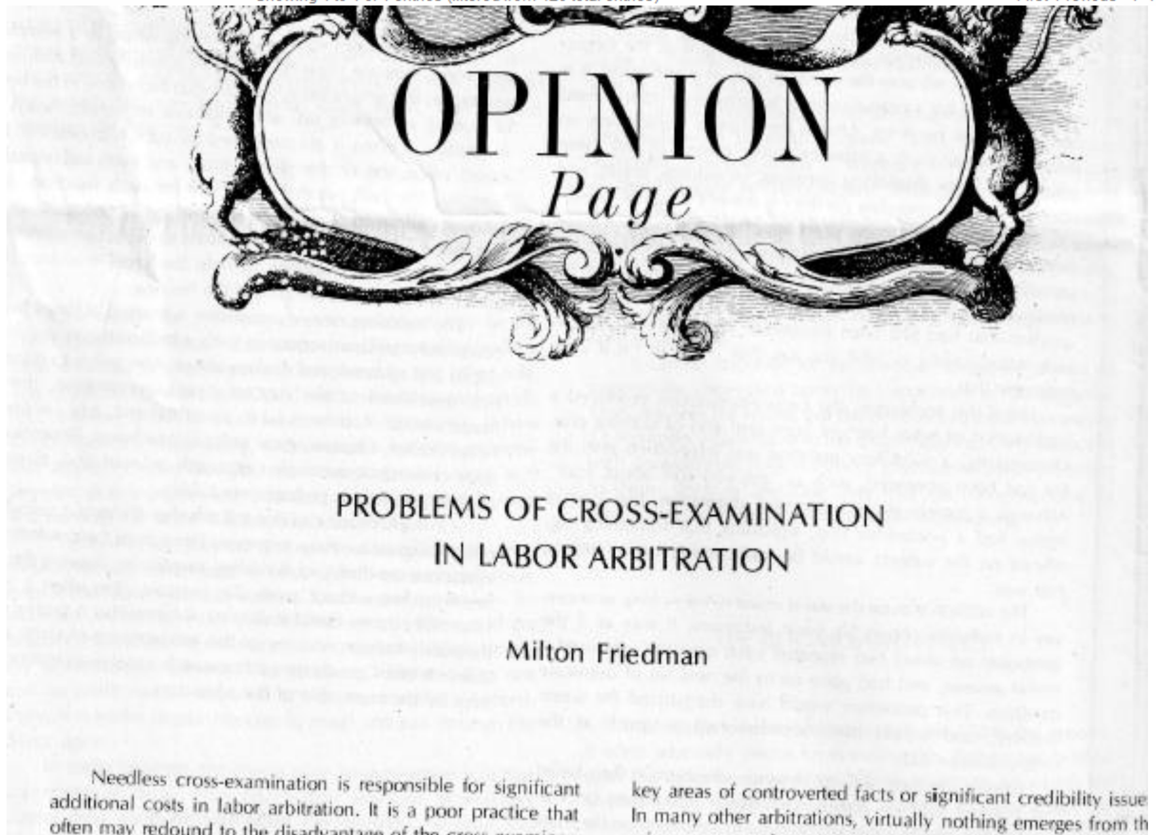
[59] See the discussion in the previous chapter and the further coverage in subsequent chapters.
[60] Friedman's views on the legal system are, however, considered in Nelson (2020b, Chapter 14) and, particularly with regard to the labor market, in later chapters of the present book.
[61] See *https://mfidev.uchicago.edu/milton-friedman/all-publications.shtml*.
[62] See M. Friedman (Judge) (1979). Friedman's longtime secretary, Gloria Valentine, told the present author (personal communication, January 5, 2009) that, in the 1972–1976 period, "From time to time[,] he would receive a letter meant for a Milton Friedman in New York who he said was an arbiter on Wall Street, and *that* Milton Friedman would receive a letter from time to time meant for Professor Friedman. Each would send the misdirected letter on... By the time we left for California, we no longer had [this] problem."



OPINION Page

PROBLEMS OF CROSS-EXAMINATION IN LABOR ARBITRATION

Milton Friedman

Needless cross-examination is responsible for significant additional costs in labor arbitration. It is a poor practice that often may redound to the disadvantage of the cross-examiner ... key areas of controverted facts or significant credibility issue... In many other arbitrations, virtually nothing emerges from th...

Figure 1. (*a*) Screenshot of search result obtained from Becker Friedman Institute website. (*b*) Opening of a December 1979 article in *The Arbitration Journal*.

Nor, for that matter—despite his teaching of price theory, and notwithstanding his fame in the research literature on business, owing to the social-responsibility piece—was business a research interest of Friedman's.[63] The fact that it was not was, of course, a key reason why the volume of

[63] For this reason, great caution should be exercised in assuming that Friedman knew about or was influenced by microeconomic research papers that appeared in the 1970s—even those that were famous and aligned with his own generalizations about microeconomic behavior. As an example: Klein, Crawford, and Alchian (1978), which at the time of writing had a vast aggregate of over 9000 Google Scholar citations, became a standard reference on the possible economic benefits of mergers. This article, which was presented at a University of Chicago seminar prior

his comments with regard to antitrust was small. His non-inclusion of the economics of the firm among his research interests was revealed in Friedman's 1971 remark, when invited to write an introduction to a microeconomic study, that reading that study made him curious about the organization of a related industry: "probably one exists" already, Friedman remarked, adding disarmingly that it "reflects my ignorance" of the industrial-organization literature that he did not know whether such an analysis actually existed. [64]

Friedman admitted to its ignorance of the field again nearly thirty years later, when asked to talk about the U.S. government's antitrust measures against Microsoft Corporation: "I am not an expert on Microsoft or the computer industry, and I really don't want to make any specific comments… because I don't know enough."[65] But in this same presentation—a Friedman luncheon talk in November 1998 in San Jose, California, at a conference devoted to the case—while stressing that "I am not going to argue about the technical aspects of whether Microsoft is guilty or not under the antitrust laws," he recalled the shift in his views on antitrust over his career. In the course of this shift, he explained, he had "gradually come to the conclusion that antitrust laws do far more harm than good, and that we would be better off if we didn't have them at all."[66]

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to its publication, was coauthored by Friedman's former student Benjamin Klein. However, the article was concerned with research topics to which Klein had moved only after finishing his dissertation work with Friedman, and there does not appear to be evidence of Friedman having known of the paper, let alone having cited it or relied on it when articulating his own position.

[64] Friedman (1971e, p. viii).

[65] Friedman (1998).

[66] *National Post* (Canada), July 5, 1999, reprinted in Beckner and Gustafson (2000, p. 111). The full talk was Friedman (1998). In quoting the statement, Appelbaum (2019, p. 157) takes Friedman as assuming the mantle, in the wake of George Stigler's death in 1991, as a leading critic of antitrust policy and as having been spurred to do this by the Microsoft case. In fact, however, Friedman devoted very little attention to the subject of antitrust in the years that followed Stigler's death. Public statements by Friedman on the matter—such as this late-1990s remark, which, as noted, largely repeated what he had said on the record since the mid-1970s—continued to be infrequent. And, as already indicated, he was explicit in stressing his continuing lack of command of details of ongoing antitrust debates, and in his late-1990s talk he refrained from specific comment on the technical merits of the Microsoft lawsuit.

The U.S. Department of Justice's antitrust action against Microsoft lawsuit produced a notable parallel with one aspect of the Keynesian-monetarist debates in which Friedman had been so engaged during the 1960s. In his deposition associated with the case, one of the arguments that Microsoft's head Bill Gates made was that he did not regard his firm's relationship with the company Java as wholly adversarial. Gates insisted that that on "anything about Java, you've got to show me a context before I can answer, because just the term 'Java' itself can mean different things." He claimed that he regarded Java's retail software as a competitor to Microsoft but was amenable to the spreading usage of "Java the language" (the computer language). Gates' stand on this matter had echoes of the position that Friedman advanced in the 1960s and later that he was opposed to Keynesianism as a specification of the model of the economy but found much of the Keynesian analytical language useful. (For the relevant portions of Gates' testimony, a recording of which is also viewable on YouTube, see the transcripts at *https://www.justice.gov/atr/videotaped-deposition-excerpts-bill-gates* and *http://techrights.org/2020/10/12/bill-gates-transcripts-part-2/*, respectively.)

However—and perhaps fittingly, in light of Friedman's mixed views on the matter—one of his last public statements concerning U.S. government antitrust actions was favorable. In remarks published in the year of his death, Friedman observed that "the breakup of the Ma Bell monopoly led to a revolution in communications."[67] The key development in the forced breakup to which Friedman referred was the agreement announced on January 8, 1982, between the U.S. Justice Department and the American Telephone and Telegraph (AT&T) Company, under which AT&T would undertake divestiture of twenty-two Bell telephone companies that were in its orbit (Carruth, 1993, p. 759).[68] This agreement flowed from antitrust moves that the U.S. government had launched eight years earlier. Ironically, therefore, it turned out that the antitrust action that Friedman would come to see as a prime example of an efficiency-improving monopoly-breakup had its beginnings in the same 1973−1974 period in which Friedman lost faith in antitrust as a policy measure.

Antitrust and interest-rate ceilings

Friedman's rethinking during the first half of the 1970s antitrust undoubtedly had to do, in part, with his concern that antitrust measures would become intertwined with what he regarded as essentially a completely separate issue: the fight against inflation.

A belief in the increasing incidence and usage of monopoly power had, of course, been a motivation for the imposition in August 1971 of the wage-price control system. In contrast to Friedman, who rejected the notion that inflation was—or even could be—due to monopoly power, Arthur Burns had, starting in 1970, articulated the cost-push view of inflation that was largely a corollary of this notion. The Nixon Administration, too, came round to this perspective on inflation—a change reflected in its series of incomes-policy-related actions in 1970−1971 that culminated in the introduction of the controls. Early in this process—in May 1970, when the prospect of such compulsory controls still seemed remote—Friedman believed that the means through which the administration might implement an incomes policy would be through "arm-twisting methods, which involve much more than just moral suasion. What they involve is calling people on the carpet and threatening them with income tax investigations, threatening them with antitrust prosecutions." (Instructional Dynamics Economics Cassette Tape 51, May 27, 1970.) Such antitrust measures would be aimed at preventing firms from increasing prices.

[67] Friedman (2006, p. 157). He had previously observed (*Washington Post*, February 19, 1995; reprinted in Bonsteel and Bonilla, 1997, p. 198): "We know how the telephone industry has been revolutionized by opening it to competition."
[68] The actual divestiture principally took place in early 1984. See Dornbusch, Fischer, and Schmalensee (1988, pp. 258–259).

Some of the Nixon Administration's early tilts toward incomes policy—such as its "inflation alerts" directed toward giving publicity to allegedly unjustified price increases—did raise the prospect that a subscription to profit-push theories of inflation would increasingly direct the administration's actions. Working against this tendency, however, was Arthur Burns' influence on national debate when he pressed the case for a U.S. incomes policy in 1970−1971. Burns' expositions of cost-push inflation in these early years of the 1970s took the focus away from firms' actions and toward (alleged) wage-push pressure on costs and prices, arising from the behavior of labor unions. Indeed, Burns' speech in May 1970 in favor of an incomes policy had actually emphasized the *responsiveness* of business pricing decisions to market pressures. In particular, that speech specifically referred to the scope for monetary policy, in the face of union wage-push pressures, to create conditions under which firms could not "simply... pass on all cost increases to their customers."[69]

However, as Burns' cost-push views of inflation hardened during the course of 1970, he came to see the link between cost increases and price rises as more mechanical in character—one implying that firms were unlikely to be deterred by slack demand in passing on hikes in costs. Nevertheless, because wage-push remained the principal focus in his cost-push conception of inflation, and because antitrust is principally a device for use against firms rather than labor groups, Burns' advocacy of incomes policy in the very early 1970s did not seem to presage a prominent role for antitrust action as an anti-inflation measure.

This situation changed during 1973. In that year, Burns increasingly stressed firms as an active source of cost-push pressure, and as being likely to superimpose profit-push forces onto wage-push pressures. As late as in a book published in 1973, Burns had seemed sympathetic with the generalization that business concentration had not increased appreciably in the United States, and he appeared averse to "the principal criticisms of large corporate business that have been voiced in recent years."[70] Starting in early 1973, however, Burns cited firm market power much more prominently in his discussions of inflation and of remedying it. In Congressional testimony on February 20, Burns remarked: "Not a few of our corporations and trade unions now have the power to exact rewards that exceed what could be achieved under conditions of active competition." He called for the public sector "to undertake to curb abuses of economic power by both business firms and trade unions." (*Los Angeles Times*, February 21, 1973, p. 15.)[71]

[69] Burns (1970, p. 12 of typescript version; p. 8 of printed version), reprinted in Burns (1978, pp. 98−99).
[70] Burns (1973c, p. xiv).
[71] See also Burns (1973d, p. 167).

Not surprisingly, therefore, news reporting on Burns' remarks linked them strongly to antitrust, with the major headlines on the testimony including "Tough Trust Laws Proposed By Burns" (*Washington Post*, February 21, 1973, p. A1) and "Tougher Antitrust Laws Urged To Curb Inflation" (*Los Angeles Times*, February 21, 1973, p. 8). Burns' remarks were also quoted when the antitrust-and-monopoly subcommittee of the U.S. Senate's Committee on the Judiciary held hearings in March. Testifying during the first day of hearings, economics professor Williard F. Mueller, of the University of Wisconsin, asserted: "A consensus appears to be emerging among economists that the existence of excessive market power in parts of the economy create[s] an inflationary bias. Put differently, when such market power exists, it is impossible to rely solely on monetary and fiscal policy to achieve and maintain full employment without excessive inflation. Although there are still unbelievers, especially in that bastion of *laissez-faire* economics, the University of Chicago, events have forced most economists to discard theoretical models that assume away the problems posed by market power."[72] As an example of how "prominent public officials have come to share this view," Mueller cited Burns' February 1973 testimony on market power and contrasted them with the sharply different views that Burns had articulated during the late 1960s.[73] On the same day, the subcommittee's chairman, Senator Philip A. Hart (D–MI), referred to a letter, recently written by Burns, in the subcommittee's possession: "He thinks that the antitrust law should be strengthened… I read him to say that industry has contributed to the problem of inflation and unemployment."[74]

Burns reaffirmed his belief in business power as a source of inflation at the Business Council conference—also attended by Friedman and mentioned above—held at Hot Springs, Virginia, toward the middle of October 1973. At this event, Burns called for the United States' compulsory wage and price controls, whose statutory basis was due to expire at the end of April 1974, to be extended beyond that point, but on a more selective basis than previously—applying not to all actors to the economy, but instead directed at unions and businesses that operated in highly concentrated industries (*Japan Times* (Tokyo), October 15, 1973).[75]

As this recommendation implied, Burns' interest during 1973 in antitrust as an anti-inflation weapon did not rest on the likelihood that it would be a complete substitute for price controls,

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[72] From Mueller's testimony of March 27, 1973, in Committee on the Judiciary, U.S. Senate (1973a, pp. 45−46).
[73] From Mueller's testimony of March 27, 1973, in Committee on the Judiciary, U.S. Senate (1973a, p. 47).
[74] From Hart's remarks of March 27, 1973, in Committee on the Judiciary, U.S. Senate (1973a, p. 84).
[75] At the same business meeting, Burns also expressed concern—widely held in late 1973 (the more so after the oil shock materialized) but one that, as discussed in earlier chapters, did not, in the event, really manifest itself—that shocks emerging on the price side in 1973 would have their effect reinforced and sustained by a subsequent catch-up in nominal wages (see *Seattle Times*, October 15, 1973). See also the discussion of Walter Heller in Section III below.

but, instead, as a measure to be used in unison with controls. Indeed, over most of 1973 he remained in favor of mandatory wage and price controls. "We must recognize that the inflation problem has not disappeared, and mandatory features may have to be used," Burns observed early in the year (*Manchester Union Leader* (New Hampshire), February 25, 1973).

In the very late 1973 timeframe, Burns moved away from advocacy of compulsory controls. As has been stressed in previous chapters and further in Chapter 8 below, even beyond 1973 Burns remained an ardent proponent of a nonmonetary view of inflation. Furthermore, the profit-push aspect of cost-push views that Burns had highlighted during 1973 received a further fillip in U.S. economic debate in 1974–1975, with revived interest in "administered price inflation" in media discussions and, to Friedman's consternation, also in federal government actions, including the jawboning of firms regarding their price decisions. But a constant after 1973 was that, as far as incomes policy was concerned, Burns had replaced his advocacy of compulsory wage and price controls with an endorsement of some other form of an organized national incomes policy.

Even during the first half of the 1970s, however, Friedman and Burns were on the same page on one aspect of compulsory controls. Both of them opposed the idea of extending controls to U.S. interest rates.

The harmony of views between Burns and Friedman on this issue may seem anomalous in view of the enormous amount of criticism that Burns took on the question of imposed interest-rate limits—both at the time (including, in certain respects, from Friedman) and, especially, in retrospect. Indeed, a number of postmortems on the 1970s experience have indicted Burns both for allegedly keeping the federal funds rate policy loose due to fear of ceilings and for essentially acquiescing in a *de facto* formalization of interest-rate controls. It turns out, however, that the first part of this indictment (that on Burns' monetary policy) lacks merit and that the second part of the indictment (on folding U.S. interest rates into the Nixon controls system) has a weak basis.

An early expression of the indictment just sketched appeared in the U.S. media in 1973 (*Manchester Union Leader* (New Hampshire), April 9, 1973).[76] Burns, it was said, had two committee-heading jobs that were "contradictory... As chairman of the Cost of Living Council's Committee on Interest and Dividends (CID), Dr. Burns has a political battle to fight—hold interest rates down. But as Chairman of the Federal Reserve Board he must, in an independent way, tighten money (higher interest rates) to fight the spreading inflation."

[76] See also, for example, *Arizona Republic* (Phoenix), February 21, 1973.

With regard to the second of the roles just enumerated, it has been alleged by some commentators that, as Federal Reserve Board (and FOMC) Chair, Burns was motivated to ease monetary policy by the obligation, seen as part of his CID-chair role, to hold down interest rates. But actual evidence that monetary policy was thus motivated is meager. Meltzer (2009b, p. 767), for example, claimed the conflict was "one explanation of expansive policy in 1972," but he did not actually pursue this explanation.[77] Furthermore, the judgment that monetary policy was expansive in 1972, although likely accurate, rests centrally on evaluations based on monetary growth. Nominal interest rates actually rose in 1972, and further in 1973 (when monetary policy tightened considerably on both interest-rate and monetary-growth criteria).

Chappell, McGregor, and Vermilyea (2005, p. 151) professed to have found a smoking gun on the matter, in a quotation they italicized that appeared in the records (released in the late 1970s) of internal policy deliberations—specifically, in the Memorandum of Discussion for the FOMC's August 1972 meeting.[78] However, the quotation in question—"In the circumstances, the Federal Reserve should not be eager to raise interest rates"—does not really validate their claim, as the actual context of the quotation indicates that the FOMC *did* decide to raise interest rates, and the question at issue in the Committee discussion largely boiled down to whether to implement the whole increase over the following four weeks or to spread the same planned increase over a roughly seven-week period through late September.[79]

Furthermore, at the same August 1972 meeting Burns indicated that he thought that the phased rate increase might well achieve a rate of monetary growth somewhat lower than that for which the FOMC was planning, and that he would be "delighted" in the event of such an undershooting.[80] Monetary growth ended up, of course, being very high in 1972 (particularly on revised data). The record of internal deliberations *does* support taking Burns to task on the ground that Friedman would frequently cite—that the FOMC had a poor grasp of what interest rates were appropriate for achieving moderate monetary growth. In contrast, the alternative and prevalent narrative, in which the focus is on the looming threat of interest-rate-ceilings—an

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[77] In a similar vein, Cagan (1973, p. 26) juxtaposed what he called the "unhelpful conflict of objectives" inherent in Burns' dual role and the fact that the FOMC did not in 1972 really adopt a reserves-based operating procedure and instead continued to set the funds rate. The juxtaposition was not really well taken, as the FOMC's concern with interest-rate stabilization had little connection with Burns' dual role, and, as Cagan acknowledged, the Committee did raise interest rates appreciably in 1972 and 1973.

[78] At different points, Chappell, McGregor, and Vermilyea (2005, p. 151) sourced this exchange to both the July and August 1972 FOMC meetings. The latter is the correct date. (The Memoranda of Discussion—transcript-like records—for 1972's FOMC meetings were only distributed internally at the time of their composition, but they had been released into the public record by the summer of 1978: see Poole, 1979, p. 473, and Chapter 8 below.)

[79] See Federal Open Market Committee (1972a, pp. 75–76).

[80] Federal Open Market Committee (1972a, p. 78).

account in which Burns, to forestall this threat, deliberately held the federal funds rate down—is not supported. Nor, as discussed in Nelson (2020b, Chapter 15), did Burns consciously cultivate an economic overheating. The FOMC records do not validate Chappell, McGregor, and Vermilyea's (2005, p. 151) inference that the specter of interest-rate ceilings meant that "Burns felt that interest rates could not be allowed to increase" (in fact, they did increase) or that "this led him to advocate monetary ease to his colleagues on the FOMC" (in fact, Burns believed he was tightening monetary policy—and a number of his colleagues favored a policy *easier* than what Burns had wanted).

Furthermore, the federal funds rate continued to be raised during the final quarter of 1972. In addition, Burns declared in early 1973 that there was "a need to practice greater moderation during 1973 in the provision of new supplies of money and credit."[81] As discussed in Chapter 2, this signal was followed up with further increases in the federal funds rate, and a fall in monetary growth, during 1973.

The conduct of monetary policy, and in particular increases in market interest rates, therefore proceeded very much as they likely would have even if Burns had not accepted the responsibility of heading the CID.

The conclusion just given does not dispose of the other part of the widespread indictment of Burns: that he held down interest rates—such as those set by commercial banks. Here too, however, Burns' record, although it did involve compromises, does not really warrant the specific charge made against him.

It is widely agreed that Burns accepted the CID post for two related reasons. First, he believed that, by doing so, he could keep monetary policy away from the jurisdiction of the formal controls system. Burns regarded this insulation as desirable in part because, although he was a proponent during the 1970s of the strength of cost-push forces arising from pricing patterns in labor, commodity, and final product markets, he did not happen to be a subscriber to interest-cost-push inflation mechanisms.[82] He therefore did not see merit in proposals to hold down open-

[81] Burns (1973d, p. 167), also quoted in *Washington Post*, February 21, 1973, p. A1.

[82] See, for example, his testimony of August 7 and 8, 1974, in Committee on Banking and Currency, U.S. House of Representatives (1974, pp. 258, 304–305), and of September 25, 1974, in Committee on the Budget, U.S. House of Representatives (1974, p. 125). In a similar vein, Paul Samuelson—although he was an advocate of incomes policy of longer standing than was Burns and was a longtime believer in the importance of cost-push forces—had indicated that he was skeptical of interest-cost-push explanations for U.S. inflation: "Although Congressman Wright Patman will not believe it, the high interest rates are more the *result* than the *cause* of the inflation." (*Newsweek*, June 23, 1969.)

market rates as an anti-inflation measure.

Second, Burns is widely accepted as having agreed to head the CID because being in charge of this monitoring body could serve as a rearguard move against the installation of a system of compulsory controls on interest rates (see, for example, Meltzer, 2009b, p. 767). This was also Friedman's interpretation of Burns' decision to head the CID. Friedman feared that, nevertheless, pressure to bring interest rates into the strict U.S. controls system would become irresistible (Instructional Dynamics Economics Cassette Tape 110, November 1, 1972). With ceilings on open-market interest rates not feasible, a network of compulsory interest-rate control was likely in practice to apply to the administered rates charged by commercial banks and similar financial intermediaries.

"Of course, in a way, I'm not really presenting the situation properly—because we now do [already] have controls over interest rates," Friedman remarked. "One really should say: 'Will the present controls over interest rates be expanded?'"

As Friedman implied, existing legislation gave the Federal Reserve Board the power to control deposit interest rates—a power it used by imposing ceiling rates on banks' demand deposit and retail time deposit liabilities, through Regulation Q. However, the Federal Reserve lacked the power to put ceilings on banks' interest rates on *loans*. The statutory powers that gave Nixon the ability to impose wage and price controls did confer to the president the authority to impose lending-rate controls (*Japan Times* (Tokyo), March 24, 1973). Friedman saw formal lending-rate controls, if introduced in the United States, as likely to be implemented via a broadening of Regulation Q system to cover loan rates, presumably on the basis of the statutory power that Nixon possessed. Such an arrangement, if implemented, would parallel the system already long in force in Australia—whose authorities imposed comprehensive ceilings on both the deposit and lending rates that commercial banks could set.[83]

Friedman did not see such a formal interest-rate control system as likely to hold down the true cost of lending—"Ingenious people will find ways around it," he remarked (Instructional Dynamics Economics Cassette Tape 110, November 1, 1972). Nor did he believe that interest-rate ceilings would prevent a monetary policy tightening from being transmitted to aggregate economic activity. But a system of ceilings on lending rates would force banks to rely on nonprice methods in allocating their loan funds and would divert resources toward the evasion of the controls. Consequently, Friedman believed, lending-rate controls would lower the efficiency

[83] See, for example, Committee on the Working of the Monetary System (1960, p. 323) and Saunders (1972, p. 156).

of the economy and reduce U.S. aggregate potential output.

In the event, such a formalization of interest-rate ceilings was avoided, even though the event that Friedman feared would provide the impetus for such ceilings—substantially rising market interest rates—did actually take place in 1972 and 1973. Burns *was* involved in more intensive use during 1973 of the CID's informal measures to discourage increases in banks' interest rates. Even here, however, he confined himself primarily to one key bank lending rate and, as will be seen, he did allow a modicum of flexibility to continue in the setting of this rate.

The rate in question was the prime rate—a rate chosen by each of the large banks for its business customers. Though an administered rate, it was traditionally quite responsive to market rates. In February 1973, Burns succeeded in persuading some key banks to claw back an increase in the prime rate to 6.25 percent in favor of a rise to only 6 percent. Consistent with Friedman's prediction that ways would be found to bypass interest-rate limitations, it was reported that, in the face of this restriction, banks were able to impose a *de facto* increase in lending rates by requiring higher compensating deposits (funds stemming from a loan that, on receipt, had to be retained on deposit with the lending bank) and by raising other bank lending rates (*Manchester Union Leader* (New Hampshire), February 23, 1973, p. 9). Also, as one reaction was that greater commercial paper issuance substituted for bank loan creation, many business borrowers did, in practice, face the higher financing costs implied by rising market rates (Abken, 1981, p. 14).

In March, Burns again intervened, with the CID issuing a statement that banks' recent increase in the prime rate to 6.75 percent was "not justified at this time." In response, the Continental Bank, for example, lowered its prime rate to 6.5 percent (*Japan Times* (Tokyo), March 24, 1973, p. 10). Even in this climate, however, there remained elements of flexibility in Burns' posture. It was understood that the authorities would not be averse to rises in the prime rate continuing, provided that these occurred in gradual quarter-point increments and that they did actually wish to see a decline in the growth of commercial bank lending growth (*Manchester Union Leader* (New Hampshire), April 2, 1973).

In April, a compromise of sorts came into force. A two-tier system was put in for the prime rate. Under this system, large firms would borrow at a rate closely tied to market rates, while smaller enterprises would face a less frequently adjusted rate (Meltzer, 2009b, pp. 812, 814). Even this "dual prime rate" compromise arrangement proved short-lived—as the CID's restrictions on bank lending rates were essentially removed in the fourth quarter of 1973 (Abken, 1981, p. 14).

The CID, along with the Cost of Living Council apparatus of which it was part, was then dissolved not long after compulsory controls themselves expired at the end of April 1974.

Crude and inelegant though they had been, Burns' CID activities had been successful in preventing the specter of interest-rate loan ceilings from either trespassing on monetary policy or impinging on commercial bank lending rates in a lasting way. And as Burns' support for mandatory controls in other areas dissipated after late 1973, and the legal authority for the Nixon controls expired, the dual-prime rate episode was followed both by the withdrawal of the voluntary CID system and by the end of mandatory controls in the wage-and-price area—and not by the development that Friedman had feared in 1972: that is, the extension of mandatory controls to the interest-rate realm. Indeed, the overall trend in the 1970s was toward more flexible bank-loan rates—a trend exemplified by the fact that Citibank and other large commercial banks moved in the first half of the 1970s to weekly, in place of monthly, variations in their posted prime rate (Wonnacott, 1974, p. 199).

As indicated in Chapter 2, Friedman was unhappy with Burns' acceptance of the CID post, and with Burns' jawboning with regard to the prime rate, not least because doing so gave official imprimatur to the idea of usury. "Arthur Burns, who in general has been a very strong and effective advocate of free enterprise and of Adam Smith's positions—at least, he *was* [before 1970]," Friedman remarked, "has… in the last couple of years in effect come out in favor of the equivalent of usury legislation. The committee on the control of dividends and interest is a form of informal usury." (Instructional Dynamics Economics Cassette Tape 122, June 6, 1973.) In this commentary, Friedman contrasted Burns' stand with that of Jeremy Bentham. Three years earlier, in a *Newsweek* column of April 6, 1970, Friedman had paid homage to Bentham (1787) by composing his own "Defense of Usury." In that column, Friedman had remarked, "I know of no economist of any standing from [Bentham's] time to this who has favored a legal limit on the rate of interest that borrowers could pay or lenders receive—though there must have been some."

By around September 1974, when Friedman was selecting this column for inclusion in his *There's No Such Thing As a Free Lunch* collection, the controversy over Burns' participation in the now-disbanded CID had cooled. By this point, Friedman evidently judged that the description of Burns as an advocate of usury was not sufficiently well established to justify appending a reference to Burns to the aforementioned sentence in the 1970 column. Friedman did, nevertheless, decide to make a different qualification to that 1970 sentence. Specifically, the

footnote that he added in 1974 read: "Since I wrote this, my attention has been directed to favorable comments on usury laws by J.M. Keynes in his *General Theory* [1936]."[84]

Friedman and the Cambridge school of monetary economics

The reexamination of Keynes' treatment of usury in the *General Theory* was one of numerous occasions in 1973–1974 when Friedman was involved in revisiting the works of what in June 1974 he called "[s]ome of the greatest minds in economics over the past two centuries."[85] That June 1974 discussion pertained specifically to indexation—a subject on which he read the works of many nineteenth- and early twentieth-century writers. Among these were "Alfred Marshall, the great English economist," who—as Friedman would stress in his 1974 booklet on indexation, *Monetary Correction*—had specifically recommended indexed bonds.[86] In that 1974 discussion, Friedman specifically drew on a Marshall submission to a U.K. parliamentary committee discussion.[87] His source for this quotation—Marshall's posthumously collected *Official Papers* (1926)—was one he would draw upon again on subsequent occasions, including for a Marshall statement quoted in 1982 in Friedman and Schwartz's *Monetary Trends*.[88]

Of course, Friedman's esteem for Marshall was already long-established by 1974. He had reaffirmed it, before his recent round of reading activities, a couple of years earlier when he remarked (Instructional Dynamics Economics Cassette Tape 93, February 23, 1972) that "Alfred Marshall was a great neoclassical economist," adding that *Principles of Economics* (Marshall,

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[84] Friedman (1975a, p. 220). The 1970 column had previously appeared in Friedman's (1972c) collection. This footnote appended to the column was, however, new to the 1975 collection.

[85] Friedman (1974m, p. 63).

[86] See Friedman (1974a, p. 20). Friedman used the words "the great English economist" to describe Marshall both in Friedman (1974a, p. 20) and in Friedman (1974m, p. 63).

[87] See Friedman (1974a, p. 21) for the quotation from an 1886 item in the *Official Papers By Alfred Marshall*. The relevant Marshall submission was printed in the Friedman booklet as an addendum (pp. 33–36).

[88] See Friedman and Schwartz (1982, p. 518). The *Official Papers* collection was an item that played a role in enabling Friedman to pay tribute to Marshall in issues of the *Journal of Political Economy* that were nearly forty years apart. The December 1949 issue had included Friedman's (1949a) paper "The Marshallian Demand Curve." The back page of the October 1989 issue of the journal printed as a curiosity item (along with an acknowledgment that Friedman had suggested its inclusion) a short excerpt from 1888 parliamentary testimony given by Marshall (see University of Chicago Press, 1989), which Friedman had found in the *Official Papers* collection.

Friedman's exposure to Marshall went back to his undergraduate days, as detailed in Nelson (2020a, Chapter 2). Friedman's first article (Friedman, 1935—produced during his years of graduate study) did not mention Marshall. But George Stigler thought that article to be so clearly a development of ideas advanced by Marshall that he used two book reviews—published two decades apart in the *Journal of Political Economy*—to rebuke those responsible for the books (which concerned the secondary literature on Marshall) for not including coverage of Friedman (1935). Thus Stigler (1962, p. 284) criticized the first book for "ignor[ing] the contributions of Friedman, Samuelson, and Georgescu-Roegen on 'the constancy of the marginal utility of money,'" and Stigler (1983, p. 191) lamented the later book's "omissions [of]… major articles by Friedman (1935) and Samuelson (1942)." (That said, as discussed in Nelson, 2020a, Chapter 4, Stigler had reservations concerning later work published by Friedman in the 1940s and 1950s that went into detail about Marshall's views.)

1920) was “a great classic in economic theory.”[89] It happened that Friedman was making these remarks at a time when, unusually, he was himself giving an economics-principles course, having accepted that teaching assignment for his semester (in 1972) at the University of Hawaii.

The reference to Marshall was an example of the affinity with previous generations’ U.K. economists—especially those who had connections to the pre-1930s Cambridge University traditions in economics—that Friedman frequently exhibited.[90]

“I think, in many ways, Friedman is kind of half a nineteenth-century economist,” Thomas Sargent observed. Sargent also recalled that he, Neil Wallace, and John Kareken would, in their lunches together in Minneapolis during the first half of the 1970s, have many discussions of the specifics of Friedman’s writings. “And I remember Jack [Kareken] saying one time, ‘You know, Friedman insisted on presenting things in a way that a nineteenth-century economist could read’” and understand (Thomas Sargent, interview, January 24, 2014). Friedman also picked up turns of phrase from earlier economists’ writings.[91]

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[89] Chow (1997, p. 80) recalled that Friedman remarked, when teaching Price Theory in the fall quarter of 1951, “Marshall is still the best text.”

With regard to contributions to economics in the nineteenth century in eras preceding Marshall’s, it is worth noting that Friedman wrote quite sparingly about one key U.K. debate, notwithstanding its relevance to his field of interest: the Banking School versus Currency School controversy. The positions Friedman took in his monetarist period from 1951 on would be said by others to make him a modern-day successor to the members of the Currency School. In support of this claim, there would be cited both Friedman’s advocacy of a monetary policy rule and his rejection of the idea that observed money/income relations were evidence that the central bank could not control the money stock (for the drawing of these parallels, see, respectively, Hicks, 1967, p. viii, and Humphrey, 1988, pp. 3, 9). But in his own writings Friedman did not dwell on the Banking School/Currency School debate. Friedman and Schwartz (1970, pp. 94–95), for example, discussed how the debate bore on the issues of defining money and monetary control, while Friedman and Schwartz (1982, p. 113) referred to the implications of 1844’s banking act for the reporting of U.K. banknote data. Also, Anna Schwartz later wrote a *New Palgrave* dictionary entry on the debate (Schwartz, 1987). Previously, she had supplied Friedman with a John Stuart Mill 1844-vintage quotation that was relevant to Friedman’s debate with Tobin about the transmission mechanism of monetary policy (see Friedman, 1972a, p. 923).

Still earlier, Friedman was involved in the supervision of a University of Chicago doctoral dissertation on the debate and the 1844 Act: Adie (1968), later partially published as Adie (1970). The dissertation originated in a suggestion to Adie made by Friedman in 1965 soon after the latter had read, with admiration, Fetter’s (1965) book *Development of British Monetary Orthodoxy*. Reading that book had left Friedman curious about why quantity-based monetary policy rules had not figured more heavily in U.K. monetary thinking beyond the 1840s. In answering this question, Adie recalled: “I used the extensive resources of the U. of C. library (Crerar and Regenstein) and took extensive notes on many old books. I noticed from the borrowing tickets that I was sometimes the first person to borrow the book since Viner, Knight, or Simons.” George Stigler was initially Adie’s principal supervisor for this thesis, but Friedman later took on that role. “Friedman put me more at ease. He made specific comments on what I should do to complete the dissertation. Friedman’s contribution was more on the overall organization of the dissertation into chapters, the theory (application of Phillip Cagan’s theory to [the] nineteenth-century U.K.), empirical work, and overall cohesion.” (Douglas Adie, personal communication, January 2, 2015.)

[90] See also Presley (1986), Nelson (2009), and Boianovsky (2018).

[91] For a couple of examples, see Nelson (2020a, Chapter 3). Leeson (1998, p. 69) might be read as having found another example, as he stated that Friedman (1955a) used, in discussing flaws in Walras’ analysis, “almost identical

The connection was also stressed in an exposition of Friedman's monetary analysis that Harry Johnson and Brian Griffiths wrote for U.K. newspaper readers in 1968. "This approach, incidentally, is logically very close to that of the Marshallian tradition at Cambridge last represented by the late Sir Dennis Robertson, which the Keynesians virtually succeeded in stamping out as an intellectual force…" (*The Times* (London), October 29, 1968.)

In 1973, Friedman further linked his name with Robertson by publishing a debate they had carried out by correspondence in 1954.[92] It hardly made momentous reading—being largely concerned with issues related to agricultural price support. Though this subject had, to a small degree, been an area of interest for Friedman during the mid-1950s, it was very distant from the main research topic shared by Friedman and Robertson—namely, monetary analysis. Friedman's admiration for Robertson's work in the monetary area—especially those Robertson writings that originated in the interwar period—was underlined in 1974 when, in his *Encyclopaedia Britannica* entry discussed in the previous chapter, he included Robertson's book *Money* (in its 1956 fifth edition) among the recommended readings.[93] Correspondingly, when, in the early 1980s, he was pressed to name a comprehensible account of monetary economics, Friedman suggested: "There is another book which I think you would also find very useful: one of the Cambridge Economic Handbook series written by Dennis H. Robertson called *Money*, which was published 30, 40 years ago [actually, longer than that] and has now [actually, in 1956] been reprinted and is published by the University of Chicago Press."[94]

In the first half of the 1970s, another giant among the Cambridge School of monetary economics had his work begin to be systematically reprinted. With the published material supplemented by new annotations, correspondence, and other items, the *Collected Writings of John Maynard Keynes* volumes for 1971 to 1973 included Keynes' key monetary books, *A Tract on Monetary Reform*, *A Treatise on Money*, and the *General Theory*.[95] Their appearance was followed by Patinkin's (1975, 1976) analysis of the development of Keynes' monetary thought.

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words" to those that Keynes had employed in his own discussion of Walras. It turns out, however, that the Friedman and Keynes discussions are not actually very similar, as the single notable similarity is the use of the one word "nonsense" by both authors. And Friedman in 1955 could not have picked this description up from Keynes, as he had not seen the Keynes critique in question. That critique had been articulated in private correspondence in which Keynes participated in December 1934 (see Skidelsky, 1992, p. 615).

[92] See Friedman and Robertson (1973).

[93] Friedman (1974f, p. 356). Previously, Friedman had included this book as a reading on various occasions when he taught a course in monetary economics, including in the 1940s (Hammond, 1999, p. 462) and in the 1960s (for example, in the 1966 course referred to below). He also paid tribute to the book during the debate (concerning utility functions) that he had in print with Robertson (see Friedman, 1955b, p. 405).

[94] In Martin (1983, p. 61).

[95] See Moggridge (1971a, 1971b, 1971c, 1973) and Patinkin (1975).

Friedman, like Patinkin, applauded the collected works' appearance. He and Schwartz updated their *Monetary Trends* manuscript in order to change their references to some of Keynes' published work to its reprinted form in the *Collected Writings*.[96] Friedman was also an enthusiastic reader of the succeeding volumes of the *Collected Writings*—an enthusiasm that would lead him on one occasion to give the curious citation "Keynes (1980, 385, 387, 388)."[97] But, with regard to the monetary writings collected in the 1971–1973 volumes, Friedman's perspective was very different from Patinkin's. Patinkin (1975, p. 249) viewed the *Collected Writings* as useful for studying the improvement in Keynes' conceptual framework: he saw the *Tract*, *Treatise*, and *General Theory* as in ascending order in their intellectual merit and as culminating in "the revolutionary work with which [Keynes] changed the face of monetary theory." Friedman, in contrast, was an admirer of the ingenuity and scope of the *General Theory*, but he saw Keynes as, nevertheless, moving further and further from the correct analysis. The *Tract*, he believed, had shown Keynes to be a "highly sophisticated quantity theorist" but eventually, in the *General Theory*, Keynes put the economics profession onto the wrong track by eschewing the quantity theory of money in his diagnosis of the Great Depression.[98]

In fact, Friedman considered the *Tract on Monetary Reform* to be Keynes' best book, and he lamented to an economist audience in December 1971 that most of them likely had not read it.[99] His assessment of the *Tract* differed greatly from that reached by Don Patinkin—even when it came to the matter of whether it was valid to classify the *Tract* as part of Keynes' monetary research. Patinkin challenged whether the *Tract* could be considered among Keynes' research writings—and even suggested that it was not really a book at all.[100] Friedman, in contrast, considered the *Tract* to be a major research contribution—though he would grant that it was "more popular, less technical" than the later *General Theory* (*The Economist* (London), June 4,

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[96] See Friedman and Schwartz's (1982, p. 46) citation of the *Collected Writings* reprint of Keynes (1923). (Like many, Friedman and Schwartz incorrectly referred to the Keynes *Collected Writings* as the "*Collected Works*.")
[97] See Friedman (1986b, p. 1). Here, Friedman really meant Moggridge (1980), which he was citing because of the Keynes correspondence printed therein. On Friedman's enthusiasm about the *Collected Writings*, see Friedman (1989, p. 68; 1997, p. 23).
[98] For the quotation, see Friedman (1974f, p. 356), and for Friedman's judgment that Keynes put economics onto the wrong track via his *General Theory* analysis, see Friedman and Schwartz (1982, p. 621) and *The Economist* (London), June 4, 1983, p. 37.
[99] Friedman described the *Tract* as Keynes' "greatest book" in Friedman (1972d, p. 16) and Instructional Dynamics Economics Cassette Tape 213 (mid-May 1977); as his "best book" in *The Economist* (London), June 4, 1983, p. 35, Friedman (1989, p. 34; 1997, p. 2), and elsewhere; and as one of his best books in *The Times* (London), September 13, 1976. The remark on the fact that many economists had not read in the *Tract* was in Friedman (1972d, p. 16).
[100] See Patinkin (1975, pp. 253–254; 1976, pp. 11–12) for his case that the *Tract* was intended for a general audience and that it had not really been transformed into a *bona fide* book from its original form of newspaper articles. (See also the index subentry in Patinkin, 1976, p. 158, in which the *Tract* was described as "not really a book.") The evaluation in Winch (1969, p. 147) was, however, closer to that Friedman voiced, as Winch counted the *Tract* among Keynes' "scholarly writings."

1983, p. 35).

The appeal of *Tract on Monetary Reform* to Friedman was essentially twofold. The first element was its domestic-economy aspects. Here, the content of the book won his praise due to Keynes' use of the quantity theory of money to explain price-level movements. Consequently, in Friedman's view, the analysis in the *Tract* was a fitting continuation of Marshall's approach toward the treatment of aggregate economic behavior (*The Economist* (London), June 4, 1983, p. 35). Keynes' aggregate economic analysis in the *Tract* also aligned substantially with Friedman's own monetarist framework, as articulated by him since 1950. When asked in a 1970 U.K. broadcast whether his position on inflation and monetary growth amounted to saying that Keynes was wrong, Friedman countered: "No—Keynes himself, in many of his writings, has said the same thing." (*The Listener* (London), February 11, 1971, p. 170.) The *Tract* was the most prominent instance of such writings, and Friedman later further highlighted the connection between the *Tract*'s analysis and his own posture when he praised the book's "straight monetarist interpretation" of historical inflations (*The Times* (London), September 13, 1976).[101]

Friedman regarded the *Tract* as exemplifying the emphasis that Keynes placed on the importance of money in his writings up to 1930. That year was a cutoff point for Friedman because of the appearance of Keynes' *Treatise on Money*. Although that book was certainly concerned with monetary matters, Friedman came to see the *Treatise* as marking a move away from Keynes' confidence in the effectiveness of monetary policy. Friedman's judgment to this effect was likely driven by the scope that the *Treatise* allowed for cost-push factors to affect inflation.[102]

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[101] In a similar vein, Winch (1969, p. 147) judged the *Tract*'s analysis to be "based squarely on the quantity theory of money."

[102] Friedman studied *Treatise on Money* in his graduate course on monetary economics at the University of Chicago in the early 1930s (see Leeson, 2003b), and Friedman and Schwartz's (1982) forty-page Section 10.7 would refer extensively to the *Treatise*. However, their coverage of that book stemmed from the empirical observations that it contained regarding the Gibson Paradox—the alleged link between the levels of prices and nominal interest rates—and not because of its framework for monetary analysis. Indeed, Friedman referred to Keynes as "a great proponent of monetary policy before 1930" (Friedman, 1973b, p. 7), implying that the appearance of the *Treatise* represented a step away from this stance. Correspondingly, Friedman's estimation was that only "parts of" the *Treatise* were compatible with the quantity-theory framework (see the quotation from his letter to Don Patinkin of December 5, 1968, given in Leeson, 2000, p. 241, and in Leeson, 2003b, p. 512).

It might be conjectured that Friedman's reservations about the *Treatise* stemmed from its treatment of money demand, especially in view of the fact that Patinkin (1965, p. 81) suggested that the *General Theory*'s "portfolio approach to monetary theory is presented even more clearly in [*A*] *Treatise on Money*." But Friedman explicitly indicated in print (for example, Friedman, 1956, p. 17, paragraph 18(iii)) and in correspondence that the element of money demand analysis in the General Theory to which he had the strongest objection was special to that book and consisted of the appeal to absolute liquidity preference or the liquidity trap. He stated, in the aforementioned letter to Patinkin of December 5, 1968 (see Leeson, 2003b, p. 512), that "much of [the Treatise and *General Theory*] analysis of liquidity preference is a carry-over from his earlier quantity-theory orientation and only the absolute liquidity preference element [in the *General Theory*] [is] truly Keynesian."

With Keynes having fallen in with nonmonetary analyses of inflation in the *Treatise* and further in the *General Theory*, the result was that the *Tract* was Keynes' best analysis of domestic behavior, in Friedman's estimation.[103]

The main qualification that Friedman appended to his favorable judgment on the domestic-economy analysis in the *Tract* was that he regretted Keynes' (1923, p. 80) deployment of the verbal flourish, "*In the long run* we are all dead" as justification for giving less weight, in policy analysis, to the long-run neutrality results associated with the quantity theory. Friedman concurred with the implied sentiment that short-run nonneutrality, with regard to real variables, of monetary policy actions was something that deserved considerable study. He felt nevertheless that Keynes' formulation about the long run was a "misleading dictum," especially in the money/prices context in which Keynes used it. The consideration that monetary policy actions ultimately find themselves manifested primarily in price-level behavior should not be neglected, Friedman argued. Because the longer-run effects of past monetary policy choices were continuously showing up in the data, it was not appropriate to regard neutrality and price-level-response results as relevant solely for some far-distant time horizon.[104]

The analysis of open-economy issues in the *Tract* provided the second factor that attracted Friedman to the book.[105] Friedman pointed to the *Tract* as illuminating a concept for which Friedman was to provide a name in the economic literature: the open economy "trilemma."[106] This was the circumstance, faced by any open economy, under which free movement of international payments was compatible with national autonomy regarding monetary policy only if the exchange rate floated. Friedman went out of his way, in a preamble in one of the chapters of his *Newsweek* collections, to stress Keynes' role in illuminating this point. "The incompatibility of these objectives [fixed exchange rates, freedom of goods and capital movements, and monetary policy autonomy] was brilliantly demonstrated by John Maynard Keynes in one of his earliest and, in my minority opinion, best books: *A Tract on Monetary*

That the cost-push analysis of inflation figured in the *Treatise*'s analysis was also stressed by Roy Harrod (see Nelson and Schwartz, 2008, p. 839), while Leijonhuvud (1968, p. 104) also acknowledged that the Treatise contemplated a scenario of " 'spontaneous' wage-push."

[103] See, however, Nelson (2020a, Chapter 3) on Friedman's high regard for Keynes' (1940) *How To Pay for the War*—which, being concerned with wartime conditions, returned Keynes' focus on the sources of inflation to excess demand.

[104] See Friedman (1970a, p. 208) and Friedman and Schwartz (1982, p. 46) for the quotation, as well as Friedman's remarks on Keynes' dictum in *The Economist* (London), June 4, 1983 (pp. 35–36).

[105] For a fairly early citation of the *Tract* that Friedman gave on this ground, see Friedman (1960a, pp. 77, 109).

[106] Neely (1999, p. 18) attributed the origin of the "trilemma" terminology (as applied to an open economy's choices) to Obstfeld and Rogoff (1998). As noted below, however, Friedman actually used this label in print fifteen years earlier.

Reform (1923)."[107] This Keynes analysis had been positive economics, of course, and not an endorsement of the floating-rates/no-exchange-controls combination that Friedman espoused.[108] But Friedman's admiration for that positive analysis was something he underlined by referring again, on multiple subsequent occasions in the 1980s, to Keynes' insights into the trilemma concept. Earlier, this admiration had been evident in the fact that in the business-cycles course that he taught at the University of Chicago from the mid-1960s to the early 1970s, Friedman included *Tract* in the reading list for the subject of international economic policy.[109]

A still earlier book by Keynes—1919's *The Economic Consequences of the Peace*—had informed a passage in *Capitalism and Freedom* in which Friedman had discussed inflation. In that passage, Friedman had referred to "Lenin's famous dictum that the most effective way to destroy a society is to destroy its money."[110] *The Economic Consequences of the Peace* had indeed attributed this saying to Lenin, albeit in a tentative manner.[111] But, for most of the late twentieth century, Keynes' attribution was believed to be in error. This interpretation was crystalized, but did not originate, in a 1977 paper by Frank Fetter.[112] Fetter, a historian of

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[107] Friedman (1972c, p. 92) (also in Friedman, 1975a, p. 163). In terms of presenting the trilemma notion explicitly in a list form—in which it is indicated that only two of three specified items are attainable, or that having the first item means that a country can also have either item 2 or item 3, but not both 2 and 3), one of the earliest presentations was by Meade (1961, pp. 37–38): "As long as we entrust to the national governments the main functions of public finance and of policies concerned with the maintenance of full employment, the control of inflation, and the stimulation of economic growth, we cannot preserve *both* a liberal cooperative system of international trade and of foreign aid *and also* fixed exchange rates between national currencies."

During the same year of 1972 in which Friedman's expression of the notion appeared in print, his fellow *Newsweek* columnist Henry Wallich also articulated it. Ungerer (1997, p. 198) observed: "In 1972, Henry Wallich had drawn attention to what he called the 'inconsistent trinity' when he wrote that 'fixed exchange rates, free capital movements, and independent monetary policies are inconsistent'…" Ungerer was specifically referring to a paper that Wallich submitted for discussion at an event on September 24, 1972, in Washington, D.C.: the passage appeared in Wallich (1972a, p. 7), in a section whose heading referred to the "inconsistent trinity" (1972a, p. 6)—a phrase Wallich repeated in his spoken remarks at the event (Wallich, 1972b, p. 42). Wallich (1972b, p. 43) also gave the trinity as a list form in this spoken contribution. Six months earlier, at an event at the Federal Reserve Bank of Chicago held in mid-March 1972 (a time when Friedman was located in the Pacific), Wallich (1972c, p. 143) had given a catalogue of "(*a*) fixed exchange rates; (*b*) free capital movements; and (*c*) ... independent monetary policies," before observing: "We can't really have all [these] three things at once."

[108] See Friedman (1980a, p. 61 [p. 60 of 1991 reprint]; 1984c, p. 157), and *The Economist* (London), June 4, 1983, p. 37. It was in the last of these items that Friedman introduced the term "trilemma" into print. See Nelson (2020d).

[109] This was the case, for example, in Friedman's Winter 1968 course, "Income, Employment and the Price Level" (Economics 332) (information supplied by Ann-Marie Meulendyke). Friedman had previously included Keynes (1923, Chapter 4) as a reading for the undergraduate monetary economics course that he taught during his early years as a member of the University of Chicago's economics department (see Hammond, 1999, p. 462).

[110] Friedman (1962a, p. 39). This passage was adapted from Friedman (1962b, p. 219; p. 174 of 1968 reprint).

[111] Keynes (1919, p. 220).

[112] Although White and Schuler (2009, p. 213) give the strong impression that doubts about the quotation originated with the appearance of the Fetter (1977) study, it is clear from Fetter's (1977) own account of earlier discussions (as well as from the 1970 Friedman statement given presently) that, in the fifteen years from 1962 to 1977, skepticism was expressed very widely about the origin of the quotation. Notwithstanding the emergence of this countermovement, attributions of the remark to Lenin also continued, both in the 1970s (with Ronald Reagan among

economic thought, had formerly been based in the greater Chicago area, and his work on the evolution of U.K. monetary doctrine had impressed Friedman.[113] With regard to the attribution to Lenin, both Fetter's (1977) study and Patinkin (1987, p. 32) took Keynes as having been found to be unambiguously incorrect.[114]

By the end of the 1960s, Friedman himself had evidently faced sufficient questioning of his own use of the attribution to Lenin in *Capitalism and Freedom* that he was moved to remark, in a talk given in May 1970: "a long time ago Lenin is supposed to have said (nobody's been able to find it in his writings but it's always attributed to Lenin—I think it is Keynes who first attributed it to Lenin[,] so maybe we should say 'Keynes said') that the way to destroy a capitalist society is to debauch its currency. Inflate."[115] Correspondingly, Friedman himself went into "Keynes said" mode on this matter. In the 1980s, in a couple of articles and in the book version of *Free To Choose* cowritten with Rose Friedman, he directly quoted Keynes (1919, pp. 220–221) on the effects of inflation and did not repeat Keynes' related invocation of Lenin's name.[116]

Friedman indicated, however, that he was not altogether convinced that the Lenin usage was apocryphal when he remarked: "You know, Lenin is supposed to have said that the way to destroy a capitalist society is to debauch its currency." (*Human Behavior*, November 1978, p. 33.) And, as it turns out, more recent investigations of the totality of Lenin's statements have found evidence of him having made a remark on inflation that is broadly in keeping with what Keynes (1919) attributed to him (see White and Schuler, 2009, as well as Bartlett, 2009, p. 44).[117] So it is possible that Keynes (1919) drew on this or an earlier Lenin remark along these

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those making such attributions: see, for example, Kiewe and Houck, 1991, p. 108) and the 1980s (see, for example, *Financial Times* (London), October 26, 1985).

[113] Friedman had included an earlier Fetter contribution—Fetter (1965), referred to above—in the reading list on monetary standards and international monetary arrangements for his Fall 1966 graduate course on money, Economics 331. Information from Ann-Marie Meulendyke.)

[114] Fetter (1977) focused on the opposition to inflation expressed in Lenin's statements—a fact that was, in itself, not inconsistent with the remark Keynes attributed to Lenin (in which inflation was said to be corrosive to society and the market system—not that it was *per se* desirable).

[115] Friedman (1970g, p. 83).

[116] See Friedman and Friedman (1980, pp. 268, 325). In Friedman (1983c, p. 55)—a discussion adapted from the script of a Friedman television appearance—Friedman quoted Keynes (1919) but claimed that Keynes made the remark "back in the twenties." This may have been the result of Friedman taking the quotation from *Free To Choose*. He may have done so without checking that book's endnotes for the source and simply assumed that it came from Keynes (1923). Alternatively, as in Friedman and Friedman (1980), he could have taken the U.S. 1920 edition of Keynes (1919) as that book's first appearance. For his subsequent piece on Keynes for *The Economist* (London) of June 4, 1983, Friedman was clear (see p. 35) that the quotation was from Keynes (1919), but he indicated that he viewed the 1923 *Tract* as adhering to an approach that was in the same spirit as the quotation.

[117] That such an item could have taken a long time to be rediscovered is understandable in light of the fact that researchers in earlier generations lacked access to digitally retrievable records of Lenin's statements. This was, however, compounded by the fact that, as White and Schuler (2009) show, the Lenin item in question—although

lines.[118] That being the case, the attribution to Lenin that Friedman made in *Capitalism and Freedom* may also have been correct, after all.

Of course, the Keynes contribution with which Friedman would always be most associated with, owing to his criticism of its premises, was not any of the pre-1936 Keynes books but, instead, the *General Theory*. With regard to one of the premises with which that book was associated—the multiplier effect on spending and output of fiscal policy actions—Friedman was involved in further contretemps during 1973 and 1974. One of these entanglements was discussed in Chapter 2: his exchange, at the November 1974 conference on monetarism, with James Tobin and Willem Buiter on the effects of "pure" fiscal policy. Another was a brief exchange with Melville J. Ulmer in the American economics magazine, *Challenge*.[119]

In contrast to many of the arguments about fiscal policy in which Friedman was involved, the *Challenge* debate pertained not to whether government deficits generated expansionary effects, but to a different scenario: the case in which a rise in government expenditure is financed by an equal increase in taxes.

Friedman had, in a 1973 interview with *Challenge*, suggested that the net outcome would be very little expansionary impact.[120] When Ulmer wrote to *Challenge* to suggest that Friedman had neglected the balanced-budget multiplier theorem, Friedman replied that Ulmer might be taking too much for granted the uniform validity of the theorem. He then offered a degenerate example—essentially one in which new government purchases can substitute completely for private consumption in household utility—that featured no expansionary effects on aggregate nominal or real spending occurring in response to a balanced-budget spending increase. Although Friedman indicated that this reply was not a "full analysis," it constituted another example in which Friedman deployed arguments other than those resting on the permanent income hypothesis to make a challenge to Keynesian propositions concerning fiscal policy.[121] He

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long in the public record—was not included in his official collected works. Another element in the picture is the fact that—as Garthoff (1982, p. 61) put it—there is an "apparently inexhaustible store of Lenin's writings"—an abundance that is also true, of course, of both Keynes' and Friedman's bodies of public statements.

[118] White and Schuler (2009) and Bartlett (2009) express confidence that a 1919 Lenin remark was the source of Keynes' (1919) attribution. The Lenin remark in question appeared in the English-language financial press in April-May 1919 (see White and Schuler, 2009, pp. 216–218, and Office of the Director of the Mint, 1919, p. 225), so it could have been the basis for the attribution in *Economic Consequences of the Peace*, which appeared near the end of the year. Of course, Lenin could well have made similar remarks concerning inflation in the public record on occasions earlier than 1919, and one of these might actually have been Keynes' underlying source.

[119] See Ulmer (1974) and Friedman (1974n).

[120] Friedman (1973c, p. 35).

[121] The quotation is from Friedman (1974n, p. 64).

had, however, drawn on that hypothesis in 1972, in his *JPE* symposium arguments against the effectiveness of fiscal policy, and would do so much more intensively during the second half of the 1970s and beyond (see Chapter 8).

The *General Theory* was famous, of course, not only for its espousal of the spending multiplier but for the challenge that it offered to the effectiveness of monetary policy and more generally to the primacy of monetary accounts of business-cycle and price movements. The fact that there was a preexisting established body of such monetary accounts was largely why, in 1977, Friedman would take umbrage at an article, written by Irving Kristol for the *Wall Street Journal* (in which Kristol was, in fact, mainly critical in the perspective he offered about Keynes). Friedman's voice was encrusted with sarcasm and scorn as he read out a claim Kristol had made that the *General Theory* had "involved the creation of 'macroeconomics' as an independent field of economic theory" (*Wall Street Journal*, May 9, 1977, quoted by Friedman in Instructional Dynamics Economics Cassette Tape 213, May 11, 1977).[122]

The liquidity trap and the real balance effect

This was not to say that the *General Theory* had not expounded an original theory. In fact, Friedman repeatedly praised Keynes for developing a coherent nonmonetary account of the Great Depression (see Nelson, 2020b, Chapter 12). Advocacy of public-works programs as a depression remedy was by no means new to the 1930s or the Keynesian revolution (see, for example, Hutchison, 1978, pp. 175−195, and Winch, 1969, p. 153), but Keynes' *General Theory* gave such prescriptions stronger analytical backing. Furthermore, Keynes' analysis, by raising the prospect of a liquidity trap and by lending credence to that trap scenario, apparently demonstrated that monetary stimulus did *not* offer an alternative way out of an economic slump.

From the later 1940s onward, Friedman eschewed the *General Theory*'s critique of monetary policy. He readily accepted the emerging findings that augmenting that book's analysis with a real balance effect, bearing on decisions to spend on consumer goods, was sufficient to refute the existence of a liquidity trap. Provided that such a wealth effect was in operation at least to some degree, it would ensure that, from a starting point of the economy having unused resources, either a sufficiently large increase in the nominal money stock (generated by policy actions) or a

[122] In this commentary, Friedman cited, as an example of a nineteenth-century economist predominantly concerned with macroeconomic matters, William Stanley Jevons—someone whose work Friedman had had occasion to revisit in 1974 when researching indexation (see Nelson, 2020b, Chapter 15) and who, he also had found, had stated that "*an expansion of the currency occurs one or two years previous to a rise of prices*" (Jevons, 1884, p. 107, emphasis in original).

sufficiently steep fall in the price level (induced by the presence of economic slack) could be counted on to deliver an eventual restoration of full employment. Crucially, this return to full-employment conditions would happen even if, as Keynes had suggested, the enlargement of the money stock was not associated with reductions in interest rates. Friedman regarded the establishment of a real balance effect of this kind as a theoretical breakthrough—and as a rebuttal to Keynes' hypothesis of the liquidity trap.

Nevertheless, and notwithstanding his focus on the connections between money and aggregate demand, Friedman did *not* regard the real balance effect as important for the understanding of business cycles.[123] Certainly, Friedman cited Don Patinkin's work on the real balance effect, and he praised Patinkin's contribution to the theoretical development of the effect.[124] But he did himself not share the Patinkin's view that this effect formed a central part of the quantity theory of money or that its operation was critical for the understanding of empirical business-cycle fluctuations.

An example of the contrast between the Friedman and Patinkin viewpoints is brought out by the fact that, in revising his famous 1948 paper on the real balance effect for a 1951 readings collection, Patinkin thanked Friedman for comments that had stimulated a new passage that Patinkin added. In this new passage, Patinkin made reference to the notion that, "as a result of increasing the amount of money in the economy, individuals' cash balances are larger than desired… so that they will attempt to reduce these real balances by increasing their money expenditures."[125] Patinkin correctly noted that this notion was embedded in older expositions of the quantity theory of money. But Patinkin evidently viewed it as axiomatic that the mechanism he had just described was that of the real balance effect in operation. Similarly, Patinkin (1965, p. 637) took for granted that, when Friedman in 1953 wrote about the reactions of household spending to monetary developments, Friedman was talking about the real balance effect.[126]

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[123] See Nelson (2020a, Chapter 6) for additional discussion and documentation regarding this point.
[124] Although Friedman (1968a, p. 2) credited the origins of the real balance effect only to Pigou and Haberler, in doing so he was, in essence, merely following Patinkin's (1951, p. 250 of 1970 reprint) own attributions to Haberler (1941) and Pigou (1943, 1947). Furthermore, Friedman (1973b, p. 6) named Patinkin among the fundamental contributors to the analysis of the real balance effect, and Friedman cited Patinkin (1948, 1951) numerous times over the decades. For example, early on, Friedman (1948a, p. 259) cited both Pigou (1943) and the then-forthcoming Patinkin (1948) article on the real balance effect. Later, Friedman cited Patinkin (1951) in Friedman (1970a, p. 206) and referenced both Patinkin (1948) and Patinkin (1951) in Friedman (1962c, p. 267; 1968d, p. 446) and Friedman and Schwartz (1963a, p. 627; 1982, p. 42).
[125] Patinkin (1951, p. 252 of 1970 reprint).
[126] This Patinkin discussion was referring to Friedman (1953c).
An examination of the discussion in Douglas Fisher (1970) indicates that Patinkin was not alone in invalidly ascribing to Friedman a belief in the strength of real balance effects. Specifically, Fisher (1970, pp. 1343−1344) stated that "it is clear" that a statement in which Friedman referred to the spending reactions that would result in the

It is true that Friedman did exhibit more interest in the empirical importance of the real balance effect in the late 1940s and the first half of the 1950s than he did subsequently.[127] But, by the mid-1950s, he did not regard the real balance effect as central to an understanding of the reaction of spending to monetary policy or as playing an important part in the quantity theory of money.

When, therefore, in 1976 Friedman remarked that his starting point was that a situation of excess money supply meant that the money had to go somewhere and would likely find itself in higher aggregate spending (*The Times* (London), September 13, 1976), he was not describing or highlighting the real balance effect but, instead, making the point that more rapid monetary expansion was associated with the generation of substitution effects. On this view, higher real balances had a counterpart in lower values of some, or many, interest rates, and these rates in turn mattered for spending decisions. And it was overwhelmingly through this mechanism, and not via the real balance effect, that quantity-theory-based connections arose between monetary policy actions and the economy's spending flows.[128] In fact, the experience of the United States in late 1974 and early 1975 provided a possible example of this multiple-rate channel: nominal and real monetary growth indicated policy restriction—in contrast with the behavior of the real federal funds rate, which was predominantly suggesting looseness. The fact that output growth

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event that "individuals as a whole were to try to reduce the number of dollars they held" was a reference to a wealth or real balance effect. Fisher went on to criticize others for treating Friedman as viewing interest rates and permanent income, but not real balances, as the only variables appearing in the consumption function (D. Fisher, 1970, p. 1345). However, the Friedman quotation (which was in his May 1959 testimony in Joint Economic Committee, 1959a, p. 609—and *not* in the Friedman, 1957, book that Fisher cited as the source of the statement) was, on its face, not a remark about agents spending in response to a higher stock of wealth, for *given* asset prices, but instead, referred to their altering their purchases of securities and other items (including some, or many, categories of goods and services), with the aim of restructuring the composition of their asset portfolio. That is, the channel described involved the operation of substitution effects—the simultaneous adjustment of asset prices and spending (real and nominal) in a manner that made these variables' levels compatible with the private sector being willing to hold the outstanding nominal stock of money. This mechanism comes into play, for example, when agents judge that the state of expected income and asset prices do not justify their continuing to hold as much of their savings in the form of money balances. These are not real balance effects in the Pigou-Patinkin sense, and their existence does not imply that real money stock or its growth rate appears directly in the structural IS equation.

[127] See Nelson (2020a, Chapter 5). In addition, Hammond (1999, p. 456) documented that at the very beginning of his collaboration with Anna Schwartz in the late 1940s, Friedman was (still) emphasizing the role of the real balance effect in the monetary transmission mechanism. (Hammond, 1999, p. 458, implied that a belief in the importance of the real balance effect made Friedman well disposed toward defining the money stock broadly, as an M2-type concept. In fact, Friedman's strongest advocacy of the real balance effect was in the late 1940s and early 1950s, when he was still often emphasizing a medium-of-exchange conception of money—see Nelson, 2020a, Chapter 6. And by the time he was stressing M2 over M1 as the definition of money, Friedman had dropped his contention that there was a strong real balance effect—see Nelson, 2020b, Chapter 14.)

[128] See Nelson (2020a, Chapters 2 and 5−6) for a detailed discussion of this aspect of Friedman's economic framework. Grossman (1969, p. 1045) was an early discussion that appropriately stressed that there was no reason to view the transmission to real expenditures associated with expansions of real money balances as corresponding solely (or even importantly) to the operation of the real balance effect, and to remark that monetary expansions should be expected to generate expenditure responses at least partly via substitution effects even when these expansions are not effected through open market purchases.

was weak in this period suggested that monetary restriction was indeed in force, while the low value of the real federal funds rate made it a strong possibility that the restriction was being manifested in other, perhaps less observable, interest rates.[129]

Friedman decided to make his denial of the centrality of the real balance effect plainer by reaffirming that denial on a couple of occasions during the mid-1970s. One of these occasions was during a session of the aforementioned November 1974 conference on monetarism. Near the end of his conference remarks, Friedman stipulated that he was not a subscriber to the position that the wealth effects of monetary changes were "of any empirical importance" for the analysis of cyclical movements.[130] The following year, he placed a similar remark in an incongruous location. Friedman used the occasion of the revision of his *Price Theory* text to insert a passage in which he voiced doubt about the quantitative relevance of the real balance effect for short-run macroeconomic analysis.[131] In these passages, both of which would see print in 1976, he was careful also to affirm the analytical importance of the real balance effect—as a mechanism by which monetary policy and the price system could restore full employment, even in the case in which no substitution effects of monetary policy were in operation.

Friedman, therefore, believed that the *General Theory*'s analysis of monetary policy had been refuted. It could be overturned analytically by the incorporation of the real balance effect into the framework under discussion. And, more generally and more practically, by allowing for multiple transmission channels of monetary policy. These multiple channels meant that money creation was bound to generate rises in asset prices and associated increased flows of spending on goods and services. He was, however, also careful to note that Keynesian analysis, along with Keynes (1936) specifically, received coverage, alongside alternative or augmented macroeconomic frameworks, in the graduate courses taught by the University of Chicago's economics department.

[129] See the next chapter. The emphasis in that discussion is on viewing the 1974–1975 period as one in which the natural short-term rate of interest fell. But this interpretation is consistent with actual interest rates other than the short-term rate rising over the period. This is because, in an environment in which multiple rates matter for aggregate demand, rates other than the short-term rate matter for the calculation of the natural short-term interest rate.
[130] Friedman (1976a, p. 317).
[131] See Friedman (1976b, p. 321). (The Pigou effect had also been referred to briefly at the close of the previous edition: see Friedman, 1962c, p. 263.) Hirsch and de Marchi (1990, p. 196) instead gave Friedman (1976b, pp. 231, 317) as discussing the real balance effect. But these references were both transposition errors. What was clearly intended was, instead, to point the reader to the discussions just mentioned here: Friedman (1976a, p. 317; 1976b, p. 321).

II. ISSUES RELATED TO DEBATES ON BUSINESS, FINANCE, AND SOCIAL POLICY, 1973–1974

FRIEDMAN AND FINANCE

Teaching of Keynesian economics at the University of Chicago took place during the 1960s and into the early 1970s not only in the economics department but also in the Graduate School of Business. It was against this background that two members of the business school, Merton H. Miller and Charles W. Upton, found that, as they later put it, by 1970, "we could no longer bring much enthusiasm or conviction to teaching macroeconomics along the then standard Keynesian lines."[132]

Of the two, Miller was so deeply steeped in Keynesian economics that as far back as 1949 Friedman had, in the *American Economic Review*, favorably referred to Miller's empirical findings concerning the fiscal policy multiplier.[133] At that time, Friedman—already skeptical about the mechanical application of Keynesian income-expenditure analysis, but still some years away from becoming an outspoken critic of approaches that emphasized the fiscal multiplier—was espousing his combined fiscal/monetary policy rule, based on the automatic monetization of cyclical deficits and surpluses. In support of this rule, he had been very well disposed toward the estimates of Musgrave and Miller (1948) concerning the effects, on aggregate economic activity, of U.S. government deficit spending.

Friedman, of course, subsequently became highly disillusioned with the very fiscal-multiplier concept. In the first half of 1974, the most recent outgrowth of this disillusionment consisted of his remarks, discussed above, in the November/December 1973 and March/April 1974 issues of *Challenge*. Miller, too, would go through his own disaffection with fiscal-multiplier analysis, so much so that he and Upton named the multiplier among the "unnecessary concepts" they had omitted from their teaching of macroeconomics in the 1970s.[134]

The occasion for this Miller-Upton remark was the completion of their advanced-undergraduate/business-course textbook *Macroeconomics: A Neoclassical Introduction* (Miller and Upton, 1974). "Both of us had come to the opinion that the old-fashioned Keynesian type stuff had to go," Upton recalled (interview, January 8, 2015). "And so we wrote our book as an

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[132] Miller and Upton (1986, p. ix).
[133] See Friedman (1949b) and the discussion in Nelson (2020a, Chapter 4).
[134] Miller and Upton (1974; p. xiv of 1986 reprint).

alternative—or purgative, depending on your point of view."

In their preface, written in February 1974, the authors anticipated a possible reaction to the appearance of their book: "Any text by authors currently at the University of Chicago will inevitably be labeled… 'monetarist.'… [I]f the operational definition of monetarism is work that has been strongly influenced by the writings of Milton Friedman, then ours is certainly in that class. In fairness to him, however, we should emphasize that his contribution has been that of a capital input through his writings and not a labor input. We have not had the chance to discuss the material with him[,] and none of our sins should be laid at his door."[135]

Charles Upton elaborated (interview, January 8, 2015) that, in the course of writing their text, they never "talked to Friedman about it, but I know Mert talked to Franco [Modigliani] about it." Relatedly, Upton noted of the book's analysis of consumer spending: "We basically adopted sort of a crude, dumbed-down version of Franco's life cycle model, because we found it easier to teach than the Irving Fisher two-period model, or Milton's permanent/temporary [income decomposition]." Consequently, when the authors observed in their preface that the book would differ from other texts by hardly mentioning the concept of permanent income, this reflected their chosen model of consumer expenditures, and the terminology associated with the life cycle model, rather than an effort to avoid coverage of Friedman's research in their book.[136]

Indeed, the Miller-Upton book as a whole was very generous to Friedman. In addition to making considerable bibliographical citation of his work, the authors indicated that they had several areas of emphasis that were shared with those in his writings. There were, to be sure, some exceptions to this pattern. For example, the authors' stated intention of embedding microeconomic foundations into macroeconomics was not an aspiration that drove much of Friedman's work. In addition, the book's implied position on fiscal policy—that Ricardian equivalence, as it would come to be called, was highly empirically relevant—was not something with which Friedman strongly agreed at this stage.[137] But, in its downgrading of fiscal policy as an influence on aggregate demand and its emphasis on the quantity theory of money for the analysis of price-level determination, the Miller-Upton text was clearly striking a tone similar to Friedman's. In addition, Miller and Upton (1974) laid stress on specific topics—inflation as a tax and the real/nominal interest-rate distinction—that had been key Friedman themes over the

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[135] Miller and Upton (1974; p. xv of 1986 reprint).
[136] Miller and Upton (1974; p. xiv of 1986 reprint).
[137] He would reconsider the matter after 1974, as already indicated and as discussed in detail in Chapter 8.

previous two decades and more.[138]

The appearance of *Macroeconomics: A Neoclassical Introduction* demonstrated, Upton noted, that it was still the case that "Mert was interested in macro" (Charles Upton, interview, January 8, 2015). Nevertheless, in the years between 1948, when the fiscal-multiplier work cited by Friedman appeared, and 1974, Miller had worked largely outside the field of macroeconomics. Instead, his research contributions were, to a great degree, in the emerging discipline of finance. Finance continued to be Miller's main interest in the years after 1974, and the scale of Miller's contributions on the subject was reflected in his being one of the winners of the Nobel award in economics in 1990.

On winning the award, Miller was, as he and Upton had been in 1974, generous to Friedman. "When I was in Sweden last summer, I was asked who I would recommend for the next Nobel award for economics. I said my colleague Gary Becker would be an excellent choice, but I also seriously recommended a second Nobel be awarded to Milton Friedman. He received a Nobel for his work in monetary theory, but I said he deserved a second for his work, and influence over the years, in championing free-market economics." (*Atlanta Constitution*, October 21, 1990, p. B1.) The scenario Miller outlined was farfetched: it would be a major break with prior practice to give two economics Nobels to the same person; and Friedman's free-market advocacy appeared largely outside the research literature and so was unlikely to be Nobel-eligible work. Miller's magnanimous remark, however, demonstrated the affinity that Miller had with Friedman on many economic matters.[139]

These generous sentiments were not entirely reciprocated. Miller received the award alongside two other leading finance theorists, Harry Markowitz and William Sharpe. Markowitz noted that "rumor has it that Friedman said [about] the three of us: Markowitz, Sharpe, and Miller: 'Why did they give the Nobel Prize to them? There's a lot of people who are real economists and should have gotten it instead.'" (Harry Markowitz, interview, February 23, 2016.)

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[138] Chapter 13 of Miller and Upton (1974) was titled "Inflation as a tax." This aspect of inflation had been highlighted in numerous past Friedman writings. In addition, Miller and Upton (1986, p. x) noted as a difference from many 1960s treatments their dealing with the real/nominal interest-rate distinction (see Miller and Upton, 1974, pp. 199–201). This distinction had received renewed emphasis after interventions on the matter by Friedman during the 1960s, in research papers and in public debate (see Nelson, 2020a, Chapter 6; 2020b, Chapter 12).

[139] So did other statements that Miller made in the same newspaper interview, such as his observation: "The real problem is not the [U.S. federal] government deficit, but the level of spending." (*Atlanta Constitution*, October 21, 1990, p. B4.)

This attribution to Friedman captured key aspects of his posture toward the field of finance. His was an attitude that was a mixture of disinterest, skepticism, and hostility. It was an attitude that he did not articulate very explicitly in his writings, yet one that nevertheless became well known in the economics profession. The widespread awareness of Friedman's position was reflected in Paul Samuelson's (2009, p. 20) reference to Friedman's "peculiar" dislike for finance. Doubt about "finance theory as a branch of serious economic theory," Samuelson remarked, "… was expressed again and again by the late Milton Friedman, a dizzy view that I still find incomprehensible."

Eugene Fama summarized matters as follows (interview, September 11, 2013): "When Merton Miller, Harry Markowitz, and Bill Sharpe got the Nobel Prize, his comment was, 'Nobody should get a Nobel Prize [if they are] in finance.' So he wasn't just not into it [finance]—he was antagonistic towards it."

"I mean, he made these comments—when Mert Miller got the Nobel Prize—on finance, that that wasn't really economics," Gary Becker recalled (interview, December 13, 2013). "But I didn't notice that [Friedman attitude] before—and I was surprised at that comment, actually."

Friedman's negativity concerning finance became more widely known when it was highlighted in print sources: Markowitz's (1990) Nobel lecture; and Bernstein (1992), an account of the development of the finance field in academia and in Wall Street circles. A much-quoted passage of the Bernstein book was its brief depiction of Harry Markowitz's Ph.D. dissertation defense at the University of Chicago (Bernstein, 1992, p. 60), in which Friedman made an intervention that seemingly threatened to scuttle Markowitz's receipt of a doctorate.[140] This incident had been what Markowitz had been referring to when, in his Nobel lecture in December 1990, he had recounted that "when I defended my dissertation as a student in the Economics Department of the University of Chicago, Professor Milton Friedman argued that portfolio theory was not economics, and that they could not award me a Ph.D. degree in Economics for a dissertation which was not in Economics."[141]

The incident in question occurred in 1955.[142] It had been oft-recounted by Markowitz, who was

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[140] The passage was highlighted in such reviews of the book as Cunningham (1993, p. 48).
[141] Markowitz (1991, p. 476).
[142] This date was given in, for example, American Economic Association (1956, p. 817) (in reference to Markowitz, 1955). Shortly after his successful dissertation defense at the University of Chicago, Markowitz worked at the Cowles Foundation (Yale University) in August 1955-May 1956. During this period, Markowitz did a substantial

Bernstein's source—"everybody in finance knows this story," Markowitz observed (interview, February 23, 2016). Markowitz's years as an on-campus graduate student had ended earlier in the 1950s, leaving him "with everything done except my dissertation. I went off [for employment] to the Rand Corporation." On his 1955 return for his thesis defense, "I remember thinking [at Midway Airport], 'I know this subject cold, portfolio theory. Not even Milton Friedman is going to give me a hard time.' And so, about five minutes into the defense, Friedman says, 'Harry, I've read your dissertation, and I don't find any bugs in it. But this is not a dissertation in economics, and we can't give you a Ph.D. in economics with a dissertation that's not in economics.' And for most of the next hour-and-a-half, it was, you know, Friedman telling me they couldn't give me a Ph.D. in economics for a dissertation that wasn't economics. At one point, he said, 'Harry, you have a problem. It's not economics. It's not mathematics. It's not business administration.'" (Harry Markowitz, interview, February 23, 2016.)[143]

Markowitz added that this story had "a little epilogue that maybe people don't know." Years later, when Friedman and Markowitz were both hired as consultants in California for a firm's project, "I reminded Friedman of this [1955] episode, which had been no big deal in his life. And I said, 'Were you serious?' And he said, "Harry, you know we'd never flunk anybody at that stage.' (*laughter*) He was just pulling my leg." Indeed, by the time of his dissertation defense, Markowitz had already started a stream of published research, including work on the Friedman-Savage utility function (Markowitz, 1952a).[144]

Friedman's posture toward finance and asset pricing

Friedman's underlying doubts about finance as a discipline were, however, deeply felt. Certainly, he did not make these doubts prominent in his own research. In particular, it would be wrong to regard him as undertaking a systematic effort to refute the research findings of the finance literature. His exposure to the literature was too peripheral for that development to occur.[145] But Friedman did make clear his skepticism about the finance enterprise and did so on

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rewrite of the material that had appeared in his thesis, and added new portions of text, thereby producing the book that had the same main title as his dissertation. See Markowitz (1959, pp. xiii–xiv).

[143] Bernstein's (1992, p. 60) account attributed different wording to Friedman (including phrases and shorthand that Friedman was unlikely to have used in 1955). Although he presented it as a verbatim account of Friedman's words, the remarks Bernstein attributed to Friedman came from his own interview with Markowitz.

[144] He had also published early versions of his dissertation work in the *Journal of Finance* (Markowitz, 1952b).

[145] One of Friedman's more abstract papers, Friedman (1969a), did make favorable reference (see its page 35) to Modigliani and Miller (1958). That Modigliani-Miller study would figure heavily in Miller's receipt of the Nobel award and in Miller's (1991) Nobel lecture. However, it was a study of businesses' decisions concerning how to finance their operations, and it had some macroeconomic application. The Modigliani-Miller study was therefore likely of more appeal to Friedman than portfolio theory of the kind associated more closely with the finance field.

many occasions beyond that 1955 confrontation. And his position was, as already noted, well known. Charles Upton, employed at the business school from 1968 to 1976, stressed the strong connections with the economics department: "I would teach a course occasionally in the econ department, and econ students went back and forth, and business students went back and forth… we were sort of thick as thieves." He added a qualification: "But my impression was that in the econ department there was some negativity about finance, because, you know, [the feeling was,] was it real economics?" (Charles Upton, interview, January 8, 2015.)

Eugene Lerner, a student of Friedman's in the 1950s whose interests would move into the finance area, stressed that the finance literature's concentrations on second moments as a driver of private-sector decisions was a source of puzzlement to Friedman. "I think that Markowitz was more mathematics than economics, from Friedman's perspective. Because Friedman was a firm believer in supply and demand, and Markowitz always believed in risk reduction and, you know, correlations and that sort of stuff." (Eugene Lerner, interview, July 29, 2016.)

The emphasis placed by finance on second moments as a prime mover with regard to private-sector decisions was made plain when Stanley Fischer was invited in the 1973/1974 academic year to present a paper at Friedman's money workshop.[146] In the paper (subsequently published as Fischer, 1975), Fischer drew on his MIT colleague Robert Merton's work on analyzing portfolio decisions in a stochastic continuous-time environment (for example, Merton, 1969). At the same time, Fischer endeavored to make his paper relevant and accessible to macroeconomics specialists. Indeed, its topic—indexed bonds—was one on which Friedman had become active, and it was an area of interest common to finance specialists and himself.

Fischer's paper included an archetypal remark of the kind found in the finance literature on prices depending on second moments, with its reference to "the key role of the correlation of equity returns with the rate of inflation in determining the market price at which indexed bonds will be sold."[147] However, the focus of the paper—the real-rate/nominal-rate distinction—was one that was close to Friedman's interests. The paper found, *inter alia*, that the nominal longer-term government bond rate did not decompose cleanly into an expected-inflation term and a real rate, being more appropriately demarcated into an expected-inflation term, a real rate, and an inflation-variance term. The "expected real return on nominal bonds is not the nominal interest

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On other occasions, Miller's writings were embedded more deeply in the latter kind of finance work. For example, he coauthored a book with Eugene Fama, *The Theory of Finance* (Fama and Miller, 1972).

[146] The workshop session was held on March 19, 1974 (University of Chicago library records). See Nelson (2018) for further discussion of this presentation.

[147] Fischer (1975, p. 527).

rate minus the expected rate of inflation," Fischer (1975, p. 519) insisted.[148]

This result provided a clear-cut instance of a finance-analysis result obtained in a monetary-economics context—specifically, an application to the Fisher equation. But it also was a result that pointed toward why Friedman tended to eschew the finance literature. He was aware that, formally, the expression for the nominal interest rate in the Fisher equation contained variance terms beyond the real rate and expected inflation.[149] But Friedman regarded such terms as being, typically, of second-order importance—not only in the mathematical sense, but in the economic sense too. Consequently, he continued to emphasize, contrary to Fischer's (1975) analysis, that the existence of an indexed bond market would allow economists to discern expected inflation from observed long-term rates.[150] This is a position that is most accurate when the variance terms that appear in the full Fisher equation can be neglected.

Friedman also stressed to Fischer, during the latter's workshop visit, that he did not sympathize with the finance literature's approach to modeling uncertainty. Fischer recalled that "at the end of the seminar, somehow it was mentioned in a conversation that my wife and I were taking separate planes back—because, at that time, we didn't have any [adult] relatives in the United States, and we had children. And Milton said, 'Well, now that—that is something I can understand. That seems to me behavior toward risk. But [not] all this stuff that you guys talk about in these papers…'" (Stanley Fischer, interview, August 30, 2013.)

Finance specialists and monetary economics

Another dimension of the problem of communication between Friedman and finance economists is that, when the latter got involved in monetary-economics issues in the 1970s, they had a perspective that was different from both Friedman's monetarism yet was not Keynesianism. Eugene Fama, for example, was skeptical about the existence of short-run wage and price stickiness. In addition—and in contrast to the rational expectations movement of the first half of the 1970s, which was supportive of some key aspects of monetarism—Fama was doubtful of monetary policy's ability to generate large effects on output. He also played down the links between Federal Reserve actions and monetary aggregates broader than the monetary base.[151]

[148] See also Fischer (1975, p. 513). See also Ireland (1996) for some important extensions and applications of this type of result.
[149] See Nelson (2020a, Chapter 6).
[150] See Clarida (2020, p. 12) for a discussion.
[151] For further discussion, see the chapter below, titled "Making Every Moment Count," on developments in macroeconomics in 1987–1992.

With regard to the approach that the portfolio theory of this era offered on monetary economics, Fama remarked that Friedman "never took it seriously, and I think that was a big shortcoming. For example, I think Friedman and Schwartz [that is, the *Monetary History*] was way off the mark... I started going mostly [to the workshop] when I got interested in writing those papers [on monetary issues]… And Fischer Black was here at that time, and he was also deeply interested in exactly the same topics. And Fischer [Black] and I were not typical money workshop people, who were basically always nodding their head at Milton. And he, frankly, did not like [having] people [at the workshop] who contradicted him." (Eugene Fama, interview, September 11, 2013.)

Charles Nelson recalled that, just as Friedman was skeptical about finance, "in turn, the finance people in the business school—Fama, Miller, and others, and particularly Gene [Fama]—were inclined to be a little dismissive of monetary economics and its importance." (Charles Nelson, interview, September 9, 2013.)[152]

When, however, Friedman and Schwartz in 1982's *Monetary Trends* referred to "the theory of efficient markets developed by Eugene Fama and others," it was in a reasonably favorable context.[153] This was because they were citing Fama's (1975, 1976) work that emphasized, as they did, the real/nominal interest-rate distinction and the operation of the Fisher effect on nominal yields.[154] Fama's 1975 study was amenable to Friedman in a further respect: it explicitly defined "the price of money in terms of goods" as $1/P_t$, the inverse of the price level for goods (Fama, 1975, p. 270). This concept of the price of money was something that Friedman, in comments made in November 1974, identified as something that monetarists advocated in their debate with Keynesians (who had, instead, tended to treat interest rates as the price of money).[155]

The modeling of interest rates

The Fama (1975, 1976) studies of the Fisher effect did, however, also provide a clear example of

[152] Though, as indicated above, Merton Miller accepted and expounded the quantity theory of money and so was nowhere near as critical of monetarism as was Fama, it was the case that Miller and Upton (1974; p. xiv of 1986 reprint) indicated their disdain for referring to the velocity of circulation in monetary analysis—a great contrast with Friedman's preference. The appearance of Gould, Miller, Nelson, and Upton (1978) indicated that Miller and Upton had softened their opposition to the employment of the concept of velocity.
[153] Friedman and Schwartz (1982, p. 478).
[154] See also Friedman and Schwartz (1982, pp. 551, 572). As Fama (1975, 1976) was considering short-term interest rates, not longer-term yields, he was able to use Friedman's preferred version of the Fisher equation—one in which the variance term emphasized by Fischer (1975) did not appear. (In contrast to their citation of Fama in this context, Friedman and Schwartz's coverage of *longer-term* interest rates mainly referenced older studies that were concerned with macroeconomic aspects rather than taking a finance-based perspective. See especially their page 501.)
[155] See Chapter 8.

Diebold and Rudebusch's (2013, p. 19) observation: "The modeling of interest rates has long been a prime example of the disconnect between the macro and finance literatures." Diebold and Rudebusch stress that, in the twenty-first century at least, macroeconomic modelers have recognized that the short-term interest rate is set by the central bank—and that it is the interest-rate reaction function that makes interest rates depend on the state of the economy. In contrast, finance-oriented researchers had traditionally tended to treat these rates as market-determined—with their dependence on the course of the economy occurring because of their status as a price, not because of a central-bank interest-rate reaction function. In this tradition, Fama (1975, p. 275) had referred to the Federal Reserve's practice of setting short-term rates as though this policy had ended with the discontinuation of the interest-rate *pegging* regime in the early 1950s. And he described the modern situation with regard to the market for U.S. Treasury bills as one in which "the market… [is] setting security prices" (Fama, 1975, p. 270).[156] Fama therefore did not allow for the fact that the Federal Reserve had continued to administer short-term market interest rates over the previous quarter-century, via its adjustable operating targets for those rates.

This was a matter of interpreting Federal Reserve practice on which Friedman, too, had been frequently criticized. Over the years, many commentators have contended that Friedman failed to acknowledge that the Federal Reserve has predominantly followed a policy based on setting short-term interest rates.[157] This criticism was not accurate: Friedman did on many occasions acknowledge (and criticize) the fact that the FOMC continued, after the Accord, to operate consciously on interest rates during the 1950s, 1960s, and 1970s.[158] Friedman did nevertheless give ample fuel to this criticism—both via his many statements that the Federal Reserve could not control interest rates, and through his numerous descriptions of the reaction of interest rates to the economy that did not present the interest-rate response as flowing from conscious decisions by the central bank.[159] For example, in April 1984, Friedman described interest rates as

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[156] The same impression permeated the study of Roll (1970) titled *The Behavior of Interest Rates*. This monograph—which was published in a book series for which Friedman played a small role, as a member of the panel of selectors—had treated interest rates as market-determined in the United States other than during the 1942–1951 pegging period. The only reference to the Federal Reserve in the book had been on a single page (p. 52) and was in relation to the central bank's role in pegging rates during that era.

[157] See, for example, Tim Congdon's discussion in *The Banker* (London), July 1983. The (possible) exception of the period from 1979 to 1982 will be considered in later chapters.

[158] See Nelson (2020a, Chapter 8) for a discussion of this point, with detailed documentation. Goodhart (1989, p. 330) and Woodford (2003, p. 25) recognized that Friedman, in his discussions of how matters operated in practice, treated short-term interest rates as being policy-determined.

[159] Friedman and Schwartz (1982) also likely encouraged this criticism through some elements of their discussion. Certainly, their theoretical analysis did stress the notion that the central bank can manipulate nominal and real interest rates in the short run, for a given rate of expected inflation (pp. 480–485). Furthermore, they interpreted certain historical increases in U.S. short-term nominal interest rates in terms of a tightening of monetary policy (p. 531) and certain decreases in terms of loosening (p. 527). However, when discussing Fama's (1976) findings,

"the most difficult of all [variables] to forecast," before outlining how short-term rates might react to the evolving U.S. economic outlook.[160]

These various Friedman statements did not actually constitute a denial on his part that the Federal Reserve set the value of short-term interest rates from period to period. They amounted instead to expressions of his position (discussed in Chapter 8 below) that the Federal Reserve would belatedly adjust its interest-rate choices in response to market forces that were creating movements in expected inflation and the natural interest rate. As these delayed adjustments took place, "more basic effects" (as Friedman and Schwartz referred to them) came to the fore as the factors responsible for the course of interest rates—superseding monetary policy's influence on the trajectory of rates.[161]

It is therefore apparent that Friedman, unlike many contributors to the finance literature of the 1970s, *did* recognize that short-term interest rates were not market-determined from quarter to quarter. But he believed that market forces usually made their effect felt on these rates before very long—so that short-term real interest rates, in particular, were essentially market-determined variables over frequencies other than the short run.

Toward an accommodation

Friedman's position toward finance also made itself felt in his interactions with economics students. Fred Levin, a graduate school at the economics department in the early 1970s, recalled that "when I was there, you had to take two [specialized] courses—take two prelims. I wanted to

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Friedman and Schwartz did not forthrightly correct his implication (one not true in a framework in which there was price stickiness in the short run) that the central bank cannot affect real interest rates if expectations of inflation are rational (Friedman and Schwartz, 1982, p. 551). In addition, they were taken to task (see *The Banker* (London), July 1983, p. 120) for uncritically quoting another researcher's statement that U.S. short-term interest rates were determined by the market (Friedman and Schwartz, 1982, p. 543).

It should be noted that many economic researchers in this era often treated it as still being an open question whether the Federal Reserve should be modeled, in the analysis of post-1951 quarterly data, as following an interest-rate reaction function (whereas, today, it would be accepted as having been a reality over this period). For example, Blinder and Solow (1974, p. 75) made the somewhat ambiguous statement that it was a "common suggestion" that "the Federal Reserve's short-run reaction function during the 1950s and early 1960s… [was geared] toward stabilizing nominal interest rates."

[160] In H.R. Heller and others (1984, p. 47).

[161] Friedman and Schwartz (1982, p. 485). This position was shared by many observers. For example, Foster (1979, p. 152), after observing that the U.K. authorities managed short-term rates, added, correctly: "However, this does not mean that a hypothesis [regarding short-term interest-rate determination] involving market forces cannot be tested… [S]uch forces play a predominating part in influencing the [interest-rate] decisions of the authorities…" The main elements left out of this characterization consists of the points that the process of management of interest rates may involve postponing the response of interest rates to market forces and that the different interest-rate pattern resulting from this postponement will have implications for the behavior of the money stock and other magnitudes.

take my other prelim in finance. And for some reason, I had to ask Milton Friedman's permission... And Milton Friedman said, 'No, it's too close to money and banking, and you can't take it.' And so I wound up taking international trade." Levin suspected that Friedman's dim view of "what was being taught over there," at the business school, in its finance courses likely had motivated him to judge that Levin "should stick with the economics department" in his selection of Ph.D. coursework (Fred Levin, interview, March 10, 2014).[162]

Several years earlier, Friedman had been involved in supervising Joseph Burns' dissertation at the University of Chicago. Burns' choice of dissertation title, "The Saving-Investment Process in a Theory of Finance," was provocative from Friedman's perspective: it implied that macroeconomics should be subsumed into the field of finance. And it did so in a manner that alluded to Gurley and Shaw's 1960 book *Money in a Theory of Finance*—a book to which Friedman objected because of its implication that financial innovation would diminish the importance for the economy of monetary policy actions. "Friedman was not very sympathetic to Gurley and Shaw," Joseph Burns recalled. "I did get resistance from Friedman on my thesis." In response to Friedman's requests, Burns made substantial revisions, including deleting some of the dissertation's theoretical analysis (Joseph Burns, personal communication, July 22, 2014).[163]

Friedman and Schwartz made their low estimation of Gurley and Shaw (1960) apparent through their brusque reference to it in *Monetary Trends* in 1982.[164] The following year, Robert Hall, who had read *Monetary Trends* and also was likely familiar with the fact that Friedman looked askance at the broader finance literature, saw an opportunity to showcase Friedman's critical perspective. Hall invited Friedman to be a discussant of Robert Lucas' paper "Money in a Theory of Finance." The occasion was a mini-conference held by the NBER in the vicinity of Stanford University on December 2, 1983.[165] Charles Nelson, who attended the event, recalled Friedman's discussion of the paper that would be published as Lucas (1984). "He began by saying that he felt that he should be proud that a student of his would write a paper that he did not understand. Then, as one might expect, he launched into a series of unvarnished critical

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[162] As noted in the previous volumes, Friedman's economics courses had numerous Ph.D. students from the business school in their enrolment. One of these, David Ranson, asked if Friedman would serve on his committee, but Friedman declined, and he was rarely on Ph.D. committees for the business-school students (David Ranson, interview, April 30, 2014). However, in a public appearance in early 1971, Friedman evidently identified Ranson as having been a student of his, on account of the coursework Ranson had taken with Friedman (*The Plain Dealer* (Cleveland, Ohio), March 9, 1971).
[163] See also Johnson (1976a, pp. 296–297). The thesis was finalized as J.M. Burns (1967).
[164] See Friedman and Schwartz (1982, p. 207).
[165] The event was held at the NBER's Palo Alto office and was a program meeting of the economic-fluctuations group (see National Bureau of Economic Research, 1983).

remarks that made it clear that he thought the paper missed the mark..." (Charles Nelson, personal communication, April 1, 2014.)

One of the conclusions of Lucas' paper was, however, a sentiment that Friedman likely found most agreeable: "financial and monetary theory have quite different objectives, and… one can identify strong forces that will continue to pull apart these two bodies of theory."[166] For one thing, this conclusion clearly rejected the notion, occasionally faced by Friedman at his workshop in the 1970s, that finance techniques should be used in place of more traditional monetary economics in the analysis of aggregate economic behavior. For another thing, the notion embedded in Lucas' statement that there could be a useful division of labor between monetary economists and finance economists was something that Friedman himself showed signs of having reached by the late-1983 period when he was Lucas' discussant. Over the years, he seemed to have reached an accommodation with financial theory. Friedman would imply that, though financial economics should not supersede monetary economics in the analysis of monetary policy's effects on output and the price level, it could be useful for the detailed study of asset price determination.

The language that Friedman used over the years would shed light on this gradual process of accommodation. In early 1968, he had used the term "financial economist" as though it referred to a macroeconomist employed in the banking or financial-market community, rather than a specialist in the discipline of finance.[167] Later in the same year, he likewise used "financial matters" as though this term was synonymous with money-and-banking topics in economics, rather than having its own distinct meaning (Instructional Dynamics Economics Cassette Tape 4, November 1968). In contrast, in 1975 Friedman referred to "financial and economic matters"—a formulation that gave more scope for financial matters as a distinct area of study from other parts of economics (Instructional Dynamics Economics Cassette Tape 176, September 1975, Part 2).

Then, in 1984, Friedman was asked on a television program to draw the implications for interest rates of his projections for the U.S. economy. Facing a panel of Wall Street specialists as well as the host, Louis Ruykeser, Friedman was specifically asked by Rukeyser: "Where do you see long-term Treasury bonds going?" Friedman replied that his economic forecast implied that the bond rate would rise, but he added: "Now, [by] how much? I don't know. You people are much more expert in the numerical aspects of that than I am."[168] This answer revealed a deference to

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[166] Lucas (1984, p. 39).
[167] Friedman (1968e, p. 16).
[168] *Wall Street Week*, Maryland Public Television, April 27, 1984, p. 8 of transcript.

finance specialists (including those in financial markets who drew on advances in the research field of finance) with regard to the details of asset price determination, while keeping the analysis of inflation and of cyclical output behavior within the domain of monetary economics.

In the economics profession as a whole, the remaining resistance to viewing finance as a key area of study also faded. William Sharpe recalled that there was a "little 'do' [that is, a formal celebration] at the American Finance Association meeting in the December following our prize announcement in October [1990], which celebrated the anointment of finance as being Nobel-worthy. And, so, I think that was a general opinion in the academic finance community: 'Oh my God, we've been let in.'" (William Sharpe, interview, February 6, 2016.) As for Friedman, however, Harry Markowitz doubted that he ever really fully changed his mind on the matter: "I have no reason to believe that he ever appreciated that portfolio theory is, essentially, microeconomics under uncertainty." (Harry Markowitz, interview, February 23, 2016.)

CULMINATION OF THE CONSCRIPTION DEBATE

In the September/October 1972 issue of the *Journal of Political Economy*, Friedman made a digression from the monetary issues that were the concern of his discussion to mention that he and Keynesian economist Abba Lerner "agreed on a large number of issues" outside domestic macroeconomics, including the virtues of "a volunteer army."[169] This remark was a rare intervention by Friedman in a research journal on a matter for which he had become well known in the public square: his opposition to conscription. The remark also saw print at a poignant time: almost exactly a decade after the publication of *Capitalism and Freedom*, in which he had articulated his anti-draft sentiment; and just a few months before a U.S. policy change—one occurring partly through Friedman's efforts—that gave official imprimatur to his sentiment. The move to an all-volunteer U.S. armed forces took place in key steps over the first half of 1973.[170]

A single paragraph in *Capitalism and Freedom*, appearing in late 1962, had considered the issue of conscription, and it had succinctly encapsulated Friedman's opposition. That passage argued that compulsory military service was disruptive to the lives of those affected and was arbitrary because of its lack of universality. Friedman further indicated that, in the U.S. armed forces,

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[169] Friedman (1972a, p. 936).
[170] This subsection is an expanded version of a discussion originally written for Chapters 13 and 14 of the previous volume but omitted from the published version (Nelson, 2020b). It was included in the drafts of that volume that were available online in the years from 2015 to 2019.

wages should be allowed to reach levels consistent with attaining the desired size of the military purely by voluntary enlistments.[171]

By 1966, the advent of a large-scale U.S. commitment of armed forces to the Vietnam War alongside a military draft had made the subject of conscription much more topical than it had been in 1962. The discussion in *Capitalism and Freedom* had been specifically concerned with peacetime conditions.[172] Furthermore, actual conditions in 1962, though featuring high geopolitical tensions in Europe, Asia, and elsewhere, and characterized by large-scale U.S. troop commitments in various parts of the world, were formally those of peace for the United States. Therefore, the opposition to conscription voiced in *Capitalism and Freedom* was not necessarily applicable to the Vietnam War setting of a few years later.

Friedman made clear, however, that he did not believe that the scaling-up of U.S. involvement in the Vietnam War had made the case for a volunteer army obsolete. In a wave of writings and activities on the issue in the 1966−1967 period, Friedman affirmed that his opposition to the draft carried through to the newly prevailing conditions.[173] Conscription was an affront to Friedman because it was a device that preempted individuals' freedom, in favor of public-sector direction of resources, and because it eschewed market mechanisms as a means of drawing labor supply toward membership of the armed forces.

Friedman himself affirmed that the world situation implied the need for "a large [U.S.] military force and a strong one."[174] But he took issue with claims that a system based on purely voluntary recruitment could not secure such a force. The price mechanism had not been deployed sufficiently for this purpose, Friedman argued in *Newsweek* (December 19, 1966). Specifically, the wages offered to troops were too low. "We could readily attract more volunteers simply by paying market wages."

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[171] See Friedman (1962a, p. 36).

[172] In addition, Brittan (2005, p. 291) implied that, when he knew Friedman in the United Kingdom in 1953−1954, Friedman was (already) opposed to peacetime conscription. (Brittan indicated that Friedman's position at this time was articulated with regard to the U.S. draft. But the subject may have come up over this period in discussion of the U.K. situation, too. After the Korean War ended in 1953, the U.K. government had continued widespread conscription, called National Service.) See also Gibbs and Perri (2021, p. 9) for recollections to the effect that Friedman had doubts about the U.S. draft when mobilization for World War II was in process in the early 1940s.

[173] With regard to these contributions: In addition to the December 1966 *Newsweek* column and other interventions discussed below, including his 1967 Congressional testimony, Friedman developed his conference paper on conscription (see Friedman, 1967a) into Friedman (1967b).

[174] From his floor discussion in the December 1966 conscription conference in Tax (1967, p. 366). Along similar lines, in his testimony of April 6, 1967 (in Committee on Labor and Public Welfare, 1967, pp. 244−245), Friedman specifically endorsed the taking by the United States of strong military action in Vietnam.

As his remarks in *Capitalism and Freedom* implied, another aspect of conscription that irked Friedman was the arbitrariness involved in its implementation. In contrast to World War II, the Vietnam War did not involve a truly large-scale mobilization of the U.S. population, and it implied little government-directed involvement of the American workforce beyond those assigned to the armed forces. Under these circumstances, conscription in practice pertained to a small subset of the young male population. In addition, enrolment in universities was an option that offered a chance, under certain conditions, for some draft-eligible persons to delay or bypass requirements to serve in the U.S. military. Consequently, the existence of conscription potentially distorted the incentives to seek or take higher education.

The alternative of making all young males equally draft-eligible, and then selecting the force by lottery would not solve the problem, Friedman argued. Instead, it would only underscore the arbitrary nature of the process by which conscription was typically implemented by the U.S. government (*Newsweek*, December 19, 1966).

For a fully-mobilized war, Friedman as of 1966–1967 saw the case for conscription as more compelling, as the draft would then apply far more widely across the (male) population.[175] By the 1970s, however, Friedman was becoming critical of the usage of conscription, even for conditions of fully-mobilized war.[176] And at the end of 1979, he declared categorically: "I am opposed to a draft under any and all circumstances, even in wartime." (*The Register*, December 23, 1979, p. E11.)

Asked during this same period how far back his opposition to conscription dated, Friedman answered: "I have been opposed to the draft for at least thirty years, well predating the Vietnam War."[177] Even at this late point, however—and as in *Capitalism and Freedom*—Friedman viewed compulsory universal military service as less arbitrary, and the case for it as being less inconsistent with his conception of *bona fide* liberal principles, than a selective draft. Indeed, he defended this practice for low-population countries like Israel, Switzerland, and the Netherlands.[178]

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[175] See Friedman (1967c, p. 200; 1967d, p. 826).
[176] See *Montana Review*, April 16, 1971, *Reason* magazine (August 1977, p. 29), and Friedman's 1979 remarks in Anderson (1982, pp. 85, 203).
[177] In Anderson (1982, p. 189).
[178] See Friedman (1982e, p. 181) and Friedman's remarks in Anderson (1982, pp. 198−199). These three countries all had national-service arrangements, though their military profiles differed significantly from one another in other ways. (For example, the Netherlands was a member of NATO, while Switzerland was not.)

Friedman's first *Newsweek* column on the draft appeared shortly after a University of Chicago conference on that subject—one held on December 4–7, 1966. The conference, which was attended by military specialists, lawyers, and politicians, together with economists, gave Friedman the opportunity to emphasize the point to the noneconomist attendees that the draft was a tax-in-kind, a tax paid other than by explicit payment: it was true that conscript soldiers received wages, but what they received could not compensate them completely for the forgone opportunity of greater choice with regard to how to allocate their time. Friedman stressed, in the same spirit, that the savings in U.S. defense expenditures—savings arising from the fact that mandatory recruitment meant labor being supplied at below-free-market wages—should, in an appropriate accounting, be weighed against the not-explicitly-recorded, but material, economic costs of the draft.[179]

Friedman memorably, and not for the last time, voiced his irritation at the fact that a voluntary army was being pejoratively labeled a "mercenary" arrangement. "My army is 'volunteer,' your army is 'professional,' and the enemy's army is mercenary. All these words mean exactly the same thing."[180]

Reflecting on the conference nearly twenty years after its occurrence, Friedman judged that the economist whose anti-conscription message made the greatest impact on the assembled participants was not himself, but instead Walter Oi.[181] Friedman saw Oi's critique of the draft as effective in its own right, but he also viewed its success as reinforced by Oi's lack of a heavy and obvious personal stake in the matter. Oi, being blind, would not be draft-eligible.[182]

In advocating a volunteer army, Friedman received support from economists who were unsympathetic to his views on many other issues. For example, the late Ronald McKinnon, who liked Friedman's work on consumption but vehemently opposed him on international economic arrangements, observed in retrospect that "his argument for getting rid of the draft was something… I admire him for." (Ronald McKinnon, interview, January 23, 2014.)
For his part, Friedman liked to emphasize the different reactions of key politicians to the volunteer-army idea. In the 1970s, Friedman would contrast the opposition to conscription of two Republican politicians with whom he had been associated, Barry Goldwater and Ronald

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[179] See Friedman (1967c, p. 200).
[180] From Friedman's floor discussion in Tax (1967, p. 366).
[181] At the time, Oi was at the University of Washington, Seattle (Tax, 1967, p. 491; American Economic Association, 1970, p. 326).
[182] Friedman (1986b, p. 8). See also Friedman and Friedman (1998, p. 377).

Reagan, with what he claimed was Senator Edward (Teddy) Kennedy's support for the draft.[183]

Teddy Kennedy, Democratic Senator for Massachusetts, had actually started as an ally on the matter. He attended the University of Chicago conference on conscription (Tax, 1967, p. 490). Kennedy would also appear alongside Friedman in a February 1967 radio discussion of the draft.[184] The senator's interest in anti-conscription arguments subsequently prompted him to invite Friedman to testify in a Congressional hearing on the matter.

It was therefore before Senator Kennedy's Subcommittee on Employment, Manpower, and Poverty (within the U.S. Senate's Committee on Labor and Public Welfare) that Friedman gave his only Congressional testimony specifically on the subject of the draft. Friedman appeared before the subcommittee on April 6, 1967. A volunteer force, Friedman testified, would make the cost of maintaining the armed forces "open and above-board and not concealed," in contrast to existing arrangements, under which part of the cost was disguised by the draft—"a tax in kind[,] imposed on the young men who serve."[185]

Friedman's activism on the subject continued after the hearing—most notably so in a piece written for the *New York Times Magazine* (May 14, 1967).[186] Teddy Kennedy, however, went down a different path. As Friedman's 1979 characterization implied, the senator would undergo a reversal of his stand regarding conscription—becoming, by the end of the 1960s, an advocate of that policy (*The Shreveport Journal* (Louisiana), March 8, 1969). "Teddy Kennedy is a mystery," an exasperated Friedman remarked (*Chicago Daily News*, December 29, 1969, p. 4). However, the point that Kennedy now stressed was one that would continue to have a notable following in the United States long after conscription was abolished. As discussed further in Section III below, Senator Teddy Kennedy's case for conscription emphasized its possible role in ensuring that the membership of the armed forces come from across the U.S. income scale (*Chicago Daily News*, December 29, 1969). Of course, one partial response to this point was that it was in the nature of Friedman's proposal that military service would be a higher-paying occupation than it was under existing arrangements. This change would increase the scope for the armed forces to compete with other potential employers.

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[183] See Friedman (1977i, p. 12; reprinted in Friedman, 1978b), and Friedman's 1979 remarks in Anderson (1982, p. 85). In Instructional Dynamics Economics Cassette Tape 45 (February 26, 1970), Friedman recalled successfully encouraging Barry Goldwater to advocate a volunteer army during the 1964 presidential election campaign.
[184] The appearance was on New York City radio station WRVR, at 10 p.m. local time (*Newsday* (Long Island, New York), February 3, 1967).
[185] From Friedman's testimony in Committee on Labor and Public Welfare (1967, p. 245).
[186] See also Friedman's article in *New Guard*, May 1967.

The abolition of conscription

As noted in Nelson (2020b, Chapter 13), Friedman anticipated that Richard Nixon, elected president in November 1968 and inaugurated the following January, would abolish conscription. In the event, this abolition was stretched out over the entirety of Nixon's first term, and indeed very slightly beyond that first term. But Friedman's expectation of the voluntary-army outcome was eventually redeemed, so the Nixon Administration's actions with regard to conscription became one of the few areas (another being international economic policy) on which Friedman did not become disillusioned with President Nixon.

Early in the tenure of the administration, Friedman accepted an advisory role that he was more formal than he had been willing to countenance on mainstream economic-policy matters. He joined President Nixon's Commission on the All-Volunteer Armed Force. Headed by Thomas Gates, this fifteen-person group's members included, alongside Friedman, Allen Wallis and Alan Greenspan (*Boston Globe*, March 28, 1969; Gates Commission, 1970). The Gates Commission reported publicly on February 21, 1970 (*New York Times*, February 22, 1970). The title of Chapter 3 of its report, "Conscription Is a Tax," was plain evidence of Friedman's presence on the commission and his influence on other commission members.[187] Indeed, it was reported at the time that Friedman and Walter Oi had largely composed the report (*Evening Star* (Washington, D.C., February 20, 1970, p. A−1).

Nixon had already indicated that his administration's goal was to establish an all-volunteer army (Ch*icago Daily News*, December 29, 1969, p. 3). The Gates Commission's report gave the idea further impetus in policy circles: "At long last, the end of the draft is in sight," Friedman observed in his *Newsweek* column (March 16, 1970). The military leadership, however, remained a source of major opposition. For example, Alexander Haig—who held senior posts, first in national security and then in the formal military command, throughout Nixon's first term—observed in 1979: "I have never been really comfortable with the 'all-volunteer' concept. I am consistent about that, because I opposed it in 1970, while in the White House, when [the [planning for] the elimination of conscription was undertaken."[188]

Against this background, Friedman's public advocacy continued. He appeared on an ABC

[187] Nonetheless, the fact that this commission had numerous advocates of a volunteer army, and not just Friedman alone, means that one must take with caution Lawrence Summers' analogy (given in *New York Times*, November 19, 2006) between Friedman's role on the commission and that of the persuasive juror (winning over fellow jurors, all of whom had been initially skeptical) in the drama *Twelve Angry Men*.
[188] From Haig's testimony of August 2, 1979, in Committee on Foreign Relations, U.S. Senate (1979a, p. 302).

national news special, *The Draft: Who Serves*?, in which he stressed the inequities and underappreciated economic costs of mandatory military service (*The Plain Dealer* (Cleveland, Ohio), June 5, 1970; *Boston Herald American*, June 6, 1970). He also conducted private activities in pursuit of the same end. In the late 1960s and early 1970s, Friedman interacted with members of Congress, in what he described as his "extensive personal lobbying" in support of the repeal of the draft.[189]

Friedman could take satisfaction in the abolition of conscription in 1973, in keeping with the Gates Commission's recommendation.[190] In his memoirs, he would date the end of conscription to January 27, 1973—that is, very early in President Nixon's second term.[191] This date (which lined up closely with what was perceived, in much U.S. discussion at the time, as basically the end of the Vietnam War) corresponded to the point at which Secretary of Defense Melvin Laird indicated that the military forces were no longer using the draft (Rostker and Yeh, 2006, p. 182).

A year further on, Friedman observed, "I regard the end of the draft as one of President Nixon's and then-Secretary of Defense Melvin Laird's finest hours. No other measure has done so much to end the divisions that were threatening to tear this nation apart." Indeed, the timing of this remark (in *Newsweek*, February 11, 1974) indicated that Friedman's belief was that the end of conscription had been such a boon that the social cohesion of the United States had actually *improved* since early 1973, despite the numerous traumas in the interim: a severe inflation breakout, the oil embargo, and Watergate.

Some vestiges of U.S. conscription arrangements remained beyond January 1973. The legal authority to use the draft remained in existence until July 1973 (Rostker and Yeh, 2006, p. 183). In light of that fact, Anna Schwartz (1993, p. 209) named the summer of 1973, rather than earlier, as the time when the U.S. conscription ended. [192] Furthermore, *registration* for the draft

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[189] Friedman (1975a, p. 188).
[190] Friedman's participation in the Gates Commission was discussed in Friedman and Friedman (1998, pp. 379–381). In that discussion, Friedman mentioned (p. 379) the final report of the Commission as having been issued on February 20, 1970 (which was the date of Thomas S. Gates' cover letter for the report). The release date was actually the next day—February 21. Soon after the report's publication, Friedman discussed its recommendations on his cassette series (Instructional Dynamics Economics Cassette Tape 45, February 26, 1970). As indicated above, the report's text has distinct Friedman writing traits. Reflecting its status as a joint product, however, some passages contain words or phrases that he seldom used (see, for example, the use of "rationale" on its page 120).
[191] Friedman and Friedman (1998, p. 379).
[192] Along similar lines, Friedman (1975a, p. 187) gave the completion of the transition to an all-volunteer force as occurring on June 30, 1973.

was not discontinued in the United States until 1975.[193]

Friedman believed that the draft remained a live issue in late 1974, when he was preparing his new compendium of *Newsweek* columns. For although he indicated to readers that columns "no longer relevant" were not being reprinted, the 1975 collection maintained a chapter, titled "A Volunteer Army," consisting of his columns on the draft.[194] "It would be nice to record that this chapter has only historical interest," he remarked in his introduction to the chapter. "… Unfortunately, that would be premature."[195]

And as will be seen in later chapters, Friedman in the late 1970s and early 1980s fought a rearguard action—including against figures in public policy and the media with whom he was in strong agreement on numerous other issues—in which he provided opposition to pressure to reinstate the draft.[196]

III. PERSONALITIES IN DEBATES ON BUSINESS, FINANCE, AND SOCIAL POLICY, 1973–1974

GARY BECKER

Neil Wallace, who took Friedman's Price Theory class in his first year (1960/1961) as a graduate student, observed: "I can remember some kind of department gathering, a cocktail party of sorts, or something, I'm pretty sure it was during my first year, at which Friedman said, 'I don't think people should teach in the area in which they do research.'" (Neil Wallace, interview, March 15, 2013.) This maxim had underpinned Friedman's continued practice of teaching Price Theory at the University of Chicago—a routine to which he adhered through the 1963/1964 year, even though he had specialized in monetary economics since the early 1950s. Subsequently, however, as already indicated, Friedman deviated from his own prescription about course instruction, as he became one of the teachers of graduate monetary economics at the university from the mid-1960s until the early 1970s.

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[193] See Secretary of Defense Harold Brown's testimony of January 31, 1980, in Committee on Armed Services, (1980, p. 34).
[194] The quotation is from Friedman (1975a, p. ix). The chapter in question spanned pages 187 to 196 of the *There's No Such Thing As a Free Lunch* collection.
[195] Friedman (1975a, p. 187).
[196] See also the discussion in Section III below of the views on this matter expressed in the late 1970s and the early 1980s by the main editorials of the *Wall Street Journal* and their similarity to those that Walter Heller had propounded several years earlier.

In the 1973/1974 year, Friedman resumed his teaching of the Price Theory course. In this initial year of his return, with his heart surgery being a recent event, he taught for only one quarter. He taught Price Theory again the following year. Anticipating that 1975/1976 would be his final year at the university and would see the culmination of his teaching of the Price Theory course, Friedman in 1974/1975 decided to undertake a substantial revision of his *Price Theory* text.[197]

The book had remained in print continuously—albeit always on softback paper stock that rapidly became dilapidated—since its appearance in 1962. There had been textual corrections embedded in the 1966 reprinting, after which further printings of this version of the book occurred every few years. A ninth printing appeared in 1973, and—even though the publishers were surely aware that Friedman's revised edition was in the works—a tenth printing in 1975. The revised edition that would appear in 1976 marked the first time the book had appeared in hardback. This revamp would see the dropping of the *A Provisional Text* subtitle that had been used on all the printings of the first edition.

The fact that the book retained its main title, *Price Theory*, affected Friedman's redrafting of the book's introduction, as he took the opportunity, in a short, newly inserted paragraph, to criticize the prevalence of "microeconomics" as the modern terminology for price theory.[198] This protest was, of course, in vain. By the 1970s, "microeconomics" was the prevalent nomenclature in the profession, and usage of "price theory" was now idiosyncratic—with the University of Chicago one of the few major economic centers still embracing it.

The obsolescence of the "price theory" terminology was brought out by the classification problems that both Friedman's and George Stigler's books encountered. Friedman and Stigler had many differences in their microeconomic viewpoints and—reflecting this reality—each of them produced a solo-authored text in the field: *Price Theory* in Friedman's case, *The Theory of Prices* in Stigler's. Claire Friedland, Stigler's longtime associate, recalled that, even at the University of Chicago, the term "price theory" in the titles was the source of "a lot of trouble in the library—because they miscataloged George's *Theory of Price*, and they miscataloged the book that Milton wrote on price theory." Friedland specifically recalled that the library shelved Stigler's book alongside empirical investigations of inflation and inflation expectations rather than microeconomic texts (Claire Friedland, interview, October 27, 2014).

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[197] See Friedman (1976b, p. vii).
[198] Friedman (1976b, p. 7).

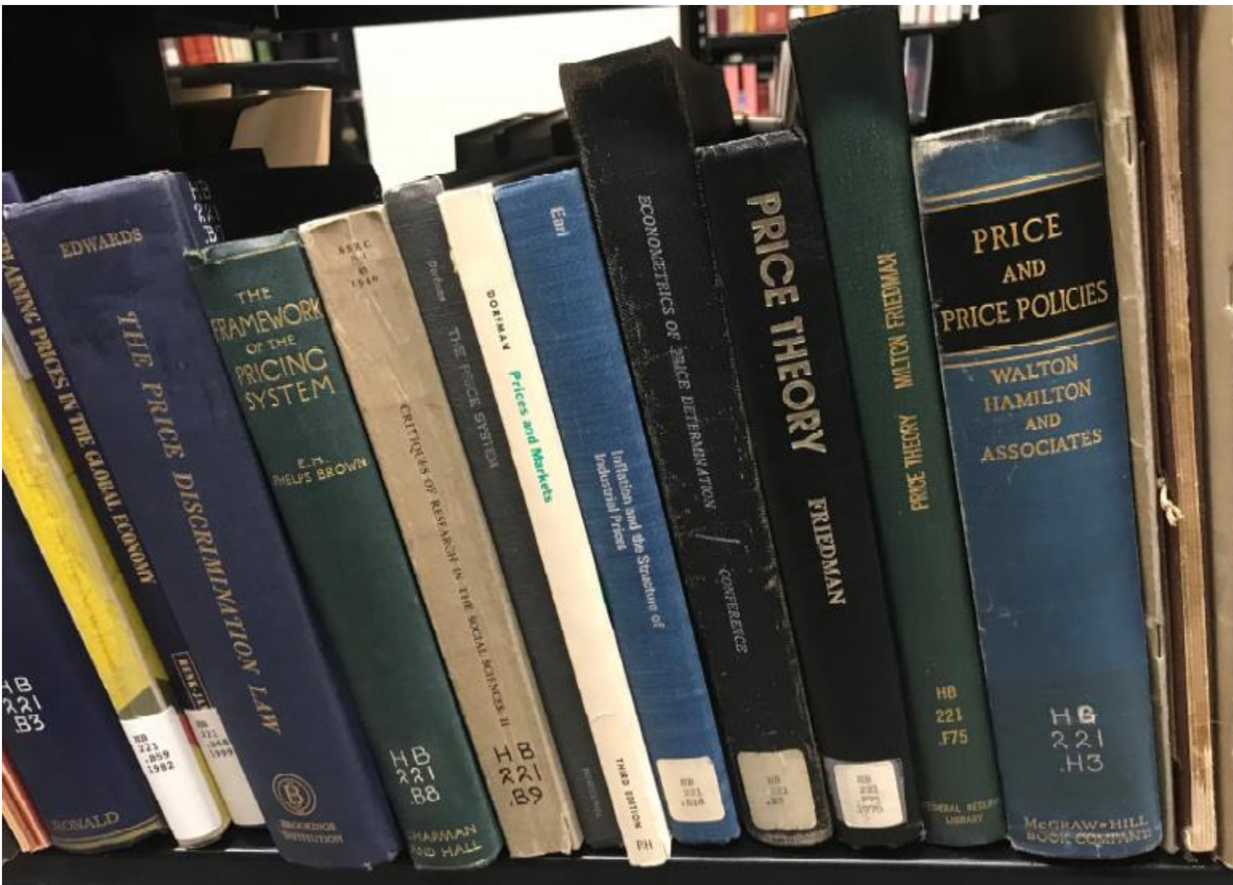

Figure 2. Friedman's *Price Theory* on Federal Reserve Board library shelf, 2019.

That this problem was not special to the University of Chicago library is brought out by Figure 2, which shows the shelving of *Price Theory* (both editions) in the Federal Reserve Board's main library.[199] The library's copy of the 1976 edition of *Price Theory* is adjacent to Otto Eckstein's (1972) collection of macroeconometric studies of price-setting.

The efforts on Friedman's part to convince teachers and researchers that the adoption of the term "microeconomics" was misplaced, and that "price theory" should be restored, amounted, therefore, to a lost cause. All the same, a still-later attempt to keep alive the "price theory" terminology, in reference to (and in preference to) microeconomics, would be made in the 1980s and 1990s by Friedman's son, David. David Friedman had already made an impact in 1973 with a popular book called *The Machinery of Freedom* that advocated a strong variant of libertarianism. Indeed, successive editions of William Baumol and Alan Blinder's undergraduate economics principles textbook, starting with the first edition in 1979, opened its

[199] In the figure, the 1962 edition of *Price Theory*, a paperback, appears to be a hardback, because of the library's hardcover rebinding of the volume.

exposition of the basics of libertarian microeconomics with examples and quotations from David Friedman's (1973) book on libertarianism, and only then turning to an extended discussion of Milton Friedman's position (as expounded in *Capitalism and Freedom*).[200] In the area of positive economics, too, David Friedman's specialization was in microeconomics. He produced his own graduate text in 1986, also called *Price Theory*, with a second edition (titled *Price Theory: An Intermediate Text*) appearing in 1990 (see D.D. Friedman, 1986, 1990).

The Becker revolution in microeconomics

Regardless, however, of whether it was called microeconomics or price theory, the fact was that the field had changed immensely in its research dimension since the 1962 edition of Milton Friedman's *Price Theory* had appeared. Friedman's introduction to the new, 1976, edition of this book made a key acknowledgment of one key advance in microeconomic research: "In recent decades, there has been increasing use of economic analysis to interpret behavior in the household that was traditionally excluded from the realm of economics. Gary Becker's pioneering work in this direction deserves special mention."[201]

But Friedman did not pretend to have expanded his text's treatment of price theory to encompass Gary Becker's work. Instead, as in 1962, Friedman's textbook remained one that kept within the conventional borders of microeconomics (other than by covering gambling, for which a treatment continued to be provided, little changed from the 1962 edition). Indeed, Friedman was neither a teacher of, nor participant in, the research program associated with Becker in the 1960s and 1970s—that of applying and expanding microeconomic tools to cover numerous social-policy topics.

The separation of Friedman from this endeavor was underlined by conference and publication developments in 1973 and 1974. The National Bureau of Economic Research held a conference on June 4–5, 1973, on the topic of "Marriage, Family Human Capital, and Fertility." The proceedings were published as a March/April 1974 supplementary issue of the *Journal of Political Economy* and included Becker's (1974a) paper, "A Theory of Marriage: Part II." Elsewhere in the proceedings, in the course of his discussion of what he called the "new home

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[200] See Baumol and Blinder (1979, p. 814). One of the footnotes in this discussion included the sentence: "David Friedman is the son of Milton Friedman, the most famous of all libertarian economists." This sentence was omitted from later editions, even though the surrounding discussion of David Friedman's and Milton Friedman's writings was retained (see, for example, Baumol and Blinder, 1982, p. 807). The sentence may have been dropped because its presence in the textbook's discussion meant that Milton Friedman's status as a libertarian economist was, in effect, stated twice in quick succession (in a footnote, then in the text).
[201] Friedman (1976b, p. 4).

economics," Marc Nerlove (1974, p. S202) observed that "much of the theoretical underpinning rests on what I have called elsewhere the 'Chicago model.'"

On other occasions during this era, both the NBER and the "Chicago model" were terms closely associated with Friedman and his work. But, on this occasion, no such connection with Friedman existed: he did not attend the conference, and he had no close connection at all with the "Chicago model" in question. Reflecting these facts, the supplementary issue in which the conference proceedings appeared—one of the seven issues of the *Journal of Political Economy* for 1974—came close to being the only one of the year's issues that contained no mention whatsoever of Friedman. In the event, his name did appear once in the issue. Because the conference underlying the special issue had been an NBER event, the front matter of the issue included, as had become *pro forma* for NBER publications, a listing the NBER's officials and senior affiliates. Friedman's name appeared on one page of that two-page listing. But this was the only mention of him in this *JPE* issue, from cover to cover: he was not cited by any author, not credited by any contributor with providing comments on their article, and not mentioned either in the issue's back matter or elsewhere in its front matter.[202]

This repeated a pattern that had been observed in 1973. Friedman was referred to by name at least once (and typically more than once) in every issue of the *Journal of Political Economy* that appeared during 1971, 1972, 1975, and 1976.[203] Indeed, in the thirty years from 1958 to 1987, 189 issues of the *Journal of Political Economy* had appeared, and Friedman, despite not being one of the *JPE*'s editors, had been referred to by name in 184 of those issues. But in 1973, there had appeared a single issue of the journal that contained no mention whatever of him. The single issue (for March/April 1973) was devoted to "New Economic Approaches to Fertility"—a supplementary issue, coedited by Becker, that showcased the proceedings of a June 1972 NBER conference (which, like its 1973 sequel, Friedman had not attended).[204] In contrast to its 1974

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[202] In every issue of the *Journal of Political Economy*, members of the department of economics and business school of the University of Chicago were credited with helping the editors produce the journal. But they were not named individually in this credit.

[203] In the full decade from 1970 to 1979, a total of seventy-one articles or books of which Friedman was solo author or coauthor (not counting cases of book chapters he did not write but for which he appeared in the citation as an editor) were specifically referred to in articles published in the *Journal of Political Economy*. Of these seventy-one items, six dated to the 1940s, twenty to the 1950s, twenty-six to the 1960s, and nineteen to the 1970s. Numerous other articles in the journal mentioned Friedman but did not refer to a specific piece of work by him.

[204] The two sets of conference papers were also published, together with Becker (1973), as a single book, *The Economics of the Family* (Schultz, 1974). Theodore Schultz was credited as the single editor of this volume, and he had been the sole credited editor for the proceedings of the second of the events on which the volume was based (that in June 1973). However, a participant in the 1973 symposium, Dudley Wallace, recalled of it that "certainly, Becker was 'front row and center' at the conference" (T. Dudley Wallace, interview, July 20, 2015).

counterpart, this 1973 special issue did *not* include a listing of the NBER's senior membership. Consequently, Friedman's name did not appear at all in this issue of the *JPE*.

As his non-attendance of the conferences—along with the absence of mentions of him in the published conference issues—attested, Friedman was, in the 1970s, wholly outside this research field. Becker had been a colleague of his since 1969, but—as was so often in the case with regard to Friedman and his fellow department members—neither their shared status as members of the economics department nor the proximity of their offices implied that they were carrying out joint research work.

Interaction between Becker and Friedman during the 1950s

Friedman could, however, lay claim to having helped Becker get a start in the economic-research world in the 1950s. Or, rather, a new start, as Becker had studied economics as an undergraduate at Princeton University, graduating in 1951 (American Economic Association, 1974, p. 25), and he actually published two research papers in journals in 1952.[205] Despite this prestigious background, which had included working with the high-performing Princeton University economics professor William Baumol, Becker would credit his 1951−1955 experience as a student of the University of Chicago, and particularly his interaction with Friedman, with producing a reincarnation. "I came to Chicago thinking I knew quite a bit about economics. I discovered I didn't know so much after all. This man shook me up," Becker remarked (*Fortune*, June 1, 1967, p. 148).[206] In his Princeton University years, Richard Posner noted, "Gary did very well, of course. But he felt that, when he came to do graduate work at UoC, Friedman exposed the fact that Gary didn't really know economics—hadn't really learned it." (Richard Posner, interview, October 27, 2014.)

As discussed in Nelson (2020a, Chapters 5 and 8), while a graduate student, and continuing into his immediate post-Ph.D. years when he was a junior member of the economics department, Becker attended Friedman's money workshop, helped him on some of the data work for *A Theory of the Consumption Function*, and produced some joint research that critiqued standard estimates of the fiscal multiplier in Keynesian models. The last of these pieces of work began to

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[205] See Becker (1952) and Becker and Baumol (1952). Both of these publications appeared toward the end of 1952, ahead of Becker's twenty-second birthday late that year. Becker therefore started publishing economic research earlier in his career than did Friedman, whose first published work (that is, Friedman, 1935) appeared when he was age 23. Nevertheless, on one occasion a bizarre typographical error had Becker appearing to suggest that Friedman had published one of his articles in 1931—a year in which Friedman was in his late teens. See Becker (1962, p. 1).
[206] See also Becker (1991, pp. 141−142) and Heckman (2011, pp. 40–41).

appear during Becker's first spell as a University of Chicago instructor and the rest of it saw print early in his 1957−1969 period at Columbia University.[207] This work confirmed that one result of Becker's University of Chicago training had been to disabuse him of adherence to Keynesianism, for in their 1957 *Journal of Political Economy* Becker and Friedman remarked that they both were among those who had accepted Keynesian theory in the past.[208]

Nevertheless, even the macroeconomic writings that Becker wrote with Friedman were not part of Friedman's monetarist body of work. Nor was Becker's doctoral dissertation concerned with either macroeconomic issues or money specifically. Instead, he produced a microeconomic study—the thesis version of what became his book *The Economics of Discrimination* (Becker, 1957). Becker had produced a preliminary plan for the thesis in 1953 and had received extensive comments on the plan from Friedman, but by postal mail. This early part of Friedman's task of thesis supervision was done by long distance, because it took place during his 1953/1954 year as a visitor at Cambridge University (see Becker, 1991, p. 144).

Becker branches out

As already indicated, Becker's affiliation over most of the 1960s was with Columbia University, and it was while located there that he won the John Bates Clark medal in 1967.[209] Becker's return to the University of Chicago's economics department started with a visiting professorship in 1969/1970 that bridged him into a permanent position.[210] He would remain at the University

[207] For the 1957−1969 dating, see Blaug and Sturges (1983, p. 29) (see also Gibbs and Perri, 2021, p. 10). (An incorrect starting date of 1958 appeared in American Economic Association, 1974, p. 25. In addition, in an article about local authors, a Chicago-area newspaper in late 1957 listed Becker as a resident of Kenwood Avenue in Chicago—see *Hyde Park Herald* (Illinois), December 18, 1957—but Becker had apparently actually moved to New York City by that point.) Reder (1982, p. 5), in contrast, gave Becker's years at Columbia University as spanning from 1958 to 1970. Becker had, however, already served as a visiting professor at the University of Chicago in 1969/1970 (see Blaug and Sturges, 1983, p. 29)—although he did not make this clear in American Economic Association (1970, p. 27), refraining from giving either university as his employer in his biographical entry.
[208] See the reference to Friedman and Becker (1957, p. 74).
[209] At the time of the present writing, the unreliable Wikipedia entry on the Clark medal incorrectly gives Becker as already located at the University of Chicago at the time of his 1967 receipt of the award (see *https://en.wikipedia.org/wiki/John_Bates_Clark_Medal*). It also gives the next winner—Marc Nerlove, in 1969—as located at Yale University when he won the medal. This is correct. However, the announcement of that award came when Nerlove was already committed to make a move to the University of Chicago, and Nerlove had taken up this new affiliation by the time of the formal event commemorating his medal, held at the American Economic Association meetings of December 1969 (see American Economic Association, 1974, p. 294). Nerlove recalled that the news that he was receiving the medal came from James Tobin, then his Yale University colleague: "He told me that I had won the award and that it was a real shame that I was going to go to Chicago. Of course, [the economics department at the University of] Chicago was very happy about it [the timing of his award of the medal]." (Marc Nerlove, interview, September 18, 2013.)
[210] See American Economic Association (1974, p. 25) and the discussion in a previous footnote.

of Chicago for the rest of his life—decades beyond Friedman's own departure at the start of 1977.[211]

The economics of the family

In the early years of Becker's permanent University of Chicago affiliation—when both he and Friedman were senior members of the economics department—Becker's efforts to seek wider applications of economic analysis were concentrated on issues concerning the family. Reflecting this agenda, Becker's "Research" subentry in the American Economic Association's (1974, p. 25) directory read: "Econs. [Economics] of the Family, [With] Special Attention to Marriage, Fertility and Human Capital." It was this research agenda, and the interest it generated, that was reflected in the June 1973 and March/April 1974 special issues of the *Journal of Political Economy* referred to above. The taking-off of this field was also encapsulated in an address Becker gave in between the appearance of these two special issues: his remarks at a session of the December 1973 American Economic Association meetings, "On the Relevance of the New Economics of the Family" (Becker, 1974b).

Friedman himself made an acknowledgment of this thriving research area a year later when, speaking in San Francisco about the December 1974 American Economic Association (AEA) meetings that had been held in the city a couple of days earlier, Friedman observed that "the papers… [presented]… deal with fundamental economic research on varieties of topics… [including] the determinants of the divorce rate or the fertility rate" (Instructional Dynamics Economics Cassette Tape 161, January 1, 1975).

At this point and later, Friedman's position in relation to this research was not as a participant, but as a sympathetic observer—usually from afar, as the December 1974 AEA meetings were a rare occasion when he attended one of the relevant conference events—and as an occasional reader of the written product associated with the research. Friedman's general sympathy with this aim of approaching social issues with the tools of economic analysis was also consistent with a remark he made in April 1975: "Social phenomena have laws of their own."[212] That particular remark, as it happened, was mainly in reference to the usefulness of *macroeconomic* theory for the understanding of national developments. Friedman had, however, for decades

[211] Later Becker would have a Hoover Institution affiliation, but—in contrast to Friedman—this affiliation did not become a substitute for teaching at the University of Chicago. Becker would continue to be based in the city of Chicago, and to have an ongoing presence on the campus of the university, until his death in 2014.
[212] From Friedman's remarks in *Monday Conference*, Australian Broadcasting Commission, April 14, 1975, p. 25 of transcript (also printed in Friedman, 1975i, p. 34).

expressed interest in extending economic analysis to unconventional areas; and indeed he had endorsed in print, as far back as 1942, the notion of applying economic analysis, including utility functions, to the analysis of decisions to have children.[213]

In the acknowledgments section of *The Economics of Discrimination*, Becker had named Friedman as an influence on his own research agenda, crediting him with "continually emphasizing that economic analysis can be used for the solution of important social problems" (Becker, 1957, p. 12).[214] Becker also came to believe that, as his own research stressed "the importance of markets, in our system, of prices and so on," it provided further "reason why [at the University of Chicago] we think 'price theory' is a better name for it than microeconomics." (Gary Becker, interview, December 13, 2013).

Consistent with this posture, Becker retained the "Price Theory" name for the University of Chicago economics department's microeconomics course. During Friedman's final years at the university, Becker and Friedman both taught graduate price theory, with Becker teaching his course in a different part of the academic year from that in which Friedman was teaching the subject (Gary Becker, interview, December 13, 2013; Charles Plosser, personal communication, April 13, 2015). Becker continued teaching the Price Theory course, under that name, for decades after Friedman's departure (see, especially, Weyl, 2019). Notwithstanding the course's title, Becker had no illusions about the prevalence of the term "microeconomics"—"there's no doubt that microeconomics is the name that everybody gives to even the stuff that we do," he observed (Gary Becker, interview, December 13, 2013). Correspondingly, when writing up remarks he had made at a January 2007 American Economic Association panel, Becker titled the article "Milton Friedman As a Microeconomist" (Becker, 2007).

Some connection also existed between the early research contributions of Friedman and the economics-of-the-family work by Becker and others that went into such high gear in the early 1970s. One of the *JPE* special issues had referred to the research as taking "the human-capital approach" to labor economics and the allocation of time (Mincer and Polachek, 1974, p. S78). Friedman, of course, had been a major contributor to, and innovator in, research on human capital in the 1940s.[215]

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[213] See Nelson (2020a, Chapter 8).
[214] In addition, Becker (1962, p. 1) included the acknowledgment: "I am also indebted to M. Friedman for insightful oral and written statements… on economic rationality." In connection with the written statements, Becker cited Friedman's (1953b) essay on methodology.
[215] See Nelson (2020a, Chapter 2) and Goldin and Katz (2020).

Even with this connection granted, however, it is important that Friedman's link to the 1970s literature of the Becker type not be overemphasized. One reason for insisting on this point is that although Friedman did, after the 1940s, use the human-capital concept in his monetary-economics and consumption work, he largely let others make the running on research in the human-capital area. This situation was reflected in Bronfenbrenner's (1981, p. 613) observation: "As economists know, the human-capital theories of T.W. Schultz, Gary Becker, and others have risen to dominance over the past quarter century." Friedman had, in effect, acknowledged this situation when he cited Schultz's and Becker's work on human capital in *Capitalism and Freedom*.[216]

A second reason is that the post-1945 human-capital research about which Friedman was knowledgeable involved its application to traditional economic topics like economic growth, and not to what Becker (1974b, p. 317) called an "extension of the scope of economic theory" to "behavior outside the monetary market framework."[217] For example, in 1961, Friedman had remarked: "We have begun to see that stressing only physical capacities does not pay off. We have begun to question just how much of economic growth is based on increases in the quantity of physical capital. In the final analysis, the answer is technology…" (*Time*, March 3, 1961, p. 22.) This observation, with its basically macroeconomic orientation, was clearly much more closely linked to Schultz's human-capital research than to Becker's work.[218] Friedman's continuing closer familiarity with Schultz's work than Becker's was reflected in the fact that Schultz's Nobel lecture (concerned largely with cross-country income performance) included a whole sentence of thanks to Friedman for comments on a draft of the lecture.[219] It was a different story in the case of Becker's (1993) Nobel lecture. That lecture—concerned with Becker's approach to expanding the coverage of microeconomics—did not mention Friedman at all (neither in the main text nor in any footnotes, including the footnote of acknowledgments).

[216] Friedman (1962a, p. 102). Becker's monograph *Human Capital* (1964) had not yet materialized when Friedman wrote, but Friedman cited earlier research on the subject by Becker, specifically Becker (1960).
[217] Although his talk was focused on the economics of the family and on human capital, Becker (1974b, p. 317) also mentioned "political behavior" as one of the examples of the widening of the coverage of economic theory. The extension of economic analysis to the subject of politics—in the form of the theory of public choice—was something that Friedman frequently applauded, although his own applications of public-choice analysis were largely limited to his popular writings. See Nelson (2020a, Chapter 9) for a detailed discussion.
[218] Friedman may have been referring in this quotation also to the research that had been published in recent years by Robert Solow. However, at around this time, even Solow's colleague Paul Samuelson associated the finding of human capital's importance in growth specifically with "Theodore W. Schultz and [his] coworkers" (Samuelson, 1962a, p. 52; p. 1720 of 1966 reprint).
[219] Schultz (1980, p. 639) had written: "My debt to Milton Friedman is especially large for his painstaking expositional comments."

In contrast to Friedman, his son David became immersed in the application of economics to nontraditional issues. David Friedman (1986, p. 332) acknowledged his own predisposition toward "economic theory of things that everyone else regards as 'outside of economics'" (D.D. Friedman, 1986, p. 332). In line with this perspective, the aforementioned D.D. Friedman (1986, 1990) price-theory textbook included coverage of Becker's economics-of-the family work, as well as a foreword written by Becker.

Reaction beyond the University of Chicago

Gary Becker and Milton Friedman's bodies of work in the 1960s and 1970s were very different from one another in subject matter, but they did parallel each other in one notable respect. This parallel lay in the fact that both sets of writings gave rise to strong opposition from economists affiliated with MIT—including two senior figures already established as Friedman's Keynesian opponents.[220]

Specifically, the reaction by key MIT economists Paul Samuelson and Robert Solow was, in essence, that Gary Becker was trying to apply tools of economics to areas in which it would not work well. This reaction from within the economics profession was, of course, a common one found among noneconomists, too. In this vein, when commenting on Becker's "Theory of Marriage: Part II" paper in the aforementioned June 1974 *Journal of Political Economy* conference issue, sociologist William J. Goode (1974, pp. S27, S28) noted that "economists have been… attempting to analyze problems usually brooded over by sociologists" and referred to "this invasion" by economists of what had been the domain of noneconomists like himself.[221]

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[220] As well as the MIT reaction noted below, there were further notable cases in which economists had unfavorable reactions to Friedman's and Becker's writings alike. For example, Margaret Reid of the University of Chicago felt that Friedman had received too much credit for the permanent income hypothesis (and she too little), and she likewise believed that the acclaim for Becker's (1965) work on the allocation of time had been associated with an overstatement of the novelty of Becker's choice of research topic and an underappreciation of her earlier contributions in the area. Reid's frustration on this score was brought out by the title and content of an article she published when an emeritus, "How New Is the 'New Home Economics'?" (Reid, 1977). Reid also strongly criticized Becker's research at a seminar presentation he gave at the University of Chicago that took place during the 1969–1973 period, when he was working on the economics of the family (Marc Nerlove, personal communication, June 26, 2014). Another example of an economist critical of both Becker and Friedman was Harold Lydall, who had been hostile to Friedman's consumption work and who, in addition, would regard Becker's research on income distribution as trespassing on an area in which he believed there were already well-established tools (see Lydall, 1979, and Bronfenbrenner, 1981).

[221] Within the economics profession, Gardner Ackley put the matter in similar terms in a popular article about economic research that he wrote at the end of the 1970s: "Today, American economists apply sophisticated economic analysis... to marriage and the family; ... and to a vast range of other 'noneconomic' topics. Indeed, economists are the great imperialists of the social sciences." (*New York Times*, October 21, 1979*a*.)

Before Goode's comment appeared in print, the same phenomenon had been noted in the U.S. press—in a *Chicago Sun-Times* article, subsequently widely syndicated in newspapers in other cities—that reported on Becker's research on marriage, as represented by Becker's "Theory of Marriage: Part I" (which had appeared in the July/August 1973 issue of the *Journal of Political Economy*: see Becker, 1973). In a tone of understated bemusement, the article observed: "The same forces may explain why you buy in the economic marketplace may explain why[,] and whom[,] you will marry, according to a paper published recently by a University of Chicago economist. Gary S. Becker, a professor in the economics department, calls it 'a theory of marriage' that explains through economic principles how we go about choosing a mate." (*Corpus Christi Times* (Texas), November 30, 1973.)

Another Chicago newspaper, the *Chicago Daily News*, published an interview with Becker about his research on marriage. In a commendable show of restraint, the article did not imply, as so many accounts by popular writers (and some researchers) did, that the fact that an economics professor of economics at the University of Chicago had produced a piece of research meant that Friedman was somehow involved in instigating the research. Indeed, the article did not mention Friedman at all.[222] In its account, the article referred to what it called the "two papers published recently" by Becker on marriage (actually, one published and one in press for publication in the spring of 1974) and allowed him to provide an exposition of his research (*Chicago Daily News*, January 18, 1974, p. 9). Whether he provided this exposition in sufficiently nontechnical terms for the newspaper's readers is questionable: "Marriage occurs if—and only if—both of them are made better off and increase their utility," Becker was quoted as saying. He also remarked in this interview: " 'Unlike' persons will marry only when their mating maximizes the total household commodity output." (*Chicago Daily News*, January 18, 1974, p. 9.)

The *Chicago Daily News* reporter raised with Becker the criticism that the subject matter of his research was not suitable for economic analysis. Becker was unruffled: "There's usually a built-

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[222] The nearly irresistible temptation on the part of many writers to connect the research of Friedman's University of Chicago colleagues to his own work rests on a narrative in which professors in the economics department (or even researchers located in different parts of the university) continuously compared notes with one another. This narrative contrasts with the more mundane reality that Friedman's knowledge of the work done by microeconomists and econometricians in his department was superficial, and that his interaction with them was, frequently, not concerned with research matters. This reality is well captured by the observation of Marc Nerlove concerning his 1969−1973 period as a professor in the department: "we [Nerlove and Friedman] overlapped in the department, and we were there together in faculty meetings, but, other than that, and other than socializing with Milton and Rose, I had very little to do with Milton." (Marc Nerlove, interview, September 18, 2013.) As already noted, from 1973 onward Friedman did attend one microeconomics workshop—the industrial-organization workshop that was oriented toward law-and-economics issues. But discussions of Becker's research on the family were mainly occurring in other workshops—including the workshop in applications of economics (Becker, 1974a, p. S11).

in hostility to economists who get involved in these kinds of problems. But I've always worked a bit on the borderline. My first studies were about racial discrimination, and later ones involved crime and punishment." (*Chicago Daily News*, January 18, 1974, p. 9.) His answer was in line with the sentiment Becker had expressed in his first "Theory of Marriage" paper: "economic theory may well be on its way to providing a unified framework for *all* behavior" (Becker, 1973, p. 813).[223]

It was this kind of sentiment—prevalent both in Becker's work on marriage and in his earlier research—that had triggered the MIT reaction. Robert Solow recalled (interview, April 3, 2015): "I remember when I was first exposed to Gary's work, it struck me as excessive." Paul Samuelson had a similar reaction, and some of Paul Samuelson's *Newsweek* columns in the early 1970s may have been intended as a covert criticism of Becker's research agenda.[224] But they did not mention Becker and, like several of Samuelson's columns, had an unclear focus. Becker was not impressed by Samuelson's skills as a *Newsweek* columnist. Friedman's columns, he felt, were "much better. I mean, he killed Paul Samuelson in these columns, everybody knew that, partly because Samuelson didn't take them that seriously. When you read Samuelson's columns—[though] he had some very good columns—mainly they were just off-the-cuff remarks." In contrast, "Friedman really put time into his columns—rewrote them, got clarity. They were very clearly done, they were powerful, and they were far more influential." (Gary Becker, interview, December 13, 2013.)

The other key MIT skeptic about Becker's research, Robert Solow, recalled: "I was at a conference once where Gary announced that he was going to analyze the role of children in the family as durable consumer goods. (*laughter*) And, since I had a couple of kids at the time, I thought, 'I think this is going to miss something.' And I rather thought it did, yeah. I'm not very friendly to the sort of imperialism of economics [according to which] you can do political science by doing economics, you can do sociology by doing economics. That—the rational-actor thing extended everywhere—is not something that appeals to me." (Robert Solow, interview, April 3, 2015.)[225]

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[223] For his subsequent reaffirmations of this aim, see Becker (1976). See also the discussion in Reder (1982, p. 34).
[224] These columns appeared mainly in the 1969–1970 period (see Nelson, 2020b, Chapter 14, for a discussion) and so they preceded the publication of Becker (1973, 1974a). But Becker's interest in research on the topic of marriage began a few years before these articles saw print (see Becker, 1973, p. 813) and, of course, it came in the wake of his application of economics to other social matters.
[225] Solow's opposition to Becker's work regarding an economic theory of marriage was briefly mentioned by Heckman (2011, p. 173).

Friedman, basically, stayed out of this particular University of Chicago/MIT dispute. But he subsequently publicly applauded the Becker research agenda concerning marriage and children when, in 1981, Becker distilled it into his book *A Treatise on the Family*. "This truly pathbreaking book," Friedman remarked in his endorsement, "... is destined to affect the foundations of every science dealing with human behavior."[226]

During the 1973−1974 period, the MIT group's disputes with the research programs of both Becker and Friedman was reflected in the heterogeneous portfolio of publications produced by Alan Blinder, who following graduation from MIT in 1971 was located at Princeton University (American Economic Association, 1974, p. 35). Blinder's research output in 1973−1974 attested to his engagement in a two-front war. Two articles that he coauthored with Robert Solow took the fight to monetarism, while a solo-authored Blinder paper offered, via *reductio ad absurdum* methods, a contrarian perspective regarding Becker's aim of stretching out the areas covered by economic analysis.

Blinder and Solow on fiscal policy

Blinder and Solow's team effort in the Keynesian/monetarist debate came because a book, *The Economics of Public Finance*, was being produced under the auspices of the Brookings Institution.[227] Of the book, Blinder noted (interview, December 6, 2013): "It wasn't all about macroeconomics, by any means." Other chapters considered public finance and federalism. The Blinder-Solow chapter was, however, solely macroeconomic in its concerns.

The authors' contribution, the ambitiously titled "Analytical Foundations of Fiscal Policy" (Blinder and Solow, 1974), though only one chapter of the volume, was book-length in its own right—113 pages. Within the chapter, a section of over 20 pages was specifically concerned with monetarism—the title of the section, and a term used by the authors to refer to "an opposing school of thought, created almost singlehandledly by Milton Friedman."[228] The article went on to cite Friedman's work copiously. As often occurred with Keynesian coverage of Friedman's writings, the discussion had a complimentary remark to make about his pre-monetarist work in 1948 on fiscal/monetary arrangements, but very little good to say about Friedman's later work on money—other than an acknowledgment that "Friedman and his allies deserve credit for having

[226] See Friedman's remarks in Harvard University Press (1981, p. 1058), endorsing Becker (1981).
[227] Blinder and others (1974).
[228] Blinder and Solow (1974, p. 57).

restored money to an important place in discussions of economic policy."[229] Friedman and Meiselman's empirical comparison of money and autonomous expenditures came in for particularly withering remarks: for example, "Friedman and Meiselman do enter an important caveat—which they subsequently ignore…"[230] Blinder noted that the chapter "was, in substantial degree, intended to be a rebuttal to, Friedman-Meiselman, and then, even more so… to Andersen and Jordan, at the St. Louis Fed" in light of the "great deal of attention had been accorded recently" to Andersen and Jordan (1968) (Alan Blinder, interview, December 6, 2013).

The Blinder-Solow chapter was originally going to be something that many people, over the years, would imagine they had seen but never actually had seen: a critique of Friedman jointly written by Samuelson and Solow. Although Samuelson and Solow did produce jointly-authored product, none of this output consisted of a rebuttal directed at Friedman. Nor did Friedman in the twentieth century ever write anything that was explicitly presented as a response to, or commentary on, Samuelson-Solow work.[231] But as Blinder explained, "The original idea for that paper commissioned by Brookings was [that it would be written by] Samuelson and Solow. Samuelson lost interest in it, or got too busy or whatever—anyway, he wasn't doing it. And this was one of the highlights of my young life: Bob Solow asked me, a newly minted Ph.D.—literally, like, three days or something like that before I got my Ph.D.—to replace Paul Samuelson and partner with him to do this. So, you can imagine what I said!" (Alan Blinder, interview, December 6, 2013.) This would serve as a preparation of sorts. For, in time, as discussed in a later chapter, Blinder would actually confront Friedman on a solo basis, in a face-to-face public debate on fiscal policy.

Blinder and Solow pooled some of the more mathematical work arising from their project into a separate paper. This was published in 1973 in the *Journal of Public Economics*: "we named it 'Does Fiscal Policy Matter?' in juxtaposition to the old saw, 'Does monetary policy matter?'" (Alan Blinder, interview, December 6, 2013.) A great amount of Friedman's work appeared in

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[229] Blinder and Solow (1974, p. 58). For the mention of Friedman (1948), see Blinder and Solow (1974, p. 63).
[230] Blinder and Solow (1974, p. 64). The existence of the Blinder-Solow piece provides another useful refutation of the position, noted in Section I, that some critics of Friedman from outside the economics profession have taken in arguing that Friedman's work reflected the views and agenda of those financing his research. In fact, the Blinder-Solow and Friedman-Meiselman papers, notwithstanding their contrasting perspectives, had largely the same source of funding: as with many economic-research projects, financing came heavily from the Ford Foundation (Commission on Money and Credit, 1963, p. i; Kermit Gordon, 1974, p. vii).
[231] In particular, Friedman apparently did not cite Samuelson and Solow (1960) in his writings in the 1960s, 1970s, 1980s, and 1990s. As discussed in Nelson (2020a, Chapter 7; 2020b, Chapter 13), there is some evidence that he did not even register the paper's existence until a couple of years after it was published. Weighing against this possibility is the fact that Friedman (1962a) cited Becker (1960), a paper that appeared in the same proceedings volume as that containing Samuelson and Solow (1960).

the bibliography of this companion paper (Blinder and Solow, 1973). However, that bibliography may actually have been largely intended as a reference list for their book chapter. In the paper proper, Friedman was mentioned only a few times in the main text and, although a footnote referred to "the writings of Milton Friedman" (Blinder and Solow, 1973, p. 320), no specific Friedman paper was mentioned by the authors.

In retrospect, it is evident how much Blinder and Solow's book chapter and their "Does Fiscal Policy Matter?" paper represented a response to the 1960s literature challenging Keynesianism.

Their analysis was less relevant to newly emerging critiques of fiscal policy. This fact was brought out in the vintage of some of the empirical work cited in the Blinder and Solow (1974) paper: its section rebutting monetarism cited as evidence from "econometric models of the whole economy" a critique of Friedman's work that Solow had published back in 1963.[232]

To be sure, the Blinder-Solow (1973) paper was both more formal and more modern than its 1974 companion paper—being mathematically more detailed, and making explicit use of the intertemporal government budget constraint. However, a common feature of the papers was their concern with challenging arguments for the ineffectiveness of fiscal policy that were centered on crowding-out mechanisms. As a result, the authors insufficiently addressed permanent-income or Ricardian equivalence arguments.[233] Crowding-out notions were, as noted, prominent in Friedman's and others' current critiques of fiscal policy. But an emphasis on Ricardian equivalence was just around the corner. The idea that households would internalize the future taxes that budget deficits implied was noted by Blinder and Solow (1973, p. 325), but they rapidly dismissed it as a curiosum—in a passage so brief that Barro (1974, p. 1096) could quote it in full when advocating Ricardian equivalence.[234] This Barro article appeared in the

[232] Blinder and Solow (1974, p. 88), citing Ando, Brown, Kareken, and Solow (1963).
[233] Part of the problem lay in the fact that Blinder and Solow (1974, pp. 55, 89−90) took it as axiomatic that a backward-looking (distributed-lag) definition of permanent income was integral to Friedman's (1957) permanent income hypothesis. This was not, however, the interpretation of the hypothesis taken by either Friedman or its later advocates. And the distributed-lag formulation did not allow for the forward-looking conception of permanent income that is associated with Ricardian equivalence. As stressed in Chapter 8 below, Friedman had prior to 1974 himself invoked Ricardian equivalence as an argument against the effectiveness of fiscal policy, although it was not yet his main such argument. In particular, Friedman (1972a) invoked Ricardian equivalence. (This aspect of Friedman's 1972a argument was misinterpreted in Takayama, 1980, p. 609, who cited the paper as evidence of monetarists having "recently" moved to downplaying the influence that wealth had on the demand for money. The Friedman analysis did not, in fact, downplay the influence of wealth on money demand. It instead questioned—as Barro, 1974, would—the inclusion of government bonds in the definition of the private sector's overall wealth.)
[234] Even in 1976, when assessing the evolution of the fiscal/monetary policy debate since 1973, Blinder and Solow (1976, p. 502) were confident enough to state, "Keynesianism, then, has triumphed." They immediately went on to claim specifically that it was now an "empty" set of economists that did not believe that a permanent increase in

Journal of Political Economy in November/December 1974. In the previous academic year, on February 12, 1974, Barro had presented it in the money workshop (the only presentation that Barro gave of this soon-to-be-famous paper).[235] Unusually for this era of the workshop, Becker was in attendance alongside Friedman and others for Barro's presentation (Barro, 2007, p. 133).[236]

Blinder takes on Becker

Becker's presence at the money workshop session was, however, a brief diversion from his ongoing concern with applying economic analysis to social phenomena. Indeed, another contribution by Becker in this vein, "A Theory of Social Interaction" (Becker, 1974c), appeared, as the so-called lead article, in the November/December 1974 issue of the *Journal of Political Economy*—directly ahead of Barro's Ricardian-equivalence paper in the same *JPE* issue.

Two issues prior to the edition of the *JPE* that spotlighted Becker and Barro, the July/August 1974 issue had included an article by Blinder. Blinder's article appeared toward the back of the journal issue, directly after an article titled "Friedman Versus Tobin: A Comment" (Van Cott and Santoni, 1974). Unlike his articles with Solow, and also in contrast to the Van Cott-Santoni paper, this Blinder contribution was not concerned with the monetary policy/fiscal policy debate. Instead, it amounted to a critique—implicit, but unmistakable—of Becker's research agenda.

Blinder's (1974) short article was titled "The Economics of Brushing Teeth." In it, Blinder suggested that, in light of the "ever-growing literature on human capital," it was time to view "brushing as a human investment" and generate predictions not obtainable from sole reliance on "sociological theory."[237] Although the tone and acknowledgments in the paper were satirical, the paper's model and results were *bona fide*, and the literature drawn upon consisted of authentic contributions to the dental research literature. Blinder presumably submitted the paper to the *Journal of Political Economy* not expecting acceptance, but Becker—who was not actually

nominal government purchases, not accommodated by monetary policy, would have a long-run multiplier effect on aggregate nominal income. Barro (1981), however, would later show that the multiplier in question is zero when the permanent income hypothesis prevails in a general equilibrium model.

[235] Information from University of Chicago Library records and from Robert Barro (March 12, 2019).

[236] With regard to sessions in earlier years, Becker recalled: "I attended the money-and-banking [workshop], even though I wasn't working in that area at the time. And they were really stimulating workshops. You learned a lot about research, even if you weren't working on monetary problems." (Gary Becker, interview, December 13, 2013.)

[237] The first quotation is from Blinder (1974, p. 887), and the others are from page 890.

mentioned in the article—turned out to be supportive of publication, on account of the research that was reflected and reported in it (see Heckman, 2011, p. 174).[238]

Either Blinder himself or one of the journal's editors (Barro, Harry Johnson, Sam Peltzman, and George Stigler) also added a joke to the article, at Friedman's expense, in the form of an "Editor's Note" stating: "This paper derives from the Princeton oral tradition."[239] This was clearly a jab at the practice—which was obviously prone to being perceived as portentous—of casting one's work as the distillation, or continuation, of a University of Chicago oral tradition. Both Friedman and Don Patinkin had engaged in this practice in their monetary writings.

Moving from research to policy conclusions

One reason for the skepticism displayed by some economists in this era toward Becker's work was not always made explicit. This was the suspicion that his application of economic tools to the analysis of family and labor matters was producing, or would produce, policy prescriptions that were antithetical to government intervention (at the microeconomic level) in the economy.

Becker became far more active on policy issues only *after* the outpouring of his research on social issues in the 1960s and 1970s. In particular, it was not until early 1985 when, as Becker later put it, he "took a decidedly different path" by accepting—with Friedman's encouragement—an invitation from *Business Week* in 1985 to write a monthly column.[240] Even in the 1970s, however, and during the initial wave of his research on marriage, Becker foreshadowed that it would lead to many policy conclusions: "With this information [from his research], we'll be able to discuss… the role of alimony and child-support payments, [of] the population growth, and even the fairness of tax laws. Our analysis will be useful in developing public policies that impinge on marital decisions." (*Chicago Daily News*, January 18, 1974, p. 10.)

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[238] From 1985 to 1994, Becker and Blinder were opposite numbers of sorts as the regular academic-economist columnists for *Business Week*. This did not prove in practice to be a repeat of the Friedman/Samuelson *Newsweek* adversarial arrangement, as Becker's columns (discussed further below) delved little into macroeconomic topics other than those pertaining to the labor market.

[239] Blinder (1974, p. 887).

[240] See Becker and Becker (1997, p. 1) for the quotation and date. Page 2 of the same source indicated that Friedman encouraged Becker to become a columnist, as well as that, to Becker's surprise, George Stigler did so too. This posture on Stigler's part is less surprising in view of the fact that, as discussed in Nelson (2020b, Chapter 15), Stigler had also, in 1966, encouraged Friedman joining *Newsweek* and, despite Stigler's jaded attitude toward economists' scope to influence economic policy, was reasonably supportive of Friedman's public-policy activity more generally. Becker and Becker (1997, p. 2) also indicated that Stigler thought Becker might run out of material after two or three years (roughly 24–36 columns). This paralleled Stigler's posture toward Friedman in 1966. At that time, when Stigler hinted that Friedman might need to drop his column after about a year (that is, after about 17 columns—Friedman's column was on a three-weekly basis). See Friedman (1993, p. 772).

The expectation that Becker's perspective on the microeconomics of the household would point him toward non-interventionist policy prescriptions was certainly borne out on one important issue: that of government support for paid family leave from work. In *Business Week* columns of July 14, 1986, and December 2, 1991, Becker opposed proposals for such support. In 2005, the year after his *Business Week* column ended, Becker emphatically reaffirmed his opposition in a blog entry titled "Should Governments Subsidize Child Care and Work Leaves?" in which Becker answered the question in the negative and concluded: "I believe that present American policy… does not need drastic changes." (*The Becker/Posner Blog*, October 30, 2005.)

It should be emphasized that opposition to this policy does not follow automatically from the overall approach to social economics that Becker pioneered. Indeed, one of Becker's former students, Claudia Goldin, would do research that was favorable to paid parental leave and to government policy that encouraged or mandated it.[241] However, in the case of Becker, it was certainly true that his research activity in connection with labor issues went alongside his expression of what were largely free-market policy prescriptions.

Friedman, too, was a subscriber to free-market labor policies. But it would be incorrect to suggest that Friedman ever really tapped heavily into Becker's research on the family in reaching his own policy conclusions. As has been stressed repeatedly above, it is simply a misconception that Friedman and Becker were steeped in each other's research or were subscribers to the same research agenda. No doubt, Friedman contributed to this misconception when he wrote an endorsement (without, however, endorsing the specifics) of a 1982 popular book called *Tomorrow, Capitalism* that portrayed Friedman's, Hayek's, Coase's, Stigler's, and Becker's writings as part of a closely integrated single body of thought in positive and normative economics.[242] And later, the University of Chicago also likely strengthened this misconception

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[241] In particular, Goldin, Kerr, and Olivetti (2020, p. 25) stated: "There is a clear role for policy. State-mandated PFL [paid family leave] or PPL [paid parental leave] fills a gap…" Although they referred to this policy as a mandate directed at firms, the authors also indicated that the policies would involve additional public expenditures (2020, p. 1).

[242] The book in question was LePage (1982). Friedman's praise for the book (see Open Court Publishing Company, 1982, p. 1326) was mainly with regard to the quality of its exposition: "a remarkable *tour de force*… comprehensive, accurate, and yet accessible to the general public." A more considered account than LePage's, albeit focused on a somewhat later period, was Krugman's (1995) book on the "arguments against activist government" (to quote the description on the back cover) that were made by U.S. economists from the late 1960s through the 1980s and were influential on the Reagan Administration. Other than the supply-side movement, which largely operated in non-research outlets, Krugman named Friedman, Martin Feldstein, and Robert Lucas as the key figures who articulated these arguments. (He also suggested that only the first two of these figures made interventions in the public sphere—a claim that was not entirely accurate, as Lucas actually did several such interventions, including some made jointly with Friedman, from the early 1970s onward.) Although the book was certainly not accurate in every aspect, Krugman (1995) was correct in implying that the bodies of work of Friedman,

via the forming of the research center (formerly two separate bodies) called the Becker Friedman Institute. But it is a misconception all the same. As indicated above, Friedman regarded Becker's research on the family as pathbreaking. But the path Becker broke was not one that Friedman himself followed.

The fact that Friedman's and Becker's research agendas went on different paths was also reflected in quite different bases for their policy prescriptions. Both Friedman and Becker were free-market-oriented economists, but it is very unlikely that Becker's work on the family had a material effect on the analyses and prescriptions that appeared in Friedman's public-policy writings. For example, the Friedmans' book *Tyranny of the Status Quo* made no mention of Becker, even though it had a discussion of issues related to the family that appeared in a chapter specifically on the subject of crime.[243] And with regard to the labor market, Friedman's views had largely crystalized before Becker's 1970s-vintage research appeared.[244] For example, although it can be confidently stated that Friedman was opposed to government-provided subsidies that were specifically in aid of parental leave or family care, his opposition did not rest on, or arise from, Becker's research findings. Instead, that opposition reflected Friedman's longstanding position that government-provided financial aid to households should not consist of funds associated with specific expenses but should instead be consolidated into a single payment, provided through a negative income tax (NIT). Friedman reaffirmed in 1976 that he favored the NIT arrangement, as a means of providing relief to households "on a general basis" and then letting "people choose how to use those funds" (*The Jay Interview*, ITN, July 17, 1976).

Friedman's continuing advocacy of the NIT in the 1970s underlined the fact that his positions on labor supply and welfare did not amount to either a fully *laissez-faire* or purely libertarian stance. Indeed, it was this facet of Friedman's views that prompted criticism of him in an editorial that appeared in several conservatively-inclined daily newspapers during the spring of 1974. "Part-time libertarian economist Milton Friedman toyed with the [NIT] idea in the mid-'60s," the editorial stated caustically. "We don't know whether he recovered from the hangover or not." (*Columbus Telegram* (Nebraska), April 1, 1974.)[245]

The occasion for that editorial was a proposal by Caspar Weinberger, U.S. Secretary of Health,

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Feldstein, and Lucas, although produced independently, were closer to each other in theme and in subject matter than were the writings of Friedman and Becker (a figure whom Krugman's book did not mention).

[243] See Friedman and Friedman (1984, 1985).

[244] Friedman's views on labor market policy are considered in more detail in Chapter 9 below.

[245] In fact, of course, although it was indeed the mid-1960s when Friedman produced a set of articles on the NIT, his advocacy of the NIT began earlier than that period.

Education, and Welfare, to introduce NIT-type top-ups to U.S. household incomes. The Nixon Administration had taken NIT-like ideas to an advanced stage in its first term, but its legislation in the area had ultimately been defeated. The Weinberger initiative, occurring in President Nixon's curtailed second term, fell short of reaching even this stage. It went nowhere. Then, in 1975, the Ford Administration would introduce a couple of policy initiatives related to NIT, and one of the associated proposals would actually become law. But Friedman disowned all the Nixon and Ford Administration proposals in this area, precisely because they did not meet the key feature of his own NIT plan: that the NIT should consolidate and replace all government-provided income support, and not merely serve as an additional means of dispensing such support.

WALTER HELLER

On December 29, 1973, during the American Economic Association meetings in New York City, Gary Becker was elected to one of the two AEA vice-president positions.[246] In becoming part of the AEA's leadership for the year ahead, Becker would serve alongside Walter Heller, who had already been elected as 1974's president of the AEA.[247]

Heller's macroeconomic orientation meant that he was far closer to Friedman in his area of specialization than was Becker. But this similarity between Heller and Friedman actually had the effect of greatly increasing the number of occasions when they would find themselves far apart. There was little inkling of this at the start of the 1940s, when Friedman had been one of Heller's teachers. That period was, as Blinder (2002, p. 401) implied, "years before" Friedman became a monetarist. Indeed, he was an enthusiastic Keynesian in the early 1940s (see Nelson, 2020a, Chapter 3). In contrast, by the early 1950s Friedman was a monetarist, and he and Heller had become outspoken advocates of contending macroeconomic schools.

Friedman and Heller would be at loggerheads on many occasions, with regard to positive and normative economics alike. Correspondingly, in numerous public debates, both written and spoken, they would advance diametrically opposed macroeconomic judgments and prescriptions. Not infrequently, the spoken part of their disagreements would unfold in one another's presence—on television or in other setpiece public debates. And, beyond their numerous

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[246] See Fels (1974, p. 445). The other vice-president elected was John Gurley, the coauthor of the Gurley-Shaw study mentioned in Section II above and, in addition, one of the first economists to use the term "Friedmanite" in print (Gurley, 1969, p. 1189).
[247] See American Economic Association (1974, p. VI).

skirmishes on macroeconomic matters, Heller would take aim at Friedman proposals outside the macroeconomic realm—sometimes viscerally so.

The Friedman/Heller competition takes shape

Like Becker, Heller had once been a graduate student of Friedman's—in Heller's case, during Friedman's brief time (in the 1940/1941 academic year) at the University of Wisconsin.[248] Friedman's period of employment with that institution had ended acrimoniously. But the same experience had also seen him establish a friendly relationship with Heller, who had socialized with the Friedmans during the year and had vigorously favored Milton Friedman's ultimately unsuccessful attempt to secure a permanent position at the university (*Time*, December 19, 1969; Silk, 1976, p. 59; *Los Angeles Times*, December 14, 1986, p. 16).[249]

When Friedman and Heller were consecutive speakers in the afternoon session of the White House Economists Conference on Inflation on September 5, 1974, a faint echo of the early origin of their relationship was heard. Remarking on the fact that Friedman was seated next to him, Heller quipped: "Milt Friedman is to the left of me today."[250] Usage of the "Milt" derivative of his name was something to which Friedman had given next to no encouragement over the prior three decades. Yet things may have been different during his first decade or so in the world of economics. For, in the 1940s and 1950s, it was a commonplace that George Stigler, whose friendship with him dated back to that first decade of Friedman's activity in economics, would open his correspondence with Friedman by writing: "Dear Milt, …"[251] It is quite likely, too, that, when Heller first crossed paths with Friedman in 1940, the latter had not yet fully eschewed the

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[248] A profile of Heller that appeared near the end of his career stated that "Heller and Friedman… met as graduate students at the University of Wisconsin" (*CLA Newsletter*, Winter 1986, p. 3). This statement was misleading, as Friedman and Heller were not actually fellow enrolled students at the university. But it was technically accurate: when he was at the University of Wisconsin, Friedman was in the process of completing his Columbia University doctoral dissertation and so was still formally a graduate student.

[249] See also Lampman (1993, p. 119) and Friedman and Friedman (1998, pp. 93, 95). A possible glimpse into the conversations about economics that Friedman and Heller had during this early period came when Heller testified on August 21, 1974, about how Arthur Burns during his NBER years "helped Wesley Clair Mitchell and Simon Kuznets perfect the national accounts" (Committee on the Budget, U.S. Senate, 1974, p. 248). Although Heller did not mention Friedman in this context, Friedman had been a witness to, and participant in, this NBER activity during the 1937−1940 period, before taking up his University of Wisconsin position.

[250] In Council of Economic Advisers (1974, p. 74).

[251] See Hammond and Hammond (2006). By the time Anna Schwartz started working with Friedman in the late 1940s, Friedman was not promoting the use of the nickname, and she did not embrace it. On Stigler's continued use of "Milt" in this period, Schwartz observed: "George and MF were bosom friends. George was a man of great good humor. For George to address MF as 'Milt' did not signify a lack of respect for MF's dignity. I think it was more likely a sign of affection by a big man for a little guy." (Anna Schwartz, personal communication, August 4, 2009.)

“Milt” nickname.[252]

The highly amicable relations established between Friedman and Heller in the early 1940s would be put under considerable strain by their exchanges over the following four-and-a-half decades—and particularly those occurring in the quarter-century that started in 1961. The year 1961 was pivotal for both Heller and Friedman. Indeed, that year can be seen as one in which matters turned in a direction that ensured that Heller and Friedman were, in the public square at least, major sparring partners in the Keynesian-monetarist debate.[253] It was the year in which Heller began a nearly four-year spell as chairman of the Council of Economic Advisers—thereby becoming a leading exponent and practitioner of Keynesian policies in the United States.[254] And it was also the point at which Friedman’s profile and productivity as a critic of Keynesian economics were moving into high gear. This shift on Friedman’s part was manifested in his spurt of publications in 1962 and 1963 and what became much-discussed pieces of Congressional testimony in both 1963 and 1964. All of these Friedman activities occurred during Heller’s CEA tenure, which covered the Kennedy administration as well as roughly the first 50 weeks of Lyndon Johnson’s presidency.

Heller in office

There were Keynesians in academia that had stronger research credentials than Heller, but, thanks to his heading the CEA in the 1960s, he became the leading national face of U.S.

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[252] In later life, Heller would also refer to Friedman (outside his presence) as “Uncle Miltie” (*Minneapolis Star and Tribune*, June 17, 1987*b*, p. 1A). Although it was occasionally applied to Friedman in media discussions (for example, *American Film*, April 1980), this appellation was more commonly associated with comedian Milton Berle, especially during his broadcasting heyday (in the 1950s) as “television’s first star” (Inman, 1991, p. 100).

[253] A pro-free-market economics journalist, Lawrence Fertig, foreshadowed the emergence of Heller and Friedman as key representatives of opposite camps by discussing both economists in a 1961 book, titled *Prosperity Through Freedom* and published by the conservative Regnery Press. As discussed below, Fertig (1961) included a polemical retrospective on Heller’s warnings to the postwar (West) German authorities about moving to reliance on market mechanisms for the allocation of resources, as well as on Heller’s deprecation, in that same advice, of the contribution that monetary measures could make to stabilization policy. Fertig was also critical of what he perceived as the mindset of acceptance of heavy government intervention in the economy prevalent in U.S. universities, “with a few notable exceptions such as The Graduate School of Chicago University [sic], with such outstanding teachers as Dr. Milton Friedman and Dr. George Stigler” (Fertig, 1961, p. 76). In line with the still-low public profile of Friedman in 1961 (see Nelson, 2020b, Chapter 11), Heller received an index entry in Fertig’s book, but Friedman did not.

[254] Heller was already prevalent on the Washington scene as an academic expert on current economic developments. Ronald McKinnon, a graduate student at the University of Minnesota up to his Ph.D. receipt in 1960, recalled that Heller “taught me public finance at the University of Minnesota. He was one of my teachers. And the problem was, as a teacher, he was always in Washington. So, the few times he did show up, he would give his latest congressional testimony. [And] he got one of the better graduate students to actually provide the formal lectures. But I kind of liked Walter.” (Ronald McKinnon, interview, January 23, 2014.)

Keynesianism—especially the policy-oriented dimension of it known as the “new economics.” “The most notable fact about the appointment is that Walter isn’t from Cambridge [U.S.],” one economist was quoted remarking after President-elect Kennedy nominated Heller to the CEA position. The columnist reporting these remarks noted accurately that nevertheless Heller would fit in at both Harvard University and MIT (*The Evening Press* (Binghamton, New York), December 30, 1960).

“I was an undergraduate at the University of Minnesota,” Thomas Simpson recalled (interview, May 29, 2013). By the time I started studying economics, Heller was in Washington, as chairman of the Council of Economic Advisers. So I never had him in the classroom. He was representing, in many respects, the Cambridge-New Haven views [on] activist fiscal policy, which meant that government really had an active role to play in economic stabilization.”

Heller’s status as the leading representative of U.S. Keynesianism in national public debate, despite not being its leader in the research world, was reflected in the fact that those who worked for him as fellow CEA members or staff included James Tobin, Robert Solow, or Arthur Okun, all of whom would during the 1960s and 1970s greatly outpace Heller in quantity and impact of their research contributions during the 1960s and 1970s. Solow and Tobin (1988, p. 6) granted that “no one could match Heller's knack of making points in concise[,] readable[,] colorful language.”

Heller out of office

The identification, in public discourse, of Heller as the leading ambassador for the “new economics” remained beyond his departure from the CEA in November 1964. Indeed, a full four years after his stepping-down as CEA head, a November 1968 newspaper analysis referred to Heller not just as “the chief architect of the United States’ current policies of ‘new economics,’” but also more broadly as “the chief disciple of John Maynard Keynes” (*New York Times*, November 13, 1968, pages 61 and 63). Heller had in previous decades produced analytical work on fiscal policy. As stressed below, however, the status that this 1968 news article attributed to him was something that resulted overwhelmingly from Heller’s public-policy participation, rather than from his research.

Heller’s preference for public-policy activity over research did not change after his return to academia. When he produced a monograph, *New Dimensions of Political Economy* (Heller, 1966), the accompanying materials described it as “his first book since leaving Washington to

return to the University of Minnesota."[255] That description seemed like a promise that Heller was returning to the economic-research world. But if this was the promise, it was not fulfilled. Although produced by a university press, the book—which Heller had developed by rewriting and expanding guest lectures he had delivered at Harvard University—was, in essence, a detailed but nontechnical discussion of the practice of Keynesian economic policy in the United States in the 1960s. The book would, in the event, attain status as a contribution worth referring to in the research literature, thanks not only to Heller's undoubted standing but also because it would be cited in such prominent journal discussions as that of Lucas (1979, p. 163). Even Lucas' citation, however, involved noting that *New Dimensions* was intended for "a general audience," and Lucas offered the book as a representation of a strategy regarding macroeconomic policy that Lucas believed had failed.

New Dimensions would also be used as an encapsulation of the Keynesian approach to economic policy in textbooks and readings collections in the 1960s and 1970s.[256] It was nevertheless, essentially, a highbrow piece of popular writing, not a research contribution. In the same vein was a 1968 book edited by Heller, *Perspectives on Economic Growth*, issued by the mass-market publishers Random House. Its contributions highlighted findings found in the research of Robert Solow and others, and it included what was basically a technical research article by Arthur Okun—indeed, one that Okun had, at an earlier point, believed would be published in the February 1966 issue of *Review of Economics and Statistics* (though, in the event, the paper did not ever appear in that journal).[257] On the whole, however, it remained the case that this Heller (1968) volume was predominantly a nontechnical presentation, for a business readership, of matters concerning economic growth.

Richard Anderson, an undergraduate student at the University of Minnesota, recalled of the economics courses he took at the university, "I took Principles from Walter Heller. That's all I

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[255] Heller had previously been at the University of Minnesota since 1946 (the same calendar year that had seen Friedman's departure). He served as an associate professor until 1950 and as a professor thereafter (Europa Publications Limited, 1986, p. 674).
[256] See, for example, McConnell (1975a, p. 2) for a textbook reference to the book.
[257] Okun's (1968) paper—which was actually concerned with the effects of the 1964 tax cut, rather than with the main concern of the Heller (1968) collection, the drivers of longer-term economic growth—had been presented at the American Statistical Association meetings in Philadelphia on September 8–11, 1965. Okun delivered his paper at a session on September 10 (Heller, 1966, p. 182). The Okun paper was also circulated as a 26-page typescript at around that time (see Congressional Research Service, 1978, p. 119). Okun refrained from publishing his study in the 1965 conference proceedings volume because of his belief that its publication as a journal article was already imminent (see American Statistical Association, 1965, p. 155). Friedman implied that he had seen this typescript—which appeared while the "AM/FM" debate on the fiscal multiplier was reaching print—but, in formally referencing Okun's paper, he cited the published 1968 version. See Friedman and Heller (1969, p. 55, 85).

knew of him. That was 1969, I suppose. He spent the entire term telling stories about his time in Washington. He probably did something more analytic than that, but I don't remember it…. [It was] my impression no one regarded him as sort of serious economic researcher. He was teaching Principles, but I didn't see him doing much else." (Richard Anderson, interview, November 14, 2013.)

The inclination on Heller's part toward public-policy activity continued into the 1970s. To be sure, he maintained ties in research and academic circles: he had resumed teaching at the University of Minnesota; he organized (in his role as AEA president-elect) the program of sessions for the 1973 AEA meetings; and he held the position of Chairman of the NBER during the early to mid-1970s.[258] But, although he would categorize himself as carrying out research on "The Role of the Economist in Public Policy Making" (American Economic Association, 1974, p. 171), Heller did not produce a material amount of research in his post-CEA years.[259]

Instead, other than the production of his popular books, Heller concentrated much of his professional activity in the years after 1964 in repeated participation in activities that—though often high-profile—were largely ephemeral: panel appearances, public debates, testimony before Congressional committees, meetings with policymakers, written and spoken analyses of current economic developments for financial institutions and economic clubs, and commentaries in the print and electronic media about the U.S. economic situation.[260] Even his AEA presidential address of December 1974 (Heller, 1975) did not mark a concerted return on his part to economic research. It was, rather, a stocktaking exercise that examined the progress made by the economics profession in expanding knowledge. Reflecting its nontechnical character, the address was reprinted by Heller in his popular book, *The Economy: Old Myth and New Realities*

[258] On Heller's NBER position, see, for example, National Bureau of Economic Research (1974b, p. Siii). In addition, according to the *Daily News* (New York), April 28, 1973, Heller was the head of the University of Minnesota's economics department during 1973.
[259] He was, however, highly esteemed figure at his university. Although Heller would state, in compressed biographical sketches, that his position as Regents Professor started in 1946 (American Economic Association, 1974, p. 171), this particular title was actually a highly prestigious one, connoting an especially high ranking in the University of Minnesota hierarchy, and it was not bestowed on him until 1967 (*The Star-Tribune* (Minneapolis), March 19, 1967; Europa Publications Limited, 1986, p. 674). Furthermore, in his post-CEA years, the university and department showcased Heller by having him as a key instructor for the very high-enrolment introductory-economics course—which Heller continued to teach in Fall 1986, even after becoming an emeritus and ceding the Regents title earlier in the year (*Minneapolis Star and Tribune*, June 17, 1987, p. 11A). Tig Inahtko—whose period as an undergraduate student in economics at the University of Minnesota occurred during Heller's final years at the department—related in 1992 (when he was a teacher, of the present author among others, at the University of Sydney) the reverence with which Heller had been regarded by the economics world of the University of Minnesota.
[260] Heller also served on the board of a commercial bank, the National City Bank of Minneapolis, from the mid-1960s to 1986 (Europa Publications Limited, 1986, p. 674).

(see Heller, 1976a, pp. 167–213).[261]

During the later 1970s, Heller maintained his status as the nation's best-known Keynesian economist, and his activity during the Carter years included a letter to *New York Times* with his one-time CEA subordinates Arthur Okun, James Tobin, and Robert Solow (March 12, 1978).

Correspondingly, at the end of the 1970s, a nationally syndicated profile of Heller claimed that he was the "high priest of economics" and "an economist's economist." Both labels were excessive, but they accurately reflected the high profile that Heller had sustained for two decades. The article quoted his friend Joseph Pechman remarking: "I don't think there's anybody in economics who is as effective in writing and speaking as he is." The same article had words from Milton Friedman, to whom it hyperbolically referred as Heller's "arch-rival," in a similar vein. Crucially, however, Friedman included qualifications that both identified Heller's position in economic debate and Heller's withdrawal from the research fray. "Walter Heller has been an extraordinarily effective political spokesman" for activist Keynesian policies, Friedman observed. "I just happen to disagree with those policies." (*San Francisco Sunday Examiner and Chronicle*, August 26, 1979, p. 10C.)

In sum, despite his very high profile in U.S. economic discussion throughout the 1960s and 1970s, Heller was not a leading contributor to the research literature on the Keynesian-monetarist debates, and he was not seen by knowledgeable observers as a prominent player during the 1960s in the journal-oriented economic debates on macroeconomic matters that involved Friedman. Reflecting that background, Heller was altogether absent from the 1972 *Journal of Political Economy* symposium on Friedman's monetary views—being neither a participant, nor a figure mentioned by any of those who did contribute (including James Tobin, who had, as noted, served on the CEA during the early 1960s, when Heller was head of the council).

Thomas Sargent became a colleague of Heller's at the University of Minnesota's economics department in September 1971.[262] Sargent recalled: "Walter Heller was a very nice man. [But] he was not an academic economist anywhere near Milton Friedman. So they couldn't have a debate on even ground. I mean, Walter didn't know models, and he didn't know math." (Thomas Sargent, interview, January 24, 2014.)

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[261] In Europa Publications Limited (1986, p. 674), Heller also implied that Heller (1975) had appeared as a free-standing book. This may have simply been a reference to offprint versions of the published article.
[262] See Sargent's resume at *https://files.nyu.edu/ts43/public/personal/resume.pdf.*

Monetary versus fiscal policy

Nevertheless, debates between Friedman and Heller occurred in the 1960s and 1970s—frequently. The reason, naturally, was that Friedman, though remaining active in research for most of this period, was himself deeply engaged in popular discourse. In the public arena, Friedman and Heller became identified as opposite numbers. The most famous and longest remembered of their various exchanges was a joint appearance on November 14, 1968, at New York University. This debate—discussed at various points in the previous two volumes—had an overflow crowd that the organizers, using technology that was sophisticated at the time, accommodated via a video feed of the debate into a second auditorium (*New York Times*, November 13, 1968; Instructional Dynamics Economics Cassette Tape 3, November 1968).

From this debate between Friedman and Heller flowed a publication in April 1969 under both their names, *Monetary Vs. Fiscal Policy*.[263] A small, low-production-values paperback—albeit a physically durable one, printed on better paper stock than that for Friedman's *Price Theory: A Provisional Text*—it numbered only 95 pages. Indeed, Wonnacott and Wonnacott (1979, p. 41)—in one of the numerous textbook discussions in the 1970s that cited the volume—could not bring themselves to refer to it as a book, opting for the label "booklet."[264]

Despite being a record of what was, formally, an academic event (the Arthur K. Salomon lecture), this Friedman-Heller publication amounted, essentially, to still another popular work in Heller's bibliography. *Monetary Vs. Fiscal Policy* did, however, help build bridges between the research and general-interest worlds of economics, as it contained Friedman's and Heller's discussion, with accompanying bibliographical citations, of recent years' research contributions (notably by Leonall Andersen and Jerry Jordan on the monetarist side, and Arthur Okun on the Keynesian side) pertaining to the effects of monetary policy and of fiscal policy. A further novelty of the book was that it included an articulation by Friedman of his position that, although he was of the opinion that fiscal policy mattered very little (given monetary policy) for fluctuations in nominal aggregate demand, he did believe it could be very important for the longer-term course of real income because of its supply-side effects.[265] The latter aspect of his

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[263] The date of publication was given in Moritz (1970, p. 154). The cover of the book, though not its interior front matter, suggested that it had a subtitle—*A Dialogue*—and this subtitle was sometimes used in citations (see, for example, Wonnacott and Wonnacott, 1979, p. 241, and Dewald, 1984, p. 17).

[264] For another prominent textbook reference to the debate, see McConnell (1975a, p. 362).

[265] See Friedman and Heller (1969, p. 50). During the same event, Friedman also remarked (see Friedman and Heller, 1969, p. 69) that he had never argued that fiscal policy provided an effective stabilization device. Friedman had, in fact, once been a proponent of the merits of fiscal policy, in its own right, as an active tool in stabilization policy. The period when this was his position had been roughly a generation earlier—in the part of the 1940s when

views was one he would elaborate upon on later occasions.

The book did Heller a lot of good. It put him on the same footing as Friedman in a key debate, and it kept his name in print in those specialized economic publications that were less ephemeral than the financial press. For example, as already indicated, *Monetary Vs. Fiscal Policy* received attention in textbooks. It also obtained some citations in high-profile research articles, such as David and Scadding (1974).[266] Most citations of the book were in relation to Friedman's interventions rather than to what Heller had said.[267] However, a prestigious readings collection edited by Arthur Okun, one of the members of Heller's 1960s CEA team, did include an excerpt from Heller's contribution to *Monetary Vs. Fiscal Policy* under the title "Doubts About Monetarism" (Okun, 1972a, pp. 108−116).[268]

Heller's own pride in the book, through the end of his life, was evident in the fact that the capsule biography he supplied for a 1987 event stated that "Dr. Heller has… authored such works as *The Economy: Old Myths and New Realities*, *New Dimensions of Political Economy*, and *Monetary Vs. Fiscal Policy* which he coauthored with Milton Friedman."[269]

The event in question was the 1987 Minnesota Economic Summit, scheduled for June 18, 1987. Despite agreeing to participate, and being listed on the advance program as a speaker, Heller never made it to this event. Friedman and Heller each had heart attacks at roughly the same age. But whereas Friedman had survived a heart attack in 1984—about three months after his seventy-second birthday—Heller died of a heart attack on June 16, 1987—a little under three months short of his own seventy-second birthday.

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he was a strong subscriber to Keynesian economics. He would acknowledge his one-time belief in fiscal management of the economy in his remarks at the later (May 1969) Karl Compton lecture, alongside Paul Samuelson (see Nelson, 2020a, Chapter 3). Heller was in a position to know firsthand (from their interaction in the 1940/1941 year, as well as their 1942−1943 period of overlap at the U.S. Treasury—see American Economic Association, 1974, p. 171) that it was an overstatement for Friedman to say that he had never propounded the stabilization properties of fiscal policy. The published version of the November 1968 debate did not, however, contain an objection on Heller's part to Friedman's characterization of his own record.

[266] Specifically, David and Scadding (1974, p. 243) pointed to Friedman's discussion of crowding out on pages 54 to 56 of *Monetary Vs. Fiscal Policy*.

[267] Indeed, Wonnacott and Wonnacott (1979, p. 40) juxtaposed Friedman's advocacy of monetary policy's importance not against Heller's views, but instead with those expressed elsewhere by Warren Smith. This made a sharper contrast because Heller was not, by the 1960s, a hardline denier of the effects of monetary policy on aggregate demand. Heller's views on monetary policy are discussed further below.

[268] Okun (1972a) also reprinted the exposition of the fiscal multiplier that he, Heller, and others had composed for the 1963 *Economic Report of the President* (Council of Economic Advisers, 1963; see Okun, 1972a, pp. 54−62), as well as a portion of Heller (1966), in which Heller had given an account of the Johnson Administration's 1964−1965 fiscal stimulus (see Okun, 1972a, pp. 140−149).

[269] In Columbia Institute (1987).

Television adversaries

The New York University debate had not been televised, but the video feed that carried it into a secondary auditorium was described by Friedman—using the terminology of the time—as "closed-circuit TV" (Instructional Dynamics Economics Cassette Tape 3, November 1968). In the years both before and after this exchange, Friedman and Heller's sparring had many incarnations on *real* TV, as they were regular opponents in on-air broadcasts. By the 1980s, both Friedman and Heller had become well-established presences in television studios, and they had often appeared face-to-face on the same program.

This pattern was set in the late 1960s, when Friedman and Heller were recurrent joint guests on a succession of news specials, discussing State of the Union speeches and other major events related to national economic policy, on National Educational Television (NET).[270] Their on-screen sparring continued into the 1970s. Among the joint Friedman-Heller appearances were a PBS special on inflation, *Inflation: The Money Merry Go Round* (see *Daytona Beach Morning Journal* (Florida), October 7, 1974), and a one-off NBC news program, broadcast on New Year's Day 1975, *If You Think It Was Tough To Make Ends Meet in 1974, Wait 'Till You Hear About 1975* (*The Star-Ledger* (New Jersey), January 1, 1975). Near the end of the decade, fellow economist Marina Whitman (who, like Heller, had once served as a CEA member) brought them on as her guests, each representing rival macroeconomic positions, in an episode, "Why Economists Disagree," of her program *Economically Speaking* (*San Antonio Light* (Texas), June 10, 1979).

This is not to say that Friedman's and Heller's interactions in the 1970s were confined to these appearances. For example, as already noted, they both participated in the September 1974 White House conference on inflation (as well as the follow-up conference in New York City later in the month), and they were among those on the program for the December 1974 AEA meetings.

There was even a further academic context for one of the 1970s encounters that the two had. In late 1974, Friedman, in a rare move by this stage, accepted an invitation to participate as a guest speaker in a research seminar—in this case, an invitation from the economics department of the University of Minnesota. Friedman would have been aware that Heller's younger departmental colleagues, like John Kareken, Thomas Sargent and Neil Wallace, were making waves by producing work critical of Keynesian economics, and he may have been curious to see how the

[270] Inman (1991, p. iii) noted: "PBS was known as NET during the 1960s."

Keynesian mainstay Heller was handling this new situation. Heller had, in effect, already accommodated himself to the changed environment somewhat by ceding the graduate teaching of the department to the younger generation of instructors. Michael Salemi, a Ph.D. student at the university during this era, recalled: "Walter Heller, by this point in his career, occupied the stage in front of the Econ 101 class [called Economics 1001 at the University of Minnesota]. So, when macro was offered to undergraduates, the drill was: once a week with Walter, and then the graduate students would have the other meetings, in which they would teach the micro theory. Walter's lectures were a series of recounting his experiences in various episodes of policy on the ground—a lot of talk about the Kennedy Administration and such." (Michael Salemi, interview, November 12, 2014.) Indeed, Heller would tell a friend about the prospect of graduate teaching, "Oh, no, I couldn't handle that. Those graduate students would eat me alive." (*Minneapolis Star-Tribune*, June 17, 1987*b*, p. 13A.)

With regard to Heller's attendance of seminars, Salemi noted: "I'm not sure he was there [on occasions when] the new young folks were coming who were exciting to Sargent and Wallace. But I'm sure that when a big-name person came, he came too—though I don't remember his voice being, shall we say, often present in the intellectual debate." (Michael Salemi, interview, November 12, 2014.) Friedman's seminar, of course, was one sure to generate Heller's attendance. If, however, Friedman hoped that the resulting event would see the younger University of Minnesota economists teaming up with himself against Heller, he would be disappointed. Instead, the contents of Friedman's seminar presentation—heavy in NBER-tradition econometrics and his own stress on protracted effects of monetary changes on output—saw him take heat from the younger seminar participants, who were steeped in rational expectations macroeconomics and modern time-series analysis (see Nelson, 2020b, Chapter 15).

It was, however, their national TV appearances together that, notwithstanding Heller's inactivity in the research debates concerning Friedman, cemented the pair's image as a double act.[271] This continued into the 1980s: the two of them were renowned as "terrific adversaries" who "do battle on macroeconomic questions," Heller reflected in 1986 (*CLA Newsletter*, Winter, 1986, p. 3). This image had lucrative results for Heller and Friedman alike. For example, the American Home company organized an event at Los Angeles' Beverly Wilshire Hotel, held on October 28, 1981, that opened with a 90-minute talk by Friedman, followed by a 90-minute address by Heller, after which a 90-minute question-and-answer session had both of them on the stage. The

[271] The fact that Heller frequently encountered Friedman on television gave their public confrontations a higher profile than those Friedman had with Paul Samuelson. During the 1970s, Friedman's debates with Samuelson appeared mainly in outlets other than television.

publicity material for this event stated that American Home had "spared no expense to bring to you the most distinguished" figures in economics, while indicating that those wishing to be in the audience for the event would be charged $250 per person (equivalent to about $700 in 2020 prices) (*Los Angeles Times*, October 4, 1981).

Fine-tuning

Consistent with Heller's public image, the back cover of *Monetary Vs. Fiscal Policy* had described him as "perhaps the most effective champion of the 'new economics.'" In the late 1960s and early 1970s, other economists who were also prominent champions of Keynesianism in the United States made clear their mixed feelings about Heller's status as the leading proponent of their position. These qualms went beyond the fact, noted above, of his fairly limited recent research credentials. Rather, the basic reservations concerning Heller that were held by key Keynesians of this era like Paul Samuelson, Robert Solow, and James Tobin centered on a hyphenated word that Heller had promulgated: "fine-tuning."[272]

"Quick and fine tuning of economic policy must be the order of the year," Heller had testified to the Joint Economic Committee on February 15, 1967, in endorsing the Johnson Administration's proposal of a temporary tax increase. In the same testimony, Heller had suggested that even legislated spending commitments like Social Security benefit increases "can be an important element in the fine tuning of economic policy" if the dates at which those increases became effective were made a choice variable and the assigned dates were then based on macroeconomic-stabilization considerations.[273] Such usage on Heller's part put the term "fine-tuning" into the idiom of U.S. policy discussions. The text on the back cover of *Monetary Vs. Fiscal Policy* reflected this development. It indicated that among the features of the Friedman-Heller symposium was a "dispute [on] whether minor economic fluctuations can best be controlled by 'fine-tuning.'"

Discussing this symposium before it was published, Friedman noted: "What I argued was that what's been oversold is fine-tuning… the idea that it is possible for the few people in Washington to turn a button here, and turn a button there a little, and through delicate adjustments in taxes and in monetary policy affect the course of the economy in such a way as to keep a perfectly even keel." (Instructional Dynamics Economics Cassette Tape 3, November

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[272] With regard to another leading Keynesian of this era—Franco Modigliani: In 1997, Olivier Blanchard (by that point a longtime colleague of Modigliani's) attributed to the 1960s-era Modigliani a belief in fine-tuning. However, Blanchard did not give a specific reference in which Modigliani used the term. See Blanchard (1997, p. 553).
[273] From Heller's testimony of February 15, 1967, in Joint Economic Committee (1967, p. 499).

1968.)[274] A few weeks later, Friedman highlighted what he perceived as likely to be one of the virtues of the economic policy of the incoming Nixon administration: “You will see less emphasis on fine-tuning.” (Instructional Dynamics Economics Cassette Tape 6, December 1968.)

By this stage—late 1968—key figures in the Federal Reserve, even though they were far more sympathetic to the Heller side of the Friedman-Heller divide on economic management, had signaled their own discomfort with Heller’s “fine-tuning” terminology. Federal Reserve Board Governor Sherman Maisel had used a speech in May 1967 to defend activist monetary policy, while acknowledging the possibility that “our goals are set so high as to require too-fine tuning.”[275] Early the following year, Daniel Brill, a senior member of the economist staff of the Federal Reserve Board, delivered public remarks that were highly critical of Friedman and that defended U.S. economic policy against “anti-Keynesians.”[276] But Brill, though he was speaking on the Keynesian side of a debate titled Can the Government ‘Fine-Tune’ the Economy?,” displayed a clear awareness of the dangers involved in attaching the “fine-tuning” label to the practice of policymaking: “ ‘Fine-tuning’… implies the Government twiddling dials to offset every minor tendency of the economic picture to [deviate from]… full employment and price stability.”[277]

Apparently oblivious to these negative connotations associated with the term, President Nixon briefly embraced it. In the president’s first official press conference, given soon after he took office in January 1969, he indicated that there would be “some fine-tuning.” Perhaps not surprisingly, Senator Edward Kennedy reacted favorably to Nixon’s press conference, which echoed terminology that had become associated with his brother John’s administration. Senator Kennedy remarked that the president “did very well” in his press appearance (*Daily News* (New York), January 28, 1969).

But other remarks in Nixon’s media conference, and subsequent statements emanating from the administration, confirmed the change in direction of the kind applauded by Friedman: a stress on a gradualist and demand-oriented approach to disinflation, together with a reduction in

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[274] See also Friedman’s remarks in Friedman and Heller (1969, pp. 47−50).
[275] Maisel (1967a, p. 12; 1967b, p. 22).
[276] The quotation is from Brill (1968, p. 156). It appeared in the course of a lengthy criticism (Brill, 1968, pp. 156–160) of Friedman’s empirical findings and policy prescriptions.
[277] Brill (1968, p. 156).

economic-policy activism.[278] Consistent with the tone set by these messages, the *Chicago Tribune*'s William Clark remarked to Friedman a few months later: "it was my impression, perhaps erroneous, that the term 'fine-tuning' became a sort of a discredited term." Friedman agreed with this observation (Instructional Dynamics Economics Cassette Tape 20, April 1969). And when he appeared alongside Friedman in a May 1969 public event at MIT, Paul Samuelson indicated that his defense of Keynesianism did not extend to embracing the "fine-tuning" terminology: it was, Samuelson remarked, an unfortunate phrase coined by Walter Heller (*The Great Economics Debate*, WGBH Boston, May 22, 1969).[279] By this stage, Heller;s former subordinate Arthur Okun had already remarked, in a speech given in February (Okun, 1969, p. 184): "the concept of 'fine-tuning' has become so mired in semantic confusion that I propose we all vow to ban it from our vocabulary. In any case, I have never liked the term because of its pretentiousness."

Even Heller seemingly recanted the term. In the November 1968 debate, he had expressed concern that employing the term "fine-tuning" might have been one of his "gaffes," while also offering some defense of the formulation, provided that it was not construed as "constant fiddling with the fiscal-monetary dials."[280] In the early 1970s, Heller went further. Friedman had already observed: "The administration correctly decided it was not going to follow a policy of fine-tuning." (*Washington Post*, June 2, 1970.) Heller seemingly concurred, remarking: "The Nixon's administration's aversion to 'fine-tuning' is one we can all share." (*Sioux City Journal* (Iowa), January 4, 1971.)

In 1972, as in the late 1960s, both Samuelson and Sherman Maisel continued to distance themselves from the fine-tuning terminology associated with Keynesian policies. Governor

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[278] Accurately reflecting the mixture of messages given by Nixon in his news conference, one report on it was headlined "Nixon Wants Fine Tuning'; Guidelines [Are] Out As Economic Curbs." See *Capital Times* (Wisconsin), January 28, 1969. Similarly, Heller remembered Nixon's first press conference for its repudiation of wage-price guidelines as an approach to inflation control (*Detroit Free Press*, January 17, 1971).

[279] In a *Newsweek* column of June 23, 1969, Samuelson was less clear, stating, "It is true that the Fed has caused money to grow at but a 3 percent annual rate since December [1968], but this 'fine-tuning' has been *against* the advice of the Nixon ideologues." This sentence should be read in the context of an earlier Friedman audio commentary, which Samuelson would have heard, in which Friedman remarked: "Fine-tuning is discredited, and I don't believe in fine-tuning, in the sense of adjusting delicately to every up and down in the economic situation. On the other hand, you right now are confronted with a situation in which, by trying too much fine-tuning, you got yourself well off the track." (Instructional Dynamics Economics Cassette Tape 20, April 1969.) Once Samuelson's observation is read in light of this Friedman comment, it appears appropriate to conclude that Samuelson was putting "fine-tuning" in quotation marks in his column as a way of attributing the label to Friedman (and to likeminded figures in the administration) who believed that the Federal Reserve had unintentionally tightened by an excessive amount and that this overreaction was a typical product of attempts to fine-tune economic policy adjustments.

[280] See Heller's remarks in Friedman and Heller (1969, p. 34).

Maisel, in a speech given in April 1972, observed that, although there was a desire "to operate at levels of output and employment that require fine-tuning," it was the case that policymakers "lack the requisite knowledge to do so."[281] Samuelson, in testimony given in late July 1972 to the Joint Economic Committee, remarked of his own policy recommendations: "No esoteric 'fine-tuning' is required."[282]

In the setting of U.S. economic policy, however, the ground had shifted between 1969 and 1972—and back toward the Keynesian approach that Maisel and Samuelson had propounded during the 1960s. The Nixon Administration had abandoned its gradualist restriction of aggregate demand and embraced a combination of wage-price controls and demand stimulation, while the Federal Reserve, too, had moved into a posture of considerable ease.[283] Consequently, even though the "fine-tuning" terminology had not made a comeback, the policies associated with that term had returned in earnest. Reflecting this new atmosphere, both Maisel and Samuelson coupled their qualms about the "fine-tuning" concept with enthusiastic endorsements of nonmonetarist policy prescriptions. Specifically, Maisel indicated that part of his caution about fine-tuning was that he felt it presupposed demand policies could deliver price stability: "In the critical sphere of inflation, we don't know how control can be accomplished successfully through monetary and fiscal policy."[284]

For his part, Samuelson, who was giving his July 1972 testimony alongside Walter Heller, combined his reluctance to be associated with the notion of fine-tuning with a call to revive the "lean against the wind philosophy" of the Kennedy years and step up the expansionary monetary and fiscal moves already underway. He underlined the fact that his disagreement with Heller was on terminology, not on prescriptions, by adding that if this philosophy "is to be dubbed

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[281] Maisel (1972, p. 2).

[282] From Samuelson's testimony of July 27, 1972, in Joint Economic Committee (1972a, p. 154). Later, in 1974, Samuelson observed that the "fine-tuning" term implies "a pretentious claim to accuracy which no informed modern economist ought to make" (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 147, February 14, 1974). He also stated that the talks published as Heller (1966), although "very good lectures, very witty, very insightful," also amounted to the "high-water mark of complacency" on the part of U.S. Keynesianism (Instructional Dynamics Economics Cassette (Paul Samuelson series), Tape 155, June 10, 1974).

[283] As stressed in the previous volume and in the discussion below of Heller's views of inflation, one difference between the policy framework prevailing in 1972 from that in the 1960s was the enhanced emphasis on cost-push forces as a source of inflation. Policymakers now took inflation, when the economy was below full employment, as basically fully cost-push in character, whereas in 1961−1969 a partial cost-push view of inflation had been more prevalent in policy circles.

[284] Again, see Maisel (1972, p. 2). Similarly, in his 1971 commentary Walter Heller had immediately followed his disavowal of "fine-tuning" with a call for the Nixon Administration to "take concrete steps to restrain cost-push inflation" by making interventions in the wage- and price-setting process (*Sioux City Journal* (Iowa), January 4, 1971).

'fine-tuning,' then we should all try to be worthy of the title."[285]

In the same hearing, Heller, like Samuelson, expressed the judgment that demand stimulation had not gone far enough to date. In this connection, Heller's testimony contended: "The greatest single threat to the five million unemployed in the United States today is the mistaken belief in high places, like the White House, the Treasury, and the Federal Reserve, that after only nine months of up-to-snuff recovery—following three long years of economic slowdown, recession, and sluggishness—the limits to U.S. economic expansion are not far off." [286]

Roughly a year later, however, it was clear that the U.S. economy was overheated or was on the brink of becoming so. As has been emphasized in Chapter 2, in the 1973−1974 period neither policymakers nor the balance of opinion among U.S. economists by any means fully accepted the monetarist diagnosis regarding why the economic situation deteriorated so much in those years. In particular, it would take several more years for the critique of 1971−1972 policies offered by Friedman—an account stressing the monetary origins of the breakout in inflation, the problems associated with policies that were strongly geared to closing measures of economic slack, and heavy (and misguided) reliance by officials on nonmonetary tools against inflation—to become more widely and fully accepted. But by the end of 1973, it was common ground among economists that the settings of U.S. aggregate demand policy had been too loose in the previous year. Even Heller acknowledged at this point: "Demand-pull was upon us faster than we figured."[287]

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[285] From Samuelson's testimony of July 27, 1972, in Joint Economic Committee (1972a, p. 154).
[286] From Heller's testimony of July 27, 1972, in Joint Economic Committee (1972a, p. 142).
[287] *Wall Street Journal*, November 26, 1973 (reprinted in Heller, 1976a, p. 45). Keynesian analysis in the 1970s emphasized output growth in excess of potential output growth as a source of inflation, via a bottleneck channel—see the discussion of Arthur Okun in the next chapter, as well as Heller's endorsements of this channel in *Boston Globe*, February 2, 1971, in Heller and Perry (1975, p. 225), and *New York Times*, March 12, 1978. Therefore, Heller's acknowledgment that demand-pull forces were present in 1973 did not necessarily imply that he believed that aggregate U.S. output had actually exceeded its potential level in that year. However, the *Wall Street Journal* op-ed in question did seem to suggest that such an overshoot had occurred, as Heller stated in it that present economic conditions featured "excess demand" (see Heller, 1976a, p. 26). This op-ed also contained the important acknowledgment, "Indexes of industrial capacity (now revised) turned out to be deceptive," and the injunction to exercise caution with regard to the use of labor-market-based measures of demand pressure (see Heller, 1976a, pp. 25, 26). Nevertheless, like many other analysts, Heller during this period used a too-low estimate of the full-employment unemployment rate: he implied that this rate was by 1973 likely a little above 4.6 percent. (See Heller, 1976a, p. 66. In addition, Heller and Perry, 1975, p. 223, defined "reasonably full employment" as "say, 5 percent unemployment," and also suggested—see their p. 222—that the U.S. economy had been at full employment in early 1973, when the unemployment rate had been about 4.9 percent.) This stand represented a change from Heller's 1972 insistence on a 4 percent full-employment unemployment rate target (see his aforementioned July 1972 testimony in Joint Economic Committee, 1972a, pp. 142−143). However, his later writings would grant that the U.S. full-employment unemployment rate had risen much more since the 1960s—to about 6 or 6.25 percent (see, for example, *Wall Street Journal*, March 23, 1983)—than he had suggested in the mid-1970s period.

With activist policy again perceived as having gone too far, renewed deprecation of the "fine-tuning" terminology pervaded economic discussion in the mid-1970s. In 1974, a letter in the *Wall Street Journal* criticized a recent op-ed by Heller that had appeared in that newspaper and concluded, "attempts at fine-tuning our economy will continue to be a resounding failure."[288] In conference remarks the following year, Allan Meltzer had occasion to refer to "what used to be known in the United States as fine-tuning."[289] Friedman himself noted in mid-1978: "Government officials go out of their way to deny that they are engaged in fine-tuning—a term initially introduced by its practitioners in self-praise[,] but which has become almost a term of opprobrium."[290] By the late 1970s, he was also deeply aware that Heller had shunned the "fine-tuning" term he once promoted.[291]

The New Years Day 1975 NBC television special, referred to earlier, was presented by correspondent Edwin Newman. Before moderating the Friedman-Heller studio debate, Newman remarked: "It wasn't very long ago that we were hearing a fair amount about economists 'fine-tuning' the economy. A certain arrogance was involved in suggesting that that could be done, but, more important, it implied that things would remain on a more or less even keel with only minor adjustments—fine-tuning—necessary. It has not worked out that way." (*NBC News Special: If You Think It Was Tough To Make Ends Meet in 1974, Wait 'Till You Hear About 1975*, NBC, January 1, 1975.)

Newman's remarks provided the key to why, despite the disillusionment voiced in the United States during the first half of the 1970s about "fine-tuning"—and notwithstanding what Friedman, in late 1976, saw as the recent "conversion of the profession to the idea that you cannot fine-tune" (*The Money Programme*, BBC2, December 10, 1976, pp. 11–12 of transcript)—U.S. economic policy actually produced a *more* excessive expansion of aggregate demand over the second half of the 1970s than that generated during most of the 1961–1974 period. To advocates of expansionary policy, what the U.S. economy needed after the 1973–1975 recession were not the "minor adjustments" that Newman associated with fine-tuning, but *major* stimulation, to close a large negative output gap. It was this logic that led a 1977 document prepared for the OECD—the McCracken Report—to suggest that its recommendation of stimulating aggregate demand in the United States and Europe did not

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[288] *Wall Street Journal*, March 15, 1974.
[289] In Meltzer (1975, p. 51).
[290] Friedman (1978c, p. R–182).
[291] For example, in *The Sun* (Baltimore), August 16, 1979, p. A17, Friedman observed that the policies Heller advocated consisted of "what he would object to me calling, but what I would call, 'fine-tuning.'"

amount to "bringing back 'fine-tuning' by the side door." Such criticism, the report claimed, "overlooks the fact that the world is at present nowhere near the narrow band close to full employment to which that concept originally applied."[292]

In retrospect, the McCracken Report's prescription was closer to the policies associated with the previous decade than it contended: it was a strategy heavily reliant on estimates of resource gaps, and as things turned out, these gap estimates exaggerated the amount of slack in both the U.S. and European economies. The criticisms of the Report articulated by Laidler (1978b) to the effect that its estimates of economic slack were implausible, when judged through the prism of a monetary view of recent years' inflation, and by Lucas (1979, p. 166) that the report's criticism of fine-tuning rang hollow, "given its advice," proved to be accurate.

Lucas (1979) also saw the McCracken Report of 1977 as in effect a successor to Heller's *New Dimensions of Political Economy* of 1966, despite the "monetarist-sounding phrases" in the 1977 document. This was certainly a justified comparison. In particular, both Heller (1966) and McCracken and others (1977) placed strong emphasis on orienting policy responses toward estimates of the output gap.[293] The report's policy recommendations also had strong parallels to those being advanced by Heller and other leading U.S. Keynesians in the 1974–1977 period.

This situation cast into a different light Blinder and Solow's (1974, p. 63) statement: "monetarists are not the only economists who are skeptical of fine-tuning. Both Walter Heller and Arthur Okun… have explicitly disavowed fine-tuning."[294] Because fine-tuning was most immediately associated with adjustments in the vicinity of full-employment goals, aversion to fine-tuning did not prevent leading Keynesians from advocating strong stimulation of aggregate demand when employment was perceived as substantially below its full-employment level—both in 1970–1971 and again from 1974 to 1977. As documented in Orphanides (2003, 2004), the amount of economic slack was severely overestimated in both periods. Leading Keynesians' policy prescriptions were, partly for this reason, even more expansionary than the policies actually followed in these years. This was true of the recommendations made by such notable figures as Arthur Okun, Franco Modigliani, and James Tobin.[295]

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[292] McCracken and others (1977, p. 191).
[293] Heller (1966, p. 1) had, in fact, opened with a discussion of the "huge production gap" prevailing in early 1961 and provided a dollar estimate of that shortfall of actual output below potential output. The McCracken Report's provision of numerical estimates of the output gap for the United States and other regions was stressed by Laidler (1978b).
[294] As documentation, the authors cited Okun (1970a, pp. 110–111) but no piece of Heller's writings. As discussed above, however, Heller had indeed publicly eschewed the use of the "fine-tuning" term.
[295] For documentation and discussion, see the next chapter as well as Chapter 8.

Walter Heller, too, was, as documented below, a strong proponent of aggressive expansion of aggregate demand during the 1974–1977 period. When, subsequently, the U.S. economy entered a new period of overheating and double-digit inflation in 1979, Friedman viewed Heller as being partly to blame: "He did a very good job in miseducating the public. He taught the public that it was possible for Washington, through manipulations of monetary and fiscal policy, to maintain the economy on an effective keel." (*The Sun* (Baltimore), August 16, 1979, p. A19.)

Even those who did not agree with Friedman on how the U.S. economy had arrived at its 1979–1980 stage of renewed double-digit inflation were aware that the term "fine-tuning" now had worse connotations than ever. In a joint appearance with Friedman in 1980, Paul Samuelson remarked: "fine-tuning is an epithet, so my answer is that I abjure fine-tuning."[296] James Tobin noted that fiscal and monetary activism had been "[d]erided as 'fine-tuning' and blamed for the ills of the 1970s" (*The Economist* (London), April 27, 1985, p. 26). By this point, the negative judgment that Tobin described had become prevalent in official circles. A quarter-century after Heller had assumed the post, the position of chair of the Council of Economic Advisers was held by Beryl Sprinkel, who had received a Ph.D. under Friedman's supervision and who had later been a sparring partner of Heller's when they were both economic commentators for *Time* magazine. In January 1986, Sprinkel testified: "I think we have learned that fine-tuning of monetary or, for that matter, fiscal policy should be avoided…"[297]

Heller's position on monetary policy and aggregate demand

In a 1986 piece, Friedman recalled that prior decades had seen him in combat with "Keynesians who regarded fiscal policy as the key to managing the economy, and monetary policy as a distinctly secondary matter" (*Wall Street Journal*, September 18, 1986). In the same article, Friedman referred to the 1968 New York University debate—thereby making it clear that he regarded Heller as one of the Keynesians who ranked monetary policy lower in importance than fiscal policy. The 1968 debate had, to some extent, borne this characterization out. This was the case particularly because of the confidence that Heller had expressed in the view that the Kennedy-Johnson tax cut was responsible for the step-up in economic expansion during the mid-1960s, that its influence owed only secondarily to accompanying monetary policy measures, and that it was not realistic to expect monetary policy actions, of a realistic scale, to have been able to

[296] From Samuelson's remarks in Friedman and Samuelson (1980, p. 27).
[297] From Sprinkel's testimony of January 16, 1986, in Joint Economic Committee (1986, p. 270). The specific reason that Sprinkel cited for this conclusion was that "policymaking lags are likely to induce unintended, destabilizing effects on the economy." For more on the destabilization argument—which was due to Friedman—see Nelson (2020a, Chapter 8) and Chapter 6 below.

generate such a speeding-up of the economy.[298]

At the same time, however, Heller's pronouncements on monetary policy by the early 1960s did conform to James Tobin's generalization (*The Economist* (London), April 27, 1985, p. 23): "Mainstream American Keynesians did not dispute Friedman's contention that 'money matters.'" Certainly, Heller very likely rated monetary policy as an influence on spending to a lesser degree than Tobin did. But the policy actions Heller advocated and facilitated during his period heading the CEA were clearly premised on the notion that monetary policy actions had material effects on aggregate demand. The Kennedy Administration's first annual economic report had listed "control of the supply of money and credit" among the "weapons of stabilization policy."[299] In this connection, as part of their expansionary initiatives, the Kennedy administration and the first Johnson administration were concerned with achieving values of longer-term interest rates that would stimulate business investment spending.[300] Furthermore, in his debate with Friedman, Heller also indicated that he did not subscribe to what he called the "pure" older Keynesian view that the numerical value of the fiscal multiplier was invariant to the path that monetary policy followed when a fiscal action was taken.[301]

In fact, with regard to the effects of monetary policy on spending, it is possible to discern a gravitation over time on Heller's part toward a position somewhat closer to that of Friedman. The specific area in which this shift is discernible pertains to Heller's perspective on the consumption channel of monetary policy. Heller was, and remained, a strong subscriber to the idea that fiscal policy had powerful effects on household spending: what he called "the most

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[298] See Heller's remarks in Friedman and Heller (1969, pp. 33, 67−68).
[299] Council of Economic Advisers (1962, p. 142), also quoted in Heller (1966, pp. 64−65). This quotation and the discussion below confirm Meltzer's (2009a, p. 284) contention that "unlike many earlier Keynesian economists, [Heller] did not dismiss monetary policy." Meltzer further suggested that Friedman thought otherwise regarding Heller. The Friedman statement Meltzer quoted in this connection (which appeared in Friedman and Heller, 1969, p. 49) was, however, somewhat ambiguous, as it specifically concerned Heller's prior views on "money." The Friedman statement may have been referring primarily to the generally low confidence that Heller had in the quantity of money or monetary growth as measures of monetary policy stance—and may not have been intended to raise questions about whether Heller was long on record in believing that monetary policy was a systematic influence on aggregate spending. (As already noted, Friedman *did* seem to believe, and reasonably so, that Heller viewed monetary policy as secondary in importance to fiscal policy in its influence on aggregate demand.)
[300] This position was also linked to a belief that Federal Reserve actions on short-term interest rates mattered for aggregate demand, largely via their implications for longer-term interest rates. The administration was, in part, concerned with keeping longer-term rates down while short-term rates rose: this was the Operation Twist policy emphasized early in the administration. However, even this policy implied a belief that, *ceteris paribus*, short-term interest rate movements would tend to move longer-term interest rates in the same direction. And when the 1964 tax cut was implemented, the administration explicitly encouraged monetary accommodation of the deficit. This was intended to forestall increases in interest rates at both the short-term and long-term maturities. See Okun (1968) and Heller's accounts in Heller (1966, p. 68) and in Friedman and Heller (1969, p. 33).
[301] See Heller's remarks in Friedman and Heller (1969, p. 68).

directly and immediately responsive spending sector in any fiscal policy model."[302] But he became, over time, more willing to contemplate a monetary policy impact on consumers' purchases—an impact that resulted from adjustments that households made, in response to interest-rate developments, to their spending flows. A belief in such a channel implied a broadened view of monetary policy's effects on consumption. This was because it was distinct from a response of consumer spending that might arise when interest rates bore on investment spending and then generated a consumption response via an investment-spending multiplier.

The change on Heller's part can be deduced by comparing his later views with advice he gave to the German government during the early life of the Federal Republic of Germany.[303] Friedman and Heller both had spells in Continental Europe in the early 1950s, each serving as visiting advisers under the auspices of the Marshall Plan. For Friedman, the most famous output of this advice was what he later redrafted into his 1953 article on floating exchange rates. In Heller's case, the main written vehicle for his policy advice was a restricted-circulation document that he, Alvin Hansen, Richard Musgrave, and others wrote titled *Fiscal Problems of Germany*. This document conceded some interest sensitivity in the case of investment spending, but very little for consumers' expenditures: "a rate of interest high enough to stimulate any large volume of personal savings would seriously curtail investment."[304] In contrast, Friedman was—even by the late 1940s—pointing to an appreciable interest elasticity of consumption (see Nelson, 2020a, Chapter 5).

Walter Heller's convergence on this subject was borne out by Congressional testimony he gave in February 1986, in which he stated that "the real rate of return" mattered for "savings and investment by Americans." This, Heller noted, was a matter on which "economists of a[s] different a stripe as Milton Friedman and myself could agree."[305]

Heller on inflation in the 1970s and incomes policy

In sharp contrast, no convergence of Friedman's and Heller's views occurred in another area—

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[302] See his remarks in Friedman and Heller (1969, p. 68).
[303] This followed an earlier spell in Germany, during which Heller had been chief of international finance in the U.S. military government. He served in this role in 1947−1948 (Europa Publications Limited, 1986, p. 674).
[304] See Economic Cooperation Administration (1951, p. 58); also quoted in Fertig (1961, p. 48) and in *Chicago Tribune*, April 7, 1961. This report was formally declassified by the U.S. government in April 1961 (Fertig, 1961, p. 48). Though written in the summer of 1951, the report mainly subscribed to the cheap-money doctrine that had governed U.S. monetary policy through April 1951, with the authors suggesting that there was "no prospect" of a return in any country of "a revival of the nineteenth-century role of the capital market." (Economic Cooperation Administration, 1951, p. 54. The passage was also invoked, albeit with slight misquotation, in Fertig, 1961, p. 49.)
[305] From Heller's testimony of February 5, 1986, in Committee on Finance, U.S. Senate (1986, p. 108).

the analysis and control of inflation.

The heart of their differences on this matter lay in the fact that, despite Heller's belief in the effectiveness of monetary policy, he perceived very little role for monetary measures in either producing or addressing the U.S. inflation problem of the 1970s—particularly after the 1973 economic boom had collapsed. Heller's rejection of a monetary policy-based approach to curing inflation arose from his conclusion—one common to so many analysts during this period—that cost-push forces had come to dominate U.S. inflation patterns. "In other words," Heller remarked in an op-ed during this time, "inflation in 1974 has a life of its own, nourished not by excess demand but mainly by a variety of cost factors beyond the reach of fiscal and monetary management." (*Wall Street Journal*, March 11, 1974, reprinted in Heller, 1976a, p. 51.)

In the 1960s, Heller and his CEA colleagues had endorsed a permanently downward-sloping Phillips curve as a description of U.S. unemployment/inflation relations.[306] According to this perspective, output gaps (including negative output gaps) did matter for the behavior of inflation, while the attainment of the full-employment unemployment rate was incompatible with price stability unless inherent upward pressure on wages and prices—resulting from cost-push forces—was curbed by direct public-sector interventions into the private sector's wage- and price-setting arrangements. The Kennedy Administration took this perspective when it set out the means that it would use to reach its inflation objective, which was of a little over 1 percent. This objective was, in fact, basically achieved during Heller's tenure. In his retrospectives on this successful outcome, Heller repeatedly argued that the administration's wage-price guideposts (introduced in 1962) had played a significant role in reconciling the move toward full employment with the maintenance of rough price stability.[307]

The principal change that occurred during the early 1970s to Heller's views on inflation,

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[306] For Heller's endorsement of the Phillips-curve tradeoff perspective, see Heller (1966, p. 5). Consistent with this perspective, Heller recalled the 1960s as a period in which economists "had to accept the proposition that the two [objectives of full employment and price stability] are inherently incompatible," in the absence of "formulas" such as wage-price guideposts that could "reconcile these two objectives" (*Detroit Free Press*, May 26, 1974). An early statement by Heller along these lines was the observation in Economic Cooperation Administration (1951, p. 32) that "excessive concern for price stability" in the formulation of aggregate demand policies could hinder the development of "an adequate program of expansion designed to increase total output and employment."
[307] On the numerical inflation objective, see Heller (1982, p. 287). In *The Sun* (Baltimore), August 16, 1979, p. A19, Heller stated: "From 1960 to 1964, inflation averaged 1.2 percent a year… We had the best combination of expansion and price stability in the postwar years. That's because our policy followed two tracks. One track was the expansion of demand. The other was the wage and price guideposts…" For other affirmations on Heller's part that guidelines made inflation lower than otherwise, see Heller (1966, pp. 44–46; 1982, p. 288). See also Okun (1972c, p. xvii) for a discussion and endorsement of Heller's interpretation.

compared with those to which he had subscribed in his CEA days, consisted of his conversion—referred to above—to a harder-line cost-push position. Unlike Friedman—but like so many other economists, commentators, and policy officials in the 1970s—Heller concluded that inflation in the United States had become unresponsive to economic slack. Along these lines, he proclaimed in late 1973 that "the '70s have been a period of rude awakening for inflation forecasters. Defying economic slowdown and slack in 1969–71, inflation took off in a self-propelling spiral."[308] The following year, Heller contended that on the basis of "1969–70 and those successive 'game plans' that never worked… we found that that kind of inflation, a price-wage spiral, is highly resistant to further turns of the monetary and budget screws."[309]

In later years, the inflation developments that had occurred in the early 1970s would be widely accepted as actually being understandable through the framework of a Phillips curve that shifted position in the short run but was vertical in the long run. Heller did not embrace this interpretation. Although Heller did grant that the "state of expectations" mattered for the rate of price increases (*Wall Street Journal*, November 14, 1973, reprinted in Heller, 1976a, p. 26), he rejected the natural-rate-reverting version of the Phillips curve. Instead, in order to account for the stagflation patterns that had emerged in the U.S. data, he relied heavily on cost-push-based explanations.[310]

Heller's cost-push vision of inflationary forces further motivated him to make, during 1973 and 1974, repeated—but unrealized—predictions that nominal wage growth would take off. In January 1973, perceiving the Nixon controls as having held back cost-push pressures in the labor market, Heller saw coming employer/employee negotiations as likely to see a surge in nominal wages, driven by influential unions at "the top of the labor batting order" (*Detroit Free Press*, January 21, 1973). When, instead, it was commodity prices that commanded attention during 1973, Heller saw those price rises as likely to presage a new, and higher, level of inflation via a wage response: "get-ahead price increases and catch-up wage increases are translating a lot of the one-shot food-fuel-commodity [price] inflation into a new price-wage spiral."[311] It was necessary, Heller suggested, for the federal government to create an "atmosphere in which an economic detente between business and labor" was possible.[312] His contention was that, unless the authorities interposed themselves in the process of wage- and price-setting, "the outlook for

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[308] *Wall Street Journal*, November 14, 1973 (reprinted in Heller, 1976a, p. 25).
[309] From Heller's testimony of August 21, 1974, in Committee on the Budget, U.S. Senate (1974, p. 247).
[310] On Heller's rejection of the natural rate hypothesis, see Nelson (2020b, Chapter 13). See also his remark in *Wall Street Journal*, September 12, 1972, on "how much inflation one is willing to trade off for lower unemployment."
[311] From Heller's testimony of August 1, 1974, in Joint Economic Committee (1974b, p. 149).
[312] From Heller's testimony of August 1, 1974, in Joint Economic Committee (1974b, p. 147).

Percent

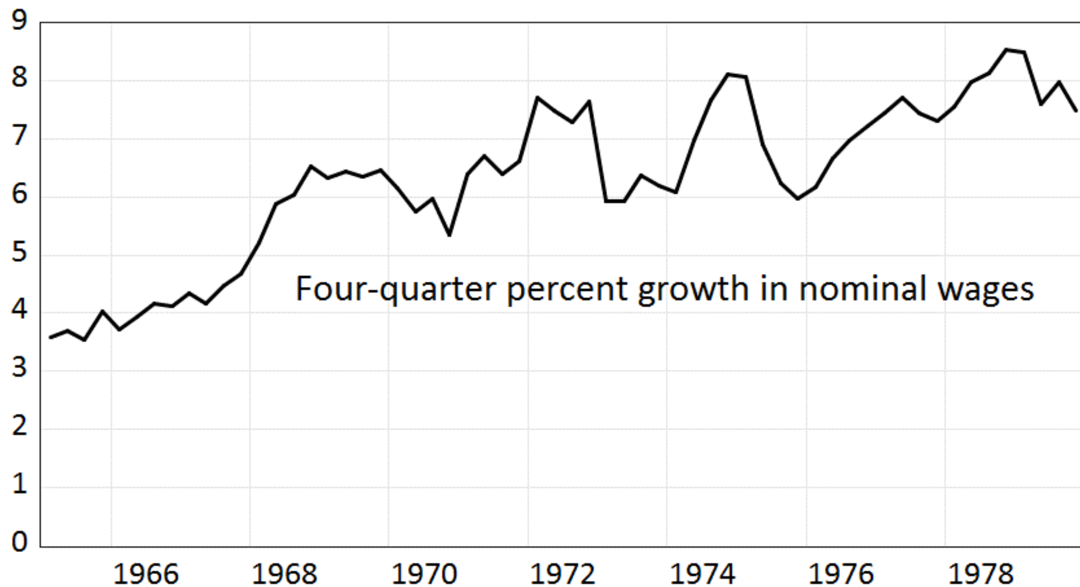

Figure 3. U.S. nominal wages, four-quarter percentage change, 1964:Q1–1979:Q4.
Source: Computed using quarterly averages of series AHETPI (average hourly earnings of production and nonsupervisory employees—total private—dollars per hour, monthly, seasonally adjusted). Available in the Federal Reserve Bank of St. Louis' FRED portal.

inserting a circuit-breaker in the new round of cost-push inflation will remain bleak."[313]

Friedman's contrary position implied, of course, that there was an *automatic* circuit-breaker that broke the link between cost-push shocks and inflation: lack of monetary accommodation.[314] The 1973–1974 inflation breakout likely reflected the prior period of monetary ease, but it did not itself give rise to monetary accommodation and, in fact, was associated with a U.S. monetary policy tightening. The economic slack that resulted from this tightening likely helped ensure that in early 1975 U.S. inflation moved off its recent peaks and that it then generally declined through late 1976.[315] U.S. nominal wage growth also remained in well inside single digits: see Figure 3.

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[313] *Wall Street Journal*, August 12, 1974 (reprinted in Heller, 1976a, p. 72).
[314] See the previous two chapters for a discussion of Friedman's application of this perspective on inflation in the 1973–1974 period.
[315] Heller, when discussing Keynesian accounts of the inflation process that attributed more of a role to economic slack than he himself did, *did* showcase what turned out to be a good quantitative prediction when he suggested that Otto Eckstein (of the Data Resources, Inc., econometric forecasting group) had suggested that a minimum of two years of 8 percent unemployment would bring U.S. inflation down to about 4 percent. (See Heller's remarks of August 1, 1974, in Joint Economic Committee, 1974b, p. 147, and his *Wall Street Journal* op-ed of August 12, 1974. See also Heller, 1976a, p. 69.) A version of Eckstein's scenario (that is, about two years of around 8 percent

In contrast, therefore, to the picture Heller had painted in August 1974 of the "self-propelled nature of the renewed price-wage spiral," the episode suggested that inflation did not appear to possess a long-lasting and sustained propulsion, provided a restrictive monetary policy was in place.[316] Heller's declaration, in his White House appearance alongside Friedman in September 1974, that "we have to recognize the chilling fact that the price explosions of '73–'74 are now being converted into a self-propelling price-wage spiral," had proved premature.[317] Indeed, by early 1975, Heller himself was conceding that "the shock waves of the 1973–74 inflation are subsiding" (Heller and Perry, 1975, p. 222).

Heller did not, however, see monetary developments as being central in producing this outcome. He repeatedly rejected monetary restriction as a means of fighting inflation, on the grounds both of its adverse effects on output and of its alleged ineffectiveness in containing wage and price pressures.[318] As in the 1960s, Heller in the 1970s remained, as already implied, an enthusiastic proponent of incomes policy. In addition, and in contrast to the 1960s, he was an outspoken advocate of the *strictest* type of incomes policy: compulsory wage and price controls.[319] For example, in April 1973, following the loosening of wage and price controls that President Nixon had implemented through the administration's Phase 3, Heller called for a restoration of the Phase 2 wage and price controls, as well as for a freeze to be imposed on food prices (*Daily News* (New York), April 28, 1973). A year later, with the controls about to lapse, Heller penned an abrasive op-ed titled "The Untimely Flight from Controls" urging that "government needs to assert its presence in wage-price developments" and that controls should continue to have a "statutory base" for doing so (*Wall Street Journal*, April 15, 1974).

Friedman indicated he was not surprised by Heller's continuing advocacy of controls. If Heller did not believe that inflation would abate in response to market forces, then, other than controls, "what else can he cling to?" The Nixon controls episode, Friedman remarked, was just one of many historical examples indicating that wage and price controls did not work. Heller's advocacy of controls in its wake reinforced Friedman's belief in the dictum that the lesson of

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unemployment on average, culminating in inflation somewhat below 5 percent) roughly matched what did occur in the United States from late 1974 to late 1976. But, because the natural rate of unemployment was likely about 1.25 percentage points or more above the roughly 4.5 to 5 percent rate that Heller propounded at the time, this disinflation represented a stronger response of inflation to economic slack than Heller was implying. (In addition, in a natural-rate model, resource slack need occur only *temporarily* to deliver a permanent decline in inflation.)

[316] The Heller quotation is from his testimony of August 1, 1974, in Joint Economic Committee (1974b, p. 149).

[317] From Heller's remarks of September 5, 1974, in Council of Economic Advisers (1974, p. 75).

[318] As well as the Heller quotations to this effect noted above, see Heller's August 1, 1974, remark in Joint Economic Committee (1974b, p. 147): "The 'old-time religion' of sky-high money costs and tight budgets will be relatively ineffectual in taming inflation short of draconian budget slashes, tax boosts and dangerously tight money."

[319] He had previously been critical of the option of direct controls. See Heller (1966, pp. 46–47).

history was that people did not learn from history (Instructional Dynamics Economics Cassette Tape 147, May 30, 1974).

In August 1974, in the wake of what he called "the unfortunate discrediting of wage-price controls," Heller still perceived merit in restoring powers to regulate wages and prices. He elaborated on the vision for future controls that he had laid out in April: comprehensive wage and price control should not be reimposed, he conceded, but a "wage-price watchdog" should be created, possessing the statutory ability to impose suspension or retractions of wage or price increases that were deemed untoward (*Wall Street Journal*, August 12, 1974).[320] When the Council on Wage and Price Stability—essentially a factfinding and reporting body—was created in the post-controls period, Heller called for it to be endowed with extra teeth, including these rollback powers.[321] Heller also recommended that the U.S. government hold food prices constant through the provision of subsidies (see Council of Economic Advisers, 1974, p. 75).

In 1976, Heller reaffirmed his recommendation of a "beefed up Council on Wage and Price Stability," endowed with the compulsory powers noted above. He also urged that firms in "noncompetitive" areas of the economy be required by law to notify the council of planned price increases (Heller, 1976b, p. 59). By 1978, however, he had moved to favoring a notionally voluntary, but elaborate and wide-ranging, form of means of influencing wages and prices: the "tax-based incomes policy" (TIP). Heller endorsed TIP in a public letter, already referred to above, that was co-signed by James Tobin, Robert Solow, and the TIP's architects Arthur Okun, Sidney Weintraub, and Henry Wallich (*New York Times*, March 12, 1978). The TIP proposal and Friedman's reaction to it are covered in the discussion of Arthur Okun in Section III of the next chapter.

Conscription and the Johnson Administration's background

Heller was CEA chair for the entirety of the Kennedy Administration, and it was that administration with which he was most associated. Heller was also, however, head of the CEA for almost the first full year of the Johnson presidency. And, just as Friedman would make little distinction between the approaches of the Kennedy and Johnson administrations in his accounts of the course of developments over the 1960s, Heller would regard U.S. economic policy as

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[320] See also Heller (1976a, p. 72). Heller made the same recommendation in his testimony of August 1, 1974, in Joint Economic Committee (1974b, p. 149).
[321] See Heller's remarks in Council of Economic Advisers (1974, p. 75). For more on the Council on Wage and Price Stability, see the discussion titled "Albert Rees" in Section III of Chapter 2.

having been continuous across the two presidencies.[322] Robert Solow, himself part of the Kennedy-era CEA, also shared that basic posture: "I don't make such a sharp distinction… In economic policy, after all, it was Johnson who, on the wave of feeling about Kennedy's murder, got the Revenue Act of 1964 [that is, the 1964 tax cut] through the Congress. We were never very sure that Kennedy was going to be able to do that. So, I don't draw that sharp line [between Kennedy and Johnson]." (Robert Solow, interview, April 3, 2015.)

Developments that would occur under Johnson, especially the escalation of U.S. involvement in the Vietnam War, would cast a pall on overall recollections of the 1960s and on the totality of the record of the 1961−1969 Democratic administrations. One upshot of this was that retrospectives on the United States' economic successes of the 1960s often tended to associate these successes primarily with the more fondly-remembered Kennedy rather than with Johnson. This proved to be the case even when those retrospectives clearly encompassed developments that manifestly were events occurring during Johnson's presidency, not Kennedy's. This tendency was evident in the innumerable occasions when supply-side commentators referred to the 1964 tax reduction as the "Kennedy tax cut."[323] It was also implicit in Paul Samuelson's (1969, p. 12) reference to "February 1961 to, let's say early 1965, … that wonderful Camelot period when the GNP grew mightily." For his part, however, Heller was happy to be associated with both presidents. As of 1979, the photographs in his office included one of Heller with Johnson, as well as one of Heller and Kennedy together (*San Francisco Examiner and Chronicle*, August 26, 1979, p. 13C).

Indeed, Heller could take pride in the fact that he had stayed on for Johnson's first year in office. This was so not only on account of the impressive national economic performance observed in that year, but also because Heller's continuation in office meant that he was part of the administration (and, in effect, Democratic party presidential campaign) that promoted, saw enaction of, and sought a new mandate on the basis of, the Civil Rights Act of 1964.

In contrast, Friedman could look back on this year with no such corresponding pride. He was an economic adviser to the opposition Goldwater Republican presidential campaign, and that campaign opposed the Civil Rights Act—its grounds being the contention that the Act was not admissible federal law under the U.S. constitution.

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[322] See, for example, Heller (1966, p. 1).
[323] See, for example, Bartlett (1982, p. 284).

Friedman, as noted in Section I above, largely deferred to others when it came to judgments about constitutional and legal matters. He did, however, bring onto Goldwater's Scholars for Goldwater committee the University of Chicago law school's Robert Bork (*Washington Post*, July 27, 1987, p. A8). When it was being proposed, as well as in the immediate aftermath of its passing, Bork had criticized the Civil Rights Act on constitutional as well as libertarian grounds—thereby taking positions that, as Bork would acknowledge in 1973, meant that he had been "on the wrong tack altogether."[324]

As was acknowledged at the time and as he himself affirmed, Friedman supported the aims of the Civil Rights Act.[325] But, like Goldwater, Friedman opposed the Act at the time of its consideration and enaction. He did so primarily not on the basis of Goldwater's legal/constitutional qualms but, instead, because he believed that repeal of discriminatory state-level laws should occur through a process of changing public opinion in the individual jurisdictions.[326] Friedman's unwillingness to endorse the use of federal legislation to overturn discriminatory state laws contrasted with the Johnson Administration's forthright approach. Via the Civil Rights Act, it forced the pace of progress—implementing swiftly, and on a nationwide basis, reforms that Friedman acknowledged were desirable but on which he had opposed statutory action at the federal-government level.

In retrospectives, Friedman would grant that the civil rights legislation had been successful. In 1970, for example, he stated that "the great advances in civil rights came about by abolishing the laws which prohibited the freedom of intercourse and relations among people of whatever color."[327] This amounted to an implicit acknowledgment of the importance of the Civil Rights Act of 1964, which had included provisions that voided Jim Crow laws imposed in U.S. states. And, several months before Johnson's death, Friedman observed that "Lyndon Johnson presided over the most effective, the most far-reaching civil rights legislation of the twentieth century" (Instructional Dynamics Economics Cassette Tape 103, July 12, 1972).[328]

From late 1964 to early 1969, as a regular face in the media and a former senior official, Heller frequently found himself defending (and acting as a *de facto* spokesperson for) the Johnson

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[324] From Bork's testimony of January 17, 1973, in Committee on the Judiciary, U.S. Senate (1973b, p. 14).
[325] For example, the *Washington Post* (September 11, 1964) noted that "Friedman is strongly opposed to racial discrimination."
[326] See Friedman (1962a, p. 111) and the 1964 Friedman statements cited in Nelson (2020b, Chapter 12).
[327] From his appearance on *NET Journal: The Conservative Viewpoint*, WTTW Chicago, May 4, 1970.
[328] This is not to say that Friedman had, after the 1960s, no remaining differences with existing approaches about how best to achieve anti-discrimination goals.

Administration.[329] Inevitably, this involved addressing aspects of the Johnson record beyond economic policy and, in particular, discussing the country's increased involvement in the Vietnam War. Occasionally, Heller's coverage of this matter occurred when he was in debate with Friedman.

One such occasion occurred during Friedman and Heller's November 1968 New York University exchange on monetary versus fiscal policy. Heller, in an ill-judged attempt at humor, gratuitously invoked the Vietnam War at the end of his contribution to this symposium. He drew a parallel between the ongoing war abroad and the long-running Keynesian-monetarist debate at home, and he compared Friedman with the leader of the Viet Cong.[330]

A later discussion of the Vietnam War in the Friedman-Heller exchange occurred in January 1970, in one of their televised debates. On this occasion, the discussion of the war covered an issue on which Friedman was a critic of existing U.S. policy: conscription.[331] It was a year since the Johnson Administration had left office. It was also not long before the Nixon Administration would receive the Gates Commission's report, penned by Friedman and others, recommending an all-volunteer army. Heller, however, defended the policy of conscription that Nixon had inherited from the Johnson Administration. In doing so, Heller linked the matter to the civil rights movement. He invoked racial equality as a justification for the draft. "A completely voluntary armed force… [would] be primarily Black and poor and let the rich off the hook."[332]

Friedman replied to Heller immediately: "What you are describing is not a possibility."[333] But

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[329] He was not entirely outside officialdom by this point. In addition to more informal interactions with the Johnson economic team, and meetings with members of Congress, Heller continued to have an official, though more distant, affiliation with the administration, as a consultant to the Executive Office of the President from 1965 to 1969 (Europa Publications Limited, 1986, p. 674).

[330] See his remarks in Friedman and Heller (1969, p. 70). Earlier in this debate, Heller made a more legitimate and less jokey reference to the war when he stated that the economy was well balanced in mid-1965 "before Vietnam escalation undid us" (Friedman and Heller, 1969, p. 33). Heller was a subscriber to the standard Keynesian narrative of the decade's developments (see, for example, Okun, 1972c, p. xvii), according to which economic overheating could have been avoided if the increase in U.S. defense spending had been financed by a tax increase in 1966.

[331] This 1970 debate was one of a sequence of appearances Friedman and Heller made on national public television during this period, in specials covering the State of the Union addresses and comparable events. The first of these appearances occurred as part of a nationwide National Educational Television special, *State of the Union 1967*. This special, one of the first truly networked live news specials on U.S. public television, began with coverage of President Johnson's annual State of the Union address and was followed by discussions broadcast from various studios, with Friedman and Heller providing the economic portion of those debates. See *Austin Statesman* (Texas), January 10, 1967, *Boston Globe*, January 10, 1967, *Boston Herald*, January 10, 1967, and *Sun-News* (Las Cruces, Nevada), January 12, 1967.

[332] *State of the Union/'70*, WNET, January 22, 1970, p. 41 of transcript.

[333] *State of the Union/'70*, WNET, January 22, 1970, p. 41 of transcript.

the argument that Heller was making was the same as what had led Senator Edward Kennedy—after apparently being on Friedman's side of the issue during 1967—to become, by the end of the 1960s, a supporter of the draft. Kennedy argued that an all-volunteer army would "magnify the present economic inequality" and suggested that, in practice, such a system would largely place responsibility for the nation's defense on the shoulders of African Americans, far out of proportion to their share of the population (*Chicago Daily News*, December 29, 1969, p. 4).

The position that conscription was justified on the basis of equity had long been sufficiently prominent for Friedman to devote considerable effort to addressing it in the critiques of the draft that he produced in the 1960s (for example, *New York Times Magazine*, May 14, 1967). It was also an argument to which commentators of different political persuasions became drawn during the 1970s. Notably, the *Wall Street Journal* editorial writers had begun the decade being mildly supportive of a shift to a volunteer army.[334] In the late 1970s, however, the *Journal*'s editorial position on this matter shifted, and multiple editorials advanced the case for the restoration of the military draft in the United States.[335] The *Journal*'s grounds for this advocacy included not only the Soviet Union's military buildup, but also the point that Heller and Kennedy had invoked—that a draft ensured that the makeup of U.S. military forces was representative of national demographic patterns (*Wall Street Journal*, May 29, 1981).

In both Democratic-supporting and Republican-supporting circles, it remained the case that the pro-draft faction remained a minority after the end of conscription in 1973. The *Wall Street Journal*'s urging of the return of the draft would, however, be one of a few key issues—another being fixed versus floating exchange rates—on which Friedman would be at odds with the editorial position of that newspaper.

Heller on Friedman and Brazil

Along with the official editorials, another staple of the *Wall Street Journal* opinion page consisted of the op-ed contributions. On September 12, 1972, an op-ed written by Heller was accompanied by an explanatory box stating: "The *Wall Street Journal* is pleased to announce a new feature, the Board of Contributors, intended to present a broad range of viewpoints on current topics." Heller was a member of this new board of opinion-piece writers. He was consequently a semi-regular contributor of economic commentaries that appeared in the *Journal*

[334] See, for example, the *Wall Street Journal* editorials of January 13, 1970, and June 19, 1970.
[335] See, for example, the *Wall Street Journal* editorials of March 16, 1979, and July 23, 1980.

from 1972 until early 1986.[336] It was in this role that, in 1974, Heller would pen an op-ed that provided a line of criticism of Friedman that would turn out to be an influential for generations.

As has been noted, the spring/summer 1974 period saw a surge of interest in proposals regarding the indexation of wage contracts, government securities, and taxes. In Heller's (1976a, p. 59) assessment of a couple of years later, the "boomlet" of interest in indexation in the United States had a clear source: it was "[s]parked by Milton Friedman (after a visit to Brazil)."[337]

At this time, and during the introduction of its indexation arrangements, Brazil was governed by a repressive military dictatorship. In his *Wall Street Journal* op-ed on April 15, 1974, Heller drew a connection between Friedman's indexation advocacy and the character of the Brazilian regime. Heller suggested that Friedman's recent stand "evokes surprise, astonishment, and disbelief" as his embrace of "Brazilian indexation" represented a break, Heller alleged, from the previous Friedman belief in "Smithian *laissez-faire*." Heller's criticism of Friedman over Brazil was not identical in character to those many later leveled at Friedman over Chile—Heller was suggesting that Friedman had dropped his belief in free-market economic arrangements—and now instead favored compulsory measures like those imposed by the authoritarian Brazilian regime. In contrast, critics of Friedman in connection with Chile would instead, over his objections, make the imputation that he favored imposing free-market policies by undemocratic means. But the outline of what would become, from 1975 onward, a very prominent allegation made against Friedman by his critics—that he had associated himself with repressive Latin American governments—had been developed by Heller in this op-ed.

Heller followed up with a new *Wall Street Journal* op-ed (June 20, 1974) on Brazil's indexation experience, as well as a companion piece, written with Albert Fishlow, published by a Minneapolis-based commercial bank.[338]

Friedman, of course, responded that he did not, in fact, favor compulsory indexation, nor—as Heller had also alleged in his original op-ed in April—did his support for indexation represent an acquiescence to, or resignation concerning the inevitability of, high inflation.[339]

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[336] In Europa Publications Limited (1986, p. 674), Heller incorrectly dated his position on the Board of Contributors back to 1970, rather than to 1972. Heller's contributions in this capacity concluded with his op-ed in the *Wall Street Journal* of February 28, 1986.
[337] On this debate, see the discussion and references, including Boianovsky (2020), provided in Chapter 2 above.
[338] Heller and Fishlow (1974), cited in Gennaro (1975, p. 8).
[339] On these points, see Chapter 2 above and Nelson (2018).

Friedman recognized, however, that there was more to refute in Heller's attack. With regard to Heller's *Wall Street Journal* June op-ed and the accompanying Heller-Fishlow piece, Friedman observed, "one thing about them really does bother me very much—and this is a personal matter." Friedman pointed to the fact that the Heller analyses had presumed "that somehow the Brazil case had an important part in shifting me to be in favor of indexation" and had judged Friedman's proposal on the basis of the merits of the Brazilian indexation arrangements. Brazil's experience with indexation was indeed interesting, Friedman remarked, but his own advocacy of indexation went back "long before I ever made any trip south to Brazil. I used the Brazilian experience to illustrate the advantages of indexing, rather than as an argument—as a fundamental argument, or as a major argument, or as a primary argument—for indexing." Friedman then got to the crux of the matter: "There are many features of the Brazilian system that I would not carry over to this country. There are many features of the Brazilian economic and political arrangements that I find objectionable." Friedman stressed that he had endeavored to make this point clear in his new article, in *Fortune* (July 1974), on indexation, in which he had separated the case for indexation from the existence of the example of Brazil. Friedman also made it plain that the *Fortune* piece had been written partly in response to Heller's attack: "I've discovered from experience [that] if people cannot attack your argument at its strongest point, they will look for whatever red herring is available with which to do so." (Instructional Dynamics Economics Cassette Tape 149, June 26, 1974.)

The criticism continued to bother Friedman over the rest of 1974. "I've been preaching indexation for several years," Friedman remarked at an American Enterprise Institute panel on indexation on July 17, 1974, "but got no attention until I happened, by accident, to use the Brazilian case as an illustration. And ever since[,] I've had Brazil draped around my neck, as if I were a protector of Brazil." He went on to stress that the "authoritarian country" of Brazil had been cited by him because it provided an example of indexation operating in practice—and that democracies like Finland, the Netherlands, Canada, and Israel also offered evidence on practical experience with indexation.[340] "The moment I refer to Brazil," Friedman added in remarks given in London the following September, "I am told: 'Oh, you like a military dictatorship?' No, I do not, military dictatorships are terrible." Friedman went on to list "totalitarian Brazil" alongside other tyrannical regimes of the twentieth century.[341]

With these clarifications, Friedman evidently regarded himself as having defused the matter.

[340] American Enterprise Institute (1974, p. 35).
[341] Friedman (1974c, p. 81; p. 121 of 1991 reprint).

But, in fact, it would turn out that the practice Heller had initiated—in which Friedman's detractors would associate him with Latin American dictatorships—was only just beginning. Heller had, in a sense, vindicated his public image as Friedman's premier adversary in debate. For Heller in 1974 had set in motion a critique that, for Friedman, would be a major, and lasting, source of acrimony.

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 5: Debates on Monetary Policy and Macroeconomic Stabilization, 1975 to 1976

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September 25, 2023

I. EVENTS AND ACTIVITIES IN DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1975–1976

The behavior of U.S. macroeconomic data in the mid-1970s set the seal on what King and Watson (1994, p. 209) described as "the general increase in the difficulty of forecasting in the post-1970 period." The restoration of more placid conditions during 1972—with a shift down in inflation and unemployment rates, together with a return to positive real short-term interest rates—had proved ephemeral. It had reflected the combination of a residue of prior—but abandoned—monetary restraint, real effects of the more-recent rapid monetary expansion, and the imposition of wage-price controls.

In contrast, the years immediately following 1972 saw multiple developments tending to magnify U.S. economic fluctuations: the abolition of controls, the prior period of monetary expansion leaving aftereffects of higher inflation and interest rates, and a shift to monetary restriction that bore adversely on aggregate output and unemployment. Together with developments in monetary aggregates and in the financial system that are discussed later in this chapter, the rough-and-tumble behavior of interest rates, output, inflation, and the unemployment rate after 1972 amply reinforced Milton Friedman's observation of early 1974: "This is a very, very unusual set of circumstances that we are facing now." (Instructional Dynamics Economics Cassette Tape 139, February 4, 1974.)

These gyrations continued as the decade of the 1970s entered its second half. Against this backdrop, in 1975 a financial-advice columnist, when asked to comment on an investment that had started in 1968, remarked that seven years would normally be a sufficient time to reach a

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[1] Email: Edward.Nelson@frb.gov. The views expressed here are the author's alone and should not be interpreted as those of the Federal Reserve or the Board of Governors. The author is grateful to the interview subjects as well as George Fenton for their generosity in providing useful information, as well as for comments from participants in a seminar at the University of California, Berkeley, at which some of the material in this chapter was presented. See the Introduction in Nelson (2020a) for a full list of acknowledgments. The author regrets to note that Martin Feldstein, whose interview with the author is quoted in this chapter, passed away in June 2019.

judgment concerning the quality of an investment. But that was not the case in this instance, he said, because "this has been a 'normal' seven-year period in about the same way that the Grand Canyon is your 'normal' ditch." (*Washington Star* (Washington, D.C.), April 1, 1975.)

Casting blame for the recession

As 1975 ended its first week, financial columnist Joseph Slevin told his readers to expect that commentary on the current U.S. recession would soon include much more in the way of suggestions that the economic downturn was produced by the monetary authorities. "The alleged culprit is the Federal Reserve System's tight money policy," Slevin wrote, "and more will be heard of the charge in coming months." Slevin noted the fact that a range of critics had concluded "that the Federal Reserve has waited too long and done too little and that, thanks to the Federal Reserve credit squeeze, the ongoing recession was 'Made in Washington.'" (*American Banker*, January 7, 1975.)

Although Slevin's column did not name him, Friedman was one of the prominent critics taking this position—as would be confirmed in a string of commentaries that he would deliver in early 1975. Federal Reserve actions, he said in one of these commentaries—a February 1975 television appearance—had put the United States "into a more severe recession than we need to have."[2]

Slevin contrasted the diagnosis that Federal Reserve actions had produced the recession with the position articulated by Ford Administration personnel "that the primary cause of the recession was inflation and its slashing impact on the buying power of the public's dollars." Friedman was receptive to a version of this explanation. "The recession is a result of the inflation; we mustn't suppose they are independent phenomena," he remarked in the February 1975 television interview.[3] However, the narratives coming from officialdom gave the impression of an exogenous increase in inflation occurring in the context of broadly appropriate rises in nominal aggregate demand and being responsible for negative growth in real demand and output. In this account, therefore, stabilization policy provided about the right course for nominal demand, and it was events outside economic policy's control that produced the poor outcomes for output and inflation.[4] Friedman, in contrast, blamed policymakers for both inflation and recession.

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[2] *Wall Street Week*, Maryland Public Television, February 7, 1975, p. 15 of transcript.
[3] *Wall Street Week*, Maryland Public Television, February 7, 1975, p. 15 of transcript.
[4] This diagnosis had begun to be propounded by the Nixon Administration as the economy slowed in the first half of 1974. It lingered in former President Nixon's accounts of the period, and in 1983 he suggested that OPEC "helped

Friedman did grant that special factors like food and fuel price increases had indeed raised inflation to some degree in 1973 and 1974 (see the previous two chapters) and that these had had the negative effect on purchasing power stressed by officials. He also acknowledged that the abolition of wage and price controls in 1974 had temporarily made both inflation and output worse: the controls' removal, he noted about a decade later, "produced a rapid acceleration in inflation which was accompanied by a decline in real income."[5]

Friedman and Schwartz's 1982 *Monetary Trends* analysis suggested that the lifting of controls amplified the recession but that output would still have declined during 1973−1975 in the complete absence of this controls system. In particular, using historical short-run relations between real and nominal variables, Friedman and Schwartz transformed the recorded U.S. output patterns from 1971 to 1974 to adjusted series that could be interpreted as how the U.S. economy would have behaved if controls had never been imposed at any time, but aggregate demand policies had been the same as those historically observed. On their reckoning, controls had made the aggregate supply curve flatter than otherwise in 1972 and 1973 and steeper than otherwise in 1974, and that, under a controls-free scenario, U.S. aggregate output might have been flat, instead of declining, in 1974, but it would still have fallen in 1975.[6]

Predominantly, of course, Friedman attributed the very high inflation of 1973−1975 to prior monetary excess rather than special factors. Consequently, even when stressing the likelihood of an inflation/recession link, he was disinclined to attribute the recession primarily to exogenous events. Furthermore, Friedman did not see the rise in U.S. inflation in 1973 as making a

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to create the inflation which later led to the recession." (From Nixon's remarks in his videotaped interview with Frank Gannon, May 27, 1983, available at *https://georgiaoralhistory.libs.uga.edu/gannix/gannix_0374*.)

[5] Friedman (1985a, p. 57).

[6] See Friedman and Schwartz (1982, p. 108). This is one interpretation of their adjustment. A different one is that the Nixon controls were *purely* cosmetic and merely distorted the recorded prices and output indices until controls were removed; that is, that the 1971−1974 U.S. controls experience was well characterized by Friedman and Schwartz's (1963a, p. 558) summation of the World War II price controls, which involved suppressed increases in posted prices alongside *de facto* price adjustment "not recorded in the [official] indexes."

Passages of Friedman and Schwartz (1982)—especially p. 82, fn. 9—and their other writings seemed to apply this interpretation to other price-control periods and to the Nixon control program in particular. However, this characterization was likely dictated by a wish to provide a symmetric perspective on all the control episodes throughout the century of U.S. and U.K. data that *Monetary Trends* covered. The balance of Friedman's writings on the Nixon controls did interpret them as having had, at least through sometime in 1973, a *bona fide* effect of making the short-run supply curve for U.S. goods and services flatter than otherwise: that is, as having given rise to genuine increases in output beyond those that would otherwise have occurred, under the same setting of aggregate demand policies, and of conversely keeping, over the period of the controls being in force, actual inflation below its counterfactual, no-controls rate. That controls could have these properties under Nixon's program is consistent with controls being more cosmetic in their effect on price indices during World War II. The wartime controls were associated with rationing and restrictions on production in the nondefense sector, and so the controlled prices in that era were not prices that consumers and firms could use as the basis for their quantity decisions.

recession inevitable, in the sense that a recession was implied by the dynamics of the economy's structure. On the contrary, his long-held position was instead that inflation or inflationary booms did not themselves necessarily give rise to recessions.[7] Certainly, as the community accustomed itself to a higher rate of inflation, the initial output-boosting effects of a shift to a more inflationary policy would recede; but output need not fall (or take below-normal values) as part of this process.

In practice, however, Friedman *did* see recessions as following inflationary periods because there would be a public reaction against higher inflation and, partially recognizing that excess demand was a source of inflation, the authorities would rein in aggregate demand: "you can't keep an inflation going indefinitely; people don't want [it, so] you try to stop it," he remarked on television in February 1975."[8] It was this policy reaction—in particular, the FOMC's tightening beginning in the first half of 1973—to which Friedman attributed the recession that had begun in late 1973. As discussed in Chapter 2, he believed that the first year of the tightening was appropriate and of about the right magnitude. But he found fault with what he perceived as a too-severe further tightening in the middle quarters of 1974. This had formed part of what Friedman called "these wild swings up and down" in monetary growth (*St. Petersburg Times* (Florida), March 11, 1975, page 2–A).

In a press interview given a week after his television appearance, Friedman observed that he had "just constructed a table [with] which I am going to soon blast the Fed" in his *Newsweek* column (*Journal of Commerce*, February 25, 1975, p. 3). This table duly appeared in Friedman's bluntly-titled *Newsweek* column of March 10, 1975, "What Is the Federal Reserve Doing?"[9] After cataloguing the excessive monetary growth of 1971–1974, his column turned to the recent downturn in monetary growth. The period on which Friedman focused, July 1974 to January 1975, predated that of Congressionally-required targets for monetary growth (see the section below titled "The Beginning of Monetary Targeting"). However, the FOMC already had published targets of sorts for M1 and M2 growth, as well for the federal funds rate: "Each month it publishes its [monetary] objectives," he had noted in the February 1975 television interview.[10] Drawing these from Federal Reserve publications, Friedman's table and the accompanying discussion made a point that monetarists would articulate repeatedly in considering U.S. monetary policy in the 1970s: the Federal Reserve had systematic misses of its targets for

[7] See, for example, Friedman (1964, p. 17; 1972a, p. 936).
[8] *Wall Street Week*, Maryland Public Television, February 7, 1975, p. 16 of transcript.
[9] In a separate *Newsweek* item in the same issue (March 10, 1975*b*), Friedman was quoted as saying that the Federal Reserve's policies were "terrible" (p. 60).
[10] *Wall Street Week*, Maryland Public Television, February 7, 1975, p. 17 of transcript.

monetary growth—M2 growth, for example, was never in its official tolerance range for the six months through early 1975 in the period—yet the federal funds rate targets were either met or missed by small amounts.[11] The FOMC's success in hitting the federal funds rate target had been "at the cost of achieving its monetary growth target."

As he was preparing the column, Friedman remarked: "In essence, what the Fed is telling us is that they want to stretch out the decline in interest rates over as long a period as possible, so that a rebound in rates can be avoided. But this means they must restrict the money supply to keep rates from falling. This, in turn, means that the recession will be stretched out over a longer period, and the rate of unemployment will climb higher than if they would let the money supply grow at their target rates." (*Journal of Commerce*, February 25, 1975, p. 3.)

Friedman's *Newsweek* column also noted that the Federal Reserve had recently been downplaying its scope to influence the money stock. But he argued that these denials should be discounted; and, as discussed in Chapter 2, in early 1975 Federal Reserve Board Governor Henry Wallich had in effect conceded that the FOMC could push up monetary growth if it were willing to permit the required federal funds rate fluctuation.[12] Similarly, Chairman Burns remarked in February 1975 that the Federal Reserve could raise monetary growth and was now taking steps to do so "Forces have now been set in motion that will, I believe, soon result in a quicker pace of monetary and credit expansion."[13] Friedman, as discussed below, thought that the Federal Reserve exaggerated the amount of interest-rate variability that a more-stable pattern of monetary growth would entail. He conceded, however, that securing such a money-stock trajectory would indeed imply some additional degree of interest-rate variation in the short run.

Such variation would be desirable, Friedman maintained in early 1975, in order to pursue the open market operations necessary to get monetary growth going again. In the absence of a

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[11] Wonnacott and Wonnacott (1979, pp. 320–321) made a similar observation regarding Federal Reserve behavior in 1974–1975. For the same point applied to 1970–1979 U.S. monetary policy more generally, see Poole (1979, pp. 476–478), Pierce (1980, pp. 82–83), Meltzer (2009a, p. 591; 2009b, pp. 886, 897), and Friedman (1980a, para. 14, p. 58; p. 54 of 1991 reprint).
[12] Likewise, in Congressional testimony on March 13, 1975, Chairman Burns acknowledged that an injection of reserves under present circumstances would lead to an expansion of aggregate credit extended by commercial banks (Committee on the Budget, U.S. Senate, 1975, p. 842). He suggested that, under the circumstances then prevailing (in which commercial banks were hesitant about expanding their loan portfolios, as discussed in "The Beginning of Monetary Targeting" below), investments in securities, as distinct from loans to the private sector, would take a larger share than usual of the increase in commercial bank credit. Such an eventuality would not have prevented the money stock from expanding appreciably in response to the policy-induced increase in reserves; and, as discussed presently, Burns acknowledged this at around the same time as this testimony.
[13] Burns (1975a, p. 152). Also quoted in *Washington Star-News* (Washington, D.C.), February 25, 1975.

pickup in monetary growth, his column observed, there was a danger of "a major contraction" in output (*Newsweek*, March 10, 1975*a*).

Friedman's column did not paint a flattering picture of the Federal Reserve's leadership. But it refrained from criticizing Arthur Burns by name. In contrast, in his own *Newsweek* column a week earlier, Paul Samuelson named names. A few years later, in recalling how the "Fed's performance during the recession led to a criticisms from a wide spectrum of economists," Wonnacott and Wonnacott (1979, p. 320) noted that "Paul Samuelson wondered if the economy was headed into 'A Burns Depression?'" Samuelson's column of that title (March 3, 1975) took Burns to task for the tightness of monetary policy since mid-1974. On another occasion in early 1975, Samuelson speculated about a time when "Milton Friedman and Anna Schwartz come to write their history of the crimes of 1974 and 1975" (*New York Times*, February 26, 1975), should they write a new volume of their monetary history (which, of course, they never did; Friedman had said in 1964 that they had "no present plans for doing any further work on the qualitative historical evidence," and, in the event, they never jointly resumed such work).[14]

Apart from his association of the recession with FOMC decisions, Samuelson's basis for raising the prospect of a Burns depression was that he saw the recession as unlikely to bottom out before October/November 1975, and the likelihood that the national unemployment rate would peak above 10 percent during the recession or in its wake (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 158, late February/early March 1975). "I submit that a sensible usage would be to *define any recession that involves* [*unemployment of*] *more than 10 percent of the labor force as a depression*," Samuelson wrote (*Newsweek*, March 3, 1975, italics in original).[15]

It is most unlikely that Friedman would have concurred with this definition. He had long been doubtful of the unemployment rate as a cyclical indicator. He had been willing to predict that, in the wake of monetary restraint, 8 percent unemployment was likely to be reached in 1970—a scenario that had prompted Arthur Burns to remark, during his December 1969 confirmation hearings: "I do not believe that we are going to be so stupid or so unfortunate as to see unemployment rise to anything like 8 percent."[16] Although this rate was avoided in 1970, the U.S. unemployment rate moved above 8 percent in January 1975 and stayed above 8 percent for

[14] Friedman (1964, p. 19; p. 277 of 1969 reprint).
[15] In the event, the unemployment rate stood above 10 percent in only one episode in the 1945–2019 period: September 1982 through June 1983. It reached, but did not exceed, 10 percent for one month (October) in 2009.
[16] From Burns' testimony of December 18, 1969, in Committee on Banking and Currency, U.S. Senate (1970b, p. 9).

the rest of the year. Friedman certainly saw an important cyclical element in this elevated level of unemployment. As discussed below, however, his preexisting misgivings about unemployment statistics were reinforced in the 1975−1976 period by emerging changes in the relationship between the unemployment rate and indices of both employment and production. He was therefore not well disposed toward associating the definition of a depression with a particular unemployment-rate threshold.

But, as already indicated, in the early months of 1975 both Friedman and Samuelson feared a further decline in U.S. aggregate output over the course of the year—with Samuelson observing, "there is still nothing to be optimistic about. The economy continues to slide, and the bottom is not yet in sight."[17] Friedman remarked in his appearance at Claremont College, California, on February 21, 1975: "As for the rate of unemployment, the odds right now are better than 50−50 that it will hit 10 percent soon—in the second or third quarter of this year." He added that he saw "no reason to predict an end to the recession," as a notable monetary expansion had yet to occur, and an economic turnaround would likely not occur until two to three quarters after that event (*Journal of Commerce*, February 25, 1975, p. 1; also in *Participant*, April 1975).

Recovery begins

As things turned out, recovery began soon after these statements of concern were made.

Consequently, the unemployment rate did not reach the 10 percent-plus rate that Friedman and Samuelson feared it would reach in late 1975: instead, it peaked at 9 percent in May 1975: see Figure 1. The second quarter of 1975 likewise saw growth in aggregate output turn positive, and the NBER would date the trough of the 1973−1975 recession to March 1975. As of the end of the 1970s, U.S. quarterly national accounts data showed that the peak-to-trough fall in output (real GNP) had been 5.7 percent (B.M. Friedman, 1980, p. 12). Data revisions and redefinitions would moderate this decline greatly: modern real quarterly GDP data show an output fall of 3.1 percent over the course of the recession.[18] However, in modern data on *annual averages* of real GDP, it remains the case that output fell in both 1974 and 1975—by −0.5 percent in 1974 and −0.2 percent in 1975—the only U.S. postwar recession other than that of 2007−2009 to include back-to-back declines in the annual-average data on total output.

[17] Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 158, late February/early March 1975.
[18] See also the discussion titled "Arthur Okun" later in this chapter.

Percent

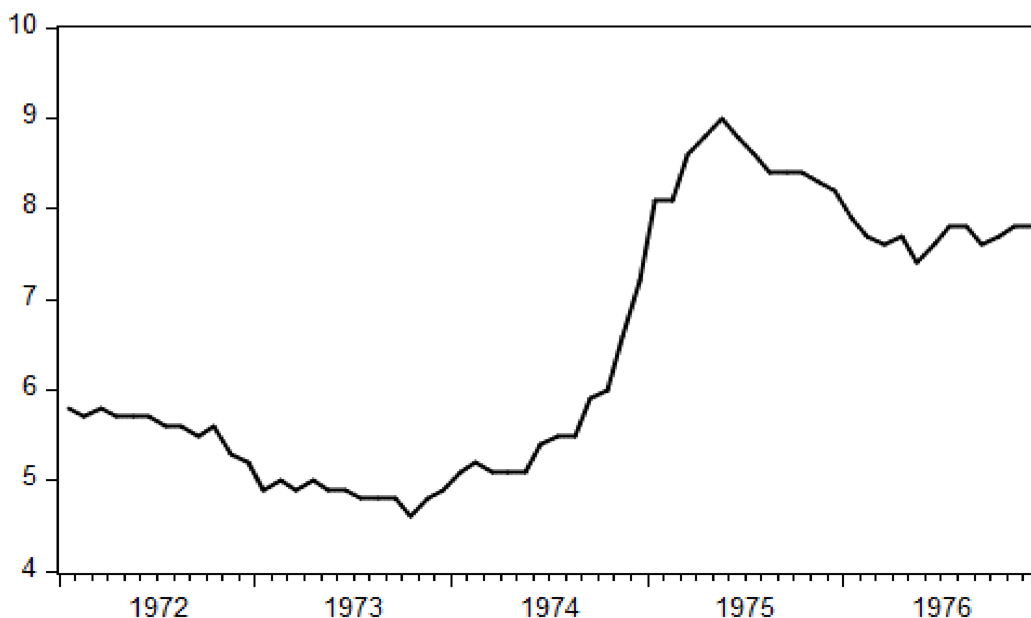

Figure 1. U.S. unemployment rate, January 1972–December 1975.
Source: Federal Reserve Bank of St. Louis' FRED portal.

Friedman, using M2, dated the trough in monetary growth to January or February 1975.[19] So a pickup in monetary growth began only one or two months ahead of the beginning of the recovery—which, as Friedman acknowledged (Instructional Dynamics Economics Cassette Tapes 174, August 1975, Part 2, and 181, November 1975, Part 2), implied an unusually short lag.[20] The modern series on M2—broader than the M2 definition that Friedman was using at the time—gives somewhat more indication of a standard lag length, as it implies that the quarterly annualized rate of growth of nominal money troughs in 1974:Q3 was 4.4 percent, before rising to 6.3 percent in 1974:Q4, 7.9 percent in 1975:Q1, and 15.5 percent in 1975:Q2. These data therefore indicate a lag of two quarters from the monetary turnaround to the nominal-income

[19] Instructional Dynamics Economics Cassette Tape 170, June 1975, Part 3; Instructional Dynamics Economics Cassette Tapes 174, August 1975, Part 2, and 181, November 1975, Part 2; *Newsweek*, October 3, 1977. Brunner (175, p. 3) also cited February 1975 as the date for the trough in money (using M1, in contrast to Friedman's focus on M2). Such dating would be broadly consistent with Dornbusch and Fischer's (1978, p. 537) observation that monetary policy was "quite restrictive until the second quarter of 1975." Meltzer (1986, p. 450), in contrast, gave monetary growth as having a six-quarter lead over the 1975:Q1 economic trough. But this was a highly implausible and awkward dating procedure, as it required assigning the trough in monetary growth to a very early, pre-recession point (1973:Q3) and overlooking the late 1974/early 1975 monetary trough.
[20] At the time (in Instructional Dynamics Economics Cassette Tapes 172, July 1975, Part 2, and 174, August 1975, Part 2), Friedman put the output trough at 1975:Q2 rather than 1975:Q1 because real GNP (on initial data) was flat in the second quarter—implying a still-short lag of about four months from monetary growth to output growth.

turnaround.[21]

Wallich (1975a, pp. 4–5; 1977, p. 286) would offer a different interpretation of the course of U.S. monetary variables observed in 1974–1975. He argued that the episode was one in which interest rates outperformed monetary growth as an indicator. In particular, Wallich contended that nominal interest rates were highest in the first half of 1974 and the economic downturn was most severe in late 1974; and interest rates plunged, then troughed roughly six months ahead of the 1975 economic turnaround. Consequently, Wallich argued that, in this episode, interest rates had exhibited the six-month lead over economic activity that Friedman so often ascribed to monetary aggregates. Furthermore, noting that nominal interest rates and monetary growth were giving opposite signals during 1974 and early 1975, Wallich suggested that interest rates outperformed growth in money as an indicator of future economic activity over this period.

Wallich's critique underscores the point that both nominal interest rates and money gave a "tight money" signal at *some time* during 1974—but at different parts of the year (in the first half for interest rates, in the second half for monetary growth).[22] In addition, Wallich was correct to suggest, on the basis of the data available to him at the time, that the lead of monetary growth over the start of the 1975 economic recovery was well below six months. Wallich's critique of money and his support for interest rates are, however, questionable in other respects. Although they rose in *nominal* terms, it is not obvious that interest rates rose in *real* terms in the first half of 1974; certainly, they did not on an *ex post* basis. This is essentially why the Taylor (1993) rule prescription for the federal funds rate moves well above the actual federal funds rate in early 1974 and then stays there throughout the rest of the year.[23] It is consequently not obvious that

[21] By 1975, lag estimates such as these had received such widespread acceptance that even Walter Heller could remark that "we know, from studies by Milton and others, it takes six to nine months" for monetary policy shifts to show up substantially in economic activity (March 12, 1975, testimony, in Joint Economic Committee, 1975c, p. 1040).
[22] A later account by Joseph Slevin (*American Banker*, October 23, 1978*a*) could, consequently, refer to "the excruciating tight-money squeeze" of 1974, without needing to be specific about whether monetary-growth rates or interest rates were being used as the criterion for evaluating tightness. Friedman's analysis of the 1974–1975 monetary squeeze resembled that he gave of developments in 1966 in having the feature that he *initially* doubted that there was a squeeze underway, as in the initial part of the year interest rates rose but monetary growth did not undergo a severe decline.
[23] This is shown in Taylor (1999, p. 337) and in the final-data Taylor rule prescriptions for 1974 shown in Orphanides (2003, p. 649). Monetary policy was therefore consistently too loose throughout 1974 when judged in retrospect by the Taylor-rule criterion. In contrast, Friedman, using monetary growth, suggested, instead, that monetary policy in the second half of 1974 was too tight. The difference may reflect the fact that the Taylor rule allows only indirectly for variations in the natural real interest rate. Indeed, one motivation for a concentration on monetary growth as an index of monetary policy, rather than constructs based on observed interest rates, is that variations over time in the natural rate of interest are substantial. In addition, as already stressed in Chapter 2, Friedman's complaint about the 1974 monetary policy shift partly rested on its suddenness (*Wall Street Journal*, August 21, 1975): the monetary growth rates to which the shift led were not below rates he likely regarded as

Table 1. Developments in monetary and economic aggregates, 1974 and 1975

	Four-quarter growth rates					
Period	Nominal GDP	Real GDP	Old M1	Old M2	Modern M1	Modern M2
1974:Q1	8.3	0.6	5.9	8.9	5.3	6.3
1974:Q2	8.2	–0.2	5.6	8.7	5.1	5.9
1974:Q3	8.8	–0.6	5.3	8.3	4.8	5.5
1974:Q4	8.4	–1.9	5.1	7.7	4.8	5.8
1975:Q1	8.4	–2.3	3.6	6.6	3.7	5.8
1975:Q2	8.0	–1.8	4.1	7.3	4.3	8.4
1975:Q3	9.6	0.8	4.8	8.3	5.3	11.2
1975:Q4	10.1	2.6	4.4	8.3	4.8	12.1

Source: In the case of real GDP, nominal GDP data are quarterly series in the Federal Reserve Bank of St. Louis' FRED portal. In the case of modern M1 and M2, growth rates from quarterly averages of the monthly data in the FRED portal. Quarterly levels data in Lothian, Cassese, and Nowak (1983) are used to calculate the growth rates reported in the table for the old (pre-1980) M1 and M2 series.

this period's interest-rate behavior—which is discussed further below—accurately pointed to an intensification of the downturn in U.S. real economic activity. In contrast, monetary growth in early 1974, although lower than that seen later in the year, did represent a step-down from prior rates: see Table 1. And this pattern is more pronounced for the modern definition of M2.

Finally, as noted above, if monetary growth is seen as particularly being a harbinger of future developments in *nominal* income growth, monetary growth (especially the movements in M2) did not obviously fail over the 1974–1975 period as an indicator of subsequent developments in income. The growth rates of the two series were in broad alignment over time—a fact discussed in Chapter 2 and amplified by Table 1. In addition, M2 growth had a lead over nominal GDP growth in this period—more clearly so in the case of the modern definition of M2 than for the 1970s M2 definition.[24]

The factor making the short-run relationship between nominal money and real output somewhat disturbed over these years was not a break in the money/nominal income relationship but,

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slightly too high in the long run. In that respect, his critique of 1974's monetary policy is consistent with the Taylor rule's message that policy throughout 1974 was too loose, but not with the notion that the degree of looseness was roughly the same over the year.

[24] In monthly data, the trough in the twelve-month growth rate in nominal personal income trough is in July 1975: the 12-month growth-rate trough for modern M1 is in February 1975, with that rate recorded again in April. The twelve-month growth rate of modern M2 troughs in January 1975, six months ahead of the nominal personal income trough.

instead, the behavior of inflation. Inflation's sharp (further) rise in late 1974, along with its distinct decline the following year, made the short-run movements of nominal GDP and real GDP more disparate than usual, and some of the downturn/recovery pattern in 1974−1975 can be considered a reflection of the shifting shares of real GDP growth and inflation within nominal GDP growth. The 1975 upturn was another case of the phenomenon of money having a shorter lead over real than nominal income—a regularity that Friedman had already highlighted in the early 1970s as a finding emerging from his monetary studies.[25] Correspondingly, another way of describing U.S. economic developments in the mid-1970s is to note that the period saw real monetary growth undergo a more severe squeeze in 1974 than that experienced by nominal money.[26]

Concerns about an overreaction

Friedman indicated in the early months of 1975 that he objected to the "over-restrictive policy" of the Federal Reserve not only because it was worsening the recession but also on the grounds that it would likely, in due course, trigger an overreaction of monetary policy in the opposite direction—a policy move that would see monetary growth permitted to move up into a range so high that inflation would eventually become worse (*Newsweek*, March 10, 1975*a*). Along these lines, he remarked on television that "the big danger now is... that in reacting to this recession, we'll again overreact, pour the money in, print money like mad, and get off on another inflationary binge. I think that's the real danger."[27]

In expressing this fear, Friedman was repeating a theme he had laid out since the early 1950s and that he believed well described previous FOMC moves in 1962 (after the economic pause of that year), 1967 (in response to the mini-recession), and 1971 (after the seemingly-slow initial recovery from the 1969−1970 recession). Indeed, even in late 1974 Friedman had expressed fears of a Federal Reserve overreaction, and he had worried in December of that year that a surge in monetary growth in October 1974 was the beginning of a move to excessive rates.[28] These early fears were temporarily superseded by the monetary data through early 1975, which suggested that monetary restriction was intensifying.

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[25] See Nelson (2020a, Chapter 6).
[26] Paul Samuelson focused on the tightness "of real money" in his *Newsweek* column of March 3, 1975, and in Samuelson and Temin (1976, p. 329).
[27] *Wall Street Week*, Maryland Public Television, February 7, 1975, pp. 16−17 of transcript. See also the accounts in *Oakland Tribune* (January 13, 1975), *Los Angeles Times* (February 24, 1975), and *Journal of Commerce* (February 25, 1975, p. 3) of remarks that Friedman made along the same lines in Indianapolis (in January) and Claremont, California (in February).
[28] For these warnings, see *Newsweek*, November 4, 1974, and Friedman (1975c, p. 178).

The turnaround in M2 growth in 1975, however, proved so sharp that over much of the year Friedman thought the Federal Reserve was overdoing stimulus and warned about the prospect that this would give rise in coming years to a resurgence of inflation. An early commentary to this effect was reported during a visit Friedman made in Japan in April 1975, when he argued that continuation of the present pace of monetary growth could lead to 20 percent inflation in 1977 (*Omaha World Herald* (Nebraska), April 23, 1975).[29] In August, he again complained that monetary growth was excessive (*Business Week*, August 11, 1975), and in December he was seeing inflation moving into a 12−15 percent range as early as the latter part of 1976 (*Mexico Ledger* (Missouri), December 5, 1975; *Kansas City Times* (Missouri), December 5, 1975).

As Table 1 shows, monetary growth did pick up over 1975. Friedman's resulting prediction (*Omaha World Herald* (Nebraska). April 23, 1975) that a strong economic recovery would occur that year was vindicated and contrasted with various predictions made by others in 1975 that the U.S. recovery would not be *V*-shaped.[30] However, to Friedman's surprise M2 growth settled down—albeit at double-digit annual growth rates—in the fourth quarter of the year, and in early 1976 he acknowledged that monetary growth had not been as high as he had been expecting (Instructional Dynamics Economics Cassette Tape 183, January 1976, Part 1). In fact, as will be discussed toward the end of this chapter, Friedman saw the cooling-off in monetary growth in late 1975 that produced this more-moderate course for the twelve-month rate as bearing importantly on the outcome of the 1976 U.S. presidential election.

However, though Friedman's warnings in 1975 of double-digit inflation in 1977 exaggerated the degree of easing by the FOMC, his relief in the first months of 1976 that monetary growth had largely been contained would prove premature. It would eventuate that his fears of an overreaction by the authorities in the wake of the 1973−1975 recession did not turn out to be far off. Monetary growth, measured by the behavior of the 1970s definition of M2, was higher in 1976 and 1977 than it had been in 1975. As this surge started to emerge in mid-1976, Friedman declared in his column: "We are currently in the midst of a monetary explosion…" (*Newsweek*, June 14, 1976*c*.) This became the second monetary explosion of the 1970s—an event discussed in detail in Chapter 7 below. And whereas, for the year to March 1976, M2 growth came in at approximately the midpoint of the FOMC's 8½−10½ percent target band, for the year to

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[29] These remarks were also reported on *CBS Morning News* (April 22, 1975, p. 21 of transcript) and in *Time* magazine, May 5, 1975, p. 70. See also Friedman's remarks in Instructional Dynamics Economics Cassette Tape 167 (May 1975, Part 1).

[30] For the predictions from economic commentators of a slow recovery, see, for example, *Time* magazine (May 5, 1975, p. 69) and Franco Modigliani's remarks in *Business Week* (August 11, 1975), as well as the discussions in López-Salido and Nelson (2010) and in the section later in this chapter titled "The Financial Crisis of 1973−1975."

1976:Q4 M2 growth was 10.9 percent—outside the FOMC's target range of 7½–10½ percent (Argy, Brennan, and Stevens, 1990, p. 54).[31]

Furthermore, although this was a period in which patterns in broad monetary aggregates were particularly good indicators of future behavior of inflation, the M2 growth data available in the mid-1970s understated the inflationary pressure in store for the U.S. economy, for two reasons. First, thrift institutions' deposit-type liabilities, which were outside the M2 definition of the time, were growing strongly, so M2 growth as measured in 1975 and in 1976–1977 (the latter period being that of the second monetary explosion) was consistently below growth in the modern M2 series.[32] Second—though would not become clear for several years—the post-1973 productivity slowdown had lowered the growth rate of potential GDP. This factor alone meant that the noninflationary rate of monetary growth after 1973 was about 1 percentage point lower than previously, moving down from about 3.5 or 4 percent in the 1960s to about 2.5 or 3 percent after 1973. This slowdown plays a large role in explaining why, as shown in Orphanides (2003, 2004) and discussed further in Section III, estimates of the output gap during 1975 reached double-digit negative values, even though modern data on the output gap—obtained using real GDP together with Congressional Budget Office estimates of potential as of 2019—the output gap got no deeper than minus 5 percent during 1975.[33]

The 1975 income tax rebate

Friedman's concern that the 1974–1975 recession would produce an overreaction in the form of a shift to excessively expansionary aggregate-demand policies had stemmed partly from the fact that numerous other leading economists had indeed been urging a decided reversal of policy settings. In many cases, these recommendations went beyond winding back the late-1974 tightening of monetary policy on which Friedman focused. A notable illustration of this came at the White House economists' summit—held in early September 1974, a time when Friedman's position was still that monetary policy was not yet too tight. In describing the proceedings' discussions, Arthur Okun characterized Friedman as an exception to the conference consensus

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[31] Argy, Brennan, and Stevens (1990) gave M2 growth in the year to March 1976 as 9.6 percent. The Lothian, Cassese, and Nowak (1983) quarterly dataset implies a rate of 9.4 percent. Both sources register agreement on M2 growth being 10.9 percent in the year to 1976:Q4, and Bernanke and Mishkin (1992, p. 190) also gave this as the four-quarter rate for that period.

[32] Simpson (1980, p. 103) showed that, although new M2 growth in 1960–1979 as a whole only exceeded that of old M2 growth by 0.7 percentage points per annum on average, this excess rose to 1.5 percentage points for the period 1975:Q1–1979:Q4. See also Chapter 10 below.

[33] As discussed further later in this chapter, the output gap using these modern estimates give a trough in the 1970s of the output gap of –4.7 percent, in 1975:Q2.

that current monetary policy was in a setting of "extreme stringency."[34]

James Tobin, who was not among the conference attendees, shared the judgment that U.S. monetary policy had been too tight throughout the period since the first oil shock. On the occurrence of that shock, Tobin, as discussed in Chapter 3, had suggested that the U.S. authorities should accommodate its effect on the aggregate price level. He characterized the actual policy response by the Federal Reserve as not following this advice and, instead, going in the opposite direction: reacting in an overtly deflationary manner to the OPEC move. By worsening the country's terms of trade, the oil shock would have tended to reduce U.S. real income in any event, Tobin granted. But he contended that the restrictive monetary policy response had compounded the reduction in real income (see Tobin, 1977a, p. 57).

It was against this background that in April 1975 Tobin testified alongside Franco Modigliani to Congress' Joint Economic Committee. At the hearing, they both advocated not only easier monetary policy but, also, further fiscal expansion.[35] Modigliani, believing the output gap was deeply negative, urged that an aggressive stimulation of aggregate demand was necessary. However, he feared that instead the authorities would "take the Friedman approach—[which is,] use the next 10 years to get back to full employment."[36] Tobin, in articulating his own case for a loosening of the fiscal and monetary reins, argued that the "recession [was] generated by tight money policy," while fiscal deficits would "not 'crowd out' private uses of credit or force interest rates to levels which prevent or impede economic recovery."[37]

Friedman obviously concurred with the Tobin remark on monetary policy and the recession. Furthermore, by 1975, after several years of emphasizing the empirical relevance of crowding out, he was already moving towards Tobin's position on the matter. By the early 1980s, in fact, Friedman had become *more* skeptical about crowding out than was Tobin, and it would instead be Tobin who was stressing the empirical relevance of the pressure on interest rates arising from government borrowing.[38] But, consistently from the 1950s to the 2000s, Friedman opposed policy recommendations that were centered on fiscal activism, such as those that Tobin and Modigliani were advancing in 1975. Automatic stabilizers aside, Friedman was against altering taxes and government spending for countercyclical purposes. As for monetary policy, at the time of the Tobin/Modigliani Congressional appearance Friedman favored a resumption of moderate

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[34] From Okun's remarks of September 5, 1974, in Council of Economic Advisers (1974, p. 117).
[35] See Joint Economic Committee (1975a). See also *Washington Star-News*, April 25, 1975.
[36] From Modigliani's testimony of April 24, 1975, in Joint Economic Committee (1975a, p. 34).
[37] From Tobin's testimony of April 24, 1975, in Joint Economic Committee (1975a, p. 36).
[38] See Chapters 8 and 14 below.

monetary growth, with this to be followed by a sequence of gradually declining rates, until the rate consistent with long-run price stability was reached.

As it happened, both monetary and fiscal settings had both in fact already shifted notably by the time the April 1975 Congressional hearing took place. With regard to monetary policy, early 1975 had seen a shift to a higher rate of monetary expansion, as already discussed. And in the area of fiscal policy, Congress passed in late March 1975 an income tax rebate that had been advanced by the Ford Administration as an anti-recession measure (Romer and Romer, 2010, p. 772).

The tax cut consisted primarily of a one-time transfer to households of part of their calendar-1974 tax payments. It was overtly advanced as temporary lowering of taxes, rather than a permanent move to new tax arrangements (Poterba and Summers, 1987, p. 386; Romer and Romer, 2010, pp. 772–773). The payment was therefore essentially a windfall affecting the flows of households' disposable income. The permanent income hypothesis has the implication that there would be little response by households' spending on nondurable goods to such a transitory disturbance to income flows. Indeed, it was on this point that Lydall (1958, p. 564) took Friedman's *A Theory of the Consumption Function* to task, arguing: "Empirical evidence shows conclusively that people who receive windfalls consume more. Friedman's attempts to get round this awkward fact are not convincing."[39]

It would transpire, however, that the 1975 tax rebate buttressed the permanent-income perspective on consumption behavior. Blinder (1981a, p. 27) noted that the U.S. household saving rate surged during the rebate episode, "suggesting that little of the rebate was spent."

The evidence on the rebate's effect on the behavior of consumption, as given by empirical estimates of the consumption function, largely confirmed the evidence suggested by the surge in the saving rate. Modigliani and Sterling (1986, pp. 1172–1173) found a significant positive effect of the tax rebate on consumption, but only when it was combined with the late-1960s tax surcharge to create a "transitory taxes" variable. Poterba and Summers (1987, p. 387) found that the 1975 rebate entered consumption functions with *t*-values less than 2 when considered individually. And significantly, Poterba and Summers found that the 1964 and 1981 tax cuts—which were permanent in character—generated far more clear-cut positive effects on U.S.

[39] Lydall's (1958) review of Friedman (1957a) is discussed further in Nelson (2020a, Chapter 5). For a latter-day assertion that "people consume windfall income at almost the same rate" as their regular income, see *New York Times*, June 9, 1995.

consumer spending. Later, Campbell and Mankiw (1990, p. 271), using a specification that embedded permanent-income mechanisms alongside more traditional Keynesian features in a general consumption function, found that the surge in disposable income in 1975:Q2 associated with the tax rebate produced a large residual in the estimated function and had little counterpart in U.S. household spending on nondurable goods.

The absence of a sustained consumption response to the 1975 tax rebate disillusioned the Ford Administration's economic team about such measures (see Taylor, 2012, pp. 54–56). The episode also likely made many economists more receptive to Friedman's position that fiscal actions had little cyclical effect when not accommodated by monetary policy. Policy suggestions along the lines of the tax rebate continued to figure prominently, however—as evidenced by President Jimmy Carter's tax-rebate proposal of early 1977 (see Chapter 8 below).

Friedman's own commentary in *Newsweek* on employing a tax cut against the recession argued that it would be ineffective in stimulating the economy. In taking this line, however, he pointed to empirical evidence against the demand-stimulating powers of fiscal policy, instead of invoking the permanent income hypothesis *per se*.

Irrespective of the specific argument on which Friedman relied in downgrading fiscal policy, it became moot if the fiscal measure was monetized. In 1975, as on other occasions, he noted that the conclusion that a tax cut was ineffective as a demand-stimulating measure would no longer hold if monetary accommodation took place. But he added that the Federal Reserve could achieve much the same effect on aggregate nominal spending via purchases of the existing stock of government bonds outstanding, with no new fiscal stimulus (*Newsweek*, May 12, 1975). This observation was a repeat in print of a point that Friedman had stressed in his Claremont College appearance a couple of months earlier, when he remarked that "you don't need a tax cut and a higher deficit to print money. You can provide the monetary growth without the tax cut." (*Participant*, Spring 1975.)

In his April 1975 proposals, Franco Modigliani had, in fact, suggested that the Federal Reserve should be required by Congress to accommodate the proposed move to a larger fiscal deficit and, specifically and relatedly, to enforce a ceiling on market rates on longer-term Treasury securities. In the event, although (as discussed above) monetary growth rose in 1975, this did not arise from conscious accommodation of the federal deficit (or from any target for long-term interest rates; these remained market-determined). The fiscal and monetary expansions were not closely

related; and a very large spike in the U.S. budget deficit in 1975:Q2 did not give rise to a comparably outsized rate of monetary growth.

That spike in the deficit has, nevertheless, played a major part in some narratives of 1970s macroeconomic behavior that are based on the fiscal theory of the price level (FTPL). After Sims (2011, p. 49) expounded the FTPL by pointing to the Ford tax cut's effect on primary fiscal deficits, Bianchi and Melosi (2013, pp. 30–32) claimed a central role for that tax cut in explaining U.S. inflation in the 1970s. However, Bianchi and Melosi's exercise—which focused on the *average* values of inflation and deficits for the 1970s—proves to be unconvincing once it is examined in light of several details about that decade's developments that they do not consider. The FTPL account requires fiscal deficits to generate higher aggregate nominal aggregate demand; but, as already indicated, aggregate nominal spending failed to respond appreciably to the 1975 tax cut. Furthermore, inflation was high in the first half of the 1970s—yet the tax cut did not occur until the start of the second half of the decade. Indeed, as Friedman remarked (*Newsweek*, February 23, 1981), the double-digit inflation of 1974 was accompanied by a low federal deficit in relation to national income.[40]

Nor can the high inflation of 1974 or preceding years plausibly be attributed to anticipation of the 1975 tax cut. The latter was not floated until January 1975 by President Ford—who in late 1974 had actually been proposing an increase in taxes (see Nelson, 2005). All in all, what Friedman called the "uneasy connection of deficits with inflation" (*Newsweek*, February 23, 1981) is well exemplified by the rebate episode. Indeed, over the course of 1975 and 1976—that is, just before, throughout, and for a year and a half after the distribution of the rebate—the U.S. inflation rate actually fell.

Inflation in 1975–1976: from new peak to local trough

With monetary growth having come down from its peak in 1972, and with output subsequently falling below potential, a partial disinflation took place. This began roughly at around the turn of the year from 1974 to 1975. Twelve-month CPI inflation peaked at 12.2 percent in November

[40] This contrast underlines the fact that, although monetary growth and deficits did indeed both rise during the first half of 1975, Bianchi and Melosi's (2013, p. 32) implication that U.S. fiscal deficits *for the 1970s as a whole* were financed by money creation is likewise not supported by the detailed record of the behavior of monetary growth and deficits over that decade. Such a lack of relationship was in keeping with Friedman's view of the choices of policy mix available for countries like the United States that had highly developed markets for government securities. As discussed in Chapter 2, Friedman certainly believed that the growth of the public sector was a source of upward pressure on monetary growth over long periods. But he did not believe that U.S. budget deficits accounted for the rapid monetary growth in the early 1970s.

1974, fell below 12 percent in January 1975, below 11 percent in March 1975, and below 10 percent in May 1975. Having returned to single digits, inflation continued to fall for another eighteen months. Early in the second quarter of 1976, Friedman predicted that inflation would fall to 4 to 5 percent by the end of the year (*The Star* (Johannesburg, South Africa), April 5, 1976). He was right: twelve-month CPI inflation stood at 5.0 percent in December 1976, while data on the GDP deflator show four-quarter changes of 5.1 percent in 1976:Q3 and 5.2 percent in 1976:Q3. The GNP deflator, used heavily at the time as a national price index, showed a similar but slightly lower rate of increase, passing slightly below 5 percent in late 1976.[41]

But Friedman believed that there was little prospect that inflation would fall further. Here, too, his prediction was accurate: the inflation rate of 5 percent, or a little lower, in late 1976 proved to be a local trough, and all of the disinflation of 1975−1976 would be erased by the end of the decade.

As far as the trough in inflation was concerned, Friedman traced it to the fact that monetary growth had bottomed out two years earlier, during the squeeze of late 1974. But his pessimism about inflation was more deep-seated. He felt that considerable complacency was developing in U.S. public debate when it came to deviations from price stability. And he believed that policymakers, especially those at the Federal Reserve, lacked the analytical and policymaking mindset required to pursue and stick to policies consistent with the achievement of price stability.

Fears of complacency

When the decline in inflation to 5 percent began to come about, Friedman voiced dissatisfaction at the delivery of inflation of that rate now being thought of "as doing a good job" (*The Evening Bulletin* (Philadelphia), November 18, 1976) or as "marvelous" (Instructional Dynamics

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[41] For example, the Council of Economic Advisers (1977, p. 190) gave the provisional reading for four-quarter GNP deflator inflation in 1976:Q4 as 4.7 percent. In addition, data in International Monetary Fund (1981, p. 65) implied a twelve-month CPI inflation for December 1976 of 4.7 percent. It is very likely that the fact that this figure is lower than that implied by the modern, downloadable CPI data in part is a reflection of rounding of the later vintages of price series. For example, the FRED data on the monthly U.S. consumer price index (available at *https://fred.stlouisfed.org/series/CPIAUCSL*) imply a local trough in the 12-month inflation rate of (slightly over) 5.0 percent in December 1976. In contrast—and reflecting the greater decimal precision of the earlier than the later vintages of the CPI data—Dornbusch and Fischer (1984, p. 555) correctly noted: "Consumer prices rose less than 5 percent during 1976." Likewise, the twelve-month CPI inflation rate for December 1976 reported in U.S. statistical releases at the time was 4.8 percent (see Nakayama, Howell, Monson, and Thomas, 1977, pp. 14, 15). (See also the Bureau of Labor Statistics' "CPI Detailed Report for December 1976," available at *https://fraser.stlouisfed.org/title/cpi-detailed-report-58/december-1976-19329*.)

Economics Cassette Tape 201, October 1976, Part 2). In 1970, he had observed that the populace a decade earlier has viewed 3 percent inflation as terrible, while now such a rate was regarded as "pretty good" (Instructional Dynamics Economics Cassette Tape 57, August 20, 1970). In that 1970 commentary, Friedman had expressed the hope that in four to five years—that is, in 1974–1975—3 percent inflation would be back to being regarded as terrible.[42] In the mid-1970s, however, such an attitude had clearly failed to materialize.

Not only did Friedman regard people in the mid-1970s as being too complacent about achieving 5 percent inflation: he also did not see the monetary regime as conducive to bringing inflation durably below that rate. What Benjamin Klein (1975b, p. 137) assessed, on the basis of inflation data up to 1973, to be a "fundamental change in the underlying monetary framework," seemed to be confirmed by the further rise in inflation in 1974 and by its limited decline thereafter. Correspondingly, in 1975 Friedman described the 1970s as being part of an era of permanent inflation (Instructional Dynamics Economics Cassette Tape 165, February 1975, Part 3). Although, as discussed in Chapter 2, he did occasionally slip into formulations that implied that high inflation was intentionally generated by policymakers, his more careful elaborations stressed that the United States' postwar inflation arose from the authorities' miscalculations: "The Federal Reserve System clearly did not want to exacerbate inflation from 1971 to 1974," he observed (*Newsweek*, March 10, 1975). Consistent with this perspective, Romer and Romer (2002) and López-Salido, Markowitz, and Nelson (2023) have provided documentary evidence suggesting that policymakers' ultimate goal regarding inflation in the 1970s was a rate consistent with price stability—specifically, about 2 percent, according to López-Salido, Markowitz, and Nelson (2023)—so the higher inflation in that decade did not reflect a change in policy goals since the 1950s or 1960s.

But, just as he saw recent years' inflation breakout as a by-product—rather than an intended result—of the U.S. economic policies pursued prior to the breakout, Friedman thought that inflation could be experienced again in coming years because the country's monetary regime had not changed. In an October 1975 appearance in which he lamented the "series of roller-coaster, go-stop policies" that had characterized U.S. economic stewardship over the previous decade and more, he observed: "I feel we are still on this course and have more cycles to go through…" (*Dallas Morning News*, October 17, 1975.)

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[42] By Friedman's own assessment, however, 3 percent inflation was (just) low enough that the economic incentives to introduce protections against inflation in contracts might not be significant (Instructional Dynamics Economics Cassette Tape 152, August 21, 1974; American Enterprise Institute, 1974, p. 51).

In particular, Friedman did not feel that policymakers yet grasped the appropriate policy response needed for eliminating inflation. "You can't stop inflation without unemployment and stagnation," he had remarked earlier in the year. "All this business about consumers saving more and reusing paper plates to control inflation—that's nonsense." (*Philadelphia Evening Bulletin*, March 2, 1975.) The methods being advocated at the policy level to were more sophisticated than the popular recommendation of paper-plate usage that Friedman cited. But, as discussed in Chapter 2, the termination, at the end of April 1974, of the United States' wage and price controls was followed by a slew of recommendations from leading economists for post-control incomes policies. Walter Heller, for example, testified to the Joint Economic Committee in August 1974 in support of a wage-price monitoring agency, and he implied that aggregate demand tightening might deliver higher unemployment but not lower inflation (*Washington Star-News* (Washington, D.C.), August 1, 1974; see also Chapter 4 above). And Heller (1976, pp. 51, 191) proposed that a tax cut could be used as a means of relieving wage-push inflationary pressure—an argument that financial columnist Sylvia Porter likewise deployed when making the case for the 1975 tax rebate (see Nelson, 2005).[43] A short-lived uptick in inflation in the latter part of 1975 also saw renewed discussion of cost-push inflation, this time thought as perhaps arising from the firm side rather than wages.[44]

At the policy level, too, elements of a nonmonetary approach to inflation had been present in the Ford Administration from its inception—as discussed in Chapter 2—and survived over 1975−1976. It is true, as has already been indicated, that both CEA Chairman Alan Greenspan and Secretary of the Treasury William Simon were sympathetic with Friedman's perspective toward inflation. Consistent with this, Simon testified to the Joint Economic Committee in February 1975 that what was required was "some margin of economic slack… for a period of years," and he criticized the alternative approach, involving a very speedy elimination of the output gap (*Washington Star-News* (Washington, D.C.), February 6, 1975). In addition, one member of the Ford Administration who had espoused cost-push views of inflation—Albert Rees—left public office at the end of July 1975 (*Washington Post*, March 9, 1979).

Crucially, however, throughout the Ford years Federal Reserve Chairman Arthur Burns remained firmly in the nonmonetary camp in his perspective on inflation. Burns had argued in June 1973 that wage-price controls should be followed by a system in which a federal government wage-

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[43] See also the discussion later in this chapter of Arthur Okun's tax-based incomes policy (TIP) proposals.
[44] See Nelson (2005). This episode to some observers validated the concerns about "administered price inflation" which, as discussed in Chapter 2, were articulated by commentators and policy agencies during late 1974 and the first half of 1975.

and-price-review board had a say on the economy's major price changes and wage disputes.[45] Similarly, in August 1974 Burns came out in favor of some federal-government direct management of private-sector pricing practices: in particular, he came out in favor of new wage-price guidelines (*Washington Star-News* (Washington, D.C.) August 23, 1974). Statements like this generated new public rebukes of Burns on Friedman's part (see, for example, Instructional Dynamics Economics Cassette Tape 151, August 7, 1974). But Burns went on to make many similar remarks in 1975 and 1976—including his March 1976 observation that "if we try to rely solely on monetary and fiscal policies to achieve general price stability, I believe we are likely to fail… I am convinced that we will return to an incomes policy sooner or later" (quoted in DiCecio and Nelson, 2013, p. 409).

As this quotation indicates, Burns did not perceive that economic slack would generate the benefits in terms of lower inflation that Friedman and Simon were postulating. His belief that a situation of output falling short of potential would not reduce upward pressure on prices mirrored his position that the 1970s surge in inflation would have taken place in much the way it did even if monetary policy had been tighter. Reflecting this difference in perspective, in the summer of 1975 Friedman criticized Chairman Burns for still failing to acknowledge sufficiently the Federal Reserve's role, via its actions of the early 1970s, in producing the inflation surge of 1973−1974 (*Wall Street Journal*, August 21, 1975).

Interest rates in 1975−1976

It was, of course, in the context of monetary policy, as practiced by Burns, that Friedman viewed the behavior of U.S. interest rates during the mid-1970s. Over this period, the Fisher effect was raising (and then lowering) nominal interest rates, and fluctuations in the natural real interest rate were creating further variations in the values of the short-term nominal interest rate consistent with price stability. In the face of these forces, the Federal Reserve could still choose to manage short-term nominal interest rates—and indeed it continued to do so, increasingly overtly, over 1975−1976 (with its pretense of managing a reserves aggregate abandoned in the latter year). But such management, if it went against the forces driving expected inflation and the real natural rate, could destabilize the economy. Friedman believed that the FOMC, even when it had decided to follow a policy of managing interest rates, would eventually have to adjust those rates in the direction of the pressures implied by underlying economic forces, simply in order to avoid

[45] See Burns' testimony of June 27, 1973, in Joint Economic Committee, U.S. Congress (1973a, pp. 181, 182). See also Chapter 4 above.

still greater economic instability.

In the short run, however, the Federal Reserve largely called the shots regarding the behavior of both nominal and real short-term interest rates. And U.S. short-term interest rates were low in real terms in the mid-1970s, especially in early 1975. In the first quarter of 1975, for example, the federal funds rate averaged 6.3 percent when the same quarter's annualized increase in the CPI was over 8 percent.

Barro and Xali-Martin (1990, pp. 17−18) have implied that real short-term interest rates in the United States and elsewhere in 1975−1977 were very likely *ex ante* positive even though they were negative in *ex post* terms.[46] These authors' discussion restricted the possibility of *ex ante* rates being negative to the case in which the natural real rate is negative—a situation they did not see as plausible except for extremely brief periods. Such a perspective, however, does not adequately allow for monetary policy as a powerful influence on *ex ante* real interest rates in the short run—an influence that Friedman stressed in his work on the liquidity effect.[47] Once monetary policy is acknowledged as a factor affecting real rates, there is no basis for believing that the *ex ante* short-term real interest rate cannot be (made) negative at the business-cycle frequency, even when the natural real rate is not negative.

For his part, Friedman in writing on short-term interest rates took the private sector as having largely rational expectations when forming its near-term forecasts of inflation. He accordingly took the negative *ex post* short-term real rates in the 1970s as predominantly implying negative *ex ante* rates.[48]

When these negative real rates coexisted with the take-off of monetary growth from early 1975, Friedman judged that the FOMC would have to let rates rise: "Short-term rates will go up practically no matter what the Fed does, even if it stands on its head." (*Business Week*, August 11, 1975.) Although the news report that contained these remarks took them as implying that "Milton Friedman is one economist who believes the Fed cannot control interest rates," it is clear from his many other statements during the period and at other times that Friedman *did* believe

[46] Brown and Santoni (1981) took an even stronger position—apparently denying that real *ex ante* short-term interest rates underwent an appreciable fall at all in 1974 and 1975. But they did not provide valid evidence that these rates did not in fact decline materially. In particular, their suggestion that real yields on some assets other than interest-bearing securities did not fall in the period is perfectly consistent (in a multi-asset world) with an outcome in 1974−1975 in which the real rates on interest-bearing securities fell and became negative.
[47] See Nelson (2020a, Chapter 6).
[48] See, for example, *Fortune*, July 1974, and *Newsweek*, June 28, 1982.

that the Federal Reserve could control interest rates (though he also believed that it should not) but that its stabilization goals would eventually force it to change its target value for the federal funds rate.

But Friedman also held that the FOMC of the post-Accord years was far too slow to make such changes. As discussed in Nelson (2020a, Chapter 8) and Chapter 2 above, Friedman's critique of interest-rate policies involved the Wicksellian point that, when there was downward pressure on the natural real interest rate, central banks might not reduce the nominal interest rate sufficiently rapidly, generating monetary restriction and forces promoting economic slowdown and recession.[49] Such a scenario, of course, had figured in Friedman's account of the 1974−1975 deepening of the recession. This critique amounted also to an acknowledgment that the natural short-term natural real rate had been low during some months in the mid-1970s. Therefore, although he felt that the FOMC had on the whole let real interest rates fall too low in the 1970s, Friedman also believed that, on occasion, it had not let real interest rates fall far enough.

In connection with longer-term interest rates, Friedman had observed in September 1973 (Instructional Dynamics Economic Cassette Tape 130, September 26, 1973): "I think inflation over the next ten years is almost sure to be higher than 4½ percent, it's very hard to believe that it's going to be that low." Longer-term rates, he observed during the previous summer, "far from being abnormally high are, if anything, abnormally low," as their values apparently reflected a presumption of a longer-term future inflation rate below 5 percent—an assessment that he believed was unlikely to be borne out (Instructional Dynamics Economics Cassette Tape 124, July 4, 1973). At this time (mid-1973), U.S. ten-year Treasury rates were about 7 percent. In August-September 1974, as we have seen, they crossed 8 percent. In 1975, longer-term interest rates failed to experience the decided decline from their previous year's values that was being exhibited by short-term interest rates. Rather, longer-term rates hovered around 8 percent in both years. They were nevertheless, as noted in Chapter 2, roughly consistent with a market expectation that inflation would return to perhaps 2−3 percent beyond the immediately approaching years. In October 1975, Friedman noted this expectation and expressed doubt about its soundness. Markets might expect 2 percent inflation by the end of the decade, he said, but it was actually unlikely to happen (*Dallas Morning News*, October 17, 1975).

But though the level of short-term nominal interest rates, and to some extent that of longer-term

[49] See the discussion later in this chapter of Friedman's November 1975 Congressional testimony, as well as the items cited in the discussion in Chapter 2 above.

rates, was inconsistent with the preservation of normal real returns, their fluctuations very closely tracked the course of inflation in 1973–1976—particularly so in the case of short-term rates. There is thus, as stressed in Chapter 2, no contradiction between the fact of the very low level of real rates in the mid-1970s and Friedman's conviction over the same period that the Fisher effect was clearly visible in U.S. short-term interest rate data—the first time this had been the case on a sustained basis.

The relationship between inflation and interest rates had made an impression on Arthur Burns, too. "Inflation is the main reason for the high interest rates that we have," Burns stated in an August 1975 television appearance, "and we're not going to bring interest rates down significantly until we bring inflation under control."[50] Burns' perspective on the relationship between inflation and demand pressure had changed greatly since the 1950s and the 1960s; but with regard to interest rates and inflation, in the 1970s Burns continued to believe strongly in the Fisher effect.[51] His change in view concerning the demand/inflation relationship put a new complexion onto his interpretation of the Fisher relationship (see DiCecio and Nelson, 2013, pp. 406–407). Burns' changed posture meant that the task of "bring[ing] inflation under control" was one he saw as ideally assigned primarily to nonmonetary measures. On this view, low real interest rates alongside inflation did not mean that monetary policy should be tightened. Instead, it implied that nonmonetary devices should be brought to bear more vigorously against inflation.

Unemployment benefits: the background

Back in July 1971—just ahead of the U.S. economy's four-year odyssey of wage and price controls, an oil shock, double-digit-inflation, and multi-year recession—Friedman was making the case for restraint in aggregate demand. He was doing so in the face of widespread commentary that more stimulus was needed because the decline in the unemployment rate since the 1969–1970 recession had been too slow. Against this tide, Friedman remarked that a factor that he had not previously considered much in his audio commentaries, but whose importance "has been increasingly brought home to me" in conversations, deserved consideration in interpreting the data. The "much more liberal and widespread welfare provisions that we now have" and "much easier access to welfare, including food stamps and other things," Friedman

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[50] From Arthur Burns' appearance in *Face the Nation*, CBS, August 24, 1975, quoted in *Banker's Monthly*, September 15, 1975, p. 2. See also Burns' testimony of September 25, 1975, quoted in DiCecio and Nelson (2013, pp. 406–407). Burns' continued belief in the Fisher relationship during the 1970s was also noted by Jordan (1979, p. 496).

[51] On Burns' espousal of the Fisher effect in the 1950s and 1960s, see Nelson (2016; 2020a, Chapter 6).

suggested, meant "a given unemployment percentage involves less pressure on the wage rate than it would have in earlier times"—that is to say, the natural rate of unemployment had increased (Instructional Dynamics Economics Cassette Tape 78, July 14, 1971).

During the summer of 1975, with unemployment only fractionally down from its May peak of 9 percent, Friedman pressed this point much more emphatically. Most notably, in his *Newsweek* column of August 4, 1975, Friedman commented that—although the recession had certainly created major job losses—"the high reported levels of unemployment are partly a statistical artifact reflecting expanding government programs to assist the unemployed." Such programs, Friedman wrote, "have made unemployment more attractive [and] have also tended to increase the number of peoples who so report themselves," rather than leaving the recorded labor force.

In the period between 1971 and 1975, much of the running on this topic had been done by Martin Feldstein, a specialist in public finance and macroeconomics and a member of Harvard University's Department of Economics since 1969. Feldstein had accepted that appointment in a situation in which he "had an offer from Chicago and from Harvard," and he had met Friedman in a job-market visit to the University of Chicago (Martin Feldstein, interview, November 21, 2013). Then, in the 1970s, Feldstein produced a sizable number of research papers on the relationship between the unemployment-benefit arrangements and the U.S. unemployment rate. This work likely was a major basis for Friedman's statement in 1980 that "in America… we have had some very careful studies" of the link between unemployment benefits and the unemployment rate."[52] The same work, along with a series of pioneering Feldstein papers on inflation and the taxation of capital, likely played a role in leading Friedman to predict in 1981 that at some point Feldstein would win the Nobel Prize in economics (Wallechinsky, Wallace, and Wallace, 1981, p. 418).

The earliest Feldstein studies of unemployment benefits cited in Chetty's (2008) discussion of that topic are Feldstein (1978) and Feldstein and Poterba (1984), while Krugman (1995, p. 73) stated that Feldstein's interventions on the matter were "[a]s early as 1973." In fact, however, Feldstein stepped into research and public debate on unemployment insurance earlier than these two discussions indicated—in a research study that underpinned Feldstein's Congressional testimony of October 1972.

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[52] *Free To Choose* (U.K. television version, debate portion), BBC2, episode "Created Equal," March 1, 1980, p. 16 of transcript. A few years earlier, specific research that Friedman (1977a, p. 25) referred to on the matter was a then-new study by Clarkson and Meiners (1977).

This testimony arose, Feldstein recalled, because "there was a debate going on between the Republican administration and Senator Proxmire, who was chairman of the Joint Economic Committee." Senator William Proxmire, a Democrat representing Wisconsin, had some inclinations toward free-market policies: in early 1974, he would remark: "I've always been a Friedmanite or tried to be a Friedmanite. Or as much as one can be who doesn't have a Ph.D. in economics. But I have great respect and admiration for Dr. Friedman and his principles, and I would agree that we should have as little government as possible; but the whole issue is how little government you can get away with."[53] As discussed further below (see "The Beginning of Monetary Targeting"), Proxmire was also sympathetic toward aspects of Friedman's macroeconomic views, too. Entering the U.S. Senate in 1957, Proxmire, had referred to Friedman's monetary research during Congressional hearings in the early 1960s (Nelson, 2020b, Chapter 11). He pushed for monetary-aggregate targets during the late 1960s—a development that led Friedman to observe that "Bill Proxmire is a good student of economics" (*Chicago Tribune*, July 5, 1968). Subsequently, Proxmire would challenge Arthur Burns in 1973 and 1974 using Friedman's arguments regarding inflation, and the senator would ultimately play a pivotal role in the formal introduction of monetary targeting in 1975.[54]

As far as stabilization policy was concerned, however, Proxmire favored more active steps on the part of the federal government and its agencies. Proxmire's longtime advocacy of monetary targets did not really clash with this posture. As eventually legislated by Proxmire, monetary targets were seen as putting monetary growth down on a glide-path down to rates roughly consistent with U.S. real output growth equaling its long-run potential rate. Subject to that endpoint, it was always possible to frame the numerical monetary targets to make them consistent with—possibly ambitious—targets for the level and growth rate of aggregate output over the interim years.[55] In keeping with Proxmire's enthusiasm for forceful demand-management policies, Friedman in early 1974 would call the senator "one of my favorite interventionists."[56]

In 1972, this attitude was manifested in Proxmire's view toward the Nixon Administration's economic goals. These were predicated on a full-employment unemployment rate of 4.5 percent, a value Proxmire considered insufficiently ambitious. "So," Feldstein recalled, "he had the brilliant idea of saying, 'What do we have to do to get the unemployment rate down to 2

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[53] From Proxmire's remarks in the panel discussion, *The Energy Crisis*, January 24, 1974.
[54] See Nelson (2020b, Chapter 13), Chapter 2 above, and "The Beginning of Monetary Targeting" below.
[55] This was certainly the case in early 1975, when there was wide agreement that there was slack in the economy and the monetary targets implied higher monetary growth than that observed in the immediate past.
[56] From Friedman's remarks in the panel discussion, *The Energy Crisis*, January 24, 1974.

percent?' And he then commissioned Otto Eckstein to do a study about that, and Otto had recently set up Data Resources, a company. Otto was too busy to do the study, and so he turned to me and said, 'Why don't you do this?' So I did it, and one of the things I focused on in that study as a way of reducing the permanent rate of unemployment was to modify unemployment insurance. And Proxmire was very unhappy with all that. What he wanted was a study saying: 'More expansionary monetary and fiscal policy; increase demand.' And [instead] here comes Marty saying, well, no, you can integrate the minimum wage and welfare, you can change unemployment insurance benefits and [undertake] a variety of supply-side policies." (Martin Feldstein, interview, November 21, 2013.)

Feldstein testified to the Joint Economic Committee on October 17, 1972, as part of hearings that Proxmire later ostentatiously titled *Reducing Unemployment to 2 Percent*.[57] Feldstein told Proxmire's committee: "Decreasing the overall rate of unemployment requires not merely more jobs, but new incentives to encourage those who are out of work to seek employment more actively, and those who are employed to remain at work." He added: "Unfortunately, the current system of unemployment compensation encourages excessive delays in returning to work. For many lower- and middle-income families, the combined effect of unemployment compensation and income taxes is to reduce greatly and often almost eliminate completely the cost of remaining unemployed for an additional one or two months."[58]

Unemployment benefits: Friedman's critique

Feldstein therefore characterized the length and magnitude of unemployment as partly a response to the incentives offered by government and the market. He consequently postulated a link between the unemployment rate and unemployment benefits, while avoiding casting aspersions on benefit recipients. Previously, it had been frequently the case that those who cited unemployment benefits as a factor boosting the reported unemployment rate had been portrayed

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[57] In his study underlying this testimony, Feldstein (1973a, p. 3) indicated that he was sympathetic toward the natural rate hypothesis and skeptically noted doubts about the pre-1972 research that had criticized the hypothesis. This, however, was not his focus, as, even if the natural rate hypothesis did not hold, whether unemployment benefits tended to have a material effect on the unemployment rate was a policy concern.

[58] From Feldstein's testimony of October 17, 1972, in Joint Economic Committee (1972b, pp. 17, 23). Feldstein's accompanying research study was published as Feldstein (1973a), and his testimony was reprinted as Feldstein (1973b). His follow-up research included Feldstein (1974a). Zell (1976) offered an opposing view.

Feldstein's findings concerning unemployment insurance were highlighted by conservative columnist William F. Buckley in a 1975 newspaper column, and the following year Buckley had Feldstein as a guest on his *Firing Line* television interview program (*Milwaukee Sentinel*, February 13, 1975; *Mobile Press Register* (Alabama), May 8, 1976). But see the discussion below for examples in which concerns about a positive relationship between unemployment benefits and the unemployment rate were voiced by figures of various political persuasions.

in political and popular discussions (and sometimes accurately so) as impugning the motives, and in particular questioning the work ethic, of those receiving governmental payments. For example, a remark by Ronald Reagan—made in the year in which he first ran for Governor of California—that unemployment insurance was a "prepaid vacation for freeloaders" came back to haunt him.[59] The quotation was thrown back at Reagan in 1966 and 1980, respectively, by incumbents whom he challenged for office: Governor Edmund G. (Pat) Brown and President Jimmy Carter.[60] During the 1980 presidential election campaign, in commenting on his early remark, Reagan qualified it and perhaps also backtracked, by saying that his comments had only meant to apply to a small number of benefit recipients (*U.S. News and World Report*, October 6, 1980, p. 60).

The stigma often attached to receiving governmental welfare payments had been both a motivation for Friedman's negative income tax idea and one obstacle that he faced in gaining acceptance of the proposal. In this connection, a *Time* magazine (December 8, 1967) discussion of Friedman's proposal had observed: "To many Americans, the idea of a guaranteed income smacks suspiciously of a dole to people who refuse to get a job." However, in 1968, Friedman himself gave what was an uncharacteristically unsympathetic discussion of the attitude taken by welfare recipients. There were members of "every economic or social class in society," Friedman said, who would gravitate toward welfare receipt if it was available because "they pay attention to immediate gratification and give little immediate thought to the future."[61]

However, Friedman's comments in the years before and after these remarks indicated that he did not view the bulk of benefit recipients in this way. Rather, his position paralleled that articulated by Reagan in 1980 that those exploiting benefits without intent of seeking a job were a small minority of overall recipients. The position that unemployment-benefit recipients did include a small, but appreciable, portion of such individuals was one voiced across the political spectrum in many countries. For example, Australia's Labor Prime Minister Gough Whitlam remarked in the mid-1970s: "there is an irreducible minimum of bludgers. And I suppose this is the experience of all government welfare agencies…"[62]

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[59] An analysis of Reagan's statements that appeared in the *Press-Telegram* (Long Beach, California), January 16, 1968, traced this remark to the *Sacramento Bee* of April 28, 1966. The quotation had, however, appeared in print slightly earlier—in a *Los Angeles Times* news report of April 21, 1966. Reagan had made the remark in a dinner speech on April 20.
[60] See, for example, *Redland Daily Facts* (California), September 7, 1966, and Carter (1980).
[61] Friedman (1968c, pp. 25−26).
[62] *This Week*, HSV7 (Australian television channel), September 21, 1975, p. 5 of transcript.

In his discussions from the mid-1970s onward, Friedman showed signs of a wish to concentrate on economic incentives to engage in longer unemployment spells—and not to be perceived as attributing bad motives to the recipients. This posture was reflected in remarks that he made indicating that it was understandable and proper for those eligible to apply for unemployment benefits. In a November 1976 television appearance, for example, Friedman observed that "a large fraction" of recorded unemployment "constitutes human beings taking advantage of very good arrangements that have been developed to help people who are unemployed. I don't blame them for doing it. They're foolish not to."[63] In the mid-1980s, Milton and Rose Friedman underlined that their emphasis on the link between governmental payments and unemployment was "not a criticism of the persons who receive unemployment benefit… So long as those programs are available, people are sensible to take advantage of them."[64]

Furthermore, Friedman indicated that it was appropriate for the government to provide unemployment benefits and that, in principle, existing amounts of benefits could be appropriate. In April 1975, he remarked that "obviously[,] the more generous are the provisions… the more unemployed there will be. That doesn't mean it isn't desirable to make provision…"[65]

In common with Feldstein's conclusion, however, Friedman's judgment was that in the United States unemployment benefits *had* become too high. Benefits were at a level that he believed would enlarge the number of people detached from the workforce by adding to those who would prefer to stay on welfare individuals who were marginally inclined to be a member of the workforce. Furthermore, it had become prevalent, in Friedman's assessment, for it to be a rational action on a recipient's part to remain a recipient of unemployment insurance, on account of the financial penalties incurred, on net, in taking a job that was available. Consequently, in Friedman's estimation, the U.S. situation was characterized by "governmental programs which encouraged a large number to become dependent."[66]

In explaining how they had reached these judgments, both Friedman and Feldstein emphasized the interaction of welfare, wages, and marginal tax rates. Feldstein's testimony, for example, suggested a situation of an 80 percent effective marginal tax rate for certain unemployed workers contemplating accepting a job offer in Massachusetts.[67] In looking only at the benefits paid, Feldstein recalled, "I think people had the wrong idea. You know, people would say, 'Look, the

[63] *Wall Street Week*, Maryland Public Television, November 5, 1976, p. 9 of transcript.
[64] Friedman and Friedman (1985, p. 108).
[65] Friedman (1975i, p. 28).
[66] *Speaking Freely*, WNBC, May 4, 1969, p. 15 of transcript.
[67] See his testimony of October 17, 1972, in Joint Economic Committee (1972, p. 17).

average unemployment benefit is only 20 percent of the average wage, so how can it make much of a difference?' And what they forgot to take into account was that the average unemployed person had a much lower pre-unemployment wage, and potentially post-unemployment wage, than the average worker. And they also failed to take into account the taxation or non-taxation of unemployment insurance which, given marginal tax rates, especially for second earners, could be a big deal. So I busied myself explaining all that—making calculations and doing a bunch of econometric studies with micro data—which I think, over time, have convinced people that it really does have a big effect." (Martin Feldstein, interview, November 21, 2013.)

In the same vein, the need for payments to the unemployed to be at levels that did not unduly deter accepting offers to work underlay Friedman's negative income tax proposal and was also the basis for his criticism of Senator George McGovern's 1972 election proposals for a negative income tax that, in Friedman's view, were too generous. However, as Friedman put it in April 1975, "I believe it is desirable to have a system under which anybody who is in distress and is assisted and enabled to have a minimum standard of life…"[68]

This mixture of attitudes on Friedman's part toward unemployment-related benefits—that provision of some benefits was desirable; that those eligible for benefits were rational to apply for them; but that benefits had become too high and, in many cases, provided too great a disincentive to seek or accept job opportunities that were available—were reflected in his numerous remarks—sometimes circumspect, sometimes acerbic—during 1975 and 1976 on the unemployment-rate/benefits relationship.

There was a "question of the *meaning* of the unemployment figures," Friedman observed in a commentary recorded in early July 1975 (Instructional Dynamics Economics Cassette Tape 171, July 1975, Part 1). Recent increases in unemployment insurance had meant that, of late, for some workers, it was "in their self-interest—and I don't blame them for doing this—to get themselves discharged rather than to leave voluntarily, and to collect unemployment insurance. In order to collect unemployment insurance, they must say that they are in the market looking for a job. The recorded number of unemployed is therefore higher than it otherwise would be."

Shortly afterward (around the time his *Newsweek* column on the subject appeared), Friedman proclaimed that the "the great expansion in government social and welfare policies have made unemployment a much more attractive status than it was before" (Instructional Dynamics

[68] Friedman (1975i, pp. 28–29).

Economics Cassette Tape 172, July 1975, Part 2). The following November, he remarked (Instructional Dynamics Economics Cassette Tape 181, November 1975, Part 2): "We have made it ... profitable for people to have the status of unemployment and therefore the numbers have tended to go up."[69] In September 1976, Friedman added that "in Britain and the United States we have made unemployment a very attractive situation. In my country, many a person can have as high an income in real terms by being employed… If there is a demand for unemployed people, as evidenced by a willingness to pay reasonable payments for being unemployed, then the supply of unemployed people will rise to meet it. Of course, there are many people who endure great hardships by being unemployed, but the numerical statistics [pertaining to total unemployment] are very misleading." (*The Times* (London), September 13, 1976.)

Unemployment benefits: the U.K. angle

The inclusion by Friedman of the United Kingdom, rather than just the United States, in the remark just given likely reflected, in part, his awareness of the increasing attention that researchers on both sides of the Atlantic were giving to supply-side reasons for the upward trend since 1966 in the U.K. unemployment rate.

Emerging findings from Daniel Benjamin and Friedman's former student Levis Kochin, ultimately published as Benjamin and Kochin (1979), argued that generous unemployment benefits were a major factor behind the United Kingdom's interwar unemployment. That the basic mechanism at work was not a new one was underscored when Friedman invited Kochin to present the Benjamin-Kochin study at the University of Chicago in 1976. It was now over twenty-five years since the spring 1951 quarter in which Friedman had launched his workshop on money and banking (see Horwich, 1964, p. 245). "We gave it at the money workshop," Kochin recalled (interview, April 23, 2013). He noted with regard to this gathering (on October 26, 1976) of the workshop: "It was very close to Friedman's retirement. And it was an extraordinary session because all the faculty who were concerned with macro turned up."[70] In time, Friedman himself would cite Benjamin and Kochin (1979) in print.[71]

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[69] Similarly, in late 1977 Friedman observed that the government had made "it profitable to be unemployed," and he added tendentiously that unemployment-insurance payments had frequently been used by recipients to finance vacations to California and Florida (*St. Louis Post-Dispatch*, December 16, 1977).
[70] The date of the workshop is provided in University of Chicago Library records.
[71] See Friedman (1989, p. 62; 1997, p. 19).

Although the paper concerned developments in 1921−1938, the Benjamin-Kochin paper was highly topical. Hamermesh (1980, p. 517) would observe: "Research on the effects of unemployment insurance (UI) on the duration of spells of unemployment has become a major growth industry in labor economics…"

The phenomenon for which Benjamin and Kochin provided evidence drawing on U.K. interwar experience was increasingly widely regarded as relevant for modern conditions too. Feldstein had been one of those documenting this in the case of the United States. But, as Benjamin and Kochin (1982) would later stress, it was also the case that the rise in U.K. unemployment from its very low mid-1960s values (with a trough of 1.2 percent in February 1966) was widely believed as partly being the consequence of enhanced generosity of unemployment benefits.[72] The enhancements were made by the U.K. government in the second half of 1966.[73] Feldstein himself had pointed to the 1966 change in UI arrangements as prompting a breakdown between the relationship between the U.K. unemployment rate and job vacancies—a result that suggested that a rise in the natural unemployment rate from the higher benefit was one reason "British unemployment began rising dramatically" under the new arrangements.[74]

Against this background, Friedman used the opportunity of a freewheeling *60 Minutes* interview on the U.K. economy's troubles to remark: "If you pay a man to be lazy, he'll be lazy. If you pay a man to be energetic, he'll be energetic…. Now, the arrangements adopted in Britain have

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[72] As well as Benjamin and Kochin's (1982) discussion of this point, see their earlier exposition in *The Banker*, February 1979 (pp. 33, 36).

[73] For the 1966 U.K. unemployment rate, see *https://fred.stlouisfed.org/series/LMUNRRTTGBM156S*.

[74] See Joint Economic Committee, U.S. Congress (1972, pp. 25, 27) and Feldstein (1973a, p. 47). The latter item is the source for the quotation.

Writing in late 1978, Dornbusch and Fischer (1980a, p. 26) made the surprising statement: "U.S. studies have shown an increase in the natural rate of unemployment in the seventies, but we are unaware of such studies for the United Kingdom. This is an issue of considerable importance that has apparently not generated much discussion in Britain." In fact, not only had prominent U.S. economists like Feldstein and Friedman highlighted the rise in the natural rate of unemployment in the United Kingdom in the 1970s, but by 1978 the matter had also been highlighted in public discussion on many occasions, including by Samuel Brittan (1977, p. 191), who referred to the "steady reduction… of the incentive to work" because of improvements in unemployment benefits, and by the National Institute of Economic Research (1977) (which was skeptical—in retrospect, unduly so—about there having been an appreciable rise in unemployment benefits). Some of the research literature had also by 1978 also suggested a rise in the natural rate of unemployment in the United Kingdom (see Trevithick, 1977, p. 67; Nelson, 2019, p. 29). Brittan (in *Financial Times* (London), April 3, 1975) also pointed to research in this direction and himself observed: "The increase in the number of unemployed associated with any given level of unfilled vacancies is the most striking development in labor market statistics in the last decade." He also suggested that "the increased level of benefits relative to normal wages, in the middle 1960s, reduced the pressure on unemployed people to take the first available job, and gave them more time to shop around, perhaps combining job search with a bit of a holiday." In an interview for this book (April 18, 2013), Brittan observed that, with regard to whether increased unemployment benefits raise the unemployment rate, his conclusion was, "Well, of course they do, [though] it doesn't mean they're a bad thing."

been arrangements in which people are paid for not working very hard. The man who works hard is no better off than the man who is lazy. The man who is lazy or doesn't work hard gets all these social benefits. If he earns a little more, if he's hardworking, that's taken away from him in taxes. What incentive is there for a man in Britain to be hardworking?" (*60 Minutes*, CBS, November 28, 1976, p. 14 of transcript.)

Unemployment benefits: the verdict

Although the U.S. unemployment rate from 1977 through mid-1982 was usually lower than the rates recorded in 1975 and 1976, it remained above levels typically seen in previous decades, and Friedman believed that unemployment benefits were a significant factor underlying the increase in the rate. "I don't blame people for collecting welfare checks," he said of recipients in a February 1977 talk. "They are fools not to do it if they are entitled to it. I blame us for being so silly as to establish a welfare system which makes it more advantageous to collect welfare than to do productive work. I don't blame them… I blame us."[75] Both in his Nobel lecture published in 1977 and in *Newsweek* (March 5, 1979), Friedman reaffirmed that higher unemployment benefits had raised the U.S. unemployment rate during the previous decade.[76] Friedman also cited the U.S. experience in a 1980 television appearance as supporting the proposition: "If you offer a higher price for being unemployed, you'll have more unemployed."[77] Two years later, he remarked: "We have been paying people not to work."[78]

As Feldstein's 2013 remarks, quoted above, indicated, since the 1970s the position that unemployment benefits are a factor affecting the natural rate of unemployment has become widely accepted. In official circles, the U.S. Department of Labor commissioned an outside study in early 1975 that that year's planned increases in unemployment benefits might add close to 1 percentage point to the unemployment rate (*New York Times*, February 25, 1975).[79] This compares with Feldstein's (1973a, p. 100) position that the U.S. unemployment rate was, in 1972, already at least 1.25 percent higher than otherwise because of disincentives from unemployment benefits. The two numbers were, taken together, broadly in accord with Friedman's statement in 1980 (on the basis of his reading of others' research) that

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[75] Friedman (1977f, p. 17).
[76] The Nobel lecture's reference (Friedman, 1977c, p. 458) would be highlighted at the start of Hamermesh's (1980) article on unemployment insurance.
[77] *Free To Choose* (U.K. television version, debate portion), BBC2, episode "Created Equal," March 1, 1980, p. 16 of transcript.
[78] Friedman (1982b, p. 57).
[79] For related discussion, see *New York Times*, March 9, 1975, and *Banker's Monthly*, March 15, 1976.

"unemployment benefits account for something like one to two percentage points of our present unemployment level." [80]

The notion that benefits had added substantially to the unemployment rate in the course of the 1970s received endorsement from officialdom, in Federal Reserve Chairman G. William Miller remarks in July 1978 to a Congressional committee. Miller remarked that the full-employment rate of unemployment had been increased by "the effects of unemployment compensation and other institutional; factors on decisions regarding work."[81] Furthermore, the modern research literature assigns an important role to benefits in affecting the unemployment rate (see, for example, Hall, 2014), with particular stress placed by Pissarides (2013) and Hagedorn, Karahan, Manovskii, and Mitman (2016) on their importance in understanding the behavior of the U.S. unemployment rate in the early years of the recovery from the 2007−2009 recession.

The *particular* importance that Friedman assigned to benefits in understanding the level of the U.S. unemployment rate in the 1975−1976 period has also been supported by later researchers. For example, in 1977 Franco Modigliani suggested that the prolonging of unemployment insurance during 1975 amounted to "making it easy to be unemployed."[82] In a similar vein, Shimer (2010, p. 73) pointed to 1975 as the historical peak of the share of unemployed workers receiving unemployment benefits, while suggesting that the extensions of unemployment insurance that created this situation were an example of "well-intentioned government interventions [that] have kept unemployed workers from putting downward pressure on wages."

Publications and research projects

During 1975 and 1976, the drawn-out work on Friedman and Schwartz's *Monetary Trends* book continued. A draft of the book manuscript's chapter on the connections between the U.K. and U.S. economies was informally circulated in 1973.[83] The same practice was followed with their draft chapter "Money and Interest Rates" in September 1976.[84] They also published a short paper, drawing on findings related to that chapter, that dealt with the emergence of an

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[80] *Free To Choose* (U.K. television version, debate portion), BBC2, episode "Created Equal," March 1, 1980, p. 16 of transcript.
[81] Miller (1978a, p. 645; p. 9 of typescript version).
[82] From Modigliani's remarks in a joint appearance with Friedman in Friedman and Modigliani (1977, p. 24). In addition, around the same time as these remarks, James Tobin called for a substantial increase in aggregate demand to address what he perceived as substantial slack, but he added, "I don't doubt that the unemployment insurance system as presently administered is faulty and gives some excess incentive for staying unemployed." (From Tobin's testimony of February 4, 1977, in Committee on Banking, Finance and Urban Affairs, 1977a, p. 193.)
[83] This manuscript was cited by Hamburger (1977a, 1977b).
[84] See Dwyer (1981).

inflation/interest rate relationship in the U.S. data since 1965.[85] In addition, Schwartz reported on the book draft's material on nominal-income adjustment and on money demand both at the Brown University conference on monetarism in November 1974 (see Schwartz, 1976) and in the *Journal of Economic History* article discussed in Chapter 2 (Schwartz, 1975). But as of December 1976—the tenth anniversary of the completion of the first full draft of the book—finalization of *Monetary Trends* remained only a remotely distant prospect.

During the 1975–1976 period, two new books by Friedman appeared in the United States. One of these, a revision and extension of 1962's *Price Theory* text, appeared in 1976. It is discussed in the next chapter. In 1975, a revised version of 1972's *An Economist's Protest* appeared. Like the earlier book, it collected most of Friedman's *Newsweek* columns, this time through October 1974. The main form in which this new book appeared was as a hardback titled *There's No Such Thing As a Free Lunch* (not a phrase due to Friedman). Small print runs of paperback versions were also issued both under that title and as *An Economist's Protest*—the latter billed as a second edition of the 1972 collection.[86] A few columns that had appeared in the 1972 collection were dropped from the *Free Lunch* compendium, and the new collection also included some non-*Newsweek* material—including some new chapter introductions and reprints of Friedman's *Playboy* interview, a September 1973 *New York Times Magazine* piece on educational vouchers, and his 1974 *Fortune* article on indexation.[87]

The year 1975 also saw new printings of some 1960s Friedman monetary writings. His paper "Money and Business Cycles" with Schwartz had been a contribution to a 1962 conference on monetary economics.[88] The proceedings of this conference, which had seen print in 1963 in the *Review of Economics and Statistics*, now were reissued as a book titled *The State of Monetary*

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[85] See Friedman and Schwartz (1976), already discussed in Chapter 2. Although the authors do not appear to have regarded this as anything but a minor publication—simply a note recording ongoing findings—it would be referenced and quoted years later in a paper in a major journal: see Barsky and Summers (1988, p. 529).
[86] See Friedman (1975a, 1975j). The 1975 collections were issued by different publishers: Open Court Publishing Company published Friedman (1975a), while Friedman (1975j) had the same publisher, Thomas Horton and Company, as that of the earlier (Friedman, 1972c) version of *An Economist's Protest*. The two collections differed also in the preamble Friedman provided: *Free Lunch* had a four-page preface dated August 2, 1974; Friedman (1975j) had a short introduction dated June 19, 1974.
[87] The dust jacket of *There's No Such Thing As a Free Lunch* highlighted the fact that the *Playboy* interview appeared in the collection, stating "Includes the *Playboy* interview" with the word "*Playboy*" set apart in a different color. The reprint of the *Playboy* interview in Friedman (1975a) (and in Friedman, 1983b) allowed the interview to be read by a wider audience. At least one economics book in the mid-1970s, however, cited the original *Playboy* interview rather than its reprint, referring to a specific page number in the interview for good measure (see Rhoden, 1976, p. 322).
[88] See Friedman and Schwartz (1963b).

Economics (though *The State of Monetary Economics in 1963* would have been a more appropriate name).[89]

Also in 1975, Fordham University Press reissued Friedman's 1960 book *A Program for Monetary Stability*. Although Amacher and Sweeney (1980, p. 301) would cite it as a revised or new edition of Friedman's book, the 1975 volume was actually a straight reprinting of the original 1960 monograph. But 1975 proved to be a good time to put *A Program for Monetary Stability* back into print. For, in that year, the key policy recommendation in the book—monetary targeting—appeared to have become official U.S. monetary policy strategy.

II. ISSUES RELATED TO DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1975–1976

THE BEGINNING OF MONETARY TARGETING

Friedman enthusiastically greeted the move in 1975 of U.S. monetary policy toward arrangements that seemed to be a stepping stone toward the constant monetary growth rule that he had long advocated.

This change came not from an initiative of the Federal Reserve, but from the U.S. Congress. Friedman had not had a high opinion of the economic understanding of the head of the U.S. House of Representative's banking committee, Wright Patman. Like Friedman, Patman was a persistent critic of the Federal Reserve's monetary policy. But, in Patman's case, this criticism was based on a cheap-money perspective. Friedman was more favorably impressed with Patman's successor, Henry Reuss, who also co-chaired the Joint Economic Committee with the aforementioned Senator William Proxmire (who had succeeded to the leadership of the Senate's own banking committee). Both Reuss and—as indicated in the previous section—Proxmire were receptive to Friedman's focus on monetary growth as a criterion for evaluating Federal Reserve policy.[90]

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[89] See National Bureau of Economic Research (1975). However, some citations of the book, and of the articles reprinted therein, have conveyed the impression that the material in the book was actually new in 1975 (see, for example, Levrero, 2019).

[90] As discussed in Nelson (2020b, Chapter 13), monetary targeting evidently appealed to Reuss (who supported from this policy from the late 1960s onward) for the contribution it could make to the stabilization of aggregate demand and for the oversight of U.S. monetary policy. He did not subscribe to Friedman's monetary view of inflation. This was true, initially at least, of Proxmire, who had written in the *New York Times* (October 18, 1970) that "the present inflation is not a demand inflation, but a wage-price-push inflation." However, the Proxmire quotation given in Section I above suggests that, by early 1974, he had likely moved closer to Friedman's overall economic views.

As discussed in Nelson (2007, p. 156; 2020b, Chapter 13), there was already sufficient Congressional interest in this issue to lead the Joint Economic Committee to begin, in the late 1960s, advocacy of a 2 to 6 percent range for the growth in the (M1) money stock. Senator Proxmire had acknowledged in 1970 that this move "was on the basis, to some extent, on the advice of Dr. Friedman and others."[91] But both houses of Congress took things a step further with House Concurrent Resolution 133, requiring that the Federal Reserve report to Congress on its "objectives and plans" for monetary growth. This resolution—passed on March 24, 1975 (Lindsey, 2003, p. 25)—made targets for monetary aggregates a more formal part of the monetary policy process. The FOMC duly responding by announcing its targets for monetary growth for the year ahead.

This requirement that the FOMC specify monetary-growth targets for the year represented a major shift from its practice in the first half of the decade. Some retrospectives (such as Bernanke, 2006a) have instead pointed to 1974 as the year in which the Federal Reserve began monetary targets. Underlying this dating is the fact that the Record of Policy Actions for the FOMC's January 1974 meeting (a document that was released publicly with a lag) recorded the Committee's hoped-for monetary-growth rates (for M1 and M2: see Federal Open Market Committee, 1974, p. 9). However, Bernanke and Mishkin (1992, p. 190) instead dated the start of monetary targeting to 1975, and accounts released by the Federal Reserve such as Wallich and Keir (1979, p. 690) and Meulendyke (1998, p. 46) similarly associated the monetary-targeting era specifically with the period, starting in 1975, of publicly-stated annual growth targets.

These assignments seem appropriate, as it was only in 1975 that, following the Congressional resolution, the FOMC started regularly expressing its monetary-growth aims in terms of targets for the whole year ahead. In contrast, the targets made in FOMC meetings in 1974 had merely referred to the one-month ahead period before the next meeting. The 1974 targets were therefore a more numerically precise and regular version of the monetary-growth goals the FOMC had set itself since 1970. The FOMC's 1973 policy directives, for example, had referred to hoped-for monetary growth rates (see Balbach and Jordan, 1974).

The fact that the pre-1975 announced targets did not express a monetary-growth objective for the FOMC longer than up to the next meeting was especially anomalous, as Federal Reserve officials frequently suggested that only over a period of several months could monetary-growth patterns reliably reflect FOMC meeting. Because the pre-1975 arrangements did not involve

[91] From Proxmire's remarks of March 18, 1970, in Committee on Banking and Currency, U.S. Senate (1970a, p. 25).

targets for monetary growth for a full year, Friedman—though fully aware of the existence of the numerical targets given during 1974 (see Section I)—did not see them as very revealing about the FOMC's longer-term plans. Information on these instead had to be pieced together from various Federal Reserve publications. Thus, in December 1974, when referring to "the tea leaves that emanate from the Federal Reserve," Friedman asked: "why do they make us read tea leaves, instead of following the ancient wisdom of Simons and the new wisdom of Lucas and Sargent and telling us clearly and simply what they plan to do?"[92] A couple of months later, he expressed concern that "they really haven't changed their basic target at all, and all this talk [by the FOMC about monetary aggregates] is just window dressing for people like Senator Proxmire, Congressman Reuss and myself." He added that he did believe that the FOMC was indeed giving more weight to monetary aggregates in its policy decisions than it had before 1970, but that the weight was still too low (*Journal of Commerce*, February 25, 1975, p. 3). It was against this background that Congress voted for Resolution 133. Friedman regarded this resolution as introducing explicit monetary targeting in the United States.[93]

Monetary strategy

The resolution was passed despite what Friedman (in *Wall Street Journal*, April 15, 1988) later called the "vigorous objections of the Fed." In documenting the Federal Reserve's opposition, Friedman would cite Burns' (1975) testimony (to the Senate's banking committee) of February 25, 1975.[94] In this testimony, Burns urged that the resolution, which was still pending a vote, not be passed. Burns objected to the focus on monetary aggregates implied by the resolution's requirement that the Federal Reserve specify target growth rates for money. But Burns' objections went beyond the references to money. In contrast to later generations' emphasis on central bank transparency, Burns took exception to the resolution's requirement that the Federal Reserve lay out its intentions regarding monetary policy. Burns (1975a, p. 154) remarked that "the Board objects to the last paragraph of [the resolution], which calls for semiannual reports to the Congress by the Federal Reserve of its plans for future monetary policy. Such a requirement could limit the flexibility of monetary policy in responding to unexpected developments, and it could undermine the capacity of the Federal Reserve to exercise its best judgment in adapting policies to changing circumstances."[95]

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[92] Friedman (1975c, p. 178).
[93] See Friedman (1978b, p. R−185).
[94] See Friedman (1982a, p. 108).
[95] In the event, in the aftermath of the passing of the resolution, it turned out that the Federal Reserve would give *four* reports a year (via testimony) on monetary policy, at roughly quarterly intervals (see Lindsey, 2003, p. 26). For example, during 1977, Burns gave testimony directed by the resolution in February, May, July, and November. The Full Employment and Balanced Growth of 1978 would change the reporting process to being semiannual.

Burns was therefore-quite apart from his suspicion of monetary aggregates—critical of what today would be called "forward guidance." For example, he believed that Federal Reserve policymakers should not talk about the future course of interest-rate policy or the likely behavior of interest rates in the period ahead.[96]

By requiring the specification of targets for monetary growth in the year ahead, Resolution 133 appeared to reduce the Federal Reserve's prerogatives in policymaking. But Friedman did not regard that as a bad thing. "I am opposed to an independent Federal Reserve," he reaffirmed at the time of the resolution's passage (*Washington Star* (Washington, D.C.), March 24, 1975).[97] In any event, he elaborated, there was an important distinction between Congress setting an annual target for monetary growth, on the one hand, and the legislature giving the Federal Reserve detailed instructions about how it should act each quarter in pursuit of that target, on the other.[98]

In this observation, Friedman was making a distinction similar to that between (possibly desirable) "instrument independence" and (likely undesirable) "target independence" of central banks, of the kind later outlined by Debelle and Fischer (1994).[99] Burns (1975a, p. 153) could point out that the Federal Reserve was subject to the Employment Act of 1946, which had been interpreted in practice as mandating both full-employment and price-stability goals.[100] However, the resolution made the requirements on the Federal Reserve much more specific, by requiring it to choose the target rate of monetary growth, subject to the requirement that the target rate be consistent with long-run price stability, and to make that target public. For Friedman, the requirement for a monetary growth target helped relieve a situation in which the Federal Reserve was, as he had put it, "subject to no very clearly defined Congressional mandate."[101]

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[96] See Fischer (2017) and Nelson (2021). See also Chapter 6 below.
[97] Reaffirming, that is, his conclusion in Friedman (1962b; 1968b, p. 125).
[98] See Friedman's testimony of January 22, 1976, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1976a, p. 2183). In that testimony, Friedman reaffirmed (on pp. 2178, 2191) his view (expressed earlier in his March 1964 Congressional testimony: see Committee on Banking and Currency, U.S. House of Representatives, 1964, pp. 1143–1144, 1152; see also the discussion in Rotemberg, 2013) that Federal Reserve policy in line with Congressional opinion would have implied fewer major monetary policy mistakes at the cost of more frequent minor mistakes. The early 1930s experience, in which Congress was a source of pressure for more expansionary policy, figured heavily in this judgment.
[99] That said, Friedman had very strong views about the appropriate choice and settings of monetary policy instruments, as discussed below.
[100] Though some commentators had over the years expressed doubt about whether price stability was embedded in the Act's "maximum purchasing power" mandate, successive policymakers, including Burns, interpreted this mandate as indeed implying an objective of price stability. In the 1960s, Friedman himself took the Federal Reserve as subject to a price-stability mandate (see Nelson, 2020a, Chapter 10), and he reaffirmed this interpretation in January 1976 Congressional testimony (Committee on Banking, Currency and Housing, U.S. House of Representatives, 1976a, p. 2178).
[101] Friedman (1968b, p. 125).

Friedman was, consequently, initially elated about Resolution 133. In *Newsweek* (June 2, 1975), he hailed the Resolution's linkage of the appropriate long-run monetary growth to longer-run growth in real potential output, and its requirements for public statements by the Federal Reserve concerning shorter-run actions, as comprising "perhaps the most important change since the Banking Acts of the mid-1930s." (*Newsweek*, June 2, 1975.)

But Friedman later retracted that assessment. In the early 1980s, he poured scorn on his initial euphoria and indicated that he now viewed Resolution 133 as only a "noteworthy minor step[,] rather than the major breakthrough that I had mistakenly interpreted it as being."[102] The reason for this disillusionment are discussed presently.

Monetary downturns and gradual disinflation

On one metric—the avoidance of severe declines in monetary growth—official monetary targeting apparently did have some effect on FOMC decisions. Wonnacott and Wonnacott (1979, p. 320) suggested that the nosedive in monetary growth in the second half of 1974 was what crystalized Congressional support for monetary targets. On the narrow criterion of avoiding another episode like this, the imposition of monetary targets was successful: over the rest of the 1970s, dips in monetary growth were less protracted than in 1974, and the period saw no recession.

But the existence of monetary targeting failed to lead policymakers to consolidate upon the 1975−1976 disinflation and, furthermore, did not prevent a renewed surge in monetary growth and a return of inflation to double digits. Burns (1975a) acknowledged that announced targets had the value of putting the Federal Reserve in a better position to resist pressures to move to an overexpansionary policy. What they did not stop, however, was the carrying-out over 1976 and 1977 of a monetary policy that *Burns* believed was anti-inflationary but that proved *in fact* to be inflation-promoting. Just as in 1970−1972, Burns during 1976−1977 believed that monetary policy was achieving a middle ground between overly-tight and overly-loose postures and that information from monetary-growth was receiving the right weight in the FOMC's decisions; but—once again, just as in 1970−1972—monetary policy in 1976−1977 was in hindsight clearly far too loose and would have been closer to the appropriate stance if control of monetary aggregates had more firmly guided FOMC decisions.

[102] Friedman (1982a, p. 108). See also Friedman and Friedman (1985, p. 108), a discussion that again quoted, and criticized the verdict of, Friedman's *Newsweek* column of June 2, 1975.

Friedman's disillusionment with the practice of monetary targeting

Indeed, as early as May 1976, Friedman confessed to being "sorely disappointed at the way in which [Resolution 133] has been converted into a meaningless exercise in public relations" (Instructional Dynamics Economics Cassette Tape 190, May 1976, Part 1). He would conclude that the targeting framework had been implemented in a way that had not led to the desired changes in monetary policy. Indeed, the fact that the Federal Reserve and other central banks found a way to reconcile monetary growth targets with policies that were contrary in substance to what Friedman recommended is likely the principal reason why in 2003 he stated that monetary targets had not been a success.[103]

In an earlier and elaborate statement of this judgment, Friedman had listed a number of aspects of monetary targeting in 1975−1979 that had "succeeded in rendering the [Congressional] requirement largely meaningless." These included two aspects, each now considered in turn: the specification of several monetary targets at once (for M1, M2, and M3); and a shifting of the base for the monetary targets.[104]

Multiple monetary targets

Resolution 133 did not specify a definition of money, but in his Congressional appearance on the proposed resolution, Burns (1975a, p. 153) noted that Senator Proxmire's explanation of the resolution had suggested that it referred to M1. However, the resolution as passed remained non-specific about the aggregate to be targeted. This lack of specificity affected how the FOMC responded to its new mandate. Proxmire would recall in 1987 that, though he had had M1 in mind in drafting the resolution, he had found that once it was passed, "Mr. Burns said, we will not only give you [targets for] M1, we will give you [targets for] M2 and M3."[105]

The Federal Reserve was always likely to have M1 among its targets. As the *Monetary History* had acknowledged, it was an M1-type aggregate that the Federal Reserve in the postwar period had regarded as "the" money supply, much more than M2, and M1 had featured in the Federal

[103] See *Financial Times* (London), June 7, 2003. See also Nelson (2007, pp. 171−172; 2016) for further discussion.
[104] See Friedman (1983a, p. 8; 1984a, p. 27). In these discussions, Friedman listed "specifying targets in terms of a range of growth rates, rather than dollar levels" and "shifting the base to which it applied its growth rates every quarter" as separate aspects of 1975−1979 monetary targets to which he objected. These two aspects can, however, be grouped under the heading of criticisms of base drift.
[105] From Proxmire's remarks of July 21, 1987, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1987, p. 58).

Reserve's discussions of monetary aggregates in the early 1970s—to such an extent that part of Friedman's critique of Burns (1973) was that Burns considered only M1 in evaluating whether monetary growth had been excessive in 1971−1972 (see Chapter 2 above). However, in its monthly non-mandated targets of 1974, the FOMC had targets for M2 as well. The Federal Reserve's 1975−1979 public targets likewise referred to both M1 and M2, but they also added a target for M3.[106]

Friedman had long favored M2. And in the 1970s (but not later), the Federal Reserve Board essentially defined M3 as M2 plus thrift deposits. Consequently, M3 in the 1970s resembled a monetary series to which Friedman had on occasion referred favorably. In addition, as discussed below, Friedman was coming to view this M3 series as a more appropriate money supply concept than the existing M2 definition. It might, therefore, perhaps be expected that the FOMC's setting of M2 and M3 targets alongside M1 targets would be something Friedman welcomed.

But this was not the case. Friedman argued that the use of targets for multiple aggregates was primarily "to obfuscate the issue and reduce accountability."[107] In this way, they were in Friedman's view "a clever device by Arthur Burns to confuse the oversight operation of the House Banking Committee" and other legislative supervisors (*Boston Globe*, April 1, 1981, p. 45). Friedman would observe of the Federal Reserve policymakers' reaction to the 1975 resolution: "They found a way to state their objectives in an ambiguous way. Nobody can really say now whether they are meeting their goals." (*The Detroit News*, March 21, 1982.)

Friedman maintained that the Federal Reserve should nominate a single aggregate as its monetary target and pursue open market operations in a manner intended to achieve that target. Although he preferred that M2 be the targeted aggregate, he also believed that an M1 target, with no other monetary targets pursued in tandem with it, was preferable to targeting a set of aggregates that included M2.[108] Multiple targets, in his view, encouraged the illusion that the Federal Reserve could or should use a variety of control techniques (such as a complicated reserve requirement structure) in order to manage different monetary targets. Multiple targets

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[106] The resolution had referred to targets for both money and credit. Other than quoting the resolution's text, Friedman largely ignored the reference to credit and, for the moment, the FOMC likewise did not specify a formal target for a credit total.
[107] Friedman (1984a, p. 36).
[108] See especially Friedman (1982a, p. 108; 1984a, p. 36). In correspondence of August 4, 1976 that he CC'd to Arthur Burns, Friedman implied that he preferred a single M1 target to the targeting of a set of aggregates that included M2 (Burns papers, Ford Presidential Library).

also produced what Friedman called the "M1, M2 game" (Instructional Dynamics Economics Cassette Tape 173, August 1975, Part 1), under which it became difficult to assess policy success when a target for one aggregate was missed while another was hit. Friedman accepted that different monetary aggregates had diverged from one another in the past, but he believed that the discrepancies between, say, M1 and M2 growth would recede if Regulation Q was removed and reserve requirements were streamlined.[109] Reserve requirements, however, continued to differ across the demand deposits that were common to M1 and M2 and those in non-M1 deposits; and interest-rate ceilings on bank and thrift retail deposits remained in force throughout the 1970s, therefore serving as a potent force promoting divergences between the aggregates.

Base drift

What came to be called "base drift" in the Federal Reserve's monetary targeting was something that Friedman identified as a problem as early as August 1975 (*Wall Street Journal,* August 21, 1975).[110] Congress' resolution had required the Federal Reserve to give a target for monetary growth for the coming twelve months. Friedman had regarded this as an important virtue of the resolution, and he contrasted it with the historical situation, in which the Federal Reserve had "never specified long-range numerical objectives" (*Newsweek*, June 2, 1975).

However, the FOMC implemented the resolution by setting targets each quarter for the year ahead. This practice partially underlay the title of a *New York Times* analysis in early 1976 titled "Monetary Policy: A Shifting Target" (January 4, 1976) as well as the analysis' conclusion that "in practice, Federal Reserve policy is almost as unpredictable as it always was."[111] However, as

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[109] On Friedman's wish to rationalize reserve requirements, see Friedman (1974d, p. 23) and his written submission for his testimony of November 6, 1975, in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1975a, pp. 47–48). He distinguished these proposals from the Federal Reserve's wish to extend reserve requirements to those commercial banks or thrifts that had opted to be outside the regulatory structure of the Federal Reserve System. Friedman felt that the Federal Reserve could easily offset by open market operations any effect on the money stock arising from nonmember bank activity (Friedman, 1974d, p. 22). The case he saw for imposing reserve requirements on these institutions was, he believed, valid only if this was part of an arrangement in which the same reserve requirement was imposed on all deposits in the aggregate targeted (Friedman, 1982a, p. 117). As discussed in Nelson (2020a, Chapter 4), he saw merit in that uniform reserve requirement being set to zero.

[110] Bordo, Choudhri, and Schwartz (1990, p. 254) gave Poole (1976) as the earliest reference on base drift, while Newton (1983, p. 103) quoted September 1976 remarks on the subject from Karl Brunner (in particular, Brunner, 1976b, pp. 11−12). But the 1975 Friedman op-ed cited here (which referred to the base drift implicit in the FOMC's July 1975 interpretation of its target) was an earlier example. Friedman did not, however, use the words "base drift" in that op-ed; he did use the term in Instructional Dynamics Economics Cassette Tape 208 (February 1977, Part 1), by which time the "base drift" terminology had been deployed in, for example, Wallich (1976, pp. 4, 5). Another early discussion of the concept was Cacy (1976a).

[111] The other, perhaps stronger, basis for the article's conclusion was that the FOMC retained the prerogative to make conscious deviations from its announced monetary targets.

Argy, Brennan, and Stevens (1990, p. 48) noted, in practice this decision-making procedure did not mean the target rate was varied each quarter. Instead, during Burns' final years in office (that is, the years of his tenure in which mandated monetary targeting was in force), the same target rate was kept over a calendar year. However, by re-expressing the target each quarter, the FOMC *did*, in effect, change the value of the money stock implied by the annual target. Its targeting practice created a situation in which a miss in one quarter became inherited in the starting point (or base) for subsequent settings of the money-stock target. That is, during the Burns years, the actual money stock in the past quarter became the reference point from which target rates of growth for the period ahead were computed (see Walsh, 1987, pp. 5–6). In 1978, the legislative requirement pertaining to the monetary targets was tightened—requiring the FOMC to fix the base for its annual target as the end of the previous year (Walsh, 1987, p. 6). Even under this arrangement, base drift continued at a lower time frequency. [112]

Instead of targets that were announced anew every quarter and expressed in growth rates, Friedman favored multi-year monetary targets, with gradually declining growth rates converted into a dollar target path for the monetary aggregate.[113] That is, he wanted the money stock to be made trend-stationary. Instead, unit-root behavior of the money stock continued, and after roughly a decade of monetary targeting John Taylor was to note that "the money supply is a highly nonstationary series."[114]

However, Walsh (1986) would defend base drift as a way in which a regime of monetary targeting could appropriately take into account permanent shifts in money demand—that is, lasting changes in the relationship governing the absolute levels of real income, real money balances, and the opportunity cost of money. McCallum (1993) provided what was, in effect, an argument complementary to Walsh's, when he contended that permanent shifts in money demand should be expected to occur continually and that the quantity theory of money should be regarded as, essentially, referring to the relationship between monetary growth and inflation—not between the corresponding levels series.

Much of Friedman's framework fits in with McCallum's characterization. We saw in Chapter 2

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[112] See also Broaddus and Goodfriend (1984) for a discussion of base drift and its practical relevance in the execution of monetary targeting by the Federal Reserve during the period from 1975 to 1984.
[113] In Friedman (1984a, p. 38), he gave a diagrammatic representation of this recommendation. By this stage (1983), Friedman was also strongly in favor of a point target rather than a target range for the aggregate. Along similar lines, Senator William Proxmire also criticized the Federal Reserve's pursuit of target bands for the aggregates (see his remarks of August 5, 1986, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1986, p. 9).
[114] Taylor (1986, p. 2016).

that Friedman did not deny that monetary velocity on occasion experienced permanent shifts. Furthermore, his policy recommendations rested primarily on the existence of reliable of growth-rate, rather than levels, relationships. There is therefore some tension between Friedman's favoring of dollar levels targets for money and his longstanding practice of focusing on growth rates of money in monetary analysis and in policy prescriptions. However, Friedman did believe that (log) M2 velocity was stationary in the period since the 1950s, so he did see quantity-theory relationships in the United States as prevailing in levels and not just growth rates. In addition, his increased insistence on levels-based targets for money also seems to have arisen from a concern that monetary-growth target misses would, in practice, be one-sided: an overshoot in period t might well be followed by an overshoot in period $t+1$.

As Wallich (1976) argued, concerns like this were not of great practical importance in the first year of monetary targeting, partly because some target misses, of mixed sign, averaged out across a number of quarters. However, persistent misses of the monetary target, and a corresponding effect of base drift on mean monetary growth, would become more important in the late 1970s.

Federal Reserve operating procedures

Neither the Federal Reserve's use of multiple monetary targets, nor the fact that it allowed the targeted aggregates to accumulate base drift, was the main source of Friedman's unhappiness with U.S. monetary targeting in practice. What was by far the greatest source of dissatisfaction for Friedman was, instead, the Federal Reserve's operating procedures. He did not believe that the Federal Reserve had subordinated the management of the federal funds rate to the stabilization of monetary growth.

Indeed, any pretense on the part of the FOMC to targeting a reserves aggregate, and not to have the federal funds rate as its operating instrument, had been largely abandoned by 1975. Even though the Federal Reserve was formally adhering to a "reserves against private deposits" (RPD) targeting procedure until 1976, it was the case that formal records of FOMC meetings, which were regularly released publicly in the mid-1970s, provided the FOMC's target band for the federal funds rate. Likewise, a November 1975 Federal Reserve Board staff memorandum (one rapidly made public, as discussed below) referred to the RPD regime in the past tense.

Informed outside commentary reflected the reality that the federal funds rate was the FOMC's instrument, one example being a news item that referred to "the key federal funds interest rate,

the rate that the Federal Reserve controls most directly" (*Washington Star-News* (Washington, D.C.), January 5, 1975).[115] In addition, to Friedman's irritation, market commentary also often focused discussions of the Federal Reserve policy in terms of its implications for interest rates. For example, one market economist said (*Bankers Monthly*, March 15, 1974): "The Federal Reserve holds the key to near-term interest rate movements."[116]

As noted in Nelson (2020b, Chapter 13), money supply theory—and, specifically, the area of central bank operating procedures—did not amount to a major research area for Friedman. It continued not to be in 1975−1976. What marked out the mid-1970s, however, was the degree to which Friedman became outspoken about the importance of operating procedures as a policy issue. This was especially the case in Congressional testimony he gave in November 1975.[117] It is worth dwelling on this matter, partly because it presaged the Federal Reserve's temporary change in operating procedures in 1979−1982, but also because it was such a sensitive issue for Friedman.

It is this matter, of all those on which Friedman dueled with central bankers and central bank economists over the years, that involved the most contentious dispute. The debates on the Fisher effect, the natural rate hypothesis, monetary policy vs. fiscal policy, and cost-push vs. monetary views of inflation, though they were larger debates in terms of their implications for economic theory and policy, were far less likely to provoke the same degree of acerbity from Friedman as that on operating procedures. During 1975−1977, the Federal Reserve's position on operating procedures magnified Friedman's feeling of having been let down by Arthur Burns. And in later years, indeed for the rest of his life—including during the 1979−1982 regime, which he considered to be very far indeed from his own recommendations—the fact that the Federal Reserve did not switch to using a total-reserves or monetary base instrument continued to be a sore point with Friedman. In July 2002, Anna Schwartz told the present author that Friedman remained resentful and unhappy on the matter—a fact underscored by his continued advocacy, in the final decade of his life, of a monetary base or reserves instrument, both in printed discussions

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[115] Similarly, the *Journal of Commerce* (March 31, 1975) observed that it was a "direct result of Federal Reserve policy" that "money market rates… have reached new [recent] lows."
[116] It is possible to defend these commentaries both because it is evident that the Federal Reserve was continuing to control the funds rate over this period and because, as stressed in Taylor (1999), regimes that do not involve an interest-rate instrument can be viewed in terms of the approximate interest-rate rule they imply.
[117] This testimony was for the Committee on Banking, Housing and Urban Affairs, U.S. Senate's (1975a) "Second Meeting on the Conduct of Monetary Policy." The first meeting in this series had been on April 29 through May 1, 1975 (Committee on Banking, Housing and Urban Affairs, U.S. Senate, 1975c). Friedman had not appeared at those first hearings. However, his former students, William Gibson and Beryl Sprinkel, and two of his former departmental colleagues, Carl Christ and William Dewald, had done so, as had Karl Brunner and Clark Warburton.

(for example, in Pringle, 2002, pp. 18–19, and in *Wall Street Journal*, August 19, 2003) and in correspondence (including in 1999 with Bennett McCallum, in 1999–2006 with the present author, and in 2006 with Gregory Mankiw).

In the mid-1970s, Friedman regarded the Federal Reserve as still overly concerned with interest rates, as noted above. But the controversy on operating procedures transcended this issue and amounted to a disagreement on the matter of what procedure—when it was agreed that monetary-growth stabilization was the policy goal—was best for achieving that goal. "The major issue has shifted, I believe, from objectives to means," Friedman testified in November 1975.[118] As the title of a late-1975 *Newsweek* Friedman column put it, the question was one of "How To Hit the Money Target."[119]

For Friedman, the fact that the putting of monetary targets on a more formal footing in 1975 was not followed by an abandonment of a federal funds rate instrument reflected conceptual errors on the part of the Federal Reserve, as well as what he regarded as an inertia that made it unamenable to a shake-up of its operations.[120] In early February, several weeks before the passing of the resolution that mandated monetary targeting, Friedman suggested that the federal funds rate instrument was "something they [the FOMC] ought to forget about."[121]

Similarly, in a newspaper interview given on February 21, 1975, Friedman remarked of current operating procedures: "It's a technique that did make sense when they were trying to control interest rates. But it makes no sense whatsoever if they really want to control the money supply. The right way to control the money supply is to forget about the fed funds rate and just decide how much you want to add to the monetary base each month." (*Journal of Commerce*, February 25, 1975, p. 3.)

To central bank economists taking the opposite position, Friedman's arguments overstated what could be achieved by a change in operating procedures—and underestimated the financial market volatility that would be associated with adoption of a bank-reserves or monetary base instrument.

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[118] From his testimony of November 6, 1975, in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1975a, p. 42; see also p. 48 of his accompanying submission). Likewise, in Instructional Dynamics Economics Cassette Tape 179 (October 1975, Part 3), Friedman remarked that the problem was now not the Federal Reserve's objectives but how to achieve those objectives.
[119] *Newsweek*, December 8, 1975.
[120] Many other monetarists took the same position as Friedman. One example is Anna Schwartz, who observed (in *Hagerstown Herald* (Maryland), March 1, 1975): "Nobody can set a price and then decide what quantity people are going to take at that price."
[121] *Wall Street Week*, Maryland Public Television, February 7, 1975, p. 18 of transcript.

And it deserves emphasis that it was indeed largely an argument between Friedman and central bank economists, rather than with academics. Tobin (1960) had come out in favor of an interest-rate instrument, but he did not contrast this with a reserves instrument, and much of his other research took the monetary base or reserves as the monetary policy instrument. Even papers that were explicitly concerned with the choice of monetary instrument (including Poole, 1970) had typically taken for granted that the central bank could control the money stock perfectly if it desired and had cast the issue as the choice between targeting money and targeting (or managing) interest rates. That was *not* the issue at stake in the 1970s operating-procedures debate. This debate concerned whether, in the context of agreement that the central bank should target the money stock, the central bank should do so via a reserves instrument or, instead, a base instrument.

The academic debate on this matter had been very light. As a Federal Reserve Board staff memorandum—written in response to Friedman's November 1975 Senate banking committee testimony—noted: "The issues involved are highly technical and detailed. While several of them have been widely debated, though not fully resolved, in scholarly journals and within the Federal Reserve System, others… have scarcely been examined by economists outside the Federal Reserve."[122] It was therefore not only not a major topic of Friedman's research, but it was also not one of the principal areas on which he was engaged in debate (in print and in conferences) with other academic economists.

The debate Friedman conducted on the matter was largely outside journals, and it took the form of other public statements as well as interactions with Federal Reserve policymakers and their staff. In this connection, Friedman would over the decade from 1975 find himself at loggerheads with his former teacher Burns, Burns' successors, and prominent Federal Reserve staff—including two former graduate students of his who now, as Board staff economists provided much of the analytical firepower against Friedman's position on operating procedures. These two were Stephen Axilrod (at the Board since 1952) and David Lindsey (a Ph.D. student of Friedman's who joined the Board from academia in July 1974). The Board staff memorandum prompted by Friedman's November 1975 Congressional testimony was, however, unsigned.

This memorandum raised many of the issues that Axilrod and Lindsey would highlight in their later public-domain work and that would feature in the debate on operating procedures in both the United States and the United Kingdom in the late 1970s and early 1980s. The memorandum

[122] Federal Reserve Board Staff (1976, p. 121).

focused on the looseness of the reserves/money relationship in the short run, as well as the possible financial disruptions that could arise from a less-accommodative arrangements for the short-run provision of reserves to banks. In addition, the memorandum indicated continued reservations, like those Chairman Martin had expressed to Friedman in 1969, about the actual desirability of the stabilization of monetary growth.[123] "In any event," the memorandum stated (Federal Reserve Board Staff, 1976, p. 127), "it is by no means obvious that it is economically desirable to severely limit shorter-run variations in money growth." What is more, the memorandum also took a position that would recur constantly in arguments against a reserves-oriented operating regime: insofar as tighter reserves control delivered tighter monetary growth control, this would be associated with "short-run movements in interest rates of much larger dimensions than have occurred in the past."

The memorandum was made available to the U.S. Senate's Committee on Banking, Housing and Urban Affairs and made public in 1976, and Friedman submitted a rejoinder to the Committee, declaring that the memorandum to consist of "vintage Federal Reservese."[124] Indeed, little in it would have surprised him, as it articulated the same arguments he had been hearing in his ongoing dialogue with the Federal Reserve—a long series of exchanges in what he would call (*Newsweek*, July 24, 1978) "three decades of personal involvement with the [S]ystem."

In his November 1975 testimony, Friedman had readily granted that for the 1974−1975 period, interest rates would indeed need to have fluctuated still further if closer control of monetary growth was to be achieved.[125] But he regarded this extra variation as contributing to economic stability: the severity of the recession would have been reduced, as the 1974 collapse in monetary growth would have been avoided. Failure to let interest rates fall fast enough in 1974 had the unintended effect, Friedman contended, of "convert[ing] the minor recession of 1973 and 1974 into a major recession from 1974 to 1975."[126] Friedman went on to suggest that the FOMC subsequently reached a position of holding interest rates too low, thereby producing excessive monetary growth in mid-1975. He suggested that the growth of the money stock had subsequently softened as the Federal Reserve kept the federal funds rate up at a time when the

[123] For discussion of these Friedman-Martin exchanges, see Friedman (1982a) and Nelson (2020b, Chapter 15).
[124] See Friedman (1976d, p. 130). "Federal Reservese" never caught on as a phrase. Instead, "Fedspeak" has become the prevalent term (see Bernanke 2004a).
[125] In addition, in his testimony of January 22, 1976, in Committee on Banking, Currency and Housing (1976a, p. 2182), Friedman said that the Federal Reserve "doesn't adjust its federal funds target fast enough to keep pace with the market." In this context, market meant market *forces* rather than market interest rates; Federal Reserve policy actions could ensure that actual federal funds market interest rates conformed to the Federal Reserve's target.)
[126] From his testimony of November 6, 1975, in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1975a, p. 38).

private sector's demand for credit was again subsiding, and that now (early November 1975) the FOMC was back to cutting rates, but not fast enough to keep monetary growth on target.[127]

Generally, however, Friedman felt that the Federal Reserve overstated the interest-rate volatility likely to occur under a regime that used reserves as the policy instrument and focused single-mindedly on stabilizing monetary growth. The analyses given by Federal Reserve officials and of their staff tended to treat all drivers of money demand other than short-term interest rates as fixed in the short run—an assumption that implied that there was a bivariate, inverse relationship between variability in monetary growth and the volatility of short-term nominal interest rates. Friedman conceded that greater control of monetary growth meant permitting greater interest-rate flexibility, especially on a day-to-day and week-to-week basis.[128] But he believed that, beyond this time frequency, the monetary-growth variability/interest-rate volatility relationship became more complex, as other variables came into play. In particular, the interest-rate swings associated with a policy of stabilizing monetary growth would be moderated by the reaction of other endogenous variables that could move in the short run; these variables' movements played a role in equating money supply and money demand. Such variables included expected future real income, expected future price levels, and the current prices of securities other than short-term assets.[129]

In addition, as Friedman wrote in his rejoinder: "Note that in practice short-term rates have been highly variable. If the Fed has indeed deliberately accepted variability in monetary growth to avoid variability in rates, it has gotten very little in return. The end result has been neither stable monetary growth nor stable rates."[130]

Friedman criticized the "trust the Fed" tone of the staff memorandum.[131] However, he later paid it, along with the numerous others that he had received from Federal Reserve Board staff over the years, the backhanded compliment that they read like "cleverly written briefs" by lawyers for the defense (*Newsweek*, July 24, 1978).

The Burns/Friedman relationship in the era of monetary targeting

Arthur Burns recognized the validity in principle of Friedman's critique of Federal Reserve

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[127] From his testimony of November 6, 1975, in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1975a, pp. 38–39).
[128] See, for example, Friedman (1980a, para. 18, p. 59; p. 56 of 1991 reprint).
[129] See Nelson (2020a, Chapter 6) for further discussion of this Friedman position.
[130] Friedman (1976d, p. 131).
[131] Friedman (1976d, p. 130).

operating procedures. In 1976, for example, Burns observed that "efforts by the Federal Reserve to sustain particular interest rates could result in inappropriate rates of growth in bank reserves and money."[132] As Friedman saw it, however, when FOMC policymakers recognized that such a situation was emerging, they changed their target value for the federal funds rate target, but too slowly. What he saw as flowing from this reaction process were stretches of time in which monetary growth and interest rates moved in tandem—rising together or falling together—as the inadequate movements that policymakers permitted to occur in interest rates failed to change the direction in which monetary growth was going (Instructional Dynamics Economics Cassette Tape 110, November 1, 1972; *Business Week*, August 11, 1975).

This position amounted, of course, to a criticism of the soundness of the interest-rate decisions of Burns' FOMC. However, for all his continuing criticisms of Federal Reserve policy, Friedman was (as indicated in Section I above) more subdued in late 1975 and throughout 1976 in his direct criticism of Arthur Burns. Friedman's hard-hitting August 1975 *Wall Street Journal* op-ed on Burns' "doubletalk" was not followed by similar columns in the *Journal* or *Newsweek* over the rest of the year or in 1976. In his vigorous criticisms of operating procedures, Friedman's emphasis on "bureaucratic inertia" (in *Newsweek*, June 14, 1976*c*, and in his November 1975 testimony in Committee on Banking, Housing and Urban Affairs, U.S. Senate, 1975, p. 45) as the source of resistance to an abandonment of a federal funds rate target allowed Burns off the hook somewhat.[133] In Congressional testimony in January 1976, Friedman remarked, "Dr. Burns is a former teacher of mine and a long-time friend, so I would certainly grant that I have been very much influenced [by him]."[134] In a similar vein, the two corresponded in a friendly manner during the course of 1976, even before Friedman's Nobel award in October of that year provided an occasion for another pleasant exchange.[135] Friedman himself paid tribute to Burns in an autobiographical essay written in connection with his receipt of the award in Stockholm in December 1976: "Arthur Burns shaped my understanding of economic research, introduced me to the highest scientific standards, and became a guiding influence on my subsequent career."[136]

Perhaps Friedman softened his direct criticism of Burns in 1976 because, although he was already convinced inflation would rise in coming years, he thought that the rise could be

[132] From Burns' written answers in Joint Economic Committee (1976, p. 377).
[133] Also taking the edge off Friedman's criticisms in his November 1975 testimony was the fact that that testimony pitted him against Paul Samuelson, who thus became, instead of Burns, the figure whom Friedman challenged on that occasion.
[134] From Friedman's testimony of January 22, 1976, in Committee on Banking, Currency and Housing (1976a, p. 2192).
[135] See the correspondence over this period in the Arthur Burns papers, Gerald Ford Presidential Library.
[136] Friedman (1977m, p. 239; p. 263 of 1992 reprint).

contained if monetary policy was moderate in the period ahead, and that he should provide backing for Burns against the many Keynesian economists calling for greater monetary expansion. Perhaps he also reasoned that a better relationship with Burns could help bring the Federal Reserve around to Friedman's position on operating procedures. In any event, the relationship between the two of them was clearly away from its nadir of 1970–1974, and the foundations had been laid for the fuller restoration of their friendship that would occur in the late 1970s.

In contrast, Anna Schwartz, after almost three decades as Burns' employee, seemed to relish the fact that she could now be uninhibited in her criticism of Burns—as she was from 1973 onward, as a member of the Shadow Open Market Committee founded by Karl Brunner and Allan Meltzer. An indication of her views on Burns' record to date as Federal Reserve Chairman was provided by her statement in an interview in early 1975: "We would not have had such a severe recession, and such a prolonged one, had the Fed been doing its job properly." (*Hagerstown Herald* (Maryland), March 1, 1975.)

Friedman continued to interact with Burns directly from time to time in the mid-1970s. As well as keeping in touch through their occasional correspondence, the two had face-to-face encounters in this period, including in front of Federal Reserve Board staff at the meetings of the Board's consultants panels. Friedman attended such meetings on June 22, 1973, December 12, 1974, June 24, 1975, and April 15, 1976. It was likely at the 1974 or 1975 meeting that one of the most-talked-about direct exchanges between Burns and Friedman occurred. Burns had developed excellent relationships with personnel in the Ford Administration (see, for example, Wells, 1994, pp. 145–146) and, according to Paul Samuelson, had acquired prestige as one of the most respected figures in Washington D.C., in light of Watergate and the fact that President Ford was still establishing himself as the nation's leader (*Newsweek*, March 3, 1975). After the discussion of monetary policy had concluded, Burns said to the assembled consultants that it was well known that he had standing in government circles and asked if there was any matter on which the consultants thought he should engage with decisionmakers. Friedman replied: "If I were you, Arthur, I'd stick to monetary policy." (John Scadding, interview, January 7, 1992.)[137]

The Bach Committee

Friedman's participation at the Board's consultants meeting of April 15, 1976, proved to be his

[137] Scadding indicated that he heard this account from a Federal Reserve colleague who was present at the meeting. In June 1998, the same incident was separately recounted to the present author by Dale Henderson.

final appearance on the panel in Burns' tenure as Chairman. On this occasion, instead of discussing current monetary policy, Friedman's role was primarily to participate in discussion of the forthcoming report by The Advisory Committee on Monetary Statistics (Bach and others, 1976).

This committee, although commissioned and published by the Federal Reserve Board and drawing on assistance from Board staff, consisted of academic economists; and Friedman, as discussed below, credited himself with suggesting such a committee in 1970. The committee was chaired by George Leland Bach (hence the committee being known as the Bach Committee), the longtime organizer of the Federal Reserve Board's consultants panels. The other members of the Bach Committee were Phillip Cagan, Clifford G. Hildreth, Franco Modigliani, and Arthur Okun.

The background of the report was dissatisfaction with the Federal Reserve's data on monetary aggregates. The perennial issue of what to include in the various monetary definitions had been given added importance by the apparent speeding-up of financial innovations. In addition, the issue of accuracy of measurement of money had been brought to the fore by the fact that the Federal Reserve's elevation in 1970 of monetary aggregates in its monetary policy discussions had been followed by a number of years in which monetary aggregate data appeared differently in retrospect from their initial estimates. In particular, the data for 1969 were revised upward in 1970, and those for the early 1970s were also revised upward substantially, on net.

A measure of the heightened interest in monetary aggregates was provided by the occasion of the first major revision, in December 1970. "A year ago it wouldn't have happened," one financial journalist noted (*Christian Science Monitor* (Boston), December 5, 1970). "Federal Reserve Board officials invite a dozen or so reporters to a background discussion on a revision of the money supply figures."

Even before this revision, Friedman had recommended to Arthur Burns that the Board appoint an independent committee to study the collection of monetary data.[138] A few years later, by which time the money growth data for 1971 and 1972 had also been subsequently revised up, Friedman remarked caustically: "It has long seemed to me little short of scandalous that the money supply figures should require such substantial and frequent revision."[139] The Bach Committee and

[138] This was in November 26, 1970, correspondence to Burns, as cited in Friedman (1982a, p. 107) and alluded to in his *Newsweek* column of July 24, 1978.
[139] Friedman (1974d, p. 23).

Friedman's participation in it was announced on January 31, 1974.[140] Its report was published in mid-1976.[141]

Despite his many other activities at the time, Friedman played an active role as a member of the Bach Committee. His copious correspondence in connection with the subject of monetary statistics during the years of the committee's existence underline the fact that—notwithstanding his drift over the previous three decades away from heavily technical work—there was still an element of the statistician in Friedman. His correspondence got deep "in the weeds" (about the mechanics of seasonal adjustment, in particular) and featured a considerable amount of handwritten formulas and tables on the subject. Indeed, although his own monetary research typically was most concerned with annual or still-lower-frequency data, the interaction of seasonality and monetary analysis continued to be an interest of Friedman's even after the Bach Committee's work was done. In the 1980s, this interest manifested itself in his agreeing to be a referee for the submitted version of Mankiw, Miron, and Weil (1987) (Jeffrey Miron, interview, June 20, 2013).

The main topic confronting the Bach Committee was, however, not seasonality, but the categories of financial assets that should appear in the definition of money. And here, notwithstanding Bach's status as committee chairman, Friedman was something of a back-seat driver in the Committee. The final report has numerous examples of its writing style and many of his previously-expressed views about how to define money. Just as Friedman and Schwartz's *Monetary Statistics* had recommended defining money primarily in terms of assets that the private sector regarded as largely equivalent, so did the Bach Committee. Thus, the committee recommended excluding negotiable certificates of deposit from the M2 definition on the grounds that, though formally classified as time deposits, these assets were *de facto* (bank-issued) commercial bills. The report also recommended that the Board publish an official monetary base series. The practice up to that point had been for the Board not to do so, with others—most notably the Federal Reserve Bank of St. Louis—constructing a monetary base series (and adjust the series for reserve requirements) from Board-published information.[142]

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[140] See Friedman (1974d, p. 23; 1982a, p. 107).
[141] The report was itself dated June 1976. However, the report was summarized in an article in the *Federal Reserve Bulletin* (May 1976) that also indicated that the report was already available to the public. Friedman gave the report's date as "early 1976" (Friedman, 1982a, p. 107), and indeed it was likely essentially complete by the time Bach and Friedman discussed the report in the April 1976 closed-door meeting at the Federal Reserve Board.
[142] Jordan (1977, p. 125) stressed this recommendation and endorsed it, observing that "it is about time that the central bank of the U.S. also acknowledged the concept by publishing the data."

One recommendation that the Bach Committee made had a counterpart in a change from the 1960s to the 1970s in Friedman's own thinking. He and Schwartz had excluded thrift institutions' deposits from M2. But, by the mid-1970s, Friedman indicated that he was now inclined to put them into an M2-type definition.[143] Customers' access to thrift accounts gave them considerable scope to withdraw funds on demand. Indeed, some monetary analysts felt that thrift accounts were more easily accessible on short notice—and so had greater similarity to demand deposits—than the retail time deposits included in M2.[144] A further key development in this area, occurred during the early 1970s, when thrift institutions in the New England area started offering Negotiable Orders of Withdrawal (NOW) accounts. New England's commercial banks were subsequently permitted to issue such accounts, starting in January 1974 (Hafer, 1984, p. 21). The growth in popularity of these accounts received considerable coverage in the U.S. media in the mid-1970s (for example, *Detroit Free Press*, December 2, 1975). NOW accounts essentially amounted to interest-bearing checking deposits.

The Bach Committee came out in favor of redefining M1 and M2 in terms of asset characteristics, rather than in terms of whether the issuer was a bank or a thrift institution. Not only did this line up with the Friedman-Schwartz vision of defining money from a demand perspective, it also led the committee to recommend that most thrift deposits be included in M2.

The Federal Reserve Board's official redefinition of monetary aggregates in 1979−1980 (Hafer, 1980; Simpson, 1980) largely reflected recommendations the Bach Committee had made with regard to the definition of money. In particular, the monetary base was added to the Board's list of official monetary aggregates, the M1 and M2 definitions were defined in terms of class of asset rather than on a bank/nonbank distinction (so NOW-type thrift-issued liabilities became part of M1, and a larger category of thrift deposits came into M2), and the criterion for excluding certain commercial bank liabilities (like large certificates of deposit) from M2 was based on the notion that M2 should be a retail aggregate (see Whitesell and Collins, 1996, and Nelson, 2020b, Chapter 14).

It seemed, however, that Friedman was left unhappy at the end of the day. He acknowledged that the report had provided the impetus for the redefinition of money. But he viewed the main recommendations of the report as not having been adopted, mainly because its recommendations concerning seasonal adjustment (a matter on which, as already noted, Friedman spent a great

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[143] See Friedman (1975d, p. 60) and Instructional Dynamics Economics Cassette Tape 144 (April 17, 1974), and his testimony of January 22, 1976, in Committee on Banking, Currency and Housing (1976a, p. 2182).
[144] See, for example, Charles Partee's remarks in American Bankers Association (1979, p. 145).

deal of energy) were not implemented.[145]

The "missing money" controversy

Alongside this ongoing debate about money supply and monetary control, there was much dispute about money demand behavior in the years 1975–1976.

A celebrated study by Goldfeld (1973) estimated dynamic money demand functions on postwar U.S. quarterly data and reported that the estimated relationships were well behaved. However, this study was followed by a 1976 article by Goldfeld, titled "The Case of the Missing Money," in which Goldfeld's 1973 M1 demand equation was shown to have overpredicted real money balances since 1974. Goldfeld's (1976) finding became a mainstay of intermediate macroeconomics textbook discussion during the 1980s (see, for example, Dornbusch and Fischer, 1981b, pp. 234–237). The notion also became quite widely accepted among researchers and professionals that U.S. money demand stability had come to an end soon after 1973. Furthermore, a number of former students of Friedman's, including Phillip Cagan, David Laidler, and John Scadding, largely endorsed the finding of a mid-1970s breakdown of the money demand function (see Judd and Scadding, 1982; Laidler, 1985; Cagan, 1987, p. 201).

Friedman himself was mentioned surprisingly little in this literature, and he was rarely called to make comment on it.[146] In a sense, he did not need to, because, though Goldfeld had reported a money demand function breakdown for M1, his 1976 study had actually affirmed the continuing constancy of an estimated M2 demand function.[147] This finding was somewhat lost in the discussion of Goldfeld's results, because policymakers and many academic economists tended, like Goldfeld, to part company from Friedman by focusing on M1 rather than M2 as the main monetary aggregate in the United States.[148]

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[145] Friedman (1982a, p. 107).

[146] Friedman and Schwartz (1982) cited Goldfeld (1973), but not Goldfeld (1976). The influence of the 1976 Goldfeld paper was felt, however, in Friedman's (1987a, p. 9) acknowledgment of "anomalies that have arisen in econometric estimates of the short-run demand for money."

[147] Goldfeld's (1973, p. 593) estimated income elasticity of M2 demand was very high—around 2—a finding that likely contributed to the low weight he gave to his M2 results. However, as the stability of M2 velocity in the mid-1970s attests, the result that M2 demand exhibited stability did not stem from setting the income elasticity appreciably above unity.

[148] As well as Goldfeld (1976), see Judd and Scadding (1982, p. 1010) on the durability of the U.S. M2 demand function beyond 1973. Puzzlingly, however, a number of authors have cited Goldfeld (1976), Judd and Scadding (1982), or later contributions to the "missing money" literature as documenting instability in the *M2* demand function—which this literature did not (for attributions of this kind, see, for example, MacDonald and Taylor, 1992, p. 195, and Rudebusch and Svensson, 2002, pp. 425–426). One study that did acknowledge that Goldfeld found M2 demand was stable was Blinder (1979, p. 188)—who, however, went on to make a seeming *non sequitur* by

For his part, Friedman put little weight on the findings of M1 demand instability. When asked in January 1977 whether the M1 or M2 demand function had shifted in recent years, Friedman replied: "Neither one. There is only a breakdown in the bad demand functions that people fit."[149] Friedman did not have confidence that money demand functions could be fitted to quarterly data, and hence he was not greatly worried when quarterly equations broke down. Stability of the long-run money demand function was what he regarded as the important criterion; fitted short-run money demand functions could too easily break down because of the complexities involved in short-run economic dynamics.

The post-1976 money demand literature gradually confirmed Friedman's position that M1 demand had not had serious instability problems during the 1970s—at least if relationships at a lower frequency than quarterly were the focus of attention. Indirect evidence that M1 demand was not behaving very badly in the mid-1970s came in the fact that, into the early 1980s, M1 growth continued to have good relationships with future rates of nominal income growth and inflation.[150] Also notable was the regularity that M1 velocity seemed to exhibit a stable trend growth rate over the 1970s—one similar to the trend growth it had exhibited in the previous decade.[151] But a more direct basis for questioning Goldfeld's instability findings came from Hamburger (1977b, 1983). Hamburger argued that Goldfeld's original (1973) specification was misspecified, because it implied an income elasticity for M1 demand well below unity—a result contrary to long-run money demand studies such as Chow (1966). Although treated skeptically by Judd and Scadding (1982, pp. 1007–1008, 1014), this critique would ultimately be strongly reinforced by long-run money demand studies of Lucas (1988a), Hoffman and Rasche (1991), and Stock and Watson (1993). All these later studies found that long-run U.S. M1 demand

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implying that, as *policymakers* were more interested in M1 than in M2 during the 1970s, the stability of M2 demand should not be deemed of interest for the purpose of studying the behavior of inflation over that period.

[149] In Friedman and Modigliani (1977, p. 24).

[150] On the survival of the relationship between M1 growth and future nominal income growth over the 1970s, see, for example, Hafer (1980). (In contrast, Robert Solow was reported in *Kansas City Star* (Missouri), January 27, 1980, claiming that the inflation and recession patterns observed in 1974 and 1975 were impossible to reconcile with a monetarist account.) The judgment that the nominal income growth/monetary growth and monetary growth/inflation relationship did not break down in the 1970s, irrespective of whether M1 or M2 is used, now appears to have become widely accepted (see especially B.M. Friedman and Kuttner, 1992).

[151] See, for example, Rasche (1987, 1990) and Poole (1988). Some of this evidence was based on estimates using the redefined M1 series (that is, the modern, post-1979, M1 definition). But the choice between old and new M1 had little bearing on Goldfeld-style demand equations, because old and new M1 growth behaved similarly through the mid-1970s. In particular, demand-for-money equations estimated on quarterly data using the Goldfeld (1973) specification continued to exhibit a break around 1974 even when the new definition of M1 was used (see Simpson and Porter, 1980, pp. 162–163, Hafer and Hein, 1982, and Goldfeld and Sichel, 1990). It is true, however, that the redefinition of M1 helped improve the stability of money demand (and the constancy of the trend in M1 velocity) in the second half of the 1970s, as that period witnessed major shifts to NOW accounts that clearly produced major discrepancies between the growth rates of the old and new M1 definitions (see Simpson, 1980).

equations with a unitary income elasticity exhibited parameter constancy as the estimation sample was extended through the 1970s and well into the 1980s.

Consequently, by the mid-1990s the professional consensus had shifted to the position that the long-run M1 demand function for the United States was stable over the decade of the 1970s, and that the shifts reported by Goldfeld's reflected his use of a too-low income elasticity and an overambitious criterion of explaining quarter-to-quarter fluctuations in real money balances.

In the mid-1970s, however, the view that money demand had become unstable was widely accepted. Indeed, *Business Week* published an article (June 7, 1976), "Is Monetarism Dead?," essentially on the strength of the money demand findings. Friedman was irritated by the omissions in the article; in particular, it overlooked the widely-acknowledged stability of M2 relationships.[152] Furthermore, he complained that he had been interviewed at length for the article, only to be barely quoted in it (Instructional Dynamics Economics Cassette Tape 193, June 1976, Part 2).

Prior even to the *Business Week* and Goldfeld articles raising its prominence, Friedman was aware of what he called an "alleged decline in the demand for money" in 1975, as this postulated decline had been prominent in the Federal Reserve's public discussions (Instructional Dynamics Economics Cassette Tape 190, May 1976, Part 1). Arthur Burns had been particularly outspoken on the matter. Burns saw the new reports of money demand instability as confirming his own long-held doubts about money/income relationships. Even in February 1975, when opposing monetary targets, Burns (1975a, pp. 153, 154) had referred to the "fact that the public's demands for currency, for demand deposits, for savings deposits, and for a host of other liquid assets are constantly changing," while pointing to M1's behavior in 1974 as confirming that "this concept of the money supply has lost much of its earlier significance."[153]

Federal Reserve Board staff studies of M1 demand would report the same sort of "missing money" phenomenon found by Goldfeld, with 1975 identified as a year in which real balances were especially short of predicted values (see Enzler, Johnson, and Paulus, 1976) and, in

[152] The stability of the M2-income relationship in the economy over this period—discussed in Chapter 2 above—was highlighted in some discussions of the time, including in Fellner and Larkins (1976)—an article that appeared in the same issue of the *Brookings Papers on Economic Activity* as Goldfeld (1976). In addition, David Meiselman testified (in a hearing held on February 4, 1975) that M2 velocity had been "essentially constant since at least 1960" (see Committee on Banking, Currency and Housing, 1975b, p. 48).
[153] Hamburger (1977b, p. 265) pointed to Paul Samuelson's Congressional testimony of November 6, 1975 (given alongside Friedman) as another early case in which the claim was advanced that the demand function for M1 had exhibited a breakdown.

February 1976, Burns (1976a, p. 121) would testify: "Increases in the turnover of money balances have been even larger than we at the Federal Reserve had anticipated." Indeed, although Clark Warburton was very much on the same side as Friedman on the issue of monetary relationships, Burns took the opportunity of correspondence with Warburton to restate his own skepticism about velocity stability. "The experience during the current economic recovery clearly illustrates the importance of velocity," Burns wrote. "The rate of turnover of M1 has shown an unusually large cyclical rise, in large part because [of] financial innovations and regulatory changes…"[154]

Parallel to the declarations of a shift in money demand were claims that the observed relationship between changes in the relationship between movements in money and in aggregate income. For example, Walter Heller (*Washington Post*, October 15, 1976) contended that "the monetarists have taken quite a beating" in 1976: nominal income growth and real income growth, he alleged, had exceeded the values that historical relationships with monetary growth would have implied. Likewise, Herbert Stein (1976, p. 73) argued that "the relation between the money supply and the national income has come completely unstuck."

In light of the fact that M1 velocity continued to keep fairly close to a roughly 3½ percent growth line over this period, Burns' emphasis on velocity instability seems overdone, even for M1. And, as Friedman emphasized when he addressed the Federal Reserve's appeals to a fall in the demand for money, that appeal could hardly be justified when M2 was considered (Instructional Dynamics Economics Cassette Tape 190, May 1976, Part 1, and *Newsweek*, June 14, 1976*c*).

Estimated equations linking nominal and real income growth to monetary growth also proved resilient over this period. For example, as discussed in Chapter 2 above, Feldstein and Stock (1994) found that the relationship between national income and (modern) M2 was constant over the 1970s. Furthermore, St. Louis-style equations linking nominal income growth to M1 (both the old and, especially, the revised definition) held up over the 1970s (Carlson, 1978; Hafer, 1980).

The likelihood that monetary growth, particularly M2 growth, remained a valid indicator over the 1970s, puts a new complexion on judgments on whether monetary policy was appropriate in 1976.

[154] Burns letter to Clark Warburton, November 5, 1976, Federal Reserve Board records.

A notable example of such judgments was Dornbusch and Fischer's (1978, p. 545) observation, with regard to monetary policy in 1975 and 1976: "It does seem that the Fed… came close to finding a path for the money stock that succeeded in financing the recovery, without putting pressure on the price level… The Fed's careful steering of an anti-inflationary course seemed to be taking the economy slowly onto a desirable path."

Although this judgment seems to hold up for FOMC policy during 1975, it in retrospect seems overly complacent regarding policy in 1976. Dornbusch and Fischer (1978) took money demand problems as clouding the signal from monetary growth in these years. Correspondingly, they interpreted the gentle decline in short-term nominal interest rates during 1976 as evidence of a moderate monetary policy stance. In retrospect, it appears that the FOMC's actions in 1976 were actually much more expansionary than the Committee intended and that Dornbusch and Fischer (1978) judged. A monetary policy in 1976 involving rising interest rates would have been more consistent with reconciling economic recovery and continued disinflation. Such a policy would have meant lower growth in M2 in 1976 than that observed, as well as slower growth in nominal income and real income over 1976 and 1977 than what was realized. But the policy would have helped avoid the excess demand and renewed inflation of the late 1970s.

Rudiger Dornbusch, who in the mid-1970s was emerging as a major member of the new generation of economists with the appearance of Dornbusch (1976) and other key studies, had been a University of Chicago student in the late 1960s and early 1970s, graduating in 1971. He briefly returned to the university as a member of the business school in 1974−1975, before joining Fischer at MIT (American Economic Association, 1981, p. 124). Dornbusch would always be a keen reader of Friedman's work, but Dornbusch's policy prescriptions in the late 1970s would, like the analysis of 1975−1976 developments in Dornbusch and Fischer (1978), diverge from Friedman's. Dornbusch's analysis during 1976−1978 would be predicated on the existence if a deep output gap that the authorities needed to eliminate by promoting quite large and rapid increases in household and business spending. Later, Dornbusch would come round to the perspective that Friedman had been articulating at the time: that, by mid-1976, the United States was back to the problem of creating too much—not too little—aggregate demand.

THE FINANCIAL CRISIS OF 1973–1975 AND ITS AFTERMATH

Somewhat lost in the accounts of the course of the U.S. economy in the mid-1970s that emphasize the outbreak of severe inflation and prolonged recession is the fact that the nation's financial system was also in deep trouble in this period. Reflecting this aspect of the situation,

study of U.S. financial developments led López-Salido and Nelson (2010) to characterize the United States as having its first financial crisis of the postwar period in 1973−1975.[155]

The tone of discussions during this period of the state of the U.S. financial system is brought out by various contributions to a conference on bank structure held at the Federal Reserve Bank of Chicago in May 1975 (a time of the year when Friedman was out of town). The preface to the conference volume (Scheld, 1975) noted that "the program dealt with the perennial, but particularly timely, issue of capital adequacy as it relates to the soundness of the banking system." The first chapter of the volume elaborated on the timeliness of the subject matter (Greenbaum and Taggart, 1975, p. 1): "Continued secular decline in recorded bank capital, heightened economic instability, and recent failures of large banks have aroused renewed interest in… capital adequacy." Another chapter (Sinkey, 1975, p. 85) opened with a reference to "recent happenings in the banking industry, namely the failures of United States National Bank of San Diego and Franklin National Bank of New York…" Still another chapter opened (Pany and Sherman, 1975, p. 226) by referring to the "unexpected failure and emergency merger of several large commercial banks."

These developments occurred against the background of a squeeze in banks' capital positions. As Greenspan (2001) would recount, large U.S. commercial banks' equity-capital-to-assets percentage reached their postwar low of 4 percent in 1974. It was during this period that Friedman observed (*Business Week*, October 12, 1974) that "the only place where I do think we have an overleveraged position—where capital is really extraordinarily low—is in the banking industry."

The financial strains on banks were also manifested in what Homan (1975, p. 265) described as "the failure since 1973 of three multi-billion-dollar banks." Here Homan's count included the aforementioned two bank failures—specifically, of U.S. National Bank San Diego and Franklin National Bank—as well as the Security National Bank, whose formal failure was averted by absorption of the institution into another bank. Homan argued that these failures had "served to undermine the public's confidence in other banks and the system itself."[156]

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[155] More recently, Baron, Verner, and Xiong (2021, pp. 83–84) correctly categorize 1973–1975 as a period of U.S. bank distress but state incorrectly that this categorization was "not previously identified by narrative-based approaches" (p. 84).

[156] In the same vein, Hirsch (1977, p. 242) observed: "The problem of banking stability came into sudden prominence in 1974... it was what the nineteenth century knew as a financial crisis."

Homan was writing in January 1975. By mid-1974, the situation was already at such a stage that one financial commentary (in *Bankers Monthly*, July 15, 1974) noted that banks were having problems rolling over their issuances of large certificates of deposit and commercial paper. This commentary added: "Financial community confidence probably is the lowest since the 1930s..." Friedman himself referred (in Instructional Dynamics Economics Cassette Tape 147, May 30, 1974) to the "great uneasiness and uncertainty about the character of the system."

About five months later, the *Federal Reserve Bulletin* (November 1974, p. 748) referred to "heightened public concern about the stability of financial institutions." And, in looking back on 1974, the Federal Reserve Board in early 1975 referred to "the weakening of confidence caused by the failure of a few banks" and referred to the public sector's and commercial banks' response: there had been "measures taken by concerned monetary authorities to preserve sound banking systems," while in "banks generally recognized that they had been expanding their operations more rapidly than was prudent in relation to their capital resources."[157]

Assessments by Federal Reserve Chairmen in subsequent years underlined the seriousness of the mid-1970s financial crisis. Testifying in March 1977, Arthur Burns referred to the "public concern which arose in 1973 and [into] 1976 about banking," and he added that "banks classified by the banking agency of our government [as being] in the 'problem' category… The number of such banks increased sharply in 1974 and 1975."[158] His successor, G. William Miller, noted of the banking situation in the four years from 1973, "That was a very traumatic period…"[159] And in 1979 remarks, Paul Volcker confirmed that the Federal Reserve had drawn up plans during 1974 for channeling funds into nonmember banks in the event that those institutions encountered "extraordinary liquidity problems."[160]

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[157] Board of Governors of the Federal Reserve System (1975, p. 39). In the same vein, Crosse and Hempel (1980, p. 326), in the third edition of a textbook on bank management, recalled "the 1974 crisis" in which the U.S. banking industry was "shaken by the failure of three large banks—U.S. National Bank, Franklin National Bank, and Security National Bank." The readers of this textbook were probably mostly unaware of the extent to which one of the authors of was recounting his firsthand experience. By the time of the textbook's appearance, Howard Crosse, a former vice president of the Franklin National Bank, had pleaded guilty to, and received probation for, helping conceal, in public records, the losses that the bank incurred in the leadup to its closure (*The Star-Ledger* (Newark, New Jersey), March 27, 1979).
[158] From Burns' testimony of March 10, 1977, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1977a, p. 24, 37).
[159] From Miller's testimony of July 27, 1978, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1978a, p. 145).
[160] From Volcker's testimony of November 13, 1979, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1980a, p. 27). Correspondingly, on October 17, 1979, when he was asked to comment on a scenario that would involve "several banks going under," Volcker partly based his rejection of the scenario on his "judgment that the banking system is in a stronger position now than it was in 1973–74," adding: "I'm not aware of

Consistent with these recollections, in 1975 the London *Financial Times* reported that the Federal Reserve was making contingency plans to provide large-scale assistance to the financial system in the event that the banking problems became still more elevated (*Financial Times* (London), March 7, 1975).

Policymaker concern was reflected also in the authorities' regulatory and supervisory postures and in their pressing for changes in prudential practices. As the Chairman of Continental Illinois Corporation—an institution based in Chicago and one that would feature heavily in a later financial crisis—observed (*Bankers Monthly*, October 15, 1974): "For probably the first time in more than 40 years[,] the financial stability of the banking system has become a matter of concern not only in the investment community at large but to the regulatory authorities as well." In this connection, one financial reporter also pointed to the fact that Burns "has been warning banks against excessive reliance on volatile short-term money [that is, wholesale deposit issuance] and has been urging them to acquire larger quantities of permanent capital." These exhortations went in the same direction of bankers' own inclination in the newly inhospitable financial environment, with Talley (1975, p. 125) observing that "even some bankers, including some very aggressive ones, nowadays seem to feel that bank capital ratios have fallen as far as they should go."

The problems of U.S. financial institutions went beyond the commercial banks proper. Friedman observed (Instructional Dynamics Economic Cassette Tape 144, April 17, 1974): "I doubt that there is a savings and loan association or mutual savings bank in this country that is not technically bankrupt."[161] Indeed, both Friedman and Alan Greenspan anticipated in 1974 that an official rescue of the thrift associations would soon be necessary.[162] This, however, did not in fact occur, as the thrifts survived on their own steam. An improvement in the thrift institutions' condition, associated with the downturn in U.S. nominal interest rates from late 1974, meant that an official rescue of them was, for the moment, forestalled.

However, strains in the financial markets continued during 1975. These were amplified by the New York City municipal government's financial problems—what Friedman called "the continuing *Perils of Pauline* of New York City" (Instructional Dynamics Economics Cassette

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any situation in the banking system comparable to the problem that was the focus of the difficulties at that time." See Joint Economic Committee (1980, p. 26).

[161] In *Washington Star-News* (Washington, D.C.), July 15, 1974 (p. A–9), Friedman was more cautious, suggesting that "some" savings-and-loan institutions were "technically insolvent."

[162] See López-Salido and Nelson (2010), American Enterprise Institute (1974, p. 41), and Friedman (1974c, p. 79).

Tape 181, November 1975, Part 2)—which came to a head in late 1975. Spreads shot up, and one commentator observed (*Bankers Monthly*, December 15, 1975, p. 2) that "banks and other investors have been much more selective in purchases of tax-exempts and non-government taxable debt obligations, resulting in yield differentials between prime and lower-rated issues that are near the widest levels in history."

The overall picture therefore is of a financial crisis in 1973−1975—one that was not on the scale of 2007−2009 in its severity, except perhaps on the dimension of the rundown of banks' capital margin, but one that nevertheless satisfies the characteristics of a financial crisis spelled out by Reinhart and Rogoff (2009)—and whose presence in the record calls into question the Reinhart-Rogoff position that the United States had no financial crisis in the postwar period until 1984.

As discussed in detail in López-Salido and Nelson (2010), the case for classifying the 1973−1975 period as one of financial crisis for the United States is reinforced by the fact that Reinhart and Rogoff classify the *United Kingdom* as having a financial crisis in 1973−1974. Yet this period saw no closures or official rescues of ordinary commercial banks in the United Kingdom, whereas the same period did see such activity in the United States, as noted above. The most prominent of these closures was the failure of the Franklin National Bank, which was declared insolvent in October 1974 after having been the twentieth-largest commercial bank in the United States at the beginning of that year (Wille, 1974, p. 1040) and which would be described by Arthur Burns as "such a large bank."[163]

In his own reaction to the situation, Friedman recognized, as indicated above, that banks had become overleveraged. The notion that a healthy bank capital margin was desirable was a persistent Friedman theme since the 1950s, and—in contrast to his hostility to many of the regulations, such as Regulation Q or variable reserve requirements, that had been justified largely for monetary-control purposes—he was receptive to the notion that a minimal capital requirement be imposed on banks for prudential purposes.[164]

Some of Friedman's statements during the 1970s on the banks' condition seemed, however, to play down the need for public-sector intervention. For example, while Franklin National was

[163] From Burns' letter (of July 27, 1977) in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1977b, p. 53).
[164] See Nelson (2013a). Of course, Regulation Q had initially had a prudential rationale—one that Friedman and Schwartz (1963a, p. 444) rejected as stemming from a misinterpretation of the 1930s downturn—but, by the 1960s, it had evolved into a monetary-control technique (with its value for this purpose being criticized by Friedman, 1970d, and defended by Tobin, 1970a).

teetering, Friedman indicated that he thought that Franklin National should be allowed to close (Instructional Dynamics Economics Cassette Tape 150, July 24, 1974). He also said of the thrift institutions (*Washington Star-News* (Washington, D.C.), July 15, 1974, p. A9): "I would let them go bust." Around the same time, Friedman also speculated (Instructional Dynamics Economics Cassette Tape 148, June 11, 1974) that it might be desirable to have a few bank failures, but not on a crisis scale, in order to induce U.S. commercial banks to raise their capital ratios.

In an important respect, however, these statements about allowing institutions to fail obscured more than they revealed, owing to the ambiguity involved in talking about the "failure" (or "closure") of a bank. By failure, Friedman in the 1970s seemed to have in mind, as did many others, letting the value of shareholders' equity in the bank dwindle or disappear. This was in contrast to the early U.S. bank failures of the 1930s, when *deposits* were allowed to be marked down in value. That is, commercial bank failures in that earlier period were associated with part of the money stock being extinguished.

The example of Franklin National—the largest bank failure in the postwar period up to that time (Litan, 1994, p. 524)—provides a clear case in point. When it was formally declared insolvent, this occurred—as Friedman's friend Walter Wriston, the Chairman of First National City Bank, noted—"without any loss to its depositors" (*Washington Star* (Washington, D.C.), April 6, 1975, p. A1). Its deposit liabilities (both retail and wholesale) were redeemed in full. This was a development of a post-New Deal practice that Friedman had commented on during the 1950s—one in which officialdom's handling of banks typically meant that depositors were unlikely to suffer losses even when a bank failed.[165]

Friedman was critical of the authorities, and the Federal Reserve in particular, for keeping Franklin National going by heavy discount-window loans during the first half of 1974, by which time its eventual closure seemed likely (*Newsweek*, July 15, 1974). He clearly would have preferred a more rapid winding-up of the bank, as already implied. But the other major part of the U.S. authorities' response—the protection of depositors—was something Friedman largely took for granted and was a move that could be justified by his own edict that the money stock in the face of financial crises. Accordingly, when he observed (*Washington Star-News* (Washington, D.C.), July 15, 1974, p. A9) that "there won't be a major financial collapse," part

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[165] See Friedman's October 1959 testimony in Joint Economic Committee (1959b, p. 3031). See also Friedman and Schwartz (1963a, p. 437). Litan (1994, p. 524), like Friedman, noted that the U.S. regulators' practice of arranging "mergers of failed banks instead of paying off their depositors and thus effectively guaranteed in full deposit accounts above the statutory insurance ceilings" dated to the 1950s. Litan saw this policy as having become explicit with the 1974 policymaker handling of Franklin National Bank. See also Tobin (1985a, p. 24).

of Friedman's confidence doubtless stemmed from his presumption that operation of large-scale deposit protection—beyond that required by law—would help insulate the overall banking system from repercussions of Franklin's problems. Indeed, a hint that Friedman did not truly advocate a *laissez-faire*-like response to the banking problems was provided in an observation that he *might* like to see market mechanisms be allowed to proceed, in the form of some institutions simply failing.[166] *Might* but not *would*: If the market solution entailed losses to shareholders but not depositors, Friedman was not disturbed. If the market solution threatened full redemption of depositors' funds, Friedman was much more congenial to public sector intervention.[167]

Nevertheless, some of Friedman's statements over this period, while recognizing and partially endorsing the likelihood of a high degree of public-sector protection of the financial system, seemed too sanguine. Foremost among these was his remark, cited earlier, that it might be useful to have some bank failures to spur the system as a whole to boost its equity cushion. As noted, Friedman qualified this statement by affirming that large-scale failures would be undesirable. But his position seemed not to appreciate the degree of interconnectedness between financial institutions—particularly in the era of large wholesale deposit markets, markets that both connected banks to each other and provided major banks with large, but volatile, private-sector sources of funds—and the associated systemic risks associated with the ripple effects of a series of failures.

Likewise, in offering criticism for the Federal Reserve's tactics concerning Franklin National Bank, Friedman overlooked the strategic success of the Federal Reserve and the other authorities in containing the scale of the systemic problems. The Franklin National Bank had experienced a run on its wholesale deposits, but a system-wide run on wholesale markets (such as would occur in 2007–2008) was most certainly avoided. A more major financial crisis was consequently forestalled, and a crisis-induced monetary contraction—that is, a decline in the deposit component of the aggregate money stock—prevented altogether.

The importance of ripple effects in the modern financial system was something Friedman

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[166] Friedman (1974c, p. 79).

[167] Likewise, Friedman's *Washington Star-News* statement that he would prefer that savings-and-loan institutions "go bust" was made in the context of comparison with a hypothetical alternative scenario in which a rescue operation protected the shareholders in these institutions from the danger of losing their investment (with this done by keeping the institutions going though government subsidies to thrifts' payments of interest to depositors). It did not signify opposition to government intervention that wound down the institutions while protecting depositors. See Nelson (2013a) and Edwards and Montes (2020) for other indications in Friedman's statements that he favored protection of depositors and other creditors, but not shareholders, in the face of a financial emergency.

seemed to have learned between the mid-1970s and the mid-1980s, when his commentaries on the Continental Illinois bank rescue and his subsequent remarks about savings and loans institutions' problems indicated strong concerns about financial-system contagion (see Chapter 14 below).

U.S. commercial banks responded to their capital squeeze in the mid-1970s by a retrenchment of their loans. The state of affairs at the end of 1975 (*Bankers Monthly*, December 15, 1975, p. 2) was summarized by a banking commentator observed as one in which, following "[t]he trauma of 1973−75," many banks, "confronted with rising provisions for loan losses, are emphasizing investment portfolio quality more than at any time since the Depression. This has been reflected in this year's sizable buildup in bank holdings of U.S. Treasuries and federal agency issues." The retrenchment thus largely took the form of a switch in banks' assets from loans to securities. In addition, growth in managed liabilities became more subdued for a time.[168]
The flipside of this was that monetary contraction was not part of the retrenchment process. One financial commentary suggested that banks were not moving to an expansion of their balance sheets in response to monetary policy's injections of reserve balances because they were rebuilding their capital positions (*The Plain Dealer* (Cleveland, Ohio), January 26, 1975). This commentary, however, seems to have been based on the misconception that the Federal Reserve had already swung to ease by January 1975. It actually only did so in that month and subsequently. As discussed in Section I, it was in early 1975 that the Federal Reserve moved to a decisively easier posture and injected reserves into the banking system at a more rapid rate than previously. In the wake of this, growth in both M1 and M2 did indeed pick up promptly and sharply. The *Monetary History*'s position that it was possible for the authorities to generate an expansion of the money stock even in the face of softness in the market for bank loans was borne out, as it had previously been in 1933−1935.

The fact of rapid monetary expansion underlay Friedman's confidence in a rapid rebound of the economy from the 1973−1975. "There has been an explosion in monetary growth in the last six months, and that means there is a great deal of steam in the boiler," Friedman observed in the third quarter of 1975 (*Business Week*, September 29, 1975). He contrasted his prediction with those of Walter Heller, Arthur Okun, Franco Modigliani, and other prominent economists, who had seen the softness of specific sectors, as well as the banks' caution about lending, as grounds for expecting that the recovery would be slow (Instructional Dynamics Economics Cassette Tape 174, August 1975, Part 2). And, as noted in Section I, the U.S. economic recovery that began in

[168] See Cacy (1976b, p. 7) and Beebe (1977, p. 22).

the first half of 1975 and continued thereafter was indeed of the *V*-shaped form that Friedman had observed as typical of the initial stage of economic expansions that came after severe recessions.[169]

III. PERSONALITIES IN DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1975–1976

ARTHUR OKUN

During the 1960s and 1970s, Friedman's views were frequently juxtaposed against those of Arthur Okun. For example, economics columnist Hobart Rowen referred to "[i]ntellectual opposites like monetarist Milton Friedman and Keynesian Arthur M. Okun" (*Washington Post*, July 24, 1977, p. G1). But, although he was one of the leading U.S. Keynesians of the 1960s and 1970s, Okun—unlike James Tobin, Paul Samuelson, Robert Solow, Franco Modigliani, and Walter Heller—had little in the way of extended exchanges with Friedman in print. Indeed, when their names appeared on the same publication in 1976, it was not as adversaries, but as coauthors of the Bach Report.[170]

Nevertheless, Friedman's and Okun's separate written contributions during the 1960s and 1970s, together with their other public statements in these decades, clearly marked one another out as a major exponent of an opposing line of thinking. However, as discussed below, over the same period Friedman and Okun absorbed concepts introduced in each other's research into their own frameworks.

An exchange the two did have in print—one that almost evolved into a lengthier exchange between them—was in 1963. Okun was one of the discussants of Friedman and Schwartz's "Money and Business Cycles" paper, presented at the 1962 conference on monetary economics that formed the basis for a 1963 issue of the *Review of Economics and Statistics* (and whose proceedings, as discussed in Section I above, would be reprinted as book in 1975). Okun (1963, p. 72) stated matters forthrightly: "I do not agree with the Friedman and Schwartz appraisal of the importance of money. I find their view of the world fascinating and stimulating, but I am not converted." His comment focused on their estimates of the demand-for-money function and

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[169] See López-Salido and Nelson (2010) and Bordo and Haubrich (2017).

[170] Later, their names also appeared alongside each other when both Friedman and Okun each provided endorsements for Gordon (1978) textbook. These endorsements were used in promotional material for the book. "One of the proudest things I achieved," Gordon remarked, was that "the launch brochure for my textbook had a quote from both Milton Friedman and Arthur Okun, right on opposite sides. And that was, for me, like a symbol of how I bridged that gap and sort of steered my way down the middle." (Robert Gordon, interview, March 21, 2013).

suggested that their case for money rested strongly on those estimates, which Okun found implausible.

In its manuscript form, Okun's comment reached Anna Schwartz who (in a letter to Friedman of September 21, 1962) suggested that they write a short rebuttal to Okun's comment. But Friedman was already on a long overseas trip when Schwartz made this suggestion, and the matter lapsed: Okun (1963) was published without an authors' reply.

Perhaps it is just as well that Friedman and Schwartz did not publish a reply on that occasion. For, though they would certainly subsequently continue to hold the position that Okun (1963, p. 72) accurately attributed to them that "brings monetary policy to the fore and pushes fiscal policy into the background," over the following years they would drop a couple of the specific elements of their "Money and Business Cycles" analysis with which Okun (1963) had found fault. In particular, they would continue to part company with Okun's (1963) implication that money was an inferior good; but they would come to view the income elasticity of M2 demand as close to unity—in contrast to their 1963 analysis, which had put the elasticity at about 1.8. Friedman's writings in the later 1960s and into the 1970s would, however, stress that his ranking of monetary policy over fiscal policy was compatible with a wide variety of values of the income and interest elasticities of money demand (see Nelson, 2020b, Chapter 13).

Early in the previous year, Friedman and Okun had had a debate in New York City at the Institutional Investors Conference (*Daily News* (New York), February 7, 1970), but the debate proceedings were not published. Friedman and Okun also came close to an exchange in print when, at the 1971 American Economic Association meetings, they both contributed papers on a December 28 session on the topic "Have Fiscal and/or Monetary Policies Failed?"[171] However, when the papers by both Friedman and Okun saw print in the *American Economic Review* a few months later, Friedman's piece did not mention Okun, and Okun's article did not refer to Friedman.[172]

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[171] See American Economic Association (1972, p. 467). This session, and in particular Friedman's contribution to it, opened the discussion of Batini and Nelson (2001)—a paper that seems to be the most cited research article on Friedman to appear in the twenty-first century and that highlighted the hitherto-neglected Friedman (1972b) symposium piece. Some nineteen years after the Batini and Nelson (2001) paper opened by discussing this session, Dimand (2020) used the session as the starting point of his own analysis. As well as conveying the incorrect impression that the session and the Friedman (1972b) article were unexamined in the research literature until his 2020 analysis, Dimand's account is marred by factual errors, as (in place of the correct 1971 date) he gives the session as being part of the 1972 American Economic Association meetings (pp. 1032−1033)—meetings Friedman did not in fact even attend—and he misidentifies Friedman's Keynesian opposite number at the session as being James Tobin, instead of Arthur Okun (p. 1033).
[172] See Friedman (1972b) and Okun (1972b).

By this point—the early 1970s—Okun's views concerning monetary policy had likely attained some appreciable, though largely unacknowledged, movement toward Friedman's own. Certainly Friedman thought this had occurred. To be sure, Okun established himself over the 1960s as a leading advocate of major multiplier effects on aggregate demand of fiscal policy measures; and in this connection he had been criticized by Friedman for, in Friedman's view, attributing national-income increases to the 1964 tax cut that should actually have been attributed to the U.S. monetary expansion that occurred alongside the tax cut.[173] But, over the same period, Okun evidently also became more sympathetic to monetary views of the business cycle than he had been in 1963.

This posture was reflected in Okun's behavior as head of the Council of Economic Advisers in late 1968 and early 1969, when he and colleagues put together the Johnson Administration's final *Economic Report of the President*. One of Okun's economist staff, Paul Wonnacott, was alarmed at a draft chapter on monetary policy by CEA member Warren Smith. "Warren Smith had a particular quirk—that he thought monetary policy didn't matter—which I thought was bizarre," recalled Wonnacott, who added that Smith's chapter "was going to take Friedman on and show how absurd Friedman's views were." Wonnacott wrote a list of objections to Smith's analysis and sent the list to Okun. "I was in to see Art on something, and when I brought the question of this chapter up, he just smiled. And that was the end of the chapter: It just got canned completely." (Paul Wonnacott, interview, May 12, 2014.) The published version of the report had a section on monetary relationships (Council of Economic Advisers, 1969, pp. 89–93) that, while critical in key respects of Friedman's findings (without mentioning him by name) and opposed to directing monetary policy toward maintaining constant monetary growth, acknowledged that money and GNP were reasonably closely related, on average. "Okun had a much more complex and reasonable position, in my opinion, than Smith did," observed Wonnacott (Paul Wonnacott, interview, May 12, 2014).

Friedman did not, however, take altogether kindly to Okun's partial adoption of his views. In a 1970 talk in London, he suggested that his challenge to the Keynesian consensus had led to a "standard pattern" like that seen in other debates. His challenge, he argued, was first ignored, then followed by efforts to "make fun of him as an extremist, a foolish fellow…" Thereafter, he said, his Keynesian opponents had followed the practice of absorbing his own views while portraying him as "an extremist, one of those fellows who says only money matters…"[174]

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[173] See Nelson (2020b, Chapters 12–13) for further discussion.
[174] Friedman (1970c, p. 22; p. 14 of 1991 reprint). Friedman had made similar remarks in *Time* magazine, December 19, 1969.

Keynesians' reaction to Friedman's monetary research did not proceed along quite these lines. Friedman had been portrayed by Keynesian critics as believing that only money matters even before much of the assimilation of his monetarist positions into Keynesian thinking had occurred. Most prominently, Okun (1963, p. 72) had characterized Friedman and Schwartz's cycles paper as arguing "for the strong view that monetary changes fully explain observed business cycles." Similarly, James Tobin gave a presentation at the American Bankers Association his review of their *Monetary History* (Tobin, 1965) by writing on the blackboard alternative positions on money—and associating with his own position the view that "MONEY MATTERS," while attributing to Friedman and Schwartz the belief that "MONEY ALONE MATTERS" (see Samuelson, 1969, p. 7). And once he had accepted a greater role for monetary policy in U.S. economic fluctuations, Okun portrayed Friedman as having been refuted, not vindicated, by the intervening years' research debate. Okun accomplished this by portraying the quantity theory of money (including Friedman's version of that theory) as inseparable from a belief in a zero interest elasticity of money demand and by pointing to recent years' studies that had confirmed that the demand for money was interest-sensitive.[175]

As early as 1964, Friedman had objected to characterizations of him as believing that money drove output to the exclusion of other factors.[176] In 1970, his frustrations with such attributions were, as indicated above, clearly evident in his lecture in London. By this time, the allegation that he believed only money mattered had become a mainstay also of Federal Reserve critiques of Friedman's work. This fact was reflected in a July variant of the lecture he would give in London. In the July draft, he remarked: "Monetarists are often caricatured by their critics as saying that 'only money matters.' This is nonsense. I am myself so tired of being charged with this view that I have a standing offer of $100 to anyone (and double [that] for a member of the Federal Reserve Board) who can find a sentence in anything I have written that can reasonably be interpreted as saying that 'only money matters.'"[177]

These frustrations also came through in print in the same year's "Theoretical Framework for Monetary Analysis." That article observed: "Not only, say our critics, do we believe that money matters, we believe that money is all that matters."[178] What is more, on this occasion he named

[175] See Okun (1970a) and the discussion in Nelson (2020b, Chapter 12).
[176] Friedman (1964e, p. 20; p. 278 of 1969 reprint).
[177] Friedman (1970e, p. 4). His grievance extended to Federal Reserve officials beyond Board members, and the same July draft took issue with the Federal Reserve Bank of New York's president, Alfred Hayes, on the same lines (p. 8). A year later, he again criticized President Hayes' characterization of the monetarist position (Instructional Dynamics Economics Cassette Tape 78, July 14, 1971).
[178] Friedman (1970a, p. 216).

names when referring to the researchers who had so characterized him, for he immediately cited Okun (1963) and Tobin (1965a).[179] This passage taking exception to the Okun and Tobin characterizations was repeated in his and Schwartz's book *Monetary Trends*—which appeared in 1982, two years after Okun's premature death at age 51.[180]

Okun and the natural rate hypothesis

Okun had initially planned to return to Yale University when he left government service in 1969, but he ended up staying in Washington, D.C. and taking a position at the Brookings Institution. Okun acknowledged that the profile of the institution's macroeconomists was overwhelmingly Keynesian.[181] "Just as it's difficult for Milton Friedman to get Keynesians at the University of Chicago, so it is difficult for us to get monetarists to come here," he would remark (*New York Times*, April 3, 1977).[182]

The Brookings Institution nevertheless emerged as a major forum for discussions of the Keynesian-monetarist debates—largely thanks to the *Brookings Papers on Economic Activity* journal that Okun coedited. The conferences underlying the successive issues of this journal featured the participation by critics of Keynesianism, including Alan Greenspan (before and after his 1974–1977 service as CEA head) and monetarist William Poole. "I was there, and David Fand was there, because Okun, particularly, wanted to have some representation from the other side," Poole recalled (interview, April 30, 2013). Indeed, one upshot of the Brookings proceedings was another notable shift of Okun's views in the direction of Friedman's.

With disarming imprecision, Kareken (1978, p. 4) referred to this shift on Okun's part when he observed: "Professor Friedman seems to have carried the day. Dr. Arthur Okun, former chair of the Council of Economic Advisers and continuing protagonist of Professor Friedman, wrote not too long ago (I have forgotten exactly where) that we are all accelerationists now." Later, Okun's former CEA staffer Paul Wonnacott provided greater precision when, in a textbook discussion, Wonnacott and Wonnacott (1979, p. 283) cited Okun (1975a, p. 356) to document their statement: "In the words of Arthur Okun, 'We are all accelerationists now.'"

[179] Although he cited a research article by Okun, Friedman may have been particularly inclined to refer to Okun in this connection because of the latter's public statement that Friedman was one of "those who take the extreme view that money is the only thing that matters" (quoted in *Austin Statesman* (Texas), January 4, 1968).
[180] Friedman and Schwartz (1982, p. 56).
[181] An exception to this pattern was the institution's employment of Friedman's former doctoral student, William Gibson. Gibson served as a fellow at the Brookings Institution from 1973 to 1975 (American Economic Association, 1978b, p. 156).
[182] In fact, by the time Okun made this remark, Friedman was no longer located at the University of Chicago.

The actual statement in Okun (1975a) was: "Clearly, the short-term Phillips curve has shifted upward. In the sense of recognizing that shift, we are all accelerationists now (to reverse Friedman's celebrated concession to Keynes)." On its own terms, this was not a very decisive acceptance of the natural rate hypothesis: even nonaccelerationist Phillips curves can imply a shifting tradeoff over time. But Okun's discussion on the previous page (Okun, 1975a, p. 355) made it clear that he did, indeed, see inflation developments in 1970 and into 1971 as vindicating the Friedman-Phelps Phillips curve and not merely the existence of increasing steepness of the Phillips curve. His highlighting of that period was also consistent with the *Brookings Papers* studies of Robert Gordon, which had endorsed the natural rate hypothesis on the basis of the accrual of data from the early 1970s.[183]

Wonnacott and Wonnacott (1979, p. 283) observed that "the concession to Friedman" by Okun in 1975 was all the more notable in view of the fact that Okun was "an economist who had originally helped to popularize the Phillips curve in the United States." Indeed, the aforementioned *Economic Report of the President* that Okun oversaw in 1969 became more well known for plotting a downward-sloping Phillips curve for the United States. Likewise, in November 1969, ten months after leaving government service, Okun (1970b) had acknowledged but rejected Friedman and Phelps' version of the Phillips curve (partly because he incorrectly took ever-rising inflation as an empirical prediction of their hypothesis) and endorsed a permanent inflation/unemployment tradeoff—a conclusion that had led his discussant to refer to "Okun's tradeoff approach."[184] Furthermore, as discussed in Nelson (2020b, Chapter 14) the 1970–1971 Brookings meetings that Okun organized came out against the natural rate hypothesis. So it was that Okun (1980, p. 166), in a talk delivered in Friedman's presence a few months before Okun's death, acknowledged that the breakdown of the 1954–1968 downward-sloping empirical Phillips curve reflected the validity of the Friedman-Phelps specification, and "most of the profession (including me) took too long to recognize that."

However, Okun's acceptance of the Friedman-Phelps specification proved ephemeral. Over the same mid-1970s period, Okun's views on modern macroeconomic developments crystalized and

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[183] See Gordon (1976a, 1976b) and the discussion in Nelson (2020b, Chapter 14).

[184] Sametz (1970, p. 185). Okun (1969, p. 182) had also, in a February 1969 speech, suggested that aggregate demand tools could remove inflation only by "destroying prosperity"—that is, by creating permanent economic slack.

For the Okun discussion of the Friedman-Phelps position, see Okun (1970b, pp. 23–24; pp. 15–16, 33–34 of 1983 reprint). Okun's discussion had quoted Friedman's (1963a) statement that inflation historically had often been steady over time as inconsistent with the natural rate hypothesis. But the natural rate hypothesis did not actually predict that inflation would inevitably rise; rather, it implied that the amount of stimulus to real economic activity from a given rate of inflation would diminish as that inflation rate came to be expected.

apparently left him believing that, although the Friedman-Phelps Phillips curve nicely described the evolution of inflation/unemployment patterns through 1970, subsequent events had rendered Phillips-curve-based analysis—including its expectations-augmented, natural-rate-reverting version—obsolete.

Thus, Okun's conversion to the accelerationist view turned out to be fleeting, as he moved to a much more hardline cost-push position and to rejection of aggregate demand restraint as a measure against inflation. This fact is brought out particularly by his numerous appearances before Congressional committees.[185]

"[T]he structure of the American economy clearly has been transformed," Okun declared in 1977 testimony, in which he also contended that "excess supply cannot break the momentum of inflation and restore price stability."[186] In contrast to Okun's November 1969 talk, which had contended that sufficiently tight aggregate demand policy was "a sure and completely reliable remedy" for inflation (Okun, 1970b, p. 8; p. 6 of 1983 reprint), and even his 1974 assessment that "demand deflation… *can* cure inflation ultimately" (Okun, 1974, p. 367), his 1977 judgment was: "Any professional economist who respects the facts must conclude, regretfully, that our 'momentum' inflation cannot be brought under control by any reasonable fiscal-monetary strategy."[187] Similarly, the following year Okun proclaimed that "fiscal and monetary restraint… would not cure the inflation of 1978."[188]

These diagnoses were encapsulated in Okun's (1979, p. 1) contention that "the chronic inflation of the seventies is a new and different phenomenon that cannot be diagnosed correctly with old theories or treated effectively with old prescriptions."

Okun's advocacy of cost-push views

The interpretation of Okun as moving over the 1970s ultimately to an embrace of hardline cost-

[185] Okun was an extremely frequent expert witness at Congressional committees during the 1970s, and he once observed: "Some of the best seminars I have ever participated in, Mr. Chairman, have been in testimony before this committee [that is, the U.S. Senate's Committee on the Budget]." (From Okun's testimony of February 22, 1978, in Committee on the Budget, U.S. Senate, 1978, p. 145.)
[186] From Okun's testimony of April 6, 1977, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1977c, p. 13).
[187] From Okun's testimony of April 6, 1977, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1977c, p. 14).
[188] From Okun's testimony of April 24, 1978, in Committee on Ways and Means, U.S. House of Representatives (1978a).

push views contrasts with what is often inferred in discussions of his well-known analysis in Okun (1978a). For example, Modigliani (1986, p. 34) portrayed Okun as accepting the long-run-vertical expectations-augmented Phillips curve, albeit with a low (though nonzero) output-gap elasticity.[189] If narratives along these lines are valid, the appropriate conclusion would be that, in the final years of his career, Okun stuck to the acceptance of the Friedman-Phelps Phillips curve that he had voiced in 1975.

Such a conclusion is, however, at variance with many statements Okun made in 1976–1980 that contradict an acceptance of a Phillips-curve framework. Indeed, in one of the final such statements—the aforementioned talk delivered in Friedman's presence—Okun (1980, p. 166) portrayed empirical Phillips-curve analysis as in disarray: "Since 1970, the Phillips curve has been an unidentified flying object and has eluded all econometric efforts to nail it down."[190] Okun (1978a) is, by contrast, a fragile basis on which to conclude that Okun endorsed the expectational Phillips curve. For in the relevant portion of that paper he was summarizing estimates from other studies: as Gordon and King (1982, p. 206) observed, this aspect of the 1978 Okun paper was actually a "survey." If, Okun (1978a) argued, *others'* estimates of the accelerationist Phillips curve were valid, they implied that efforts at disinflation relying on demand restriction would produce an inordinate amount of lost output in the interim.

It would appear that the Okun (1978a) assessment of estimates of the inflation-dividend-from-slack was part of marshaling an overall position against demand restriction against inflation—a position he also backed with harder-line arguments. The latter, harder-line, arguments seem to be closer to Okun's preferred ones. When he was describing his own central estimate of how the economy behaved, Okun implied that a given amount of slack would *not* lastingly reduce the inflation rate. That is, the reduction in inflation forthcoming from a period of slack would be *nonexistent* (and not merely small, as was the case the accelerationist-model estimates considered in Okun, 1978a). Evidently, Okun believed that *if* a Phillips curve did describe the inflation process at all levels of slack, the coefficient on expected inflation in that Phillips curve was unity (alongside a small slope on the unemployment-rate gap or output gap, capturing the

[189] Similarly, Taylor (1997, p. 279) suggested that in the 1970s in the United States the view that Friedman found himself arrayed against "was that it was hardly worth the high costs to reduce inflation, and this view was based on the expectations-augmented Phillips curve…," while James Tobin (in *New York Times*, November 11, 1979) seemingly used the Okun (1978a) summary of Phillips-curve estimates to assess the implications of demand restriction for U.S. inflation.

[190] As will be seen, this was essentially the culmination of a sequence of statements Okun had made in which he expressed pessimism about understanding inflation in terms of the demand/supply balance. It was foreshadowed as far back as the first half of 1974 in his statement: "We just don't understand inflation very well. There's something missing." (*Mobile Register* (Alabama), June 19, 1974.)

slack-to-inflation channel). But he implied that, in the period after 1970, U.S. inflation was not described by this process, and the actual process was instead asymmetric: excess *demand* could drive up inflation, but excess *supply*—negative levels of the output gap—did *not* matter for price changes after 1970.

This perspective was reflected in numerous Okun statements in the later 1970s that indicated categorically that economic slack did not matter for inflation.[191] One example: "despite the persistence of huge excess supplies, the basic inflation rate got stuck close to 6 percent, where it has essentially remained since mid-1975."[192] Another: "the facts of 1975 and 1976 should rebut the fantasies that inflation would disappear naturally in a slow recovery."[193] And still another: "we did have a double-size recession [that is, the 1973−1975 recession], and we gave a weak economy every possible chance to cure inflation, and it didn't work."[194]

A motivating force taking Okun to this position seems to have been the phenomenon of stagflation. As stressed in Chapter 2's discussion of stagflation, Friedman's framework could account endogenously for stagflation via Phillips-curve dynamics, while pure cost-push theories could also provide an account for it (though by, essentially, making inflation exogenous). The implication was, as David Laidler stressed in a mid-1970s discussion (*Financial Times* (London), November 7, 1974), the monetarist position implied that stagflation was a temporary phenomenon, it was a *permanent* phenomenon according to the strict cost-push views associated with hardline Keynesianism. In particular, as Okun observed correctly, the accelerationist model implied that a recession permanently reduced inflation (see Okun and Perry, 1978, p. 213). Consequently, as Friedman noted repeatedly during the 1970s, a period of aggregate demand restriction would initially see high inflation and recession coexist, before giving way to a noninflationary or lower-inflation expansion. It was this prediction that Okun rejected, with Okun (1978b, p. 119) stating that his thinking in recent years had been driven by "the stubbornness of the inflation rate at high unemployment rates."[195]

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[191] In addition, his posthumously published treatise on inflation affirmed (see Okun, 1981, p. 239) that "all economists" had become accelerationists in the sense that they recognized that the Phillips-curve relationship prevailing in 1954−1969 U.S. data was gone—but, in connection with his own preferred framework, it also indicated that "the vanishing (or nonvanishing) tradeoff is not a part of this analysis" (1981, p. 243).
[192] From Okun's testimony of April 6, 1977, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1977, p. 13).
[193] From Okun's testimony of March 16, 1977, in Committee on the Budget, U.S. Senate (1977, p. 87).
[194] From Okun's testimony of April 24, 1978, in Committee on Ways and Means, U.S. House of Representatives (1978a, p. 6297).
[195] See also Okun (1981, p. 242).

Cost-push factors came to dominate Okun's narrative of U.S. inflation developments. Even in the 1960s, when he adhered to the original Phillips-curve analysis, he believed that cost-push factors tended to exert a positive effect on the inflation rate. Indeed, his analysis in Okun (1970b) had criticized the Nixon Administration for abandoning wage-price guidelines, which he suggested could block "the substantial market power" of large firms and businesses" as holding up inflation.[196] Correspondingly, in 1971 Okun praised the same administration's adoption of wage and price controls and its rejection of Friedman's opposition to such measures (see Nelson, 2020b, Chapter 15).

As the 1970s unfolded, however, Okun scaled up his estimates of cost-push shocks' size, viewing the decade as characterized by "upward cost shocks" (Okun, 1978a, p. 352). True, he did acknowledge as contributors to inflation what in retrospect he judged to be the "serious mistakes of overstimulation that were made in the late 1960s and again in 1971−72."[197] But with the "prolonged period of excess demand" that this stimulation generated being over by the mid-1970s (Okun, 1978a, p. 352), Okun viewed developments in U.S. inflation in 1974−1980 very much in cost-push terms.[198]

In Okun's account, following the commodity price shocks of the first half of the 1970s, a "wage-price spiral became operative and continued to turn even when excess supply dominated the economy."[199] Okun described the situation that the United States faced as follows: "In many sectors of industry, prices are closely geared to direct costs plus a percentage markup, while wages follow a standard of equity relative to wages elsewhere or to the cost of living. With cost-oriented prices and equity-oriented wages, excess supply cannot break the momentum of inflation and restore price stability."[200]

This characterization predominated in Okun's discussions of inflation by early 1976. After Okun's presentation at a January 1976 congressional hearing, the committee's chair noted that

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[196] See Okun (1970b, p. 26; p. 16 of 1983 reprint). See also his claim that the abandonment of guidelines produced a "very marked acceleration" of prices in key industries (1970b, p. 43; p. 27 of 1983 reprint).
[197] From Okun's testimony of April 6, 1977, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1977, p. 14).
[198] In Okun (1974, p. 365), he specifically associated "the 1973−74 inflation" with excess-demand pressures and indicated that these pressures were receding. Nevertheless, Okun's assessment even in 1977 was that tight labor markets in 1973 "contributed very little to the upsurge in inflation" (Okun, 1977, p. 9).
[199] From Okun's testimony of February 5, 1979, in Committee on Ways and Means, U.S. House of Representatives (1979, p. 334).
[200] From Okun's testimony of April 6, 1977, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1977, p. 13).

Okun, like Arthur Burns, was contending that inflation in the modern Western world was insensitive to supply and demand conditions.[201]

Okun did acknowledge that agricultural prices were responsive to market conditions, including demand.[202] But this concession did not really contradict his position that excess supply had ceased to be an influence on the aggregate inflation rate, because it was often supposed that commodity market conditions were not responsive to the *level* of aggregate economic activity but only to its *growth rate*.[203] Consistent with this, Okun (1978a, p. 350) suggested that the *change* in the output gap was a likely influence on inflation via a commodity-price channel, but that the *level* of slack did not exert lasting downward pressure on inflation.[204] This belief—in an influence on U.S. inflation of the growth rate, but not the level, of the gap—was also one adhered to during the 1970s by Federal Reserve Chairman Burns (see DiCecio and Nelson, 2013, pp. 410–411).

Developments in Okun's advocacy of incomes policy

Okun's sympathy with cost-push views had a counterpart in his vigorous advocacy of incomes policies.

When the Nixon wage-price controls were expiring, Okun remarked (*Kansas City Star* (Missouri), April 30, 1974): "I think we'll be out of the controls business for a while. Then, after its bad reputation wears off, we'll be back into it. This has been the history of a lot of European countries." Furthermore, Okun believed that a return to some form of national incomes policy was, in part, desirable. In April 1977 he testified: "I think that countries that have adopted structural measures and incomes policies have found them worth having."[205] Correspondingly, around the same time Okun maintained (*Fortune*, April 1977, p. 117).: "I don't think that fiscal and monetary policy can do that job [eliminating inflation] alone. We have to use every promising tool available."

[201] See the proceedings of January 27, 1976, in Committee on the Budget, U.S. House of Representatives (1976, p. 64).
[202] See, for example, Okun's remarks in *Kansas City Star* (Missouri), April 30, 1974, and his testimony of April 6, 1977, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1977, p. 28), and his testimony of May 22, 1978, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1978a, p. 310).
[203] Specifically, the belief was that commodity demand depended on the growth rate of aggregate economic activity.
[204] This position implied that a recession, by itself, could reduce inflation, but that the disinflation would be wiped out if economic recovery was strong.
[205] From Okun's testimony of April 6, 1977, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1977, p. 36).

With regard to specific incomes policy, Okun in his aforementioned April 1974 remarks had suggested that he would prefer some statutory control of wages and prices. However, he subsequently pursued proposals for incomes policies that, as he saw it, would generate private-sector adherence through incentives rather than compulsion. Okun specifically advanced a scheme that he labeled "real wage insurance" (see Okun 1974, pp. 369–371, as well as Okun, 1977, 1978a, 1979). This plan was also placed under the heading of what was called "tax-based incomes policy" (TIP).

Variants of TIP had, as Okun acknowledged, previously been advanced by both Sidney Weintraub (a longtime proponent of cost-push ideas in the United States) and Federal Reserve Board Governor Henry Wallich. In Okun's version of TIP, firms and workers would be eligible to pay a smaller amount of federal income taxes if they agreed to adhere to limitations, laid out by the federal government, on wage and price increases.[206] Okun argued that TIP was far preferable to demand restriction, which—as already indicated—he saw as ineffective against inflation: "The American public ought not to be satisfied with 5 percent or 6 percent inflation, but if we are going to have a serious effort to reduce that inflation rate, we have to adopt new policy strategies."[207]

When in 1978 Friedman was pressed to comment on TIP idea, he was withering: "The TIP plans are another form of government wage and price control, differing only by explicitly spelling out the penalties for violations. They would prove no less monstrosities than earlier price and wage controls." He reiterated that the "one and only one cure for inflation" was demand restraint (*Newsweek*, May 29, 1978, p. 81). Around the same time as these remarks, Friedman likely expressed the same sentiment to Okun in person, when he served as discussant of Okun's presentation "Current Monetary Policy" for the Federal Reserve Board's consultants' meeting of May 11, 1978.[208]

Earlier in that year, Okun had publicly expressed indignation at what he considered the "groundless, indeed irresponsible, criticism that they [TIP arrangements] are a form of controls."[209] Okun suggested that as businesses and households could opt out of the TIP's wage and price limitations, they were not controls. However, as discussed in Chapter 2, Friedman saw little distinction between formal controls and other types of incomes policies introduced by

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[206] From Okun's testimony of February 22, 1978, in Committee on the Budget, U.S. Senate (1978, p. 149).
[207] From Okun's testimony of March 16, 1977, in Committee on the Budget, U.S. Senate (1977, p. 87).
[208] Federal Reserve Board records.
[209] From Okun's testimony of February 22, 1978, in Committee on the Budget, U.S. Senate (1978, p. 149).

governments. Whether imposed by statute or introduced using carrot-and-stick methods, incomes policies aimed to allow the government to preempt the price- and wage-setting decisions of the private sector.

Unemployment, potential output estimates, and measures of resource slack in the 1970s

The 1970s are now seen as a period for which accelerationist-style Phillips provide a creditable performance. Indeed, even simple bivariate plots of unemployment and the output gap against the change in inflation look impressive for this period (see Blanchard, 1997, p. 344, and Roberts, 2004, Figure 1). Yet, it seems that at roughly the midpoint of this period, in 1975, even economists like Okun who were receptive to Phillips-curve approaches (including the accelerationist variant) then came to turn against it.

A less dramatic rejection of Phillips-curve mechanisms came from Paul Samuelson in September 1975. Samuelson remarked that while he believed that the recession was indeed a factor that had brought inflation back into single digits, he was now inclined to think that the "primary elements" of inflation's behavior did not reflect a response to demand/supply factors but, instead, the influence of shocks to food and fuel prices, built-in momentum in wage contracts, and "the puzzling tendency of large corporations to administer their prices even in periods of weak demand" (*The National Observer*, September 20, 1975). Why were such cost-push explanations so in vogue during a period in which unemployment was high and inflation was falling?

One part of the explanation, already discussed in Section I, is that inflation exhibited a brief spike in the latter half of 1975 that seemingly defied the behavior of demand. However, the doubts about inflation's sensitivity to the output gap continued after this short-lived rise in inflation. And for an understanding of the prevalent and long-lasting nature of these doubts, the vast differences between real-time and retrospective estimates of the output gap, as documented and stressed by Orphanides (2003, 2004), appear to figure crucially. They are especially relevant to the mid-1970s period covered in the present chapter. Orphanides presented real-time estimates of the output gap that exhibited a trough of around −15 percent in 1975. This number contrasts with revised estimates, as of 2019, that indicate that the actual trough of the U.S. output gap during the 1970s was −4.7 percent (in 1975:Q2).[210]

[210] From FRED series on U.S. real GDP and U.S. potential GDP.

Orphanides (2004) also provided empirical evidence that the real-time output-gap estimates figured significantly in the estimated reaction function for the federal funds rate in the Burns-era FOMC. This is consistent with Burns' public references to assessments of economic slack in his public statements during the 1970s. It also lines up with Paul Samuelson's remark that although Arthur Burns had criticized the official estimates of the output gap when the Kennedy Administration introduced them into public discourse in the early 1960s, it was the case that "since then, the Federal Reserve has come around to using the tool of potential national product."[211]

A contrary position was laid out by Taylor (2000). Taylor emphasized the aforementioned −15 percent trough in real-time estimates of the output gap when arguing that, during the mid-1970s, "serious economists," including those in policy positions, did not take the gap estimate reported at the time seriously, notwithstanding the fact that it was prepared and issued by the U.S. government. It certainly seems to have been the case that the published official output-gap series underwent revisions at a slower rate than did policymakers' own assessments of the gap. For example, even in 1975 the official figures were not indicating that the output gap became positive at any time in 1972−1974, yet by 1975 it was widely recognized by economists—including Okun, as indicated above—that, during 1973, output had overshot its potential level.[212] That is the most clearly valid aspect of Taylor's position.

However, Taylor likely overstated leading economists' and policymakers' comprehension of the actual state of excess demand. A number of pieces of evidence indicate that leading economists actually believed that the output gap was in double digits during 1975, even if some of them did not suggest that it was quite as large as (negative) 15 percent.

Arthur Okun's statements are particularly important in this context. In July 1975 Congressional testimony, Okun compared actual real GNP with various possible trajectories potential real GNP, each of these trajectories being based on different full-employment unemployment rates. Under 4.5 percent unemployment—the consensus full-employment rate of unemployment rate during this period (see Orphanides and Williams 2005)—Okun's figure implied an output gap in

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[211] Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 198 (June 1976). See Nelson (2020b, Chapter 11) on Burns' early-1960s critique of estimates of the output gap. Examples in which Burns, as Federal Reserve Chair, referred to the existence of economic slack in the 1975−1977 period included his observation in November 1976 that "progress in unwinding inflation must remain a major objective of public policy, along with reestablishment of reasonably full employment and reasonably full utilization of our industrial capacity (Burns, 1976b, p. 910; also quoted in *Daily News* (New York), November 12, 1976).

[212] See Wonnacott and Wonnacott (1979, p. 333). In contrast, the modern CBO output gap series (as of 2013) registers output above potential for beginning in 1972:Q1 and continuing through 1974:Q2.

1975:Q2 of about −15 percent.[213] In the same testimony, Okun called for demand stimulation to generate 8 percent real output growth over the year ahead.[214]

Other leading Keynesian economists, as well as economists in U.S. policy circles, expressed similar views. For example, James Tobin described the U.S. output gap in the mid-1970s as in double digits, while Paul Samuelson called for 7 percent growth in real output in 1976 (Nelson, 2005). Congressional testimony by Walter Heller on January 28, 1975 (reprinted in Heller, 1976, p. 102) took the U.S. output gap as zero in 1973 and then widening to about 15 percent in 1975. Furthermore, the OECD-commissioned McCracken Report (McCracken and others, 1977, p. 82) used an estimate of the U.S. output gap that reached about −11 to −12 percent in the first half of 1975.

In his July 1975 testimony, Okun indicated that he did see the level of slack then prevailing as likely to reduce U.S. inflation.[215] But it is easy to see why he soon shifted to a view that inflation was insensitive to the output gap when the gap was negative. Okun believed the 1975 output gap was deeply negative and then saw the subsequent economic recovery was a disappointing one featuring a great amount of continuing resource slack. U.S. inflation fell in this recovery—but was higher than values often prevailing before 1973. Consequently, despite the decline in inflation, Okun came to see inflation and slack as disconnected in the United States.

Subsequent revisions to both output and potential output—most of them occurring long after Okun had moved over to the pure cost-push camp—did much to reconcile the behavior of inflation over the mid-1970s with that of the output gap. With regard to actual output, Okun's July 1975 testimony had put the peak-to-trough decline in U.S. real GNP at 8 percent.[216] Modern quarterly data on U.S. output (real GDP) instead put this decline at 3.1 percent. And, as already indicated, 2019-vintage Congressional Budget Office estimates of the output gap in 1975:Q2

[213] See the hand-drawn chart in the written portion of Okun's testimony of July 23, 1975, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1975c, p. 78). The retrospective attempt to generate this chart in printed form in Pechman (1983, p. 475) did not appear to be numerically identical to the original figure but also implied an output gap of around the same magnitude.
[214] From Okun's testimony of July 23, 1975, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1975c, p. 66).
[215] His testimony of July 23, 1975, in Committee on Banking, Currency and Housing, U.S. House of Representatives, (1975c, p. 66), had said that an "encouraging aspect" of the situation was that slack would bear down on inflation.
[216] See his written remarks in Committee on Banking, Currency and Housing, U.S. House of Representatives (1975c, p. 75). Similarly, shortly after the 1975 trough, financial commentator Sylvia Porter gave the estimated decline in real GNP during the 1973–1975 recession as being 7.7 percent (*Detroit Free Press*, June 5, 1975).

suggest it was about –4.7 percent rather than a double-digit gap. They also imply the output gap had narrowed to about –2 percent in the first half of 1976.[217]

In the mid-1970s, Milton Friedman had little in the way of special insights into looming revisions to U.S. output statistics. In addition, like Okun and so many others, he did not anticipate the major downward adjustments to historical estimates of potential output. Nevertheless, Friedman's sense about the state of demand in the economy during 1975 was better than Okun's.

The contrast between the perspectives of Okun and Friedman on this matter was brought out in their public comments during early 1975. As discussed in Section I, in early 1975 Paul Samuelson saw a danger of depression if the U.S. economic situation deteriorated over the course of that year. But Okun, in testimony given on February 28, 1975, presented the prospect of depression as much more imminent: "It becomes ever more likely that the history books will record this episode as a depression."[218] Friedman, in contrast, gave no credence to such concerns: "There isn't the slightest indication that the things which happened in the Great Depression are about to happen now." (*Tampa Times* (Florida), March 11, 1975.)

Friedman's implication that the output gap during 1975 was not as negative as Keynesian analysts were suggesting arose partly from his distrust of the unemployment rate as an indicator of unused resources. One aspect of his skepticism about unemployment statistics in this period has already been discussed in Section I: he believed that unemployment benefits had raised the natural rate of unemployment. On its own, this factor would narrow the output gap, by implying a lower level of potential output. But Friedman's commentaries in 1975 also foreshadowed the upward revisions to *actual* real output that would *further* narrow the estimates of the output gap. Specifically, he implied that the demand for labor was stronger than the unemployment data suggested, because the rise in unemployment in the mid-1970s was accompanied by strength in the employment-to-population ratio, as teenagers and women entered the labor force in greater numbers (*Newsweek*, August 4, 1975).[219] In July 1975, Friedman called for "looking not only at unemployment figures, but at employment figures." With regard to the behavior of the latter set of data in recent years, he observed: "If you looked only at the employment figures... you might think we were in a boom." (Instructional Dynamics Economics Cassette Tape 171, July 1975,

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[217] These calculations are based on data for the modern series in the Federal Reserve Bank of St. Louis' FRED portal.
[218] From the written portion of Okun's testimony, in Joint Economic Committee (1975b, p. 912).
[219] More recently, Erceg and Levin (2014) have advocated the use of a closely related measure (the U.S. labor force participation rate) in the construction of estimates of economic slack.

Part 1.) The recession had seen the unemployment rate reach a (then) postwar peak. In contrast, Friedman noted, on the criterion of the employment ratio, the recession was the third-mildest of the postwar period (Instructional Dynamics Economics Cassette Tape 172, July 1975, Part 2).

What Friedman later in the year called the "value of looking at employment [ratio data] as opposed to unemployment" (Instructional Dynamics Economics Cassette Tape 181, November 1975, Part 2) was, in fact, something he had expounded for several years. As early as March 1970, Friedman was proclaiming the merits of the employment/population ratio as a measure of labor market conditions, writing on that subject to his longtime NBER colleague, Geoffrey Moore, who had become the Commissioner on Labor Statistics in the Nixon Administration.[220] After leaving government service and returning to the NBER, Moore himself did much of the running on the issue, using contributions to the U.S. press to make the case for the employment ratio as a labor-market indicator (see, for example, *Wall Street Journal*, May 9, 1975, and *Washington Post*, July 25, 1975).

Friedman's commentaries in the mid-1970s therefore gave multiple grounds for believing the output gap was narrower than Okun and others supposed. However, Friedman was not ahead of Okun in perceiving another reason why the gap estimates in 1975 were exaggerated: the potential output slowdown, arising from the post-1973 step-down in long-run productivity growth. In a 1974 discussion of U.S. historical patterns, Friedman gave the long-term U.S. economic growth rate as 3 to 4 percent.[221] But in a discussion the same year, when concentrating on the period since the early 1960s, he gave U.S. potential output growth as averaging "about 4 percent" (*Newsweek*, June 24, 1974). Friedman essentially reaffirmed a number close to 4 percent when, in November 1975, he described his 3 to 5 percent rule for M2 growth as meaning that monetary growth would "roughly match the rate of growth in our productive potential."[222] He continued to give rates of 3.5 percent or 4 percent for potential output into the late 1970s.[223] This was not a very different pattern from that in Okun's assessments. A longtime proponent of the 4 percent

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[220] Friedman's letter to Moore (of March 9, 1970) on the issue was cited in an article on the employment ratio by Julius Shiskin, then Commissioner on Labor Statistics, in a December 1975 speech (see Shiskin, 1976, fn. 4). Relatedly, Friedman had argued in his *Newsweek* column of October 30, 1967, that labor force participation needed to be considered alongside the unemployment rate in judging whether conditions prevailing in the U.S. economy earlier that year fell into the category of a "full-fledged recession."
[221] Friedman (1974a, p. 13).
[222] From Friedman's testimony of November 6, 1975, in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1975a, p. 43).
[223] See, for example, Instructional Dynamics Economics Cassette Tape 175 (September 1975, Part 1), Instructional Dynamics Economics Cassette Tape 198 (September 1976, Part 1), and Instructional Dynamics Economics Cassette Tape 208 (February 1977, Part 1).

potential growth number, Okun reaffirmed that figure in July 1975 Congressional testimony. [224] Along similar lines, Okun (1977, p. 10) gave potential output growth as 3.75 percent.

These assessments also lined up with official estimates. It was not until January 1979 that the Council of Economic Advisers marked down its estimate of U.S. potential output growth from 3.5 percent to 3 percent.

Okun's law and the sacrifice ratio

The fact that Friedman's estimates of potential output growth paralleled Okun's reflected not only the fact that both economists failed to perceive the post-1973 slowdown, but also the reality that the measurement of productive capacity was not an area in which Friedman competed with Keynesians—and particularly with Okun, who had established himself as a leading researcher on the behavior of potential output.

In particular, a major reason why Friedman used the same potential-output growth estimates as Okun is that the latter economist had, in Okun (1962), done the pioneering quantitative and analytical work on constructing estimates of U.S. potential output, and Friedman largely conditioned on those findings. Certainly, Friedman had disagreements with the way Okun-style estimates of potential output had been put to use since the 1960s—for example, in forming the basis for policies aimed at targeting real variables, and in estimation of permanent-tradeoff Phillips curves. But he did not disagree with the notion of potential output and had used that concept (one that was already present in macroeconomics long before Okun's work) himself for many years in his discussions of aggregate supply (see Nelson, 2020a, Chapter 7). From this perspective, Okun's work in constructing empirical potential-output estimates for the United States was an exercise to be welcomed.

The same was true of another innovation of Okun's (1962) paper. As Okun recalled in 1978, "I have an article, [written] way back in 1961, which tried to link the relationship between unemployment and GNP. The result has held up remarkably well. It is called 'Okun's law' by some of my friends."[225] One of the users of Okun's law in the 1970s was Milton Friedman—bearing out B.M. Friedman and Wachter's (1974, p. 167) observation that Keynesians and monetarists alike relied on some version of Okun's law. The deployment of Okun's law by both

[224] See his testimony of July 23, 1975, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1975c, pp. 67, 77).
[225] From Okun's testimony of February 22, 1978, in Committee on the Budget, U.S. Senate (1978, p. 188).

these camps reflected their shared position that aggregate demand principally drove cyclical fluctuations in output. Okun's law was a numerical representation of one corollary of that position. It was a numerical representation of what Milton Friedman called the observed tendency for the "labor market to respond to changes in aggregate demand."[226]

This is not to imply that the unemployment/output relationship was not clouded in the 1970s: indeed, at the White House conference on Inflation (of September 5, 1974) that Okun and Friedman both attended, Alan Greenspan remarked: "Everyone is ready to repeal Okun's law…" However, Greenspan himself went on to defend the concept (Council of Economic Advisers, 1974, p. 20). And some of the structural changes in the labor market during the 1970s that Friedman stressed—such as the rise in the natural rate of employment and the increase in the employment/population ratio—did not necessarily fundamentally invalidate Okun's law. For Okun's law as expounded by Okun (1962) was a *first-difference* relationship, linking the percentage change in the output gap to the change in the unemployment gap.[227] An empirical first-difference relationship of this type could be quite robust in the face of one-time shifts in the natural rate of unemployment that were not incorporated into the equation. Such shifts implied a large residual for the equation for a single period—not ongoing systematic errors. In light of the robustness of Okun's law on this dimension, it is perhaps not surprising that Friedman used Okun's law both in November 1970 (before he started pointing to likely increases in the U.S. natural rate of unemployment) and in November 1975 (by which time he was strongly emphasizing recent increases in the U.S. natural rate).[228]

Even in its first-difference form, however, Okun's law—in the manner it was typically implemented in the mid-1970s—did prove to have systematic flaws. Two such flaws were very important. First, applications of Okun's law were systematically astray in imposing a 4 percent potential growth rate in output when that value had become obsolete. Secondly, the coefficient, κ, that translated output changes into unemployment changes in the Okun's-law equation, $\Delta(y_t -$

[226] Friedman (1976b, p. 237).
[227] It is true that Okun's law was sometimes empirically implemented using the assumption that it applied in levels rather than only in first differences. See, for example, its use in the construction of output-gap series in OECD (1973, p. 8). But this should be seen as an attempted extension of Okun's law rather than Okun's law itself.
[228] See Instructional Dynamics Economics Cassette Tapes 60 (November 4, 1970) and 181 (November 1975, Part 2). (In both these applications, Friedman seemed to be using a multiplier of 2 to translate from unemployment-gap changes into output-gap changes, in contrast to Okun's usual number of 3.) At roughly the same time as the first discussion, in a November 1970 talk in Chicago, Friedman (1971b, p. 62) in effect invoked Okun's law (without giving a coefficient) when he remarked that real output growth had to exceed 4 percent for economic expansion to tend to reduce the unemployment rate. He made a similar remark in Instructional Dynamics Economics Cassette Tape 63 (December 16, 1970). For further discussion of Friedman's use of Okun's law, see Nelson (2020a, Chapter 8).

$y_t^*) = \kappa(u_t - u_t^*)$, underwent a decline from about 3 to 2 after the early 1970s (see Blanchard, 1997, p. 363).[229] Indeed, this decline in the Okun's-law coefficient is believed to be one reason why the increase in the unemployment rate associated with the United States' disinflation of the early 1980s proved to be less than the empirical accelerationist Phillips-curve estimates, surveyed by Okun (1978a), supposed (see Fischer, 1985, p. 57; B.M. Friedman, 1988, p. 65).

As Taylor (1992, p. 14) observed, "lower estimates of the costs of disinflation came to be more widely accepted," and this was the case in the wake of the macroeconomic outcomes seen in the United States during the first seven years of the 1980s: a large increase in unemployment was associated with a dramatic decline in inflation, and the unemployment rate then declined in the course of a noninflationary economic recovery. In addition, as indicated above, the unemployment rate actually rose less in the course of the 1980s disinflation than was suggested by previous estimates.

This result did not, of course, overturn the fact that a temporarily negative output gap—of the kind Friedman in the 1970s insisted was an inevitable implication of a successful anti-inflation program—implied that unemployment would be unusually high (that is, u_t would be notably above its natural rate u_t^*) in a period of aggregate demand restriction. Therefore, Friedman accepted the validity of the concept of what came to be called the "sacrifice ratio" (Gordon and King, 1982, p. 206)—a temporary increase in unemployment or loss of output required to restore low inflation. Indeed, the fact that Friedman's discussions of the Phillips curve were mainly in terms of inflation and unemployment, rather than inflation and output, implied a sacrifice-ratio concept, expressed (as in Okun, 1978a) in units of the unemployment rate.[230]

Economists' lack of knowledge of economic dynamics and uncertainty about estimates of the natural rate of unemployment continued to be reasons why Friedman was doubtful that a durable, structural Phillips-curve specification could be implemented empirically. To be sure, he maintained his position that the appropriate Phillips-curve specification was an expectational version (see the next chapter). But Friedman stopped short of publishing his own estimates of the expectations-based Phillips curve, partly because that would involve estimating a measure of

[229] These estimates provided retrospective validity to Friedman's use of a lower Okun's law multiplier in the discussions mentioned in the previous footnote.
[230] Blanchard (2018, p. 98) observed that "while Friedman [1968a] referred to unemployment, he clearly had in mind output more generally," so that the propositions about inflation and unemployment in Friedman (1968a) implied parallel propositions about inflation and output. Although Blanchard did not note it, Friedman actually made this connection explicit in his writings—for example, by using the natural-output concept in Friedman (1975c) and output-gap-based Phillips curves in Friedman (1970a) and Friedman and Schwartz (1982).

economic slack. As was stressed in Chapter 2 above, the postulate that a short-run unemployment/inflation relationship existed was not something he doubted even in an era of stagflation. But, in light of the uncertainty associated with quantitative aspects of the short-run unemployment/inflation relationship, Friedman preferred to lay out a disinflation plan in terms of nominal variables alone.

When he was asked, during a visit to the United Kingdom in 1974, how much unemployment was needed to cure inflation, Friedman replied: "Nobody can answer that question and it's like the question of 'When did you last beat your wife?'"[231] He was, however, forthright and consistent in his writings and statements in making the point that a period of high unemployment and economic slack was necessary to restore price stability. Hobart Rowen—the columnist noted earlier for contrasting Friedman and Okun—highlighted Friedman's openness in acknowledging that his recommended monetary approach to reducing inflation would initially hit real economic activity.[232] Indeed, during the 1969–1971 episode Paul Samuelson had praised Friedman for accepting and explicitly acknowledging that a demand-oriented disinflation policy would entail an increase in unemployment and he contrasted this position with other, unnamed Nixon economic advisers (possibly Alan Greenspan, whom Samuelson at that point did not rate highly) (*The Great Economics Debate*, WGBH Boston, May 22, 1969). Friedman himself had spoken critically of remarks Nixon had made during the 1968 presidential campaign, as the future president had "said he could stop inflation without a recession—and you can't do that" (*Chicago Daily News*, May 21, 1971).

Friedman therefore did not shirk from acknowledging, even before the major surge in the U.S. unemployment rate in late 1974, that disinflation would indeed entail a period of unemployment. "There is no way of slowing down inflation that will not involve a transitory increase in unemployment, and a transitory reduction in the rate of growth of output," Friedman noted in September 1974. "But these costs are far less than the costs that will be incurred by permitting the disease of inflation to rage unchecked." (*The Guardian*, September 16, 1974.)

Friedman was also sporadically attentive to others' estimates of the Phillips curve and the sacrifice ratio, as the citations of Robert Gordon's 1970s work in his 1976 Nobel lecture would confirm. Prior to this, at the second Economists Conference on Inflation on September 23, 1974,

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[231] *Newsday* (U.K. television program), BBC2, September 20, 1974, p. 4 of transcript. Similar remarks that Friedman made in the same trip appeared in Friedman (1975f, p. 32) and in *Irish Times* (Dublin), September 18, 1974.

[232] See Rowen's discussion in *Washington Post*, October 15, 1976, which quoted Friedman (1973c) on the matter.

Friedman expressed curiosity about the estimates reported at the meeting of the short-run unemployment implied by a disinflation (Council of Economic Advisers, 1974, p. 308).

Perhaps reflecting those estimates, Friedman in October 1974 went so far as to nominate values of 6.5 to 7 percent for the range of the U.S. unemployment rate that would need to be tolerated for a period of two to three years "in order to achieve the goal of reducing inflation" (*Chicago Tribune*, October 18, 1974). In March 1975, after the recession had emerged and become severe, he said that the unemployment rate had overshot the rate necessary for an orderly disinflation, and he added that 1975 and 1976 would be years of falling inflation and unemployment rates, higher productivity, and decreasing interest rates (which, generally speaking, they were). However, Friedman also expressed concern that the Federal Reserve would "step too hard on the accelerator" in 1975 as it had in 1971 (as discussed in Section I above, the feared repeat of excessive easing this did indeed occur, but primarily in 1976 rather than in 1975). As for what unemployment should be if the decline in inflation was to be maintained and consolidated upon, Friedman suggested that, beyond 1975, several years of unemployment at 5 to 6 percent would be required (*Tampa Times* (Florida), March 11, 1975).

This was a revealing comment: Friedman clearly believed that such rates exceeded the natural rate. Yet, in retrospect, the baseline assessment is that the United States had a natural unemployment rate of about 6 percent over much of the 1970s (Orphanides and Williams, 2005, p. 1931). In this light, the vintage of Friedman's statement—March 1975—is important. It predated two developments. First, he was taking 1975's unemployment to be higher than it proved to be: he did not anticipate that economic recovery would begin almost immediately, in 1975:Q2 (a few months after the easing of monetary policy) instead of nearer to the end of the year. Second, Friedman did not foresee the degree of the rise in the natural rate of unemployment. The attention he drew to an uptick in the natural rate of unemployment arising from the enhancement of unemployment benefits would be prominent in his public statements later in the year and would follow developments that were still unfolding in March 1975. Friedman would, in particular, cite 1975:Q1 and 1975:Q2 as the period in which increases in unemployment insurance raised the U.S. unemployment rate (*Newsweek*, February 7, 1977; see also Section I above). It was, therefore, not until the later 1970s that Friedman became more aware of the extent of the rise in the natural rate of unemployment that had occurred.

Belated though Friedman's recognition of a higher natural unemployment rate was, it was well ahead of Arthur Okun, who in 1978 Congressional testimony declared that "the evidence today is that there is no more structural unemployment than there was in 1971."[233]

Friedman's perspective on the inflation/unemployment relationship during the 1970s also appear more far-sighted, consistent over time, and modern than Okun's. In 1969, Okun had remarked: "Just as we'd love to make omelets without breaking eggs, so we would love to correct our current price performance with no increase in unemployment. But no one in the world has a recipe for doing that."[234] Friedman himself closely echoed this sentiment nearly a decade later when, while stressing that "the economy would turn around" after an initial recession, he confirmed that a recession was indeed part of the remedy for inflation: "I don't mean to say nobody's going to get hurt. There's no way to make an omelet without breaking some eggs." (*San Jose Mercury News* (California), February 12, 1979.) In contrast, by the late 1970s Okun not only deprecated the influence of economic slack against inflation, he also believed that incomes policy allowed high inflation to be removed *without* any period of deficient aggregate demand: a complete disinflation could occur alongside "prosperity," he insisted (Okun, 1978c, p. 284). And, starting from the situation of slack that Okun saw as prevailing from 1974, incomes policy could remove inflation, while aggregate demand stimulation could eliminate slack, with the stimulus modulated so that "the economy stays out of the zone of excess demand," that is, the region of a positive output gap.[235] A period of economic slack was an inevitable and unavoidable part of Friedman's, but not of Okun's, account of the disinflation process.

RONALD REAGAN

When President Gerald Ford took office, Friedman was well disposed toward him. They had known each other since the days when Ford was a senior Congressman, through their shared connection to the American Enterprise Institute (*Chicago Tribune*, November 28, 1976). On the

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[233] From Okun's testimony of April 24, 1978, in Committee on Ways and Means, U.S. House of Representatives (1978a, p. 6341). Policy agencies seem to have assessed the full-employment rate of unemployment as 4 percent through 1970, when they raised their estimate to 4.5 percent (see Orphanides and Williams, 2005; Nelson, 2005). Then in 1977 the estimate was further raised to 4.9 percent (see Taylor, 1997, p. 278; Orphanides and Williams, 2005). Okun's (1977, p. 9) discussion indicated a retrospective acceptance that 4.9 percent, or perhaps even a slightly higher figure, was valid since the early 1970s. But an implication of his 1978 statement that the natural rate had not risen since 1971 indicated that, unlike Friedman, he did not acknowledge any further increase in the natural rate later in the 1970s.

[234] Okun (1970b, p. 8; p. 6 of 1983 reprint).

[235] From Okun's testimony of April 24, 1978, in Committee on Ways and Means, U.S. House of Representatives (1978a, p. 6296).

basis of that interaction, Friedman judged Ford "a splendid Congressman, a fine man…"[236] Upon Ford becoming president in August 1974, Friedman said he expected Ford to "take the long view of things" (*National Journal Reports*, August 17, 1974, p. 1224).

In August 1975, on the anniversary of Ford's ascendancy to the U.S. presidency, Friedman gave a generous assessment (*Milwaukee Journal*, August 11, 1975). In his judgment, Ford had "performed very well." Friedman added: "There's no doubt that he has encouraged an atmosphere of greater understanding of free enterprise in the maintenance of a free and productive society."

Nevertheless, over the course of that first year of the Ford Administration, Friedman was discouraged by the president's performance on numerous economic matters. And a little over six months after his August 1975 praise for Ford, Friedman was, in effect, calling for the president's removal from office. He was doing so by writing supportively of Ronald Reagan, Ford's challenger for the Republican party's 1976 presidential nomination.

Losing confidence in Ford

Inflation was one of the areas in which President Ford's conduct disappointed Friedman. Ford never reintroduced general wage and price controls or sought such control powers. But he nevertheless conveyed—in his own public statements, and in some of his actions—a largely nonmonetary perspective toward the sources and control of inflation. In this connection, Friedman's unhappiness with Ford's WIN program against inflation has already been noted in Chapter 2. Even after he had essentially dropped WIN, Ford irritated Friedman on the subject of inflation by failing to mention monetary policy when discussing inflation in an address to the nation (Instructional Dynamics Economics Cassette Tape 162, January 1975, Part 2).

Fiscal policy was another area in which Ford disappointed Friedman. Here one source of grievance was Ford's confidence in fiscal policy as a macroeconomic-stabilization tool. When Richard Nixon was first elected in 1968, Friedman had proclaimed that, now, in the wake of the experience of the Johnson Administration's income tax surcharge, "everybody believes" that the effect of a tax increase on inflation was negligible (*Chicago Daily News*, November 8, 1968, p. 45). Yet roughly six years later, President Ford announced a proposal in late 1974 to increase income taxes as an anti-inflation move.

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[236] *Milton Friedman Speaks*, Episode 2, "Myths That Conceal Reality," taped October 13, 1977, p. 25 of transcript.

As discussed in Section I of this chapter, clinching evidence of recession materialized in late 1974 and Ford dropped his tax-increase proposal and in early 1975 he proposed, and succeeded in getting enacted, an income tax rebate as a stimulus measure. Other than a change in sign, the action was similar to the 1968 tax surcharge, and the arguments Friedman applied against the effectiveness of that measure once more applied.[237] As noted earlier, Friedman was dismissive of the effect a tax cut might have on aggregate demand. If anything, this argument applied more strongly to a rebate than to other types of tax reduction, and in February 1975, when the rebate was being proposed, Friedman lamented (*Journal of Commerce*, March 4, 1975, p. 1; also quoted in *Participant*, April 1975): "Today it is taken as an article of faith, as sure as two and two equal four, that deficits and tax reductions are a way to fight recession. I believe there is no theoretical or empirical evidence that this is a correct proposition." Friedman's retrospective judgment was similar: the 1975 rebate was "an absurd measure" (Instructional Dynamics Economics Cassette Tape 211, April 1977, Part 1).

Alongside his misgivings about individual fiscal initiatives of the Ford Administration, Friedman was dissatisfied with progress in containing the growth of the federal budget. One way in which this growth was manifested was in the size of the budget deficit. In April 1972, Friedman, describing fiscal policy as "highly expansionary" (Instructional Dynamics Economics Cassette Tape 96, April 5, 1972), had predicted that the United States was "heading toward [budget] deficits greater than at any time in peace both in absolute terms and as a percentage of national income" (*Japan Times*, April 15, 1972). In October 1972 he had further predicted a "super gigantic" deficit for the 1973 fiscal year (*American Banker*, October 9, 1972). But, in large part because of the economic boom and the surge in inflation, the U.S. budget deficit actually was lower as a share of output in fiscal 1973 than in the immediately prior years. In fiscal years 1975 and 1976, however, federal budget deficits (according to modern data) exceeded 3 percent of GDP—the first time they had done so in the post-World War II period (Council of Economic Advisers, 2018, Table B−18).

Furthermore, to Friedman, the budget deficit remained an inadequate metric for representing the

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[237] One difference between 1968 and 1975 was that the 1968 measure was designed to affect both aggregate demand and inflation, while the 1975 tax cut was intended specifically to affect aggregate demand. Another difference concerned Friedman's respective postures toward the two tax measures. Though he regarded both as ineffective against aggregate demand, by 1975, he was becoming more inclined to invoke Ricardian equivalence, rather than crowding out, as the reason the measure would have little effect on aggregate demand. However, it would still be several years before his first reflex was not to appeal to crowding out, as Friedman would not really fully embrace the Ricardian equivalence view until the 1980s: see Nelson (2020b, Chapter 13) and Chapter 8 below. For the application of Ricardian-equivalence arguments to both the 1968 and 1975 temporary tax measures, see Kormendi and Meguire (1986).

fiscal situation: it was the size of government spending that most concerned him. With regard to public expenditure, the picture had changed greatly since Nixon's election in November 1968—an event that had led Friedman to predict "an absolute leveling off of the budget" (*Chicago Daily News*, November 8, 1968, p. 43). Matters had also altered drastically since March 1970, when the Nixon Administration economist Murray Weidenbaum (1970, p. 89) had laid out official projections indicating that real federal government purchases would fall by fourteen percent from 1969 to 1975 as the U.S. economy shifted to a post-Vietnam War footing. This decline in aggregate real federal purchases did not occur. Instead, growth in real nondefense spending swamped the decline that did take place in real U.S. defense expenditures. Friedman put this development into a longer-term perspective just before President Ford took office, by suggesting that a mechanical extrapolation of the trend since 1929 implied that total government spending (federal, state, and local), might exceed 50 percent of national income in 1988 (*Newsweek*, August 5, 1974).

Initially, Friedman hoped that Ford would create a change in course. He noted a possible contrast Ford might create with his predecessor (*National Journal Reports*, August 17, 1974, p. 1224): "While Nixon gave continuous lip service to cutting the budget, in point of fact he shifted back and forth." But when, five months later, Ford laid out budget proposals, Friedman was discouraged: "Despite President Ford's desirable and admirable emphasis on holding down government spending, even his [proposed] budget involves a very large increase in government spending." (Instructional Dynamics Economics Cassette Tape 163, February 1975, Part 1). Friedman did affirm, in commentaries later in the year, that he believed that Ford was sincere when expressing a desire to reduce taxes and diminish the role of government in the economy (Instructional Dynamics Economics Cassette Tape 176, September 1975, Part 2; *Newsweek*, October 27, 1975). Indeed, senior members of Ford's economic team expressed sentiments on these matters that lined up with Friedman's own. In particular, in April 1976 Ford's Secretary of the Treasury William Simon, who had already established a reputation for being concerned about budget deficits, stressed the argument—already long emphasized by Friedman—that high public spending could be socially and economically damaging even when it was not associated with budget deficits (see Simon, 1978, pp. 10–13).

In the face of these expressions of intent, the actual Ford record on public spending was disappointing to Friedman. Of course, budgetary outcomes, as distinct from the administration's budget proposals, reflected decisions by Congress, as well as the influence of U.S. economic performance. But Friedman was disappointed by what he saw as the president's inadequate resistance to the expansion of public spending. Historical data on U.S. federal government

outlays (that is, a total encompassing both purchases and transfer spending) show that these reached 20.6 percent of GDP in fiscal 1975 and 20.8 percent in fiscal 1976—higher than any values observed in any of the fiscal years 1947 through 1974 (Council of Economic Advisers, 2018, Table B−18).

When, in early 1976, Ford offered his proposals for the fiscal 1977 budget, Friedman concluded that, though they were being portrayed in media accounts as being tough, the appropriate characterization of them was different: on "any reasonable measure, Ford's budget is huge…" (*Newsweek*, February 9, 1976.) By this point, Friedman's criticism of the president had intensified sharply, and he was very amenable to the prospect of having Ford displaced on the Republican presidential ticket by Ronald Reagan.

Early Friedman/Reagan interactions

Although Friedman and Ronald Reagan had both played public roles in the Barry Goldwater presidential campaign of 1964, they did not meet until early 1967 (shortly after Reagan had been sworn in as Governor of California), during the months in which Friedman had a visiting position at the University of California, Los Angeles. Friedman would, however, later note that he might have influenced Reagan earlier than their first meeting: "we happen to know that Reagan read *Capitalism and Freedom* before I ever met him" (quoted in Roberts, 2006).[238]

In addition to meeting Reagan, Friedman had occasion during this sabbatical to make his first endorsement of Reagan's policy initiatives. Friedman was reported in the national media as supporting Reagan's efforts to increase fees for University of California tuition (*Fortune*, June 1, 1967). Friedman applauded what he saw as a move away from subsidizing middle-class households, and he spoke in favor of the fee increases in a public debate with UCLA's economics professor Michael Intriligator.[239]

With regard to his first meeting with Reagan, Friedman said, "I was very, very favorably impressed. I thought he was a very serious, thoughtful person. He is interested in the principles, and he has reached them by thinking them through and by reading." Describing these and later

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[238] This piece of information in part supported Herbert Stein's (1981, p. 57) statement that "Ronald Reagan is said to have read… Milton Friedman… but a reasonable guess is that it was the political and ideological writings" that Reagan read, rather than Friedman's "scientific economics." *Capitalism and Freedom* was, indeed, intended for a popular audience. But it acquired considerable status in the economic-research literature, too, and it covered much technical economics.

[239] Friedman and Friedman (1998, p. 210).

meetings, Friedman observed, "I have found Reagan will discuss and understand complicated ideas."[240] Reagan's grasp of, and—as Friedman saw it—his largely correct posture toward, key economic issues, were what Friedman emphasized. This contrasted with one feature that many critics, along with some supporters, stressed as a key characteristic of Reagan both before and during his presidency: his poor command of, and likely lack of interest in, detail.

In the early 1970s, Friedman related Reagan's virtues to his junior colleague and office neighbor Stanley Fischer. "He taught me how smart Reagan was… He called him correctly. I mean, [Reagan] wasn't a genius, but he had a very consistent worldview, and he seemed to have a lot of common sense. And that takes you far." (Stanley Fischer, interview, August 30, 2013.)

Interactions during Reagan's second term as governor

Friedman and Reagan made a couple of joint television appearances in Reagan's second term (that is, 1971 to 1975) as Governor of California. On the first of these, in an appearance at the start of 1972 on William Buckley's public-television program *Firing Line*, Friedman was distressed to find that Reagan was defending Nixon's August 1971 New Economic Policy as an appropriate response to the danger of a severe recession. Friedman's reaction was: "You've been snowed, Governor."[241]

In the same broadcast, however, Friedman also praised Reagan as an exception to the tendency of leaders of both major U.S. political parties to favor growth in the public sector. It was Reagan's position on this matter that led to the second Friedman/Reagan joint television appearance.

On this occasion, in late 1973, they appeared on another PBS debate program, *The Advocates*. This time, in a program taped in Hollywood, they acted as a team—both arguing in favor of the referendum Reagan was holding on a measure to cap the growth of (state-level) government spending.[242] Although they stood side-by-side on the program, this joint television appearance actually had less on-air interaction between Friedman and Reagan than their 1972 *Firing Line* panel did. Because of the mock-legal format of the program, each of them was "cross-

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[240] *Boston Globe*, April 3, 1983, p. 43. In this interview, Friedman mistakenly recalled his UCLA visit and meeting with Reagan as taking place in 1970, and this incorrect date was reused in Rayack (1987, p. 1).
[241] *Firing Line*, PBS, January 5, 1972, p. 17 of transcript.
[242] The appearance was taped on October 29, 1973 (*Pasadena Star-News* (California), October 30, 1973) and was broadcast on November 1, 1973.

examined" by a hostile interlocutor about the matter under debate. The program therefore did not really give rise to televised exchanges between Friedman and Reagan.

But, as Friedman acknowledged on the program, he had served on the task force that led to the Reagan ballot initiative under discussion—Proposition 1. And that initiative itself is noteworthy because of its intersection with Friedman's developing views about how to limit the growth of the public sector.

Friedman had concluded that California's constitutional limitations on borrowing had not served as an adequate curb on the individual totals of taxes and spending (Instructional Dynamics Economics Cassette Tape 115, February 14, 1973). In addition, he felt that while the population was becoming concerned about the scale of public spending, the way in which appropriations were decided upon meant that U.S. legislatures were not sufficiently receptive to this concern. To Friedman, each proposed individual government spending program might not encounter much opposition—either because it was generally popular, or because few people had a strong incentive to oppose it, as the financing of the program (notably taxes) and any negative externalities that the program generated were community-wide, while the benefits it produced tended to be concentrated. Yet this process might generate an aggregate level of public spending programs that exceeded what the populace would choose if it had an opportunity to choose that aggregate directly. Friedman had applied this argument to some degree in previous years, but it was one he articulated with increased emphasis during the 1970s.[243] Later still, during the Reagan presidency, it became a central theme of the Friedmans' book *Tyranny of the Status Quo*.[244]

Friedman articulated the matter in his *Advocates* appearance as follows: "There is a fundamental defect in our present democratic structure. That defect arises from the fact that the legislature, the government, approves individual programs and that nobody takes a look at the whole… The problem is that if you handle each item of expenditure separately, the sum of the pieces tends to

[243] He did so in, for example, in *Playboy* (February 1973, p. 56, as reprinted in Friedman, 1975a, p. 11; 1983b, p. 22), *Newsweek*, April 2, 1973, *Manion Forum*, October 28, 1973, p. 3, *Monday Conference* (Australian Broadcasting Commission), April 14, 1975, p. 8 of transcript, *Newsweek*, November 15, 1976*a*; *Donahue*, NBC, November 24, 1976, Friedman (1977g, p. 89; 1978a, p. 9 [also in Friedman, 1978b, p. 18]), and *Milton Friedman Speaks*, Episode 3, "Is Capitalism Humane?," September 17, 1977, p. 26 of transcript. He had previously applied the argument in Friedman (1962h, p. 4) and, in a more limited context, in Friedman (1962a, p. 181). As discussed in Nelson (2020a, p. 574), Friedman in the 1960s traced this argument to Wesley Mitchell (1912)—an attribution he reaffirmed in *Milton Friedman Speaks*, Episode 8, "Free Trade: Producer Vs. Consumer," April 27, 1978, (p. 7 of transcript; see also *Manhattan Mercury* (Kansas), April 28, 1978, and *San Francisco Chronicle*, January 23, 1979).
[244] That is, Friedman and Friedman (1984, 1985).

be larger than we would want to spend if we looked at it as a whole. The great virtue… of this proposal [Proposition 1] is that it gives the people for the first time an opportunity to look at the whole pie and decide how much of their income they want government to spend."[245]

Galvanized by this reasoning, Friedman devoted a considerable portion of his time in the decade from 1973 to 1982 to the promotion of constitutional-reform initiatives at the state and federal level that sought to put a cap on the overall budget. California's Proposition 1 ballot was an early example. In addition to his help in preparing Proposition 1 and promoting it in his television appearance with Reagan, Friedman spoke in its favor in other media appearances and in the promotional literature for the initiative.[246]

"I think Reagan quite liked Friedman," observed Arthur Laffer (interview, June 10, 2014), who remarked that Friedman's interaction with Reagan really got going "with Prop. 1 in California, which I thought was a mistake at the time. And not that I didn't want spending [restraint] as they did, but I'd much rather do tax limits than spending limits, because they're far more successful. Everyone loves government spending; they just hate paying for it. So if you go against government spending, you usually lose." This proved to be the case with Prop. 1, which was heavily defeated.[247] Friedman had been optimistic about the likelihood of a victory, believing that the general public would see that a limit on aggregate spending meant a limit on taxes. Indeed, Friedman would retrospectively refer to Prop. 1 as a tax-limiting initiative.[248]
The proposition had indeed included tax limits and cuts among its provisions. However, its most prominent element, on which public debate had been centered, was, of course, its cap on the state government's spending as a share of state (personal) income. And the governor's advocacy of constitutional limits on spending would subsequently lead Friedman to attribute to Reagan a "pioneering role" (*Newsweek*, November 10, 1980).

The electorate's rejection of Prop. 1 took place after the campaign against the proposition pointed to possible implications of the expenditure ceiling for individual state government programs. This line of criticism was buttressed by the actual text of the ballot paper for Proposition 1. Opponents of the measure succeeded in having a statement put on the ballot paper

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[245] *The Advocates*, PBS, November 1, 1973, p. 7 of transcript.
[246] See, for example, the radio interview transcribed in *Manion Forum*, October 28, 1973, and the interview Friedman gave in a promotional booklet (Government of California, 1973). The latter item was excerpted in *Los Angeles Times*, June 8, 1973, some months before voting day.
[247] Friedman learned of the defeat while recording his regular audio commentaries. Instructional Dynamics Economic Cassette Tape 133 (November 7, 1973) featured a break in recording in order for Friedman to hear news of the referendum defeat.
[248] Friedman (1975a, p. 41).

to the effect that cutbacks in public services would flow from enactment of as the proposed amendment—an inclusion Reagan objected to, and about which he would express anger during his appearance on *The Advocates*. As for Friedman, the possibility of reductions in government services had been raised in *The Advocates*, when the cross-examination cited his recommendation (in *Capitalism and Freedom*) that the U.S. government should sell off national parks. Friedman dismissed the citation of Capitalism and Freedom as a red herring for the existing discussion, as the ceilings implied by Prop. 1 did not imply reductions from the existing level of government spending. He himself wanted public spending reduced, he explained, but the measure under discussion implied restraint but not absolute reductions in state government expenditure.[249]

Notwithstanding the defeat of Prop. 1, Friedman continued to see the introduction of restrictions on the government's taxing and spending powers as the way ahead for the United States (*Newsweek*, June 23, 1975). At the federal level, one route, apart from constitutional change, that he saw as a way in which the population could make its views felt concerning restriction of the overall budget was through the executive branch. As the only positions subject to choice by the entire voting population, the U.S. president and vice president were, Friedman felt, the only posts in government on which public dissatisfaction with the overall size of government spending and taxes could systematically exert more weight than could the popularity of individual spending programs.[250]

Ronald Reagan was an appealing politician to Friedman from this perspective both because he was known to have presidential ambitions (having had a short-lived participation in the 1968 Republican nomination contest) and because he shared many of Friedman's views about the role of government. In November 1975, Reagan—who the previous January had ceased being California's governor—had announced that he would be challenging Ford for the Republican nomination for the presidency at the 1976 election. Well ahead of that announcement, press coverage noting the possibility of a Reagan candidacy had pointed to Friedman as an influence on Reagan's economic thinking (see, for example, *Dallas Morning News*, February 18, 1975).

With regard to inflation, Reagan's position paralleled Friedman's in blaming inflation on aggregate demand policy rather than on cost-push factors. But Reagan in the 1970s articulated a far more mechanical view of the connection between fiscal deficits and inflation than Friedman

[249] *The Advocates*, PBS, November 1, 1973, p. 11 of transcript.
[250] See Friedman and Friedman (1985, p. 64) and Friedman (1987b, p. 220).

could countenance (see Nelson, 2005). This partly reflected a presumption that the occurrence of federal deficits led to monetization of the deficits—a sequence whose empirical applicability Friedman had come to doubt for the United States. Even state government deficits—for which there was even less basis to suspect a link with money creation—drew a negative reaction from Reagan: in his *Advocates* appearance, he described financing California's government spending by borrowing as a "fraudulent" practice.[251] Reagan's strong expressed aversion to government debt issuance stretched right into the period when he started running large deficits himself as U.S. president. For example, a few weeks into his presidency, Reagan declared that "we cannot continue on the path of ever bigger deficits and just endlessly running up debt… We've gone along for decades now with this belief that we could spend more than we take in…"[252]

Friedman lashes out at Ford

It was energy policy (discussed further in the next chapter) that proved to be the final straw in Friedman's evaluation of President Ford. This subject had already generated some sharp criticism by Friedman of the Ford Administration when, in early 1975, it had been proposed to tax oil and reroute the resulting revenue to households in the manner of a negative income tax (NIT). Friedman's reacted viscerally. He was already opposed to increases in gasoline or oil taxes as a response to the oil shock (see the previous chapter); now, he was incensed at seeing it combined with a version of the NIT. Friedman labeled the latter aspect of the Ford proposal as "a bastard version [of the NIT] that trivializes a good idea" (*Newsweek*, January 27, 1975*a*, p. 25).[253]

Less than a year later, and several weeks after the Reagan challenge was launched, Ford took an action in the energy area that Friedman would later identify marking the point "when he lost me."[254] This was Ford's signing of an energy bill that continued price controls on oil. In one of the regular commentaries he was broadcasting on *CBS Morning News* during this period,

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[251] *The Advocates*, PBS, November 1, 1973, p. 5 of transcript.
[252] *A Day With President Reagan: NBC News Special*, NBC, February 13, 1981.
[253] Friedman might have been expected to be pleased with the introduction later in 1975 of the federal government's Earned Income Tax Credit (EITC), under which low-income households received top-ups of their labor income via the tax system. But he was not. Becker (2007, p. 185), Robert Barro (in *Business Week*, July 13, 1998), and Baumol and Blinder (1982, p. 659) all cited the EITC, which continues to this day, as an implementation of Friedman's negative income tax (NIT) proposal. However, Friedman's objection to measures like the EITC was that, in contrast to his own NIT proposal, they were introduced as a supplement to, rather than a replacement of, existing welfare programs. Friedman wanted the negative income tax to supplant the existing welfare system—not to be the basis for an additional element in that system. Consequently, after 1975 Friedman continued to regard his NIT proposal as never having been implemented (see Nelson, 2020b, Chapter 13).
[254] *Milton Friedman Speaks*, Episode 2, "Myths That Conceal Reality," taped October 13, 1977, p. 25 of transcript.

Friedman lashed out at Ford's "demonstrated capacity for waffling" and his ratification of a law that departed so brazenly from free-market principles.[255] Friedman underlined this criticism in a *Newsweek* column (January 19, 1976), titled "Rising Above Principle," which concluded that "the president's behavior widens still further the gap between his rhetoric and his actions… His actions are weak and vacillating." Friedman would later judge that Ford's signing of the energy-price control extension as a "disastrous economic decision." The move, Friedman suggested, had hurt Ford both in the primary and national elections because it both alienated voters in Texas and, in going against Ford's previous statements, suggested an unwillingness on the president's part to "stick to his principles" (*Saturday Evening Post*, May/June 1977, p. 20).[256]

Friedman made various interventions during 1976 that were supportive of Reagan's candidacy. One of these was an appearance in February 1976 at the third annual Conservative Political Action Conference, in Washington D.C. At the event, Friedman supported Reagan's calls for federal spending reductions (*The Detroit News*, February 15, 1976). However, the specific item under discussion—a $90 billion cut in federal spending that Reagan had floated—had damaged Reagan's campaign against Ford and contributed to that campaign's near-collapse in February 1976 (see Cannon, 2003, pp. 409–411). Reagan's campaign, however, regained momentum from late February onward, as he focused on tax cuts and, especially, foreign policy (Garthoff, 1994, p. 604; Cannon, 2003, pp. 420–424).

Friedman used his *Newsweek* column (March 1, 1976) to defend Reagan against the charge of hypocrisy with regard to public spending. Friedman granted that government spending in the state of California had more than doubled during Reagan's tenure as governor. But this had been a smaller increase than that in several other U.S. major states, such as New York state, and the resulting contrast showed, Friedman believed, that Reagan had "successfully coped with tremendous pressure" for the creation of a larger public sector.[257] This argument would prove useful again in Friedman's retrospectives on Reagan's national administration in the 1980s. For it would transpire that President Reagan's tenure would see general restraint in the growth in—but not a lower absolute level of—federal spending and regulations.

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[255] *CBS Morning News*, January 9, 1976, pp. 23–24 of transcript.

[256] See also Instructional Dynamics Economics Cassette Tape 202 (November 1976, Part 1) and *Newsweek*, October 16, 1978.

[257] The Governor of New York with whom Friedman contrasted Reagan was Nelson Rockefeller. Rockefeller had been the major competitor against Barry Goldwater for the 1964 Republican nomination. Ford and Congress had chosen Rockefeller to be vice president after Ford became president, but Ford announced in late 1975 that Rockefeller would not be his running mate in the 1976 election (and so would leave office in January 1977, even if Ford won the election).

During Reagan's revitalized primary contest with President Ford, Friedman remarked that "I am very sympathetic" to the Reagan candidacy (*Chicago Tribune*, May 23, 1976): he explained that this was because "he is a major believer in a smaller central government and in promoting free enterprise." Friedman further contended that Reagan's proposal to reduce the functions of the federal government aligned with Adam Smith's vision of the public sector's responsibilities.[258] And when comparisons were made with the very unsuccessful Goldwater run, Friedman defended Reagan's presidential bid. Friedman contended both that Reagan was a more appealing candidate than Goldwater had been and that U.S. voters in 1976 were more congenial to a small-government platform than they had been in 1964 (*Christian Science Monitor* (Boston), August 26, 1976).

By the time these last remarks had appeared, the Republican convention had taken place, and Reagan had narrowly lost the Republican party's nomination to President Ford. It was Ford who would face the Democrats' candidate, Jimmy Carter, at the November 1976 election.

The wrath of Ford

Along with nine other outside economists, Friedman participated in a discussion of the U.S. economy with President Ford and a few senior government officials at a lunch at the White House on May 20, 1976.[259] The economists present also included outspoken Keynesian critics of the president like Paul Samuelson, Arthur Okun, and Walter Heller. The prestigious attendee list likely reflected the choices of CEA Chairman Alan Greenspan, who also attended the lunch. In any event, however, the event established that relations between Friedman and Ford had not been cut off.

Ford, however, did seem to register his displeasure with Friedman's decision to support Reagan's challenge by apparently snubbing him on a couple of notable occasions that occurred later in 1976. The first of these was connected to an official visit to the United States by Australia's prime minister, Malcolm Fraser, in July 1976. As of 1976, Fraser was associated both with an endorsement of the assignment of monetary policy to the task of inflation control and with a belief in reducing the role of government in the economy—though, over time, his words and actions created great distance from both these positions. In his early period as prime minister, Fraser was often described as having been influenced in his thinking by the writings of

[258] Friedman (1977h, p. 8).
[259] See *https://www.fordlibrarymuseum.gov/library/document/0036/pdd760520.pdf*.

both Friedman and Ayn Rand. In 1970, when the United Kingdom's Edward Heath, also associated at that time with free-market views, had visited the White House, Friedman had attended the state dinner for Heath, at President Nixon's invitation.[260] But when President Ford hosted Fraser for a state dinner on July 27, 1976, Rand was a guest, but Friedman was notably absent.[261] As Friedman would later note, in the case of Rose Friedman and himself, the 1970 event for Prime Minister Heath proved to be "our first and last White House dinner."[262]

From Friedman's perspective, the guest list for the 1976 White House dinner in honor of Prime Minister Fraser had two notable consequences. First, he never did meet Rand, who died in 1982 (*Reason*, June 1995, pp. 35–36). Second, he did not meet Fraser until a visit to Australia in April 1981—when his encounter with the prime minister led Friedman to judge him one of the rudest people he had ever met (Maurice Newman, interview, September 18, 2013).[263]

The next notable seeming snub of Friedman on President Ford's part occurred after Friedman won the Nobel award in economics on October 14, 1976. About two months had passed since Ford had seen off the Reagan challenge and received the Republican nomination, but "President Ford did not forget that [i.e., Friedman's support for Reagan], and he never sent a congratulatory letter to Professor Friedman" when he received the Nobel, recalled Friedman's secretary Gloria Valentine. Indeed, she recalled that Friedman's office heard "not a peep" from the president in the wake of the announcement of the award (Gloria Valentine, interview, December 5, 2013). The documentary record indicates that this recollection was largely shaped by the absence of a telephone call from Ford. Ford did put his signature on a letter of congratulations to Friedman on October 14, 1976.[264] In contrast, as discussed in the next chapter, former president Nixon

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[260] See Friedman and Friedman (1998, pp. 390–391) and Nelson (2017) for further discussion of Friedman's meetings with Heath.
[261] See "President's Daily Diary, July 27, 1976," Gerald Ford Presidential Library, *https://www.fordlibrarymuseum.gov/library/document/0036/pdd760727.pdf*. In contrast, O'Brien (2018, p. 204) asserts that "Fraser met Milton Friedman… in the early months of his government," meaning during Fraser's 1976 trip to the United States. But O'Brien does not document this statement—which likely arises from a confusion of Rand with Friedman.
[262] Friedman and Friedman (1998, p. 390). Friedman went on to state (p. 630, endnote 15) that he had turned down some later invitations; he did not, of course, refer to dinners he would like to have attended but to which he received no invitation. (Ford's presidential papers do indicate that the Friedmans were included on a list of possible invitees for a December 6, 1976, state dinner, but that someone in the White House wrote "?" on the margin alongside their names. See the memo dated November 18, 1976, in "Presidential Handwriting, 12/6/1976" at *https://www.fordlibrarymuseum.gov/library*.)
[263] See also Friedman and Friedman (1998, pp. 431–432) on Friedman's account of his meeting with Fraser.
[264] See "Presidential Log, October 1976" at *https://www.fordlibrarymuseum.gov*. The notes indicate that Ford was asked to sign a prepared letter to "Milt Friedman on receiving the Nobel Prize for Economics." The shortening "Milt," little used for Friedman except by George Stigler (see Chapter 4), may indicate some confusion between Milton Friedman and Ford's White House assistant, Milton A. Friedman, who did embrace the "Milt" usage.

phoned Valentine, and then Friedman, on the day of the award, while at the end of 1976 President-elect Carter rang Friedman with his own congratulations. As for now-former presidential candidate Ronald Reagan: On October 18, 1976, in recognition of the Nobel award announcement of the previous week, he made two recordings about Friedman's views for his series of regular radio commentaries (Skinner, Anderson, and Anderson, 2001, p. 510).[265]

Ford versus Carter

"Personally I prefer Mr. Ford to Mr. Carter, quite obviously," Friedman observed in his final cassette commentary before the election (Instructional Dynamics Economics Cassette Tape 201, October 1976, Part 2). In a network television appearance around the same time, Friedman indicated that while he felt that Ford would—if he won a new term as president—likely only contain the size of the U.S. government rather than scale it back, he preferred Ford to Carter because the latter proposed to expand the public sector.[266] He observed at a Wisconsin press conference that he was "far from confident that the election of Ford would stop the trend to greater government control of our lives," but suggested that a Ford victory would slow this process. In contrast, with regard to Carter, Friedman observed, "I challenge you to give me one item in his program which, in fact, would reduce the power in Washington." (*Milwaukee Journal*, October 29, 1976.)

As of the middle of 1976, the U.S. economy had reached a combination of economic circumstances that was not unpromising for President Ford's reelection. The first two years after Nixon's reelection had, of course, been one of extreme economic as well as political turmoil, and this was reflected in the Republican party's poor performance in the November 1974 Congressional elections. The worst period for both output growth and the four-quarter inflation rate had, however, been not long after the 1974 election results. In a column early in 1975, Friedman predicted that "the state of the economy in 1976 will be a strong plus for President Ford," as inflation would be lower and the economy would be growing (*Newsweek*, February 17, 1975). One favorable item he cited in his column—the collapse of OPEC by late 1976—did not eventuate, but Friedman's domestic macroeconomic predictions were largely borne out during

[265] These commentaries are held in the Hoover Institution's audio collection. As might be expected, Reagan's rematks elided the differences between Friedman's status as what Reagan called "a free-enterprise advocate" and Friedman's research contributions. Nevertheless, he did refer to Friedman's "scientific research and analysis" and "body of scientific work." (All quotations are from the first broadcast. In the second broadcast, Reagan quoted and endorsed Friedman's statement that "inflation is always and everywhere a monetary phenomenon" and also praised Friedman's assessment regarding the burden of corporate taxation.
[266] *Meet the Press*, NBC, October 24, 1976, p. 3 of transcript.

1975 and 1976. In mid-1975, therefore, Friedman was observing that the timing of economic developments was indeed turning out well for Ford: the recession was out of the way, inflation and unemployment would improve in the period ahead of the 1976 election, and a renewed breakout of inflation arising from possible overstimulation of the economy would not occur until 1977 (*Richmond Times Dispatch*, June 23, 1975). He reaffirmed in October 1975 that the 1976 economic outlook looked favorable and so increased the president's prospects of electoral success (Instructional Dynamics Economics Cassette Tape 179, October 1975, Part 3).

U.S. economic developments in the first half of 1976 bore out this assessment. True, output-gap estimates, as already indicated, suggested a great amount of unemployed resources—far more than than seems justified by retrospective analyses of the state of the 1976 economy.[267] But in terms of the direction—the behavior of the time derivatives of key economic variables—the Ford Administration seemed to be in a favorable position as the election approached: inflation continued to decline rapidly, unemployment was declining, and output was growing rapidly. In April 1976, Paul Samuelson remarked that the first-quarter national accounts suggested that "mostly the picture is good" regarding real output and "the other part of the good news [is that]… the inflation picture came in a very pleasing way." "I've been saying on these recordings that the incumbent president looks in pretty good shape," Samuelson further noted (Instructional Dynamics Economics Cassette Tape 197 (Paul Samuelson series), April 1976, Part 2). In August 1976, President Ford himself emphasized the pattern of economic improvement when, in his speech accepting the Republican party's nomination for president, he proclaimed that "on balance, America and Americans have made an incredible comeback since August 1974" (*State-Times* (Baton Rouge, Louisiana), August 20, 1976).

However, the pattern of an improving real economy was by this time experiencing a change—a temporary one, but one that had material consequences for the election outcome. A slowdown in monetary growth had occurred in late 1975—one that, as noted earlier in this chapter, Friedman attributed to the FOMC keeping up its federal funds rate target in the face of a weak credit market.[268] Friedman warned after the first quarter of 1976 that, although U.S. monetary growth

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[267] Official output-gap estimates stood at around 10 percent during 1976 (Orphanides, 2003, p. 645). Estimates of excess capacity quoted in the press and by presidential candidate Carter were actually more negative than this (see Nelson, 2005). This difference may have reflected the use of estimates of the gap based on industrial-production or capacity indices, rather than assessments of the percentage difference between actual real GNP and estimated potential real GNP.

[268] Friedman also highlighted the slowdown in monetary growth in the second half of 1975 in his *Newsweek* columns (December 8, 1975; June 14, 1976) and Instructional Dynamics Economics Cassette Tape 184 (January 1976, Part 2). The reduction is masked in the four-quarter growth rates reported in Table 1 above, but it is evident in quarterly annualized rates. On this criterion, M2 growth using the old definition slowed down from 10.5 percent to

had recently picked up, the prior monetary slowdown could interrupt the nation's economic expansion (*The Star* (Johannesburg, South Africa), April 5, 1976). Indeed, Friedman's later verdict on 1976 was that "the [monetary] slowdown in late 1975 produced the economic pause in the second half of 1976 that played such a prominent role in the Ford-Carter election battle" (*Newsweek*, October 3, 1977). Some thirty years after the 1976 presidential election, Ford's former press secretary expressed the judgment that a slight rise in the U.S. unemployment rate before the election—a rise associated with the economic pause—was a decisive factor in President Ford's narrow loss to Jimmy Carter in the November 4 national vote (*USA Today*, December 27, 2006).[269]

In the aftermath of the presidential election, *Time* magazine (November 15, 1976) noted that Ronald Reagan "will be 69 in 1980—which may be too old to try again." *Newsweek*'s analysis (November 15, 1976*b*) of Reagan's prospects was even blunter: "Only the truest of believers believe Reagan could be the GOP nominee in 1980—when he will be 69." In his own post-election analysis, Friedman seemed to share *Newsweek*'s negative assessment, speaking as though Reagan was finished as a political candidate (Instructional Dynamics Economics Cassette Tape 202, November 1976, Part 1).[270]

However, far from being truly politically finished by November 1976, Ronald Reagan would dominate the 1980s. Indeed, a full decade on from November 1976, on November 21, 1986, President Reagan would be receiving Friedman in the White House, for what would turn out to

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6.6 percent from 1975:Q3 to 1975:Q4; for the modern M2 definition, the corresponding slowdown was from 15.4 percent to 10 percent. The monthly data on modern M2 also indicate that the August-December 1975 period saw distinctly slower monetary growth than the rates recorded in the immediately prior and later periods. Friedman attributed the weakness in the credit market, which in turn triggered an unintended move to monetary restriction, to a glut of supplies of funds to U.S. short-term securities markets, *vis a vis* longer-term markets, in the wake of the New York City financial crisis (see the discussion earlier in this chapter as well as his testimony of November 6, 1975, in Committee on Banking, Housing and Urban Affairs, U.S. Senate, 1975a, pp. 67–68, and his testimony of January 22, 1976, in Committee on Banking, Currency and Housing, U.S. House of Representatives, pp. 2182–2183).

[269] The unemployment rate in the third quarter of 1976 (7.7 percent) was fractionally higher, on average, than in the previous quarter (7.6 percent).

[270] This view was widely shared at the time, and retrospective statements to the contrary should be treated with suspicion. For example, Cannon's (2003, p. 436) declaration that Reagan came out of the 1976 Republican convention as "presumptive front-runner" for the 1980 nomination was at variance with Cannon's (2000, p. 253) own statement that Reagan's 1976 convention speech was widely seen at the time as the finale to his political career. It also overlooked the fact that, in light of the closeness of the 1976 presidential election result, the prospect of a 1980 Ford-Carter rematch was taken very seriously in the late 1970s. Gerald Ford kept a high profile well into 1979 finding fault with the Carter Administration (see, for example, *Marshall Evening Chronicle* (Michigan), August 10, 1979), and Carter would later observe (*The View*, ABC, September 20, 2010) that Ford "spent a lot of the time when I was president criticizing me."

be the last of the meetings of Reagan's Presidential Economic Policy Advisory Board.

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 6: Debates on Regulation, the Phillips Curve, and Policy Rules, 1975 to 1976

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July 30, 2023

I. EVENTS AND ACTIVITIES RELATED TO DEBATES ON REGULATION, THE PHILLIPS CURVE, AND POLICY RULES, 1975–1976

In 1976, G. & C. Merriam, producer of the celebrated Webster's dictionary of American English, issued a free-standing book, intended as a "supplement" to the dictionary and titled *6,000 Words*. Its 220 pages listed, and provided definitions of, the components of "new English"—that is, "words and meanings that have become firmly established in the language largely during the past fifteen years."[2]

One of the words included was "counterproductive." Usage of "counterproductive" happened to date back further than fifteen years: it had apparently originated in U.S. national security discussions in the mid-1950s, and it was used in the economic-research literature by Francis Bator—a rare example of an author steeped in both macroeconomic and national-security discourse—as early as 1957.[3] By 1976, the word was pervasive, and Milton Friedman confirmed his own acceptance of it when, in Congressional testimony early that year, he referred to the "counterproductive role" played in the U.S. financial system by the nationwide legal ceilings on interest rates on commercial bank deposits.[4]

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[1] Email: Edward.Nelson@frb.gov. The author is grateful to the interview subjects, Michael Oliver, George Tavlas, and Mark Wynne for their generosity in providing useful information. See the Introduction in Nelson (2020a) for a full list of acknowledgments. The views expressed here are the author's alone and should not be interpreted as those of the Federal Reserve or the Board of Governors. The author regrets to note that since the research underlying this chapter began, three of the individuals—Samuel Brittan, Rachel McCulloch, and Leland Yeager—whose interviews with the author are quoted in this chapter, have passed away.
[2] From the text on the cover of G. & C. Merriam Co. (1976). The book came out around mid-November 1976 (see *Daily Press* (Newport News, Virginia), November 19, 1976).
[3] See Bator (1957, p. 104). For early usage of the word in national-security discussions, see, for example, the remarks of Thruston Morton, Assistant U.S. Secretary of State, in *Tyrone Daily Herald* (Pennsylvania), June 14, 1955.
[4] See Friedman's testimony of January 22, 1976, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1976, p. 2152). Among Friedman's fellow monetary economists, James Tobin and Henry Wallich had used "counterproductive" in print over a decade earlier (though, in each case, in discussions of national policy outside the monetary area): see Tobin (1965b, p. 889) and Wallich (1965, p. 150).

In a speech given in London the following August, Friedman was less generous about another neologism, "technostructure," defined by *6,000 Words* as "a large-scale corporation or system of corporate enterprises."[5] He implied that it was puzzling that "such a clumsy word as 'technostructure'" had gained wide acceptance. But Friedman was magnanimous enough to acknowledge that the fact that the word had become embedded in public debate attested to the gifts of John Kenneth Galbraith, the inventor of the term.[6]

Another word to appear in *6,000 Words*, "scenario," was not really new at all. But its narrow prior meaning of "the outline of a drama or motion picture" had, of late, largely been superseded by different uses in American political and economic discussions.[7] In national political commentary, "scenario" had gained notoriety during 1974 when it was revealed that President Nixon and his aides had, in their taped White House conversations, deployed this term in reference to a narrative or gloss that might be applied retrospectively—for public-relations purposes—to the Watergate-related activities in which they and their subordinates had engaged.[8] In economic discussions over the same period, however, the word "scenario" acquired a more respectable and enduring function—to refer to a hypothesized or simulated trajectory of the economy under a specific path for aggregate demand policy or some other key driver.[9] In this vein, in a talk given at Harvard University, Nicholas Kaldor (1975, p. 347) observed that "scenario" was "the now fashionable word in America." Conforming to this characterization, Friedman himself used the word in remarks published a few months before Kaldor's talk

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[5] G. & C. Merriam Co. (1976, p. 198).
[6] See Friedman (1977i, p. 17); also in Friedman (1978b, p. 57). The same passage recognized some phrases Galbraith had coined, but without explicitly nominating one that Friedman himself would find highly useful: "conventional wisdom." Elsewhere in this 1976 lecture, Friedman did, in effect, acknowledge Galbraith's origination of that phrase by giving part I of the lecture the title "The Conventional Wisdom of J.K. Galbraith" (see Friedman, 1977i p. 12; 1978b, p. 52). For his part, Friedman had matter-of-factly used "conventional wisdom" in a 1974 discussion of current economic developments (see *Daily Mail* (London), September 19, 1974). He and Anna Schwartz would later use "conventional wisdom" in their monetary writings, while giving a faint nod to the reasonably recent origin of the phrase by putting it in quotation marks (Friedman and Schwartz, 1982, p. 463; see also Friedman, 1980a, p. 60 [p. 58 of 1991 reprint]). Friedman would then use the term "conventional wisdom" many more times in public statements over subsequent years.
[7] The older meaning of "scenario" just quoted was that given in Morehead and Morehead (1972, p. 407). During the early 1970s, one of Friedman's former students, William Gibson, used "scenario" in essentially this sense in a book of monetary-economics readings, coedited with George Kaufman. Gibson and Kaufman (1971, p. 2) portrayed the Keynesian-monetarist debate as a drama, and their readings as something that could guide "the reader through the more recent developments in this scenario." Duesenberry (1969, p. 88), for his part, referred to "the scenario I have just outlined" in reference to his description of arrangements under which policy decisions might be made—and did not use it to characterize a particular policy or its economic consequences.
[8] See, for example, *Los Angeles Times*, May 2, 1974.
[9] Like some other economic terminology, this usage of "scenario"—which would become a part of textbook discussions (see, for example, Dornbusch and Fischer, 1984, pp. 450, 490, 573, 607)—had previously been applied in national-security discussions, in which it was used to refer to the hypothetical situation assumed in large-scale military exercises (see especially MccGwire, 1970, p. 40, and Erickson, 1971, pp. 24, 67, 68, 93, 94).

appeared in print.[10]

As it happened, Friedman's usage of "scenario" in its modern, economic-policy-related sense dated back some years—and he had used it in reference to President Nixon. Notably, in a talk in May 1970, he had suggested "two different scenarios that I might outline for you."[11] In the outline that followed, Friedman contrasted a "Scenario 2" in which U.S. policymakers (the Federal Reserve and the Nixon Administration) kept their resolve to pursue aggregate-demand restraint and a "Scenario 1" (which, in the event, was largely realized) in which they precipitately abandoned restraint in favor of a mixture of demand stimulation and wage-price controls.[12]

Four other words in *6,000 Words* involved matters even closer to home for Friedman. All four of them could, if one looked hard enough, actually be found in print items dating to the 1960s. But, in each case, their usage in English-language discourse had stepped up dramatically in the first half of the 1970s.

One was "stagflation."[13] In another of the guest lectures he gave during the mid-1970s, Nicholas Kaldor (1976, p. 704) had observed: "This combination of inflation and economic recession is a new phenomenon, the explanation of which presents an intellectual challenge to economists." Kaldor implied that his own preferred analytical framework, in which cost-push forces dominated the inflation process, had fully met this challenge. This framework, by denying the endogeneity of inflation and pointing to shocks to particular sectors' wages and prices as a dominant influence on aggregate cost and price levels, did indeed make recession and high inflation readily compatible. It is fair to say that, in policy circles in the United Kingdom, the United States, and several other countries had since 1970 there had been a large degree of concurrence with Kaldor's cost-push explanation for stagflation and had formed economic policy accordingly. By 1975–1976, however, some cracks in this policymaking consensus had emerged. And in the U.S. case, advocacy of Friedman's endogenous explanation for stagflation—an expectational, long-run-vertical-Phillips-curve view, as part of a monetary-policy-centered conception of the inflation process—had prominent sympathizers in the Ford

[10] See Friedman (1975c, p. 176).
[11] Friedman (1970g, p. 72). Friedman had earlier used the word "scenario," essentially in its modern sense, in Instructional Dynamics Economics Cassette Tapes 21 (April 1969), 35 (October 8, 1969), 36 (October 22, 1969), and 42 (January 15, 1970).
[12] Friedman (1970g, pp. 72–76). Friedman's audio commentaries over the course of 1970 also cast likely outcomes in terms of these two scenarios. See Instructional Dynamics Economics Cassette Tapes 45 (February 26, 1970) and 60 (November 4, 1970).
[13] See G. & C. Merriam Co. (1976, p. 189).

Administration in the form of William Simon and Alan Greenspan—though these two figures were not steering U.S. monetary policy, nor did their advice, in practice, guide many of Ford's own key actions in the area of economic policy.[14] In the economics profession outside policymaking, furthermore, Friedman's endogenous explanation for stagflation was gaining greater traction and, as discussed in Sections II and III of this chapter, widespread professional acceptance of that explanation helped underpin the awarding to Friedman of the economics Nobel in October 1976.

The second and third of the strongly Friedman-linked components of the "new English" laid out in *6,000 Words* were "monetarism" and "monetarist."[15] To anyone in 1976 who was already familiar with those words, the close connection between them and Friedman's name was clear enough—even though *6,000 Words* itself did not mention Friedman in its entries defining the two words.

There was no escaping a mention of Friedman, however, when it came to defining a fourth word: "Friedmanite." This was described in *6,000 Words* as referring to "a monetarist who adheres to the theory of economist Milton Friedman that economic regulation should be through direct governmental manipulation of the money supply."[16] There are a number of respects in which Friedman would likely have balked at this definition. He would certainly not have been pleased to see his views described almost entirely in terms of normative rather than positive economics. Like most macroeconomists by this point, he would probably have found it unhelpful to use the term "regulation" in the context of describing stabilization-policy tools that influenced aggregate demand, rather than in reference to particular measures affecting aggregate supply. And he was prone to see his prescription of a constant rate of monetary growth as more appropriately viewed as a way of *ending* policies that amounted to "manipulation of the money supply," and not itself as being such manipulation. Notwithstanding qualms of this kind, it could only be a source of satisfaction for Friedman to have his name become a part of the English language.

The advent of the new word was also a reflection of the fact that Friedman had made a distinct mark in economic discussions. He was accepted as having taken the leading role in developing a line of thinking that was distinct from Keynesian economics and that had a sizable following. "The monetarist view has wide acceptance at the moment here and abroad," syndicated economics columnist Hobart Rowen noted (*The Plain Dealer* (Cleveland, Ohio), May 22, 1975).

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[14] See the previous chapters as well as Section II below.
[15] See G. & C. Merriam Co. (1976, p. 127).
[16] See G. & C. Merriam Co. (1976, p. 79).

The situation had changed since 1966, when Walter Heller declared that recent developments in the United States had seen "the completion of the Keynesian Revolution" and had evidently taken remarks quoted in *Time* magazine as indicating Friedman's own capitulation: "When Milton Friedman, the chief guardian of the *laissez-faire* tradition in American economics, said not long ago, 'We are all Keynesians now,' the profession said 'Amen.'"[17] By the time of his public debate with Friedman in late 1968, however, Heller had been disabused of the notion that Friedman had been co-opted into Keynesian macroeconomics, or that his dissent *vis a vis* his fellow economists concerned only size-of-government issues.[18] In contrast to 1966, when he had been under the misapprehension that Friedman had come into the Keynesian fold, Heller now accepted that Friedman's position on the effects of fiscal and monetary policy differed greatly from his own—while insisting also that Friedman was "dead wrong!" in his view of macroeconomic relationships.[19]

By the mid-1970s, Friedman's leading role in establishing a rival macroeconomic framework to Keynesian economics was a standard feature in a key element of economic discussions—one considered further in Section III below: undergraduate textbook discussions. One example of textbooks' recognition of the modern state of affairs could be found in an introductory text by James Boughton and Elmus Wicker, *The Principles of Monetary Economics*, which was published in 1975 and referred to "America's reigning quantity theorist, Milton Friedman."[20] Furthermore, the textbook endorsed the validity of Friedman's position on monetary matters, at least in extreme cases: "Wherever we find the worst examples of inflation or depression in the history of the modern world, we are likely to discover a poorly planned or poorly executed monetary policy."[21] This contrasted with previous skepticism, commonly expressed in the 1960s, about whether monetary policy had much to do with producing or ending the United States'

[17] The quotations are from pages 2 and 9, respectively, in Heller (1966).
[18] The fact that Heller had been prepared to take Friedman as converted to Keynesianism largely on the basis of a short quotation from Friedman in the media is surprising, as a slew of key Friedman contributions to the monetarist literature had appeared from 1963 to 1965. But that Heller saw Friedman as having suddenly become converted was a reflection of the strong momentum that Keynesianism had in 1965−1966. It also was an example of the strong tendency, noted in Nelson (2007, p. 171), for observers to ascribe to Friedman, on the basis of the appearance of a new statement by him, a major change in views—when, in fact, major changes of this kind were infrequent after 1951. By the time of the appearance of Friedman and Schwartz's *Monetary Trends* in the early 1980s, the authors' desire to forestall further reactions of this kind was made plain in an explicit indication that the book's theoretical analysis built on, and was not superseding, the prior body of solo- and coauthored Friedman work: "this chapter supplements, rather than replaces, others of our writings on issues in monetary theory." (Friedman and Schwartz, 1982, pp. 16–17.)
[19] From Heller's remarks in reply in Friedman and Heller (1969, p. 65).
[20] Boughton and Wicker (1975, p. 157).
[21] Boughton and Wicker (1975, p. 464).

postwar recessions.[22]

*The continuing influence of the **Monetary History** in the 1970s*

In the case of milder fluctuations in output and prices, Boughton and Wicker (1975) were less wholehearted in their acceptance of the monetarist position. Even in this case, however, the influence of Friedman, and especially of the Friedman-Schwartz *Monetary History of the United States*, was manifested in their discussion. Boughton and Wicker prominently cited the book, both for its data and for its historical analysis.[23] Notably, the citations of it went beyond the book's coverage of the Great Depression and considered World War II and postwar episodes in light of the *Monetary History*. It pointed readers toward the Friedman-Schwartz account of the Federal Reserve's 1942−1951 pegging of interest rates as the standard reference on this arrangement (p. 413). It strongly endorsed the Friedman-Schwartz position that constant rates had not implied a neutral stance on the part of the Federal Reserve, with Boughton and Wicker (1975, p. 466) indicating that, during the war years specifically, the policy of pegging amounted to "an aggressively expansionary monetary action that contributed to the wartime inflation." And for the period beyond the war, including the era after the peg, Boughton and Wicker indicated that "we can find contributory failures of monetary policy" for each postwar recession in the United States (1975, p. 468).

Discussions such as this one bore out a prediction that Robert Solow had made in 1964 when

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[22] For example, Colm (1967, p. 894) doubted "how effective monetary policy alone is as an anti-recession device."
[23] The fact that a notable element of Friedman and Schwartz (1963) was its provision of historical data related to money casts light on a recent discussion of the Friedman-Schwartz research program. Hendry (2020, p. 9) objects to the detrending of nineteenth-century U.S. M2 velocity data that Friedman and Schwartz (1982) used in their consideration of velocity in the course of their econometric analysis. (Hendry alleges that this detrending was done "to sustain his [Friedman's] claims." Friedman and Schwartz, however, provided *economic* reasons for their detrending that Hendry does not mention: see Nelson, 2020b, Chapter 12.) Hendry points to Ericsson, Hendry, and Hood (2017) for a "graph [that]… reveals, without his [sic; Friedman and Schwartz's] 'adjustment,' velocity falls from more than 5 in 1880 to almost 1 in 1930." Actually, Friedman and Schwartz's *Monetary History*—which Hendry does not cite—"revealed" this fact about U.S. M2 velocity over half a century before Hendry and his coauthors considered the matter. Specifically, they gave a table in Friedman and Schwartz (1963, Table A−5, p. 774–775) listing annual observations on velocity, and that table's M2 velocity numbers were later plotted by P.S. Anderson (1969, p. 40), Gould and Nelson (1974, p. 408), and Boughton and Wicker (1975, p. 159). The specific values for velocity that Friedman and Schwartz gave in the table (for a series constructed using M2 and nominal income series of earlier vintages than those used in their 1982 study) were 4.97 in 1880 and 1.70 in 1930 (diminishing to 1.28 in 1932). *Monetary Trends* also had a plot of the same decline in velocity (that is, of velocity's behavior absent the detrending of nineteenth-century velocity), albeit in phase-average time aggregation of the data: see Friedman and Schwartz (1982, p. 159). So did Schwartz (1975, p. 139). (*Monetary Trends* also gave annual-data tables from which M2 velocity could be calculated. The 1880 value of U.S. M2 velocity implied by their annual-data tables was 5.30, and that in 1930 was 1.68. The annual series on U.S. M2 velocity implied by Friedman and Schwartz's *Trends* data was also plotted in Bordo and Jonung (1981, p. 100; 1987, p. 5).

reviewing the *Monetary History*: "ten years from now… it will still be read" (*The Banker* (London), November 1964, p. 711). The *Monetary History* had endured into the 1970s as a major, and regularly consulted, reference. Much economic research of the same vintage had not: Solow himself noted in a 1975 book review for the *Times Literary Supplement* of a pair of new studies of the "Cambridge vs. Cambridge" capital debates, "Much of the work reported [on] dates from the early 1960s and rather shows it."[24]

The feature of the *Monetary History* that it had not used formal modeling was something that had served the book well, as the *History* became less dated than more technical work of the same vintage.[25] Even Nicholas Kaldor, in the course of delivering criticisms of Friedman, acknowledged that "his [sic; his and Anna Schwartz's] monumental book, *A Monetary History of the United States*, is an interesting piece of history and makes good reading" (*The Times* (London), April 6, 1977). And, contrary to Kaldor's implication, the book's substance—the economic content of its account of U.S. monetary and economic developments—and not merely its readability was widely judged to lie behind its appeal.

The *Monetary History*'s influence had overshadowed work on Federal Reserve history by others that had appeared in the years since its publication—a fact of which one of the authors of the Boughton-Wicker textbook would have been all too aware of this fact, as Elmus Wicker had produced a study of Federal Reserve history in 1917−1933 (Wicker, 1966). Perhaps benefiting from the attention that the *Monetary History* had received, Wicker had his book released by a mass-market publisher (Random House). Notwithstanding this high-profile outlet, Wicker's monograph proved to be little noticed, and certainly not enduring in influence, in comparison with the *Monetary History*.

The impact of the *Monetary History* extended to discussions in policy circles in the mid-1970s. As early as June 1965, the Federal Reserve leadership had given a *de facto* acknowledgment of the importance of the book when Chairman William Martin chose as the subject matter for a speech, "Does Monetary History Itself?" The speech itself did not cite Friedman and Schwartz, but they were presumably among the experts to whom Martin (1965, p. 13) was referring when

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[24] *Times Literary Supplement* (London), March 14, 1975, p. 278. Solow was reviewing Pasinetti (1974) and Blaug 1975). See Chapter 9 below for a discussion of this debate.
[25] As David Romer has commented to the present author, the same is not true of the companion study, Friedman and Schwartz (1963b), which relied on statistical analysis (and to some extent rested on NBER-centric statistical techniques, which were not widely accepted in the profession even in 1963). Friedman and Schwartz (1963b), though reprinted in a book in 1975 (see the previous chapter), had endured as a reference in large part because of its verbal discussion of the transmission mechanism rather than because of its empirical work.

he observed in the speech: "Few experts today would want to argue that it was right for the German Reichsbank in 1931… to follow the gold standard rules by raising its discount rate to 7 percent merely in order to stem an outflow of gold; or that it was right for our own Federal Reserve to take similar restrictive action, for the same reason, in the fall of 1931." When, in a Congressional hearing the following month, Senator Paul Douglas raised the topic of the 1931 discount-rate increase and whether it had intensified the U.S. economic contraction, Martin noted: "That's the judgment that some people have placed on it." Upon being pressed, Martin confirmed that this was indeed "my judgment [too], and I have already expressed it… If you read the talk that I made on June 1 [that is, Martin, 1965], you will see that I spelled it out."[26]

Senator Douglas suggested that this actually might be the first occasion on which a Federal Reserve official had publicly conceded that the 1931 move had been a mistake. That discount-rate increase was, however, a comparatively uncontroversial example of the Federal Reserve taking an inappropriately restrictive move during the Great Contraction.[27] It had been surrounded in Friedman and Schwartz's narrative by other examples, in 1931 and in adjacent years, of the monetary authorities being contractionary in their actual posture, even when they thought their stance was one of ease or neutrality. This overall picture of U.S. monetary policy settings in 1929–1933, and the implications of those settings for the behavior of the aggregate money stock, received an endorsement from the Federal Reserve leadership in 1975, a decade after Martin's more limited concession regarding the single 1931 action. Specifically, in Congressional testimony in March 1975, Arthur Burns referred to the "tragic mistake that it [the Federal Reserve] made in shrinking the money supply during the years 1929 to 1933."[28]

About six weeks earlier, Friedman and Schwartz had been invoked directly when Hendrik S. Houthhakker, economics professor at Harvard University and a member of the Council of Economic Advisers in the early Nixon years, testified before the Joint Economic Committee. Houthakker was appearing at the committee's hearing of January 29, 1975, on energy policy, but he opened by remarking that he "would first like to say something about more important matters."[29] He noted the "terrifying steepness" of the recent U.S. economic downturn and pointed to monetary policy as a major and continuing source of the recession.[30]

Houthakker cited the fact that the *Monetary History* had pointed to low nominal interest rates

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[26] From Martin's testimony of July 21, 1965, in Committee on Banking and Currency, U.S. Senate (1965, p. 59).
[27] See Nelson (2020a, Chapter 4; 2020b, Chapter 11) for discussion.
[28] See Nelson (2013, p. 24).
[29] In Joint Economic Committee (1975e, p. 188).
[30] In Joint Economic Committee (1975e, p. 189).

and money-supply weakness in the 1930s and suggested that the same phenomenon might be in operation again. "It is true that interest rates have declined somewhat," he remarked of developments since late 1974, "but this should not be interpreted as indicating a less restrictive monetary policy. The fall in interest rates appears to be due not to easier monetary policy but to a decline in the demand for money, which is normal in a deep recession." (Here, Friedman would have insisted—and did in his 1975 commentary, including Congressional testimony that November—that falling demand for credit, not for money, was the source of declining interest rates.) Houthhakker went on to observe: "There is much evidence that a sustained fall in money supply will lead to a fall in money GNP, and hence to an even steeper fall in real GNP when prices are rising. Since the apparent reduction in the money supply is only a few weeks old… [but] it would not be amiss to remember what Friedman and Schwartz had to say about the effects of perverse monetary policy on the early development of the Great Depression."[31]

Notwithstanding its deep impression on professional thinking, the *Monetary History* was far from perfect. For one thing, the book certainly contained the minor misquotations that are to be expected in a massive volume that drew on so many sources, and reflecting the various slips involved in (*a*) transcribing a passage from a hardcopy or microfilm source, (*b*) deciphering the handwritten transcription upon returning to it later, and (*c*) keying a passage of the transcription into the book manuscript.[32] More vitally, Friedman was recorded as indicating in a 1987 retrospective that some "substantive criticisms" of the book had been proved valid (Bordo, 1989, p. 76), and Schwartz (1998a) acknowledged that Wicker's research on the 1929−1933 period had included some details regarding the commercial banking crises that Friedman and Schwartz's account overlooked. In addition, a key resource—Federal Reserve internal documents, especially Federal Open Market Committee minutes—was essentially unavailable to the authors for the period beyond the 1930s. With much of this material released publicly after 1963, later

[31] From Houthakker's testimony of January 29, 1975, in Joint Economic Committee (1975e, p. 189).

[32] For example, Friedman and Schwartz (1963a, p. 144) sourced to the New York-based financial newspaper *Commercial and Financial Chronicle* of January 6, 1900, p. 4, the statement, "By our wild speculation on the stock market we favored a return of large amounts of our European-held securities. All through the early months sales of gilt-edged securities for foreign account were recorded, the inducement being the high prices prevailing here for this class of investments." The scanned copy of the relevant edition of the newspaper, available on the Federal Reserve Bank of St. Louis' FRASER archival website, indicates that the quotation as appearing on page 5 (not 4) and reads (with the differences from Friedman and Schwartz's rendering given in italics in what follows): "By our wild speculation on the Stock *Exchange* we *forced* a return of large amounts of our European-held securities. All through the early months sales of gilt-edged *American* securities for foreign account were recorded, the inducement being the high prices prevailing here for this class of investments." In the same footnote, Friedman and Schwartz sourced to the *Commercial and Financial Chronicle* of January 5, 1901, p. 6, the quotation: "…with the great advance in our Stock Exchange prices following the November elections, European holders of our securities and bonds disposed of them in enormous amounts." The actual quotation, on page 8 (not 6) of the indicated edition, was: "With the great advance in *prices on our Stock Exchange* following the November elections, European holders of our *shares* and bonds disposed of them *here* in enormous amounts."

authors could fill in this gap in the Friedman-Schwartz account.[33] An early step in this direction was Wicker's (1969) account of the 1940s Federal Reserve policy of pegging interest rates. This study used the historical run of FOMC minutes that had become available in 1964, as did later studies such as Meltzer (2003, 2009a).[34]

The year 1976 saw the appearance of a full-fledged, book-length challenge to the centerpiece of the *Monetary History*: its account of the Great Contraction. Peter Temin's *Did Monetary Forces Cause the Great Depression*? certainly was successful in putting down a marker. It became one of the most famous critiques of the *Monetary History*, and was certainly known as the most diametrically different account from that of Friedman and Schwartz.[35] But Temin's book was not influential in the sense of establishing a narrative that gained a substantial following or that displaced the *Monetary History*'s judgment regarding the Great Depression.

In their jointly written work, Friedman and Schwartz limited their reply to Temin mainly to a footnote in *Monetary Trends*.[36] In that discussion, they argued that Temin's account had frequently elided the differences between nominal or real variables—a shortcoming that, they argued, undermined his attempt at a rebuttal to monetary explanations of the Depression. In a 1987 observation, Friedman added that he felt that Temin's focus on whether the 1929−1931 output decline was attributable to monetary forces was at cross-purposes with the *Monetary History*'s focus on 1931−1933.[37] It was the latter period, which featured the largest declines in U.S. money and output, that for Friedman and Schwartz contained the most concentrated stretch of Federal Reserve tightness and policy errors. However, 1929−1931 could not be ignored: though Friedman often described the period starting in December 1930 as that in which the 1930s downturn became an authentic depression, he had also acknowledged that, even ahead of the large-scale failures of U.S. commercial banks, calendar 1930 had already seen a severe output contraction. Temin's categorization of this period as part of the Depression was therefore

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[33] For further discussion of the chronology of the release of the historical minutes, see Chapter 8 below.
[34] In the 1970s, however, and notwithstanding the precedent set by Wicker, accounts that drew on FOMC minutes were quite rare. (For some examples of studies that did so, however, see Nelson, 2020a, Chapter 12, as well as Chapter 8 below.) For example, Timberlake's (1976) critique of monetary policy decisions in the late 1960s drew on sources long in the public record like the period's *Federal Reserve Bulletin*, and not the transcript-like minutes for the 1960s FOMC meetings. These historical minutes, which today are freely downloadable, were on the public record by 1976. However, although they were publicly accessible at Federal Reserve bank libraries, the minutes did not have a major print run and were not held widely by university libraries.
[35] The description of the book on its back cover did not, however, mention Friedman and Schwartz, and it even claimed that "economists… have for the most part shied away from putting their theories to the test by examining what actually happened" in the Great Depression.
[36] Friedman and Schwartz (1982, p. 33). See also their page 51.
[37] See Bordo (1989, p. 76). It would be more accurate to suggest, however, that the *Monetary History*'s main indictment was with regard to the period beginning in December 1930 rather than beginning in 1931.

defensible, and his question about whether monetary factors had produced it was a germane one. Temin's answer was in the negative: nonmonetary factors affecting real aggregate demand had, he suggested, been responsible for the early stages of the 1929–1930 downturn. Furthermore, Temin argued that in those years and indeed beyond them, U.S. monetary policy had been easy, not tight.

It is here that the element of the Friedman-Schwartz account of the 1930s that Henrik Houthakker's 1975 testimony highlighted came into play. For they indicated that while the early stages of the Depression, like the later stages, saw declines in short-term nominal interest rates, this should not be treated as connoting monetary ease. Instead, monetary policy was contractionary, and this stance was evident in quantity series. In arguing that monetary policy was easy in the early years of the Great Depression, Temin cited the decline in nominal interest rates. Indeed, he did so at the expense even of real interest rates, contending that there should be "a presumption that nominal interest rates rather than some estimates of real interest rates were important in the early 1930s."[38]

In addressing Temin's argument, Friedman and Schwartz's *Monetary Trends* pointed readers to Schwartz (1981).[39] Of the two authors of the *Monetary History*, it was Schwartz who devoted more effort to defending the book's historical account against critics. Furthermore, the main copy she kept of the book (its 1964 hardback second printing) had a small amount of handwritten notes that suggested plans for a possible update of the book that never emerged. She also took responsibility for collecting and putting in typographical corrections, when the occasion arose, as further printings were made of the book in the period through the 2000s.

Schwartz's 1981 paper was one of several articles in which she, usually with coauthors, defended and extended the *Monetary History* analysis. The 1981 paper had a specific appendix (pp. 27–42) on "Dissents from the Views in *A Monetary History*." The appendix was to a paper on the 1929–1933 period, so the dissents with which Schwartz was concerned were to her and Friedman's account of the *Great Contraction*—especially Temin's book-length dissent.

This appendix took issue with Temin's focus on nominal interest rates, especially short-term rates, in assessing monetary policy stance.[40] As well as stressing respects in which conditions as measured by real interest rates and spreads showed evidence of monetary tightness in the early

[38] Temin (1976, p. 164).
[39] See Friedman and Schwartz (1982, p. 33).
[40] See Schwartz (1981, pp. 30–38).

1930s, Schwartz challenged Temin's emphasis on the expansion of the aggregate monetary base (from late 1930 onward) as a sign of Federal Reserve ease. The base had indeed expanded, but other developments in the central bank's balance sheet over the same period cast doubt on whether monetary policy, as judged by quantities, had been, on net, a source of stimulus (instead of restraint) for the money stock and the economy. In this connection, Schwartz made specific points about Federal Reserve portfolio behavior in the early 1930s that could largely be found in the *Monetary History* but that were likely insufficiently pressed in that source. In particular, she noted that Federal Reserve actions could have been, but were not, taken to make sure that the move by the nonbank public to increase its currency holdings did not make inroads into the total volume of commercial banks' reserve balances—and that likewise, on the assets side of the central bank's balance sheet, policy actions could have been, but were not, taken to forestall a decline in Federal Reserve credit.[41] The net outcome that the Federal Reserve had "contractionary effects on the reserve position of the banking system—hardly impressive evidence of monetary ease."[42]

For his part, Temin later shifted his position, arguing in Temin (1989) that monetary policy had an important role in producing the Depression and, especially, in generating the subsequent economic recovery.

Monetarism and the major universities

The weight given to the *Monetary History*'s account was an example of the integration into the majority thinking of the profession of macroeconomic positions that Friedman had espoused since the early 1950s. With regard to the structure of the economy and the interpretation of historical U.S. economic behavior, many aspects of his monetarist framework—defined broadly to include his views on the Phillips curve—had become accepted.

The extent to which Friedman's monetary views became accepted "on the ground" in the economics profession—as represented by research sympathetic to those views being produced at smaller and teaching-focused universities, and by the prominent weight assigned to those views in undergraduate course syllabuses and major undergraduate textbooks—will be considered later in this chapter (see the discussion titled "Campbell McConnell" in Section III).

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[41] Schwartz (1981, pp. 29–31).
[42] Schwartz (1981, p. 31).

As far as *major* universities were concerned, the situation was mixed. In terms of personnel, it is true that, as elaborated on in Chapter 8 below, that there was never a critical mass of senior monetarist economists spread widely across those U.S. universities most known as the country's economic-research centers. Friedman himself had to take considerable responsibility for this adverse outcome. Gary Becker, while stressing that "Friedman exerted a profound influence on students," recalled that "many of the best ones" ended up doing their dissertation work with economics-department members other than Friedman (Becker, 1991, pp. 143, 144). "I think there may have been an impression among the students at the time," observed Robert Gordon, a member of the economics department from 1968 to 1973, "that Friedman was just too hard to work with—that he would sort of steamroll you into something that would be a footnote to one of his papers." (Robert Gordon, interview, March 21, 2013.) Referring to the situation prevailing a few years earlier, Sam Peltzman remarked: "Friedman students … [at] the money and banking workshop, which he dominated—they had ready-made topics. At that time, Friedman was interested in the demand for money. You walked in there, and you 'took a number' [a research topic] off the wall, and you cashed it in." (Sam Peltzman, interview, March 1, 2013.)[43] With regard to the job placement of his dissertation students, Friedman simply failed to rack up the stellar track record that other major names in the profession, and indeed several of his departmental colleagues, were able to attain.

In terms, however, of the *influence of his ideas* on major U.S. research centers, Friedman did enjoy a great degree of success. This was clear by the mid-1970s, and that Friedman thought that was it was the case was clearly relayed by his declaration in 1984: "At first, opponents ridiculed monetarism. Then they adopted it and called its earliest proponents extremists."[44] Only slightly less provocatively, fifteen years earlier Friedman had offered the generalization that what was received wisdom about monetary policy at the University of Chicago in one year became received wisdom at Cambridge, USA, five years later (Instructional Dynamics Economics Cassette Tape 17, March 1969).

Notwithstanding the remarks he made along these lines, Friedman did hold in major regard the

[43] David Laidler and Robert Lucas—who were both prominent proponents of monetarist positions in academia in the 1970s and in later decades—had taken Friedman's Price Theory course, and both worked as research assistants on some of Friedman's monetary work in the 1960s. But they did not have Friedman on their dissertation committees. Neil Wallace formally had Friedman as his dissertation-committee chair, but this role for Friedman was decided on only after Wallace had completed most of his thesis work (Neil Wallace, interview, March 15, 2013). Among other prominent monetary economists who were still active in the 2000s, however, two who had had Friedman as their dissertation adviser were Michael Bordo and Robert Hetzel.
[44] Friedman (1984d, p. 3). As discussed in the previous chapter, one, but not the only, individual that Friedman had evidently had in mind when he had earlier made statements of this kind was Arthur Okun.

achievements of one of the major economics powerhouses at Cambridge, USA: the Massachusetts Institute of Technology (MIT), which he described as having "one of the premier [economics] departments in the world" (*Newsweek*, November 9, 1970). Friedman's admiration for MIT's economists had been evident in the early 1960s when, visiting Cornell University for a talk, he met Edwin Burmeister, an undergraduate student. "He knew I was trying to attend MIT," Burmeister recalled, "and he said something along the lines of, 'Well, if I can't convince you to come to Chicago, that is certainly your next best choice.'… It was clear—he said, you know, 'You will get a good education with Samuelson.' So there was mutual respect there that I was aware of before I ever set foot at MIT." (Edwin Burmeister, interview, November 20, 2014.)[45] This mutual respect, and Friedman's high regard for MIT, were also brought out in a tribute that Friedman wrote to Samuelson in *Newsweek* (November 9, 1970), commemorating Samuelson's receipt of the 1970 Nobel award in Economics. Friedman also held in high esteem Robert Solow's work on economic growth (such as Solow, 1956, 1957).[46]

The relationship between Friedman and the major MIT figures was, of course, not sweetness and light by any means: Richard Anderson, who was a graduate student at the department from 1972 to 1976, observed: "Samuelson, as far as I could tell, did not like Milton Friedman." (Richard Anderson, interview, November 14, 2013.)[47] But the guarded acknowledgment by MIT of the importance of Friedman's monetary work was reflected in the MIT economics department's junior hiring decisions. Becker's (1991, p. 143) impression of the state of affairs prevailing in the early 1950s was that "many prominent economics departments refused to appoint any Chicago graduates." In contrast, in Paul Samuelson's assessment, the situation prevailing about a quarter-century later was that every major U.S. economics department now felt it necessary to

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[45] On the other hand, Friedman clearly had mixed feelings about the undergraduate economics that had been instilled in the United States by Samuelson's textbook. He applauded the textbook's progressively greater coverage of monetary policy. But he also implied that he disagreed with some of Samuelson's undergraduate teachings (*The Great Economics Debate*, WGBH Boston, May 22, 1969) and he lamented the fact that there had been a "whole generation of people who have been brought up on the economics principles books of the last twenty years to believe" in a mechanical fiscal policy multiplier (Instructional Dynamics Economics Cassette Tape 91, January 26, 1972). And when, in the late 1960s, Friedman's graduate student Benjamin Klein told him that he lacked an undergraduate background in economics, Friedman replied, "You have an advantage over all the other students, because you don't have to unlearn anything." (Benjamin Klein, interview, March 4, 2013.)

[46] See, for example, the reference to Solow (1956) in Friedman (1970a, p. 221). In addition, the references for "Growth" in the reading list for Friedman's Winter 1968 course, "Income, Employment, and the Price Level" (Economics 332), included Solow (1956, 1957). (Information from Ann-Marie Meulendyke.)

[47] Eric Hanushek, a Ph.D. graduate in economics at MIT in 1968 (see *http://hanushek.stanford.edu/sites/default/files/cv%20_%20Eric%20A.%20Hanushek.9-29-21.pdf*) who would later work at the Hoover Institution, recalled: "While I don't believe we were prohibited at MIT graduate school from reading Friedman, we were certainly not encouraged." (From his remarks at a session of the Federal Reserve Bank of Dallas conference, "The Legacy of Milton and Rose Friedman's *Free To Choose*: Economic Liberalism At the Turn of the 21st Century," October 2003. Recording provided to the author by Mark Wynne.)

include a representative of Friedman's monetary positions (*Newsweek*, October 25, 1976).[48]

Indeed, back in 1966, Friedman had encouraged a new Ph.D. graduate of the University of Chicago economics department, Miguel Sidrauski, to accept an invitation to join the staff of the MIT economics department. Although Sidrauski's dissertation committee chairman had been Hirofumi Uzawa, not Friedman, and its content was more technical than was implied by Friedman's own inclinations, Friedman applauded the emphasis that Sidrauski had given in his dissertation on the economic content of the formal analysis.[49] From his new MIT base, Sidrauski continued to speak highly of graduate study at the University of Chicago.[50] Furthermore, Sidrauski's research was perceived as validating some of Friedman's perspective, and he became something of a MIT representative of monetarism—a status cut short by Sidrauski's premature death in 1968.[51]

In the early 1970s, also, a significant number of MIT graduate students felt that, in the

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[48] In this *Newsweek* column, Samuelson listed Harvard University and Oxford University as having economic departments that took this attitude, pointedly excluding Cambridge University. Consistent with Samuelson's characterization, the desire to have a Chicago School perspective in the economics department figured in Harvard University's hiring in 1969 of two new University of Chicago Ph.D. graduates—Michael Connolly and Nicolaus Tideman (Nicolaus Tideman, interview, May 15, 2015).

[49] See Friedman (1969b) for Friedman's praise for Sidrauski's dissertation. Sidrauski's dissertation committee consisted of Uzawa, Friedman, and Arnold Harberger (Sidrauski, 1967a, p. 534). Guillermo Calvo, who knew Sidrauski and also visited the University of Chicago as a student during the 1960s, observed, in relation to the composition of the committee, that on many matters of advice, "I suspect that Sidrauski had to choose between Uzawa and Friedman," adding that their joint presence on the committee likely was "quite difficult" for Sidrauski—on account of Uzawa's advocacy of, and Friedman's aversion to, mathematical economics. (Guillermo Calvo, interview, April 1, 2014.)

Duncan Foley, a Yale University Ph.D. who worked with Sidrauski when both were assistant professors at MIT, observed that, in part, Foley's hiring had been motivated by the MIT department's desire to hire a representative of the Tobin view of money and Sidrauski's by a desire to have a representative of the Friedman viewpoint. Foley noted, however, that the papers Sidrauski worked on at the University of Chicago and finalized at MIT did not uniformly take a monetarist line, as the "optimal quantity of money paper [Sidrauski, 1967a] is very Friedman-ish, and the money and growth paper [Sidrauski, 1967b] is very Tobin-ish." Foley elaborated that Sidrauski (1967b) "is basically a very Tobinesque paper… in which the idea is that more-rapid monetary growth, by creating more inflation, changes the incentive to invest. But the other [Sidrauski, 1967a], which has been probably even more influential, is a paper on the optimal quantity of money, which has an intertemporal Ramsey model kind of approach, with money balances in the utility function. And in that paper, money is neutral: the paper has all of these neutrality properties that are very characteristic of the Friedman way of thinking about the quantity theory." (Duncan Foley, interview, October 2, 2014.) For an exposition of the Sidrauski (1967a) model that emphasizes these neutrality properties, see McCallum (1990).

[50] Rachel McCulloch, who spent time at MIT before starting graduate studies, recalled that "in macro, I went for the whole semester to Miguel's course, which was very helpful. And when I was moving to Chicago and had a choice between going to Chicago and going to Northwestern, the faculty advised me on the whole to go to Northwestern. But Miguel said, and actually even Joe Stiglitz said, 'Chicago is better.' And I ended up following that advice. He [Sidrauski] was a wonderful mentor." (Rachel McCulloch, interview, October 3, 2013.)

[51] Sidrauski's death on August 31, 1968, was noted in some of his posthumous publications, including Shell, Sidrauski, and Stiglitz (1969, p. 15). See also Foley and Sidrauski (1971, p. viii).

department's economics teaching, they were missing out on more modern developments in U.S. macroeconomics. MIT had been one of the major holdouts against the natural rate hypothesis and some of the other related attempts to recast the dynamics of macroeconomic models. Around the very early 1970s, Richard Anderson, "there was a revolt among the graduate students," and the upshot was that when Anderson himself became a graduate student in 1972, his cohort a recently revamped graduate-course curriculum that reflected "the big revolution… about two years before that" in the teaching of economics at MIT. This revolution continued, and with the arrival of Stanley Fischer in 1973, and later Rudiger Dornbusch, it would involve still further attempts to bring aspects of Friedman's approach to monetary economics into the practice of macroeconomics at MIT. At the end of 1974, MIT's graduate-economics student association gave Fischer a teaching award—he was "a superb teacher," recalled Anderson (Richard Anderson, interview, November 14, 2013). Fischer's teaching and writing in their 1970s and their connection to Friedman's work are discussed further in Chapter 8.

Moving further into public-policy activity

It was just as well for Friedman that the next generation of economists was taking up some of his ideas in macroeconomic teaching. For, apart from his money workshop—which was not truly a graduate class—Friedman himself had not taught a macroeconomics course since the very early 1970s. And as of the start of 1975, he was poised to wind up his teaching altogether. After calendar 1976, he would do no more teaching of university classes.

In addition, he was largely leaving research. This was something that Friedman did not really acknowledge it at the time. Indeed, he probably did not appreciate in 1975−1976 the degree to which his time would be taken up by matters other than research. Michael Darby (interview, October 15, 2013) observed: "I think he never lost his interest in hard academic research [even though] he may not have continued to do a lot of it. But certainly, when he went to Hoover, it was not obvious that he was not going to discontinue doing a lot." Darby recalled visiting Friedman in the second half of the 1970s, when they still had their Vermont home. Darby talked to Rose Friedman while Milton Friedman was "sort of lifeguarding" as Darby's young daughter went for a swim in the pool. "And Rose said, 'All these working papers, all these working papers, I have to show Milton that a paper's been published before I can put it in the mulch.' (*laughter*) I mean, he had stacks and stacks [of others' working papers]."[52]

[52] As of 1982, he had still kept working papers received in the 1960s—a fact evident in the inclusion in Friedman's (1982a, p. 106) references of an item listed as an "unpublished paper dated December 1969." In this case, Friedman had overlooked the fact that the paper in question had in fact been published, as R.G. Davis (1971).

In the event, Friedman was preoccupied with non-research activities. In 1976 the occasion of his Nobel Prize—discussed in Section II below—brought his research achievements to the fore. But his move away from research, already noted in Nelson (2020b, Chapter 15) as being evident in his changing outlook during 1972, continued in 1973–1976, and foreshadowed the 1976–1985 decade that Walters (1987, p. 426) characterized as one in which Friedman put research on the back burner.

Going back to Adam Smith

Some inkling of the niche in public-policy activity that Friedman would find for himself in future years was provided by his lament in 1973 (Instructional Dynamics Economic Cassette Tape 122, June 6, 1973): "We really badly need an Adam Smith who, with the same kind of eloquence, ability to write, ability to attract attention, could somehow or other bring the peoples of the world to their senses. It is incredible, the kind of monumental folly that is being engaged in everywhere throughout the world." In the years after 1976, even more than during his teaching career, Friedman would come to be seen as taking this role.

The topics in which he engaged remained much the same. The circumstances of U.S. debate in the 1970s were of a kind that meant that his shift in public profile during the 1970s did not imply a very sharp change in content from what had characterized the macroeconomic dialogues in which Friedman was frequently engaged. Particularly in the first half of the decade, he had often found himself needing to raise free-market and price-theoretical arguments in *macroeconomic* contexts—most notably, those of inflation and disinflation. Friedman believed that these matters should be regarded principally relating to monetary policy and ultimately involving adjustment of nominal variables. But his opponents in effect obliged him to make use of price-theoretical arguments in the debate, as part of his basis for criticizing the various nonmonetary, market-preempting tools that nonmonetarist economists were offering, in preference to aggregate-demand restraint, as a means of fighting inflation.

On this matter, and applicable also to developments before the 1970s, a relevant observation is Mayer's (1998, p. 288) conjecture, which received strong agreement on the part of McCallum (1998, p. 308), that the fact of "Friedman being so strongly identified with pronounced free-market views" likely intensified the vehemence of the Keynesian-monetarist debate. In the 1960s, that this was the case was evident in the 1966 Walter Heller quotation given earlier, as Heller's description of Friedman as the "chief guardian of the *laissez-faire* tradition" reflected the fact that Heller was accustomed to regarding Friedman as an opponent on both

microeconomic and macroeconomic matters. But in the 1970s, the Keynesian-monetarist and free-market-vs.-intervention debates became further intertwined in the United States—especially in policy-related discussions—because measures against inflation centered on direct intervention in general goods and prices markets became a more central aspect of the policy prescriptions of American Keynesians, and because certain microeconomic interventions by the U.S. government in a specific sector—energy, an area affected by internal oil price controls and various federal regulations of allocation and production—were justified in part on anti-inflation grounds.[53] Conversely, Friedman's objections to these approaches stressed microeconomic arguments—the need for market-clearing prices and relative price adjustments—as well as macroeconomic arguments—principally, the case that the cause of, and solution to, inflation lay in monetary adjustments.

There nevertheless remained a degree of separation of the two sets of debates in principle, even though Keynesians' advocacy of nonmonetary tools against inflation was blurring the distinction. Friedman himself was fond of noting that one could use the quantity theory of money to understand economic relationships and still advocate a large role for the public sector in the economy.[54] In addition, he spelled out the point that, in his view, a state of affairs in which the public sector was large and intruded on market mechanisms would imply worse real economic outcomes even in the event that the interventionist government presided over conditions of monetary and price stability (*Financial Times* (London), January 6, 1977). In the limit, he remarked, unbounded government spending "even if it were accomplished without any inflation whatever… would ultimately destroy our freedom and prosperity." (*Evening Capital* (Maryland), November 18, 1978.)

Another sense in which the two debates were separate rested in the fact that the monetary work was a staple of Friedman's main research work. His discussions of arranging domestic economic activity according to free-market principles were not. It fell mainly into the category of his public-policy activity rather than his research. Correspondingly, for the most part Friedman did not see his advocacy of market economics as constituting an attempt to break new ground but simply to apply existing economic ideas to current problems. "I thought I was going back to

[53] U.S. energy policy in the mid-1970s is considered in Section III below (see the discussion titled "Henry Jackson").
[54] The possibility—which Friedman stressed on a number of occasions in the 1980s—that officials in centrally-planned economies might put considerable store in the quantity theory of money—was something he had previously implied when, in the early 1970s, he hinted that orthodoxy in the Soviet Union may not have had a period of disillusionment regarding monetary policy's effectiveness comparable with the one through which opinion leaders in the Western world went in the 1930s and 1940s. See Friedman (1972e, p. 184).

some fundamentals rather than creating anything new" was how Friedman would characterize his contributions in this area (*Reason*, June 1995, p. 35). In particular, although he was generous in acknowledging his debts to the contributions to the understanding of markets provided by Marshall, Hayek, and others, Friedman was also, on repeated occasions, unequivocal when it came to whom he regarded as his main antecedent in this area. "The theory of a free market in a systematic organized way dates back to Adam Smith in 1776," Friedman remarked in a 1977 television interview.[55] (*The Open Mind*, PBS, May 31, 1977, p. 11 of transcript.) On issues concerning the market, he added to a different interviewer five years later: "When you ask about 'Friedman-type economics,' I would rather say 'Adam Smith economics.'"[56]

The year 1976 saw a number of occasions when Friedman highlighted Smith's views and their parallels with his own. In commemoration of the two-hundredth anniversary of the publication of *The Wealth of Nations*, he gave a talk titled "Adam Smith's Relevance for 1976" at the Mont Pelerin meetings held in St. Andrews, Scotland, in August 1976. Before the anniversary year was over, the University of Chicago's Graduate School of Business had released this Friedman lecture, alongside others concerned with the anniversary, in a packet of pamphlets titled "The Adam Smith Lectures."[57] Friedman then put a light revision of his speech into *Challenge* magazine in the spring of 1977, under the dateless title "Adam Smith's Relevance for Today."[58]

In other speeches that he gave during 1976, Friedman highlighted the fact that the U.S. Declaration of Independence and Smith's tome each dated back two hundred years, to 1776. One of these speeches was a contribution to a conference on the American bicentennial held by the business school of Dartmouth College—a university located not far from Friedman's spring/summer Vermont home. At this conference, held in late May 1976, Friedman essentially chose Adam Smith (and modern applications of Smith's ideas) as his subject for the lecture, which was titled "The Invisible Hand."[59] An earlier talk, given in February 1976 at the University of Pittsburgh, was less specifically about Smith: Friedman's subject this time was "The Future of the American Economy." But on that occasion, Friedman was, if anything, more overt than usual in drawing a lineage between himself and Adam Smith: Friedman opened the talk noting the two bicentennials and stated that, in addressing current U.S. economic issues, "I

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[55] *The Open Mind*, PBS, May 31, 1977, p. 11 of transcript.
[56] In Friedman (1982d, p. 60).
[57] See Friedman (1976f).
[58] See Friedman (1977h).
[59] See Friedman (1977o). The conference was on May 28–29, 1976 (see *Rutland Daily Herald* (Vermont), May 26, 1976). Friedman gave another speech at Dartmouth College the following August (*Christian Science Monitor* (Boston), August 26, 1976).

shall let myself represent Adam Smith's voice."[60]

Some of Smith's contributions so permeated economic thinking that Friedman's championing of Smith's views did not much mark him out from others in the profession. For one thing, advocacy of free trade was largely common ground among U.S. economists. Robert Eisner, during a television debate in which he strongly took issue with Friedman on matters concerning inflation and unemployment, acknowledged this common ground when he observed that "economists, almost as a part of their professional gospel, believe in free trade and comparative advantage." (*University of Chicago Round Table: The Nation's Economy Out of Control*, PBS, May 1, 1974.) For another thing, and notwithstanding disagreement in the 1970s among economists on the role that price controls could play in fighting inflation, there was wide agreement among them in principle with the idea of an exchange-and-distribution system was not governed by central direction but was decentralized and guided by price signals. Friedman emphasized this point and its status as a key Smith insight—too, while also acknowledging Hayek's (1945) work for elaborating on the signaling function played by the price system in this connection.[61] Again this was a role embedded in textbook economics rather than special to free-market exponents, a fact reflected in Robert Eisner's observation in his 1974 appearance with Friedman: "Like the great bulk of economists, I have a very healthy respect for the market processes and the use of the market and the price mechanism."

Free markets and economic growth

Friedman was, however, notably concerned with pushing pro-market arguments further, making them more central in accounts of national economic performance. In expounding his interpretation of Smith's case for the market—and consistent with the title of Smith's 1776 book—Friedman emphasized the application of that case to the macroeconomic realm: he did not see its relevance as being limited to matters pertaining to international trade or individual markets or sectors. Specifically, Friedman saw empirical validation of Smith's views on market economics as having been produced by the historical behavior of productivity growth and growth in living standards under conditions of liberal economic arrangements.

In considering this subject, Friedman zeroed in on the nineteenth century as a period that had

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[60] Friedman (1976h, p. 5).
[61] Among many instances in which Friedman expounded this aspect of the price system and attributed it to Smith, see for example Friedman (1970f, p. 71). In Friedman (1976f, p. 15; 1977h, p. 11), he pointed toward Hayek's (1945) development of the invisible-hand aspects of the price mechanism, and he would cite Hayek (1945) on this point in his 1976 Nobel lecture (see Friedman, 1977c, p. 467).

provided useful case studies. This approach especially characterized the various talks on the market system that he gave during the mid-1970s. With regard to a truly free-enterprise system, Friedman suggested in a 1974 talk published in 1976, “it is wrong to say that it really never existed. It existed about as close to a pure form as you could want in the nineteenth century in both Great Britain and the United States.”[62] In his 1976 speech on Adam Smith’s relevance, he affirmed that “both the United States and the United Kingdom, that century comes about as close to Smith’s system of natural liberty as it is reasonable to hope to achieve.”[63] It was consequently the “the nineteenth century high tide of *laissez faire*” on which Friedman placed heavy emphasis in evaluating the macroeconomic implications of a highly liberalized economic system.”[64]

And as far as macroeconomic results observed in the nineteenth century were concerned, Friedman judged them a considerable success. This evaluation paralleled judgments made by specialists on U.K. historical economic performance, such as Phyllis Deane, who had observed (*New Society* (London), August 11, 1966, p. 220): “There is a good deal of evidence for the view that the British economy reached some kind of peak in its performance in the latter part of the nineteenth century.” In his own citation of the United Kingdom’s experience in this era, Friedman characterized it as the “period when Britain emerged as the leading nation.”[65]

Friedman gave great weight to the role played by internal economic conditions in having produced this outcome, and he observed in a U.K. television interview in mid-1976 that the “ordinary man... improved his lot in the last quarter of the nineteenth century.”[66] In tracing the output and productivity growth that underlay this situation, Friedman differed with some other economists’ judgments not on the facts of real growth but on their basic source. With regard to U.K. economic growth, Austin Robinson (1954, p. 445) had referred to “the largely unexplained acceleration from the slower processes of earlier centuries to the rapid cumulative progress of the past two hundred years.” In contrast to this consensus, Friedman did not regard the acceleration as unexplained. Compared with many other accounts, Friedman emphasized much more forcefully the forces of free enterprise as a driver of growth in incomes and in productivity.[67]

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[62] From Friedman’s remarks in Friedman and Kristol (1976, p. 22).
[63] Friedman (1976f, p. 17), also in Friedman (1977h, p. 12).
[64] The quotation is from Friedman (1976f, p. 17) and also appeared in Friedman (1977h, p. 8).
[65] *Milton Friedman Speaks*, Episode 13, “Who Protects the Worker?,” taped September 29, 1977, p. 32 of transcript. Similarly, Friedman and Friedman (1980, p. 37) referred to “the success of nineteenth-century Great Britain.”
[66] *The Jay Interview*, ITN, July 17, 1976.
[67] Gary Hansen and Edward C. Prescott (2002) also considered the pickup in economic growth after 1800. Most of their account focused on exogenous changes in population growth. However, the authors did, consistent with Friedman’s own account, state (p. 1215): “The fact that the industrial revolution happened first in England in the early 19th century… is perhaps due to the institutions and policies.”

In particular, Friedman and Schwartz had stated in their *Monetary History*: "The 'real' forces are the capability of the people, their industry and ingenuity, the resources they command, their mode of economic and political organization and the like."[68] Friedman regarded the free market as having galvanized these forces in the United Kingdom. "It is no accident that the industrial revolution… had its home in Britain… or that it had its greatest flowering after Britain adopted *laissez faire* as a national policy," he observed in 1970.[69]

Friedman therefore expounded a narrative concerning development and real output growth in which economic and political liberalization to innovations to productivity and economic growth and so living standards. He had said in 1962 that in the United Kingdom over the nineteenth century "individualism and *laissez-faire* seemed to be producing the results that their proponents had promised in the form of an expansion of economic activity, a rise in the standard of life, and the like."[70] He elaborated in 1980: "Britain entered into a period of free trade, and in the latter half of the nineteenth century, *laissez-faire* produced everything that its proponents had claimed for it. Britain prospered and became the leading nation in the world, economically and politically. The level of well-being of the ordinary people in Britain rose at a rate and to a level that had never been seen before."[71] His accounts along these lines did not deny the decomposition of growth according to increases in total factor productivity and production inputs of the kind contributed by Solow (1956, 1957). But they sought to ascribe the increases in factor supplies and in technology to the incentives, generated by free-market conditions, promoting innovation and economic progress.

Friedman saw further examples in nineteenth-century experience beyond the U.K. case. In the Friedmans' book version of *Free To Choose* in 1980, the authors, after contending that the "combination of economic and political *freedom* produced a golden age in both Great Britain and the United States in the nineteenth century," added that, of the two economies, the U.S. economy "prospered even more."[72] Similarly in 1977, Friedman had referred to "the whole period of the nineteenth century… the whole period of the great growth of the U.S."[73] In 1981, he summed up what he took to be the international lessons arising from the era: "If you go back to the nineteenth century, the *laissez faire* policies were very successful… Britain had an enormous

[68] Friedman and Schwartz (1963, p. 696).
[69] Friedman (1970f, p. 70).
[70] Friedman (1962d, p. 3).
[71] Friedman (1981, p. 9).
[72] Friedman and Friedman (1980, p. 3; italics in original).
[73] *Milton Friedman Speaks*, Episode 13, "Who Protects the Worker?," taped September 29, 1977, p. 32 of transcript. See also Friedman (1976j) and *Milton Friedman Speaks*, Episode 2, "Myths That Conceal Reality," taped October 13, 1977.

growth in income, prosperity[,] and influence in the latter half of the nineteenth century when it was following a *laissez-faire* policy. Japan emerged as a modern nation at the end of the nineteenth century[,] when it was following essentially the same policy. And so on down the line."[74] In 1970, he had suggested that still earlier examples in which "widening individual freedom and quickening of economic growth went hand in hand" could be found in certain epochs in Ancient Greece and Ancient Rome, as well as in the Renaissance.[75]

From the example of the nineteenth century, particularly that of the United Kingdom, and the older examples, Friedman distilled the lesson that it was possible for societies to move both *away from* and *toward* free-market arrangements. The examples dating to the more remote past were early cases showing that "when freedom was destroyed, economic decline was not far behind," with golden ages followed by dark ages.[76]

But Friedman also saw a key historical precedent for a sharp move toward economic liberalization in the case of the United Kingdom. Adam Smith, he reminded readers and viewers in 1976, had not written *The Wealth of Nations* in defense of the *status quo* economic system, but as a critique of impediments in force in his own country, imposed or backed by the state, to the workings of internal and external competition.[77] "Adam Smith was a radical and revolutionary in his time—just as those of us who preach *laissez-faire* are in our time," Friedman observed in his main 1976 lecture on Smith.[78] Conversely, in 1969, Friedman observed that in the eighteenth century the United Kingdom was a society and economy subject to heavy government control, so that "the most dramatic example in history" of rolling back state powers was that country's adoption of a *laissez-faire* system in the nineteenth century (Instructional Dynamics Economics Cassette Tape 25, May 25, 1969).[79] That system, as discussed further below, had in turn been

[74] From his remarks in Friedman, Porter, Gruen, and Stammer (1981, p. 4).
[75] Friedman (1970f, p. 70).
[76] Friedman (1970f, p. 70). See also *Milton Friedman Speaks*, Episode 1, "What Is America?," taped October 3, 1977, p. 8 of transcript.
[77] See, for example, *The Jay Interview*, ITN, July 17, 1976, and, later, Friedman (1977f, p. 8; 1978b, p. 8). In keeping with this assessment, Hutchison (1978, p. 24) referred to *The Wealth of Nations* as "a strong attack, from a minority position, against established ideas on economic policy."
[78] Friedman (1976f, p. 1; 1977h, p. 6).
[79] Although Friedman's command of the histories and backgrounds of countries other than his own was, by his own admission, imperfect, his generalizations about the U.K. economy—state-controlled before the nineteenth century, and adopting *laissez faire* in that century—were soundly based. Similar generalizations have been made by many other commentators. For example, Crosland (1957, p. 85) stated that "the *laissez faire* view prevailed for the whole of the nineteenth century." With regard to the forces that Smith criticized that had held back the emergence of market mechanisms and a decentralized economy, see, for example, Winch (1978, pp. 67, 78-80, 95). Friedman (1970f, p. 70) stressed that government control of the economy was particularly acute in the United Kingdom prior to the eighteenth century, and consistent with this position, John Cochrane (*The Grumpy Economist* blog, March 18, 2019) has viewed the country in the sixteenth century as featuring a system of government-supported monopolies.

replaced in the United Kingdom by an economy featuring greater governmental control than that seen in the United States and several other Western economies. In the mid-1970s, Friedman suggested that the United Kingdom might again lead the way among these economies in reversing the increased role of government: "just as Britain has led the world down the path of socialism, she will lead us back out" (*Daily Express* (London), November 30, 1976).[80]

Despite this conjecture, Friedman—in urging a reduction in the role of government in the economy in his own country—was *not* waiting for a move to economic liberalization to take hold in the United Kingdom. Instead, he pressed the case, as he put it in his February 1976 talk in Pittsburgh, for American economic policy to reverse "the movement of the past few decades" and move to a smaller role for the public sector.[81] The respects in which he urged the government's intervention to be pared back were multifold. They included government spending and taxes (discussed in the next section), specific and general price controls, and reduced public ownership of industries.

A further area in which he called for change was captured by his support for another word identified as part of the "new English" by the *6,000 Words* book: "deregulation."[82] Friedman had used this term on his audio commentary series in late 1974 (Instructional Dynamics Economics Cassette Tape 155, October 10, 1974; Instructional Dynamics Economics Cassette Tape 160, December 19, 1974). His usages had mainly been in the context of energy-price decontrol, rather than the broader application of the term to refer to the removal of regulations affecting businesses. However, he was personally closely associated, particularly in public discourse, with the broader usage of "deregulation," with *New York Times* economics writer Leonard Silk going so far as to assert: "A great deal of recent work in regulatory theory—and [of] the push in Washington for deregulation—is the result of his work." (*New York Times*, October 15, 1976*a*.)

In addition, it should be noted that Friedman (1962a, pp. 5−6) and other discussions granted that state controls had been relaxed by Smith's time from their stricter configuration of the seventeenth century. Therefore, the fact that growth speeded up *before* the nineteenth century did not contradict Friedman's narrative.

[80] Broadberry (2022) traces the slowdown in U.K. economic growth to slower growth in total factor productivity. Although no doubt differing from Friedman's diagnosis on many details, his assessment for this slowdown is broadly consistent with that outlined by Friedman, with Broadberry (2022, p. 1), observing: "TFP growth has been an important proximate source of Britain's rise to GDP per capita leadership and also of Britain's relative economic decline since 1870. However, the ultimate source of these developments in technology lies in the institutional framework. Britain's rise to GDP per capita leadership occurred as innovators responded to the factor price combination that they faced within an environment shaped by the Enlightenment. After 1870, British relative decline occurred as barriers to competition arose and slowed the response to technological change."

[81] Friedman (1976h, p. 5).

[82] G. & C. Merriam Co. (1976, p. 66).

This broader conception of deregulation was the concern of a National Conference on Regulatory Reform at Loews L'Enfant Plaza Hotel held in Washington, D.C., on May 25−27, 1976 (see Biegler, 1976). Friedman attended the conference, which included a session devoted to a debate, on regulatory agencies, between himself and Ralph Nader.[83] In his opening contribution to this debate, Friedman proclaimed that it was "appropriate to assert that regulation by government of business has, on the whole, done far more harm than good," and had hindered the ability of "market competition to play the role it should play in forcing, inducing, and cajoling all of us to act in the public interest."[84]

This debate between Friedman and Nader reinforced their image as a double act in public debate. They had previously debated the social responsibility of business, in one of Friedman's later major interventions on this matter, at a conference at an executives' conference at the Chicago Marriott hotel in November 1973 (*Chicago Daily News*, November 20, 1973). And later in 1976, in September, they would be arrayed against each other to give live commentary on the just-concluded first Carter-Ford presidential debate, in a panel discussion broadcast on Los Angeles radio (*Los Angeles Times*, September 23, 1976).

The other major broadcast medium, television—on which Friedman was already a familiar face—became, in the mid-1970s, an outlet in which he participated to an even greater degree than previously. The macroeconomic strains of these years played a key role in generating this outcome. Outside the financial pages, there had a boom of broadcast-media interest in Friedman's views before—in the early Nixon years of 1969−1971. This media ubiquity had receded somewhat with the apparent success of wage and price controls. But the collapse of wage and price controls after 1972 had then forced a reconsideration of the merits of Friedman's economic perspective: reviewing the 1975 edition of Friedman's collected columns, Samuel Brittan noted that the collection "has attracted a great deal of attention because of his prescience (not complete, but greater than that of most policymakers) on the money supply, inflation incomes policy[,] and related topics."[85] In addition, of course, since 1972 the economic matters with which Friedman was most connected once again even more central items in daily news in the United States. Introducing a television special on the economy that featured Friedman as a guest panelist, Edwin Newman observed (*NBC News Special: If You Think It Was Tough To*

[83] See Friedman and Nader (1976).
[84] Friedman and Nader (1976, p. 4).
[85] Brittan (1977, p. 162). Brittan was reviewing the differently-titled variant (Friedman, 1975j) of Friedman's (1975a) *There's No Such Thing As a Free Lunch*. Notwithstanding Brittan's observation, *Free Lunch* was not a major seller, and, when mentioning the book in an appearance on *Donahue* (NBC, September 30, 1975), Friedman took it for granted that neither host Phil Donahue nor most of the program's studio audience had heard of the book.

Make Ends Meet in 1974, Wait 'Till You Hear About 1975, NBC, January 1, 1975) the priority that U.S. citizens had come to give economic news: "The economy is now the chief preoccupation of the people of the United States." Correspondingly, as Friedman observed around the same time (Instructional Dynamics Economic Cassette Tape 159, late November/early December 1974), "Economic activity has become the major obsession of every politician."

Friedman appeared on television programs produced by channels local to the city of Chicago, as a panelist on PBS specials, and, as indicated in previous chapters, as a guest on national television shows, including the public-television financial program *Wall Street Week* in both 1975 and 1976 and NBC's *Meet the Press* in October 1976.[86] In 1975–1976, he also had a recurring slot as a commentator on network television's *CBS Morning News*, giving direct-to-camera capsule commentaries on recent news items. Friedman's appearances in this capacity were sufficiently frequent that, in the mid-1970s, he was listed as part of the lineup of regulars on *CBS Morning News*.[87]

II. ISSUES RELATED TO DEBATES ON REGULATION, THE PHILLIPS CURVE, AND POLICY RULES, 1975–1976

POLICY RULES AND THE CONSTITUTION

"I have just been reading a book about the ideological origins of the American Revolution," Friedman told Phil Donahue in an appearance on the latter's daytime talk show (*Donahue*, NBC, September 30, 1975). The mid-1970s period in which Friedman made this remark was, as we have just seen, one that saw him reflecting on the trends in thinking in the direction of economic liberalism that had prevailed on both sides of the Atlantic in the eighteenth and nineteenth centuries. But over this same period, it was also the case that one outgrowth of the American Revolution—the U.S. Constitution—was weighing heavily on the way in which Friedman came to look at his longstanding macroeconomic research interest of rules for monetary policy and fiscal policy.

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[86] He also featured prominently as an interview subject in a *60 Minutes* segment, broadcast in October 1976, on the economic problems of the United Kingdom. See the previous chapter.

[87] See David (1976, p. 30). One of Friedman's *CBS Morning News* contributions in this period was discussed in the previous chapter. Also in the mid-1970s, Friedman provided commentaries for CBS radio news (Instructional Dynamics Economics Cassette Tape 160, December 19, 1974). Over the years, Friedman had also appeared occasionally as an interview guest on another national morning news program, NBC television's *Today* show, which featured Friedman in late 1969 (see Nelson, 2020b, Chapter 15) and on July 18, 1974 (*Independent/Press-Telegram* (Long Beach, California), July 18, 1974).

In the same 1975 *Donahue* appearance, Friedman remarked that one of his grounds for optimism about the United States was that "we can change the way in which our government operates, and we can do it by legal and constitutional means." Indeed, increasingly, Friedman saw constitutional amendment as a means through which to institute rules for U.S. monetary policy and for state and federal fiscal policy.

Reactions functions and rules

As of 1975−1976, it was becoming best practice in the economic-research literature to represent U.S. economic policymakers as, in a sense, *already* following rules. Robert Lucas remarked in 1976 that the Federal Open Market Committee very likely had some implicit rule connecting its decisions to observed aggregate magnitudes: if this were not the case, he observed, its actions would have no statistical relationship to the evolution of the state of the economy.[88]

On this interpretation, policy rules should be defined not to include hypothetical prescriptions—like Friedman's constant-monetary-growth rule—but also the systematic component of the actual, empirical policymaker "reaction function." The latter term was apparently coined, in a microeconomic context, by George Stigler in the 1940s.[89] By the 1970s, it had become a prevalent way of referring to empirical approximations, made by researchers, of the law of motion followed by the authorities when setting the values of their monetary and fiscal policy instruments.

Consistent with Lucas' 1976 outline, the monetary policy research literature had, by the early 1980s, become accustomed to treating different policy positions as choices of different rules, rather than dichotomizing them in terms of rules vs. the absence of rules (see Sargent, 1987, p. xxi).[90] This practice was subsequently cemented by prominent studies such as Taylor (1993) and Clarida, Gali, and Gertler (2000), which approximated actual FOMC behavior using policy-rule

[88] See Nelson (2020a, Chapter 8) for documentation regarding this matter.
[89] James N. Marshall (1992, p. 10) observed: "[Microeconomist] James W. Friedman… believes that it was William Fellner [1949] who coined the term 'reaction function,' which is now part of the permanent lexicon of the economist." The "reaction function" formulation was, however, used earlier, in Stigler (1940, pp. 526, 532).
[90] For specific examples during 1980–1983 in which Karl Brunner, Bennett McCallum, and Allan Meltzer recognized and followed this practice, see Nelson (2019; 2020a, Chapter 8). To some extent, this recasting of the debate reflected the fact that the term "discretion" had been used loosely in the previous literature and had often been invoked in reference to what were in fact activist, but fixed-response, policy rules. For example, Okun (1975b, p. 84) referred to Friedman's (1953d) analysis (discussed in detail in the next subsection, on the complexity of stabilization policy) as concerned with "discretionary fiscal-monetary policy policies," when, in fact, that 1953 study had been concerned with different settings of constant-response-coefficient policy rules or reaction functions.

equations.[91]

The notion that actual policy could be understood as partly the result of the operation of a systematic reaction function was something Friedman acknowledged. For example, in the 1970s he referred to the Federal Reserve's typical reaction pattern.[92] And in his 1987 *New Palgrave* dictionary entry on the quantity theory of money, he distinguished the ordinary course of policy decisions from policy actions that arose in an "unpredictable, *ad hoc* way" of the kind associated with unanticipated policy changes.[93]

Friedman nevertheless continued to see a material difference between actual policy and his own policy-rule recommendations. As he saw it, the rules he recommended amounted to material improvements in the reaction function that would favorably affect economic performance.

The differences between Friedman's recommended rules and actual policy were multifold. First and most obviously, and as discussed further below, Friedman saw aggregate-demand settings, and no alternative approach, as capable on their own of delivering conditions of average price stability, and he sought formulations of monetary and fiscal policy rules aimed at achieving that to that end. Second, with regard to monetary policy, and as stressed in the discussion of monetary targeting in the preceding chapter, he wished to make monetary quantities both the instrument and target of monetary policy. Third, he wanted the rules announced in advance. For Friedman, there was a very substantial difference between a rule made explicit in advance and a reaction function or policy pattern that was left implicit, to be deduced or estimated after the fact. He had long believed in the notion—for which the rational expectations literature would

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[91] These later studies, of course, focused on estimated reaction functions for the short-term interest rate, a practice much less common in the earlier literature. This practice was not, however, unheard of. William Dewald, who was a member of the University of Chicago's economics department in the early 1960s, recalled that, during that period, Harry Johnson "asked me to coauthor something with him [that would] basically be on the basis of some work that had been done by some Canadian friends of his that were working on kind of a reaction function." The resulting paper, Dewald and Johnson (1963), presented estimates of what "got to be a big thing later on"—interest-rate reaction functions. "Harry and I did something like that there. Basically, my minor contribution was to suggest that, instead of just doing a reaction function in terms of the money supply, let's do a reaction function in terms of some of the things these central banks are focusing on. So we did some preliminary kinds of reaction functions with free reserves and short-term interest rates [as left-hand-side variables]." (William Dewald, interview, April 25, 2013.) Indeed, Svensson (2003, p. 427) conjectured: "The first empirical estimates of interest-rate reaction functions may have been in the 1960s by William Dewald and Harry Johnson (1963) and James Christian (1968)." In fact, however, Tinbergen's (1951) study of the historical behavior of the U.K. economy had estimated a discount-rate (Bank rate) reaction function.
[92] See Nelson (2020a, Chapter 8).
[93] Friedman (1987a, p. 15).

eventually provide strong support—that the stabilization properties of a policy rule rested heavily on the rule being known in advance.[94]

Fourth, Friedman sought rules that involved less policy activism, and specifically a smaller reaction to variations in real economic activity. His monetary policy rule did imply responses of interest rates to real economic developments but not the large response to estimated output or unemployment gaps seen in practice. And the fiscal policy rules he advanced, like those he had advocated in the past, would concentrate on automatic reactions of tax revenues to the state of the economy and avoid adjustment of tax rates or spending programs to the business cycle. In addition, as his rules would eliminate nonsystematic policy adjustments, they would also forestall any policy responses resource utilization that were due to *ad hoc* deviations from the reaction function.

This case against policy activism was gaining greater support by the mid-1970s. As discussed in Chapter 4, repudiation of the notion of "fine tuning" was widespread, though great differences still remained between Keynesians' and monetarists' policy proposals regarding the degree of policy activism. And despite President Ford's success in pushing through a tax rebate as a one-time anti-recessionary measure in 1975, the doubts on the merits of this as a stabilization action were more pervasive than in the case in 1968 regarding President Johnson's tax surcharge. By 1975, the 1968 surcharge episode was already being cited in textbook analyses as a vindication of Friedman's position, with Paul Wonnacott (1974, p. 346) noting that "Friedman's theories… suggested that the failure of the economy to respond quickly to the tax surcharge of 1968 should have come as no great surprise."

On the climate of opinion during this time, Paul Krugman (*The Conscience of a Liberal* blog, July 9, 2017) has observed: "In macroeconomics, there's no question that Milton Friedman and, initially, Robert Lucas performed a useful service by challenging the case for policy activism, especially fiscal activism. Circa 1976 the track record of Chicago macroeconomics was impressive indeed."

A fifth and crucial feature of rules from Friedman's perspective was that should be adhered to, once introduced. Though he accepted that, for specified stretches of years, consistent aspects of policy behavior could be identified, he was also struck by the changeability of policy. In 1963,

[94] Of course, such advance knowledge of the reaction function could arise if the general form of the policymaker's reaction function was widely understood by the private sector even though not explicitly announced in advance.

the *Monetary History* had criticized the recurrent "backing and filling" on the part of policymakers during the history of the Federal Reserve.[95] Furthermore, in the 1960s and 1970s, some of the consistent aspects of stabilization policy that Friedman perceived were features that he viewed as undesirable. These included the tendency of the Federal Reserve to generate undue volatility in monetary growth and, after the 1950s, the unwillingness by the administration and the central bank to stick to a disinflationary policy once it was initiated. In this connection, his *Newsweek* column of November 4, 1974, had highlighted the succession of monetary policy U-turns in the period since 1963. Against this background, Friedman became well disposed toward introducing a hard edict to stick to preannounced monetary and fiscal policy rules by embedding those rules in the American polity, through making them constitutional requirements.

Economic research on public choice

The shift on Friedman's part toward constitutional amendment as a means of instituting policy rules reflected, to an important degree, his exposure to, and positive reaction regarding, the economic-research literature on public choice. *University Economics*, a 1964 text by Armen Alchian and William R. Allen that Friedman had cited in the mid-1960s, was an early example of research of this kind that impressed Friedman. Allen recalled that "while I think we probably did not ever use… the term 'public choice,' in fact, there is a great deal of that sort of thing scattered all through the book as an integral part of the theory of the firm and economic activity in general… [T]he substance was there, even if we didn't use the terminology." (William R. Allen, interview, March 14, 2014.)[96]

Friedman's interest in the public-choice literature picked up during the early 1970s, and in that period he had outside speakers at his money workshop present their contributions to the literature. One of the workshop attendees, Phillip Cooper, sensed a "much greater orthodoxy" in these presenters' outlook than in Friedman's own (Phillip Cooper, interview, September 18, 2015). Friedman subsequently showed increasing signs of absorbing public-choice arguments into his own thinking, with arguments of this kind figuring particularly prominently in his writings in the period from 1975 onward (see Nelson, 2020a, p. 322). Most likely, Friedman overestimated the capacity of the public-choice approach to provide useful positive analysis of historical policy developments. For example, the public-choice literature tended to portray inflation outcomes as part of a policymaker masterplan. This approach neglected the possibility

[95] See Friedman and Schwartz (1963a, p. 193).
[96] For Friedman's citations of Alchian and Allen (1964), see Friedman (1966a, pp. 23–24; pp. 103–104 of 1968 reprint).

(considerably stressed in Friedman's own accounts prior to 1975) of policymaker misunderstandings of economic relationships prompted misconceived policy actions and associated aggregate economic results, including high inflation.

In the area of normative economics, the public-choice literature suggested that constitutional change was a means of locking in arrangements that were conducive to longer-term economic stability. This appealed to Friedman, for whom the benefits of policy rules included the role they played of imposing a self-denying ordinance—one that prevented policymakers from considering their choices on a case-by-case basis.[97]

Monetary policy rules

Friedman affirmed in the mid-1970s that he favored monetary arrangements that delivered zero inflation on average: "now we say, 'How fortunate we are to have only 4.5 percent [inflation],'" he remarked unhappily in an October 1976 television interview on. "We cannot tolerate—[or], it sems to me, this economy and society should not tolerate a rate of inflation that high. We have to make our goal to have zero inflation." (*Meet the Press*, NBC, October 24, 1976, p. 8 of transcript.)

Friedman had indicated in a 1974 television panel appearance that he felt that zero inflation was "politically viable" in a way in which a 5 percent inflation rate was not: "there is some sense to a no-inflation position." Maintenance of any inflation rate in preference to a higher one required a longer-term time perspective to be adopted in policymaking, and he suggested that "if the government does not have a zero inflation target," it was harder to instill that longer perspective into public discourse. Friedman believed the general public would be able to accept the reasoning "that we should have no inflation" and so be willing to accept the consequences of policies designed to bring inflation back to zero after a disturbance: in contrast, it might be harder to gain acceptance for actions designed to bring inflation back to 5 percent after it was pushed up to 7 percent (*University of Chicago Round Table: The Nation's Economy Out of Control*, PBS, May 1, 1974).

Stanley Fischer, for one, concurred with this argument. In a July 1974 conference, Fischer was noted observing that "pressures on a central bank… make it hard to stabilize the rate of price increase at any value other than zero. Theoretically, there is no particular reason to choose a

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[97] Friedman (1962b, p. 241; p. 192 of 1968 reprint).

zero rate; yet it is probably the only rate the central bank can choose. Once there is a move from zero, there are pressures to keep increasing the inflation rate."[98]

In the event, of course, central banks (including two on which Fischer would serve as a monetary policy committee member) would embrace price-stability objectives but favor expressing these in terms of a 2 or 3 percent inflation rate, rather than a zero one. Although he favored a zero inflation rate, and continued to do so in later decades, Friedman was in the 1970s not badly disposed toward an inflation rate of about 2 percent. In 1976, when a questioner referred to "perhaps a 2 to 3 percent inflation rate" as something to which the country could aspire, he did not raise the same objections that he voiced about 4.5 percent inflation (*Meet the Press*, NBC, October 24, 1976, p. 8 of transcript).

In the mid-1970s, however, the era in which most central banks accepted their responsibility for establishing the average inflation rate was still far off, and Friedman was concerned with instituting arrangements that would hasten the arrival of this situation. In terms of specifics, however, he did not advocate direct targeting of the inflation rate. His longstanding position was that the looseness over short periods in the relationship between central bank actions and an economy-wide aggregate like the price level meant that a direct objective of price stability should not be assigned directly to the monetary authority, which instead should be assigned an intermediate objective: a monetary-growth target, one that was numerically specified to achieve a steady-state inflation rate of zero.

It was on the desirability of imposing a policy rule on central banks, rather than on constitutional change, that Friedman had focused when he gave a lecture, "Should There Be an Independent Monetary Authority?," for a 1962 book, *In Search of a Monetary Constitution*. That book's editor, Leland Yeager, recalled: "The University of Virginia had a series of lectures on a monetary constitution in 1960, and these lectures were eventually published as a book in 1962. In his lecture, Friedman discussed whether there should be an independent central bank. I wonder if he would take a different stand nowadays than what he took in 1960, because nowadays, it seems to be standard doctrine among the theorists of central banking that

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[98] In Aliber (1977, p. 253). Fischer was speaking at the conference, "The Political Economy of Monetary Reform," held at Racine, Wisconsin, on July 24–27, 1974. Fischer restated the argument, albeit as one that should be accepted only on a provisional basis, at a conference held twenty years later, on June 9, 1994, in London: see Fischer, 1994, p. 281.) In July 1975, Herschel Grossman also referred to this argument for zero inflation that Friedman had made, sourcing it to the appearance Friedman had made on PBS in May 1974 alongside Herbert Stein (see Kyn and Schrettl, 1979, p. 452).

independence of central bankers is desirable—[as it means] that there's less inflationary bias and that they're sheltered, somehow, from politics." Yeager went on to observe that "the theme of the lecture was that the central bank is part of the government, that if we want a democratic government, it ought to be under government control," and that, in light of the fact that "the Treasury and the Federal Reserve are unavoidably late in [appreciating] the consequences of what they're doing," that control should include the requirement to follow a constant-monetary-growth rule (Leland Yeager, interview, August 8, 2013).

Friedman's recommendation in this lecture was that constant monetary growth should be made "a legislated rule."[99] In 1975, as discussed in the preceding chapter, Friedman believed that major strides had been achieved in this direction, via Congress' resolution that the Federal Reserve state, and report on, its planned monetary-growth rate. By 1977, however, he very unhappy with the degrees of freedom that the Federal Reserve was giving itself in the setting, interpretation, and pursuit of monetary targets. Monetary targeting had given rise to some improvements, he suggested, by making the central bank lay out plans regarding monetary policy. But he called for going beyond legislation when it came to imposing a monetary policy rule: "It would be far preferable to have it as a constitutional amendment." (*Reason* magazine, August 1977, p. 27.)

As well as being an extreme and logistically difficult to achieve step, having monetary-growth targets as a constitutional requirement had inherent tensions with Friedman's monetary analysis. Over the decades, he almost invariably believed that a monetary aggregate that included key commercial bank deposit liabilities, such as M1 or M2, was more closely related to the economy than was the monetary base. Correspondingly, his recommended targets had typically been for these aggregates, especially M2. And although the monetary base and M2 enjoyed a statistical relationship with one another, the two series by no means moved in lockstep. Notably, there was evidence that the money multiplier, especially when M2 was used as the measure of money, had exhibited an upward trend in 1970–1975. In the face of this situation, Friedman argued that "the Fed can allow for changes in the multiplier" in pursuing monetary targets (Instructional Dynamics Economics Cassette Tape 183, January 1976, Part 1).

In particular, Friedman wished the authorities to use the base or reserves as their instrument and "offset by open market operations" major changes in the money multiplier, in order to achieve a

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[99] Friedman (1962b, p. 242; p. 193 of 1968 reprint).

desired growth rate in the overall money stock.[100] Although this was practical advice that might well be straightforward to follow in the case of a legislated target, it was difficult to embed into a constitutional amendment as an unambiguous edict. The tentative solution (of sorts) that Milton and Rose Friedman eventually settled on, in their books published in the 1980s, was to offer for consideration, as a proposed constitutional amendment regarding monetary policy, a rule requiring that the rate of monetary base growth stay in a constant range.[101]

Monetary policy, in general, and monetary-growth rules, in particular, were not, in any event, central to Friedman's interest in using constitutional alteration to shape the U.S. economic-policy framework. Instead, in the area of constitutional reform, his main focus was on fiscal policy.

Fiscal policy rules

With regard to national fiscal policy, the necessity that Friedman saw for a fiscal policy rule arose in part simply from the need to make it possible for the authorities to follow his preferred monetary policy rule. In order for such a monetary rule to be adhered to, the fiscal arrangements had to be of a form that foreclosed the possibility of inflationary creation of base money due to large-scale monetization of deficits.

In the case of the United States, however, Friedman did not regard this requirement for the separation of monetary policy and fiscal policy as very demanding. He was confident that U.S. Treasury securities markets were well enough developed that even the occurrence of large federal budget deficits (by U.S. standards) would have no necessary implications for monetary base growth, monetary aggregates, and inflation.

It was in light of this judgment that Friedman indicated that his concerns about fiscal policy primarily lay elsewhere. "There is an enormous problem about fiscal policy, but I do not believe that problem has much to do with the deficit. It has to do with the size of spending," Friedman remarked in March 1971.[102]

In particular, the longer-term supply-side effects of fiscal policy underlay Friedman's interest in rules that put restrictions on the size of the budget. In 1965, he had laid out among the questions for fiscal policy in the decade ahead "[c]riteria for determining the fraction of national income

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[100] Friedman (1974d, p. 22).
[101] See Friedman and Friedman (1980, p. 308; 1985, p. 99).
[102] Friedman (1971f, p. 15).

that is appropriately spent through the Federal Government," as well as an assessment of the role played by various taxes "in terms of cyclical effects and effects on long-term economic growth."[103] The latter comment indicated his position that both the tax mix and the overall size of taxes affected the supply side of the private-sector economy. But the concern he expressed with public spending *per se*, not just the taxes that partly financed it, reflected Friedman's view (already discussed in Section I above) that a shift to a larger-scale government budget tended inherently to have adverse consequences for the supply side of the economy.

Friedman's concern about government spending flowed also from his views about economic freedom, political freedom, and the sources of and interrelations of these freedoms. In his view, expressed especially forcefully in the mid-1970s, a large national expenditure share on the part of the public sector preempted private-sector decision-making, thereby inhibiting economic freedom, and undermined political freedom, both by reducing the amount of activity in the community that arose from voluntary cooperation and by taking the amount of resources through government decisions to a level beyond what could be sustained by a community consensus.[104] A public sector whose spending was a large share of national income "constitutes, in my opinion, the most serious threat to the maintenance of a free society," Friedman observed (*The Economist* (London), September 28, 1974). He added in a television appearance two years later: "I think that the trend toward ever-higher government spending must be stopped if this country is to retain its freedom." (*Meet the Press*, NBC, October 24, 1976, p. 6 of transcript.)

Friedman recognized that much of the postwar expansion of public spending in the United States and elsewhere had come from growth in transfer expenditures, rather than from increased government purchases of goods and services.[105] He was, however, resistant to the notion that transfer expenditures should be regarded as private-sector spending or as tax relief for the private sector. Friedman's discussions of government spending, once he became a monetarist, reflected an assessment that government outlays that constituted transfer spending, on the one hand, and outlays that were the public sector's purchases of goods and services, on the other hand, were more similar to each other than economic analysis often implied. In particular, he believed that, as an analytical matter, it was often *not* useful to view transfers as negative taxes and that instead, they should be accepted as being much more akin to government purchases in their economic implications.[106] In particular, Friedman stressed that transfer payments, like purchases,

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[103] Friedman (1965a).
[104] See, for example, Friedman (1968c, p. 27; 1976c).
[105] See, for example, Friedman (1975g, p. 12).
[106] In his pre-monetarist period, in which he subscribed to a belief in strong effects of aggregate demand on fiscal policy, Friedman did make the standard spending/transfers distinction. For example, Friedman (1948, p. 246)

involved a compulsory rerouting of funds from the activities to which they would have been directed had they not been absorbed into the government budget, to activities that were basically of the public sector's choosing.[107] He would therefore very likely have approved of Dornbusch and Fischer's (1981, p. 38) observation, "we speak of transfers plus purchases as government expenditure."

The growth in public expenditure had occurred principally because of appropriations made by U.S. state and federal legislatures. But, Friedman argued, there was "a fundamental flaw in the budgeting process" (*Detroit Free Press*, October 15, 1976, p. 4A).[108] As discussed in the previous chapter, in the 1970s Friedman came to put increased emphasis on the notion that individual government interventions, including spending initiatives, achieved legislative passage as part of a discourse in which a small group was vocally supportive, while the community at large was indifferent or only mildly supportive. The ramifications for the total budget and the tax burden, still less what Friedman saw as the adverse supply-side implications of a larger public sector, did not figure importantly in the legislature's considerations of each spending bill individually. The net effect, he believed, was a larger public sector than the community would have approved if given an opportunity to have its will reflected in decisions concerning the aggregate size of the public sector.[109]

Against this background, as Friedman explained in a retrospective, he concluded that "the constitutional route is the only effective way out."[110] He sought, he explained in 1978, support for a constitutional ceiling on overall public spending, with the aim of "having a setup under which we, in trying to fight for our special interest, have to fight within a limited budget."[111]

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specified that government purchases should be "defined to exclude transfer expenditures of all kinds." His later analysis acknowledged that transfers were more automatically cyclically sensitive than purchases, but it did not make a strong analytical distinction between the two categories of government outlay. This change in perspective resulted in part from the fact that he was no longer a subscriber to frameworks that hinged on the existence of multiplied effects on aggregate spending of (unmonetized) fiscal policy actions. Analyses predicated on the usefulness of the fiscal multiplier did (and do) tend to stress the transfers/purchases distinction.

[107] See, for example, his discussions in Instructional Dynamics Economics Cassette Tape 109 (October 18, 1972) and in Friedman (1976a, p. 315).

[108] See also his remarks in *Newsweek*, November 15, 1976*a*.

[109] In criticizing Friedman for disregarding "the preferences of citizens as voters" (*The Economist* (London), October 23, 1976*a*, p. 94), James Tobin apparently overlooked this aspect of Friedman's argument. The same discussion erroneously stated that Friedman approved only voluntary—presumably Coasean—solutions to the pollution problem, thus ignoring Friedman's support for pollution taxes. These features of the article point toward the likelihood that Tobin had been sparing in his reading of Friedman's work of the previous several years—especially those writings that were outside the field of monetary economics.

[110] Friedman (1989b, p. xiii).

[111] *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978, p. 18 of transcript.

The U.K. example

One of the examples that Friedman cited, in October 1975 Congressional testimony, as demonstrating his proposition that "Government both retards economic growth and destroys human freedom" was the recent experience of the United Kingdom.[112] Over the subsequent year, the example provided by the U.K. economy would likewise figure prominently in his campaign for constitutional reform of the U.S. budgetary process.

A perspective on the economic failures of the United Kingdom at the time—and the consequent status of that country as a negative example for the United States—can be brought out by considering a concept that Senator Hubert Humphrey highlighted at the same October 1975 hearing: the economic "misery index."[113] The misery index simply consisted of the sum of the unemployment rate and the (twelve-month) inflation rate. Humphrey suggested that, in the previous couple of years, the index had been recording a value of about 16 in the United States. This assessment was accurate: for example, in June 1975, the index's value was 17.9 (the sum of the country's 9.3 percent consumer price inflation and its 8.6 percent unemployment).[114] But this performance was far better than that of the United Kingdom—which in the month of June 1975 recorded a misery index of 30.9 (26.2 percent inflation and 4.7 percent unemployment).[115] The U.K. reading on the misery index had deteriorated dramatically since June 1965, when that country's misery index had been 6.2 (a 4.9 percent inflation rate plus a 1.3 percent unemployment rate).[116] In addition, the United Kingdom's real output growth rate was slower than that of Japan, Germany, France, and the United States in both 1960−1973. This ranking below the other four countries was maintained in 1973−1979—a period in which all five countries grew more slowly than previously (see Morgan, 1982, p. 21).

For Friedman, the United Kingdom's poor performance in the 1970s and that country's previous loss of its nineteenth-century status as an economic pace-setter had a common source: the expanded role of government. The U.K. experience, in his view, provided a good example of the

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[112] From Friedman's spoken testimony of October 20, 1975, in Joint Economic Committee (1975d, p. 48).

[113] Humphrey referred to "what I call, to put it in simple terms, the economic misery index" (Joint Economic Committee, 1975d, p. 2). The "misery index" terminology and definition had apparently been devised by Arthur Okun (*New York Times*, December 12, 1980).

[114] For the inflation and unemployment components of the misery index in that month, see, respectively, OECD (1981, p. 65) and *Chicago Tribune*, July 4, 1975.

[115] For the inflation and unemployment components of the U.K. misery index in that month, see, respectively, OECD (1981, p. 65) and *Aberdeen Evening Express* (Scotland, U.K.), July 24, 1975.

[116] See respectively, *Liverpool Echo and Evening Express* (England, U.K.), July 17, 1965, and Department of Employment (1971, p. 319).

defect in the political structure that he stressed, under which a sizable shift in the direction of a greater role of government in the economy had largely arisen from the cumulation of incremental increases in the public sector's role. "A major reason why public opinion turned against *laissez-faire* was precisely because it was so successful," he argued, suggesting that piecemeal growth in the U.K.'s public sector had been prompted by the combination of economic growth and the belief that economic conditions could be improved still further by more interventionist policies.[117] "After *laissez faire* delivered everything it had promised, England started its slide toward socialism," he proclaimed.[118] The growth in government's role was reflected in the fact that in the mid-1970s, as Friedman frequently pointed out, U.K. public spending was well over 50 percent of national income.[119] In mid-1976, Friedman discussed the country's loss of stature on U.K. national television: "Certainly nobody would describe it as a leading country—economically, politically or in any other way." (*The Jay Interview*, ITN, July 17, 1976.)

The example that the U.K. economy provided was salutary, Friedman suggested. When campaigning in October 1976 for constitutional reform aiming to restrict the size of government

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[117] Friedman (1981, p. 9). Later still, the United Kingdom also had its counterpart of the Great Depression experience in which opinion leaders largely blamed the economic slump on the remaining *laissez faire* features of the U.K. economy. As discussed in Nelson (2013b) and in the previous chapter, Friedman saw the U.K. interwar slump as resulting not from these features but instead to monetary restriction, compounded by generous unemployment benefits.

[118] In comments at his October 1972 Festschrift, recorded in Selden (1975, p. 51).

[119] In an article in the London public-policy magazine *Encounter* (see Friedman, 1976c) and in numerous interviews in the mid-1970s, Friedman prominently cited a 60 percent ratio of public spending to national income as that prevailing in the United Kingdom. Some subsequent accounts condemned Friedman for doing so—contending that he was irresponsible in accepting and repeating this number (see, for example, Browning, 1986, p. 232, and Tomlinson, 1990, p. 287). Indeed, in late 1976 Chancellor of the Exchequer, Denis Healey, strongly criticized Friedman on this score, implying that Friedman's recitation of the 60 percent value showed him to be ill-informed (see Nelson, 2009b, p. 494). In fact, however, the number of 60 percent came from an official U.K. government source—Healey himself. A U.K. Treasury report on the public finances, presented to parliament by the Chancellor of the Exchequer in February 1976, had stated on page 1: "The ratio of public expenditure to gross domestic product has risen [since 1972] from 50 percent to 60 percent." (HM Treasury, 1976, p. 1, paragraph 2.) Subsequently, the U.K. government adopted OECD methodological practices when measuring government spending, and this change played a major part in reducing the reported public-expenditure share (see Gowland, 1979, pp. 231−232). However, the OECD methodology did *not* classify most of the expenditures made by public-sector-owned enterprises as government spending (see Alesina and Perotti, 1995, p. 215). This expenditure was, all the same, government spending, and Friedman believed that it should be so classified. (The OECD's basis for excluding these expenditures was that the enterprises in question largely financed their operations by selling their product to the private sector. But this really pertained to how the expenditures were *paid for*—and did not amount to a denial of the status of the expenditures as government spending.) Under its own classification of public expenditures, the OECD gave the United Kingdom's government-spending-to-GDP percentage for the year 1975 as 46.2 percent (*OECD Economic Outlook*, June 1989, Table R−14, p. 185). In the United Kingdom during the early 1970s, public-sector corporations' spending was considerably above 10 percent of GDP (*Financial Times* (London), August 14, 1975). Taking these two items together, it is evidently accurate to state that U.K. public spending in 1975 was well over 50 percent of GDP and was, indeed, likely close to 60 percent. This number was also used at the official level in the United States, with President Ford stating in a speech given on October 28, 1976 (Ford, 1976): "Government spending in Great Britain now accounts for 60 percent of the entire British economy."

budgets in the United States, Friedman declared: "We are on a path, this nation, like that of Great Britain, which is in great financial difficulty—in an enormous financial crisis. We are following the same road, only about twenty years behind. If we keep going this way, we, too, will face utter chaos." (*State Journal Register* (Springfield, Illinois), October 15, 1976.)

Character of a fiscal policy rule

Friedman told an audience in London in September 1976: "There is one device we have been trying to work out in the United States that has been receiving wide attention… We have tried to institute constitutional provisions setting a limit to the maximum amount of money as a fraction of the national income that governments may spend in all directions."[120]

Starting with California's Proposition 1 in 1973 (discussed in the previous chapter) and well into the 2000s, Friedman would devote much of his own time in providing advice on, and campaigning for, measures of this kind. Among his activities in this area were testimony he gave on January 7, 1976, to an Illinois House of Representatives revenue-committee hearing in favor of a constitutional amendment limiting state government spending (*Chicago Sun-Times*, January 8, 1976; *Journal Star* (Peoria, Illinois), January 8, 1976), a press conference in mid-September 1977, which he gave jointly with national Congressman Phil Crane (R-IL), on a joint American Conservative Union/National Tax Limitation Committee campaign for constitutional limits on states' taxation (*Chicago Daily News*, September 14, 1977), and an appearance in Boston a few days later in favor of a limitation on spending by Massachusetts' government (*Boston Globe*, September 17, 1977).

Almost from the start, however, Friedman sounded a note of caution about attempts to restrict the economic footprint of government via constitutional means. The example of the New Deal, he cautioned, suggested that, if the mood of the American public overwhelmingly favored an expansion of the role that government played in the economy, the constitution would be reinterpreted to accommodate this change.[121] Constitutional provisions would eventually be reinterpreted to accommodate this expansion. Consequently, in his view, constitutional

[120] Friedman (1977i, p. 60).
[121] Friedman (1978a, p. 13; reprinted in Friedman, 1978b, p. 21). In this discussion, Friedman argued that, "sixty or a hundred years ago," both the general public and the country's courts would regard the U.S. government's powers over the economy, on the scale they had as of the 1970s, as being unconstitutional. "Once the fundamental attitude of the public changed, however, constitutional restrictions became very much less effective against the growth of government," Friedman contended, with the Supreme Court's interpretations of the U.S. constitution changing conformably.

amendments had the status of "a stopgap measure to hold back the tide"—and should be regarded as designed to limit the expansion of government until a broader community consensus in favor of limiting the public sector's economic role was attained.[122] Friedman indicated that he believed that this judgment applied to both state and federal constitutions (*New Guard*, April 1977, p. 8).

Their stopgap status notwithstanding, Friedman was excited about the prospect of constitutional limits on government budgets. At first, he concentrated on activities in this direction that were proceeding at the state level. Different parts of the United States had varying degrees of popular support for restrictions on government, with some states in the mid-1970s characterized by a stronger sentiment against an increased role of the public sector in the economy than the case in the nation as a whole. In addition, many state constitutions could be altered by referenda—a situation that made campaigns for state constitutional change more akin to an election campaign to attract voters' endorsement than to a legislative process of seeking support from elected officials for a new measure. Largely for these reasons, the initial major constitutional moves with which Friedman were involved pertained to proposed changes in the governance of specific states. In his 1976 remarks, he mentioned two of these: 1973's defeated Proposition 1 in California, and a referendum scheduled for "this fall" (that is, coincident with the November 2 presidential election) in the state of Michigan.[123]

As in the case of California, Friedman was involved in the composition of the measure that was being advanced—Proposal C—of Michigan's constitution, and he campaigned for it in the state.[124] The manner in which Friedman articulated the case for Proposal C hinted at the gulf that was emerging between himself and fellow advocates of constitutional limits on state and federal budgets. Proposal C would impose both tax and public-spending limitations: it aimed to put a ceiling on both the tax take of, and spending by, the state government—specifically, of 8.3 percent of state personal income (*The Detroit News*, October 15, 1976).

In his advocacy of Proposal C, Friedman stressed the spending limitation aspect of the proposition. He remarked on national television with regard to growing government outlays (*Meet the Press*, NBC, October 24, 1976, p. 2 of transcript) that "in order to stop this growth, we need a change in our political structure. I said this in connection with campaigning in Michigan

[122] Again, see Friedman (1978a, p. 13; reprinted in Friedman, 1978b, p. 21).
[123] Friedman (1977i, p. 60).
[124] On Friedman's role in working with the makers of Proposal C, see *The Detroit News*, October 15, 1976, and his remarks in *New Guard*, April 1977, p. 8. On his campaigning for the amendment, see the discussion of Friedman's Nobel Prize announcement below.

for a proposal, on their ballot, which would set a maximum limit to government spending." While present in Michigan to campaign for the measure, he had said of it that "it gives the taxpayer a chance to express his view on how much of his money he wants the state to spend" (*The Detroit News*, October 15, 1976), while helping assign priorities: "It would force government spending to be devoted to vital expenditures." (*Detroit Free Press*, October 15, 1976, p. 4A.)

This emphasis on the spending-restraint conflicted with many others on Friedman's side. They preferred to stress the tax-limiting, rather than public-expenditure-limiting, aspect of such amendments. This difference of opinion regarding which aspect of these restrictions should be given center stage went back to the 1973 proposal under Governor Reagan. This schism was clear from the name of the government-appointed committee that had, starting in 1972, shaped that proposal: the tax-limitation tax force.[125] Lewis Uhler, who had chaired this committee, was, on returning to the private sector, a major financier and organizer of the constitutional-amendment movement in California. The group he formed in 1975 to broaden the movement was called the National Tax Limitation Committee (*San Francisco Chronicle*, March 5, 1978).

Friedman was a member of this committee—indeed, a 1979 discussion referred to "the National Tax Limitation Committee, of which Dr. Friedman is a principal member."[126] His name was therefore closely linked with tax limitation—so much so that, when the tax-revolt movement reached its highest-profile success at the state level in 1978, he was probably the most well-known public face of that movement. And though, as it happened, Uhler himself shared Friedman's emphasis on restrictions on public spending alongside limitations on taxes, this was less true of the broader limitation campaigners, and the constitution-amendment drive would become perceived as part of a broader movement known as the "tax revolt," rather than a spending revolt.

Reflecting the emerging intramural dispute in the constitutional-limits movement, Friedman wrote an article in 1978 on the "limitations of tax limitation," in which he indicated that he did not regard it as a major victory if taxes were restricted but government spending was not. Though he continued to support measures to reduce taxes, Friedman's image in public discourse with tax limitation would decline as his disagreements with tax limitation (as an aim in isolation) became apparent and as the profile of the supply-side movement—which was critical of, and

[125] See *San Francisco Chronicle*, March 5, 1978, and Friedman (1989b, p. xiii).
[126] Association of the Bar of the City of New York (1979, p. 123).

competitive with, Friedman on a multitude of issues—increased.

Federal versus state constitutional amendments

This difference in emphasis between tax limitation and spending limitation figured still more importantly at the federal level. For federal—as distinct from U.S. state—budgets, the distinction between taxes and government expenditure was underlined by the fact that, compared with the situation pertaining to state budgets, U.S. federal government spending had much greater scope to move in relation to tax revenue. This was on account of the fact that the federal government had far greater legal and financing wherewithal to run sustained deficits than state governments did.

Friedman believed that, even when governments possessed greater scope to run deficits, tax revenue ultimately *did* limit U.S. government spending. But he believed it did so through a "starve-the-beast" political mechanism—a mechanism that he believed was powerful, but whose operation was not an automatic, official part of the budgetary system and was not necessarily rapid in exerting effects on legislated appropriations.[127] Matters were further complicated by the possibility that tax cuts might actually raise tax revenue. This was not a scenario Friedman believed was usually realistic as he did not believe the short-run supply responses to tax cuts would be strong enough to dominate the responses of tax revenue.[128] But large supply responses, and associated benefits for tax revenue, were touted as an advantage of a tax cut by many campaigners for limitation of taxes—a position that would become famous when illustrated using the Laffer curve. Indeed, it was not unknown in the national political debate of the 1980s for Arthur Laffer and others to oppose particular proposed federal spending cuts while enthusiastically endorsing tax cuts—testament to their conviction that restraints on tax rates need not be, and perhaps should not be, a means of restricting public expenditure.[129] So, in the late 1970s and early 1980s, when it came to the federal campaign for constitutional limits on the budget Friedman was even more at odds with those emphasizing restriction of tax rates than he had been in the state campaigns.

In the mid-1970s, there was not much of a concrete national political consensus in favor of restricting federal spending—much less imposing such a restriction by permanent constitutional

[127] See Chapter 9 below for a full discussion.
[128] At least in the case of unmonetized deficits, he furthermore did not expect a tax cut to provide a major stimulus to nominal aggregate demand.
[129] See later chapters for discussions of these debates.

means. In 1976 Friedman indicated that the Southern Governors Conference had assigned a task force to design a constitutional amendment that would limit federal spending to a specified share of national income.[130] Involvement of state governments in a proposed constitutional amendment made sense, as one condition for the passing of an amendment was ratification by three-quarters of the fifty U.S. states. However, the fact that the Southern Governors' Conference was a prime mover regarding the proposed amendment pointed toward the obstacles to passage that it would face. For that fact underscored the absence of really broad-based political support for such an amendment. Positions on the matter did not divide wholly along party lines, as the Southern Governors' conference included Southern Democrat governors. But the Southern states were perceived as traditionally better disposed toward fiscal restraint, and belief in federal spending restriction had not really attained strong national bipartisan support. In 1975−1976, there *was* a national consensus to reduce public spending in the United Kingdom. But a parallel consensus had not arrived in the United States. The increase in spending had been bipartisan—"over the past decades, government spending has risen both in Republican and [in] Democratic administrations," Friedman noted—and the resistance to winding back spending also had bipartisan foundations.[131]

This state of affairs would continue during the Carter years. Though that presidency opened with the collapse of a tax-rebate stimulus proposal in 1977 (see Chapter 8) and so featured some hint of resistance toward traditional fiscal policy activism, it saw federal spending grow as a share of GDP over the rest of the decade. Reining in federal government expenditure remained politically polarizing in the second half of the 1970s, in contrast to another small-government initiative: deregulation, aspects of which the Carter Administration embraced. And if there was not a national mood in favor of a change in current policy action that would move current fiscal policy firmly in the direction of restriction of public spending, there was hardly going to be a mood favoring the *permanent imposition* of restrictiveness, by constitutional amendment.

Despite these developments, Friedman came out of the 1975−1976 period strongly favoring constitutional limits on federal government spending as the next step. "I think myself that there is fundamentally only one way in which you can remedy it—that is by a political change in the form of constitutional provisions which will set a limit to government spending," he observed in February 1978.[132]

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[130] Friedman (1977i, p. 60). See also his remarks in *New Guard*, April 1977, p. 8.
[131] The quotation is from Friedman's appearance on *Meet the Press*, NBC, October 24, 1976, p. 2 of transcript.
[132] *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978, p. 17 of transcript.

Partly because of popular dislike of the idea of unbalanced budgets, the notion of a federal balanced-budget requirement had more community appeal than spending limits. In recognition of this reality, Friedman—as discussed further in later chapters—tended in the period from 1978 onward to cast his recommended limits on the federal government's spending share as a component of a proposed balanced-budget amendment. He therefore went from having described a federal balanced-budget amendment as "a serious mistake" (*New Guard*, April 1977, p. 8) to speaking in public forums from 1979 to 2003 in favor of such amendment.

This switch obviously put Friedman in the position of having to give elaborate explanations of his own posture, as he continued to discuss his support for balanced-budget amendments mainly in terms of the spending limitations they included and to be quite dismissive of the macroeconomic implications of budget deficits. Indeed, critics of his budgetary recommendations could point to statements, including those of recent vintage, in which Friedman had articulated the view that deficits' effects, both good and bad, were exaggerated. He continued to subscribe to this position, and he also maintained his longstanding assessment that cyclical federal deficits were desirable if they arose automatically. In common, therefore, with his support of tax limitation, it was the indirect or subsidiary aim of the balanced-budget amendment that was the basic source of Friedman's support for such an amendment. What the *Wall Street Journal*'s writer Irving Kristol said of Friedman's and some others' aligning themselves with the 1978 agitation for a federal income tax cut was also true of Friedman's subsequent attitude toward a balanced-budget amendment: "so many… take the odd position that the [main] thinking behind [it] is all wrong… but they (including Dr. Friedman) support it anyway." (*Wall Street Journal*, August 10, 1978; p. 50 of 1979 reprint.)

These further activities of Friedman in the area of constitutional amendment had occurred in the wake not only of the failure of California's Proposition 1 in 1973 but also of Michigan's Proposal C in November 1976. The referendum on the latter proposition saw the "yes" side for which Friedman had campaigned lose by about fourteen percentage points (*Detroit Free Press*, November 4, 1976).

Friedman attributed the failure of this initiative to a scare campaign similar to that he believed had occurred in connection with the California ballot in 1973 (*Newsweek*, November 15, 1976*a*). In particular, he identified teachers' unions, in the Michigan case the Michigan Educational

Association, as important forces campaigning for a "no" vote.[133] Friedman was far from flattering about such groups: "The enemies of this proposal are now feeding at the trough of state government," he had remarked when campaigning for the amendment (*Detroit Free Press*, October 15, 1976, p. 4A). But, although he had made a point of stressing that his visit to Michigan to go on the stump for the amendment had been made at his own expense (*The Detroit News*, October 15, 1976), Friedman could hardly complain about the idea that interest groups on the other side of the debate had mobilized and financed a concerted campaign against Proposal C. Although he recorded that he was "dismayed" by the defeat and attributed this outcome to the campaign against the proposal made by "government employees and other special-interest groups," Friedman's postmortem on the referendum also acknowledged that "we call [something] a special-interest group [only] if we are not members of it" (*Newsweek*, November 15, 1976*a*). In the same vein, a couple of months later he referred to "the special interests, and here I mean you and me."[134]

Nobel recipient

During the closing years of Friedman's time at the University of Chicago, his departmental colleague Harry Johnson had, as Friedman did, a regular arrangement of being located away from the city of Chicago, and correspondingly the university, for roughly half of each year. Johnson nevertheless was struck by the degree of Friedman's frequent absences, on account of outside speaking engagements, during even the part of the year in which Friedman was stationed in the city of Chicago—so much so that Johnson remarked critically, and publicly, on this fact.[135]

For other in-demand academic economists, of course, absences to give research seminars or conference appearances at other universities were a common reason for short trips away. In Friedman's case, however, the opportunity to participate in public-policy activities had become, by the 1970s, a major reason for him to fly out of Chicago. Friedman's campaigning in Michigan for the passage of Proposal C, discussed above, was one example of such engagements. That day-trip took place during the academic year 1976/1977, which had begun on August 31, 1976 with an orientation/registration week, followed by the start of classes on

[133] See *Newsweek*, November 15, 1976*a*, Friedman (1977f, pp. 11−12), *Saturday Evening Post* (May/June 1977, p. 18), *Reason* (August 1977, p. 26), *Milton Friedman Speaks*, Episode 6," Money and Inflation" (taped November 7, 1977; p. 33 of transcript), and *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?" (taped February 6, 1978; p. 18 of transcript). For its part, the Michigan Educational Association was happy to take the lion's share of the credit for Proposal C's heavy defeat (*Detroit Free Press*, November 4, 1976).
[134] Friedman (1977f, p. 10).
[135] Specifically, Johnson (1974, p. 219), in an article titled "Major Issues in Monetary Economics," insisted that discussion at the university of these issues "continues to flourish while the master is orating elsewhere."

September 7 (*Daily Republican Register* (Mount Carmel, Illinois), September 2, 1976). Jerry Dwyer, one of the final students among those whose dissertation Friedman supervised, recalled: "I remember when I was a graduate student… he went off one day to campaign for some initiative in some state. And then came back the same day. You know, most professors don't do that sort of thing." (Gerald P. Dwyer, interview, August 20, 2013.)

This also turned out to be one of Friedman's final absences during his years at the University of Chicago. It took place only two and a half months before the start of January 1977—the time when the Friedmans relocated to California, ahead of his formal retirement from the University of Chicago at the end of the 1976/1977 academic year.[136]

Specifically, the trip to Detroit, Michigan, took place on October 14, 1976.[137] The key event on Friedman's schedule for this visit was participation in a press conference in favor of Proposal C to be held at the Detroit Press Club. And, although Proposal C would in the event be defeated, its proponents could not complain that Friedman's brief visit to Detroit in connection with the proposal had not generated major media interest. It turned out to be a far higher-profile trip than planned, gaining international news coverage. For the trip occurred exactly when both Friedman and the world at large learned that he had won that year's Nobel Prize in economics.

One of the vice presidents of the University of Chicago called Friedman's personal assistant, Gloria Valentine, at home early in the morning. Her initial reaction was that she "thought something awful must've happened for them to call me" until the caller let her know "that they had been trying to reach Professor Friedman because he had gotten the Nobel Prize. And immediately, I said that he had left, on an early flight, and I gave them the flight number, to

[136] For more on Friedman's retirement from the University of Chicago and his closing months at the university, see the next chapter.

[137] The recent trip to Michigan was alluded to unfavorably by Larry Martz in a *Newsweek* profile (October 25, 1976*b*) of Friedman that marked his Nobel award. The profile overall found favor with Rose Friedman (1977, p. 26). But a passage of it essentially implied that Friedman's campaign for Proposal C as an example of Friedman's "penchant for controversy and right-wing political activism" (p. 88). (Martz wrote this passage partly in the context of describing the attitude toward Friedman prevailing in Sweden, so it may have been primarily a reference to earlier connections Friedman had with the activities of Barry Goldwater and Ronald Reagan. However, the *New York Times*' Leonard Silk had also referred to "his right-wing political views" in a post-Nobel piece on Friedman appearing on October 15, 1976.) In the United States, unlike for example the United Kingdom, "right-wing" has traditionally strongly connoted sympathy with non-democratic or fringe political views—in a manner in which "conservative" (to describe Republican-sympathetic political persuasion) or "pro free-market" (to describe adherence to economic liberalism) do not. The term was therefore eschewed by Friedman as a label of his own position and regarded by him as an epithet. Friedman lamented in October 1977: "I have often seen… references [in the U.S. media] to 'right-wing Republicans.' When's the last time you saw a reference to a 'left-wing Democrat?'" (Friedman, 1977k, pp. 490–491.)

Detroit." Valentine called the airline in order that the plane crew on Friedman's flight could be asked to hold Friedman back as the other passengers disembarked and he could be apprised of the news. In the event, this did not occur, and the organizers of his Detroit event, who took him from the airport, did not know of his receipt of the prize (Gloria Valentine, interview, December 5, 2013).[138] When Friedman arrived at the parking lot of the Detroit Press Club, he found heavy media attendance (*The Detroit News*, October 15, 1976). "Professor Friedman said he thought, when he saw all those reporters, that they had really done a good job of publicizing the tax limitation thing. (*laughter*) And one of the reporters asked him how he felt or something, and he didn't know what he was talking about. And it was a reporter who told him that he'd gotten the Nobel Prize." (Gloria Valentine, interview, December 5, 2013.)[139] Rose Friedman similarly recalled on the basis of what had been relayed to her by her husband: "While he knew the amendment was getting coverage, he wondered, 'This much?'" (*The Anchorage Times* (Alaska), June 10, 1977.)

Friedman noted to the Detroit reporters that he felt "a lot richer than I did a half-hour ago"—a reference to the payment of $180,000, tax-free, that came with the prize (*The Detroit News*, October 15, 1976).

Also among those in the city of Chicago who had been involved in unsuccessful efforts to notify Friedman of his award were Rose Friedman and George Stigler. Stigler was occupying the Friedmans' longtime apartment, located in Dorchester Avenue, Chicago, after it had been decided that he would take it over after the Friedmans' move.[140] It was Stigler who was the first in Friedman's circle who learned that he had won the Nobel prize.[141] Stigler then contacted Rose Friedman through a 6.30AM telephone call (*The Anchorage Times* (Alaska), June 10, 1977), but Milton Friedman had already left earlier in the morning for his flight to Detroit.[142]

As news spread of the award, it was the case that, as Valentine recalled, at Friedman's office for the remainder of "that day, the phones were ringing like crazy… A lot of big shots called that day, and I enjoyed talking to them." Paul Samuelson was "one of the first calls… which I

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[138] The organizers with whom he drove included the aforementioned Lew Uhler. See Friedman's remarks in Friedman and Friedman (1998, p. 441) and Friedman (1977j).
[139] Again, see also Friedman and Friedman (1998, p. 441) and Friedman (1977j), as well as Friedman (1986c, p. 90).
[140] See Friedman (1993, p. 773).
[141] In Friedman (1993, p. 773), this information was indicated as having been relayed by a phone call between the Nobel Committee and Stigler. In addition, R.D. Friedman (1977, p. 24) relayed the fact that the Friedman household received a telegram from the Nobel Committee, one possibly initially communicated by being read out over the telephone.
[142] Again, see Friedman (1993, p. 773).

thought was pretty neat." Former Governor Ronald Reagan also called. As Reagan called direct—that is, his was the first voice Valentine heard when she took the phone call—Valentine's was dubious. Her first reaction was that because, in the case of public officials, "I was so accustomed to assistants or secretaries asking you to hold on and then putting the person on… I thought some of the young professors in our econ department were pulling a joke on me… And two of them passed the door, so I decided, maybe this is real, because they are not on the phone, obviously, they [just] walked by. (*laughter*)." With regard to the Reagan call, Valentine remarked that "he was very pleasant, and I enjoyed talking to him." (Gloria Valentine, interview, December 5, 2013.)

Valentine also noted that "one of the people who called was President Nixon. And he was rather strange, to say the least." The former president had been living in California following his resignation over two years earlier and, other than a February 1976 visit to China that was poorly received in the U.S. media, had mostly kept a low profile since accepting a pardon from President Ford in September 1974. Valentine expected that Nixon would not stay on the line once informed that Friedman was not present: "Everybody else who called—and a lot of people, government officials, called—they were to the point. [Including] some of our local congressmen—Illinois [congressmen] who were in Washington." In contrast, Nixon seemed in no hurry to hang up and proceeded to chat at length with Valentine, who said that "all I can remember is that it was sort of a wandering conversation." Valentine did not relish a long talk—"I never liked Nixon"—but went along with the former president's open-ended approach to their conversation. During the exchange, Valentine signaled Jose Scheinkman, an assistant professor in the department who worked in an adjoining office and who was helping to take that days' stream of Nobel-related calls, to join her phone line and listen in on the Valentine/Nixon conversation. "I wanted him to hear. (*laughter*) So he silently picked up the phone to listen." Scheinkman "had a peculiar look on his face… as he listened. (*laughter*) And after Nixon hung up, he came out and shook his head, like, 'He's crazy.'" (Gloria Valentine, interview, December 5, 2013.) For his part, Friedman, who later caught up with Nixon for their own telephone conversation, was delighted that the former president had taken the time to call personally with his congratulations (*Chicago Tribune*, November 28, 1976).

Friedman indicated that, though he had not specifically anticipated winning the Nobel prize that year, receiving the award had not come as a major surprise to him. Within the economics profession, even without inside information about the Nobel Committee's deliberations, "everybody knows who are the people who are in contention" for a Nobel Prize, on account of the small pool of likely candidates (*Dinah*!, March 23, 1977). Even a magazine distant in subject

matter from economics, the U.K. popular-science magazine *New Scientist*, had suggested (December 4, 1975, p. 601) that "Milton Friedman… by now must be an odds-on favorite for a Nobel prize."

Indeed, Friedman's name had regularly been mentioned in the American press as a leading candidate for the economics Nobel since 1969, when the economics Nobel was first created.[143] In 1973, Niels Thygesen was asked by the Nobel Committee to write a report on Friedman's research contributions. This request was an example of the Committee's practice of asking "for evaluations, normally two or three years in advance—so they have a stock, always, of candidates that they look at" (Niels Thygesen, interview, February 10, 2015).

Friedman gave a press conference in his lecture room the day after his receipt of the prize.[144] On this occasion, he did allude to reports that the Nobel Committee was reluctant to give the award in light of the disapproving attitude, prevalent among committee members, toward his political and public-policy activities, and he concluded that the award for his research contributions demonstrated that they were capable of "making that separation successfully" (*Chicago Tribune*, October 16, 1976).[145] Rose Friedman indicated that she and their son and daughter (who in 1976 were based on the East Coast and West Coast, respectively, rather than in the city of Chicago) took the award announcement in their stride: "we're not a very excitable family." (*The Anchorage Times* (Alaska), June 10, 1977.)

THE PHILLIPS CURVE AND THE COMPLEXITY OF STABILIZATION POLICY

On his national television appearance on *Meet the Press* ten days after winning the Nobel prize, Friedman remarked that, pleased though he was about having received the award, "I believe the true test of a scholar's work is the judgment that is made not at the time his work is being done, but twenty-five and fifty years later… [T]he embodying in the whole body of economic analysis of some of the work I have done… is what I would regard as a real pinnacle." On this basis,

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[143] See, for example, *Boston Globe*, October 27, 1969, and *Chicago Daily News*, October 27, 1970.

[144] Friedman had rejected the suggestion made to him on receipt of the news of his Nobel that he immediately fly back to Chicago, in order to give a same-day press conference. He insisted on instead fulfilling his commitment to promote the Michigan initiative (*The Coshocton Tribune* (Coshocton, Ohio), October 27, 1976).

[145] On Friedman's visit to Sweden to accept the award, see the next section and, especially, the next chapter. It is also worth noting that while Solow (2012, p. 43) states that he had never (until 2012) heard that Friedman's free-market advocacy had been a possible factor slowing down his receipt of the Nobel award, it is the case that as early as October 15, 1976, a *New York Times* analysis stated that that advocacy had "probably kept him waiting in line for a few extra years."

Friedman suggested that with regard to the pinnacle of his career, “I hope it is in the future.” (*Meet the Press*, NBC, October 24, 1976, p. 2 of transcript.)

As of 1976, however, the Nobel Committee’s assessment of Friedman’s major research contributions was relayed by its citation for his prize: “for his achievements in the fields of consumption analysis, monetary history and theory, and for his demonstration of the complexity of stabilization policy.” (Quoted in *Washington Post*, October 15, 1976, p. A4.)

The award was therefore essentially threefold: for his consumption research; for his work specifically on the role of money, in particular the U.S. *Monetary History* with Anna Schwartz; and for his research on the complexity of stabilization policy, as represented most prominently by his theoretical studies of full-employment policy and the Phillips curve.

The permanent income hypothesis concerning consumption

When asked by reporters in Detroit about the Nobel citation, Friedman replied with regard to its first part: “I guess they’re referring to a book I wrote thirteen [sic; nineteen] years ago, *A Theory of the Consumption Function*, that explains what determines the percentage of their income [that] people will spend on consumption.” (*The Detroit News*, October 15, 1976.)[146]

As discussed in Nelson (2020a, Chapter 5), this research had two parallel characteristics: (i) it was what Friedman regarded as his best piece of scientific work; and (ii) it was his contribution that received the most widespread acceptance across the economics profession. His consumption findings bridged the Keynesian-monetarist divide because, despite respects in which the permanent income hypothesis was a challenge to existing Keynesian analysis, it could be regarded as offering a refinement of Keynesian concepts. Reflecting this status of Friedman’s consumption work, as early as 1955 Friedman’s book (cited as being tentatively scheduled for publication) appeared in the reading list for a business-cycle course taught at Harvard University

[146] Friedman immediately went on to discuss the *Monetary History*, so it is possible that the incorrect “thirteen years ago” time frame he gave for Friedman (1957a) resulted from his confounding of the publication dates of the consumption and monetary-history books. It is also possible that the transcribing reporter transposed “thirteen years” and “nineteen years” in Friedman’s overall reply. A still further possibility is that Friedman erroneously associated *A Theory of the Consumption Function* with 1963 because that was the year in which his follow-up article regarding (and fixing up) the book’s analysis appeared (see Friedman, 1963b).

A later instance in which Friedman discussed the reasons for his Nobel award consisted of a talk in September 1983, given at Stanford University, that was specifically concerned with what research had led to his prize (*San Francisco Chronicle*, September 23, 1983).

by Alvin Hansen and John H. Williams (see Department of Economics, Harvard University, 1955).

Friedman's analysis of consumption also encountered less resistance than much of his post-1951 macroeconomic work because the former was more technical and explicitly model-based, putting it more in step with trends in economic-research practices. Over and above this factor, however, *A Theory of the Consumption Function* had a material, and widely accepted, economic implication—one stressed well before its publication when Margaret Reid, who had worked on similar ideas herself, observed at a May 1952 conference: "if we could measure and classify families by the permanent component of their income[,] we would find the same percentage of their income saved at all economic levels."[147] It was exactly this aspect of the permanent income hypothesis on which Paul Samuelson 1976 concentrated when, a short time after Friedman won the Nobel, he explained the hypothesis to readers of Samuelson's *Newsweek* column.[148]

Monetarism: monetary history and the role of money

Something of an urban myth developed over the years that Friedman's analysis of consumption was the *sole* basis for his receipt of the Nobel prize. Certainly, the press release (of October 14, 1976) announcing the award—a document that is not to be confused with the much briefer official citation, quoted above—did include the statement: "From the purely scientific point of view, Friedman's other achievements are of greater interest than his monetary analysis."[149] In the former connection, the release then referred to the consumption work. But the press release also made it clear that this was not the sole item underlying the award. So even that release, in its totality, did not contradict the official Nobel citation—which, as noted, listed multiple grounds

[147] Reid (1953, p. 219). Earlier in the page, Reid referred to the work of Friedman and Kuznets (1945), which had coined the "permanent income" terminology.

The connections between Reid's research on consumption and that of Friedman are discussed in Nelson (2020a, Chapter 5). Also discussed therein is the connection with Rose Friedman's research, including Brady and R.D. Friedman (1947). In an interview for this book, Michael Walker, a close friend of the Friedmans, observed that "in a curious way, Milton's work on the consumption function came out of Rose's work. Dorothy Brady and Rose were working on that kind of thing, on distributions of income and consumption and poverty and all that kind of stuff… But it isn't until you read [Brady and R.D. Friedman, 1947]—you know, until you read that, you don't realize how much of it [Friedman, 1957a] was derivative of ideas that Rose would have inculcated in him... I know in the case of *A Theory of the Consumption Function* that some of that was as a result of her work: it caused Milton to think [further] about distributions of consumption." (Michael Walker, interview, June 21, 2013.) Rose Friedman herself remarked: "When Milton received the Nobel prize, I felt I shared it with him." (*California* magazine, October 1984, p. 72.)

[148] Specifically, Samuelson remarked (*Newsweek*, October 25, 1976*a*, p. 131 of 1983 reprint): "Friedman's investigation… revealed that, once we get used to being permanently at a higher income, we in fact save *much the same fraction* of income!"

[149] Royal Swedish Academy of Sciences (1976).

for the prize.

In creating a widespread perception to the contrary—one that supposed that *A Theory of the Consumption Function* fully encompassed the material underlying Friedman's prize—some of the early press coverage of the award likely played a key role. A next-day analysis (October 15, 1976*a*) by the *New York Times*' Leonard Silk opened by asking the question: "Why was Milton Friedman awarded the 1976 Nobel Memorial Prize in Economic Science?" Later in the opening paragraph came an apparent answer. Crucially, this was a sentence that—apparently because of a typesetting error—did not scan and was also factually erroneous: "Dr. Friedman's contributions in economics to be [sic] singled out by Sweden's Royal Academy of Science was his consumption analysis." (*New York Times*, October 15, 1976*a*, p. 13.)[150] This sentence probably meant to start with something like the word "Among." Absent such a word, however, the clear implication of the sentence was that Friedman received the award for nothing other than the permanent income hypothesis. In fact, both the Nobel citation and the remainder of Silk's article made it clear that this was not the case.

The notion that Friedman's consumption work was his sole research contribution nevertheless died hard. For example, Brittan (2005, p. 295), while validly noting of Friedman that "[h]is most widely praised achievement" was *A Theory of the Consumption Function*, made the additional, and far less well founded, observation that it was "*the* work prominently mentioned in the citation for the Nobel Prize" (emphasis added). Similarly, Solow (2012, p. 43) not only registered his own clear judgment that consumption-function work should have been the only basis for Friedman's Nobel award, he also stated that that work was what had been "*particularly* cited" by the Nobel Committee (emphasis added). In fact, as the citation quoted above indicated, other Friedman work was mentioned too.

A depiction of Friedman's prize that went to the opposite extreme appeared at the end of the 1970s, when the *New York Times* gave a capsulized account of Nobel winners in economics (October 21, 1979*b*). This account described Friedman as having won the award (solely) "[f]or his influence in developing monetarism, the school that stresses the importance of the money supply."[151] This, too, was a misstatement. What *was* true was some of Friedman's monetarist work was unambiguously *part* of the work underlying Nobel. This fact was apparently denied

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[150] This is in the form in which the sentence appears in the copy of the article archived electronically in the *New York Times* historical digitized back-run. An identical version appears in a version of the article, obtained in 2004 by the author, in a press-clippings collection.
[151] Likewise, *U.S. News and World Report* (January 31, 1983, p. 66) stated that Friedman received the "Nobel Prize in 1976 for his work in monetarism, the theory of money supply."

by another economics newspaper writer, J.A. Livingston, who claimed that it would be a mistake to see the award as constituting a win for monetarism (*Philadelphia Inquirer*, October 20, 1976). It emerged, however, that Livingston was merely noting that the award was not an endorsement of Friedman's constant-monetary-growth rule. Livingston himself in effect acknowledged that some of the research contributions associated with Friedman's monetarism *had* been honored by the award in his concluding paraphrase of the citation: "the Nobel award was conferred for his achievements in consumption analysis, monetary history and theory, and for his demonstration of the complexity of stabilization policy."

Precisely what were the specific contributions to monetary theory that underlay the award was left unclear in the wording of the citation. Little clarity was provided by the accompanying press release, which referred to such a laundry list of Friedman contributions in the monetary area that it was possible to point to some of them (notably on decision lags and on floating exchange rates) as separable from Friedman's work specifically on monetary aggregates. The press release accompanying the Nobel citation remarked that Friedman's studies were "founded on a new formulation of the theory of demand for money or liquid resources." [152] But the citation itself did not specifically refer to Friedman's 1956 restatement of the quantity theory of money. And that 1956 article had probably lost prestige by the mid-1970s, even while Friedman's overall standing had risen.[153]

The citation was, however, quite specific in referring to Friedman's work on monetary history. And the Friedman-Schwartz *Monetary History* was unmistakably part of Friedman's monetarist body of writings.

Friedman's research work in the area of monetarism had therefore been part of the foundation for his receipt of the Nobel.[154] Consistent with this, Paul Samuelson, in an audio commentary

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[152] Royal Swedish Academy of Sciences (1976).

[153] The *Financial Times* editorialized (October 15, 1976) that "his restatement of the quantity theory of money… is not, in fact, as the Nobel citation points out, his most fruitful work." This was not an accurate characterization: though the press release (which was distinct from the official citation) contained words to this effect, the citation did not. However, the fact that the restatement was not referred to clearly in the citation might carry such an implication. Friedman's (1956) article had been described by Johnson (1962, p. 350) as "probably the most important development in monetary theory since Keynes," but other accounts, such as that of Modigliani and Ando (1965, p. 708), clearly considered it a much less important contribution than this. See Nelson (2020a, Chapters 1, 4, and 6) for a discussion of Friedman (1956). That discussion emphasizes that Friedman's emphasis on imperfect substitutability of assets was more important than other aspects of the analysis that have been highlighted over the years (such as the inclusion of expected inflation as a variable in the money demand function), and that key elements of Friedman's monetarist theoretical framework were primarily expounded in places other than Friedman (1956).

[154] In *The Money Programme*, BBC2, December 10, 1976 (p. 4 of transcript), Friedman conjectured that the citation's reference to "monetary theory" was also in reference to material in the *Monetary History*.

recorded very soon after the award was announced, stated that Friedman's "work on monetarism of course immediately comes to mind" as itself "strong enough for Milton Friedman to have been awarded the Nobel prize" (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 208, October 1976). In his *Newsweek* column shortly afterward, Samuelson added: "Of course it is monetarism that marks Friedman's lifework of the last twenty years. His monumental *A Monetary History of the United States, 1867–1960*, written with Anna Schwartz, clinched his international reputation." (*Newsweek*, October 25, 1976*a*, p. 131 of 1983 reprint).[155]

The Nobel helped underline the fact, noted at the start of this chapter, that by the mid-1970s Friedman had established a definite and sizable school that had challenged the Keynesian revolution. This represented a change from a quarter-century earlier. At that earlier point, there had been a revival of interest in monetary policy but not the larger-scale conceptual and empirical challenge to Keynesian analysis that Friedman would offer. In May 1952, Alvin Hansen was able to point out that the main opposition offered to Keynesianism in the economics profession was "a small—but I would say rather anemic—counterrevolution which we might summarize under the head of the 'Pigou effect.'"[156] Friedman, in contrast, offered a framework that did not rest on the long-run conditions associated with the Pigou effect but instead pointed toward monetary factors as more important than fiscal policy (and other drivers of autonomous spending) in understanding business-cycle fluctuations, while also restoring important aspects of pre-Keynesian accounts regarding both the dynamics and long-run behavior of interest rates and inflation.

By mid-1969, Harvard University's James Duesenberry was juxtaposing the conception of economic relationships that was commonplace as of 1964 against that prevailing "[t]oday, [when] a large number of economists are prepared to agree that monetary policy plays the dominant role in determining the movements of aggregate demand."[157] And in 1972, notwithstanding recent setbacks in having his policies accepted at the national level, Friedman himself was sufficiently satisfied with the direction in which practitioners' thinking had gone to remark that his own views concerning monetary policy had, a decade earlier, been regarded as "crackpot" among central bankers and commercial bankers—but were not anymore.[158]

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[155] In the same vein, Arnold Harberger remarked in the wake of the news of Friedman's award, "We [in the economics department] knew it was only a matter of time—his works on monetary policy have been so influential…" (*Chicago Tribune*, October 15, 1976, p. 18.)
[156] Hansen (1953, p. 52).
[157] Duesenberry (1969, p. 83).
[158] Friedman (1972c, p. 39).

The intervening decade between 1962 and 1972 had, of course, seen the release of *A Monetary History*, and its role in creating a shift in views was essentially recognized in the Nobel citation. It must be noted, however, that a significant blight associated with this recognition was the concerted non-acknowledgment of Anna Schwartz. In August, Friedman had reproached a sympathetic writer for having "given me more credit than I deserve… by not mentioning my coauthor of the *Monetary History*, Anna Schwartz" (*The Times* (London), August 23, 1976). The documents released the following October in association with Friedman's Nobel suffered from the same fault. The citation did not mention her role but, in mitigation, it made no claim that all the research described was solo-authored. What cannot be excused, however, was the accompanying press release, which referred to the *Monetary History* as follows: "His major work… one of Friedman's most profound and most distinguished achievements… his original and energetically pursued study."[159]

Schwartz was delighted and excited at Friedman's Nobel award, and she herself thought that it was appropriate that he should indeed be the sole recipient. But the Nobel officialdom would have incurred no cost in explicitly acknowledging, in the press release, the fact that the *Monetary History* was coauthored. Instead, the press release repeatedly gave the opposite—and most untruthful—impression.

The complexity of stabilization policy

In his elaboration of his interpretation of what work underlay his Nobel citation, Friedman mentioned—alongside *Theory of the Consumption Function* and the *Monetary History*—what the report on his remarks labeled "a series of essays on monetary theory" (*The Detroit News*, October 15, 1976). Friedman had long taken monetary theory to encompass business-cycle-oriented macroeconomics—not just analyses of money or monetary policy specifically. Under that broad heading, two of these essays on monetary theory were specifically related to the Nobel citation's reference to Friedman's "demonstration of the complexity of stabilization policy." One of the essays, to be considered at the end of this section, was 1968's "The Role of Monetary Policy"—an article that covered narrowly monetary matters like interest-rate pegging and the Fisher effect but also considered the repercussions of the specification of aggregate supply (most particularly, a long-run-vertical Phillips curve) for what monetary policy could, and perhaps should, do.[160] Another was his much earlier essay, "The Effects of a Full-Employment Policy on

[159] Royal Swedish Academy of Sciences (1976). See Nelson (2004a, p. 395) for an earlier discussion.
[160] Friedman (1968a).

Economic Stability: A Formal Analysis." This article, published in French in 1951 and in English in 1953, concerned the implications of a countercyclical ("full-employment") policy.[161]

Each of these articles, their relationship with Friedman's other work, and their impact on economic thinking in the leadup to the Nobel award, is considered in what follows, beginning with the 1953 article.[162]

The 1953 "formal analysis": Quadratic objective function and destabilizing stabilization

The "formal analysis" of full-employment policy in the 1953 paper dealt with the problem facing a policymaker who wished to minimize the variance of a national-income aggregate. As that aggregate consisted of a policy-sensitive and policy-insensitive component, an optimal decision rule solving the policymaker's problem would involve making the policy-sensitive component of income a linear function of the exogenous component.

A catch, however, revealed by Friedman's analysis was the following: Although the variance-minimizing reaction did imply making this response negative, *not* all negatively-signed responses would actually reduce the variance of income below that obtained under a wholly nonactivist policy (that is, under a zero response to exogenous income variations). This was so because the variance of income necessarily depends positively not only the variances of the two components of income but also on a covariance term. A nonactive policy would have the automatic implication of making this covariance term and one of the variance terms zero. An active policy offered the possibility of reducing aggregate income's variance but also created the danger of raising it—the latter scenario occurring if the policy response (even when negative) was far from the numerically most appropriate coefficient (the one associated with optimal policy). And, because of uncertainty about the effects of policy, the policymaker might choose an inappropriate, destabilizing coefficient when embarking on an activist policy. This was Friedman's key destabilization result.

Beyond that result, two other aspects of Friedman's 1953 analysis are of special note here. First, the question asked by Duarte (2009, p. 43), "why did… Milton Friedman avoid formalizing [his] discussion about optimal monetary policy in terms of a quadratic loss function," does not amount to a valid way of framing the matter, as Friedman's paper of 1953 provides a counterexample to

[161] Friedman (1953d).

[162] For related analysis of both articles, see Nelson (2020a, esp. Chapters 4 and 7−8; 2020b, Chapters 13 and14).

the basis of Duarte's question.[163] By using the second moment of national income as the criterion function, Friedman in effect postulated that policymakers had quadratic objectives (that is, that the disutility associated with actual or expected deviations from policy goals should be expressed in terms of squares of the deviations).[164] Indeed, he may have been the first in the macroeconomic literature to have carried out policy analysis in this way.[165]

Second, Hirsch and de Marchi (1990, p. 159) were not correct to see the paper as solely a critique of fiscal policy. By casting Friedman's result as that "fiscal intervention can be destabilizing," they neglected the fact that the 1953 paper dated to an era in which he was himself an advocate of (rule-constrained) fiscal policy intervention, via his 1948 proposal to have budget imbalances when the economy was away from full employment. Friedman's 1953 analysis provided a caveat regarding the appropriate degree of activism to be assigned to aggregate demand policy, whether in the form of fiscal policy or monetary policy, and at the time of its publication he saw it as a caution about taking a more activist posture than that implied by his 1948 rule.

The 1947 precedent for the formal analysis' result on second moments

Friedman's 1947 *Journal of Political Economy* article, which reviewed Lerner's (1944) *The Economics of Control*, provided an explicit, albeit exclusively verbal, statement of the destabilization result that he laid out in detail and derived formally in 1953.[166] After the text of

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[163] See also the noting of Friedman's (1953d) use of quadratic objectives in Okun (1971d, p. 134) and Budd (1979, p. 205).

[164] Duarte's (2009) overlooking Friedman's (1953d) use of a quadratic objective function occurred despite the fact that Duarte (2009) did cite the article, albeit in a separate and fleeting context.

[165] See Nelson (2008, 2020a, Chapter 8) for further discussion of Friedman's views regarding policymaker objectives. On the relationship between the arguments appearing in quadratic objective functions and the unconditional variances of economic series, including in an intertemporal context, see Svensson (2002, p. 278; 2003b, p. 431).

[166] See Friedman (1947, pp. 414–415, fn. 12).

Forder and Monnery (2019) instead cite Friedman (1944) as the antecedent of Friedman (1953d). Although, as discussed in Nelson (2020a, Chapter 4), the 1944 Friedman article (a book review, concerned with Altman 1941) was important in disclosing in print Friedman's status as a critic of Keynesian multiplier analysis, it did not significantly prefigure his destabilization result. In this connection, Friedman (1947), cited only fleetingly by Forder and Monnery (2019) and only as part of a list of papers, is vastly more relevant, because of the footnote cited in the previous paragraph.

Forder and Monnery (2019) additionally give the strong impression that no study of Friedman prior to their own has considered Friedman (1944). This is most certainly not the case. That it is factually incorrect to suppose that Friedman (1944) was unexamined in the literature prior to 2019 is established by their own reference list, which includes the manuscript version of Nelson (2020a). Chapter 4 of that reference, available online starting in September 2015, opened by discussing Friedman (1944) and its relevance. Nor is Forder and Monnery (2019) the first journal article to reference Friedman (1944). Other than Thygesen (1977), the first such article was evidently

this article had noted the possibility that activist aggregate demand policy could magnify economic fluctuations, a footnote had elaborated on the result, noting the variance as the criterion for measuring fluctuations, and concluded that "a considerable ability to guess correctly would be required to convert government into a stabilizing influence," that is, to lower the second moment of the economic-activity series of interest.[167] In the 1953 reprint, Friedman added to this footnote an explicit reference to the "formal analysis" appearing elsewhere in *Essays in Positive Economics*.[168] In addition, a piece of text of the 1947 paper was repeated as text in the 1953 article—a fact that underscores the status of Friedman's 1947 *JPE* article as a prototype for his 1953 exposition.[169]

The 1947 paper became well known in its own right, the more as after it reappeared in 1953's *Essays in Positive Economics*.[170] It was, however, on the basis of the original appearance of the article in 1947, as well as discussions with Friedman, that the result to be stated again in the

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Nelson (2009b, p. 478), when Friedman's review was referred to in connection with its status as "Friedman's earliest dissent" from Keynesianism.

[167] Kasper (2003), although on the right track in associating the Friedman (1947, 1953d) papers with one another, failed to note the specific, crucial link between the two papers: the fact that they both stated the variance-reduction condition. In addition, contrary to Kasper's conclusion, neither paper made the case for *laissez-faire*, nor did the 1953 article constitute an attack on the "IS-LM model" (a term apparently used by Kasper, 2003, p. 90, as a shorthand for Keynesian approaches to viewing expenditure determination). Instead, the 1953 analysis called attention to the differences in stabilization outcomes that could arise when the policymaker misjudged the effects on aggregate demand of their own actions.

[168] See Friedman (1953e, p. 316).

[169] See Friedman (1947, p. 415; reprinted in Friedman, 1953e, p. 316) and Friedman (1953d, p. 132). Both passages referred to an "exhortation to do the right thing[,] with no advice how to know what is the right thing to do."

[170] Mayer (1998) claimed that the paper was not well known—pointing to Social Science Citation Index (SSCI) data in support of this contention. However, SSCI data on citations of Friedman's (1947) review of Lerner (1944) systematically neglected cases in which its reprinted version in Friedman (1953e) was cited—including numerous instances in which the reference to the article was accompanied *solely* by citation of the book title and not by the specific title of the article. This procedure was started off when the review of Lerner was explicitly referred to in numerous reviews of *Essays in Positive Economics* (such as Hahn, 1954) without being formally cited as a distinct bibliographical reference, still less in its original *JPE* form. Furthermore, even outside the context of reviews of that book, in the 1948−1993 period explicit mentions in the economic literature of Friedman's critique of Lerner were far more numerous than Mayer reported. Mayer (1998, p. 261) suggested that only six citations of the Friedman article appeared in the *whole* social-science literature. But, over this period, specific citations of the article in the *economic* literature alone over this period far exceeded six and included Brownlee (1950, p. 424), Galenson and Leibestein (1955, p. 344), Klappholz (1957, p. 344), Mishan (1960, p. 214), Samuelson (1964, p. 175), Olsen (1969, p. 39), Sen (1973a, pp. 1022, 1023; 1973b, pp. 83, 85), James and Nobes (1983, p. 130), Scitovsky (1984, p. 1553), Hammond (1990, p. 202), and Hirsch and de Marchi (1990, pp. 192−193). To this must be added those citations of all or part of pages 301 to 319 (the portion of the book in which the article was reprinted) of *Essays in Positive Economics* that did not give the associated article title: among the examples of such citations during the 1948−1993 period were Bent Hansen (1963, p. 76), Shull, Delbecq, and Cummings (1970, p. 26), and Breit and Ransom (1971, pp. 233−235). Furthermore, JSTOR data confirm that there were also multiple citations in the political-science or public administration journals over the same 1948−1993 period. The Friedman critique of Lerner was also mentioned—without an explicit bibliographical citation—in Brunner and Meltzer's (1993, p. 18) account of the development of monetarism.

Journal of Political Economy by Brownlee (1950, p. 424), who specifically cited the footnote containing the result.

Indeed, the 1947 paper provided, in an important respect, an overall more careful statement of the destabilization result than the 1953 "formal analysis" did. McCallum (1998, pp. 304, 308) noted that the income variable for which stabilization is being sought in the analysis is best regarded as real income in relation to its potential (or, in later terminology, natural) level.[171] Indeed, the fact that the reference level of output was the full-employment or potential level underlay the title of Friedman's 1953 paper.[172] The analysis in the same paper had verbally referred to the variance of income but had simply written the concept down as squared income without an expectations operator. Once, however, it is recognized that this income term was in fact an output-gap term, it becomes clear that not including an expectations operator amounts to assuming that potential output (as well as, in Friedman's 1953 analysis, the exogenous component of actual output) is fully observable.

Friedman in 1947 had in fact stressed the unobservability of full-employment output.[173] As Svensson (2003b, p. 431) observed, having an expectations operator in front of even the period-t term in an intertemporal loss function allowed for the case (inevitable in practice) in which potential output is not a directly observed series and so has to be estimated. The same dictate applies to Friedman's 1953 analysis: it considered the problem in terms of optimization for a single period, but the income-gap variable being stabilized was partially unobservable—so expectations in front of Friedman's objective function (and in front of the right-hand-side of the decision rule that he derived) would have been more appropriate. This was also a consideration more apparent in Friedman's 1947 exposition, which had referred to the exogenous and government-generated fluctuations in income alike as random series.

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[171] Specifically, McCallum suggested that the variable to be minimized was the logarithmic difference between output and its natural level, or, alternatively, in terms of appropriately defined nominal counterparts of actual and potential income. Argy (1992, p. 28) similarly took Friedman (1953d) as in effect stipulating that aggregate demand policy should minimize deviations of unemployment from its natural rate.

[172] Indeed, Fischer and Dornbusch (1983a, p. 137) simply defined "stabilization policy" as government measures designed to keep real output "close to its potential level." Of course, in practical discussions (see, for example, Romer and Romer, 2002, p. 35, and Ben Bernanke's remarks in Rolnick, 2004), inflation control is also encompassed by stabilization policy.

[173] It was on this basis that the paper has been cited in many modern discussions of the difficulties of estimating potential output. See the numerous post-2000 citations of Friedman (1947) catalogued in *https://ideas.repec.org/a/ucp/jpolec/v55y1947p405.html*. It is also a notable fact that the article was cited both by Professor John H. Williams, an eminent monetary economist in academic and policy circles during World War II and the initial postwar decades (see the citation of the *JPE* in Department of Economics, Harvard University, 1955) and by prolific researcher John C. Williams, who would become vice chair of the Federal Open Market Committee (see the citation of Friedman, 1947, in Orphanides and Williams, 2005, p. 1928).

The role of lags

The aspects of model uncertainty stressed in Friedman's 1953 formal analysis were complemented by his emphasis elsewhere on the length and uncertainty associated with the lags in the effect of policy actions. The 1953 model did not have explicit lags, but it would be a mistake to describe that model as necessarily a static one. For, as Friedman noted, the policy-sensitive component of income in that analysis might fully respond to policy actions only with delays. Friedman's late-1940s published work had in fact seen his first use of the term "long and variable lag"—a formulation that would eventually so influence both economic idiom and conventional wisdom that Benjamin Friedman (2004, p. 148) would remark that "a belief in 'long and variable lags' is nowadays part of the common ground of monetary economics…" and Federal Reserve Chair Jerome Powell (2019, p. 18) would observe that "we think monetary policy works with, as [Milton] Friedman said, long and variable lags. [174] The research represented in the 1953 "formal analysis" dovetailed with Friedman's stress on lags, and when in the late 1940s he impressed on his colleague Lloyd Mints the possibility of output destabilization arising from economic policy, he emphasized lags as part of the reason for this possibility (Mints, 1950, p. 138, fn. 8; Patinkin 1969, p. 52, fn. 24).

As Friedman and Schwartz's work proceeded in the 1940s, Friedman's concentration on lags was reinforced by their empirical investigations of money-to-income lags.[175] This empirical work also affected his policy prescriptions, which shifted toward advocacy of constant monetary growth.[176] When, in the early 1960s, Friedman reviewed the evidence on lags and its relationship with his policy views, he was also able to link them up with his "formal analysis" by citing the 1953 article "for the theoretical analysis underlying my conclusion."[177]

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[174] On Friedman's initial uses of the term "long and variable lag" in the late 1940s, see Nelson (2020a, Chapter 4). Friedman himself lost track of his first usage of the term. This would become evident when, in the *Wall Street Journal* (August 20, 1985), he incorrectly implied that this occurred only in 1959. On "long and variable lag" entering the idiom of monetary policy discussions, see Nelson and Schwartz (2008, p. 846).

[175] The 1953 paper, of course, provided no empirical evidence on lags in the effect of monetary policy. Indeed, as already indicated, it was not concerned with monetary policy specifically. Citing Friedman (1953d), as Rosenbaum (1985, p. 21) did, as an instance in which Friedman concentrated on monetary policy's lags (to the exclusion of those associated with fiscal policy) would therefore not be appropriate.

[176] In light of this, Meiselman (1969, p. 150) considered that "the long lags Friedman found in his research have been a crucial factor in his case against discretionary monetary policy."

[177] Friedman (1961c, p. 464, fn. 39). The Friedman (1961c) summing-up article, which consolidated various positions and findings on Friedman's part concerning lags while also containing an articulation of his multiple-yield view of monetary policy's effects, is something that bolsters Laidler (1984, p. 601) observation that "Friedman's views on the complexity of the transmission mechanism" were on the record by 1962. The 1961 paper's status as a restatement and updating of Friedman's position on stabilization policy is demonstrated by the fact that Fischer and Cooper (1973, p. 847) cited it as the sole source for Friedman's "assertion that the existence of long and variable lags in the effects of monetary policy is likely to make countercyclical monetary policy destabilizing." However,

The "formal analysis"—reception and absorption

An early appreciation of Friedman's "formal analysis" came from Kenneth Boulding. Boulding, who by no means had much agreement with Friedman's views on economic matters, indicated in his 1954 review (Boulding, 1954, p. 132) that the "most interesting part" of *Essays in Positive Economics* included "a most important article (previously published only in French) on the effects of a full employment policy on economic stability, in which it is argued that unless full employment policies are carefully timed they may actually increase rather than diminish the amplitude of the cycle, especially where lags in effect are important."

Later recognition of the importance of Friedman's "formal analysis" included its incorporation into the reading list, already mentioned, for a Harvard University course (Department of Economics, Harvard University, 1955). In addition, although Phillips' (1954) celebrated work on stabilization policy had few references and did not cite Friedman's paper, others soon saw Friedman's article as a precursor to Phillips—a fact brought out in Prest's (1956, p. 516) mention of "the articles (by Milton Friedman or A.W. Phillips) dealing with the possibilities of Government fiscal intervention being destabilizing rather than stabilizing."

Impressions that the "formal analysis" made during early career stages

Three notable figures who would be associated with debates, in research and the public square, on policy rules were each influenced by Friedman's 1953 formal analysis at early stages of their respective careers.

Future *Financial Times* columnist Samuel Brittan evidently read *Essays in Positive Economics* when he was a student of Friedman's at Cambridge University in 1953/1954. It may have been the "formal analysis" chapter in that book, possibly along with Friedman's analysis of consumption, that underlay Brittan's (2005, p. 294) recollection of Friedman as "somebody who could hold his own with the most advanced of whizz kids." Later, when he concentrated on macroeconomic affairs in his *Financial Times* columns and other writings, Brittan would indicate that Friedman's "Role of Monetary Policy" in 1968 was the most important of Friedman's articles in shaping his thinking on demand management. Nevertheless, Brittan also assigned credit to the 1953 "formal analysis" of Friedman's. In a discussion published a little

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the fact that these authors cited only Friedman (1961c) (in its reprinted form in Friedman, 1969c) may have played a role in leading Buiter (1981, p. 651) to make the incorrect statement that the finding that "[m]isdirected 'stabilization policy' can be destabilizing… was first formally demonstrated by Phillips [1954]."

over a year after the Friedman Nobel award, Brittan included in his opening chapter of a new book a verbal description of the 1953 article's variance result and formally cited the article (Brittan, 1977, pp. 6, 9).[178] Indeed, Brittan had evidently been partly inspired by the 1953 "formal analysis" when, in the early 1960s, he had articulated his early criticisms of U.K. economic policy. Even at that stage, Brittan was concerned about the possibility that policymakers would be unsuccessful in their demand-management activities—a concern related in particular to the "fear that, because of the difficulty of timing, they would actually destabilize rather than stabilize the economy" (Samuel Brittan, interview, April 18, 2013). Brittan's work in the early 1960s argued that U.K. aggregate demand policy of late had been destabilizing in practice, and he had made the case for more rule-like behavior on the part of the authorities (*The Banker* (London), September 1962; Brittan, 1996, p. 10).

Another economist whose early career was in the United Kingdom was Michael Parkin, who recounted of his own experience: "The very first paper of Friedman's I read was one reprinted in *Essays in Positive Economics* about the challenge of stabilizing real GDP. This paper—it was originally published in a French journal—was one where he writes down a model that has the variable you're trying to control, and broken down into the bit that you can manage, and the bit that you can't. It shows that under pretty simple and reasonable conditions, attempting to stabilize will end up in greater instability. That was a very *simple* model—but it is [also] a *formal* model—of aggregate fluctuations." (Michael Parkin, interview, May 29, 2013.)

In the United States, John Taylor read Friedman's 1953 article as an undergraduate student in economics at Princeton University. Taylor recalled that his wish to put monetary policy into the models he was using in his senior "just lent itself very much to policy rules, and that meant thinking about Friedman… So that's when I read most of his stuff for the first time… [especially on] operationally economic things like stabilization policy." (John Taylor, interview, July 2, 2013.) Taylor cited Friedman's "formal analysis" paper in his 1968 undergraduate senior thesis, which was concerned with fiscal and monetary stabilization policies (see Taylor, 1968).

Fame and reach of the "formal analysis," 1961–1997

By 1961, White (1961, p. 142) could refer to "Professor Milton Friedman's well-known *a priori* arguments" concerning destabilizing effects of policy responses, and an article by William Baumol on the "pitfalls" of countercyclical policy could point to both Friedman's 1953 paper

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[178] Brittan's book appeared around December 1977 (see *Financial Times* (London), December 8, 1977).

and Phillips (1954) as “warnings on this problem [that] have already occurred in the literature” (Baumol, 1961, p. 21).

Later in the 1960s, Brainard (1967) appeared, thereafter becoming a standard reference on policymaking under model uncertainty. Brainard did not cite Friedman’s paper. But the parallelism between the two analyses was stressed when, in commenting on an expanded version (Brainard, 1969) of the 1967 article, Dewald (1969, p. 326) noted: “This conclusion is akin to Milton Friedman’s classic argument that the effects of policy action on economic objectives must be sufficiently negatively correlated with the effects of noncontrolled factors to justify the use of discretionary policy actions. Brainard’s analysis might be interpreted as a generalization of this argument…” In the same vein, Okun (1971, p. 66) referred to “the decision analysis set forth by Milton Friedman and William Brainard” and cited their 1953 and 1967 papers, respectively.

Further into the 1970s, the first article ever to appear in the *Journal of Public Economics* was in effect a twentieth-anniversary review and development of Friedman’s 1953 analysis: see L. Johansen (1972).[179] Subsequently, Blinder and Solow (1974, p. 70) referenced Friedman’s analysis while also noting other work that “formulates the stabilization problem as Friedman and Johansen do.”[180] And in the unlikely location of his 1975 study of issues concerning income distribution—*Equality and Efficiency: The Big Tradeoff*—Arthur Okun indicated his presumption that macroeconomists were familiar with Friedman’s 1953 analysis and the key result therein.[181]

Friedman’s “formal analysis” article was, furthermore, cited in several textbook presentations. Gordon (1978, p. 360) referred to Brainard (1967) and then pointed to Friedman’s 1953 work as an “earlier analysis” in this area. The U.K. textbook discussion of Demery (1984, p. 31) cited Friedman’s 1953 paper as “an early formal analysis of the conditions under which stabilization policy destabilizes the economy.” In U.S. undergraduate textbooks, other citations of Friedman’s 1953 analysis included Thorn (1976, p. 440), who had noted that “monetary policy can be destabilizing as well as stabilizing” (p. 388), and Sheffrin, Wilton, and D.M. Prescott

[179] L. Johansen (1965, p. 58) had previously cited the 1953 article in his monograph on economic policy.
[180] Also in 1974, a book specifically on the lags in the effect of monetary policy cited the “formal analysis” and expounded its key analytical result (see Uselton, 1974, pp. 16–19), before adding (p. 19) that the “theoretical frame of reference established” in the 1953 paper provided context for Friedman’s empirical work on lags (work that Uselton proceeded to survey).
[181] See Okun (1975b, p. 84). Another prominent Keynesian, Robert Eisner, included the Friedman article in his graduate course in macroeconomics at Northwestern University—doing so, for example, in the late 1970s (Paul Pieper, interview, April 12, 2023).

(1988, p. 174). Other undergraduate texts in effect referred to Friedman's work without direct citation. For example, Baumol and Blinder (1979, p. 259) plotted hypothetical movements of actual output around potential output and remarked that under "long lags, attempts at stabilizing the economy can actually succeed in destabilizing it," while immediately adding that this was a key reason why "some economists, most notably Milton Friedman" had argued against activist policies. Along similar lines, Poindexter (1981, p. 500) named Friedman as an exponent of the view that "poorly timed pursuit of a stabilization policy can be destabilizing!"

References to Friedman's 1953 work on destabilizing stabilization policy also appeared in the retrospectives on his research in Breit and Ransom (1971, p. 235) and Cagan and Schwartz (1975, pp. 262, 289). Butler's (1985) book on Friedman also stressed this particular contribution: Butler observed that "policy can be destabilizing, even if it is of the right strength and in the right direction" (p. 160) and cited the 1953 Friedman paper that produced this result (see pp. 17, 158 247, 248, 258).[182] The 1953 "formal analysis" also figured in the accounts of the evolution of the debate on policy rules by Bordo and Schwartz (1983), who opened and closed their first section with what they called "Friedman's (1953[d]) procedure for evaluating stabilization policy" (p. 68), and Brunner and Meltzer (1993, pp. 182–183), whose exposition also took readers through the second-moment result in Friedman's analysis.

In the early 1990s, Edmund Phelps cited Friedman's 1953 analysis prominently in two books. In a book of guest lectures, Phelps (1990, pp. 30–31) re-presented Friedman's key statistical result and went on to consider an extension of it made in a Ph.D. dissertation, Brito (1970). In a three-volume collection published the following year, *Recent Developments in Macroeconomics* (Phelps, 1991a), Phelps made an exception to his rule of limiting the items reprinted to contributions that had appeared since the late 1960s by reprinting, in the first volume, Friedman's 1953 paper, which appeared in the collection just ahead of the follow-up analysis (previously unpublished) of Brito (1970). In the same book's introduction, Phelps (1991b, p. xiv) observed that "[t]he classic 1953 analysis of Friedman was the first in an impressive line of formal arguments cautioning against [activist] stabilization policy" and went on to note (p. xv) Brainard (1967) as a continuation of this line.

A little further along in the decade, Argy (1992, p. 28) cited and described the 1953 Friedman analysis and immediately linked the 1953 paper's principal finding to the "important

[182] Forder and Monnery (2019, p. 137) nevertheless state, with regard to Friedman (1953d), "Butler (1985) … does not mention it." This statement is simply not correct, as the multiple citations of Friedman (1953d) in Butler (1985) establish.

contribution" by Brainard 1967 on uncertainty about the effects of economic policy. In a subsequent textbook analysis, Argy (1994, pp. 418–419) cited the 1953 article and laid out the derivation of the destabilization result. An economists' report to the European Union published in 1993 (Commission of the European Communities, 1993) referred (p. 44) referred to "the well-known Friedman (1953[d]) critique" while also citing a study supporting the report (Goodhart and Smith, 1993) that had cited the 1953 Friedman analysis extensively.[183]

As of 1997**,** a new undergraduate macroeconomics textbook described the consensus, in a discussion that did not cite Friedman's article but clearly reflected its influence. Blanchard (1997, p. 553) observed: "Today, most economists recognize that there is substantial uncertainty about the effects of policy. They also accept the implication that this uncertainty should lead to less active policies."

Limitations of the "formal analysis" as a critique of stabilization policy

Tellingly, however, some later editions of Blanchard's textbook (for example, 2017, p. 459) added a qualification to the final sentence: "except in special circumstances, such as 2008–2009." This qualification underlined the fact that Friedman's result lacked universal applicability. Although Brunner and Meltzer (1993, p. 183) stated that "Friedman's analysis is completely general," it does not follow that that analysis' key result bears heavily and adversely on any treatment of stabilization policy.

The sense in which Friedman's analysis was general was, as McCallum (1998) stressed, via its focus on unconditional moments. But that focus itself rendered the analysis *non*-general in another sense. A concentration on unconditional moments involves moving away from the information available in any period t about variables' behavior up to that time. Such information may, in fact, make the policymaker's knowledge about the state of the economy in period t greater, and perhaps much greater, than the values of the unconditional moments alone suggest.

[183] As these citations indicate, Forder and Monnery's (2019) implication that Friedman (1953d) is not a widely cited paper is not correct. Three specific problems regarding their analysis should be mentioned: (1) In considering Google Scholar, they consider specific citations of Friedman (1953d) but not the thousands of references to *Essays in Positive Economics* (Friedman, 1953e). If only a small percentage of citations of *Essays in Positive Economics* were in reference to the "formal analysis" article in that book, hundreds of references to Friedman (1953d) would be added the article's aggregate citations. (2) They imply that, when citing Friedman (1953d), Victor Argy did not refer to its analysis of second moments (p. 138). As indicated above, however, this implication is shown to be unsupported once the Argy (1992, 1994) books are considered. (3) They point (pp. 137, 138) to particular cases in which David Laidler and Thomas Sargent did not cite Friedman (1953d). But both these authors did cite Friedman (1953d) prominently over the course of their writings: see, for example, Laidler (2012, p. 16) and Hansen and Sargent (2008, pp. 6, 416). The second of these references is the major text on uncertainty in economic models.

In particular, the existence of serial correlation in economic dynamics may imply that the policymaker has valid grounds to believe that in any given period they do possess the information they need to take policy actions that move output closer to potential. A policymaker facing an initial condition of a large negative or positive output gap, and aware of forces tending to make that gap persistent, should be well positioned to make an activist response whose effect is stabilizing. This point, stressed by Orr (1960) in a formal generalization of Friedman's analysis, underlay Bailey (1962, p. 174) observation that though Friedman's analysis established with regard to formulating appropriate policy responses that the "problem is a serious one, it is not necessarily intractable," gave continued support to activist policies even in situations in which it was clearly the case that (in Bailey's words) "it is unlikely that stabilization policy will ever be perfectly successful."

Therefore, although Friedman suggested that the problem of stabilization policy that he found in 1953 would be magnified if explicit lags were introduced, in some respects dynamics actually make Friedman's 1953 critique far less devastating.[184] It is in light of this that Samuelson (1967, p. 10), though not citing Friedman, conceded that time-lags required introduced an element of fragility into policymaking—because they mandated that activist policies rely on forecasts—but insisted that, as of January 1961, it was the case that U.S. policy officials were confident "there would continue to be an unemployment gap for some considerable time" and so could be assured that aggregate demand stimulation would bring the gap closer to zero.[185]

Consequently, Friedman's 1953 analysis, although widely noted and indeed accepted, only mildly modified professional thinking about stabilization policy.[186] A more drastic modification would, however, arise from a later contribution, also encompassed in the Nobel Committee's reference to the complexity of stabilization policy: his work on the Phillips curve.

Friedman's contribution on the development of the Phillips curve

Franco Modigliani's presidential address to the American Economic Association in 1976 set out to "examine the issue of the supposed destabilizing effect of pursuing stabilization policies," and

[184] This may help explain why Hahn (1954, p. 400) was highly unimpressed by Friedman's result, including its associated "simple, yet by no means unambiguous statistical formula."
[185] Of course, potential-output mismeasurement could mislead policymakers into perceiving a persistent output gap when one might not be present. But, as the citations in Orphanides' (2002, 2003) work implied, this is a point more firmly associated with Friedman (1947, 1968a) rather than Friedman (1953d).
[186] Even so, when a major conference on rethinking stabilization policy was held in August 2002—nearly fifty years after the English-language version of Friedman's article appeared—that article was still remembered, being cited in the proceedings volume by Feldstein (2002, p. 153).

in the following section the key paper he cited as claiming such an effect was Friedman's own presidential address of nine years earlier.[187]

Unusually, due to the AEA meetings being held earlier than the then-customary date of late December, Modigliani's address was given in September—*before* the Nobel announcement in October. Therefore, with Modigliani having provided a prominent instance of "The Role of Monetary Policy" being described as a Friedman contribution on destabilizing stabilization policy, it was convenient for the Nobel Committee to put that paper under the umbrella of research contributions considering "the complexity of stabilization policy," along with Friedman's 1953 paper (which Modigliani's address had not cited).

Friedman himself recognized that the Nobel citation spanned "The Role of Monetary Policy." Explaining his Nobel lecture topic of "Inflation and Unemployment," he remarked: "The tradition is that the lecture is supposed to refer to some of the work for which the award was made. So I tried to do that." (Instructional Dynamics Economics Cassette Tape 205, December 1976.) He also specifically indicated that the part of the citation that recognized "The Role of Monetary Policy" was the citation's reference to "the complexity of stabilization policy."[188]

Friedman's "The Role of Monetary Policy" on interest rates and unemployment

Friedman's "The Role of Monetary Policy" had, of course, led into its case for the absence of a long-run tradeoff between inflation and unemployment by expounding how a central bank could not lastingly peg (that is, keep at a fixed, administered value) a (short-term) nominal market interest rate. The appeal of this material as a lead-in to his natural-rate result lay not only in the parallel between the concepts of the natural interest rate and the natural rate of unemployment but also on the consensus already in existence among economists that an interest-rate peg was unsustainable. In the era after the Accord, rejection of the feasibility of interest-rate pegging was widespread among economists, so Friedman's articulation of this rejection was a "relatively uncontroversial" part of his address, as McCallum (1986b, p. 155) noted. Pegging of interest rates was also something policymakers in the post-Accord period recognized as being prone to generate economic instability.[189]

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[187] Modigliani (1977, p. 1). This was an address given on September 17, 1976.
[188] *The Money Programme*, BBC2, December 10, 1976, pp. 4–5 of transcript. By this time, Harry Johnson had also observed in the London *Economist* (October 23, 1976*b*) that "'his demonstration of the complexity of stabilization policy'… runs through his publications… It received its most explicit and widely publicized form in his 1967 presidential address to the American Economic Association…"
[189] For example, in early 1955, Federal Reserve Chairman William Martin remarked that the interest rate consistent with price stability changed over time and that if policymakers attempted to enforce an "artificial interest rate… you

Friedman's case against interest-rate pegging in his 1968 paper has, however, come under latter-day dispute: see, especially, Cochrane (2017, 2023). As discussed in Nelson (2020c), these objections do not actually provide a challenge to the conclusions Friedman made on the basis of the thought experiment in which he engaged.

The later literature actually considers a *different* experiment, one in which the monetary authority, provided it is able to affect instantaneously the expected-inflation term in the interest rate of interest, might be able to maintain a peg indefinitely. Friedman was, in contrast, concerned with whether a central bank can permanently fix the nominal interest rate via actions that alter the *real-rate* component of the nominal interest rate.[190] Most immediately, he was considering whether a one-time increase in the money stock could permanently lower the real interest rate and, by that means, bring the nominal interest rate to a desired pegged level. The analysis involved expansionary monetary policy that initially raised the stock of real money balances and, via that mechanism, lowered nominal and real interest rates. Friedman indicated that this liquidity effect wore off, because the nominal monetary expansion would eventually give rise to price-level responses that tended to cancel the initial increase in real money balances and so restore the original level of real interest rates (as well as of other real variables). This was not, as indicated above, really a controversial point to make in 1968, because it was already widely accepted that one-time expansions of the nominal money stock eventually were ultimately felt only in the price level and not in real variables.[191]

However, the example to which the rate-pegging example was a preamble—the Phillips curve—was one for which there was indeed a preexisting consensus that monetary policy actions or other policy measures affecting the growth of nominal aggregate demand could permanently affect real economic activity.

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will have an unrealistic supply of money. Now[,] you can do that for a certain length of time, but then the payoff comes." (*U.S. News and World Report*, February 11, 1955, p. 59.)

[190] As Taylor (1972, p. 518) put it, it is "the conclusion that the monetary authority can peg *both* the nominal and the real rate of interest" that is "contrary to the views of Professor Friedman." (Emphasis added.) Friedman (1968a) largely took it for granted that readers understood that he was stressing the transitory character of the liquidity effect—and so was concerned with measures that moved nominal rates and real rates in the same direction. Friedman made this point more explicit in other accounts emphasized that his 1968 analysis was a critique of the feasibility of targeting the real interest rate. For example, in Jacobs and Pratt (1968, p. 47) he observed that "[n]o permanent effect on the real rate remains" after economic reactions wipe out the liquidity effect on the nominal interest rate, and in Friedman (1977d, p. 13) he summarized Friedman (1968a) as having provided a demonstration that "it is not possible to use monetary instruments to achieve a real target, whether the real target be the real interest rate or real output or [the] unemployment rate."

[191] For example, James Duesenberry, although opposed to a constant-monetary-growth rule, acknowledged that that rule possessed automatic-stabilization properties and an interest-rate peg did not. See Meiselman (1969, p. 149).

Reverberations of Friedman's Phillips-curve research

It is against this background that Friedman offered the proposition that real economic activity could not be boosted permanently by monetary means, even when the monetary measures implied a permanently higher inflation rate. Friedman had outlined the natural rate hypothesis previously. But his extended outline of the hypothesis in his article, published in March 1968, together with his articulation of lessons for monetary policy flowing from a long-run-vertical Phillips curve, drew more professional and media recognition of the scale of the challenge to existing thinking that he was posing.

"Monetary policy, he said, cannot peg interest rates or the unemployment rate for more than very limited periods," the *Chicago Daily News*' Washington correspondent observed with regard to the newly delivered address, in a report published on December 29, 1967. With a similar focus on this bottom line of the article, Harry Johnson remarked in a lecture given about six weeks after Friedman delivered his talk (and before it had appeared in print): "As Milton Friedman put it succinctly in his (1967) Presidential Address to the American Economic Association, what the central bank can control ultimately are monetary magnitudes such as the quantity of money and the price level, and not real magnitudes such as unemployment and interest rates."[192] And when the talk was published in March 1968, Friedman's verdict regarding Phillips-curve dynamics and long-run tradeoffs impressed itself on Robert Lucas. On the basis of absorbing Friedman's article, Lucas judged that "his reasoning appeared conclusive" (Lucas, 1981a, p. 5). The natural rate hypothesis heavily shaped Lucas' subsequent research.

Two of the figures noted earlier as influenced early in their careers by Friedman's 1953 analysis of stabilization policy were also quick to be affected by his 1968 article in the same area. John Taylor cited the 1968 Friedman article in his first published article, in which Taylor defended its natural-rate position (Taylor, 1972). Samuel Brittan would recount how, during a lunch he had with Charles Goodhart in London in 1968, Goodhart "drew my attention to Milton Friedman's 1967 presidential address to the American Economic Association. It was this address which persuaded me that there was no long-term tradeoff between inflation and unemployment…" (*Financial Times* (London), January 19, 1984.) Brittan would further recall that "until then, I had been a sort of Keynesian... And that [the Friedman article] actually persuaded me that there were real factors determining the unemployment rate and that if a government or central bank tried to push too hard, they would not get just inflation—which didn't bother me so much—but

[192] Johnson (1970, p. 16, fn. 6).

accelerating inflation—which did. And that, in one sentence, is what changed my attitude." (Samuel Brittan, interview, April 18, 2013.) Writing in 1975, Brittan had underlined the great extent to which this article had influenced his position about practical economic policy: "It was Professor Milton Friedman who removed the scales from my eyes… by his analysis of the effects of demand management on unemployment. He did so in one single paper"—that is, his "Role of Monetary Policy" article (Brittan, 1975, p. 12).[193]

Stagflation and acceptance of the natural rate hypothesis

By the time Brittan made these remarks in the mid-1970s, empirical evidence had mounted in favor of the natural rate hypothesis. In the case of the United States, the aggregate data, most notably the inflation/unemployment combinations registered in successive years, had by 1972 already shifted estimated price-level adjustment equations toward parameter settings that were consistent with the hypothesis. The spreading acceptance of the hypothesis among academic economists—a development discussed in Chapter 3 above and further in the next section of this chapter—was further consecrated, and its global dimension particularly underscored, by Friedman's 1976 Nobel award.

Rejection of an inflation/unemployment tradeoff was also something sounded repeatedly in U.S. policy circles in the mid-1970s, after the experience of multiple bouts of stagflation. Henry Wallich took the opportunity of a speech in Washington D.C. in May 1974, early in his term as a Federal Reserve Board governor, to observe with regard to the tradeoff that "the empirical evidence pointing to increasingly steeper Phillips curves as successive years of new inflationary data were added to the earlier vintages, together with the accelerationist challenge, raised increasing questions about its application."[194] The following year, Wallich indicated that he believed that the debate had progressed further. Wallich acknowledged that the "Phillips curve… may be the most significant piece of analysis for policy purposes" since the *General Theory*, but he added, in reference to the "long debate between the orthodox [that is, non-vertical] Phillips curve supporters on the one side and the accelerationists on the other," that he himself believed that one should "side with the accelerationists… on the grounds both of the econometric evidence and of naked-eye observation of the world in which we live."[195]

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[193] See also Brittan (1977, pp. xii, 237) and ft.com, October 12, 2020.
[194] Wallich (1974b, p. 2).
[195] Wallich (1975b, p. 3).

About a year further along, in mid-1976, House of Representative member Augustus Hawkins (D–CA), invited to speak before a Senate committee hearing, quoted a sentence from a speech Federal Reserve Chairman Burns had given in the previous September: "Whatever may have been true in the past, there is no longer a meaningful tradeoff between unemployment and inflation."[196]

Hawkins continued: "And even President Ford seems convinced." Hawkins proceeded to quote from the main text of the 1975 *Economic Report of the President*: "Despite considerable empirical work allowing for the role of further variables and of lags, it has proved difficult to defend the claim of a long-run Phillips tradeoff between inflation and unemployment." The subsequent sentences of this passage, not quoted by Hawkins, had been more specifically supportive of the Friedman-Phelps alternative: "It should also be noted that a series of shifting, negatively sloped short-run curves relating inflation and unemployment is theoretically consistent with the concept of a 'natural' rate of unemployment which is independent of the rate of inflation in the long run… Thus, no stimulus toward lowering unemployment can be derived from a higher inflation rate once the public has adjusted to it."[197]

Hawkins could actually have quoted from a more recent pronouncement by President Ford on the matter. In his signed portion of the January 1976 *Economic Report of the President*, Ford had stated: "The events of the past several years have once again convincingly demonstrated that accelerating inflation causes instability and disruptions, increases unemployment, and ultimately precludes real prosperity."[198]

Still other governmental statements along these lines, in this case emanating from U.K. and Canadian officialdom rather than from the United States, would be quoted by Friedman himself in his December 1976 Nobel lecture.[199]

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[196] From Hawkins' May 25, 1976, appearance in Committee on Banking, Housing and Urban Affairs, U.S. Senate, (1976a, p. 156). The quotation was from Burns (1975b, p. 12) and would be reprinted in Burns (1978, p. 221).
[197] Council of Economic Advisers (1975, p. 96). It continued: "The long-run vertical line … [is situated at] the 'natural' unemployment rate, a rate which, as noted earlier, depends on the level of frictional and structural unemployment and on other fundamental characteristics of the economy." As a graphical confirmation of the breakdown in the relationship, the previous page had plotted inflation rate according to the PCE deflator (a price series that CEA's chair, Alan Greenspan, would long favor) against the unemployment rate.
[198] From Ford's remarks in Council of Economic Advisers (1976, p. 4).
[199] See Friedman (1977c, p. 460). Friedman's favorite quotation in this connection, and one he used in his Nobel talk, came from a September 1976 speech by U.K. Prime Minister James Callaghan. (See Chapter 8 for further discussion of Friedman's references to this speech.) The speech, widely viewed as informed by natural-rate ideas, had actually already been quoted by President Ford, about a month after Callaghan delivered it. On October 28, 1976, Ford (1976a) remarked (see also *Financial Times* (London), October 29, 1976): "The continuing economic crisis in Great Britain… tells us all we need to know about the dangers of too much government, too much spending

The existence of such statements exaggerated the degree of policymaker acceptance of Friedman's natural-rate alternative to the traditional Phillips curve. This judgment applies even to the U.S. examples quoted above. In the executive branch of the U.S. government, key economic-policy officials like Alan Greenspan and William Simon had indeed largely been won over by Friedman's argument. But this was not true, as has already been stressed, elsewhere in the Ford Administration's hierarchy. And it was certainly not true of Arthur Burns at the Federal Reserve. Burns' position instead lined up more closely with Representative Hawkins' interpretation: that rejection of a Phillips-curve tradeoff was appropriate but should rest on the notion that inflation had become disconnected from demand-and-supply factors.[200] And even Burns' colleague Henry Wallich regressed on the matter, from his 1975-vintage acceptance, noted above, of the natural rate hypothesis, to a largely cost-push-based explanation of the behavior of U.S. inflation in the 1970s.[201]

These setbacks in the acceptance of the natural rate hypothesis contrasted with the smoother increase in its acceptance in academic circles—a situation brought out in Harry Johnson's (1976b, p. 320) remark that "research into the theory and empirical measurement of the so-called 'Phillips curve' tradeoff between inflation and unemployment has verified that there is no such tradeoff in the long run..." Johnson similarly wrote in the aftermath of Friedman's Nobel that "research… has amply confirmed his central proposition" regarding the Phillips curve (*The Economist* (London), October 23, 1976*b*).

The 1970s inflation and the inheritance of the 1960s policy atmosphere

The evidence that had accumulated from U.S. economic experience by the mid-1970s in favor of the natural rate hypothesis resulted from policies that generated high inflation. Was this evidence—so strongly pointing toward the absence of a long-run Phillips-curve tradeoff—ironically the result of prior policies that deliberately sought to exploit a perceived long-run

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on borrowed money… The courageous—and I emphasize courageous—British Prime Minister of that troubled nation has gone to the very heart of the problem. Listen to what he said just a few weeks ago [actually, exactly one month before] to his own Labour party, the party that played an important role in helping to create the crisis that they face. Prime Minister Jim Callaghan said, and I quote: 'We used to think that you could spend your way out of a recession and increase employment by cutting taxes and boosting government spending.' He went on to say: 'I will tell you in all candor, that option no longer exists and that insofar as it ever did exist, it worked by injecting inflation into the economy. And each time that has happened, the average level of unemployment has risen.'"

[200] This was, of course, a variant of the position discussed in Section I above in connection with Nicholas Kaldor's views. Burns' position on inflation is discussed in detail in Nelson (2020b, Chapter 15) and in Chapters 2, 3, and 8 of the present book.

[201] This was reflected in Wallich's intensified advocacy of incomes policy in the later 1970s. See the discussion of Wallich's views in this connection in the subsection titled "Arthur Okun" in Section III of the previous chapter.

tradeoff? Some commentaries made in the mid-1970s suggested so. For example, in the 1975 talk quoted earlier, Federal Reserve Board Governor Wallich stated: "The Phillips curve has been one of the principal analytical devices brought to bear on inflation… Almost certainly[,] it has been the most disastrous in its effects."[202] The following year, Friedman's former student, Warren Nutter, testified in a Congressional hearing (Committee on Finance U.S. Senate, 1976, p. 67) that, though he himself did not believe in a long-run Phillips-curve tradeoff, a conviction by U.S. officials of previous years that such a tradeoff existed may have helped generate "the experience of stagflation in recent years." That testimony (of February 17, 1976) suggested that the notion that "if society wants more output and less unemployment, it need only raise the rate of inflation" was an idea that had "played a powerful role in getting us into the fix we find ourselves in today."

Friedman, however, was more circumspect on this matter. In his *Meet the Press* appearance in the wake of his Nobel award, Friedman stated: "What we have observed, not only in the United States but in every Western [country], is that the attempt to use inflation to stimulate the economy may work for a short time—but sooner or later [it] backfires." He made clear, however, that, in referring to policymakers' using inflation, he did not mean policies that consciously sought to raise the inflation rate but, instead, "any attempt to stimulate the economy by increasing government spending or by promoting a more rapid growth of the money supply."[203] Friedman was therefore attributing inflation to excessive additions to aggregate demand—but he was not ascribing this ease to a deliberate policy of higher inflation.[204]

A cautious attitude toward attributing consciously inflationary motives to U.S. policymakers was valid.[205] This point is brought out by juxtaposing two retrospectives on the intellectual climate guiding U.S. policymaking in the 1960s. On the one hand, Wonnacott and Wonnacott (1979, p. 269) argued: "[U.S.] policy makers in the 1960s believed they faced a well-defined Phillips curve. This belief presented them with a policy dilemma." On the other hand, Tobin (1982, p. 305) offered the apparently contrasting judgment that "the Phillips curve was never 'a cornerstone of economic policy.'"

The difference between these two statements cannot validly be explained by conjecturing that

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[202] Wallich (1975b, p. 3).
[203] *Meet the Press*, NBC, October 24, 1976, p. 5 of transcript.
[204] By taking major easings of aggregate demand policy as connoting inflationary policies, Friedman was implicitly taking the U.S. economy as starting close to a position of no resource slack. This assumption was a reasonable one for much of the period from 1964 to 1979.
[205] For further discussion, see Nelson (2020b, Chapter 12).

one of them was written without adequate knowledge of actual policy deliberations in the 1960s. Such a suggestion would be incorrect, as both accounts were informed by practical experience in the U.S. economic policy process. One of the authors of Wonnacott and Wonnacott (1979), Paul Wonnacott, was a CEA staff economist in the late 1960s, while Tobin was a CEA member in the early 1960s. In the event, there is no need to adjudicate between the two statements. Appearances to the contrary notwithstanding, they are consistent with each other. And both statements are accurate.

The reconciliation of the two statements can be found by considering is an earlier Paul Wonnacott discussion. That account—P. Wonnacott (1974, pp. 264−265)—was, like that of Wonnaccott and Wonnacott (1979), emphatic that the Phillips-curve dilemma guided U.S. policy thinking in the 1960s. But it was also careful to indicate that the perception in officialdom of the dilemma did not instil a deliberate policy of inflation. Instead, Wonnacott's account indicated correctly that the Phillips-curve relationship underlay the U.S. authorities' use of wage-price guideposts alongside their expansion of aggregate demand in the first half of the 1960s.[206]

The upshot is that traditional Phillips curve analysis did influence 1960s U.S. policymaking—but the principal action galvanized by this part of policymakers' conceptual framework consisted of efforts to avoid purely market-based outcomes for prices and wages and to try to use incomes policy to reconcile full employment and price stability.[207] In terms of an equation-based representation of the Phillips curve, this strategy involved trying to make the perceived cost-push shock term in the equation less positive in value. And expressed in terms of a downward-sloping Phillips curve, the strategy implied efforts to shift the curve to the left by means of direct interventions in wage- and price-setting. The perspective underlying this strategy was expressed by Paul Samuelson—outside the 1960s U.S. administration but in sympathy with their economic policies—who stated (Samuelson, 1967, p. 11): "can we contrive stabilization devices to ensure a healthy Phillips curve for the American economy?"

Samuelson went on to say that he did not know what the answer to this question was but that he

[206] This is not to say that other commentators in the 1970s did not attribute to 1960s policymakers a deliberate policy of raising inflation in order to lower the unemployment rate. Economists who did so included Karl Brunner (see Nelson, 2019, p. 23) and Henry Wallich (1975b).

[207] This view is detailed in DiCecio and Nelson's (2009, pp. 427−432) account of 1960s policymaking. (The end of that exposition needs a correction, however: on page 432, "a zero" should be "a nonzero"—or, better, "a positive.") The extensive account provided by Romer and Romer's (2002) of policy thinking in the 1960s is also consistent with the interpretation outlined here. In this connection, it is worth underscoring that, although Romer and Romer stated (2002, p. 20) that "policymakers in the 1960s came to believe in a long-run tradeoff between unemployment and inflation," they did not claim that the actual settings of aggregate demand put in place by policymakers during that decade were designed to generate inflation or motivated by an intention to raise the rate of inflation.

had been discouraged (because it made it less likely that the answer would turn out to be "Yes") by various countries' lack of success in sustaining a successful incomes policy. Indeed, in a 1962 television appearance with Friedman, Samuelson had made it clear that countries would likely need to acquiesce in some positive inflation as a *quid pro quo* for full employment (see Nelson, 2020b, Chapter 13).

Robert Solow expressed a similar attitude to that of Samuelson on this matter. Rachel McCulloch, who enrolled as a special student at the Massachusetts Institute of Technology in the later 1960s, recalled: "I hadn't really seen much of Milton's work. And what I heard about it was not favorable. I remember Bob Solow saying that Milton and others of the Chicago School cared more about inflation than they did about unemployment—and so were unwilling to tolerate even a slight increase in inflation in order to increase employment. So, at that time, and actually for many years thereafter, Solow seemed to believe in a long-term Phillips curve." (Rachel McCulloch, interview, October 3, 2013.)[208]

It should be stressed that policy views like those just described did *not* amount to an endorsement of high inflation—indeed, it is quite possible that they amounted to a prescription of inflation rates not very different from 2 percent.[209] But they *did* apparently reflect a view that there was a long-run inflation/unemployment tradeoff—or, more specifically, that policymakers would have to face such a tradeoff if wage-price guidelines were not brought to bear on inflation. And as a fallback position in the event of the failure of incomes policy, these leading Keynesians in the 1960s seemed to regard some inflation as part of the price of securing full employment.

Officialdom in the 1960s, however, took the perspective that moving along a downward-sloping U.S. Phillips curve tradeoff was the implication of expansionary policies in a wholly *market*-based system for setting wages and prices, but that a combination of lower unemployment and little-changed inflation was possible if price and wage outcomes were shifted by the administration guideposts. In practice, U.S. officials may have taken the view that these

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[208] Such a recollection is also consistent with Fischer and Dornbusch's (1983b, p. 748) reference to "the 1960s concept of the inflation process as [involving] a tradeoff between inflation and unemployment." The present author would not, however, endorse Fischer and Dornbusch's additional statement (1983, p. 750) that "[i]n the 1960s macroeconomic policy [as distinct from much *academic* analysis of inflation] was based on the belief that there was a tradeoff," without adding the qualification that 1960s policymakers saw guidelines as a means of tempering (or, ideally, eliminating) the tradeoff.

[209] For example, the Cowles Foundation reported, on the basis of research subsequently published as Bodkin (1966), that the "tradeoff between the conflicting goals of full employment and price level stability" was one under which "the price stability 'cost' of full employment (defined as 3 percent unemployment) might be expected to be an inflation of consumer prices approximately equal to 1½ percent annum." In contrast, it stated, zero inflation would require "much unemployment" (Cowles Foundation, 1964, p. 20).

guidelines were not going to be fully adhered to by the private sector—so that a conflict between price stability and full unemployment was not wholly avoidable in real-world policymaking. Nevertheless, it needs to be underlined that Tobin (1982, p. 305) was correct both in stating that "the [Kennedy] Administration was not complacently willing to accept more inflation for less unemployment" and in indicating that President Johnson's "objective was not to reduce unemployment by a ride up the Phillips curve."[210]

Although pursuit of a lower-than-natural unemployment rate had not actually characterized U.S. monetary (or fiscal) policy in the 1960s and 1970s, there had been so many episodes of unintentionally overexpansionary policy. Multiple overshoots in the 1960s and 1970s by actual output of potential output, along with generally high inflation, reflected the fact that U.S. aggregate demand policy was repeatedly excessively lax. The reasons for this excessive ease have been discussed in the previous volume and in the earlier chapters (and again in Chapter 8, when the monetary explosion of 1976–1977 is considered). Over these decades, it was occasionally the case that policymakers thought that they were making aggregate demand settings (for given estimates of potential output) more restrictive than they actually were—for example in 1968, when a perceived change in the policy mix in favor of fiscal tightening and monetary easing produced a large overall relaxation of policy. But a recurring additional problem, as repeatedly noted in preceding chapters and as shown in Orphanides' (2003) pioneering work, was that estimates of potential output and potential employment were overestimated.[211]

In 1968, Friedman acknowledged that an excessive expansion arising from the monetary system could be generated by a decline in the natural rate of interest but seemed skeptical about the

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[210] Tobin (1982, pp. 304–305) did not, however, actually deny that U.S. policymakers believed in a long-run non-vertical Phillips curve (absent the successful implementation of measures such as wage-price guidelines). His account of the thinking underlying U.S. policymaking during the 1960s was, therefore, largely consistent with that given above.

[211] This was true, on a notably larger scale, also in the United Kingdom. As Budd (1979, pp. 205−206) observed when considering that country's demand-management experience, the scenario laid out by Friedman (1968a), involving permanent increases in inflation arising from expansionary policies, seemed relevant for the 1960s and 1970s, even though Friedman envisioned this as arising from attempts to push unemployment below its natural rate—something that not correspond to U.K. policymakers' likely intention. The scenario's practical relevance arose from the fact that U.K. policymakers' estimates of full-employment output had proved to be unduly high over this period (see Nelson and Nikolov, 2003; see also the discussion of U.K. unemployment in the previous chapter). Consequently, in the mid-1970s, having recapitulated the theoretical scenario in which policymakers followed policies that generated successively higher inflation rates, Friedman added that "the resemblance between that analysis and what has been happening in Britain is not coincidental: what recent British governments have tried to do is keep unemployment below the natural rate" (Friedman, 1976a, p. 227; see also Friedman, 1975f, p. 25)—though his exposition could have benefited from including the words "in effect" or "in practice" after "have."

empirical relevance of this possibility.[212] His doubt that a decline in the natural rate of interest could be a strong catalyst for monetary expansion may have been well founded. But events in the 1960s and 1970s showed that adverse shifts in potential output and the natural unemployment rate could produce monetary expansion if policymakers' actions rested importantly on obsolete estimates of these series. That Friedman's presidential address of 1967 entailed an edict against relying on estimates of productive or employment potential in policymaking was stressed in some retrospectives on that address, such as Holly and Hughes Hallett (1989, p. 69) and Orphanides (2002). Evidently, however, it was a concern that, in the period from the late 1960s onward, had greater immediate relevance—as a warning about the dangers associated with current U.S. policymaking—than Friedman himself realized at the time. And importantly, it was the case especially from 1970 onward that the U.S. authorities' errors in estimating full-employment output and the natural rate of unemployment occurred in circumstances in which policymakers' nonmonetary view of inflation slowed down their revisions of these estimates.[213]

Friedman's "Nobel Lecture: Inflation and Unemployment"

The announcement of Friedman's Nobel prize provided the trigger for his final presentation to his money-and-banking workshop at the University of Chicago. That presentation, delivered on November 23, 1976, was one in which he tried out a draft of his Nobel lecture, "Inflation and Unemployment."[214] This title was eventually revised to "Nobel Lecture: Inflation and Unemployment" in the May 1977 *Journal of Political Economy* published version. Apart from a subsequent filling-out of the reference list, Friedman's published Nobel lecture was very similar to his workshop draft.[215]

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[212] See Friedman's remarks (given in May 1968) in Jacobs and Pratt (1968, p. 47).

[213] Trevithick (1980, p. 74) took Friedman as being wedded to the assumption of a constant natural rate of unemployment in Friedman (1968a) and as consequently only willing to contemplate transitory changes in the natural rate in his follow-up discussions of the Phillips curve, such as that in Friedman (1977c). In fact, Friedman (1968a) had indicated that the natural rate of unemployment *could* move. And as discussed in the previous chapter he had, since the early 1970s, stressed the phenomenon of upward pressure on the U.S. natural rate of unemployment due to shifts in the structure of the labor market. Consequently, permanent increases in the natural rate of unemployment, although they were not the driving force of inflation/unemployment dynamics concentrated upon in Friedman's (1968a, 1977c) accounts of shifts in the Phillips curve, did form an important part of his overall interpretation of economic developments in the 1970s in the United States and other countries. (See also the discussions in previous chapters of potential-output estimates in the 1970s.) In addition, the notion that a preoccupation with stabilizing the economy at its full-employment level could lead to inflation if signs of excess demand were misdiagnosed as cost-push pressures was a warning Friedman advanced as part of his overall reservations concerning stabilization policy: see Friedman (1954a), as well as his remarks in American Bankers Association (1967, p. 118) and the discussion in Nelson (2020a, Chapter 10).

[214] This was also the version of the draft sent to Anna Schwartz.

[215] Even the published version of the Nobel lecture, however, gave a slightly incorrect title for the Phillips (1958) article—see Friedman (1977c, p. 472)—though Friedman did give the correct title both previously (for example, in Friedman, 1976a, p. 215) and later (for example, in Friedman and Schwartz, 1982, pp. 440, 649). The Nobel lecture

The paper recapitulated the arrival of, and reasoning underlying, the natural rate hypothesis and what it implied for the specification of the Phillips-curve relationship.[216] In doing so, it provided some connections between that literature and other work: prior eras' contributions on monetary neutrality and on specifying price-level adjustment; the more recent rational-expectations approach to real/nominal interactions; some of the empirical work done by economists in the 1970s on the Phillips curve; and, most tentatively, the public-choice literature and its implications for the modeling of policymaker behavior. But Friedman tried to go beyond this synthesis by examining reasons why the unemployment and inflation rates might not only have a lasting negative relationship but also might, on occasion, have a *positive* relationship. As motivation, he could point to the simultaneous rise in inflation and unemployment in many countries in the 1970s.

A fleeting positive relationship between inflation and unemployment was already allowed for in the natural rate hypothesis as part of the dynamics of the adjustment of expected inflation to actual inflation. This element of the hypothesis, one highlighted by Friedman even in 1966, was, of course, the key reason why the expectations-augmented Phillips curve was seen as valuable in explaining the pronounced stagflation experienced repeatedly since 1966. Friedman's Nobel lecture, however, suggested further reasons why the relationship between the two series might (on occasion) be positive.[217] Notably, high absolute levels of inflation might tend to go hand-in-hand with greater variability of inflation. With a greater propensity for mistakes in pricing decisions in such an environment, price signals would not correspond to those that those that price-setters intended to send. The associated distortion to the relative-price structure might make for a less efficient economy, lower output, and higher unemployment.[218] The economic

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also gave the incorrect reference in directing readers to Friedman's 1966 outline of the natural rate hypothesis: Friedman (1966a) was referenced, instead of the Friedman (1966b) item that should have been cited. The claim by Freedman, Harcourt, Kriesler, and Nevile (2013, p. 21) that Friedman's Nobel lecture gave the "wrong page" when quoting Keynes (1936) is, however, simply not accurate. The page number that Friedman (1977c, p. 469) provided for the quotation was, in fact, the correct one.

[216] Public talks in the two years ahead of the Nobel lecture in which Friedman had reaffirmed the absence of a long-run Phillips-curve tradeoff had included an address in Portland, Oregon, on December 16, 1974 (Friedman, 1975e, p. 24), at the American Economic Association meetings held at the end of 1974 (Friedman, 1975c, p. 176), and an Australian television appearance in April 1975 (*Monday Conference*, Australian Broadcasting Commission, April 14, 1975, pp. 3–4 of transcript). See also Chapter 2 on his corresponding discussions during 1973 and 1974.

[217] As a popular exposition of views on inflation that appeared in this timeframe (Trevithick, 1977, p. 67) noted, according to "Friedman's rejection [sic; version] of the Phillips curve, it is evidently possible for both the unemployment rate and the rate of inflation to rise simultaneously provided that the unemployment rate [starts]… below the natural rate."

[218] See Friedman (1977c, pp. 464–467). In Friedman's preexisting framework, as with later New Keynesian analysis, relative price distortions associated with deviations from price stability would routinely make output differ from potential output and so are associated with economic efficiency. In his Nobel lecture, however, Friedman was concerned with the related but different possible implication of an inflationary environment—according to which

costs of distortions to relative prices had featured prominently in Friedman's critiques of wage and price control and in his advocacy of indexation.[219] But he was now applying it more heavily to the interpretation of Phillips-curve inflation/unemployment tradeoffs.

Another theme that Friedman repeated from his previous work was that inflation might trigger price controls that, in turn, worsened the allocation of resources, lowered potential output, and raised the natural rate of unemployment.[220]

Friedman emphasized, however, that the economy would eventually ride out the uncertainty associated with distortions of this kind. Any economy-wide wage and price controls imposed in response to inflation would collapse. And in the face of an open and variable inflation, he expected that the economy, and particularly price-setting arrangements in goods and labor markets, would ultimately adjust in a way that meant that a vertical Phillips curve would continue to characterize longer-run stretches of data. He consequently did not expect a positive inflation/unemployment relationship to endure. This point, already expressed in the Nobel lecture delivered in December 1976, was one Friedman reaffirmed shortly afterward (January 6, 1977) in the London *Financial Times*: on that occasion, he underlined the fact that the Nobel lecture did not advance an upward-sloping Phillips curve as a permanent part of the economic landscape and that he only viewed it as a temporary phenomenon, associated with shorter-run responses to a higher inflation rate.[221]

Friedman had presented similar arguments in his pre-1976 writings and statements on the adverse consequences for real economic activity arising from inflation.[222] He nevertheless

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uncertainty about inflation promoted more frequent price-setting mistakes by suppliers, in a manner that tended to lower overall production in the economy.

[219] See, for example, Friedman (1958a, p. 252; p. 183 of 1969 reprint) and the discussion in Nelson (2018; 2020a, Chapter 7).

[220] See Friedman (1977c, pp. 467–468). Before Friedman delivered his lecture, conservative newspaper columnist Phyllis Schlafly took the Nobel citation's references to Friedman contributions to the complexity of stabilization policy as signifying a recognition of his critique of wage and controls—and perhaps especially his opposition to those instituted by President Nixon (*The Times-Picayune* (New Orleans), November 4, 1976). This speculation was, basically, not well informed. Most of Friedman's critique of controls had appeared in research and public-policy writings separate from his major research contributions on stabilization policy, and this was certainly true of his critique of the Nixon controls in particular. A possible element of validity in Schlafly's interpretation lay in the fact, already mentioned above, that one of Friedman's extended critiques of the likely course that stabilization policy would take—Friedman (1954a)—suggested that policies aiming to manage aggregate demand could generate inflation that, in turn, was inappropriately interpreted as due to non-demand forces. As Friedman underlined in other writings, this danger of misguided cost-push interpretation of inflation's sources was a major reason for recourse to wage and price controls.

[221] For the relevant statements in the Nobel lecture, see Friedman (1977c, pp. 464, 468).

[222] See Nelson (2020a, Chapter 7).

expected his use of them in his Nobel lecture to make a more sizable impact on economic research than it did. He evidently believed that laying out a sequence, apparently plausible as a description of experience in the past two decades—in which the Phillips curve was successively found to be downward-sloped, vertical, and upward-sloping not only made for an elegant exposition in his lecture but was also something that would be seen by economists as a major insight, because the lecture provided a cohesive economic explanation for the whole sequence.

Such a highly positive reaction to the research contribution of the Nobel lecture generally did not emerge. The reception for Friedman's workshop presentation foreshadowed the low-key reaction that the lecture received in the research world. Workshop participants seemed underwhelmed by Friedman's talk. Indeed, one of the criticisms raised by a participant, Eugene Fama, took the objections to Friedman's argument altogether away from the accelerationist-versus-accelerationist or Keynesian-monetarist lines of debate. Instead, Fama foreshadowed real business cycle (RBC)-type arguments against Phillips-curve analysis—to the effect that Phillips-curve debates were misconceived because output was always at its potential value anyway. Fama recalled that, in the workshop session, "I was badgering him on" the point that, rather than try to reconcile upward-sloping, downward-sloping, or vertical Phillips curves in the data, why should Friedman not conclude that the heterogeneous mixture of historical unemployment/inflation combinations "means there's no Phillips curve" relationship at all (Eugene Fama, interview, September 11, 2013).[223]

Friedman's Nobel lecture was delivered at the Stockholm School of Economics on December 13, 1976.[224] The considerable international attention given to the lecture immediately after it was delivered did point toward the possibility that it would acquire considerable prestige. In the United States, the lecture attracted widespread newspaper headlines (for example, *The Oregonian* (Portland, Oregon), December 14, 1976) and television coverage (for example, on the U.S. national network CBS).[225] In due course, the published form of the lecture also obtained a

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[223] As Fama, like many other finance economists then and later, favored a flexible-price-based interpretation of macroeconomic behavior, his vision was formally equivalent to postulating a vertical Phillips curve at all time horizons. This framework also implied that variations in the unemployment rate could not be interpreted (as Friedman was partly doing) as reflecting movements in the amount of economic slack in the economy. That being the case, estimated Phillips curves did not really cast light on structural nominal/real interactions. See Atkeson and Ohanian (2001).
[224] Unusually for a Nobel lecture, the published version in the *Journal of Political Economy* (Friedman, 1977c) did not specifically give its date and location of delivery.
[225] *CBS Evening News* anchor Walter Cronkite led an item, in his broadcast of December 13, 1976, with the words (p. 7 of transcript): "In Stockholm today, Nobel economics prizewinner Milton Friedman said the Western world may have to live with high inflation and high unemployment for several decades." The lecture captured a sufficient hold on the public imagination that a selling point of a new edition of a popular book on inflation was that "the

very large number of citations in the economic-research literature. But Friedman's 1968 article remained his definitive Phillips-curve paper. In contrast, the Nobel lecture was not perceived as breaking much new ground.

III. PERSONALITIES IN DEBATES ON REGULATION, THE PHILLIPS CURVE, AND POLICY RULES, 1975–1976

CAMPBELL MCCONNELL

The conferring to Friedman of a Nobel award amounted to an acknowledgment by the elite members of the economic-research world that he had shifted the center of macroeconomic opinion significantly. The breadth of the citation confirmed that the shift engendered by Friedman had not been limited to the acceptance—widespread by the 1970s—of his permanent income hypothesis concerning consumption behavior. Rather, the change in viewpoints undergone by macroeconomists in response to Friedman's research had also involved a considerable degree of conversion to the more-divisive, and more monetary-policy-focused, aspects of his work discussed in the previous section: his solo- and coauthored studies of the role of money, his analysis of the specification of the Phillips curve, and his work on the formulation of monetary policy rules. In 1966, as discussed in Section I of this chapter, Walter Heller had confidently spoken, albeit on slender evidence, that Friedman had fallen in line with "the profession" on macroeconomic matters. By 1976, one could suggest, and with stronger foundations, that it was Friedman who had reshaped the consensus of the economics profession, especially in the United States.

Although the decision to give Friedman an award made by the leaders in the economics profession, the Nobel award was also important in signifying a change in *rank-and-file opinion* among economists regarding the views on macroeconomics that Friedman had espoused. Previous chapters of this book, together with the preceding volumes, have already considered notable cases of conversions to Friedman's macroeconomic views—as well as the giving of ground, and other implicit or explicit softening in the opposition to Friedman—that occurred among figures based at prominent universities and economists who were regular fixtures at major research conferences. But it is important also to grasp the importance, as part of the process in which Friedman's views gained traction in the economics profession as a whole, of

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author brings us up to date with such things as Friedman's Nobel Lecture [published] in 1977." (From the back-cover text of Trevithick, 1980.)

the acceptance and assimilation of Friedman's views among more junior figures: individuals who held lower-ranking positions in major universities, who held junior and senior positions at smaller universities, or who reflected and influenced the overall professional consensus through teaching, dialogue with other economists, and their own publications, including textbooks.
A case of an up-and-coming economist whose work reflected, developed, and expounded Friedman's approach to macroeconomics was Michael Wachter. Wachter was (and is) located at a major university: the University of Pennsylvania, one of the so-called "top 20" U.S. centers for economics. In time, he would become highly senior at the university, but he joined it as an assistant professor (at its business school, the Wharton School) in 1969, shortly before receiving his Ph.D. from Harvard University (American Economic Association, 1974, p. 421). Wachter was therefore initially junior in an institution whose senior figures included two longtime adversaries of Friedman: Lawrence Klein and Albert Ando. Wachter's research in the 1970s would line him up with Friedman's positions. Being a labor-oriented macroeconomist, Wachter had little to do with debates on the role of money specifically. But in focusing on the labor-market aspects of macroeconomic debates, he took much inspiration from Friedman's work in macroeconomics. Specifically, in his research in the 1970s, Wachter followed up Friedman's research on three major dimensions: he found in favor of the natural rate hypothesis; he shared Friedman's skepticism about cost-push inflation; and he doubted existing estimates of the output gap and the full-employment unemployment rate.

Ross and Wachter's (1973) study of the Phillips curve noted that "Friedman and Phelps have argued that the curve will shift vertically upward by 1 percent for every 1 percent increase in expectations."[226] Their paper appeared in *American Economic Review* eighteen months after Tobin (1972) had used the publication of his AEA presidential address in the same journal to argue against the natural rate hypothesis. In contrast, while they had their own proposed modifications of the Phillips curve, Ross and Wachter strongly echoed the Friedman-Phelps message that long-run real output gains could not be purchased by inflation: "if the government interprets the short-run Phillips curve as a loci of attainable long-run options, attempts to control the economy can be destabilizing."[227]

Nine months later, a study by Wachter of the early stages of the Nixon price controls appeared in the *American Economic Review*. "The basic point of this paper is that cost-push inflation has not been the source of the 1970–71 inflation," Wachter wrote, while adding that his findings

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[226] Ross and Wachter (1973, pp. 686–687). By this point, Wachter had become an associate professor (p. 675).
[227] Ross and Wachter (1973, p. 691).

"discounted the likelihood that the current inflation is due to exogenous cost-push elements" (Wachter, 1974, p. 489). He concluded that wage and price controls were therefore not needed, while also pointing out that the "cost of an inappropriate program of wage and price controls is discussed by Milton Friedman."[228] Wachter tried in his paper not to ascertain the causes of inflation and to restrict himself to testing, and then ruling out, certain explanations for inflation. But in a talk given later in the year, he was less reticent on the matter of causes. Wachter sided with the monetarist explanation, citing the "massive easy-money policy" that had been followed during the Nixon years after the administration's early restraint, and lamenting the "gimmickry" of wage-price controls, while affirming that "by and large, the unions have not been a factor" (*The Courier-News* (Bridgewater, New Jersey), October 15, 1974).

Furthermore, during the mid-1970s Wachter became one of the economists who spoke out on the point that the natural rate of unemployment was being understated in the estimates reported by U.S. officialdom and used by leading Keynesians. In July 1978, it was reported: "One economist, Michael L. Wachter of the University of Pennsylvania, has estimated that the changes in the workforce have added 1.5 percentage points to what would be considered the 'full employment' rate." (*Omaha World Herald* (Nebraska), July 16, 1978.) Indeed, in a paper presented at the Carnegie-Rochester conference series the previous April, Perloff and Wachter (1979) had used a long-run vertical Phillips-curve framework in conjunction with an analysis of the labor market as the basis for their contention that, notwithstanding recent official downward revisions in estimates of the size of potential output, and a corresponding narrowing of the estimated output gap to less-negative levels, "even this gap may be too large and may need to be halved again."[229] This caution about output-gap estimates echoed warnings in Friedman's past work and foreshadowed major official revisions of potential output and the output gap that were made in 1979.[230]

Meanwhile, an economist who was not located in any of the top-20 universities, but who made a contribution to the Phillips-curve debate in a prominent U.S. research outlet, was Roque P. Fernandez. Fernandez was on the staff of the International Monetary Fund when he published an article in the September 1977 issue of the *American Economic Review*. Fernandez's paper, which used Latin American data to examine output/inflation dynamics, had been presented in

[228] Wachter (1974, p. 489). In this connection, Wachter cited Friedman (1966a), although his page numbering (and the incorrect title he used in his citation) indicated that he also meant to cite Friedman (1966b).
[229] Perloff and Wachter (1979, p. 114). The conference at which this paper was presented was held at the University of Rochester on April 28–29, 1978 (Brunner and Meltzer, 1979, p. 1).
[230] On these revisions, see Chapter 10 below.

preliminary form at an NBER conference in Panama on October 31–November 2, 1975.[231] He had also presented the paper at Friedman's money-and-banking workshop in the mid-1970s, and Fernandez's published acknowledgments thanked Friedman for comments. [232] The results in the article, Fernandez observed, indicated "some evidence in favor of a short-run tradeoff between inflation and output… However[,] the short-run tradeoff that we have found does not contradict the natural rate hypothesis of Milton Friedman."[233] Indeed, Fernandez summarized his analysis as combining two "relatively new aspects of macroeconomic theory," namely, the natural-rate specification of the Phillips curve and rational expectations.[234]

By 1977, in fact, the main debate on the natural rate hypothesis was nearly complete in U.S. academic debate. In 1978–1980, James Tobin and Paul Samuelson, who had hitherto been two of the major Keynesians holding out against the hypothesis, would both indicate that they now largely accepted the validity of the long-run vertical restriction implied by the Friedman-Phelps augmentation of the Phillips curve.[235]

The Campbell McConnell phenomenon

As it became clear during the mid-1970s that the natural rate hypothesis was now subscribed to by the majority of U.S. economists, a logical next step was for this consensus to be consecrated by its inclusion in undergraduate textbook discussions. Undergraduate textbooks were, of course, a traditional vehicle by which the articulation of the consensus view of the profession could be relayed to new students in economics. This was a matter on which Friedman himself felt strongly. He remarked on television five months after receiving his Nobel award that the "true test" of his or his contemporaries' contributions consisted of whether these contributions were "cited in the textbooks"—and not just ephemerally, but in successive editions of textbooks (*Dinah*!, March 23, 1977).

And in the world of such textbooks—specifically, the mass-circulation undergraduate

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[231] See Fernandez (1977, p. 595) and (for the specific dates of the 1975 conference) Schydlowsky (1979, p. 311).
[232] See Fernandez (1977, p. 595).
[233] Fernandez (1977, pp. 604–605).
[234] Fernandez (1977, p. 608).
[235] See Nelson (2020b, Chapter 13) and Chapter 8 below.
There were some isolated instances of economists going along a different, and more convoluted, path during the 1970s: first moving toward *acceptance* of the Friedman-Phelps version of the Phillips curve, but *later* rejecting it in favor of some other specification of inflation behavior. Arthur Okun may have been one example of an economist following this course: see Section III of the previous chapter. Other possible examples were Leonard Rapping and, outside the United States, James Trevithick and David Shephard.

economics-principles textbook—one of the leading players by the mid-1970s was Campbell R. McConnell. McConnell, of the University of Nebraska-Lincoln, was a prime example of how success in textbook authorship could turn the author into a giant. He was never employed at a "top-20" university. Indeed, he had been based continuously at the University of Nebraska-Lincoln since receiving his Ph.D. in 1953.[236] And, though he had a creditable publication record, McConnell had certainly not marked himself as a leading contributor to research in either microeconomics nor macroeconomics, and he was very far from a regular presence at major research conferences. Yet, after an experience of teaching undergraduate economics in which "I used a number of books in class and thought there was a better way of presenting things" than what was available in the existing texts (*Omaha World Herald* (Nebraska), April 4, 1976, p. 2A), McConnell successively reworked his own course expositions of each economics-principles topic into prose form (*New York Times*, April 4, 1976, p. F1). The result was the first edition of his text, *Economics: Principles and Policies*, in 1960.[237]

In producing this text, McConnell would profoundly influence teaching of introductory economics at universities in the United States. In 1976, the sixth edition of the textbook (McConnell, 1975a) was poised to amass 400,000 in sales, ahead of the 300,000 projected for the contemporaneous edition of Paul Samuelson's *Economics*. This development prompted the *New York Times* to publish a profile of McConnell, headlined "An Economist Who Outsells Samuelson" and proclaiming McConnell "The New Textbook Champion" (*New York Times*, April 4, 1976, p. F6).[238]

A key reason for McConnell's success was his adeptness at exposition and organization. McConnell described himself as "sort of a stickler for organization" and that he aimed to produce "a book that would anticipate [which] concepts students had the most difficulty with" (*Sunday World-Herald* (Omaha, Nebraska), April 4, 1976, p. 2A). In contrast, as Gregory Mankiw—a leading textbook writer in a later generation (see, for example, Mankiw, 1997)—observed with regard to Samuelson's textbook: "It was not a super clearly-written book. It's kind of a bit shocking that it was as successful as it was, given that it tends to be a little opaque at times. But it was a hard book, and I don't think it ever sold as well at the mid-level schools."

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[236] See American Economic Association (1974, p. 264).

[237] See McConnell (1960). (The first edition's main title was *Elementary Economics*.)

[238] Although he was not a writer of undergraduate textbooks and, after the 1940s, had little involvement in teaching undergraduate courses, Friedman was interested in the content and sales performance of different economics-principles texts. When Gregory Mankiw, the doyen of modern textbook authors, visited the Friedmans in the early 2000s, he and Milton Friedman "talked about economics textbooks… And he was curious [about] how many copies Samuelson's books had sold, and we talked about that." (Gregory Mankiw, interview, September 24, 2013.)

This was especially the case once McConnell's book—issued by the same publisher as Samuelson's, McGraw-Hill, but marketed as more accessible than Samuelson's text—gained popularity.[239] Mankiw recalled, "When [the] McConnell [text]—when it first came out, I think it came out largely to be an easier version of the Samuelson text: to cover the material in much the same way that Samuelson did, with the choice of topics that Samuelson made. But in a way that was much more accessible." (Gregory Mankiw, interview, September 24, 2013.)[240] Indeed, the *New York Times* profile of McConnell indicated that a critic of Samuelson's textbook had said that "[in] each new edition [Samuelson] puts in more difficult material and [then] takes out the easier reading [in order] to keep the text size [the page length] unchanged" (*New York Times*, April 4, 1976, p. F1).

Another reason, by the mid-1970s, for the success of McConnell's textbook was the division that had emerged in macroeconomics. Stephen Axilrod, upon leaving the University of Chicago in 1952 for the Federal Reserve Board, quickly realized that the quantity-theory-of-money approach— emphasized in the Friedman money-and-banking workshop in which Axilrod had participated—was not received congenially in the American economics world: "I went to the Board, and I started in the Division of International Finance, where I spent four or five years. The director of the International Division was Arthur W. Marget. He was famous mainly because he wrote two volumes against John M. Keynes, which was not the way to become famous in a positive way in the 1950s."[241] In this atmosphere, Samuelson's Keynesian-economics-focused textbook became predominant at U.S. universities. Looking back on this development, Gregory Mankiw commented: "I think that shows that the world was looking for a different approach and a more modern approach to economics, and Samuelson was the person on the scene with it." (Gregory Mankiw, interview, September 24, 2013.)

McConnell had been a university student over much the same period as Axilrod, but at institutions (Cornell College, the University of Illinois, and the University of Iowa) that had far less of a stake in either the Keynesian revolution or in the nascent monetarist counterrevolution than those (Harvard University and the University of Chicago) at which Axilrod had studied.[242]

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[239] McConnell indicated that Samuelson was likely consulted by McGraw Hill in the early 1960s about whether he was agreeable to the publisher putting out McConnell's text. "Professor Samuelson has been very generous, very cordial. I suspect he [had] had a chance to read the manuscript before it was printed, and he might have said to go ahead with it." (*Sunday World-Herald* (Omaha, Nebraska), April 4, 1976, p. 1A.)
[240] The *New York Times* profile of McConnell had itself stated (April 4, 1976, p. F1): "By all accounts, including his own, Professor McConnell's successful formula has been to hold close to the Samuelson framework while explaining economics in simpler language."
[241] In Small and Carter (2008, p. 4).
[242] See American Economic Association (1974, p. 264).

Combined with the fact that McConnell's focus was on teaching, not on research or public-policy activity, the result was that, as McConnell himself acknowledged, "I have no strong feelings about economic policy" (*New York Times*, April 4, 1976, p. F1). This neutrality situated McConnell better than Samuelson in the 1970s. Samuelson was a key player in the Keynesian-monetarist debate, particularly in the public-policy area and, although his textbook was said to aim to be neutral on the debate (*New York Times*, April 4, 1976, p. F1), this objective was not universally regarded as achieved. Alan Stockman, who had been a graduate student at the University of Chicago in the mid-1970s and had begun his dissertation work with Friedman, would judge that Samuelson's textbook failed on the criterion of neutrality: Stockman's verdict was that it "often uses language in ways that impart a strong bias to the discussions" while treating matters of ongoing controversy in the research literature as though they had been settled in a manner favorable to Samuelson's own position (Stockman, 1982, p. 3; see also *Washington Times* (Washington, D.C.), January 28, 1983).

McConnell, in contrast, stated diplomatically in his *New York Times* interview that there were "valuable insights in monetarism" while "Keynes shouldn't be thrown out" (April 4, 1976, p. F1). The 1975 edition of his textbook, in a section titled "The monetarist position," praised "Professor Friedman's pioneering work in monetary economics," although he maintained that the share of monetarists in the profession remained "relatively small" (Campbell, 1975a, p. 358). The same chapter closed by giving an extended excerpt from Friedman's exposition of inflation as a monetary phenomenon, while also contrasting this with an "eclectic view" that offered some mild qualifications regarding this matter.[243] McConnell's recognition of the need not to present the Keynesian revolution as the last word on macroeconomics was also evident in the readings book (McConnell, 1975b) that accompanied his text. The chapters in this book included multiple reprints of Friedman pieces of writings. In addition, in reprinting *Time*'s 1965 article on Keynesian economics, Campbell replaced the original title, which had actually consisted of the infamous Friedman quotation "We are all Keynesians now," with the more neutral title "Keynes and the New Economics" (McConnell, 1975b, p. 81).

McConnell's ascendancy to the highest reaches of the economics-textbook world came at a time when U.S. undergraduate enrolment in economics had soared, from about 800,000 at the start of the 1970s to about 1.1 million in the mid-1970s. This reflected a galvanization of public interest in the field: "Economics news has been taken off the back pages and put on the front," Michael Elia, who as a senior editor at McGraw Hill oversaw production of both the McConnell and

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[243] See McConnell (1975a, p. 366). The Friedman excerpt was from Friedman (1966a, pp. 25−26).

Samuelson textbooks, was quoted as observing (*Sunday World-Herald* (Omaha, Nebraska), April 4, 1976, p. 2A). The increased interest was mainly arising from the macroeconomic developments that, as discussed at the start of this chapter, had a counterpart in further increases in Friedman's media profile.[244] During the macroeconomic turmoil of the mid-1970s, McConnell indicated that a major current interest of his was achieving improvement in the teaching of *intermediate* undergraduate macroeconomics (American Economic Association, 1974, p. 264). This interest hinted at the possibility that a McConnell text on intermediate macroeconomics was around the corner. In the event, this did not occur, and another McGraw-Hill product, the Dornbusch-Fischer (1978) textbook, which is extensively discussed in Chapter 8 below, materialized and became predominant in the area of intermediate-macroeconomics texts.

Dornbusch and Fischer ran aground, however, in their later attempt to challenge McConnell's supremacy in the principles-textbook realm. "From two internationally distinguished authors and economists, the book you've been waiting for… *ECONOMICS* by Stanley Fischer and Rudiger Dornbusch… Coming in Spring 1983!," proclaimed a full-page advertisement in the February 1983 *Journal of Political Economy* (McGraw-Hill, Inc., 1983, p. 201). But this new economics-principles text (Fischer and Dornbusch, 1983b) failed to gain a foothold in terms of take-up at universities. Dornbusch and Fischer then threw in the towel on competing with McConnell in the macroeconomics-principles area. Instead, with their colleague, MIT microeconomist Richard Schmalensee, they revamped the microeconomics material of their 1983 book into a standalone microeconomics-principles text, *Introduction to Microeconomics*. This textbook did not appear until 1988. When it did so, the McConnell juggernaut crushed the new competition in book sales and in take-up in course teaching, and no further version of *Introduction to Microeconomics* ever appeared.

McConnell's coverage of the expectational Phillips curve

One of the ways in which Dornbusch and Fischer had marked themselves out in macroeconomics-textbook treatments was their taking the Friedman-Phelps version of the Phillips curve not as a possibly-valid challenge to the preexisting consensus but, instead, as a legitimate and settled modification of the Phillips relationship—and the one to be used in the

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[244] Of course, this process had built up during the 1960s too. McConnell's awareness of this phenomenon was underlined by his inclusion in McConnell (1975b) of a 1969 article on the Keynesian-monetarist debate that had referred to the "money supply controversy now raging in the popular press" (Cox, 1969, p. 72; p. 98 of 1975 reprint).

textbook's baseline model and to be integrated into aggregate-demand/aggregate-supply curve analysis.[245] This was the approach that Dornbusch and Fischer took in their texts from 1978 onward, as did another major intermediate-macroeconomics text of that year, Gordon (1978).[246]

The Friedman-Phelps challenge to the Phillips curve was, however, a matter on which McConnell proved to be something of a laggard. He recognized its importance only belatedly in his textbook's treatment.

McConnell's encounter with the Phillips-curve literature occurred after he had established himself as a longtime opponent of approaches viewing nominal wage growth as an institutional datum. He had described one of his own longtime concerns as being to refute the fundamental assertions of the institutionalist view on a point-by-point basis" by making the case that "underlying economic forces continue to dominate wage determination."[247] Therefore, like Friedman, he had no time for pure cost-push views of inflation. Unlike Friedman, however, McConnell saw "the pioneering efforts of the British [sic] economist A.W. Phillips in 1958" as having provided the solution," and McConnell was satisfied with a specification of nominal wage-growth behavior that was based on a permanently downward-sloping Phillips relationship, one that implied that could be shifted by incomes policy, but that, absent incomes policy, implied a "dilemma" for aggregate-demand policy in choosing a combination price stability and full employment: "society in can effect select a position on the Phillips curve."[248] Notably, in a book of readings on wage determination that he oversaw in 1970, McConnell seemed very well disposed toward the partial-cost-push or dilemma version of the Phillips curve and concluded the readings with an abridged reprint of Solow's (1966a) case for guideposts, without publishing Friedman's comment on the Solow article (in which Friedman had made the case for the natural rate hypothesis).[249]

It was against this backdrop that, in contrast to the attention given in McConnell (1975a) to Friedman's views when the textbook covered the power of monetary policy versus fiscal policy and the link between monetary growth and inflation, his consideration of the debate concerning

[245] See Nelson (2020a, Chapter 1) as well as Chapter 8 below.
[246] An earlier intermediate textbook, P. Wonnacott (1974), mentioned earlier in this chapter, is worth noting for having included an extended exposition of the accelerationist view (pp. 281−289). Wonnacott explicitly cited Friedman and Phelps on the matter, and he offered one graphical representation of their Phillips curve (p. 287). However, he did not actually plot the long-run-vertical curve graphically and did not integrate the Friedman-Phelps arguments into aggregate-demand/supply analyses. Furthermore, he presented the Friedman-Phelps argument as one "of several possible explanations" for recent years' outward shifts in the empirically-observed curve (p. 292).
[247] McConnell (1970, pp. 2, 85).
[248] The quotations are from McConnell (1970, p. 293). Phillips was, of course, originally from New Zealand.
[249] See McConnell (1970, Chapter 36).

the Phillips curve was brusque. McConnell's two-page discussion plotted only downward-sloping Phillips curves, stated that it was a fact that society had to choose between price stability and unemployment, in the absence of incomes policy (p. 371), and did not refer to Friedman or Phelps by name. The only concession to the latter two's arguments was the statement (p. 372) that among the explanations offered for the simultaneous deterioration of inflation and unemployment in the early 1970s was that "the prolonged inflation of 1966–1969 had inflamed inflationary expectations." This partial and *pro forma* acknowledgment of the natural-rate argument contrasted with the overt drawing-upon of that argument by the *Economic Report of the President* in the same year.[250]

McConnell was likely taking a very conservative approach to interpreting the emerging consensus in the profession on the Phillips curve. Writing this edition in 1973 and 1974, he was evidently still reluctant to give the imprimatur of his textbook to the natural-rate position.

A similar caution was exhibited by Alan Blinder when he and William Baumol first produced a principles textbook. The Baumol-Blinder textbook, which first appeared in 1979, was, like the later Fischer-Dornbusch (1983b) book, a principles text written by "top-20"-university professors. Unlike Fischer and Dornbusch's textbook, it proved to be a major commercial competitor to McConnell's text over many editions. In the macroeconomics portion of the 1979 edition, largely drafted by Blinder during 1976 and 1977, the natural-rate argument was not applied to the Phillips curve. Instead, the Phillips-curve discussion (Baumol and Blinder, 1979, pp. 286–290) used a permanently downward-sloping curve. Such a fixed curve, the authors acknowledged, "cannot explain the inflation of the 1970s," but they implied that the reconciliation between the curve and the recent data was solely attributable to the existence of exogenous price shocks in the 1970s.

Blinder explained this choice: "As a textbook writer—an elementary-textbook writer; this is not a graduate text—I have the view that the textbook should lag substantially behind the frontiers of research, because the frontiers of research are too mercurial—they change too often. And a textbook should be a lagged and very much modulated version [of the professional consensus]—not swinging to extremes in the way the faddishness of the frontier does." Blinder by 1976 had decided that the no-long-run tradeoff view had likely prevailed, but "when it came to writing the textbook, I said, 'You know, this is a real intellectual battleground. Who knows how this is going to come out? And I don't think I should put it in the textbook.'" This changed in the 1982

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[250] See Section II above.

edition, which endorsed the long-run-vertical Phillips curve and even used "natural rate of unemployment" terminology (Baumol and Blinder, 1982, pp. 301−309).[251] "I said, well, now it's time to put this into the minds of college freshmen." (Alan Blinder, interview, December 6, 2013.)[252]

Unlike Blinder, McConnell was not in the trenches of macroeconomic debates. But he, too, was undoubtedly aware that his 1975 textbook discussion of the Phillips curve—already obsolescent as a representation of current views when it appeared—was becoming increasingly inaccurate as a description of the professional consensus as the 1970s progressed. Therefore, though in April 1976 McConnell suggested that it would be "giving away trade secrets" if he disclosed what material was likely to be added to future editions (*Sunday World-Herald* (Omaha, Nebraska), April 4, 1976, p. 2A), a knowledgeable reader of the 1975 edition of his textbook would have known that an overhaul of his Phillips-curve discussion was inevitable. The need for such a revision was underlined six months later by the Nobel Committee's recognition of the importance of the natural rate hypothesis, through its award to Friedman.

McConnell's bow to the inevitable was crystalized in the eighth edition of his textbook in 1981—an edition that reflected "the most extensive revision [ever] undertaken" of the text (McConnell, 1981, p. xxiii). The eighth edition contained a section on the "accelerationist view" of the Phillips-curve relationship (pp. 364−365). Although he portrayed the debate as still ongoing, and he associated the accelerationist position specifically with Friedman instead of being part of a consensus (pp. 364, 366−367), McConnell conceded that the 1955−1979 data on inflation and unemployment for the United States were, if the period was taken as a whole, consistent with a vertical Phillips curve, in which case "in the long run there is no tradeoff between inflation and unemployment" (McConnell, 1981, p. 364).[253] McConnell's textbook

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[251] See page 303 of the text for the deployment of this terminology. Baumol and Blinder followed their revised Phillips curve discussion with another new page of discussion, headed "The Theory of Rational Expectations" (pp. 309−310). Blinder recalled: "Three years later, I wouldn't say the debate was over, but I think we all, researchers, were getting much closer to consensus. And at least as important than that: Any doubt I might've had in my head in '77, [to the effect] that the rational expectations revolution was a fleeting fad that would go away, was gone. I didn't entertain that thought anymore." (Alan Blinder, interview, December 6, 2013.)
[252] Blinder, of course, still did assign a major role to price shocks in understanding the inflation of the 1970s. But he viewed these as a source of disturbance to a Friedman-Phelps-style Phillips curve.
[253] Prior to this, the economics-principles text of Wonnacott and Wonnacott (1979, p. 281) had discussed the long-run-vertical Phillips curve and plotted it. They had also explicitly cited Friedman and Phelps on the matter (p. 279). Still earlier, the undergraduate money-and-banking textbook by Thomas Simpson, a former student of Friedman's, had described the downward-sloping Phillips curve as reflecting "fundamentally a short-run tradeoff" on the grounds that "[i]n the long run, there is a tendency for the unemployment rate to return to its initial (natural) level" (Simpson, 1976, p. 347). Simpson then plotted a wage-growth/unemployment rate diagram featuring successive downward-sloping short-run Phillips curves and a vertical long-run curve (p. 348).

discussion had therefore caught up with the outcome of a debate that was largely settled in U.S. academic exchanges by the mid-1970s.

Although Friedman in 1975 and 1976 could look with satisfaction at how the debate on the inflation/unemployment relationship was being resolved in U.S. academia, he had far less reason to be happy with how the discussion of the very same issue was proceeding in national popular discourse. As indicated in Sections I and II above, it was not uncommon at all in U.S. policy circles for the advent of stagflation to be treated *not* as a vindication of Friedman and Phelps, but, instead, as a confirmation of hardline cost-push views—and, correspondingly, as a basis for *rejecting* the centering of anti-inflation policy on aggregate-demand restraint.[254] A notable political figure taking this stand was Senator Henry Jackson. In a June 1975 speech, Jackson blamed the high U.S. unemployment rate on the economic views of President Gerald Ford—who, Jackson claimed, "thinks you can bring prices down by putting people out of work" (*Asbury Park News* (New Jersey), June 14, 1975). As Friedman saw things, Jackson was drawing exactly the wrong lessons from the United States' recent macroeconomic experience. It was, however, in a different area of domestic economic policy that Jackson would particularly infuriate Friedman—the area, intersecting microeconomics and macroeconomics, of energy policy.

HENRY JACKSON

Throughout President Ford's time in office, and particularly in the first eighteen months of his tenure, his administration was heavily concerned with the ongoing adjustment by the United States to the OPEC oil shock of 1973−1974—including notably, as part of that adjustment, the development of the federal government's legislative and regulatory response.[255] To Friedman's frustration, the net outcome of these efforts was largely a continuation of the controls-heavy approach to energy policy that had been the subject of such withering commentary on his part during 1973−1974. When, six months into Ford's tenure, Friedman considered why the

[254] This point was also stressed in Chapter 2 above. See also the analysis in Chapter 8 of the debates in the 1975−1978 period on the Humphrey-Hawkins bill. As that discussion stresses, advocates of this bill pointed to the fact that inflation and unemployment had risen together as a reason for believing that highly stimulative economic policies would not be inflationary. Although these arguments were largely made by Democratic party politicians, some Democratic-affiliated economists spoke out against the bill because they were convinced by natural-rate-based arguments that suggested that inflation and demand were still linked. Michael Wachter, for example, worked for the Carter presidential campaign but criticized the Humphrey-Hawkins bill in its existing form—arguing of its advocates, "people like that have no idea how economics works." (*Philadelphia Inquirer*, September 12, 1976.)
[255] Some of the foreign-policy and international-economic-policy aspects of the Ford Administration's response to the first oil shock are considered in the next chapter.

president was not espousing a more full-fledged free-market outlook toward energy issues, he suggested that the answer lay in the fact that Ford was trying to secure a political compromise that could "appease" a key critic of the president in Congress—the aforementioned Henry M. "Scoop" Jackson, Democratic U.S. senator, of the state of Washington (*Newsweek*, January 27, 1975*a*, p. 25).

Jackson was one of the figures in U.S. public discourse who would come to be known as neoconservatives—defined by Garthoff (1994, p. 455) as "combining a liberal domestic political stance with a harder foreign policy line." "Liberal" in this sense meant favoring a high degree of public-sector intervention in the U.S. economy. Friedman repeatedly expressed his unhappiness about the term "liberal" being used in this way. He preferred its older usage: "I call myself a liberal in the true sense of liberal," he remarked on television in December 1975, and he and Rose Friedman would later describe their own position as one embracing "liberal ideas—in the original nineteenth-century meaning of liberal."[256] But they also accepted that, in modern U.S. political dialogue, "liberal" meant something different. Against that background, they granted that the neoconservatives of the 1970s were, on account of their activist economic prescriptions, correct to consider themselves to be "(modern) liberals" as far as domestic U.S. policy issues were concerned.[257]

In common with other neoconservatives in this period, the hawkish outlook toward foreign and defense policy articulated by Senator Jackson included deep skepticism regarding actual and prospective U.S.-Soviet arms-control agreements, opposition to various other aspects of the U.S.-Soviet détente of the 1970s, and concern about the cutbacks in U.S. defense spending programs that occurred during 1970–1975. These positions put Jackson at odds with many in his own party and also, to a very considerable degree, with the Nixon and Ford Administrations.[258] They also made Jackson unusual among leading Democratic politicians in being the frequent recipient of highly favorable commentary in the editorials of newspapers like the *Wall Street Journal*—which shared his perspective on national-security matters, including his reservations about many

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[256] The quotations are, respectively, from *The Open Mind*, PBS, December 7, 1975 (p. 1 of transcript) and Friedman and Friedman (1988, p. 464). In personal communication with the present author on July 21, 2003, Friedman likewise referred to "good economics and liberal, in the true sense, policy."
[257] Friedman and Friedman (1988, p. 464).
[258] Much of Jackson's break with the Nixon Administration on matters concerning defense and foreign policy occurred in 1972. In mid-1971, for example, he had defended Nixon's actions regarding the Vietnam War even while declaring that the administration had been a "colossal failure" on economic policy and criticizing its opposition (at that time) to wage-price controls (*Record-American* (Boston), June 14, 1971).

of the trends in foreign and defense policy under Nixon and Ford.[259]

Such praise contrasted with the tone of commentaries about Jackson made by Milton Friedman, who had very little good to say about the senator. Jackson's stance on world affairs and defense did not play a major role in shaping Friedman's opinion of Jackson. He had little reason to see Jackson's foreign policy positions as marking him out as especially notable or commendable: for, although Jackson was advocating more hawkish policies than those taken actual Republican administrations since 1972, so too were the Republican politicians with whom Friedman was most associated, Barry Goldwater and Ronald Reagan. Furthermore, these two leading Republicans were, of course, much closer to Friedman on economic matters than Jackson ever was. And, in any event, it was economic policy, rather than foreign and defense policy, which was Friedman's own area of expertise on which he was most often led to comment. With regard to economics, Jackson was unequivocally on the other side of the spectrum from Friedman, as Friedman would make clear through numerous trenchant remarks about the senator's statements and actions. The differences between the two came out most prominently in discussions of U.S. energy policy—a topic on which Jackson was particularly active.

"Obscene profits"

Jackson was extremely critical of the behavior of U.S. oil companies in the wake of the first oil shock, and during 1974 he alleged that the companies were making "obscene profits" at the expense of American citizens in the wake of the oil price increase (see, for example, *The Evansville Press* (Illinois), April 1, 1974; *Omaha World Herald* (Nebraska), December 3, 1974). The phrase became well known, and it prompted a response from Friedman. "It is absolutely disgraceful that you have had public servants, people like Senator Jackson, stand up and make statements that they must know to be without foundation," Friedman remarked in an interview (*Black Enterprise*, June 1974, pp. 118, 120). Friedman suggested that the higher an oil company's profits had been during the recent oil embargo, the more successful it had been able to supply oil to the U.S. market, and so the better it had been at providing fuel supplies to U.S. customers in the face of the embargo (*Black Enterprise*, June 1974, p. 120).

Friedman later alluded to Jackson when he referred in his *Newsweek* column (of January 27, 1975*a*) to "the demagogic attack on 'obscene profits.'" The same period also saw Friedman

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[259] The conservative Phoenix-based newspaper *Arizona Republic* likewise praised Jackson in an editorial (March 20, 1974) for having "talked a great deal of sense" in his criticisms of the Nixon Administration since early 1972 in the areas of defense and foreign policy issues.

criticize Jackson by name in his cassette-commentary series, when he referred to "Senator Jackson… and people of his ilk" (Instructional Dynamics Economics Cassette Tape 160, December 19, 1974) and the "demagoguery of Senator Jackson" (Instructional Dynamics Economics Cassette Tape 162, January 1975, Part 2).

His highest-profile attack on Jackson occurred in late May 1975, when Friedman took the opportunity of his recurring television slot on *CBS Morning News* to revisit Jackson's reference to "obscene profits." Friedman contrasted the situation prevailing a year earlier, when "Senator Jackson called hearings at which executives of oil companies were berated," with the general reaction to the most recent profits releases by major oil producers: "No front-page headlines this time; no Congressional hearings; only the news that earnings fell 14 percent for Shell, 44 percent for Getty, and 63 percent for Sun Oil Company."[260] While observing that "oil companies are by no means innocent angels; their managements have tried, like the rest of us, to make as much money as possible," Friedman stressed that the earnings results observed since the OPEC move had reflected market mechanisms: "The increase in profits last year was a signal that more oil was needed. It was also an incentive to companies to provide more oil… Profits were serving their proper function in a free-enterprise system."[261]

Friedman added regarding these profit results: "This year's decline is less justified than last year's rise. Oil is still expensive; more production is called for." In this connection, he pointed to price controls on petroleum products as holding down both oil production and oil companies' profits.[262] That price controls were making profits lower than otherwise was one thing on which he and Jackson likely agreed. But, for Jackson, this amounted to a *desirable* effect of the price controls. Indeed, as will be seen below, he favored using price controls even more aggressively by using them to force domestic oil prices down: "It is virtually impossible to justify today's fuel prices in the face of such profits," he had remarked in early 1974 to his Senate committee.[263] The suggestion to deploy federal government powers with the aim of restricting the energy sector's profits seemed to deliver political benefits to the senator. By early 1975, Jackson was among the front runners, and perhaps the favorite, in the contest regarding the 1976 Democratic nomination

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[260] At this point, Friedman briefly explained how the reported numbers presented "a misleading picture" because they were obtained using conventional percentage changes (rather than changes based on continuous compounding)—a problematic procedure if applied to variables can both decline and increase by substantial amounts.
[261] *CBS Morning News With Hughes Rudd*, May 26, 1975, p. 11 of transcript.
[262] *CBS Morning News With Hughes Rudd*, May 26, 1975, p. 11 of transcript.
[263] From Jackson's opening comments at the hearings on oil price rollback legislation, in Committee on Interior and Insular Affairs, U.S. Senate (1974, p. 2).

for president, for which he had announced his candidacy (*Evening Post* (Wellington, New Zealand), February 7, 1975).

Therefore, despite Friedman's opposition to the phrase, Jackson's reference to "obscene profits" had gained traction in political debate. Friedman in essence acknowledged this reality by coming back to the phrase in later years, including during a speech in early 1978 in which he noted that there had been "a great deal of talk about obscene profits."[264] The wish to curb oil company profits helped underpin political opposition to a freer market in energy in the United States. Jackson was a particularly prominent figure among the advocates of federal government intervention in the energy market—and in particular of continuation of controls on domestic petroleum prices.

The petroleum price control system

As discussed in Chapter 3, the controls were a legacy of the 1971−1974 Nixon price controls, but they had been continued beyond the 1974 lapse of the general controls program and so were inherited by President Ford. Five years after the start of the large world oil price increases of the 1970s, Friedman speculated that if those increases had occurred six months after the expiration of the Nixon controls, there might not have been the network of federal controls on petroleum prices and allocation seen in the Ford and Carter Administrations.[265] "We tend to forget that the price and wage control measures of 1971 bear a great deal of responsibility for our present oil problem," he remarked in this connection.[266]

The controls pertained to both crude oil prices paid to U.S. producers by oil refineries and the retail gasoline prices paid by U.S. motorists. However, of these two sets of price controls, the controls on domestically produced oil, not on ultimate gasoline prices, were crucial to the distortion engendered by the controls.

To be sure, retail gasoline prices were held down in the wake of the oil price explosion. They rose sharply: U.S. gasoline prices in the personal consumption expenditures index rose 19.6

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[264] *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, p. 18 of transcript. Also in Friedman (1983d, p. 149).
[265] *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, p. 17 of transcript. Herbert Stein, who was a member of the Nixon Administration during the onset of the 1973−1974 shock, later expressed the judgment that the U.S. oil price controls that lasted through 1981 would, indeed, never have existed had the first oil shock not occurred during a period of general price controls. See Stein (1988, p. 193).
[266] Friedman (1983d, p. 148). See also *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, p. 17 of transcript.

percent in 1973, 20.7 percent in 1974, and 11 percent in 1975 (Council of Economic Advisers, 2018, Table B−10, p. 543)—but their strong cumulative updrift over these years was insufficient to reflect the warranted increase in final product prices warranted by the move up in the world oil price.

Controls on prices at the pump, though present, were not, however, the decisive factor in producing this outcome. Certainly, the retailers were subject to important and far-reaching U.S. government controls: in particular, the Emergency Petroleum Allocation Act, passed in November 1973, assigned to the federal authorities the responsibility to decide how refined oil supplies would be allocated to service stations in the United States.[267] But mandatory controls on retail petroleum *prices*, though in force in this period, did not play a central role in the federal energy control process. This was partly because there were so many service stations in the United States that rigid enforcement of a control system at the retail level was impracticable.[268] Even more crucially, however, the controls on retail petroleum prices were essentially controls on *markups*. Retailers were, consequently, permitted to pass on increases in their wholesale costs and, in particular, cost rises associated with their purchases of refined petroleum products. This feature of the retail price controls meant that, as Secretary of the Treasury George Shultz put it in early 1974, "the control system in a sense rides on top of crude," and that even retail prices would tend to be artificially low even had they not been subject to formal controls.[269] The controls in the wholesale realm—controls on domestically produced crude oil and so on the prices faced by refineries for much of the oil product that they purchased—were, therefore, the centerpiece of U.S. oil price controls.[270]

Wholesale oil prices—specifically, the domestic price of crude oil produced within the United States—had been held constant during President Nixon's initial wage-price freeze. Following the freeze, the next major change permitted in the domestic prices of oil-based products was an

[267] See Yang (1977, p. 11) and the written submission by Robert E. Montgomery, Jr., and Gorman C. Smith, provided for the hearing of March 12, 1975, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1975a, p. 449). The allocations were based on historical sales patterns by region. See the testimony given by Edward J. Mitchell on May 31, 1979, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 467).
[268] See Edward J. Mitchell's testimony (May 31, 1979) in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 468).
[269] The quotation is from Shultz's testimony of February 4, 1974, in Committee on Ways and Means, U.S. House of Representatives (1974, p. 139).
[270] The prices charged by refineries to the retail dealers were also controlled, but refiners were allowed to pass cost increases into the prices charged to dealers on the refined product (Sweeney, 1977, p. 187)—a feature that pointed up the fact that the key price controls in the oil market were on what crude oil refiners were charged when purchasing domestically produced oil.

increase permitted as part of the January 1973 liberalization of the Nixon wage-price controls.[271] The surge in world oil prices in the mid-1973 meant that, even before the OPEC shock proper, rigid control of all domestic oil prices was untenable, and it was against this background that the two-tier oil price system was introduced in August 1973.[272] The two-tier system, which would last in its basic outline through 1981, assigned different prices to two main classes of oil produced in the United States: "old oil" and "new oil." As the price of "new oil"—an output concept encompassing (*i*) the production of preexisting oil fields that was in excess of 1972 levels and (*ii*) oil generated from those oil fields set up after 1972—was, initially, essentially not subject to control, it from 1973 onward was accordingly allowed to equal the new high world levels. Reflecting this, as of mid-1975 the new oil price was in the range of $11 to $13 per barrel. However, "old oil," defined as that amount of oil production that came from oil fields that were already in existence in 1972 and that did not exceed those fields' 1972 production levels, was subject to a strict ceiling, which as of mid-1975 was $5.25 per barrel.[273]

This arrangement obviously made "new oil" production far more profitable than "old oil" production. As might be expected, it gave rise to attempts to violate the rules—a phenomenon reflected in news reports of prosecutions of attempts by companies (producers or refiners) that had allegedly misrepresented old-oil production as new-oil production in the selling or reselling process (Sobel, 1982, pp. 79–81). But Friedman's great concern about price controls pertained to the adverse economic effects that occurred when controls were observed and enforced, rather than bypassed. The U.S. petroleum price controls of the 1970s were, in fact, adhered to on a large scale and did give rise to considerable distortions to price signals and production decisions.

The key distortion related to the discouragement of "old oil" production stemmed from the ceiling on its selling price. The very term "old oil" conveyed the impression that such oil was already produced before 1972 and was stockpiled or virtually ready to be shipped. Indeed, opponents of a deregulated old-oil price came to describe the profits that would arise from sales of old-oil product at world prices as "inventory profits." In fact, however, "old oil" was a flow of new production. Only the oil fields that were the source of that oil were "old," as they were already in existence before 1973. In addition, although, as a figure in the energy sector noted, discussion of oil production was sometimes spoken of as though an oil field was "some sort of

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[271] See George Shultz's testimony of February 4, 1974, in Committee on Ways and Means, U.S. House of Representatives (1974, p. 128).
[272] Again, see George Shultz's testimony of February 4, 1974, in Committee on Ways and Means, U.S. House of Representatives (1974, p. 128).
[273] See the testimony of William A. Johnson on July 10, 1975, in Committee on Finance, U.S. Senate (1975a, p. 224). As discussed below, price controls on new oil were introduced into U.S. law late in 1975.

cavern"—so that, once an oil field was set up, it was a trifling matter to pump oil out of it—the process of generating increments of oil output from an existing oil field could be cumbersome.[274] In particular, it was not necessarily deemed worthwhile by producers in the presence of the old-oil price ceiling.

William Johnson, an economics professor and a former U.S. government energy official, testified on some of the problems in Congressional testimony in 1975: "Even though the two-tier system has provided ample incentives for new drilling, it has, at the same time, discouraged production from existing wells. First, it frequently does not pay to employ secondary and, especially, tertiary methods of recovery of oil sold at $5.25 per barrel… Second, and perhaps worse, there is an incentive under the two-tier price system to shut in and plug old wells prematurely."[275] There were some elements of flexibility in the two-tier system: for example, production in excess of the 1972 level at old-oil wells was encouraged not only by it being able to be sold at the new-oil price but by the fact that, upon exceeding their 1972 output level, oil producers were permitted to reclassify a percentage of their "old oil" production as new oil.[276] But there were also elements of rigidity: for example, a newly opened oil well on an old oil field was classified as generating old oil, provided that aggregate oil production on the field was below the field's 1972 level.[277] Indeed, in the face of the production disincentives facing "old oil" producers under the two-tier price system, one option some of them reportedly exercised to increase profits involved drastically cutting back their production: scaling back operations so much that their regulatory status changed to being operators of small, or "stripper," wells—which were not outside the price-control arrangements altogether.[278]

Friedman's position, in contrast to that taken by the authorities, was that "there is no justification for distinguishing between new oil and old oil."[279] In his view, the pricing of all oil production in the United States should have been free of government controls. Even if, as the advocates of price controls sometimes seemed to imply, old oil was tantamount to a preexisting inventory of

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[274] The quotation is from the testimony of May 16, 1979, of Charles J. DiBona (of the American Petroleum Institute), in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 138).
[275] From the testimony of William A. Johnson on July 10, 1975, in Committee on Finance, U.S. Senate (1975a, p. 225).
[276] See the written statement by Herbert S. Richey for the hearing of July 11, 1975, in Committee on Finance, U.S. Senate (1975a, p. 287).
[277] See the testimony of Secretary of Commerce Rogers Morton given on July 10, 1975, in Joint Economic Committee (1975f, p. 32).
[278] See Sweeney (1977, pp. 183–184) and the written statement by Arthur W. Winter of Pasco, Inc., for the hearing of March 12, 1975, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1975a, pp. 488–489).
[279] Friedman (1977k, p. 494).

already-produced oil, Friedman would have been well disposed toward a situation in which prices were unregulated and oil producers were able to reap the proceeds of an appreciation in the market value of their holdings of oil.[280] But, as "old oil" was itself a flow of new output, the case for a free oil price was stronger, as it rested on the need to prevent oil production from being discouraged.[281] In the event of removal of the control on the old-oil price, Friedman observed, owners "of old oil [properties] would have an incentive to exploit their wells more fully and [therefore] to get more old oil." (Instructional Dynamics Economics Cassette Tape 165, February 1975, Part 3.)

The notion that old-oil output was being held back by price controls was partially conceded by Senator Jackson in 1974: "I'm not so sure that the old oil is being pumped out as fast as it could be, because I think a lot of the companies are sitting on it, hoping that the price will double."[282] A related perspective was given in mid-1975 by Charles Schultze of the Brookings Institution. Later, in the second half of his period as CEA chair in the Carter Administration), Schultze would become a strong proponent of old-oil price deregulation (usually called "decontrol" in U.S. debate at the time). But, as of the mid-1970s, he was lukewarm concerning the merits of decontrol. Schultze nevertheless saw the specter of decontrol as an important factor affecting old-oil supply. Citing a hypothetical example of an oil field that was producing 70 percent of its 1972 output, Schultze noted that the company running the field had little incentive to take measures that would raise production by up to 30 percent because all the oil thereby generated would have to be sold at old-oil prices: "you read in the newspapers that old oil is going to be decontrolled [in the future], so you hold back [until then]."[283]

Neither Jackson nor Schultze acknowledged in these remarks the likelihood that the controls were, in many cases, making it *unprofitable* to extract more oil from old-oil fields. Their statements amounted only to a concession that oil companies might be making a calculation that it would be *more* profitable to postpone such extraction. These sketches of a restraining effect of controls on oil production were therefore not founded on quite the same basis as Friedman's

[280] See the January 1974 Friedman remarks quoted in Section III of Chapter 3 above.
[281] In this connection, the submission of Arthur Winter in March 1975, already mentioned, had stressed: "And yet most petroleum engineers believe that there are hundreds of millions of barrels of crude oil in *already known* and partially depleted fields which could be recovered if the economics justified the secondary and tertiary recovery techniques now available." (Committee on Interstate and Foreign Commerce, U.S. House of Representatives, 1975a, p. 489.)
[282] In Mitchell (1974, p. 92). Jackson was speaking on the second day of an American Enterprise Institute conference held in Washington, D.C., on October 3–4, 1974 (Mitchell, 1974, p. 1; *Dallas Morning News*, October 5, 1974).
[283] From Schultze's testimony of July 14, 1975, in Joint Economic Committee (1975f, p. 85).

belief in this effect. Like many other critics of control, Friedman regarded old-oil production as capable of being viewed in terms of standard economics: an upward-sloping supply function, with the output generated being a continuous function of the price offered for it. It followed that an artificially-imposed price ceiling implied lower U.S. oil output than otherwise and that decontrol provided the means of stimulating U.S. production. Jackson's different way of seeing old-oil production decisions meant that he, in contrast, felt that U.S. policy solutions lay not in decontrol but instead in *dispelling* beliefs that a move of old-oil prices to the current world price would be permitted. The possibility of permanent price controls also heightened the prospect of future regulations that circumscribed producers' prerogatives over quantities supplied by, in effect, compelling them to offer oil at a single-digit dollar selling price.

Jackson also quickly established himself, in the wake of the 1973−1974 oil shock, as an advocate of making the domestic price of *new* oil a controlled price. He told a Senate committee hearing on January 31, 1974: "we start from that premise—that we need to roll back the price."[284]

The first oil shock becomes permanent

It became increasingly clear during 1975 and 1976 that it would not be the world oil market that would produce an oil price below $10 a barrel. For it was in this period, particularly from the second half of 1975 onward, that the widespread realization set in that the OPEC oil price increase of 1973−1974 that had pushed the price to $10 or above had, for the moment, stuck. Reflecting this situation, Council of the Economic Advisers chair Alan Greenspan contended in July 1975 that it was an "illusion" to proceed on the premise that world oil prices would move into a lower range in the short or medium term.[285]

The posture Greenspan articulated represented a divergence from opinions that had been expressed at the official level in the early months after the oil shock. For example, in February 1974, when the OPEC price increase had only recently been put in place, Secretary of Treasury Shultz had conceded that there was some permanent element to the increase but predicted that the world oil price would start going down and settle in at about $7 per barrel in the coming years.[286] Greenspan's outlook also contrasted with prominent views previously expressed outside the government. For example, Harvard University's Hendrik Houthakker had stated in October

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[284] In Committee on Interior and Insular Affairs, U.S. Senate (1974, p. 44).
[285] From Greenspan's testimony of July 10, 1975, Joint Economic Committee (1975f, p. 9). For more on views prevailing in 1975 on this matter, see Chapter 7 below.
[286] See George Shultz's testimony of February 4, 1974, in Committee on Ways and Means, U.S. House of Representatives (1974, pp. 135, 189).

1974: "Last February, I stuck my neck out and said that the price of oil would come down within two years. I still believe that is a good guess."[287] But at the end of 1974, the world oil price was still at its new post-OPEC shock new high level, and the July 1975 hearing (Joint Economic Committee, 1975f) at which the aforementioned Greenspan remark was delivered was actually concerned with the possibility of a "forthcoming OPEC price rise." This scenario was realized, to some extent, when OPEC raised the world oil price by 10 percent in the fourth quarter of 1975 to restore some of the recent loss in the real price—a move that raised the world price of crude petroleum from $10.46 to $11.51 per barrel (Sobel, 1977, p. 123; International Monetary Fund, 1976, p. 144).

Friedman, of course, had been one of the most outspoken among those forecasting an oil-price decline. Taking stock in his *Newsweek* column in February 1975, he stated that the OPEC collapse that he had predicted over the previous seventeen months had been "taking longer than I expected, partly because of the ill-advised policies we have been following under the demagogic influence of Senator Jackson and some of his colleagues." Friedman nevertheless maintained: "Almost regardless of our energy policy, the OPEC oil cartel will break down. That is assured by a worldwide reduction in crude-oil consumption and expansion in alternative supplies in response to high prices. The only question is how long it will take… But the chances are good that… the cartel will begin to disintegrate by 1976 and crude-oil prices will start to tumble." (*Newsweek*, February 17, 1975.)

Furthermore, during early 1975, and notwithstanding the resilience of the oil price, there were notable expressions of confidence arising from other quarters to the effect that the pressures making for an unwinding of the first oil shock were indeed sizable. With regard to demand conditions, Paul Samuelson remarked (*Newsweek*, March 24, 1975) that "the OPEC monopoly… has already set in motion strong forces making for lessened energy use." On the supply side, Richard Mancke, in an article, "The Future of OPEC," appearing at the start of 1975 in the University of Chicago business school's policy/research periodical, *Journal of Business*, did not share Friedman's confidence regarding a decisive OPEC breakdown. Mancke was, nevertheless, reasonably optimistic: "Because of the large and politically secure new oil supplies that are just now starting to come on stream, it is likely that OPEC's monopoly power has already reached its zenith and will soon begin to ebb."[288]

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[287] In Mitchell (1974, p. 13).
[288] See Mancke (1975, p. 18). In the same passage, Mancke suggested that "even if OPEC retains all of its present monopoly power, it appears unlikely that its members would take measures causing world oil prices to rise much above current real levels." This prediction did hold good until the first half of 1979.

Such optimistic perspectives were not borne out. That they were not was reflected in the fact that the oil price did not retreat in 1975 and 1976—indeed, as already indicated, it rose in 1975:Q4. And on December 16, 1976, OPEC mandated a further price increase of about 5 percent (*Springfield Leader and Press* (Missouri), December 26, 1976).

On both the supply side and the demand side, the strength of the forces that would wind back OPEC's oil-price rise had been considerably overestimated. On the supply end, from 1973 to 1977 OPEC's share of non-Communist countries' crude oil production barely fell, from 64.7 percent to 64.4 percent.[289] A major reason for the weakness of the rise from 35.3 to 35.6 percent in the non-OPEC non-Communist economies' total was a sharp absolute decline in the absolute volume of crude oil produced in North America—from 13.1 million barrels per day in 1973, to 11.4 million in 1977.[290]

The decline in the U.S. contribution to this North American production aggregate was something that soon became manifest in the wake of the 1973−1974 oil price increase. Testifying in July 1975, Frank G. Zarb, the Administrator of President Ford's Federal Energy Administration, observed of oil, "what is happening [is] that our imports are increasing… Our domestic production is declining."[291] Nearly a year later, Eric Zausner, the FEA's Deputy Administrator, at the May 1976 Washington D.C. conference on deregulation for which Friedman was also a guest speaker, noted the "rather dismal supply statistics" prevailing with regard to energy: "we are in substantially worse shape" than before October 1973. Among the unfavorable developments catalogued by Zausner: "Oil production is down over a million barrels a day…"[292]

When oil production had fallen in the first year after the oil shock, Senator Jackson cited it as a vindication of his opposition to liberalization of prices: "It has been argued that there should be no price ceiling on new oil so that new discoveries of oil would be stimulated… As you know... it just hasn't worked out that way."[293] Jackson thus suggested that, as new-oil supply had not turned up, the supply function was not price-elastic. This inference neglected the fact that the course of new-oil supplies was already set to decline in the mid-1970s, notwithstanding the higher world price of oil, because of a major downturn in U.S. oil exploration that had occurred

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[289] Based on the numbers given in Houthakker (1980, p. 343).
[290] Houthakker (1980, p. 343).
[291] From Zarb's testimony of July 10, 1975, in Joint Economic Committee (1975f, p. 11).
[292] In National Center for Productivity and Quality of Life (1976, p. 1 of the conference proceedings' section on energy).
[293] In Mitchell (1974, p. 96).

in around 1970.[294] Against that backdrop, it was old oil—which accounted for about 66 percent of domestically-produced oil, and 40 percent of all oil consumed in the United States—whose output would need to pick up strongly in order for aggregate U.S. oil production to rise after 1973.[295] Instead, an energy official noted in July 1975, "The production o[f] old oil has declined slightly over the last year."[296] To Friedman, it was clear that the price ceiling on old oil was the culprit—old oil needed the "production incentive [arising] from higher prices" (*Newsweek*, January 27, 1975*a*, p. 24).

"Subsidizing OPEC oil"

The underpricing of domestic oil in the United States was also a major factor slowing down the response on the consumption side. As discussed in Chapter 3 above, one point Friedman highlighted when looking at cross-country responses to the oil shock was that the United States had not witnessed the decline in demand experienced elsewhere. As an energy official recalled in 1979, Japan and the Federal Republic of Germany "went cold turkey in the pricing [that is, allowed their domestic economy's energy prices to reflect the oil shock fully], and we did not."[297] Even the United Kingdom, which responded to the initial 1973 oil event through quantitative controls (mandated reductions in household and businesses' calls on energy) soon removed those controls and allowed the oil-price increase to be felt fully in domestic wholesale and retail prices. The United States was a rare instance in which the government tried to stop the oil shock from being fully felt by home users. In light of this situation, energy consumption per capita in the United States was in 1975 actually slightly higher than its value in 1970.[298] And in 1976, retail gasoline consumption moved above its 1976 level (*Springfield Leader and Press* (Missouri), December 26, 1976).

As of early 1975, Friedman initially did not appreciate the extent to which gasoline demand

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[294] See the testimony of Bruce A. Pasternack (Associate Administrator, Federal Energy Administration) of July 11, 1975, in Committee on Finance, U.S. Senate (1975a, p. 442). There was actually some sign of a pickup in exploration for new oil in the United States during 1974 and 1975, following the first oil shock. See Sobel (1977, p. 9).

[295] The 66 percent share of U.S. production was given by Herbert S Richey in a written statement for the hearing of July 11, 1975, in Committee on Finance, U.S. Senate (1975a, p. 286). The 40 percent share of overall U.S. consumption was given by William Johnson in his written statement for the hearing of July 10, 1975, in Committee on Finance, U.S. Senate (1975a, p. 224), as well as by Friedman in Instructional Dynamics Economics Cassette Tape 168 (end-May 1975).

[296] Testimony of Bruce A. Pasternack, July 11, 1975, in Committee on Finance, U.S. Senate (1975a, p. 441).

[297] Testimony of May 16, 1979, given by John J. O'Leary (Deputy secretary in the Department of Energy in the Carter Administration), in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 173).

[298] See "Table 1.7 Primary Energy Consumption, Energy Expenditures, and Carbon Dioxide Emissions Indicators," U.S. Energy Information Administration, *https://www.eia.gov/totalenergy/data/monthly/pdf/sec1_19.pdf*.

reflected artificially low retail prices. As noted above, retail price control was not central to the U.S. energy price controls. Furthermore, as Friedman noted, that there had not been gasoline shortages (manifested in lines at service stations) since the end of the 1973–1974 embargo. On the basis of these facts, Friedman correctly inferred that, for retail gasoline, "the price had been clearing the market." But he incorrectly made the further inference that the price control on old oil must have led to offsetting actions by refiners, further along the gasoline-production chain, in the form of increases in markups—so that oil-price decontrol, though it would raise crude oil prices, would not raise retail petroleum prices (*Newsweek*, May 12, 1975).[299]

Friedman soon backtracked on this prediction and acknowledged that retail and wholesale petroleum prices would both go up, initially, if decontrol occurred (Instructional Dynamics Economics Cassette Tape 168, end-May 1975; *Newsweek*, June 23, 1975). The change in his prediction reflected in part an obvious point clearly inadequately emphasized in his initial analysis: the fact that oil imports were a factor ensuring that pervasive shortages in the retail market were not implied by U.S. petroleum price controls.[300] Importantly, however, it also reflected an increase in Friedman's familiarity with the overall setup of oil controls. This improved understanding was in part due to a dialogue Friedman had with his former student, Michael Canes, who had become an economist at the American Petroleum Institute: "I got in touch with him and described some of the features of the program to him" (Michael Canes, interview, November 7, 2013).

In the corrected analysis given in his June 1975 *Newsweek* column, Friedman now appreciated the significance of the "entitlements" system, introduced in late 1974 in the United States wholesale market for petroleum. This was a system under which the Federal Energy Administration required oil refiners that had disproportionate access to old oil to make payments to refiners that relied more heavily on market-priced oil (such as new oil and imported oil). The result was to spread the underpricing of crude oil, so that it was felt in all the units of refined petroleum products that sellers of retail gasoline purchased.[301] Although it remained true that, as

[299] Similarly, around the same time, when updating his *Price Theory* text, Friedman implied that the energy-price increase had already been relayed in full to U.S. consumers and that the subdued initial adjustment of U.S. energy consumption to the OPEC shock largely reflected the difference between the short-run elasticity and the (much larger) long-run elasticity of the demand for energy. See Friedman (1976b, p. 160).

[300] He had, of course, written the column with the knowledge that imports helped equilibrate gasoline demand and supply in the United States. But Friedman presumably had (incorrectly) taken for granted that the cost involved in importing petroleum products was appropriately reflected in the retail price of gasoline.

[301] Michael Canes described the entitlements system as in effect implying that the situation facing an oil refiner was that "for every barrel you *imported*, you got a certain fraction, perhaps, of a barrel of old oil." Canes recalled that, in talking to Friedman about the U.S. government's system of controls on energy, the entitlements program "was the feature that I think I really educated him on, as we talked our way through how this thing worked and in the column,

Friedman had put it in his earlier column, permitting a "rise in the price of old oil would also give producers an incentive to produce more oil" (*Newsweek*, May 12, 1975), he now understood that high retail gasoline prices were, indeed, artificially low and higher prices would be one of the likely immediate implications of oil-price deregulation.[302]

As in his discussion of rent control in the 1940s, however, Friedman still believed that a decontrol action that raised prices to be desirable because the quantity supplied (specifically, the amount produced in the United States) would also go up. Furthermore, it remained true that price controls were not protecting the country as a whole from the higher cost of oil implied by the OPEC shock. Indeed, expenditure in the United States on energy items rose as a share of nominal GDP from 7.7 percent to 10.2 percent from 1970 to 1975, reflecting this extra cost.[303]

What the controls were instead doing was failing to relay this shock fully as a price signal to gasoline purchasers. Indeed, Friedman titled his column giving his corrected analysis "Subsidizing OPEC Oil," because his increased knowledge of what Stein (1988, p. 193) would call the "myriad of auxiliary regulations" accompanying oil-price controls had increased Friedman's appreciation of the extent to which the energy policy framework currently in place was encouraging U.S. importation of petroleum.[304] He had already observed that control had raised "the real costs of the energy crisis to the U.S. citizen" (*Newsweek*, February 17, 1975), but the confirmation that consumption of imported gasoline was being stimulated by the entitlements scheme underscored this conclusion: "it gave him an even more persuasive line of argument to take," Michael Canes noted (interview, November 7, 2013).[305] Friedman's increased knowledge

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one of the things he said, as I recall, was something like: The program is even worse than I understood it—you know, they have this feature in it that's making the problem worse—that's exacerbating the problem." (Michael Canes, interview, November 7, 2013.) Mitchell (1977, p. 284) pithily described the mechanics of the entitlements program as involving "some refiners writing out checks to the FEA which are then turned over to other refiners," although he also noted that the net effect of entitlements, when operating in conjunction with the price control on domestic crude oil, did not amount merely to a reshuffling of funds among refiners but, rather, was material in its character, in lowering the overall cost to refiners of importing oil by about $2.50 to $3 per barrel (p. 283).

[302] Mitchell (1977, pp. 283–284) made a contention closer to Friedman's original position, that is, that crude oil prices but not retail gasoline prices were artificially held down by the controls. But Mitchell's basis for this conclusion seemed slender. The formal analysis of Cox and Wright (1978) concluded (see their page 14) that the crude-oil price controls did hold down gasoline prices.

[303] See "Table 1.7 Primary Energy Consumption, Energy Expenditures, and Carbon Dioxide Emissions Indicators," U.S. Energy Information Administration, *https://www.eia.gov/totalenergy/data/monthly/pdf/sec1_19.pdf*.

[304] Although Cox and Wright (1978, p. 1), in the preamble of their own formal analysis of U.S. energy policy, categorized Friedman's May 1975 *Newsweek* treatment as among the "general, rather simplified treatments of the issue," their own conclusion—"the entitlements program provides a subsidy to imported crude oil" (p. 14)—matched that of his column.

[305] This aspect of the entitlements program was also remarked on in a submission by William Johnson prepared for a July 10, 1975, Congressional hearing: "There is a strong incentive under the entitlements program for companies to shut-in production of domestic old oil and to run imported oil instead. The entitlements program, in effect,

of federal energy policy therefore reinforced sentiments that he had already expressed. They also gave him an additional grievance against what he called "that bureaucratic monstrosity, the Federal Energy Administration," which had produced the entitlements program in the early months of the Ford Administration.[306] He remarked that, while "I know that the FEA is not in fact being run by secret agents of the Shah [of Iran] and sheiks" (*Newsweek*, June 23, 1975), the system the FEA instituted had had effects that were beneficial to oil producers outside the United States, by subsidizing U.S. consumption and discouraging domestic production.[307] Indeed, in March 1976, nine months after Friedman's column, the United States' imports of oil exceeded the nation's production of oil for the first time (Sobel, 1977, p. 3).

Rollback versus decontrol

The Federal Energy Administration was indeed the agency primarily responsible for implementing the U.S. energy policy framework of which Friedman was so critical. But in pointing the blame at the FEA and therefore at the executive branch of the U.S. government, Friedman was eliding the fact that, in the national policy debate on energy, the Ford Administration was—until late 1975—in favor of prompt decontrol of old-oil prices and the corresponding installation of a market-based, single-tier price system regarding crude oil.

In fact, in making the case for oil-price decontrol over much of 1975, administration officials cited the adverse effects—on adjustment, efficiency, and incentives—implied by the country's existing energy arrangements, and they articulated various points on this matter that Friedman himself had voiced. For example, Secretary of the Treasury William Simon testified in early 1975, "The President has proposed… that we reduce consumption of oil through the most neutral and least bureaucratic system available—through the price system. The [Ford Administration's] energy proposals would raise the price of oil."[308] The boost to production was also highlighted in

subsidizes imports." (In Committee on Finance, U.S. Senate, 1975a, p. 224.) An acknowledgment at the official level of effects of this kind came when, in congressional testimony given on March 12, 1975, the Federal Energy Administration's Robert E. Montgomery, Jr., stated: "The crude oil entitlements… to some degree have the undesirable effect of encouraging imports, since the burden of their higher cost is not borne solely by the importer, but shared with his competitors." (In Committee on Interstate and Foreign Commerce, U.S. House of Representatives, 1975a, p. 443.) For tabular and diagrammatic elaborations of these effects of the entitlements program, see Helbling and Turley (1975, pp. 3−5) and Yang (1977, pp. 10−11).

[306] The quotation is from *Newsweek*, September 15, 1975. The entitlements program had been announced and instituted during 1974, in the late-November and early-December time-frame (*Chicago Metro News*, November 23, 1974; *Dallas Morning News*, November 30 and December 3, 1974).

[307] For a later Friedman remark of this kind, see Feldstein (1980, p. 95). This 1980 comment was made in the wake of four-and-a-half more years of controls-oriented energy policy.

[308] From Simon's statement to the Committee on Ways and Means, U.S. House of Representatives on January 22, 1975, reproduced as a submission in his subsequent testimony of March 3, 1975, in Committee on Ways and Means,

the U.S. government's case for decontrol: for example, FEA Administrator Frank Zarb testified in July 1975 that "industry will be given an impetus to increase the production of our own supplies of petroleum as domestic oil prices are permitted to rise."[309]

In 1974 and 1975, it was instead the legislative branch that was leading the advocacy within the U.S. government of oil-price controls. Here, Senator Jackson was a major. Not only did Jackson believe in continued domestic crude oil price control—he also wanted, as already indicated, *intensified* control. In January-February 1974, Jackson chaired Congressional committee hearings on proposed "Oil Price Rollback Legislation" (Committee on Interior and Insular Affairs, U.S. Senate, 1974). "Rollback" meant imposing controls on the new-oil price and lowering that price below the world level.

The Nixon and Ford Administrations opposed rollback: for example, Secretary of the Treasury Shultz observed in February 1974: "It would be a fundamental mistake—for everyone except foreign oil producers—to roll back oil prices to some former level."[310] But Jackson continued to push for rollback in the context of permanent oil-price controls. Jackson secured passage of a bill to have the new-oil price lowered below $8 per barrel, but President Ford then vetoed the bill (Mitchell, 1974, p. 91). Later, in October 1974, Jackson suggested imposing a price ceiling for new oil of $7.09 a barrel. Such a move would have brought the price down by about four dollars, but, Jackson asserted, it would have left the price well above that required "to provide the incentive to get new oil" (Mitchell, 1974, p. 96). Secretary Shultz had warned that the "mere presence of ceilings of any sort will tend to dampen the new investment required to produce the increased oil we need."[311] Indeed, the specter of new oil being brought into a very strict price-control system was cited by some economists as a factor that, during the middle and late 1970s, was holding back expanded private-sector investment expenditures bearing on the exploration for new oil (see Kydland and Prescott, 1977, p. 486, and Committee on Policy Optimisation, 1978, p. 78).

U.S. House of Representatives (1975, p. 28). Similarly, in outlining a new set of energy proposals in July 1975, Simon testified: "Decontrolling prices and eliminating allocations are, perhaps, the most important parts of the President's program." (Testimony of July 14, 1975, in Committee on Finance, U.S. Senate, 1975a, p. 370.)

[309] From Frank Zarb's prepared testimony of July 10, 1975, in Joint Economic Committee (1975f, p. 14). In his own prepared testimony for the same hearing, Secretary of Commerce Rogers C.B. Morton stated (p. 23) that "decontrol is required in order to stimulate domestic production and check demand growth."

[310] From Shultz's testimony of February 4, 1974, in Committee on Ways and Means, U.S. House of Representatives, 1974, p. 138).

[311] Again, see Shultz's testimony of February 4, 1974, in Committee on Ways and Means, U.S. House of Representatives, 1974, p. 138).

Friedman recognized the importance of the legislative branch as a force agitating for the continuation of price controls: "Congress still seems to behave as if it is in the pay of the Arabs, though of course it is not." (Instructional Dynamics Economics Cassette Tape 161, January 1, 1975). And, as already indicated, he was particularly critical of the stand on energy taken by Senator Jackson.

Friedman nevertheless found considerable fault with the Ford Administration on the matter of petroleum-price controls. He did not regard the administration as wholeheartedly in favor of oil-price deregulation and was critical of its approach on two points in particular. First, as had also been the case during the later months of the Nixon Administration, Friedman opposed the Ford Administration's position that special new taxes on the oil industry were a *quid pro quo* of price decontrol—an approach captured in Zarb's remark that "we are going to affect the price increase by two means: One, taxes; two, decontrol."[312] As discussed in Chapter 3, Friedman opposed proposals in which oil-price decontrol was necessarily accompanied by new energy-sector taxes, because he felt that such additional taxes might have too detrimental an effect on the incentives that were required to generate substantial extra production. The tax aspects of the energy proposals of successive administrations during the 1970s, and Friedman's stand on them, are discussed further in later chapters.[313]

The second area in which Friedman was critical of the Ford Administration's decontrol position has already been indicated: he felt that the executive branch had been too willing to make an accommodation with Congress on the matter of oil-price controls. As of the start of 1975, President Ford actually possessed the legal power to lift crude-oil price controls outright: as Friedman remarked (Instructional Dynamics Economics Cassette Tape 162, mid-January 1975): "He [Ford] can also administratively decontrol [the price of] old oil."[314] Indeed, as of early 1975 the president was poised to enact full oil-price deregulation: "Ford originally wanted immediate decontrol," the Associated Press would note (*The Spokesman-Review* (Spokane, Washington),

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[312] From Zarb's testimony of March 10, 1975, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1975a, p. 179). As the Ford Administration primarily favored windfall taxes on profits rather than gasoline taxes, its tax proposal was mainly a device for rerouting the sales receipts generated by higher prices rather than a means of increasing the price. In February 1975, President Ford put the matter as follows: "I have requested the Congress to enact a tax on producers of domestic crude oil to prevent windfall profits as a result of price decontrol." (In Council of Economic Advisers, 1975, p. 6.)
[313] See also the detailed study of this matter by Knittel (2014).
[314] Ford had this power under the Emergency Petroleum Allocation Act of 1973. See the prepared testimony of Rogers C.B. Morton, for the hearing of July 10, 1975, in Joint Economic Committee (1975f, p. 23).

November 15, 1975).[315] The president planned to issue a decontrol order by April 1 of that year—only to retreat from taking this step, in favor of making an arrangement with Congress on U.S. energy policy.[316] Friedman, who had already viewed Ford's oil-tax proposals as being unduly influenced by Jackson's attacks on oil profits, regarded the president's move away from prompt decontrol as a further, and undue, concession to Jackson and other members of Congress.

Ford's advocacy of decontrol over the course of 1975 confirmed that he had made a substantial compromise: in place of his original proposal of instant price deregulation, Ford's July 1975 proposal to Congress instead involved "phased decontrol," under which the old-oil price ceiling would be lifted slowly but not fully removed until late 1977.[317] As proposing phased decontrol amounted to an acquiescence to a prolonged period of continued price control, Friedman was extremely dissatisfied with this shift.

One of the factors buttressing opposition to prompt decontrol was that many economists in the mid-1970s looked somewhat favorably at the prospect of continuation of controls.[318] Certainly, some economists outside the administration joined Friedman in favoring a rapid move to market pricing of energy. Most notably, Kenneth Arrow was, like Friedman, an eminent advocate of prompt price decontrol.[319] And Michael Mussa, then a member of the University of Chicago's business school, remarked at an April 1975 conference sponsored by the university that the "two-part, old/new price of oil" was in the process of "creating very substantial distortion[s] of economic incentives and allocation of resources."[320]

Numerous prominent Keynesian macroeconomists, however, opposed rapid decontrol. Their difference from Friedman in analyzing inflation mattered importantly here, because their opposition was especially based on the grounds that—according to the cost-push reasoning to

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[315] For example, Ford had stated in the February 1975 *Economic Report of the President* that, in order that full "use of the price mechanism" was made in response to the United States' energy problems, he favored "decontrol of the price of crude oil." (In Council of Economic Advisers, 1975, p. 6.)
[316] Again, see the prepared testimony of Rogers C.B. Morton, for the hearing of July 10, 1975, in Joint Economic Committee (1975f, p. 23).
[317] See Frank Zarb's prepared testimony of July 10, 1975, in Joint Economic Committee (1975f, p. 14), as well as his spoken testimony on page 11 of the same hearings volume.
[318] Although at the time this opposition was portrayed in the media as being a stance against energy taxes (see especially *Detroit Free Press*, January 31, 1975), as will be seen, much of the opposition went deeper and amounted to any measures that allowed substantial short-term increases in domestic energy prices.
[319] See Chapter 3 as well as subsequent chapters.
[320] From Mussa's remarks in Birnbaum and Laffer (1976, pp. 144, 146). In contrast, in the retrospective on the period in Mussa (1994, p. 87) overlooked the fact of domestic oil price controls in the United States in the 1970s and so referred to the "nearly complete absorption of the price level effects of the increase in world energy prices by early 1975." This statement was correct only if the "effects" referred to were the effects allowed under the price-control system.

which they subscribed—raising domestic energy prices was inflationary in its own right and might, furthermore, trigger a surge in wages.[321]

In this vein, George Perry warned against "the inflationary effect of the price increases that would come from the Administration's energy proposals…. [B]ringing down the international price of oil is a very desirable goal, [but] increasing energy prices in the United States is an extremely costly and unpromising way of trying to achieve it."[322] Walter Heller likened the early-1975 "Ford program for abrupt decontrol" to "hitting the economy with a huge new oil price shock all at once," and he contended that prompt decontrol would worsen both inflation and real economic activity.[323] In March 1975, Arthur Okun criticized the "ripples into wages or other costs" implied by the Ford Administration's decontrol proposal—and indicated that he would be "perfectly happy to live with essentially a two-price system over a period as long as eight years."[324] Paul Davidson testified the following July that "in no case should [the] wellhead price of oil... be suddenly decontrolled."[325] Charles Schultze stated that "I do not believe old oil should be decontrolled," although he later qualified this statement by observing, "I would gradually deregulate old oil, very gradually."[326]

As for Paul Samuelson, Friedman would take him as believing that oil-price decontrol was economically desirable but politically infeasible.[327] This characterization was, however, not accurate. Samuelson did suggest that heavy federal government intervention in the allocation and pricing of energy was likely to be a politically mandatory response to OPEC's actions, because of the possibility that the free-market alternative implied substantial cross-country

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[321] Recall from Chapter 5 that fears of wage-driven follow-on inflation were highly prevalent among Keynesian economists in the aftermath of the first oil shock.
[322] From the prepared statement issued as an addendum to Perry's testimony of February 18, 1975, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1975c, p. 356).
[323] Heller (1976, pp. 100, 105). Heller (1976, p. 100) implied that he would not be averse to the removal of the oil-price controls in some future year. For the medium term, however, he favored instead quotas on oil imports and a phased increase in gasoline tax (Heller, 1976a, p. 105).
[324] From the prepared and spoken portions, respectively, of Okun's testimony of March 4, 1975, in Committee on Ways and Means, U.S. House of Representatives (1975, pp. 299, 321). Notably, however, and unlike many commentators (including many advocates of decontrol), Okun opposed a windfall profits tax because it would discourage oil production (p. 299).
[325] Testimony by Davidson, July 11, 1975, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1975b, p. 155).
[326] The quotations are respectively from Schultze's testimony of March 4, 1975, in Committee on Ways and Means, U.S. House of Representatives (1975, p. 311), and his testimony of July 14, 1975, in Joint Economic Committee (1975f, p. 88).
[327] *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, p. 18 of transcript.

differences in the retail price of fuel.[328] But, over and above this, Samuelson actually saw *economic* merit in the federal government's energy control system of the 1970s. He did favor changing it in a manner that made U.S. businesses and consumers face energy costs commensurate with the new world levels (*Newsweek*, January 27, 1975*b*). But Samuelson viewed the means of achieving this change as primarily via the imposition of additional retail taxes—and not through ending, through decontrol, the two-tier price system for crude oil. Indeed—taking a stand that even the Carter Administration in 1979 would repudiate—Samuelson contended that there was very little disincentive to old-oil production associated with the two-tier price system—a system that he suggested had actually "worked out pretty well." Samuelson contended that the fact that the domestic price of new oil reflected the market price, when taken in combination with "all of these gadgets and gimmicks" that had been added to the federal energy regulations on an *ad hoc* basis, generally provided sufficient incentive to U.S. producers (Instructional Dynamics Economics Cassette Tape 187 (Paul Samuelson series), September 1975, Part 2).

In the event, by the end of 1975, changes to U.S. law implied that even the new oil price was no longer a fully market-determined price. President Ford—whose revised phased-decontrol proposal had been defeated in Congress in July (Knittel, 2014, p. 117; Sobel, 1977, p. 69)—signed, on December 22, 1975, the Energy Price and Conservation Act of 1975.[329] His doing so amounted to a major capitulation on the president's part to Congressional sentiment favoring ongoing oil-price controls. William Simon would assess that, in what he considered "the worst error" the president made during his time in office, "Ford caved in," in an attempt to forestall still more interventionist energy legislation (Simon, 1978, p. 86).

Senator Jackson's long advocacy of rollback had recently been defeated when, on July 21, Ford had vetoed legislation that rolled back the new oil price from about $13 to $11.28 (Sobel, 1977, p. 68). But rollback was realized in the new December 1975 legislation, via its imposition of a ceiling on the new-oil price, with the aim of pushing the average domestic price of oil more than a dollar below the existing domestic price. In addition, the two-tier price system for oil was locked in by the legislation until the end of the decade. Media coverage of the new law

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[328] He expressed that view, during the initial oil-embargo period, in Instructional Dynamics Economics Cassette (Paul Samuelson series) Tapes 141 (November 15, 1973) and 142 (November 29, 1973).

[329] After Ford's phased-decontrol plans were defeated, U.S. law imposing oil-price controls was set to expire, in so doing generating full decontrol by default. Friedman relished this prospect (Instructional Dynamics Economics Cassette Tape 173, August 1975, Part 1; *Newsweek*, August 25, 1975), and Senator Jackson remarked of himself and his fellow advocates of control, "I think we're in trouble" (*Washington Star*, September 9, 1975). On September 9, President Ford vetoed a bill that would have extended controls, but later in the month he agreed to continue to enforce the controls until later legislation formally restored them (Sobel, 1977, pp. 69–70).

portrayed it as a compromise between opponents and advocates of decontrol, as it contained provisions for the oil price ceilings to be raised roughly with the general U.S. price level, gave the president the power to remove price controls starting in 1980, and allowed controls to expire altogether in 1981, absent further legislation. But the bill ensured that the presidential power to decontrol oil, which had existed previously, was now gone for the rest of the decade. The passage of the bill amounted to a substantial victory for Senator Jackson and the broader pro-controls movement.

"Calling the bill… a 'compromise' is an egregious case of misleading labeling," a disgusted Friedman wrote in his column after Ford had signed the bill. "…By keeping an unrealistic price ceiling on 'old' oil and imposing one on 'new' oil, it will discourage domestic production. We are already subsidizing the importation of oil… [and this measure] raise[s] this subsidy substantially." (*Newsweek*, January 19, 1976.) As noted in the previous chapter, it was largely in response to Ford's signing of the bill that Friedman ended his support for the president and favored Ford being displaced by Ronald Reagan as the Republican party's 1976 presidential nominee.

Oil prices and the presidential election

Friedman nevertheless remained optimistic about the trajectory of energy developments would likely take during 1976: in a talk on January 23, he remarked that notwithstanding U.S. policy that "mistakenly encourages consumption of foreign oil through artificial price ceilings instead of encouraging domestic consumption," he expected that responses by the private sector in the United States and abroad to the earlier OPEC shock "is diminishing demand, and this will further loosen the cartel's monopoly." (*New York Times*, January 24, 1976.) In contrast, and as discussed above, the rest of 1976 witnessed both a strengthening of U.S. energy demand and resilience of the OPEC-imposed oil price increase.

In terms of domestic political developments, the presidential primary contests served to rule out what would have been, from Friedman's perspective. the best- and worst-case scenarios for the future of energy policy. Ronald Reagan, a strong champion of decontrol, was beaten by Ford for the Republican nomination, but Henry Jackson, controls' leading proponent, was also defeated in the primaries in the Democratic contest. The degree to which Jackson had established himself as an interventionist with regard to economic matters had probably hurt his presidential chances—a vulnerability he had partly anticipated when he had insisted, in the speech launching his

candidacy: "I am not against big business. I am for the profit motive. I am for incentives." (*Evening Post* (Wellington, New Zealand), February 7, 1975.)

Nevertheless, even after his departure from the presidential race in the spring of 1976, the influence of Jackson was clearly felt.[330] He left a mark on the Democratic party platform that came out of the party's summer convention and on which Jimmy Carter formally ran as the party's presidential nominee. Contradicting views that Carter would eventually propound as president, the platform stated that increasing energy prices to OPEC-imposed world levels would "simply produce high-cost energy—without producing any additional energy supplies." For its part, the Republican party platform stated that the United States "must eliminate price controls on oil… in order to increase supply," thereby taking a harder line on the matter than Ford had taken in the previous year.[331]

Views on the future of government price controls

After the election, an aside that Friedman made about compulsory government price control in his Nobel lecture immediately provoked discussion in a Congressional hearing. As noted in Section II above, Friedman used his Nobel lecture to give a reiteration of the distortion that volatility of inflation tended to create in the structure of relative prices—a distortion that damaged economic efficiency and social welfare. Observing that temporary nominal rigidity in private-sector wages and prices was one reason this distortion arose, he added: "In practice, … the contamination of price signals will almost certainly be reinforced by legal restrictions on price change. In the modern world, governments are themselves producers of services sold on the market: from postal services to a wide range of other items. Other prices are regulated by government and require government approval for change: from air fares to taxicab fares to charges for electricity. In these cases, governments cannot avoid being involved in the price-fixing process."[332]

In a Senate committee hearing, held on December 14, 1976, on the subject of the monitoring of U.S. wage- and price-setting, Senator William Proxmire read this and other passages of the Nobel lecture, delivered the previous day, as the basis for his statement that in the lecture Friedman "said some things I have never heard him say before. He points out there is no way

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[330] Jackson announced the suspension of his presidential campaign on May 1, 1976 (*Logan Daily News* (Ohio), January 12, 1977).
[331] See Sobel (1977, pp. 26, 27). The platforms were issued in July 1976.
[332] Friedman (1977c, p. 467).

you can avoid government fixing prices..." Proxmire's witness, Barry Bosworth, agreed with Proxmire: "I read the speech, too. I was in some respects a little surprised, because I think it seemed to indicate a change [in] his views." In Bosworth's interpretation, whereas Friedman had once wanted "to go back to some [sort of] competitive economy," he now seemed "to have agreed with much of the rest of the profession that that is not politically practical, technically practical, or economically practical—and he is going to accept the system the way it is, much more than he ever did before."[333]

Once read in the context of the lecture and his other writings, it becomes clear, however, that the construction that Proxmire and Bosworth put on Friedman's statements was misconceived. The passages in Friedman's Nobel lecture signified neither a change in his position on economic relationships, nor a declaration that he was now resigned to the continued government intervention in the price system. The passages amounted, instead, to *positive-economics* analysis—specifically, analysis in which Friedman indicated that price changes that occurred in sectors in which the government had price-control powers produced an extra source of variability in relative prices—and hence of short-run disruption to the capacity of the inflation rate to adjust to the longer-run conditions governing nominal variables (with these conditions in turn determined by monetary policy). He had made this point before—for example, in his 1968 entry on the quantity theory of money for a social-sciences encyclopedia.[334]

And in the area of normative economics—which was not the concern of the Nobel lecture—Friedman remained vehement in urging greater reliance on market mechanisms. Notably, and contrary to the inference made by Proxmire and Bosworth, Friedman was strongly in favor of curbing governments' involvement in price setting. Indeed, not long after his Nobel lecture, a Friedman *Newsweek* column appeared (December 27, 1976) advocation privatization of public-sector enterprises.[335] For the United States, the sectors in which Friedman wanted minimal government involvement certainly included the areas of rail transportation and postal services

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[333] From the exchange in the hearing of December 14, 1976, in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1977b, p. 42). Bosworth was generous, however, in indicating that "most of us have come around to his [Friedman's] point of view" with regard to the appropriate specification of the Phillips curve (p. 42).
[334] See Friedman (1968d, p. 435). That discussion was consistent with Friedman's viewing these government-provided price changes as a source of price-level shocks that could be represented as a zero-mean cost-push disturbance to the expectational Phillips curve. See Nelson (2020a, Chapters 2, 7, and 10), as well as the discussion in Chapters 2 and 3 above of Friedman's perspective on another form of relative-price shocks—commodity price shocks. (This was one aspect of Friedman's Phillips-curve views that someone like Bosworth—who gave great credence to the notion that cost-push shocks were persistent and tended to have a positive mean—would not have accepted. See Bosworth and Lawrence, 1982.)
[335] During this period, Friedman was active in U.K. public debate in advocating, in that country, the privatization of government-owned utilities and other public-sector-operated companies in that country. See Nelson (2017).

that Proxmire named. And far from being resigned to government price control in areas of private-sector production, Friedman remained a passionate proponent of the repeal of such control. This included the price controls applying to oil and related petroleum products.

Occasions for further interventions by Friedman on the matter of petroleum price control would soon arise. Although the December 1975 legislation signed by President Ford had ostensibly put the crude-oil-price control issue to rest for the rest of the 1970s, energy policy, including the matter of the two-tier price system—would figure prominently in American public decade over the rest of the decade, even before the second oil shock occurred in 1979. President Ford himself revived the matter of decontrol via a last-minute action he took in connection with energy pricing. On his last full day in office, on January 19, 1977, Ford used his executive powers to order the removal (effective March 1) of government controls on retail gasoline prices. Ford could do this because prerogatives concerning the operation of these controls, in contrast to the controls on crude oil prices, had remained with the U.S. president. But, as crude-oil price controls were really the key petroleum price controls in the United States, Ford's move was largely symbolic. And in any event, before it could take effect, Ford's decontrol proclamation was soon rescinded by President Carter during his first week in office (see Sobel, 1977, p. 71).

When taking this action, Carter himself acknowledged the problems in the present system of energy pricing in the United States, as he indicated that he would soon unveil his own proposed revamp of the federal government's energy arrangements. He indeed went ahead and did so, as discussed in Chapter 9 below. But, instead of comprehensive decontrol, either instantaneous or phased, Carter proposed to make crude-oil price controls permanent—as part of an energy plan that left Friedman aghast. At the same time, Carter's plan aimed to raise domestic energy prices to world levels—and it therefore attracted the ire of Henry Jackson, who after his defeat in the presidential contest remained a powerful senator and Congressional committee chair. In 1977, the new Carter Administration would therefore face a two-front battle regarding its energy-policy plans: one in which was subject to a free-market-based critique, advanced by Friedman and others; and one in which it confronted several leading members of Congress, most prominently Jackson in the U.S. Senate, who sought to amass public support for keeping oil prices in the United States below the world market level.

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 7: Debates on International Economic Policy and Geopolitical Developments, 1975 to 1976

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July 30, 2023

I. EVENTS AND ACTIVITIES RELATED TO DEBATES ON INTERNATIONAL ECONOMIC POLICY AND GEOPOLITICS, 1975–1976

On Friday, October 29, 1976, Milton Friedman journeyed to Milwaukee. Due to Milwaukee's reasonable proximity to Chicago—which was, for the moment, still his primary location—Friedman was able not only to make the Milwaukee visit a day trip but also to incorporate into his itinerary, *en route* to home, an evening visit to Illinois' Lake College, in order to participate in a question-and-answer forum arranged by a student group. Consequently—and exemplifying what had been a frequent feature of Friedman's travels over the previous decade—the October 29 excursion saw him combine academic, media, and business engagements. The academic portion—the Lake College campus visit—followed a set of appointments in Milwaukee that was partially captured in a newspaper account the next day: "Friedman had a press conference, talked twice with groups of VIP businessmen, … [and] taped radio and television interviews." (*Milwaukee Journal*, October 30, 1976.) In capping off his engagements, when traveling back to Chicago in the later evening, Friedman also gave a long newspaper interview, published early the following week (*Milwaukee Journal*, November 1, 1976).

Along with the activities already mentioned was the centerpiece of Friedman's October 29 trip: a luncheon talk, organized by a local financial firm, to an 850-member business audience at Milwaukee's Marc Plaza Hotel. At the event, Friedman had occasion to remark that he used to live near Milwaukee, on account of his 1940–1941 affiliation with the University of Wisconsin. This affiliation ended, Friedman told the audience, when the university "did me the great and

[1] Email: Edward.Nelson@frb.gov. The views expressed here are the author's alone and should not be interpreted as those of the Federal Reserve or the Board of Governors. The author is grateful to Oliver Bush, Rex Ghosh, Leonidas Montes, and Maurice Obstfeld for providing information on issues covered in this chapter and to Andrew Farrant for comments. See the Introduction in Nelson (2020a) for a full list of acknowledgments. The author regrets to note that, since the research underlying the present chapter started, nine individuals whose interviews with the author are drawn upon below—Gary Becker, Robert Lucas, Rachel McCulloch, Bennett McCallum, David Meiselman, Robert Nobay, Edward C. Prescott, Richard Timberlake, and Paul Volcker—have passed away.

good service in deciding that they didn't want to keep me." (*Milwaukee Journal*, October 30, 1976.)

Friedman elaborated later in his Milwaukee appearance that this departure had not actually been a case of an outright termination of his employment. His exit from the University of Wisconsin had had a voluntary dimension, as it came only after he had received from the university a provisional offer of a longer-term appointment that would succeed his existing, temporary position.

This aspect of the University of Wisconsin episode was also made explicit when the matter was covered in the Friedman-profiling chapter of *New York Times* reporter Leonard Silk's *The Economists*—a book that, as it happened, would reach print within a couple of weeks of Friedman's 1976 Milwaukee remarks. After having interviewed Friedman about 1941's developments, Silk (1976, p. 59) wrote: "Friedman received and accepted the offer of a longer-term appointment." As Silk's account indicated, after receiving this offer, Friedman had nevertheless departed: when his proposed continuation was still subject to a veto, the dispute spiraled at the university on whether he should be appointed, and Friedman withdrew himself from contention.

This was much the same basic account that Friedman gave fifteen years later, when—approaching the fiftieth anniversary of the incident—he remarked: "I quit. I asked the dean to withdraw my name from consideration and quit."[2] But, notwithstanding this aspect of the episode, Friedman's implication—in his initial October 1976 Milwaukee remarks—that his employment had been terminated in 1941 accurately conveyed the reality that the acrimony surrounding his possible continuation at the University of Wisconsin had made it untenable for him to stay on.

The fact that there was major opposition to Friedman receiving a permanent job at the university had been long in the public record, even being reported in the Wisconsin press at the time.[3] And

[2] Also consistent with Friedman having already received an appointment, Silk's (1976, p. 59) account had stated that Friedman "withdrew his acceptance." Friedman's 1991 account was accurate in implying that a further step (a Regent's vote—see *Winona Republican Herald* (Minnesota), May 16, 1941) was needed to confirm the appointment and that he withdrew from consideration ahead of this step.

[3] The *Milwaukee Journal* (May 16, 1941), citing United Press as its source, had stated: "Reappointment of Milton Friedman, 26 [sic; 28]… to the University of Wisconsin economics department staff [on] Thursday bore the approval of President Clarence A. Dykstra and Dean George E. Sellery and the opposition of Prof. E.E. Witte,

Friedman himself had discussed that opposition when asked about the episode by those, including Silk, who had written profiles of Friedman in the early and mid-1970s. It was notable, nevertheless, that he highlighted it himself again on his 1976 return to Wisconsin. This unsolicited reference underlined the effect on Friedman of the 1941 episode—an episode that he later further described as "a very traumatic experience."[4]

Friedman also likely felt inclined to mention the incident in his remarks to the Milwaukee business audience because the contrast between his position in mid-1941 and what he faced on October 29, 1976, was so great. He had undoubtedly had many ups and downs in the intervening thirty-five years—with the downs including practitioners' and fellow researchers' shunning, during the 1950s, of his monetary work and the landslide defeat of the 1964 Goldwater presidential campaign for which he had been an adviser. But, overall, as of late October 1976 Friedman was at a vantage point from which he could look on his career as having been a major success.

It was also a career that was, in important respects, reaching its triumphant culmination. In contrast to his insecure position in 1941, Friedman's had high professional and public standing in 1976—manifested in part in national fame, as reflected in being invited to speak at the Milwaukee business luncheon as well as in the fact that, on making that appearance, he received a standing ovation from the audience. Furthermore, this event came fifteen days after the esteem he had established in the economics profession was manifested in his Nobel award—a milestone highlighted by the photo captions in the press coverage of the Milwaukee appearance, in which he was labeled "Milton Friedman, Nobel Prize Economist" and "Milton Friedman, Nobel Laureate."[5] And a few days prior to this appearance, NBC news had had Friedman as their studio guest for the national Sunday morning program *Meet the Press*—with Friedman being interviewed, nine days ahead of the 1976 presidential election, about U.S. economic prospects. On a still grander scale, in December he would go to Sweden to attend the Nobel award ceremonies. And all these accolades would be made more poignant by the reality that Friedman's time as an economics department member at the University of Chicago was, after just over thirty years, coming to a close in the period bridging the closing days of 1976 and the early days of 1977.

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department chairman. Witte, in a letter to Sellery, declared [that] Friedman is 'too young and inexperienced' for a $3,500 a year position as assistant or associate professor."

[4] Academy of Achievement interview with Milton Friedman, January 31, 1991, p. 8 of transcript.

[5] In, respectively, *Milwaukee Journal* and *Milwaukee Sentinel*, October 30, 1976.

Yet this period of fall and winter 1976 would not consist of an unqualified period of celebration for Friedman. He had reacted with some caution on receiving his Nobel award on October 14, eschewing the characterization of it as the capstone of his career.[6] But the qualifications that he had expressed at the time of the Nobel Committee announcement had related mainly to whether getting the award should serve as judge and jury in evaluating the success of a research career. His doubts in this connection did not really conflict with the notion that the time of a Nobel award was one of great celebration for the recipient. In Friedman's case, however, such a celebratory atmosphere was not altogether what materialized, and three decades on, one of the final profiles in which Friedman participated noted that "Friedman's 1976 Nobel was bittersweet." (*Rutgers* magazine, Fall 2006, p. 27.)

The reason for this mixed verdict on the period was that the two months after the October 14 award announcement, through the end of Friedman's week in Stockholm attending the prize-related events in mid-December, saw heavy criticism and protests directed toward him that considerably diluted the coronation-like air typically associated with winning a Nobel. Instead of being positioned to bask as congratulations and celebratory events came his way, and so able to treat the period as an extended awards ceremony, he encountered evisceration in much of the media, as well as heated mass demonstrations.

Specifically, this period featured repeated argument—including multiple responses by Friedman—regarding his trip to Chile in March 1975, a trip that had included a meeting with the leader of the ruling military junta, General Augusto Pinochet. This was a revival on an amplified scale of the controversy that this trip had generated in the United States in 1975, particularly in the fall of that year. Within days of his receiving the Nobel award, it was manifested in adverse letters to the editor in the U.S. press, and the furor was on such a scale that, in Friedman's October 29 speech in Milwaukee, his invocation of his University of Wisconsin experience had been in the context of describing his introduction to controversy.[7]

In 1941, he had publicly foreshadowed his withdrawal from consideration for the position "if it would mean a controversy raging about me" (*Wisconsin State Journal*, May 15, 1941). In time,

[6] See the previous chapter.
[7] Similarly, Friedman remarked fifteen years later: "I have since been involved in similar public disputes, but that [in 1941] was the earliest and the defining one, if I may say so, for me." (Academy of Achievement interview with Milton Friedman, January 31, 1991, p. 8 of transcript.) He had, however, been involved in a more muted controversy that attracted press attention when, in 1939, the preliminary results of his work with Simon Kuznets were released by the NBER. See Nelson (2020a, Chapter 2).

he became accustomed to being involved in controversy, including disputes that went beyond the forum of research debates: "You mustn't get into this business of public policy if you don't have a pretty hard skin."[8]

By 1976, therefore, Friedman had great experience in being at the center of polemics. The latest controversy, however, was the greatest one in which he had been involved. Ten days after he won his Nobel, he was asked in his *Meet the Press* interview, "Many people wonder how you defend a relationship with a military regime," and he had responded, "I have no relation with the Chilean official regime," before proceeding in the interview to discuss, as he would on many occasions in this period and subsequently, the reasons why his critics contended that there was a relationship, including the 1975 meeting with Pinochet and the University of Chicago training of several economic personnel in the junta's government.[9] He would defend the trip to Chile further in media appearances during the Nobel events in Stockholm. Friedman's time in Stockholm would also see him be the subject of protest demonstrations, and this would be a situation that would be repeated in many of his subsequent public appearances in the United States. This state of affairs was something to which he would have to accept as the norm. Although, on several occasions starting in December 1976, Friedman would suggest that the storm over his having gone to Chile in 1975 had now died down, this was not the case. The matter of Chile would never really stop being a major source of controversy in public discourse concerning Friedman, as will be discussed further later in this chapter.

Inflation and political turbulence

Over the course of 1974—a year spanning most of the time between the September 1973 coup that installed the Pinochet military junta in Chile and Friedman's March 1975 trip to that country—Friedman had occasion to discuss the Chilean situation in a number of writings and talks. In particular, he did so when discussing the political instability that had arisen in many countries across the world from high inflation rates. In these and later commentaries, Friedman cast the political-instability/inflation relationship as involving gradations—ranging from the unpopularity with voters (manifested in opinion polls and election results) experienced by the leaders of Western democracies that had recently had inflation in or near double digits, to examples of military takeovers that had taken place in countries that had had more extreme inflation in the triple, or higher, digits.

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[8] *Rutgers* magazine, Fall 2006, p. 27.
[9] The quotations are from *Meet the Press*, NBC, October 24, 1976, p. 2 of transcript.

As of the mid-1970s, the case of the United States was not available as a clear-cut example of the political-instability/high-inflation nexus. In 1980, the electoral defeat of Jimmy Carter would provide a prime illustration of the connection between inflation problems and U.S. presidential fortunes. But with respect to the bout of high inflation that the United States had experienced by the mid-1970s, it was difficult to ascertain the political repercussions, on account of the great importance of domestic noneconomic developments in public discourse over the period. In particular, the whole episode was clouded by the fact that Richard Nixon's post-reelection decline in popularity—although it had coincided with, and certainly been accelerated by, the take-off of the U.S. inflation rate that had become especially evident since the start of 1973—had been driven very largely by the Watergate scandal. It was, therefore, developments in *other* countries in the decade to date that Friedman concentrated on when—in the conclusion of his piece in the July 1974 issue of *Fortune* magazine—he provided examples of his proposition: "Throughout the world, inflation is a major source of political unrest."[10]

Consistent with his belief in gradations in the effect of inflation on political instability, Friedman coupled his examples of countries that continued to be democracies with the recent events that had seen the loss of democracy in Chile: "Edward Heath surely lost an election [in February 1974] because of inflation. Prime Minister Tanaka's popularity is at an all-time low because of inflation. President Allende of Chile lost his life at least partly because of inflation."[11]

In other accounts that he produced over this period—including a booklet version, published in late July in London, of the *Fortune* piece—Friedman expanded his account of the connection between Prime Minister Heath's fortunes and departures from price stability by noting that both Heath's February 1974 defeat and his earlier June 1970 victory had been in U.K. national elections in which inflation was a leading campaign issue.[12] In a talk given in Pittsburgh on December 5, 1974, Friedman was also able to update his account of inflation's influence on political turbulence by noting of Japan's now former prime minister: "Mr. Tanaka has just lost

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[10] Friedman (1974b, p. 176; p. 161 of 1975 reprint). See also Friedman (1974a, p. 32; 1978b, p. 47; 1991a, p. 44).
[11] Friedman (1974b, p. 176; pp. 160–161 of 1975 reprint).
[12] The elaboration in the U.K. piece (Friedman, 1974a, p. 32; see also Friedman [1978b, p. 47; 1991a, p. 44]) read: "Inflation surely helped to make Mr. Edward Heath Prime Minister in 1970 and, even more surely, ex-Prime Minister in 1974." In a luncheon address on October 17—a week after Heath had lost an election to Harold Wilson for the second time in 1974—Friedman told a group of commodity market specialists assembled at the University of Chicago's business school (Friedman, 1974c, p. 19): "We know very well that Mr. Wilson in Britain lost his position in the first place some years back because of inflation, not because of unemployment. Mr. Heath succeeded him and lost his position [as prime minister] because of inflation, as well."

his job in Japan because of inflation."[13] And with respect to military coups that had occurred against a background of severe inflation, at the end of the 1970s Milton and Rose Friedman noted that "inflation contributed to the overthrow of Salvador Allende in Chile in 1973 and of Isabel Peron in Argentina in 1976, followed in both countries by the assumption of power by a military junta."[14]

Even before the events of 1975−1976 linked Friedman's name with Chile in public discussion, his emphasis on the link between inflation and political instability, along with his application of it to democratic change and coups alike, was controversial due to the fact that he accompanied these comparisons with his judgment that an important connection existed between high inflation—particularly that occurring under peacetime conditions, as seen in various countries during the first half of the 1970s—and the growth of the public sector's role in the economy. But the basic contention that inflation was a source of political turbulence across countries, being associated with leaders' unpopularity and with changes in government in many democracies, as well as the point that more extreme cases of inflation in other countries had been associated with overthrows of democracies or with changes in the ruling dictatorship, was less controversial, and it was supported by other analyses that did not make Friedman's policy conclusions. In particular, the regularity that inflation was politically destabilizing across the world was stressed by an analysis that the *Financial Times* provided in this period (May 11, 1974). This piece, although it excluded Latin American nations, included some dictatorships in its catalogue of what it called "20 Countries With a Common Thread." The 20 countries comprised various OECD members plus some European nations that were not members of the OECD. Also like Friedman, the *Financial Times* account cited coups as being among the reactions to high-inflation conditions—the *FT* analysis pointing to Portugal as an example.

During the same May 1974 timeframe, an Associated Press analysis had focused on democracies only and reached much the same conclusion about the repercussions of inflation for political stability—a conclusion that the AP piece summarized in the heading: "Statesmen's 'Killer': Inflation." The text of the piece observed: "From Iceland to Australia, from Belgium to Japan, [inflation] has either toppled governments or threatens to force changes at the top." (*Japan Times* (Tokyo), May 31, 1974). It was, therefore, a widespread observation that democracies in

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[13] Friedman (1975c, p. 17; p. 709 of 1979 reprint).

[14] Friedman and Friedman (1980, p. 253). As this reference appeared in a passage of the *Free To Choose* book, the statement in *The Economist*'s (March 8, 1980) review of that book that "Chile is not mentioned at all" in it was not correct.

the mid-1970s were experiencing a combination of economic turmoil—especially inflation that was very high by their postwar standards—and electoral and leadership instability.

The specific analyses quoted above, including Friedman's, were written in 1974. But the year 1975 did little to shake the profile of the new decade of one of simultaneous political and economic instability. In the case of the United States, although the Watergate scandal no longer took center stage, other issues kept up the tempo of political and economic turmoil. In the previous two chapters, which covered the U.S. domestic economic scene in the mid-1970s, it was stressed that 1975 saw the trough of the worst postwar American recession to date, and a then-record postwar unemployment rate, alongside abating but still high inflation. In light of these events, Norman N. Bowsher (1976) gave his account of monetary and economic developments for the year the title "1975—Year of Economic Turmoil."

Only in 1976 could matters be said to have reached a calmer state—with both U.S. unemployment and U.S. inflation well off their peaks of the preceding years. Even so, amid the pattern of generally more satisfactory outcomes, a temporary uptick in the unemployment rate during 1976 may well have proved decisive in President Ford's election loss in November (see the previous chapter). In addition, the monetary policy decisions taken by the Federal Reserve in the course of 1976 appear, as discussed in the next chapter, to have produced a setting of excessive ease that continued in 1977 and that paved the way for the return of double-digit inflation in 1979.

The year 1975 had been unstable for the United States geopolitically as well as in terms of economics. When Henry Kissinger (1982) wrote his memoir account of the truncated second Nixon term, he would title the book *Years of Upheaval*. Kissinger put a more favorable construction on events in his 1999 account of the Gerald Ford years (August 1974 to January 1977), with the title *Years of Renewal*—here referring in part to the notable stabilization of the domestic political climate that occurred after Nixon's resignation. But in the area of U.S. domestic economic policy, such a characterization would seem less appropriate in view of the aforementioned developments during the Ford years, especially up to the start of 1976. Kissinger's assessment that the period was one of renewal may well have reflected the trait that many, including Friedman, attributed to him of being focused on aspects of public policy other than economics—which, as discussed later in this chapter, was not a subject matter in which Kissinger was steeped. The characterization of renewal is, furthermore, of questionable validity when applied to developments during 1975 and 1976 in Kissinger's own field of foreign policy

and geopolitics: The year 1975 would see the fall of Saigon—confirming the U.S. defeat in the Vietnam War, while more generally Garthoff (2001, p. 299) referred to "three years of turbulence" from 1974 to 1976 in the United States' relations with its allies.

The global economic outlook for 1975 and geopolitics

At the very start of 1975, Friedman appeared on a nationally televised NBC news special discussing the U.S. economic outlook for the year ahead. In the course of his contributions to the program, he displayed a tendency—one that would recur in his commentaries over the years from 1975 to 1976, but which had not been a prominent trait of his beforehand and that would be less in evidence after 1976—to allow himself to get involved in discussions of geopolitics.

Friedman's appearance, in which he and Walter Heller were interviewed jointly by Louis Rukeyser, took place against the backdrop of an event that had intersected economics and geopolitics: the OPEC shock of 1973−1974. The war that was initially associated with the shock had been brief, but the shock itself was proving far more permanent. Its economic repercussions were continuing into the new year, with the higher world oil price now having been fully in force for a year at the time of the television program's broadcast of January 1, 1975.

It was Heller who, in the panel discussion, emphasized the oil price shock, in conjunction with a host of other world factors, when asked by Rukeyser about economists' record in predicting inflation. Claiming that the behavior of U.S. inflation had been well understood through the end of 1972 by economists of his—Keynesian—persuasion, Heller provided a catalogue of international forces that he believed accounted for errors in predicting inflation in 1973 and 1974: "we did not foresee that there would be a quadrupling of oil prices, we didn't see that there would be world food shortages, we didn't see that the anchovies would disappear, we didn't see that there'd be the Great Grain Robbery by the Russians. It's true we didn't foresee those. Neither did anyone else." Later in the discussion, Heller later added yet another international factor, the devaluation of the dollar, in his account of the forces making for high U.S. inflation over the period (*NBC News Special: If You Think It Was Tough To Make Ends Meet in 1974, Wait 'Till You Hear About 1975*, NBC, January 1, 1975).

An explanation of recent years' U.S. inflation centered on global factors was also outlined by James Tobin on April 17, 1975, in the opening session of a policymaker/researcher conference organized in part by the University of Chicago. Referring to "the special transient causes of the

double-digit inflation," Tobin suggested: "A good part of the rate of inflation… was not internal to the industrial American economy but was the result of oil prices, the world food supply-demand situation, previous depreciations of the dollar due to our own deliberate policy, and other factors." As discussed in the previous chapters, a fear voiced by leading Keynesians in 1974–1975 was that, having been generated by international forces, might be perpetuated by a wage-price spiral. By early 1975, however, with nominal wage growth showing little step-up, they were more optimistic about a substantial decline ahead—one that Tobin suggested would "result not so much from the recession, but simply because of the special transient causes of the double-digit inflation that provoked the recession policy."[15]

These internationally focused explanations of U.S. inflation were rejected by Friedman. Even in this period of the mid-1970s, during which his interest in global developments was heightened, Friedman eschewed appeal to world forces when it came to the understanding of inflation, particularly U.S. inflation. He told Heller in their television debate that the blame for high U.S. inflation lay instead with the expansionary demand policies that originated at home and that Heller still favored: "It is precisely those policies that have put us in our present situation." Friedman was in Japan at the time when Tobin was giving his own version of the international-factors-centered explanation for inflation at the April 1975 University of Chicago conference. But while in Australia about a week earlier, Friedman had made observations that amounted to his rebuttal to the kind of account that Heller and Tobin advocated. "Inflation is a worldwide phenomenon but not an international phenomenon," he observed in these remarks. "That is to say, it occurs in many countries because all of those countries tend to follow roughly the same policies."[16] Similarly, in an appearance in Portland, Oregon near the end of 1974, a couple of weeks before his television appearance with Heller, he stated: "Inflation is occurring in many countries because the same forces that I talk about are at work in those countries [separately]."[17]

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[15] In Birnbaum and Laffer (1976, p. 19). Nevertheless, as discussed in Chapters 4 and 5 above, leading Keynesians did indicate that weaker aggregate demand conditions provided one reason for expecting inflation to start falling (often stressing the impact of negative growth in demand, rather than negative output gap levels *per se*, in this connection). For example, with respect to inflation prospects for 1975, Walter Heller stated that "we have already squeezed the economy so hard that the inflation problem is ebbing" (Shane, 1975, p. 475) and pointed to the likelihood that the U.S. unemployment rate would rise through midyear as a basis for believing that "weakness of the labor market… will have a retarding effect on wage increases" (p. 474).
[16] Friedman (1975h, p. 59). He subsequently made similar observations in *Milton Friedman Speaks*, Episode 6, "Money and Inflation," taped November 7, 1977, p. 13 of transcript, as well as Friedman and Friedman (1980, pp. 262–263).
[17] Friedman (1975e, p. 27). Friedman was speaking at a one-day joint business-academic forecasting conference held on December 16, 1974.

An area of common ground that Friedman had with Heller and Tobin was that he believed that "special, transitory phenomena" had had a bearing on the rate of inflation reached in the mid-1970s (Instructional Dynamics Economics Cassette Tape 144, April 17, 1974). But a good deal of what Friedman emphasized in this connection was not international factors but the unveiling, over 1973−1975, of inflation previously suppressed by price controls.[18] He also, as discussed in Chapter 3, conceded a role for specific price shocks, including those arising from world commodity price developments, in affecting recorded U.S. inflation rates in the mid-1970s. But in Friedman's assessment, these shocks—in common with price controls' imposition and removal—affected primarily the *timing* of aggregate price changes, rather than their average value.

By the time of the peak in the mid-1970s, Friedman had come to the conclusion that he would maintain in later years (*Newsweek*, April 24, 1978; *Wall Street Journal*, April 15, 1988) that the average rate of inflation over the 1970s was much as what would be expected on the basis of prior M2 growth and that a policy of about 4 percent a year M2 growth would have avoided the strong 1970s surges in inflation. Under this interpretation, the shocks to prices and costs specifically cited as drivers of inflation were contributing to the average inflation rate only through the narrow, and quantitatively modest, means of their effect on the average growth of potential output. Similarly, while he did not disagree with Heller and Tobin that inflation would come down notably from its peaks over 1975 and 1976, Friedman was inclined to attribute far more of the decline to demand restriction since 1973 than were his Keynesian opponents. Correspondingly, he saw the U.S. economy as being highly susceptible to a revival of inflation later in the decade, and he warned of "double digits with a vengeance" in the event of the Federal Reserve returning to expansionary settings of the kind seen before 1973 (*Newsweek*, February 17, 1975). This indeed occurred—although mainly as a result of relaxed monetary policy settings in 1976 and 1977, during the second and third years of U.S. economic recovery, and not (as Friedman had feared in 1975) as a result of very large contemporaneous policy responses in the immediate wake of the 1973−1975 recession.

Friedman's casting of the sources of inflation through the start of 1975, and the prospects for inflation in the second half of the decade, in terms of domestic monetary policy decisions implied that he was confining the role of international factors very largely to their influence on monetary growth per unit of output. This analysis applied both to the United States and

[18] See, for example, Instructional Dynamics Economics Cassette Tape 147 (May 30, 1974). See also Nelson, (2020b, Chapter 15) for a detailed account of Friedman's perspective on the controls' effects.

elsewhere. He granted that, even within this framework, it was the case that domestic autonomy depended on the international monetary regime: "No one country by itself can control inflation unless it is willing to change its exchange rate." But this qualification had now lost its applicability in the era of floating exchange rates—and had not really applied anyway to the United States ever since the Federal Reserve's creation. Hence, for many countries in the 1970s, and for the United States in this decade and many previous ones, Friedman was comfortable with an explanation for inflation centered on the domestic monetary authorities' decisions and actions: "in a world of floating and flexible exchange rates as we now have, each country is an island unto itself with respect to inflation."[19] He was correspondingly basically unmoved by citations of global developments, including the 1973−1974 oil shock.

As a factor bearing on real outcomes, as distinct from the ongoing inflation rate, Friedman did see the oil shock as very important. He articulated this perspective in his television appearance with Heller. "The oil crisis has caused us great economic costs, there's no doubt," Friedman remarked. "You've had a large transfer of wealth from us and from [other] countries who are consumers, who are consuming oil, to countries who are producing oil." Indeed, his estimate of a 1.5 percent effect on the price level for a given money stock was derived from an assessment of the aggregate effect on U.S. potential output of the transfer of resources, and associated adverse terms-of-trade movement, of the oil shock. Friedman was using the 1.5 percent estimate in public commentary by late 1974 (see Chapter 3) and committed it to print when, in 1975, he covered the oil shock's U.S. effects in his revision of his *Price Theory* text, in which he wrote: "For the country as a whole, the higher cost of imported oil amounted to roughly 1.5 percent of the national income; this was the 'real cost' imposed on the nation, the amount transferred from United States consumers to the owners of the foreign oil sources."

In the television appearance at the start of 1975, it was in light of the oil price increase being an important real shock that geopolitical considerations came into Friedman's discussion. He cited prospective geopolitical developments as a complicating factor for the analysis of developments in 1975: "the most serious problem for this year of the oil crisis, in my opinion, is its political effects in the Middle East. The cartel, at the moment, is holding. It will, sooner or later, break. But the problem is that, before it breaks, it may bring down the Middle East in flames—and goodness knows what the consequences of that would be."[20]

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[19] See Friedman (1975e, p. 27) for such a discussion, including the specific quotation given in the text.
[20] *NBC News Special: If You Think It Was Tough To Make Ends Meet in 1974, Wait 'Till You Hear About 1975*, NBC, January 1, 1975.

Oil price declines and cartel behavior

In the same remarks, Friedman affirmed: "The price of oil will come down." His citation of geopolitical complications did not really serve as a hedge on this prediction. Instead, he was referring to the tension, within the cartel, that he saw as likely to build up in response to the downward pressure on oil prices associated with falling world petroleum demand—tension that would lead, he believed, to the failure of the cartel to maintain a united policy on oil supply. Friedman's implication was that the intra-OPEC dissension might be associated not only with the loss of solidarity among member (or, in the event of disintegration, former member) countries in exercising market power, but also with *military* conflict among members—possibly bringing the United States into a major foreign policy crisis.

In sketching this scenario, Friedman brought the geopolitical outlook prominently into his public commentaries. This was an unusual step for him to take: his coverage of international issues had been extensive over the years but had usually adhered more strictly to economic and financial aspects, with his discussion of foreign policy implications largely confined to such matters as the positive example that the United States could provide through a free-trade policy. But, having veered in these early-1975 television remarks into commentary on geopolitics and into speculation regarding particular eventualities in world affairs, he would do so again at various times over the course of 1975 and 1976—including, as discussed in later sections of this chapter, occasions when he did so well outside the context of discussing oil.

The fear of an intra-OPEC war that Friedman articulated at the start of 1975 was, however, was an off-baseline risk rather than his central scenario. His baseline expectation regarding 1975, as it was in surrounding years, was that what he perceived that normal dynamics governing the operation of a cartel would be in operation and lead to the acrimonious, but peaceful, breakdown of the OPEC cartel and to a decline in oil prices.

It was these normal cartel dynamics that were Friedman's basis for his repeated predictions, dating back to the onset of the oil price shock and discussed in previous chapters, of the shock being wholly wiped out before long by a price decline. Although he would, during the late 1970s, praise developments in the economic analysis of cartels that had taken place in recent years (see Chapter 9 below), it was the preexisting body of work on the matter that constituted what in early 1978 he called a "well-developed economic theory" and on which his well-

publicized forecasts of an OPEC collapse rested.[21] Friedman had previously invoked this theory of cartels in 1965, when characterizing U.S. commercial banks as able to command a low price in obtaining funds from depositors, thanks to the backing of the federal government's Regulation Q interest-rate ceilings. The evolution of commercial banking in recent years, he suggested at the time, "was an obvious example of what every textbook says about cartel agreements"—as banks' profits would initially be boosted by Regulation Q, "but, over the long period, the cartel tends to decline and becomes a smaller and smaller part of the total universe."[22]

In the case of U.S. banks as of 1965, the source of decline was the fact that the banking system was losing deposit business to thrift institutions that offered instruments to the general public that possessed at least some deposit-like characteristics, while having the additional attraction of bearing interest rates in excess of the rate ceilings to which the banks were subject.[23] In the case of the world oil market in the 1970s, Friedman saw a springing-up of petroleum substitutes and non-OPEC oil sources as likely to occur promptly in the post-shock period, undermining the cartel's ability to sustain the post-1973 price. In addition, on the demand side of the petroleum market, he envisioned a backlash occurring on the part of buyers across the world, with high prices leading to conservation and substitution efforts that further weakened the cartel.

Friedman's position that OPEC fell into the category of a cartel that had overplayed its hand by using its market power was one that he maintained consistently after the first oil shock. By 1977, he was willing to see some justification for the 1973−1974 oil price rise in terms of the prior substantial buildup of oil demand in the context of a largely unchanged price. But this was a minor qualification in his overall analysis. Friedman would continue to believe that the price rise associated with the oil shock had been overdone. And he still saw an eventual price decline to *below* the pre-oil-shock level (in real terms) as the eventual upshot. This development was part of the theory of cartels to which he believed the OPEC sequence conformed: Friedman remarked in his early 1978 talk on energy that "ultimately the price is not only back to where it was to begin with but [also,] it's lower than it was" before the cartel-induced price rise—owing especially to new, non-cartel sources of supply having been encouraged by the environment of

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[21] The quotation is from *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, p. 11 of transcript (and also appeared in the version of these remarks that reached print in Friedman, 1983b, p. 146).
[22] In Ketchum and Strunk (1965, pp. 120 and 120−121).
[23] In later years, with thrifts being brought into the rate-ceiling umbrella, banks' loss of business due to Regulation Q would be to other financial entities: in the late 1960s, open-market lending and, from 1970 onward (when Regulation Q's coverage was limited to retail deposits) money market funds. See Nelson (2020b, Chapter 14) and Chapter 14 below for further discussion of Friedman's position on Regulation Q in these later periods.

high selling prices.[24]

In keeping with his view that the OPEC episode was providing a practical demonstration of long-established microeconomic principles, Friedman covered the energy crisis during the years from 1973 to 1976 when he was again teaching price theory at the University of Chicago. And although, surprisingly, there were no entries on "oil," "energy," "gasoline," "petroleum," or "OPEC" in the index of Friedman's 1976 edition of the corresponding book *Price Theory*, Friedman did, in fact, add two passages on the OPEC shock to the book when revising it during 1975. One of these passages, already mentioned, discussed the effect of the oil shock on U.S. aggregate real income before proceeding to consider the distribution of this aggregate shock across consumers and the repercussions for U.S. relative prices.[25] The other *Price Theory* passage on OPEC also referred to oil's importance for the behavior of U.S. aggregate real income but focused on oil's status as a production input—one used in conjunction with but (in the kinds of production functions that Friedman was contemplating) also substitutable for, and by, those other inputs. The passage, in effect, allowed Friedman to buttress but also qualify his existing public statements that the oil shock would generate reduced oil usage and downward pressure on oil prices. Specifically, in relation to the demand backlash that would follow oil becoming a more expensive input, *Price Theory* noted that "petroleum is far more essential (closer to fixed proportions) in the short run than in the long run" and suggested that a longer-run effect of the oil shock would be to reduce oil's role in the production process and put downward pressure on petroleum prices.[26]

It was not at all unusual for Friedman to make the point that, as time passed, the scope for substitution between alternative factors of production increased substantially. But his stress on the point when discussing oil in 1975 likely related to his revised assessment during this period of the timeframe over which the principal market reactions to higher oil prices would be stretched out. The 1975 discussion, in focusing on U.S. producers' oil demand, provided an additional ground, on top of those resting on the behavior of U.S. consumers' demand, as the basis for a growing price elasticity of demand for oil over time and hence for increased pressure on OPEC to break.

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[24] *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, pp. 12–13 of transcript.
[25] Friedman (1976b, pp. 260–261).
[26] Friedman (1976b, p. 160).

These arguments for believing that what was happening was a protracted adjustment process, in which there was an underlying strong dependence of oil demand on prices, dovetailed with another Friedman point regarding the elastic nature of oil demand: that the adjustment in the United States to the OPEC price rise had been restricted by the country's continuing controls on domestic petroleum prices. Friedman did not mention this point in *Price Theory* and had also refrained from referring to it in his January 1975 television appearance. But, as documented in the previous chapter, it was a factor that he emphasized heavily in many of his other discussions of oil prices over 1975 and 1976. He stressed it, for example, in his *Newsweek* column of February 17, 1975, when noting that the breakdown of OPEC "has been taking longer than I expected."

Reinforcing this position was the fact that, as Debs (1976) observed, Europe and Japan had registered a more decided reduction in energy demand since 1973 than that seen in the United States. Debs pointed to different efficacies across regions of government-led conservation programs as a source of the difference in outcomes. But the fact that the non-U.S. regions had, by 1975, allowed full or near-full adjustment of petroleum prices to the OPEC shock was likely a more crucial source of difference from the U.S. pattern than the aspects of energy policy in each region that were more overtly labeled conservation programs. Outside the United States, higher prices were essentially being allowed to bite in full force.

The difference with the U.S. case was, however, one of degree. Oil price controls in the United States took a form that did not prevent sizable, though incomplete, price adjustment in U.S. petroleum markets after the oil shock. Their presence kept the increase in prices below that warranted by the OPEC shock, but major increases occurred nevertheless—as the regulations made final U.S. oil and gasoline prices weighted functions of uncontrolled imported oil prices, controlled "old" domestic oil prices, and prices of "new" domestic oil (which were initially free of control then, as discussed in the previous chapter, subjected to a ceiling, albeit quite a high one, by new national legislation that came into effect in late 1975).

Therefore, despite the presence of controls that impeded the process, price signals were at work to a significant degree even in the U.S. market, and Friedman saw the resulting market forces in the United States and other oil-consuming countries building up over 1975 and likely to be realized in OPEC breakdown by some time in 1976 (*Newsweek*, February 17, 1975).

In the event, there was evidence of strain on OPEC during 1975, in the sense that it had to reduce oil production to 70 percent of capacity (Debs, 1976, p. 11). But OPEC proved sufficiently cohesive to exercise this discipline on supply and, aided by the economic recovery in the United States that began in early 1975, was by the end of the year able to impose a modest increase in prices—one that it maintained in subsequent years.

OPEC was therefore more stable and had more solidarity over 1975 and 1976 than Friedman expected. It followed that the catalyst—OPEC disintegration—in his scenario of an intra-OPEC military dispute was absent. Irrespective of whether Friedman's fears were well grounded that an OPEC breakup at this stage might have spilled over into armed conflict, the fact is that the key event on which his fears of such large-scale political turmoil in the Middle East were based did not materialize.

Friedman would raise other geopolitical scenarios over 1975 and 1976: the pattern that he set at the start of 1975 by discussing foreign policy and military issues was one he continued. This pattern would be particularly manifested in various Friedman discussions during 1975 and 1976 of East-West relations and other matters related to world affairs.

In making public interventions of this kind, he was no doubt stimulated by the fact that he and Rose Friedman were now major international travelers, and his visits abroad generated considerable media attention. The intensified interest on his part in other countries and in the United States' place in the world would help explain why Friedman, in his *Newsweek* column and other commentaries, now covered quite a range of international issues. Some of these issues lay within his economic expertise (for example, whether U.S. trade with the USSR was desirable for the United States on economic grounds), some lay closer to the borderline between his and other fields (for example, whether such trade was desirable from a U.S. security perspective), and some concerned matters basically outside his professional expertise (such as the possibility of a USSR/South Africa alliance materializing).

Away from East-West matters, Friedman also had occasion over this period to comment on international economic policy in the advanced nations. Topics of this nature sat much more clearly within the field of economics. One of the matters—highly prominent in the wake of the oil shock—concerned the intersection of two unfolding global economic issues: exchange-rate arrangements and the prospects, post-oil shock, of the recycling of OPEC's export earnings.

Recycling of oil revenues and the new international monetary system

"A year ago, the potential power of OPEC (The Organization of Petroleum Exporting Countries) to control the economic and financial destiny of the industrialized world seemed almost unquestionable. Today, the issue is not so clear," a couple of business economists wrote in the late summer of 1975 (Safer and Mills, 1975, p. 21). One aspect of these authors' doubts about OPEC's strength—the durability of the 1973–1974 price rise—did not prove well founded. For, noting some wavering in that year's traded prices of oil, they incorrectly suggested that "secular economic forces will weaken OPEC's monopoly position and eventually lead to a more lasting break" in prices. Erroneous suggestions along these lines confirmed that, by early 1975, numerous professional forecasters were predicting that a major oil price decline would occur during that year or sometime later in the 1970s. They would all be proved wrong. But the prevalence of this forecast showed that Friedman was not alone in predicting future OPEC weakness.[27]

But, among those propounding this projection, Friedman remained the more famous case. In part, this was because of the scale of his prediction: he stood out in continuing to predict a complete price reversal, and Safer and Mills (1975, p. 21) were at pains to distinguish their own prediction of a softening in oil prices in coming years from those who claimed to see "the beginning of the end for OPEC." It was also because Friedman had staked a position so soon after the OPEC price rise that it would be temporary. As already indicated, the need that Friedman saw in 1975 to qualify his oil-price-decline predictions by providing a time perspective was likely driven by unflattering commentary on his well-publicized erroneous predictions of an early dissipation of the oil shock. The most well-known of the erroneous Friedman statements

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[27] As far as opinion within the U.S. government was concerned, it would be fair to state that there did not exist unanimity in the mid-1970s on whether the first oil shock would prove to be largely permanent. President Ford and Secretary of the Treasury Simon were united with many others in the administration in taking the view that, so long as the world oil price was high, it was appropriate that it be felt fully in final U.S. oil and gasoline prices. But with regard to the durability of those high oil prices, a variety of views were expressed in U.S. government circles. Ford suggested in a meeting held on January 30, 1975, that the world recession might break OPEC: "Producer cartels work well in good times, but I wonder about it in bad times." (In Galpern, 2012, p. 127.) Secretary Simon from the start was on record as being optimistic about the prospects of a significant unwinding of the shock: "You know already my views about oil prices," he remarked quite early in his tenure (testimony of August 14, 1974, before the Subcommittee on International Finance and Resources of the Senate Finance Committee, as reprinted in U.S. Department of the Treasury, 1975, p. 462). In contrast, Assistant Secretary Gerald L. Parsky remarked on January 14, 1975 (U.S. Department of the Treasury, 1975, p. 438) that "there is no way that the forces of supply and demand will be able to force the price to decline for at least three years." In addition, U.S. Secretary of State Kissinger's 1974–1975 proposals on energy policy (discussed below, in the part of this chapter titled "Henry Kissinger") were widely criticized for apparently acquiescing to the notion of permanently high oil prices.

had been in interventions that he had made during 1973–1974 in the U.S. national media and at business conferences. But he had articulated the argument over the same period in other venues—such as a February 1974 press conference that he held with European journalists that was subsequently broadcast on University of Chicago campus radio. On this occasion, Friedman had declared: "There is plenty of crude oil in the world. There is no shortage of it. The world crude oil price cannot stay at $10 a barrel; it will drop drastically within the next six or nine months as the reductions in consumption stimulated by the rise in price start taking over, and as the rise in price stimulates additional supplies. From the long-run point of view, there is absolutely no reason to suppose that we are in a completely new ball game or a new world."[28]

Mistaken predictions like this, together with what proved to be his inadequate qualifications to them in 1975, would dent Friedman's forecasting reputation: his statements in this area would be quoted repeatedly by critics over the following decade. But, although the failure of the oil price to decline promptly and the confirmation over subsequent years that—contrary to Friedman's diagnosis—the first OPEC shock was indeed here to stay would have a lastingly negative effect on his image, another aspect of the oil shock would help vindicate another well-known, and very longstanding, facet of Friedman's thinking: his belief in floating exchange rates. The existence of floating exchange rates was one way in which the post-1973 world environment was, indeed, "a completely new ball game or a new world"—and floating rates would be judged as having greatly helped advanced economies in their handling of the financial aspects of the oil shock.

The financial aspects of the oil shock were part of what Safer and Mills (1975) had been referring to when they noted that OPEC's price rise had been seen as casting a shadow over the "economic and financial destiny of the industrialized world." In fact, by the time of the appearance of their article in the later months of 1975, the specter of an aggregate-demand crunch being imposed on the advanced economies by the oil shock had faded, and business columnist Joseph R. Slevin had noted via the title of his article that, after it had been considered "'Unmanageable' Not Long Ago, Petrodollar Crisis [Is] Vanishing" (*Kansas City Star* (Missouri), August 5, 1975).

The hypothesized "petrodollar crisis" pertained to the possibility that the oil shock might deal a large blow not just—as was already widely acknowledged—to the aggregate supply of importing economies but also to their aggregate demand, thanks to the flow out of the higher oil payments,

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[28] Quoted in *University of Chicago Magazine*, Autumn 1974, p. 2.

or petrodollars, from those economies. In January 1974, Friedman summarized the key source of the fears (Instructional Dynamics Economics Cassette Tape 138, January 16, 1974): "The Arab oil countries are going to be accumulating very large reserves."

Here, Friedman was moving away from his usual custom of using "reserves" in reference to bank reserves and instead deploying the term to mean foreign exchange reserves. And, as it happened, expressing the matter in terms of an accumulation of foreign exchange reserves was a relic of the fixed-exchange-rate era. What were really being referred to in these discussions were the dangers associated with the fact that the OPEC nations were likely, post-shock, to have much-enhanced oil export revenues. They would therefore tend, at least initially, to run large current account surpluses—surpluses that would have as a counterpart some kind of accumulation by the OPEC nations of financial credits to the rest of the world. In a floating-rate system, this accumulation would occur in the form of a capital account deficit—that is, net lending and investing abroad—not in an increase in official foreign exchange reserves.[29] In this situation, the increase in foreign-currency financial assets associated with a capital account deficit would arise from home residents sending back abroad the excess funds that they acquired through a surplus in the current account of the balance of payments. Consequently, that surplus would not lead to an increase in the country's aggregate foreign exchange reserves.

But whether taking the form of a larger capital outflow or higher foreign exchange reserves, this financial stockpile on the part of OPEC was seen as a threat to economic activity in the oil-importing nations. The increased current account surpluses of OPEC meant that a significant amount of spending in the oil-importing countries formerly devoted to home economic activity was now being devoted to OPEC product, and there was considerable concern this move would drain aggregate demand in the importing countries. Columnist Slevin sketched the scenarios offered: "Financial officials conjured up visions of economic collapse, of shattering shifts of huge amounts of money…" (*Kansas City Star* (Missouri), August 5, 1975.)

Among the officials expressing these concerns, Friedman named the International Monetary

[29] Although it was the OECD bloc that had undergone the most systematic shift to floating exchange rates in the early 1970s, oil-exporting Middle East countries evidently also conducted their international financial arrangements in a manner fairly closely resembling those prevailing under floating than fixed rates. The International Monetary Fund (1974, p. 24) noted: "For the oil exporting countries (predominantly in the Middle East), only a minor part of the 1972–73 rise in their current account surplus appears to have been reflected in any further growth of their overall balance of payments surplus… [T]he bulk of the increment in current account earnings was apparently absorbed by financial flows other than reserve accumulation." Over the period from 1973 to 1979 as a whole, however, OPEC's share of world foreign exchange reserves did increase very strongly (see Schwartz, 1983, p. 33).

Fund's managing director, H. Johannes Witteveen (Instructional Dynamics Economics Cassette Tape 138, January 16, 1974). This was, however, a widespread concern across officialdom. The World Bank, for example, suggested that oil prices at the post-shock level would lead to a cumulative OPEC surplus of $650 billion by 1980 (*Kansas City Star* (Missouri), August 5, 1975).

Friedman was, of course, optimistic from the start about the scope for floating exchange rates to facilitate the orderly flow of payments in the international monetary system. His reasoning was emphasized by Secretary of the Treasury George Shultz on March 3, 1973, in an Oval Office conversation with President Nixon and Nixon's national security adviser, Henry Kissinger. Shultz contrasted the feeling of crisis engendered among policymakers and exchange-market participants by recent events with the perspective taken by some economists: as the latter group saw it, what had happened "since August of '71 is that we have gradually made progress toward a more flexible system, and we made a lot more progress a couple of weeks ago." This was the way in which "say, Milton Friedman would describe" the process and the destination, Shultz explained, while also confirming, "I checked with Milton—he thinks the situation is great." Friedman's advice, he remarked, had been "Let them float"—indeed, "Force them to float" by not proposing a further revamp of the fixed-rate system.[30] Although, as discussed in Chapter 3, official efforts to salvage fixed exchange rates continued over the two weeks following Shultz's remarks, the authorities had conceded by March 17 that a floating-rate system was now in effect. The oil price increases that occurred over the following year, and the oil embargo in late 1973 and early 1974, provided an early test of the merits of the new system.

With regard to a key financial and economic dimension to this test—whether the floating-rate system could facilitate an orderly adjustment to the emerging OPEC current account surpluses—Friedman was confident. But he strongly discounted the basic premise that there would be sustained surpluses of the magnitude contemplated. An analysis that "has as an implication that you are going to have a net outflow of 60 or 80 billion dollars a year from one group of countries to another group of countries has something wrong about it. That will not happen," he declared (Instructional Dynamics Economics Cassette Tape 138, January 16, 1974). "Exchange rates will give. There will be greater conservation of oil. There will be more alternative [oil] supply. Something or other will happen. But one thing you will *not* have is any accumulations of that amount in the hands of the Arab countries. You may have *some* temporary surpluses while their

[30] In Rasmussen (2009, p. 76).

cartel policy is working. But it will not be of that magnitude."

Although these remarks embedded Friedman's erroneous baseline expectation that oil prices would fall before very long, they were prudent in not excluding the possibility that increases in OPEC current account surpluses near the $60 billion to $80 billion range might well occur in the short run. Shifts of about this magnitude *did*, in fact, occur in 1974, the first full year of the post-shock oil price. Specifically, estimates as of 1975 suggested that the OPEC current account surplus in 1974 was about $65 billion (Safer and Mills, 1975, Table 8, p. 29).[31] Later estimates put it at $70 billion (Federal Reserve Board, 1981, p. 349). This compared with a 1973 surplus of about $10 billion (Safer and Mills, 1975, Table 8, p. 29). In 1974, therefore, there was indeed a shift in current account flows of about $60 billion, in line with the lower bound of the $60 to $80 billion values that Friedman cited as being mooted at the start of the year and on which he had commented.

Friedman was also on the right track in suggesting that these OPEC current account surpluses would not last even if high world oil prices continued. The World Bank projections mentioned above had been predicated on the surpluses staying at elevated levels. Even Safer and Mills (1975, Table 8, p. 29), who suggested that the surpluses would diminish later in the decade, predicted that the 1975 surplus would be essentially the same as in 1974. In the event, however, the current account surplus more than halved to $31 billion in 1975 and stayed at around that level in 1976 and 1977, before moving to single-digit values in 1978 (Federal Reserve Board, 1981, p. 349).[32] Although the development—a 1974 oil price decline—that he cited as most likely to prevent these surpluses from being lasting did not eventuate, Friedman was right that, as he had put it, something would "give" and have the effect of lowering the surpluses significantly. OPEC's consumption of output produced by the oil-importing countries increased greatly. After a year or so, a high proportion of their oil revenues was being spent on purchases of goods and services rather than being put into financial assets: "a spending spree by the oil-exporting countries" on their customers' output, as one retrospective cast matters (*American Banker*, April 30, 1980). In the wake of these purchases, OPEC's current account surplus was back to around its moderate 1973 level by the time of the second oil shock in 1979.

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[31] Earlier, Secretary of the Treasury William Simon had given an estimate of $60 billion. See his testimony of January 30, 1975, in Committee on Finance, U.S. Senate (1975b, p. 4).

[32] Consequently, by the time Hirsch's (1977, p. 253) reference to "the huge continuing surpluses of most of the oil exporting countries" saw print, those surpluses were, in fact, rapidly diminishing.

In the initial wake of the first oil shock, however, there was indeed a period during which the OPEC current account surplus attained an extremely high level—accompanied by concerns in international policy circles and economic commentary that this would produce a major contraction in aggregate demand in the advanced economies. These concerns were prevalent over the whole of 1974.

An early contrary judgment was registered by Friedman during the first quarter of the year, soon after the oil embargo of 1973–1974 had been lifted. He suggested that the floating-rate system had weathered the preceding months of the embargo, as well as sharp fluctuations in the world price of gold, admirably. Friedman contrasted this performance with what might have resulted if the past year had seen the continuation of the Bretton Woods system—under which the same commodity market developments could well have led to exchange-rate crises and emergency policy summits (*Newsweek*, April 1, 1974).

Many economists, however, were at this stage likely to agree only provisionally with Friedman's accompanying judgment that it had been established that "the world continued about its business" in the face of the oil shock (*Newsweek*, April 1, 1974). One major feature that Friedman listed as a success of the new system—the absence of large inter-country swings in foreign exchange reserves—was certainly a valid, and permanent, distinguishing feature of the floating-rate system and marked it out from all earlier, fixed-rate systems. But, as already noted, the oil shock would still see large international payments flowing out of the importing countries during 1974, even if these did not show up primarily as changes in foreign exchange reserves. And OPEC's oil price rise endured beyond the lifting of the embargo in early 1974. The continuation of high oil prices pointed toward the possibility that the outflow of oil payments might yet prove to be a major drain on total demand in the major importing economies.

Proponents of the petrodollar problem therefore continued to express concern about the OPEC surpluses over the rest of 1974 and into 1975. In late November 1974, Arthur Burns articulated his own continued worries. Burns granted that "financial flows to and from OPEC countries have been handled mainly—and also reasonably well—by private markets," and also that there was already evidence that the scale of these flows was being held back by a large increase in OPEC demand for imports.[33] He nevertheless saw very large surpluses in the years ahead under current oil prices and, in the event that those high prices persisted, Burns judged it "extremely

[33] In Burns (1974; quotation from p. 6, remark on imports on p. 4).

doubtful whether the financial problems released by the huge increase in the price of oil will prove manageable."[34] At the end of the year, Harold M. Williams, dean of the graduate school of management at the University of California, Los Angeles, was even more pessimistic. Williams noted a projection of a $1 trillion cumulative transfer to OPEC over the following decade and contended: "Massive transfers of wealth to [OPEC]… will precipitously drain the resources of oil-importing countries. It is unrealistic to think that these petrodollars going to OPEC members can be channeled into global investment and production through normal channels." (*Los Angeles Times*, December 22, 1974, Part IV, p. 1.)

But even as these words were being published, unfolding events indicated that the fears outlined were not becoming reality. By late January 1975, Secretary of the Treasury William Simon was able to note that "the massive shifts in financial assets did not lead to the financial crises that some envisioned."[35] In part, as already noted, this was because, after the first year, a large amount of funds flowed back to advanced economies, thanks to substantially higher import purchases by OPEC. But it also reflected the fact that the financial "recycling" of OPEC surpluses through capital flows into the OECD economies—a process about which Arthur Burns and Harold Williams had expressed doubts—proved to be feasible and orderly.[36] In January, Simon noted that "these surpluses were not somehow withheld by OPEC but were placed somewhere in one [or other] of the oil-importing countries."[37] By the summer of 1975, columnist Slevin could pass judgment: "Far from collapsing, the free world is pulling out of the recession that the OPEC price hikes helped bring on… Private and government borrowers are absorbing the Arab surpluses without blinking an eye." (*Kansas City Star* (Missouri), August 5, 1975.)[38]

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[34] Burns (1974, p. 8).

[35] From Simon's testimony of January 30, 1975, in Committee on Finance, U.S. Senate (1975b, p. 5). There were certainly major banking problems in the United States over these years—but not ones for which the shift of funds to the OPEC countries played an important role. See the previous chapter.

[36] Mussa (1986a, p. 112) correspondingly dated the start of recycling to 1974.

[37] From Simon's testimony of January 30, 1975, in Committee on Finance, U.S. Senate (1975b, p. 5). About five weeks later, the Brookings Institution's C. Fred Bergsten, despite offering a generally positive verdict on the record of floating rates to date, suggested that the recycling was not working so far as the United States was concerned. In testimony of March 5, 1975 (Joint Economic Committee, U.S. Congress, 1975g, p. 958), he stated that "the rush of petrodollars into the United States has never materialized." Unlike several other major countries, however, the United States ran current account surpluses in the 1973–1976 period surrounding the first oil shock (Council of Economic Advisers, 2011, Table B–24, p. 218). It therefore did not have occasion to receive a capital inflow coming largely from the OPEC nations. In the event, a substantial flow of OPEC funds did come into the United States in 1974 via the current account, with the OPEC block's imports from the United States rising by 85 percent in that year (see page 187 of the written responses of William Simon and Alan Greenspan Committee on the Budget, U.S. House of Representatives, 1975).

[38] Slevin, like Simon, noted the diminution, currently in process, of the surplus: "the OPEC countries… are spending their earnings at a surprisingly rapid clip [on purchases of goods and services from the OECD]."

The process took place largely without public-sector guidance—with Hirsch (1977, p. 253) somewhat grudgingly acknowledging that the OPEC "funds [have been] channeled.. since 1974, through the private sector and predominantly through the banks in unmanaged market processes."

Furthermore, the financial system proved to be capable of directing the surplus funds, once invested, to those countries that were in need of financing their current account deficits (see, for example, Johnson, 1975, p. 61). In particular, there was heavy recycling of oil revenues to developing economies (Obstfeld and Rogoff, 1995, p. 1732).[39]

In light of this outcome and the contrast with the previous fears about the practicability of recycling, the process was judged to have been a success. One practitioner, Willard C. Butcher (president of the Chase Manhattan Bank), contended: "When oil prices quadrupled in 1973, the international banking system rose to the challenge and took a leading role in what, despite initial forebodings, emerged as a surprisingly smooth recycling process in view of the unprecedented size of the financial surpluses and deficits then accumulating. While it wasn't possible to avoid a worldwide recession, it was clearly mitigated." (*American Banker*, September 29, 1980.)

Although Butcher's commentary was praising his own industry of commercial banking, the fact that he could do so reflected the reality that it had become uncontroversial to regard the financial adjustment to the first oil shock as having been successful and to see this outcome as largely the result of what another senior banker called the "successful recycling of the 1973−75 period" (*American Banker*, September 29, 1980*b*).[40]

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[39] U.S. commercial banks played a major role here (see, for example, Mussa, 1986a, p. 112). One of the prime movers in recycling the OPEC funds to developing economies, particularly in Latin America, was Citibank, then headed by Friedman's friend, Walter Wriston. This was a matter on which commentators would, during the Latin American debt crisis of the 1980s, find fault with Wriston, on the grounds that he had underestimated the credit risk involved in making loans to the governments in question (for example, *Sydney Morning Herald*, March 12, 1987).

[40] The recycling process involved, to a considerable degree, not only commercial banking activity centered in the United States but also actions taken by banks in the Eurocurrency markets (into which large volumes of OPEC proceeds had been placed). Their part in the recycling achievement was stressed by Eurocurrency lenders in 1979−1982, when a new oil shock had emerged and, in addition, industry leaders had reason to stress the strengths of an unregulated Eurocurrency market, as they found themselves coming under renewed scrutiny from a monetary-control perspective. For example, Dennis Weatherstone of Morgan Guaranty Trust stated in testimony of June 26, 1979 (Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1979c, p. 21): "The Euromarket also has helped the world cope with serious international payments imbalances, particularly since the quadrupling of OPEC oil prices at the end of 1973. In recycling surplus funds from members of OPEC as well as from several large industrial countries to oil-importing countries in deficit, the market has permitted the subsequent adjustments in the borrowing countries' domestic economies to take place more gradually and with less disruption." Likewise, Serge Bellanger, of Credit Industriel et Commercial, observed: "While the Cassandras were wringing their hands, ... the world's major commercial banks assumed a key role in accepting deposits of surplus funds from OPEC

A related development—one that led to wider support for one of Friedman's longstanding positions—was that a highly favorable verdict on floating exchange rates came out of the first oil shock and the associated successful feat of surplus recycling. Bernanke (1982, p. 154) would observe: "The international financial system proved its worth, despite dire predictions, during the aftermath of the 1973–74 oil price shock; the tremendous oil payments were 'recycled' through the international system without substantial problems."

Similar judgments had been expressed in U.S. and world officialdom in the years following the first oil shock. For example, the Council of Economic Advisers (1976, p. 140) stated: "The financing of the large external deficits of oil importers over the past two years has been accomplished considerably more smoothly than had been anticipated earlier. Financial markets turned out to be very adaptable, and the more flexible exchange rate system helped to avoid the market disruptions so often experienced during past periods of strain." Similarly, Jacques de Larosiere, managing director of the International Monetary Fund—an institution that, as Friedman stressed, was a longtime defender and guardian of the Bretton Woods system—similarly granted: "We all recall the concerns that were expressed about the ability of the system to accommodate the first round of oil price increases, but the response proved flexible and financing flows moved apace with the emerging demands." (*American Banker*, November 18, 1980.)

Friedman, who had remarked at an early point of the recycling debate that "so long as you have floating exchange rates, you will not have [an] international monetary crisis" (Instructional Dynamics Economics Cassette Tape 138, January 16, 1974), was reinforced in his position that the new system was well situated to handle the financial implications of the oil price increase. Floating rates had been "effective shock absorbers" in the face of recent years' events such as the OPEC shock, and he again contrasted them with a Bretton Woods-type system, which would have been deeply disrupted by events like the oil price increase (*Wall Street Journal*, June 30, 1975).

The role of floating exchange rates as shock absorbers

Friedman's 1975 judgment that flexible exchange rates had been useful shock absorbers in the

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and lending them to oil-importing countries. The Eurocurrency market recycled OPEC's petrodollars on a massive scale, and meanwhile, OPEC expenditures on imports rose much faster than anticipated." (*American Banker*, March 26, 1982.)

previous two-and-a-half years rested on more than just the manner in which they had helped recycle OPEC's oil revenues. It also related to the fact that floating rates had given individual countries the ability to stabilize aggregate demand conditions at home even in the face of external shocks like the oil shock.

Much of the policymaker concern that a demand crunch would be generated by the OPEC oil shock had been on traditional Keynesian-multiplier lines: the oil purchases would add to trade deficits, and this excess of imports over exports would, on Keynesian reasoning, constitute a source of leakage for the flow of national income in the importing countries, with a multiplied negative effect on those countries' aggregate demand.[41] This was reasoning that Friedman rejected.[42] He had eschewed the multiplier view of spending determination as an analytical device for decades, and he had challenged it on empirical grounds in his 1963 paper with David Meiselman. With the oil shock occurring under flexible exchange rates, Friedman's view of its aggregate demand implications was very different. Under a floating rate, current account deficits had no tendency to generate balance-of-payments deficits and hence monetary contraction—so their aggregate demand implications were strictly circumscribed.[43] In the face of an increased current account deficit, the authorities of a floating-rate economy were able to steer aggregate demand independently of the behavior of oil import payments, by using the autonomy over monetary policy (including specifically the ability to determine the money stock) that was conferred to them by a floating exchange rate.[44]

Indeed, Friedman's long-held contention was that, in an era in which domestic governments took responsibility for the management of aggregate demand, they would find command over the money stock so important a power to have in the exercise of this responsibility that they would insist on such power even under a fixed-exchange-rate regime. From this contention flowed his

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[41] Walter Heller, for example, stated: "The oil potentates, for example, are exacting a tribute of about $25 billion a year. And this is money that isn't coming back into our economy." (In Shane, 1975, p. 475.)

[42] In the case of the oil shock, some of the disagreement among economists on its demand implications was obscured by the existence of considerable common ground among them, and among many policymakers, in likening the shock to a tax increase. For example, U.K. Chancellor of the Exchequer Denis Healey stated at a G5 meeting on September 28, 1974: "These surplus revenues cut economic activity just like an increase in taxes." (Galpern, 2012, p. 35.)

[43] See Chapter 15 for further discussion of monetarist skepticism about linkages between current account deficits and aggregate demand.

[44] Some of the concerns that there would not be large-scale recycling, although they still implied a negative multiplier response of the economy to higher imports, were likely of a somewhat different character. To some, large current account deficits may have been considered so hard to finance that a shock that tended to produce larger deficits was likely to produce a contraction of the home economy (possibly reinforced by policy actions) on a scale sufficient to ensure that current account deficits remained small.

position that the support in many policymaker and business circles for major nations' commitment to fixed exchange rates implied, for practical purposes, directing those nations toward arrangements in which they imposed extensive foreign exchange controls on their citizenry and on foreign investors. Such controls helped separate the behavior of a country's money stock from the state of its balance of payments.

This position, which had permeated Friedman's 1953 essay on exchange rates, would be challenged by some advocates of fixed exchange rates as being out of date by the time the Bretton Woods system had been in operation for around two decades. Robert Mundell, whose analytical open-economy research would often be treated as supportive of floating exchange rates, had by early 1966 moved from favoring flexible rates to advocating fixed rates.[45]

Subsequently, Mundell used the occasion of a September 1968 conference at Ditchley Park, United Kingdom, that Friedman attended to argue that fixed exchange rates no longer in practice went hand in hand with controls on payments flows. "It is not hard to understand Friedman, Meade, and Lutz, living in a world of noxious controls and advocating, back in the 1950s, a

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[45] Mundell (1997, p. 32) identified an early occasion on which he signaled his opposition to floating exchange rates with his refusal to sign a public letter of support for greater exchange-rate flexibility in 1966. This letter, of which Friedman was one signatory, was published in the *New York Times* (February 21, 1966) and reprinted in Joint Economic Committee, U.S. Congress (1968b, pp. 90–91).

Paul Krugman (Voxeu, April 12, 2021) suggested that Mundell switched from advocacy of flexible rates to support for fixed rates "[a]t some point in the 1960s" but argues that the absence of a "paper trail," due to the dissipation in Mundell's written output, makes pinpointing the date difficult. Along similar lines, Fritz Machlup in 1979 suggested that "around 1967" Mundell was still an advocate of floating rates and had been a rare instance of an economist who had become "disenchanted" with floating rates in the period since then (see Triffin, 1979, p. 71). There does, however, exist a substantial body of Mundell public statements over the 1960s, and they support Mundell's (1997) retrospective dating of his change in position to about 1966. In particular, on November 15, 1963, Mundell testified in favor of greatly increased adjustability of exchange rates, and he described his proposal as "very similar" to Friedman's stand in favor of floating rates (Joint Economic Committee, U.S. Congress 1963, p. 572). In contrast, as already indicated, Mundell (1968b) would favor fixed rates, with currency unification as the ultimate goal.

The move by Mundell in the direction of favoring continuation of fixed exchange rates was registered in a revision of a talk that he gave on December 12, 1964. At this stage, he still seemed to want to reduce the role of gold in the Bretton Woods system (Mundell, 1965, p. 6) and suggested also that "a pure gold standard in its original form has to be ruled out" (Mundell, 1965, p. 58). But he distinguished his own position from Friedman's advocacy of floating exchange rates (p. 5) and indicated that he still wanted a gold-linked system, albeit one containing more "room for exchange-rate adjustments" between three key blocs of countries (p. 58).

Subsequently, a sign of Mundell's break, in 1966, from the tendency of both Keynesians and monetarists to call for greater exchange-rate flexibility and enhanced autonomy of individual countries' monetary policies was his incensed reaction to a memorandum on international economic policy that Franco Modigliani had prepared for the Federal Reserve Board consultants' meeting of October 20–21, 1966. "I dissent completely [from] the logic and relevance of Professor Modigliani's proposal," Mundell wrote on December 15, in a post-event memorandum to meeting participants (Federal Reserve Board records).

regime of flexible exchange rates. But the world of the late 1960s is completely different."[46] Similarly, Milton Gilbert, an economic adviser to the Bank for International Settlements who had been pitted against Friedman at an American Economic Association meeting panel on exchange rates in December 1964, had observed in his panel remarks that Friedman supported moving to a pure floating regime and commented: "I cannot imagine, however, that it is a plan with great appeal to members of our profession."[47] Gilbert later contended that while the main arguments in Friedman's 1953 essay had "often been called the classic case for floating," those arguments had soon been superseded by the fact that Bretton Woods country members liberalized trade and capital flows after 1953.[48] Late in 1969, Gilbert stated (1970, p. 133) that, under the Bretton Woods system, "trading practices have been liberalized and exchange restrictions enormously reduced."

These generalizations notwithstanding, it remained the case in the 1960s that numerous countries continued to have a considerable exchange-control apparatus. Furthermore, that decade saw prominent cases of intensifications of controls. In the United Kingdom, exchange controls were tightened in the mid-1960s in the face of downward pressure on the sterling exchange rate (Crawford, 1983, p. 423; Allen 2019, p. 134).[49] And the United States also saw major measures in this direction during the 1960s, with Friedman remarking in 1986: "It's hard to recall that Presidents Kennedy and Johnson introduced a series of measures that were the equivalent of exchange controls," including interest-equalization taxes.[50] In both the United Kingdom and the United States, the increased use of exchange controls was used as a means of addressing balance-of-payments deficits without relying on adjustments to domestic monetary conditions.

Friedman did acknowledge that there was some notable alignment of monetary policy across countries during the Bretton Woods years. In particular, he observed that this period had seen a good deal of harmonization of average inflation rates and summarized those rates as having been "more or less the same" across nations.[51] But even this similarity in price trends was qualified by the fact that the U.S. inflation rate achieved under Bretton Woods through the mid-1960s was not much different from what policymakers in other countries would have found desirable to have as

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[46] Mundell (1968b, p. 22).
[47] Gilbert (1965, p. 187).
[48] Gilbert (1980, pp. 25–26; quotation from p. 25).
[49] Among the measures taken in this period was a tightening of the foreign currency totals that U.K. residents were permitted to acquire for tourism activities (see Solomon, 1977, p. 92). Such restrictions underlined Friedman's view of exchange controls as an inhibition not only on the allocation of capital but also on households' personal freedoms.
[50] In Hinshaw (1988, p. 195).
[51] Friedman (1975c, p. 27).

a domestic economic goal. And differences in average monetary policy performance did, in fact, emerge even during the less turbulent pre-1967 years of the Bretton Woods system. These differences were reflected in the German mark's revaluation in 1961 and the emergence, a little later, of what Friedman and Schwartz would regard as a lasting shift upward in U.K. monetary growth, to rates above U.S. values, in the years leading up to the pound sterling's devaluation in 1967.[52] It was consequently not clear that domestic authorities were really willing to surrender monetary sovereignty under the Bretton Woods regime.

Friedman also acknowledged that the late Bretton Woods era did see, contrary to his generalization about modern-day conduct of stabilization policy, some major cases in which exchange-rate considerations were allowed to drive the behavior of the money stock. He noted, in particular, that during the early 1970s, both the Federal Republic of Germany and Japan had allowed a commitment to a fixed exchange rate to spill into rapid monetary expansion and higher inflation rates for themselves.[53] But even these instances provided evidence of countries' low tolerance for subordinating monetary and aggregate demand management to the exchange rate. Neither country allowed the exchange rate to dominate monetary policy on a longer-term basis. Germany's response to the rapid monetary growth was to tighten its exchange controls and, subsequently, to float the mark, while Japan, which already had very extensive exchange controls but found them overwhelmed by the vast capital inflows that it experienced, moved to a floating yen at the same time the mark floated. In the days following these market moves, which helped end the Bretton Wood system, Friedman judged that the episode "was a good thing. It forced governments to face market realities."[54]

Furthermore, as it became clearer that a follow-on fixed-rate system covering most major economies would not be forthcoming, foreign exchange controls were relaxed. They were removed in the United States in the early years of floating (see Chapter 3), and in March 1975 Secretary of the Treasury William Simon affirmed: "I have made clear that the administration has no intention of imposing capital controls—on inward or outward flows."[55]

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[52] On the U.K/U.S. comparison, see Friedman and Schwartz (1982, p. 157).
[53] For his remarks about Germany importing inflation through 1971 by letting balance-of-payments surpluses add to monetary growth, see, for example, Friedman (1975e, p. 27) and Instructional Dynamics Economics Cassette Tape 74 (May 20, 1971). For Friedman's commentary on the excessive monetary growth that Japan experienced as a result of to pre-March 1973 exchange-rate policy, see, for example, Instructional Dynamics Economics Cassette Tape 146 (May 20, 1974) and *Newsweek*, September 4, 1978.
[54] Friedman (1973d, p. 14).
[55] Testimony of March 24, 1975, in Joint Economic Committee, U.S. Congress (1975h, p. 6).

Other countries, too, removed ended their exchange controls in subsequent years. These countries included the United Kingdom (in 1979) and Japan (in 1980).

Anchoring inflation under floating exchange rates

With regard to macroeconomic behavior under the new floating-rate regime, Friedman acknowledged in the March 1973 remarks referred to above an apparent missing element in much monetary analysis: he observed that key writers of the past, such as Keynes and Fisher, had lacked an explicit treatment of the open economy.[56]

In making this observation, Friedman implicitly indicated that the same characterization also applied to himself. John Williamson—a younger-generation advocate of fixed exchange rates whose unhappiness with the emergence of the floating system was evident in the title of his 1977 book *The Failure of World Monetary Reform, 1971–74*—made this point explicit in a further book, this one an open-economy-oriented macroeconomics text, that Williamson produced in 1983. Williamson (1983, p. 372) observed: "The two leading macroeconomic theorists of the twentieth century, Keynes and Friedman, both constructed their principal theoretical analyses for closed national economies. An unfortunate legacy… is that analysis of macroeconomic phenomena… all too often still pays insufficient attention to the international dimension, despite the best efforts of many international economists over the years."

A decade before Williamson's complaint, Friedman's March 1973 remarks had also suggested that he had a reasonable basis for lacking a detailed account of monetary behavior in an open economy. Specifically, his 1973 discussion contended that, under the combination of a floating exchange rate and a monetary policy directed at internal conditions, closed-economy results concerning price-level determination carried through, with the application of the quantity theory of money to this setting being "entirely valid."[57] In particular, the amount of nominal aggregate demand in an economy could be shaped by monetary policy under a floating exchange rate, and, as Friedman had stressed both in 1953 and in the mid-1970s remarks quoted earlier in this chapter, inflation was determined very largely by the decisions of the domestic monetary authorities.

On this point, Friedman parted company with Keynesians like Walter Heller and James Tobin

[56] Friedman (1973d, p. 8).
[57] See Friedman (1973d, p. 9).

who, as noted above, stressed global cost-push factors as common drivers of inflation across the world. He also was taking a position very different from Robert Mundell and others, including Arthur Laffer, who, although highly sympathetic with the notion that monetary factors drove inflation, had by the mid-1970s become convinced that the private sector's decision-making on portfolio composition had become such an internationally oriented process that inflation rates were linked across countries, irrespective of the exchange-rate regime in force, with these individual inflation rates most appropriately being viewed as components of a world inflation rate. The world inflation rate, on this hypothesis, was governed by the behavior of global aggregate monetary growth, to which particular countries could contribute but that they could not individually determine.[58]

Friedman, in contrast, remained convinced that while the notion of world inflation or world monetary growth might well be useful in a situation of fixed exchange rates, in a flexible-exchange-rate system nominal variables' behavior in each country arose principally from developments in that country, and the world aggregates were of questionable analytical significance: "Under a floating-rate system, there *is* no such thing as a world money supply."[59] On the price side, in 1979 Friedman went so far as to say that, under a float, "I do not believe there is any such thing as world inflation; there is only inflation in individual nations."[60] Correspondingly, he and Rose Friedman affirmed the following year in the *Free To Choose* book that "each country separately" could determine its own inflation rate.[61] This contention would be largely supported by studies of the international transmission (or lack of transmission) of inflation that covered the first decade or so of floating rates (see Darby and Lothian, 1983, 1989).[62]

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[58] For Laffer's advocacy of this position, see, for example, Laffer and Miles (1982, pp. 262–277). Mundell's endorsement of the idea appeared in Birnbaum and Laffer (1976, p. 82) and in other places.
[59] Instructional Dynamics Economics Cassette Tape 190 (May 1976, Part 1).
[60] *Director* (Institute of Directors, London), December 1979, p. 35. Friedman and Schwartz (1982, Chapter 7) took an apparently more open-minded stand, presenting exploratory empirical analyses of U.K./U.S. economic interrelationships and concluding that the two nations' price changes had linkages to one another for given home monetary growth. See especially their page 336. But very little of the period covered consisted of floating exchange rates, and some of the cross-country linkage that Friedman and Schwartz contemplated (such as common elements affecting velocity in each country—see their pages 334–335) was consistent with monetary policy autonomy in each country prevailing under floating.
[61] Friedman and Friedman (1980, p. 263).
[62] Another notable item, to which Friedman (1984a, p. 59) would himself refer on this point, was an unpublished study by Goldstein and Haynes (1983). These authors found a "marked increase in the dispersion of annual inflation rates after the adoption of floating rates, again suggesting an increase in policy independence under that regime" (p. 4).

The international role of the dollar

The Bretton Woods system had been set up on the premise that, in the postwar world, the U.S. dollar would be the leading international currency. The dollar's claim for this status strengthened over the years spanning the Bretton Woods arrangements—so much so that Clark Warburton's (1953, p. 19) statement that there was "no national currency which is so universally used in international commercial contracts" as the pound sterling once had been had eventually become out of date by the 1960s. Indeed, one aspect of the latter half of the Bretton Woods era that was seen as a sign of weakness of the dollar—the occurrence of a deficit in the U.S. balance of payments—was, despite giving rise to titles of such volumes as *The Dollar in Crisis* (see Harris, 1961), something that Friedman and Robert Mundell agreed could actually indicate *strength* of the dollar (at least in the case of deficits of moderate size) and did not in itself demand U.S. policymaker action—their reasoning being that a payments deficit might reflect other countries' determination to acquire U.S. financial assets.[63]

In contrast to Mundell, however, Friedman saw the dollar's preeminence in international transactions as likely to be consolidated in a system of floating rates. "The emergence of a system of floating rates in a very important sense strengthens the position of the dollar as the international medium of exchange," Friedman remarked at the onset of the system (*Japan Times* (Tokyo), March 16, 1973).

In giving this assessment, the proviso that Friedman continued to atttach was that the strong international status of the U.S dollar required that the United States avoid substantial inflation, so that it became a benchmark currency associated with stable purchasing power.[64] The periods of near-double-digit U.S. inflation rates cast some doubt on the dollar's future as an international benchmark currency. Even in this troubled period, however, the dollar was heavily favored by countries as the currency used in international transactions as well as in actual holdings of foreign exchange reserves.[65] Subsequently, the U.S. dollar was indeed confirmed as the *de facto*

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[63] On Friedman's expressions of this point, see Nelson (2020b, Chapters 11 and 13). Mundell articulated the same point in Mundell (1968c, p. 42). Triffin (1960) had strongly propounded the basic point (while dissenting from Friedman's policy conclusions, as Triffin favored fixed exchange rates).
[64] See also *Newsweek*, January 29, 1968.
[65] In this connection, Murphy (1979, p. 24) noted during the high-inflation period: "To a surprising degree, the dollar has retained the roles it played during the Bretton Woods period. Although no longer redeemable in gold at a fixed price, in 1977−1978 the dollar continued to be the leading medium chosen by governments for their international reserves." Murphy judged that the dollar figured very heavily in market economies' current account

reserve currency in the floating-rate era.[66]

The role of gold in the international monetary system

The strength of the U.S. dollar as an international reserve currency therefore continued, despite there now being no link of the dollar to the provision or price of gold. Friedman had noted in testimony supporting floating exchange rates in 1963 that, as part of the United States extricating itself from its Bretton Woods obligations, "We would have to get out of the gold market, and we would have to get out of the exchange market."[67] As he saw things, the United States' commitments regarding gold principally mattered for the way in which the country conducted its international transactions. He did not see Bretton Woods as a *bona fide* Gold Standard system that constrained domestic monetary policy, and, in particular, he saw the Federal Reserve, during the Bretton Woods era, as able to make, and in practice usually actually making, the determination of the U.S. money stock separate from gold market developments and from international payments more generally. "There is very little relationship to a gold standard in our money system," he had remarked in early 1964, but "the fiction that gold has something to do with money" was maintained in that system by the pegging of the price of gold.[68]

By the time of the start in earnest of floating exchange rates in March 1973, the gold price had already been unpegged, thanks to the previous changes to the system made in 1968 (when the international privately traded gold price was unpegged) and 1971 (when the U.S. government

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transactions as well (p. 25). Schwartz (1983, p. 33) similarly noted that the "main source of growth in foreign currency reserves since 1973, as in earlier years, has been in the form of dollars."

[66] As discussed in Chapter 15 below, the means by which other countries acquired dollar assets in the floating-rate era proved to be very largely through the capital account surpluses that the United States ran as a counterpart of recurrent current account deficits. This contrasted somewhat with Friedman's expectation as of the early 1970s. He had then expected that the United States would run current account surpluses and that other countries would acquire U.S. financial assets by fixing their exchange rates against the U.S. dollar for stretches of time (see Nelson, 2020b, Chapter 15). An aspect of the modern financial system that his pre-1973 analyses did capture, however, was that a current account deficit did not inevitably connote a weak currency and that a capital inflow could be a source of exchange-rate, and possibly economic, strength. He was able to explain in this way the 1980s combination of a current account deficit and a strong dollar (again, see Chapter 15 below) and remained comfortable with persistent U.S. current account deficits through the end of his life, in large part because, as he put it, under floating rates, the current account "deficit is not—it`s not a deficit in another sense. It is a capital surplus." (*The Charlie Rose Show*, PBS, December 26, 2005, p. 2 of transcript.) In contrast, C. Fred Bergsten of the Brookings Institution, in his testimony of March 5, 1975, inaccurately foresaw a post-oil shock longer-term future in which there would be "secular downward pressure on the dollar" that the United States would have to forestall by running sizable current account surpluses (see Joint Economic Committee, 1975g, p. 958), while Williamson (1977, p. 82) took for granted that there was a permanent tendency toward U.S. current account surpluses irrespective of exchange-rate regime.

[67] From his testimony of November 14, 1963, in Joint Economic Committee, U.S. Congress (1963, p. 518).

[68] *The Freedom School Newsletter* (Rampart College, Colorado Springs, Colorado), March 15, 1964, pp. 1, 4.

stopped making gold available to other governments at a fixed price). A couple of years into the operation of the flexible-rate system, Friedman considered the possibility of the return to a true Gold Standard. He strongly discounted such an occurrence: "As the U.S. is today, I do not believe that there is any possibility that a government will accept the discipline of gold, that it will in fact give up its authority to [decide when to] print money."[69]

This conclusion underscored, in his view, the need for a constant-monetary-growth rule: "I believe that we nonetheless have to discipline governments... The only way I have been able to see to discipline them is by adopting a mechanical rule to which they are required to adhere, provided it is a rule that can be checked on."[70]

Friedman had previously called a system in which not even lip service was given to linking monetary policy to exchange rates or commodity prices a "paper standard."[71] Building on this terminology, with which he was likely familiar in part through his years through 1970 as a colleague of Friedman's, Robert Mundell would call the variant of this standard in which the monetary authorities provided constant monetary growth "a Friedman paper standard" (*Wall Street Journal*, January 31, 1983). Mundell was not, however, using this term to describe actual practice in the decade after 1971−1973. Although he certainly regarded Friedman as having been too influential on the conduct in the United States and elsewhere of domestic and international monetary policy, Mundell was not suggesting that Federal Reserve policy in the floating-rate era followed the Friedman paper standard. Indeed, although numerous OECD countries would adopt monetary aggregate targeting in the years following the start of floating, in every case their implementation of this policy involved marked differences from Friedman's domestic-policy prescriptions, and it was in fact actual practice in these years that Anna Schwartz used to demonstrate her comment (1983, p. 43): "Even when autonomy exists, monetary policy may perform badly." In the United States specifically, the execution of monetary targeting diverged significantly from Friedman's recommendations from the beginning, and his initial optimistic attitude about the policy move dissipated (see Chapter 5 above).[72]

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[69] *Reason* magazine, June 1975, p. 91.
[70] *Reason* magazine, June 1975, pp. 91, 92.
[71] *The Freedom School Newsletter* (Rampart College, Colorado Springs, Colorado), March 15, 1964, p. 2. Friedman's detailed treatment of commodity standards had been Friedman (1951a), and he had contrasted arrangements since the New Deal, including the Bretton Woods system, with a true Gold Standard in Friedman (1960a, pp. 77−84; 1961a) and Friedman and Schwartz (1963a, pp. 472, 789).
[72] It would not therefore be appropriate to conclude, as Williamson and Miller (1987, p. 40) did, that after 1973 central banks "had adopted national money supply targets as the new nominal anchor to replace a fixed exchange

It was true, however, that the United States and other countries had moved to a policy arrangement that had once been seen as a very unlikely permanent regime. In December 1936, John H. Williams had observed that "internal money management without intervention in the exchange market… appear[s] to be outside the range of practical consideration."[73] Four decades on, this combination had become the practical reality. Even if governments of major countries were not really accepting his recommendations regarding domestic monetary policy, the fact was that what Friedman referred to as an "honest-to-God paper standard"—in which the monetary system was disconnected from commodity prices or undertakings regarding exchange-rate parities, and in which policymakers made this disconnection clear—was in force.[74] This was a setting that the U.S. authorities had previously associated solely with transitory arrangements that would prevail solely in severe crisis conditions.[75]

Speaking on Australian television in April 1975, Friedman pointed to "the arguments I used to hear ten and fifteen years ago about how it was utterly academic dreaming to suppose you could have a system of flexible exchange rates in the world. The truth is that it is the 'unrealistic academic'—who can look at the situation in its broadest context and get away from the immediate policy decision—who is the most realistic."[76] Similarly, he told a Congressional testimony the following January that he believed that "ivory-towered theorists are often more realistic than the most practical of men."[77]

And with regard to what he had previously called pseudo-gold standards—systems like Bretton Woods—Friedman judged these to be "far worse" than the paper standard at a basic level. The basis for this assessment was that the Bretton Woods system was not transparent: the limitation

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rate, thus approximating the policy regime long advocated by Milton Friedman (1953[a])." The quoted conclusion was unwarranted not only because the practice of monetary targeting deviated from his recommendations, but also because Friedman's recommendation of a constant-monetary-growth rule was actually something that he developed some years after Friedman (1953a).

[73] Williams (1947, p. 212).

[74] The quotation is from *Reason* magazine, June 1975, p. 94.

[75] In particular, Friedman (1960a, p. 78) characterized the period of installation of the New Deal monetary changes (March 1933 to January 1934) and the Civil War-associated "greenback period" as the occasions on which "we were on an inconvertible paper standard with flexible exchange rates." The notion that floating rates could only hope to have the status of a stopgap arrangement had also been expressed forcefully by Lionel Robbins in January 1967. Robbins stated that "it is clear to me that a general system of freely fluctuating rates between existing national areas is not permanently viable. It is just not on." Robbins envisioned floating rates as having a role at best "as a temporary substitute for a fixed devaluation of one currency" (in Hinshaw, 1967, p. 11).

[76] *Monday Conference*, Australian Broadcasting Commission, April 14, 1975, p. 25 of transcript. He made a similar remark in Friedman (1984a, p. 53).

[77] From Friedman's testimony of Committee on Banking, Currency and Housing, U.S. House of Representatives (1976a, p. 2179).

on money creation ostensibly implied by fixed exchange rates and a pegged gold price proved to be, in fact, hardly any restriction on the average stance of U.S. monetary policy and had only haphazardly constrained monetary conditions in other countries.[78]

Beyond the mid-1970s, some support for fixed exchange rates lingered among U.S. economists in academia and policy think tanks. Support for fixed exchange rate systems linked to gold was, however, much more limited. The most outspoken advocates of such systems in the mid- and late 1970s were Laffer and Mundell. Both of them were convinced that, in contrast to Friedman's assessment, the good record of economic and price stability seen between the Korean War and the later 1960s reflected the operation of the exchange-rate system—with Mundell (1992, p. 64) identifying "the collapse of monetary discipline when the Bretton Woods system broke down in 1971 and flexible exchange rates came into being in 1973"—and both of them ascribed a much more material role to gold in that system (particularly the pre-1968 version of that system) than Friedman believed gold actually had possessed during the Bretton Woods years.

Beginning roughly with the start of the floating-rate era, both Laffer and Mundell were increasingly concentrating on public-policy debate rather than research forums. Toward the end of Laffer's time as a member of the University of Chicago's business school, this tendency was reflected in the April 1975 conference on international monetary matters, already mentioned, that Laffer co-organized and that he held in the city of Chicago. Although many major academic macroeconomists—including several Keynesians—were in attendance at the conference, the other participants also included numerous representatives of business, central banking, and the media.

Indeed, it was the print media, specifically the editorial page of the *Wall Street Journal*, that now made much of the running in promoting fixed exchange rates. A *Journal* editorial (June 16, 1975*a*) signaled this development, as the editorial writers acknowledged that "we were reasonably receptive to the advent of floating exchange rates" but now opposed floating. A rebuttal letter that Friedman published (*Wall Street Journal*, June 30, 1975) in response to this anti-float editorial would be an early salvo in the exchanges that he would have with the *Journal* on this matter over the following three decades. "It's generally in favor of free markets," Friedman remarked of the *Journal*. "But, when it comes to exchange rates, it has a touch of the

[78] *Reason* magazine, June 1975, p. 94.

disease, of the 'gold-standard disease'—that is, of wanting fixed exchange rates." (Instructional Dynamics Economics Cassette Tape 184, January 1976, Part 2.)

From his vantage point as the U.S. Secretary of the Treasury, William Simon—who, like Friedman, was a strong supporter of floating rates—was also struck by the *Journal*'s turnabout in recent years on the issue of exchange-rate flexibility, observing: "For some time I have known that at least one of your Editorial Board believes that we should attempt to return to a rigid, gold-based international monetary system." (*Wall Street Journal*, June 16, 1975*b*.) Simon did not specify which Editorial Board member he was referring to. There were in fact two members who met Simon's characterization: editor Robert Bartley and associate editor Jude Wanniski. Both of them were highly sympathetic to the Laffer-Mundell position and had, like Mundell, attended the Laffer-organized April 1975 conference.

Bartley would be the most persistent champion of floating rates at the *Journal*, with Friedman noting during his third decade of debate with the line taken by Bartley and his fellow editorial writers: "My God, how can they can stick with that? They've just got an *idea fixe* about it… The debate about floating exchange rates has been won by the floaters. Other than Mundell, who is a 'nonfloater' among major American economists?" (*Forbes*, December 29, 1997, p. 55.)[79] But Wanniski, especially in interventions made in the 1980s and 1990s after he had left the *Journal*, would be the more virulent opponent of Friedman. Unlike Mundell, Wanniski was inclined to assign blame to Friedman for the high inflation and economic stability that often characterized the United States and other economies in 1971−1982—with Wanniski describing high inflation as the outgrowth of "the Friedman experiment" (*National Review*, June 11, 1990). And unlike Bartley and Laffer, whose sharp divergence with Friedman on monetary issues was tempered by friendly personal relations founded on their shared policy prescriptions in other areas of economic policy, Wanniski's gold-standard-focused opposition to monetarism and to flexible exchange rates would give rise to make numerous public interventions intended to lower Friedman's standing among the general public and to discredit him in free-market and Republican circles.

In addition to the small degree of lingering support for the Gold Standard in the economics profession and the media, it continued to have considerable backing in the periphery of economic writings. Reflecting the fact that an appreciable amount of the general public accepted what

[79] Friedman had initially erroneously thought that the *Journal*'s stand in favor of restoring fixed exchange rates would prove to be short-lived (Instructional Dynamics Economics Cassette Tape 184, January 1976, Part 2).

Friedman called "the mythology of gold" (*Reason* magazine, June 1975, p. 91), the advocacy of a gold-based monetary system remained a mainstay in various U.S. financial newsletters. The divergence between the perspective given in such newsletters and Friedman's monetary economics put the differences in views between himself and such Keynesians as Walter Heller in the shade. Heller shared Friedman's negative view regarding gold's monetary role: "The real value of a currency is not based on the gold behind it… [and] when we were on the Gold Standard, we fell into five major depressions and uncountable panics." But Heller added: "My doctor reads a hard-money newsletter religiously. He is worried about preserving his wealth." (*Los Angeles Times*, March 16, 1979, Part I, p. 24.)

The market for gold

Friedman's longstanding wish that governments get out of the gold market was not accompanied by a strong position about where the gold price would or should go, once unpegged: "If the price of gold rises, fine. If it falls, fine, too." (*Christian Science Monitor* (Boston), December 5, 1963.) Indeed, while he maintained his stand that the world's public sectors should abandon the gold market, his own perspective on the likely course of the gold price in the event of such a policy change was something that evolved during, and beyond, the years of the Bretton Woods system.

On the gold price, Friedman did continue to adhere to his position that, at the U.S. general price level prevailing in 1934, "gold was grossly overvalued at $35 an ounce" when the peg was first imposed in that year (Instructional Dynamics Economics Cassette Tape 81, August 25, 1971). That is, the peg put the price above the level that it would have attained at the time in the absence of official intervention (*Reason* magazine, June 1975, p. 94). But Friedman changed his mind about whether, in the years beyond 1934, the peg amounted to a force keeping the price up. In 1959, he acknowledged a widespread view that unpegging price might, at this stage, give rise to a higher price than $35. But he expressed the opinion that, owing to the reduction in the demand for gold that would occur in the event of the removal of gold from international monetary arrangements, it was possible that the gold price would decline below $35 upon being unpegged.[80]

This assessment, however, was made at a time when Friedman still entertained the possibility

[80] Friedman (1960a, pp. 83–84).

that the post-World War II U.S. price level might be stationary (see Nelson, 2020a, Chapter 10). In retrospect, however, it is clear that this was not the case: American prices rose substantially during the Korean War, and the price level continued to rise, albeit gently, in the decade after the Korean War. Consequently, one financial commentator's characterization of U.K. economic conditions during that war—"these days of rising costs and an obstinately constant price of gold" (*News-Chronicle* (London), June 5, 1951)—was roughly accurate also as a description of the United States over the 1950s and the first half of the 1960s. General world economic conditions were therefore putting the gold price under upward pressure over time, and this pattern was reinforced by higher U.S. inflation in the second half of the 1960s. The upward pressure was reflected in the private-sector world price of gold once that price was unpegged in 1968: this price's average annual value was in the range from $39 to $41 in the years from 1968 to 1971.[81]

Friedman would be caught off-guard by the surge in the world gold price after the United States' August 1971 closure of the so-called gold window. The world market price averaged $58.42 in 1972 and $97.39 in 1973. Friedman consistently argued that the free-market gold price would likely rise in the long run (Instructional Dynamics Economics Cassette Tape 122, June 6, 1973; *Dallas Morning News*, October 17, 1975). But he was skeptical about the sharp rise in the gold price observed in 1973: "the gold bubble is going to burst," he testified (on June 21, 1973, in Joint Economic Committee, U.S. Congress, 1973a, p. 139), and during the same period he predicted that a "sharp decline" in the price would occur over the following year (Instructional Dynamics Economics Cassette Tape 122, June 6, 1973). As with the price of another commodity, oil, the decline that Friedman predicted for the gold price had not occurred by spring 1974—when, undeterred, Friedman again predicted a major decline in the gold price (*Newsweek*, April 22, 1974). This did not eventuate in 1974—during which the gold price averaged $154.

In 1975−1976, however, and in contrast to the oil price, there was a decline in gold's price. Having passed $180 in February 1975, the price underwent a sharp fall. This decline nevertheless left gold at well above its pre-1973 price, and the gold price resumed an upward path after troughing at about $104 at the end of August 1976, moving above $130 in November 1976 (*Bank of England Quarterly Bulletin*, December 1976, Table 28, p. 511).

The end of the U.S. price commitment saw a development in the gold market that Friedman was pleased to see: the end, as of the turn of 1974/1975, of the prohibition on U.S. households' right

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[81] See *https://nma.org/wp-content/uploads/2016/09/historic_gold_prices_1833_pres.pdf*.

to purchase and own gold in significant amounts (*Newsweek*, January 6, 1975). He had called for the repeal of this ban for a long time (see, for example, *Christian Science Monitor* (Boston), December 5, 1963).[82] Friedman believed that the ban, although ostensibly imposed with the aim of helping maintain the official gold-price commitment, had really been instituted with the aim of preventing U.S. households from benefiting financially from the introduction of the peg—the peg having, as already indicated, seemed at first to be a force tending to push *up* the gold price.[83]

The U.S. government's changed attitude to gold was also reflected in its own holdings of the commodity. Friedman suggested (*Reason* magazine, June 1975, p. 92): "We might be very well advised to sell off some of our gold at the present price of $150 an ounce. That would get rid of a frozen asset and might enable us to turn Fort Knox into some useful purpose." By the standards of Friedman's statements on gold, this was a cautious recommendation, and he would later much more firmly advocate a phased sale of all the United States' gold inventory.[84] In January and June 1975, the U.S. Treasury did auction off 13 million ounces of gold, with resumed sales, totaling 15.8 million ounces, occurring in 1978 and 1979 (Schwartz, 1983, p. 35).

Although welcoming the restoration of Americans' right to purchase and own gold, Friedman indicated that his own view was that the gold market was something that they would be prudent to stay out of (*Dallas Morning News*, October 17, 1975). He acknowledged that, if an individual had been legally permitted at the time to carry out the indicated actions, purchasing gold in 1968 and holding it through late 1974 would have generated a windfall: the price rose from $35 to $150, with the real price 2½ times as great as that in 1968. "You would have done very well—enormously well—over that six-year period," Friedman admitted.[85] But he stressed that gold's real value had fallen in the 34 years through March 1968 as the basis for his conclusion: "Gold has not been a reliable inflation hedge."[86] And for Friedman, the post-1968 fluctuations in the

[82] Other occasions on which Friedman made this recommendation included his testimony of February 1, 1968 (Committee on Banking and Currency, U.S. Senate, 1968, p. 155; also quoted in *Chicago Daily News*, March 26, 1968, page 37) in which he stated, "We should simultaneously remove all legal restrictions on transactions by U.S. citizens in gold," and in earlier testimony on November 14, 1963, in which Friedman remarked, "we should simultaneously remove all present limitations on the ownership of gold; and the trading in gold; by American citizens." (In Joint Economic Committee, U.S. Congress, 1963, p. 459.)
[83] See Friedman (1961a, p. 73) and *Reason* magazine, June 1975, p. 87.
[84] In the aforementioned 1963 testimony (Joint Economic Committee, U.S. Congress, 1963, p. 459), Friedman had been even more cautious, mentioning selloff of the gold stock as an option but not firmly recommending it. He was unequivocally in favor of selling off the stock in later discussions. See Chapter 15 below for further coverage of these discussions.
[85] *Reason* magazine, June 1975, p. 93.
[86] *Reason* magazine, June 1975, p. 94. See also *Newsweek*, January 6, 1975.

gold price confirmed that gold was "a lousy store of value."[87] He suggested that the fact that it no longer had an officially enforced connection to the dollar, as it had had under the successive pegging arrangements through 1971, meant that gold should be regarded as in the same category as other commodities subject to heavy investment speculation, such as soybeans (*Newsweek*, April 1, 1974; *Reason* magazine, June 1975, p. 93).

Floating exchange rates are ratified

As discussed in Chapter 3, until his departure from the Treasury in July 1974, Paul Volcker had overseen the U.S. government's major discussions of future exchange-rate policy. Since the outset of the Nixon Administration, Volcker had headed a permanent, across-government working group (the so-called Volcker Group) on international economic policy (Duncombe, 2001, pp. 290–291), and he was the main U.S. representative on the International Monetary Fund's intergovernmental Committee of Twenty, or C–20, set up in 1972 (Deane, 1976, p. 6; Williamson, 1977, p. 175). After the move to floating exchange rates in March 1973, both these bodies continued to operate, each having the aim of designing a permanent, non-floating, system in the wake of the collapse of the Bretton Woods arrangements.

John Williamson, who was a junior staff member working for the C–20, granted that Volcker had a "genuine enough desire to see agreement reached" (Williamson, 1977, p. 175). Volcker's public statements after March 1973 likewise indicated that he did not, as of 1973–1974, want floating to continue, and he was criticized publicly by Friedman on precisely this ground (see Chapter 3 above). Volcker's wish to shift back to some form of exchange-rate management was evident in a private meeting that Volcker and Secretary of the Treasury Shultz had with French government officials on May 31, 1973. At the meeting, Volcker portrayed floating in a permanent system as an option available to a country in particular situations rather than the ongoing state of a currency (see Rasmussen, 2009, p. 148).

The chapter of the U.S. Department of State volume in which this meeting's minutes were eventually published was titled "Negotiating the New Rules, May 1973–June 1975" (Rasmussen, 2009, p. 133). Of course, eventually the main "rule" turned out to be, as Anna Schwartz (1983, p. 32) would put it: "Market forces had triumphed." The existing floating-rate system would remain in effect. But in the meantime, until Volcker left government, he was involved in much

[87] *The Sunday Bulletin* (Philadelphia), March 2, 1975, Section 1, page 25. See also Friedman's remarks in *Newsweek*, April 22, 1974, and January 6, 1975.

back and forth about the reintroduction of a nonmarket system to govern exchange rates. Marina Whitman, a member of the Volcker Group, noted that they sought an arrangement of "somewhat greater flexibility of exchange rates—but still by no means fully market-determined... We favored room for a certain amount of floating." Although she certainly saw some positive aspects of the more thoroughgoing floating that was instead the outcome (see Whitman, 1975), her judgment was nevertheless: "I think Volcker and the members of his team, including me, felt in a way that we had failed to get the system back under any kind of control." (Marina Whitman, interview, May 1, 2019.)

In April 1975, now out of government for a spell, Volcker voiced dissatisfaction with floating rates but remarked: "I don't think we are going to go back to fixed rates very soon anyway."[88] Volcker's retrospective verdict on these years was: "We went to floating exchange rates when people exhausted every other possibility." (Paul Volcker, interview, October 16, 2013.) Volcker's ambivalence toward floating contrasted with that of George Shultz, as well as of Shultz's successor as Secretary of the Treasury, William Simon. In office through early 1977, Simon was, as already indicated, overtly supportive of floating. "I regard policies which look toward the establishment of foreign exchange rate pegs, targets, or zones, as unwise," Simon testified in March 1975. "… The world moved to the present arrangements of 'managed floating' for very good reasons—we needed greater flexibility and greater reliance on market forces at a time of great uncertainty… These arrangements have served us well in enabling the world economy to absorb some rather severe shocks in the past two years without the crises of earlier years."[89]

During 1975, too, the International Monetary Fund's public stand changed, becoming more receptive toward floating exchange rates. The C–20 had been dissolved in 1974. Although the C–20 was succeeded by an Interim Committee (Deane, 1976, p. 6), Williamson (1977, p. xi) saw the disbanding of the C–20 as the effective end to the IMF's efforts to restore a fixed-exchange-rate system. Williamson also acknowledged that, ultimately, the C–20's "negotiations achieved so little"—the major changes in the international monetary system that proceeded concurrently with the C–20's existence being driven by other developments.

In a brief account based on his own interaction with the IMF's leadership, Friedman dated the increased receptiveness of the IMF toward floating exchange rates to the period between 1969

[88] In Birnbaum and Laffer (1976, p. 311).
[89] From Simon's testimony of March 24, 1975, in Joint Economic Committee, U.S. Congress (1975i, p. 5).

and 1972.[90] But, while the IMF had certainly become critical of aspects of the 1960s exchange-rate system, its move to being reconciled to a floating-rate system actually came later than 1972 or 1973. The C–20's report of June 1974 had still proposed to limit exchange-rate adjustment, by replacing floating with an adjustable-peg system (Hansen and Hodrick, 1983, p. 122). The subsequent change in the situation was reflected in the headline of a wire news item: "Fund Blesses Floating Currency" (*Omaha World-Herald* (Nebraska), August 25, 1975). The accompanying news report highlighted the IMF's new annual report, which stated: "Fluctuations in rates have at times been erratic, but there is little evidence thus far that this factor has seriously impeded the growth of world trade. On the whole, exchange-rate flexibility appears to have enabled the world economy to surmount a succession of disturbing events, and to accommodate divergent trends in costs and prices in national economies with less disruption of trade and payments than a system of par values would have been able to do."[91]

The same report documented the fact that, following agreements by major governments on the matter, the IMF would abolish the official price of gold and would sell off part of its own gold holdings. Auctions began in June 1976 as part of a planned four-year divestiture of one-third of the IMF's gold holdings (Murphy, 1979, p. 175; Schwartz, 1983, p. 35).

The August 1975 wire report on the IMF's changing position noted that the following week's IMF annual meeting would need to face the issue of whether the Fund's articles of agreements would "'legalize' floating." In the event, the IMF devoted a further policymaker conference—held in Kingston, Jamaica, on January 7−8, 1976—specifically to this subject. The performance of floating was covered in the leadup to the conference in a piece by Alan Day. Day, a member of the London School of Economics who, like Friedman, combined academia with writing an economics column (in Day's case, for London's *Observer* newspaper), had been a co-signatory with Friedman of the February 1966 letter calling for greater flexibility in exchange rates. Nearly a decade on, Day saw their stand as having been vindicated by the performance of floating rates in recent years: "The floating exchange regime has absorbed many of the strains which might otherwise have been expected on the balance of payments between major

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[90] Friedman and Friedman (1998, p. 220). Here, Friedman specifically referred to panel appearances he had had alongside the IMF's "secretary general" (likely meaning its managing director) in 1969 and 1972. The panel in which the IMF managing director spoke favorably of floating rates likely occurred at a later event than the international monetary conference in Montreal in 1972 that Friedman associated with it—and probably at one of the conferences on international economics that Friedman attended during 1974−1980.

[91] International Monetary Fund (1975, p. 33). Passages like this represented a break in position from those that the IMF had taken even in the recent past and so rendered somewhat out of date references like Johnson's (1975, p. 45) to "the interests of the IMF in rebuilding a fixed-rate system."

countries." (*Montreal Star* (Quebec, Canada), January 2, 1976.)

The Jamaica summit, attended by twenty finance ministers of various nations, formally ratified the floating exchange-rate system (*Daily News* (New York), January 9, 1976). But, as Friedman noted, because that system had already emerged and (in contrast to the Bretton Woods system) did not require international agreements on parities, the holding of the summit was unnecessary for the continued operation of floating rates and its participants were simply acknowledging the reality of floating rates. He likened the summit's outcome to a court's recognition of a common-law marriage—in this case, one that had, essentially, started in August 1971 (Instructional Dynamics Economics Cassette Tape 184, January 1976, Part 2).[92]

This status of the Jamaica agreement put in perspective the formally correct statements that "floating is legalized in the new Articles" (that is, those agreed in 1976 and subsequently implemented) (Argy, 1981, p. 2) and: "After the agreement was ratified by the Interim Committee of the IMF in January 1976 in Jamaica, the countries of the world were free to adopt the exchange rate regime of their choice."[93] In effect, what had happened was that countries were declared free to do something (float) that they were already doing. The float had been formally breaching the IMF rules, with Friedman observing (Instructional Dynamics Economics Cassette Tape 184, January 1976, Part 2) that "until the Jamaica agreement… most of the countries have been in violation of the words of the International Monetary Fund statutes, which require each and every one of them to state a par value, to maintain it, and so on."[94] World policy organizations had simply gone along with the protracted instances of violations seen to date—and, Friedman stressed, they had had to: "[The] IMF has no police force, [so] it can't enforce it [the fixed-rate requirement]." Now, the IMF was acquiescing in the change in the system and adjusting its own modes of operation in recognition of the change.

Therefore, although the 1975−1976 agreements among the IMF and multiple governments put a more orderly sheen on the move to a floating exchange-rate system, that move had already occurred. Floating rates had been operative for nearly three years. Furthermore, they were to be in operation indefinitely. Argy (1981, p. 2) did note that "the Jamaica Agreement of January

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[92] Friedman later alluded to the 1976 conference in Friedman (1982a, p. 99) as the point at which attempts to restore fixed exchange rates were belatedly abandoned.

[93] Hansen and Hodrick (1983, p. 122).

[94] Secretary of the Treasury William Simon had likewise noted nearly a year earlier that "we and all other countries… are in technical violation of the [IMF] articles because our currencies are floating" and stated that the U.S. government "strongly supports an amendment to bring floating exchange rate within the [IMF's] legal framework" (testimony of March 24, 1975, in Joint Economic Committee, U.S. Congress, 1975i, p. 8).

1976 pledged countries to 'promote a stable system of exchange rates.'" This "pledge" was, however, made in the context of acceptance of a continued float. Correspondingly, Friedman deprecated interpretations of the agreement that portrayed it as a commitment to restoring fixed exchange rates (Instructional Dynamics Economics Cassette Tape 184, January 1976, Part 2).

Despite the 1975−1976 formalities, therefore, it was true in these years what McCallum (1996, p. 86) had noted of the prior change in 1973: the flexible-rate regime did *not* come about "because the world's central banks decided that a floating-rate system would be superior." McCallum was one of several writers who indicated their support for Friedman's 1953 analytical case for floating rates but who also stressed that floating rates did not emerge out of an explicit victory for this case but out of a chaotic dissolution of the fixed-rate arrangements. Other writers making observations like McCallum's included Brunner and Meltzer (see Chapter 3 above) and Haberler (1984, p. 101), who remarked that "floating was imposed by events on reluctant policymakers." For his part, Friedman too agreed with this interpretation and articulated it himself: see Chapter 9.

As stressed in Nelson (2017), however, there is an important sense in which policymaker acceptance of floating *did* flow from an acceptance of Friedman's case and not just from market pressures. This fact is brought out by the April 1975 remarks of Yale University's Robert Triffin, leading international economist and a longtime fixed-rates advocate. Triffin observed: "The adoption of flexible rates was essentially due to the inflationary implications of this system rather than to the academic preachings of Professors Friedman, Haberler, Fellner, Machlup, etc. The Germans, for instance, would not have moved from fixed to flexible rates if Germany had not been flooded with the sudden [previous] surpluses of reserve currencies unleashed upon the world by enormous U.S. deficits."[95]

Although intended to be derisive of Friedman's influence on events, Triffin's remarks implied that, even in the course of the 1973 disruptions, the acceptance of floating by key countries was importantly based on individual governments' desire to have, or have restored, monetary policy autonomy. This, of course, reflected a key Friedman analytical argument. And it is possible to see that although the timing of the start of floating was forced on policymakers by market forces, their *acceptance of the float over the longer term* resulted, in part, from the emergence of wide agreement with this argument. Even in March 1975, before Triffin's remarks, Secretary of the

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[95] In Birnbaum and Laffer (1976, p. 27).

Treasury William Simon stressed that, although U.S. policymakers had initially had floating thrust upon them, they had since had the opportunity to think about "what policies should the United States adopt with respect to exchange rates" and had come out in favor of floating.[96] There were "very good reasons" for the present arrangements, he suggested, among them "greater flexibility and greater reliance on market forces at a time of great uncertainty."[97] Elsewhere in the administration, the Council of Economic Advisers under Alan Greenspan had cited monetary policy autonomy as a desirable feature of floating, observing: "The new system has also enabled countries to manage their money supply with a greater degree of independence."[98] Simon, too, would specifically invoke such policy autonomy as a virtue of flexible rates—including in testimony in October 1975, when he stated that "although monetary policy in the United States affects the economies of the European countries, and *vice versa*, neither they nor we can allow our domestic monetary policies to be determined by the other."[99]

Similarly, Simon had suggested in March 1975 that beyond the United States, there was "agreement that floating is the only desirable and practical course."[100] Consistent with this characterization of world opinion, Otmar Emminger—who was president of the Bundesbank at the end of the 1970s but who, in prior senior posts, had experienced at close hand the exchange-rate crises under the Bretton Woods system, noted in 1979 that the floating system met the description of the least bad system among all the options available (see Argy, 1981, p. 3).

The reconciliation of the fact that floating was a forced event with the likelihood that its permanence reflected, in part, the strength of Friedman's economic arguments for floating was encapsulated in Schwartz (1983, p. 43) noting that, in order for achievement of "independent monetary policy, the only workable exchange-rate system was floating."[101]

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[96] Testimony of March 24, 1975, in Joint Economic Committee, U.S. Congress (1975h, p. 5).
[97] Testimony of March 24, 1975, in Joint Economic Committee, U.S. Congress (1975h, p. 6).
[98] Council of Economic Advisers (1975, p. 202).
[99] Testimony of October 22, 1975, in Committee on Foreign Relations, U.S. Senate (1976, p. 549). Some of Simon's other statements on exchange rates, although still supportive of floating, were not as compatible with Friedman's perspective on the subject. For example, Simon stated: "The price of the dollar is the most important price in the United States as it relates to exports, jobs, and our balance of payments." (*Daily News* (New York), January 9, 1976.) Friedman was usually wary of statements about the paramount importance of the exchange rate for the behavior of the U.S. economy as a whole (as opposed to that of the tradable-goods sector). And in the 1980s he strongly stressed the point that there was no fixed relationship between the quantity of employment in the United States and the exchange-rate value: see Chapter 15.
[100] Testimony of March 24, 1975, in Joint Economic Committee, U.S. Congress (1975h, p. 8).
[101] For further discussion, see Chapter 9.

The role of the IMF in a floating world

One of Friedman's most extended post-1953 presentations in favor of floating exchange rates had been his Congressional testimony of November 1963. After Friedman had outlined the institutional changes needed for the United States to detach itself from the Bretton Woods system, Henry Wallich, testifying alongside him, added another requirement: "And beg off from the IMF." "That is right," Friedman agreed."[102]

"Begging off the IMF" here meant the United States withdrawing the various price and transaction commitments that it had made to the Fund in connection with enforcing the Bretton Woods arrangements. The same matter came up in late 1968 when, in Friedman's memorandum to President-elect Nixon recommending reforms to international economic policy, he noted that these reforms would require a U.S. "exemption from IMF from present obligations."[103]

These remarks underscored the fact that, since the beginning of its existence, the IMF had been deeply involved in the operation of the postwar fixed-rate system. That being the case, the possibility existed that the IMF might find itself bereft of its basic function if the world adopted floating exchange rates.

The notion that flexible rates would deprive the IMF of its original reason for existence has sometimes been portrayed as something that Friedman advanced only in the 1980s as part of a pejorative characterization of the Fund.[104] But he had discussed this matter explicitly in his 1953 exchange-rate essay (in a passage of that essay recently highlighted by Tavlas, 2023b).[105]

Furthermore, it was not very controversial at the time to imply that the fixed exchange-rate system was the IMF's original *raison d'etre*. A key financial service of the IMF was to provide "international liquidity"—that is, to be a source of the funds that countries might need in a fixed-rate system to cover their balance-of-payments deficits.[106] International liquidity was a major research and policy concern during the Bretton Woods era.[107] The prominence of this concept

[102] From the hearing of November 14, 1963, in Joint Economic Committee, U.S. Congress (1963, p. 518).
[103] Friedman (1988, p. 431).
[104] See, in particular, Kuehn (2020).
[105] See Friedman (1953a, p. 191).
[106] See, for example, Triffin (1960), McKinnon (1968), and Williamson (1973). The last of these references noted the close overlap between the concepts of international liquidity and foreign exchange reserves.
[107] See the previous footnote. Conversely, the concept could be treated as largely of historical interest in modern-day textbooks on international monetary economics (for example, McCallum, 1996, pp. 84–85). The textbook of

reflected the regime in force: for, by its very nature, the demand for international liquidity was something that arose from the fixing of exchange rates. Even Robert Mundell, a star researcher-economist at the IMF in the early 1960s who, in his subsequent career as a leading academic, maintained excellent relations with the Fund (for example, he coedited an IMF research conference volume that was published in 1977), acknowledged in 1963 that, under a pure float, "There is no need for liquidity."[108]

A possibility that Friedman mentioned matter-of-factly in the 1953 essay—that, in a floating world, the IMF might become a source of "advice about internal monetary and fiscal policy"—accurately foreshadowed a major role that the Fund carried out in the post-1973 environment.[109] Once this role became the reality of the postwar world, Friedman became critical of it. "The establishment of floating exchange rates in the world has destroyed the function of the IMF," he suggested in mid-1977, while going on to say that the Fund should be abolished.[110] The following year, he attributed the ongoing high profile of the IMF in international matters to its "highly developed capacity for survival."[111] "The IMF, sitting on a pile of funds, sought and found a new function: serving as an economic consulting agency to countries in trouble," he wrote in 1998, in one of many observations he made to this effect (*The Times* (London), October 12, 1998).[112]

Friedman's negative remarks regarding the Fund came despite the fact that a number of its economist staff over the years had taken his graduate courses. "There were lots of us," Warren Coats, who also had Friedman as his thesis adviser, observed (interview, October 21, 2013). "I mean, it was strange, in that Friedman never liked the Fund. (*laughter*) But he prepared, and sent a lot of, students there—or prepared students who chose to go there." Friedman himself remarked in 1998, while outlining his by-now-familiar critique of the "super consulting agency" role of the post-Bretton Woods IMF, that the Fund "got lots of clients all over the world, and its advice was sometimes very good. They had very able people at the IMF—they had some of the best economists around. Some of them were even students of mine."[113]

Obstfeld and Rogoff (1996) went a step further by taking for granted from the outset that, in the overall balance of payments, the current account and capital account imbalances invariably summed to zero (pp. 5−6).

[108] From Mundell's testimony of November 15, 1963, in Joint Economic Committee, U.S. Congress (1963, p. 572). The 1977 conference volume collected the contributions to an IMF conference held on November 11−12, 1976: see Mundell and Polak (1977, p. vii).

[109] Friedman (1953a, p. 191).

[110] Friedman (1977r, p. 30).

[111] Friedman (1978c, p. R−181).

[112] Another of these many examples was Friedman and Friedman (1998, p. 220).

[113] Friedman (1998b, p. 4).

Another factor making for the IMF's continuation was that a large number of small countries continued to have fixed exchange rates of some kind after the major countries floated in 1973 (Brooks, 1979, pp. 204–205). In addition, examples emerged of floating countries that, in the face of large exchange-rate depreciations, relied on IMF support to limit the decline. The IMF's role in facilitating such managed floats sometimes overlapped with its function as an adviser on, and monitor regarding, domestic economic policy—a famous early case, on which Friedman commented extensively, being the IMF loan to the U.K. government in 1976.[114]

One notable feature of the countries that were IMF members was the disparity among them in degrees of political freedom. The proceedings volume recording the IMF's annual meeting in October 1976 had contributions by the finance ministers or central bank governors of many leading democracies, among them William Simon (U.S. Secretary of the Treasury) and Phillip Lynch (the Treasurer of Australia). But it also had an opening address by President Ferdinand Marcos of the Philippines and a subsequent contribution by a representative of Marshal Josip Tito's Yugoslavia, while a notation in the volume indicated that the finance minister of Pinochet's Chile was a member of an IMF committee.[115] The OECD membership was similarly a mixture: Fisher and Sheppard (1972, p. ii) listed as OECD members a large number of major democracies but also Portugal, Spain, and Greece, which all remained dictatorships until the mid-1970s, while the OECD (1983, p. 4) noted that, since October 1961, it had been the case that the "Socialist Federal Republic of Yugoslavia takes part in certain work of the OECD."

These economic links with the undemocratic regimes in essence reflected a judgment, often not made explicit, that cooperation and dialogue on economic matters with governments that practiced repression did not connote endorsement of those regimes and also that technical economic advice to a country that was ruled undemocratically could benefit the citizenry of the dictatorship, by encouraging economic policies that were more likely to improve the standard of living of the population. Friedman shared this judgment himself. Furthermore, he felt that it justified the occasions on which he himself traveled to dictatorships and offered views and, indeed, advice on their economic problems. At the start of 1975, Friedman was about to find himself the center of a giant, and enduring, controversy when this frame of mind led him to travel to Chile, as well as to meet with its leadership during his time in that country.

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[114] See Chapter 9 for more on this episode. The episode has been the subject of many accounts—an early one, focusing on the IMF's role, being Crawford (1983).
[115] See International Monetary Fund (1976, pp. v, vi, 273).

II. ISSUES RELATED TO DEBATES ON INTERNATIONAL ECONOMIC POLICY AND GEOPOLITICS, 1975–1976

THE CHILE TRIP: CONTROVERSY AND RESPONSE

Both during the period of Pinochet's rule over Chile (1973−1990) and in subsequent years, the feature of that regime that correctly received overwhelming attention was its oppressiveness, including its stifling of democracy and its violations of human rights. The regime was a violent one from its assumption of power in a military coup. Against this background, those based outside Chile who interacted with Pinochet and his junta government risked being perceived as endorsing the government's conduct as a whole, including its violent suppression of dissent.

Among those who acquired, and resisted, the perception of being someone who was endorsing the regime was Milton Friedman. As well as affirming his opposition to the oppressive character of the junta, he would continually stress that his personal involvement in the regime's economic policy, in terms of his direct dialogue with members of the government of Chile, was small—being mainly concentrated in his March 1975 trip to the country.[116] But that trip would help provide a basis for, and subsequently lock in, the perception of an association between himself and the regime, especially because it included a meeting between Friedman and Pinochet.

The perception of a Friedman/junta link was consolidated by the fact that the regime did eventually follow economic policies whose general form had become associated with Friedman and the University of Chicago. Friedman did not deny this broad influence, and he made favorable commentary on many of the economic policies that the junta-led government followed—including, on the macroeconomic side, the anti-inflation policy pursued over the second half of the 1970s.

In effect, Friedman was involved over the final three decades of his life after 1975 in a threefold defense of his role in matters concerning Chile under Pinochet.[117] First—and the issue on which he spent the most time—was to defend the judgment and ethics involved in providing, even in an informal manner, economic advice to the military-junta regime in 1975. Second, Friedman—

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[116] The trip was in the later days of March and, reflecting its proximity to April 1975, would sometimes be erroneously dated to the latter month (see, for example, *New York Times*, December 2, 1977, and Friedman 1977i, p. 36 [also in Friedman, 1991a, p. 144]).

[117] His engagement on these points will be brought out in the course of the account that follows.

here dealing with a matter that he was more accustomed to debating, as he had been involved in similar arguments in democratic-country contexts, and one that he likely was far more comfortable discussing, as it focused on economics—argued that, contrary to some commentators' claims, the economic policies that he was advocating would indeed improve Chile's living standards. Third, he was concerned with rebutting many critics' contention that the fact that a dictatorship was implementing economic policies that he felt had merit showed that those policies' implementation automatically went hand in hand with political repression—and that, the critics likewise argued, the policies would not be possible in a democracy, as they would be unacceptable to the population and to an elected government and legislature.

A long discussion of the controversy

In addition to receiving heavy media coverage—especially during 1976 and 1977—the controversy over the interaction between Friedman and the Chilean junta generated extended accounts in popular and research outlets from an early stage. A book on the matter by Axell and Swedenbourg (1977), *Milton Friedman och ekonomipriset*, appeared in Swedish in 1977, after the controversy had played a prominent role during Friedman's December 1976 visit to Stockholm for the Nobel ceremonies. Subsequently, the book on Friedman by Frazer (1988) had a chapter (pp. 327–364) on the Chile controversy, while Friedman's trip to Chile and economic policy under Pinochet formed the principal subject matter of Naomi Klein's 2007 book *The Shock Doctrine.*

More recently, book compendiums of retrospectives on the Chicago School of economics have devoted chapters specifically to the controversy, with one of these books including a negative judgment on Friedman's involvement by Schliesser (2010), and another having had two chapters on the matter, with Peck (2011) critical of Friedman's role and Hammond (2011) defending it.

Still more recently, there have been some key research papers centered on the 1975 Friedman trip to Chile and subsequent developments. These have included Montes (2015) and Edwards and Montes (2020)—studies that provide detailed accounts of Friedman's 1975 trip and his return to Chile, in 1981, for a Mont Pelerin Society meeting, as well as covering some of the developments connected with the Friedman/Chile controversy in the years between these trips—and Farrant (2020)—who details the planning of the trip and the criticism that Friedman received, in early 1975, regarding his intention to travel to Chile and examines the arguments made in the associated private correspondence.

In light of the detailed and informative contributions of the 2020 research papers, the coverage of the matter in this chapter is circumscribed: the discussion below focuses on the controversy that ensued in the United States and on Friedman's response to the controversy, as revealed in public statements in the U.S. and U.K. media. Many of these statements have not been considered in the prior research literature on the controversy.[118] The discussion also considers the matter of whether, with regard both to the way in which to achieve disinflation and what could be expected in the aftermath of disinflation, the analysis and recommendations that Friedman was producing have tended to be widely incorporated into mainstream U.S. economists' frameworks, or whether, as some critics contended, they were prescriptions that were very special to his brand of economics and, furthermore, would not have the promised economic results.

Connections via the University of Chicago students

Although Friedman did not go to Chile before 1975, an important link between the University of Chicago economics world, especially in the area of monetary analysis, and Chile had developed during the 1950s. Chilean students had featured regularly in the Department of Economics' graduate classes at the University of Chicago, thanks in large part to a formal program. Friedman remarked on television in October 1976: "we had for a long time—the University of Chicago, for some ten years, had a cooperative arrangement with the Catholic University in Chile under which we helped them down there to improve their economic program, and they sent students to study with us."[119]

Several weeks later, Arnold Harberger would explain the program—which actually lasted about eight years—in more detail. Harberger described it as having involved "a contract between the University of Chicago and the Catholic University of Chile for the years 1956 to 1964, financed by AID [the U.S. Agency for International Development]" and as having led to "many Chilean students" taking doctoral studies in economics at the University of Chicago (*Wall Street Journal*, December 12, 1976*a*).[120] As AID was an agency of the U.S. Department of State, it was also accurate to describe the exchange arrangements as a "State Department exchange program," as

[118] An important item—a Friedman interview/profile in *Sunday Times* (London), December 12, 1976—that appeared in the U.K. side of the coverage and that has featured in the recent literature (for example, Farrant, 2020, p. 120) was quoted in this connection in Nelson (2009a) and is also discussed below, along with other U.K. media items.
[119] *Meet the Press*, NBC, October 24, 1976, p. 3 of transcript.
[120] Schliesser (2010, p. 195) gives the years over which the program was in operation as having been 1955 to 1964. But the program can, instead, be definitively established as having begun in 1956, as the contract launching the program came into effect on March 30, 1956 (see Bodenman, 1957, p. 89).

the *Wall Street Journal* (December 12, 1976*b*) did.[121] The terms of the initial contract indicated that "as many as five Chilean participants per year may be trained" in graduate economics at the University of Chicago under the program (Bodenman, 1957, p. 89).[122]

A discussion by the London *Economist* (February 2, 1980) of the University of Chicago-trained Chilean economists—and specifically the subset of them who served in Pinochet's government—claimed that "few had actually been taught by Professor Friedman." In saying this, the *Economist* article lapsed into a particular type of mischaracterization apparent over the years in numerous discussions of the connections between Friedman and Chile. Specifically, in making the point that Friedman's own role in matters concerning Chile was both small and much smaller than other figures connected with the University of Chicago, such characterizations veered into misstatement by portraying the link as having been smaller than it actually had been. Contrary to the statement made by *The Economist*, Friedman had, in fact, been a teacher of many of the students.

With regard to Friedman's position *vis a vis* these students, Arnold Harberger had earlier provided a more accurate account than the one given by *The Economist*. Harberger noted that Friedman was not involved in organizing the exchange program itself but that the Chileans that came out of the program were his students because he taught graduate classes: "As it happens, Mr. Friedman had no active part in that program, although of course the Chilean students, like the rest of our students, took his courses." (*Wall Street Journal*, December 12, 1976*a*).[123] Over 1956–1964, these classes included not only his Price Theory course in the time of the year during which Friedman taught it, but also his graduate monetary classes in the 1963/1964 year. For that reason, a *New York Times* report (March 21, 1976) was accurate in describing the University of Chicago-trained Chileans as having "studied under Mr. Friedman." And Friedman himself indicated his part in their studies when he stated that the students "under me, did graduate work at the University of Chicago and got their Ph.D.s there" (*California* magazine,

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[121] The specific arrangement under which Chilean students came to the United States under the program was the Point Four program of the International Cooperation Administration (*Watertown Daily Times* (New York), July 6, 1961).
[122] As was often the case in Ph.D. programs, a masters degree was also conferred to students in the course of their doctoral studies.
[123] One indication of Friedman's inactivity with regard to the program was that he did not go to Chile in the 1950s and 1960s. In contrast, by mid-1956, two of his colleagues were visiting Santiago in connection with the program (Bodenman, 1957, p. 89). One of the University of Chicago economics department members who was an early participant in the program was Simon Rottenberg, an associate professor who taught at the Catholic University of Chile from 1956 to 1958 (*Providence Evening Bulletin* (Rhode Island), November 21, 1958).

October 1984, p. 77), and in referring to the exchange program as producing students "many of whom were in my classes and whom I got to know" (*Reason* magazine, August 1977, p. 25).[124]

Along with being one of the teachers giving course lectures to these students, Friedman interacted with some of the students through his money workshop. Both during and beyond the 1956–1964 period of the formal program sponsoring Chilean graduate students, Chile—as one of Latin America's high-inflation countries—was one of the nations studied recurrently by the money workshop. Over a long period, numerous workshop presentations took place on Chile's monetary and inflation experiences, being given by both Chilean and non-Chilean economics graduate students as well as department members.

Instances of such workshop sessions dated from the mid-1950s to the early 1970s. For example, John V. Deaver made a presentation, "The Chilean Inflation, 1938–1954," to the workshop—laying out the study that was later developed into his 1961 dissertation, "The Chilean Inflation and the Demand for Money," which Friedman included in reading lists in his University of Chicago monetary course in the 1960s, and whose published version, Deaver (1970), would be cited in Friedman and Schwartz's *Monetary Trends*.[125] Carlos Massad presented "Implicit Monetary Policy Rules in the Chilean Economy" at the workshop on March 16, 1964. Chile's Rolf L*ü*ders—the finance minister in the junta government from 1982—was "the only Chilean who wrote a Ph.D. thesis under Friedman's supervision" (Edwards and Montes, 2020, p. 110), and his thesis seminar, "A Monetary History of Chile, 1926–1962," was the basis for the workshop session of July 9, 1964.[126] Another mid-1960s Friedman-supervised dissertation, by Morris Perlman (who originated from the United Kingdom), included Chile in a multi-country study of money-holding propensities that Friedman referenced in print in 1969 and whose published version—Perlman (1970)—Friedman cited in 1971.[127]

In addition, Tom E. Davis presented "Inflation in the Chilean Economy" to the workshop on October 17, 1961. Davis was an assistant professor in the economics department of the

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[124] In Friedman and Friedman (1998, p. 403), Friedman favorably quoted an assessment that stated *inter alia* that he did not know any of the Chilean students well. That particular part of the assessment likely was an overstatement.
[125] See Friedman and Schwartz (1982, pp. xxiv, 150). Friedman (1971d, p. 851) had also cited Deaver (1970). The dissertation version (Deaver, 1960) of this study was in the reading list of Friedman's 1966 classes (information from Ann-Marie Meulendyke).
[126] On account of L*ü*ders being the only doctoral student among the Chilean students, Zvi Griliches was also correct in stating that "most of these Chilean economists were not direct students of Friedman" (*Harvard Crimson* (Harvard University), November 1, 1976), provided that "direct" was taken to refer to the supervision of dissertations.
[127] See Friedman (1969a, p. 4; 1971d, p. 852).

University of Chicago from 1956 to 1963.[128] A different paper of his on inflation in Chile would appear in the *Journal of Political Economy* in 1963, and Davis would produce many other studies of the Chilean economy. Davis was also in charge of the department's Chilean graduate student program for part of the time of its operation—with a profile of him that appeared in connection with Congressional testimony that Davis delivered on May 10, 1962, noting that he "has recently returned from Santiago, Chile, where he was in charge of the University of Chicago's cooperative program with the Catholic University of Chile."[129]

The professor in the economics department who had the most involvement with the study of Chile and the Chilean graduate student program was Arnold Harberger. Harberger served as an economic consultant to Chilean governments in 1959 and 1965−1969 (*Wall Street Journal*, December 12, 1976*a*). His research writings and talks also reflected his interest in Chile. For example, Harberger also gave a presentation, "Notes on the Chilean Inflation," to the money workshop on June 1, 1961. A little earlier, during the first few months of 1961, the department held a special one-off workshop on the Chilean economy that Friedman attended.[130] Earlier still, it may have been Harberger, possibly along with former graduate students or Davis, to whom Friedman spoke, when, as he later recalled (Instructional Dynamics Economics Cassette Tape 142, March 20, 1974), he talked by telephone to friends who had listened in Chile on short-wave radio to the Kennedy-Nixon presidential debate: they had judged Nixon to have been the victor, and their reaction was cited by Friedman as one example of the oft-made observation that the visual aspects of the debate had contributed to the perception of a poor showing by Nixon. Friedman chose an unfortunate paraphrasing of their verdict when he described them as suggesting that Nixon "had just wiped out Mr. Kennedy."[131]

The research activity that Harberger undertook on Chile's economic record had, by the mid-1970s, been a major factor in establishing a perception among researchers of the University of Chicago's economists as being among those most interested in the Chilean inflation experience.

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[128] See Kate Blackwood's article of November 23, 2022 (*https://economics.cornell.edu/news/economist-tom-davis-dies-93*).
[129] In Joint Economic Committee, U.S. Congress (1962, p. 4).
[130] Letter from Milton Friedman to G. Warren Nutter, March 8, 1961, Milton Friedman Papers, Hoover Institution archives (Box 31, Folder 16).
[131] Rolf Lüders may have been one of the Chile-based friends to whom Friedman spoke on this occasion. Although Lüders did not receive his University of Chicago Ph.D. until the mid-1960s, his years on the University of Chicago campus as a student had been through mid-1960, by which time he had picked up a masters degree from the university. He left the campus to take up a position, starting in September 1960, as an assistant professor at Catholic University of Chile (*Quincy Patriot Ledger* (Massachusetts), June 11, 1960).

A book on inflation by Trevithick and Mulvey (1975, pp. 137), in noting, "The Chilean inflation is particularly interesting on a number of counts," added that it was "hardly surprising that economists from more advanced economies should specifically focus upon Chile when they are examining how economies learn to live with inflation." In this connection, the authors specifically cited Harberger (1963), a study concerning Chile's money/prices relationship. A later Harberger (1966) analysis, dealing with the wider topic of Latin American inflation, would be cited in a Congressionally commissioned study of that subject: see Mikesell (1967, p. 17).

Friedman and discussions of Chile's pre-1970 inflation experience

As Friedman's research interests came to focus from the late 1940s onward on monetary matters, the high-inflation experiences of Chile and other Latin American countries were of interest to him in view of the light that they threw on the relationships between money and other variables. This was particularly so because they stood out in terms of the scale of their postwar economic fluctuations and in the contrast between the movements of nominal and real economic series. The magnitude of these fluctuations was underlined in a talk that the senior Federal Reserve Board staff member Arthur W. Marget gave in January 1956. Marget (1956, p. 9) noted that, in Argentina over 1945−1955, the money stock had risen 500 percent, while output had risen by only 3.5 percent.[132]

By 1954, Friedman had been able to use U.S. data on the Depression and wartime experiences as a means of demonstrating quantity-theory propositions. From the end of the Korean War through the mid-1960s, however, the experience of the United States and other key advanced economies seemed to provide less clear-cut support for quantity-theory postulates. On the one hand, they were associated with business-cycle relationships between money and the economy that Friedman judged to be generally consistent with his framework. But he conceded that, on the other hand, these years were also widely considered consistent with Keynesian interpretations of economic behavior, as well as with the broad notion that Keynesian demand-management methods (which were associated particularly with fiscal policy) had been successful in subduing business-cycle variations. Shortly before his death in 2006, Friedman observed: "On the whole, during the '50s and the '60s, it looked as if the Keynesian interpretation was right. After all, during that period, we had relatively prosperous countries, relatively stable prices, and

[132] In a similar vein, but with regard to a much later period in the country's postwar history, Buera and Nicolini (2021, p. 48) would remark that Argentina's high inflation of 1973−1991 "coincides with the worst experience in terms of output growth," with the level of real per capita GDP ultimately falling to around its 1960 level.

relatively low interest rates."[133] Earlier, in his 1987 *New Palgrave* dictionary entry on the quantity theory of money, Friedman noted: "The apparent success during the 1950s and 1960s of governments committed to a Keynesian full-employment policy in achieving rapid economic growth, a high degree of economic stability, and relatively stable prices and interest rates, for a time strongly reinforced belief in the initial Keynesian views about the unimportance of variations in the nominal quantity of money."[134]

Against this background, the availability of Latin American data—encompassing the postwar experience of Chile and other countries of inflation rates far into double digits—may have provided impetus to the acceptance among a number of Friedman's students and colleagues of the empirical relevance of the quantity theory. More specifically, Robert Gordon, a member of the University of Chicago's economics department for a period of years (1968–1973) that followed those in which the department had a formal student exchange program with Chile, has long stressed that—as he put it in the mid-1970s—"the close contacts of the University of Chicago with Latin America provided them with another set of empirical observations which were consistent with the natural rate hypothesis."[135]

The Latin American connections, Gordon suggested, imbued University of Chicago-based monetary economists acquired greater familiarity with the region's data and with postwar historical episodes of severe inflation situations. As indicated above, by the mid-1950s it was already possible to ascertain that these countries' outcomes indicated that triple-digit rates of monetary expansion were manifested in very rapid inflation but not in above-normal growth in output. It would also become clear on the basis of the region's experience that high-inflation periods could be associated with severe impairments of real growth. Bergoeing, P. Kehoe, T. Kehoe, and Soto (2002, p. 173) would later summarize the record as indicating that "in most Latin American countries, high inflation is often associated with large drops in output, not with substantial recoveries." This pattern was already becoming evident already in data for the 1950s and 1960s: even Paul Samuelson (*Newsweek*, February 25, 1971) acknowledged that the "cantering inflations" of Chile and Brazil had been associated with poor outcomes for real variables.
The different data pattern in Latin America, Gordon suggests, may have spurred the empirical

[133] In Russ Roberts (2006).
[134] Friedman (1987a, p. 13).
[135] For more recent discussions of this point, see Gordon's (2011, p. 16) further remarks, as well as Boianovsky (2020).

acceptance in the money workshop of analytical frameworks that made the price level endogenous and ensured long-run links between monetary growth, nominal interest rates, and inflation. In this way, the Latin American data contributed to the divergence in thinking between the University of Chicago and other major bases of U.S. research on aggregate economic behavior (Robert Gordon, interview, March 21, 2013). This disposition toward the natural rate hypothesis and related propositions contrasted, as Gordon (1976a, p. 54) observed, with the "forced acceptance" of the hypothesis among many other U.S.-based economists in response to the accrual over the early 1970s of incoming U.S. data on inflation and on real variables.

Gordon suggested that this background accelerated Friedman's own development of the natural rate hypothesis and made other workshop members receptive to the hypothesis. It also, he felt, was what helped make them skeptical about the notion that "monetary expansion would drive down interest rates" and subscribers to the idea that, over a period of years, empirical data were described by "the long-term model in which prices are completely flexible… so a monetary [expansion]… would cause ultimately just a change in the price level." Exposure to the Latin American data, in conjunction with the light it threw on the appropriate analytical framework, "was, I think, the key reason for the distinction between what they [at the University of Chicago] thought and what they [at other major U.S. universities] believed." (Robert Gordon, interview, March 21, 2013.)[136]

Friedman's invocation of the Latin American experience, including Chile's, was a recurring feature of his discussions of inflation and of monetary relationships in the 1960s and 1970s. Although Schliesser (2010) takes references to Chile's inflation experience in Friedman's December 1976 Nobel lecture as indicating his preoccupation with the recent controversy over his own connection with that country, this interpretation may well rest on the fact that Schliesser considers very little of Friedman's body of statements. For a notable feature of Schliesser's paper is that its confident declarations about Friedman's thinking are accompanied by very sparse citation of Friedman's work. Even Friedman's Nobel lecture and his 1982 *Newsweek* column on Chile are not referenced in their original form in Schliesser's paper, which instead relies on excerpted or reprinted versions when quoting from these articles.

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[136] Robert Lucas—who was not a member of the money workshop in his years through 1963 as an on-campus student at the University of Chicago—was initially surprised at the seeming failure of plots of Latin American data on inflation and real series to conform to an unaugmented Phillips curve (see Lucas, 1981a, p. 12). Of course, by the late 1960s he had become a strong proponent of the natural rate hypothesis himself—and, in Lucas (1973), he interpreted Latin American countries' output and inflation data as supportive of the hypothesis.

In contrast to any notion that citations of Latin American experience were newly prevalent in Friedman's discussions as of 1976, the fact is that they had appeared frequently, and prominently, in his prior statements. For one thing, as Boianovsky (2020) stresses, Friedman citations of Latin American experience were common to his 1967 American Economic Association presidential address, well before the Chile controversy, and his Nobel lecture. It is also the case, as the quotations in the first section of this chapter indicated, that Friedman also talked about the inflation/threat-to-democracy connection, raised in the Nobel lecture, before his trip to Chile, let alone before the post-Chile-trip public controversy.

In the 1967 presidential address, as in numerous other forums in surrounding years, Friedman would particularly cite Latin American countries for the evidence they supported regarding the Fisher relationship. "Studies for countries like Argentina and Brazil and Chile, countries that have had very substantial inflation, show the same phenomenon," he remarked at a 1968 banking conference held in the city of Chicago. He stressed that these nations provided strong confirmation of the interest-rate/inflation link, much more so than did the United States at that time.[137] In the same presentation, he had linked the matter to rates of monetary growth: "If I ask in what countries in the world are interest rates high, there will be widespread agreement that they are high in Brazil, Argentina, and Chile. If I [then] say, 'I take it that in those countries there are very low rates of increase in the quantity of money and that interest rates are high because money has been tight,' you will laugh at me. Those are countries which have had very rapid increase in the quantity of money and inflation."[138] Friedman gave a similar formulation in the London *Sunday Times* of September 20, 1970, again naming these three countries. Similarly, a little further into the 1970s, in Instructional Dynamics Economics Cassette Tape 75 (June 2, 1971), Friedman observed that the "much wider range of [data variation in] countries in South America… indeed fits in with what I was saying earlier about the short-run and longer-run effect of changes in the quantity of money. If you look at countries like Mexico, Brazil, Argentina, Chile, and so on, you will find that wherever you have a high degree of inflation, you have high interest rates."

When it came to the details of these countries' experiences, Friedman was relying on others' work. For example, he noted: "I am not an expert on Mexico, and I do not know anything in detail about the Mexican experience." (Instructional Dynamics Economics Cassette Tape 75, June 2, 1971.) But through his colleagues, and in part via the work of students who had been

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[137] Friedman (1968e, p. 19). See also his reference to "South American countries" on page 22.
[138] Friedman (1968e, pp. 12–13).

part of Friedman's workshop, the University of Chicago's Department of Economics became known for its studies of inflation situations in Latin America. Some of this work bore on another front of battle in the contest between monetarists and nonmonetarists: the fight between monetary vs. nonmonetary (or, in its Latin American variant, "structuralist"—see, for example, Mikesell, 1967) explanations of inflation. One of the University of Chicago economics department members active in this area, Larry Sjaastad, would remark in April 1975: "It took us fifteen years to stamp out the doctrine of structural inflation in Latin America."[139] Being a type of cost-push view of inflation, the structuralist position pointed to the desirability of direct government intervention in the price-setting process. Consequently, Friedman recalled structuralist policies in the 1950s and 1960s as being packages of measures that interfered with the operation of the market.[140]

Friedman's commentary on Chile during the Allende tenure (*1970 to 1973*)

Following his election victory in September, Salvador Allende ascended to the presidency of Chile on November 3, 1970. In his regular media commentaries and various additional op-eds during the 1970–1973 Allende tenure, Friedman's remarks on the policies of Chile's government were quite limited. One of its actions to which he did make reference on more than one occasion was the takeover of copper mines that had hitherto been under U.S. corporate ownership. Friedman criticized the carrying-out of the nationalization—the fact that the government's acquisition took place at below-market prices (*Newsweek,* August 16, 1971; *New York Times*, October 29, 1971).

This was a feature that marked the move out from some other nationalizations around the world, of both domestically-owned and foreign-owned firms or industries. Friedman was, of course, frequently, and outspokenly, skeptical regarding the economic merit of nationalizations. But the cases in which nationalization did not involve market-consistent compensation of the previous owners was, in his view, a breed apart—being deserving of special criticism, as it amounted to clear-cut public-sector confiscation of resources. A decade earlier, he had criticized the nationalization of real estate and firms' capital stock in Castro's Cuba. Friedman recalled these Castro nationalizations in the same August 1971 *Newsweek* passage that contained his criticism

[139] In Birnbaum and Laffer (1976, p. 184).
[140] Friedman (1981a, p. 7).

of the 1971 Allende copper nationalization.[141]

This distinction between different ways of proceeding with nationalization was, in fact, widely made. In particular, the fact that the nationalization of the 1956 Suez Canal Company by the Nasser-led government of Egypt took place at market prices was a feature of the move that was regarded as preventing the U.K. government from making a viable legal case against this takeover of what had previously been, to a large degree, a U.K.-owned firm (Love, 1969, p. 365; *Daily Telegraph* (London), June 10, 1995; Thorpe, 2005, p. 478).

Paul Samuelson similarly highlighted the Allende nationalization moves as breaking with commercial norms and as raising the specter of further expropriation of American companies' funds invested abroad. "We have only to look at Chile and other countries to realize that we should always keep our fingers crossed—and that there is an element of contingency in our ownership abroad," he remarked during Allende's first year in office (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 81, July 26, 1971).[142] Samuelson later added (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 103, June 5, 1972): "Can one really believe that in the last three decades of the twentieth century, the rest of the world can be confidently counted on to permit the continuing flow of dividends, repatriation of earnings and royalties to large corporations [that are] owned here? I don't think I am paranoid to raise a doubt in this matter. Think of Chile, think of Cuba."

The nationalization steps that both Friedman and Samuelson discussed were part of an overall radical economic program of the overtly socialist Allende Chile government. On account of

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[141] See Friedman (1961a, p. 73) and *Newsweek*, August 16, 1971. Both these 1961 and 1971 discussions were actually made primarily in the context of criticizing historical *U.S.* government policy. In both pieces, Friedman noted that the U.S. government's requirement in the mid-1930s to have citizens turn in their gold holdings was analogous to the Castro or Castro-Allende procedure of implementing nationalization through forced sales to the government at below-market prices. In particular, in August 1971 Friedman argued that, on account of the 1933 U.S. precedent that the U.S. government did "not have a leg to stand on" in objecting to the cases of foreign expropriation like the recent nationalization in Chile. His position was, of course, not that expropriation was appropriate or excusable but that there were (also) examples of U.S. government measures that were tantamount to this practice. His manner of putting the matter in these terms was not likely to have been particularly amenable to the Nixon Administration, which during 1971 was making vigorous public and private objections to the details of the Chile copper nationalization. Notably, on behalf of the administration Admiral Elmo Zumwalt made the case directly to Allende, in a February 1971 meeting with Chile's president, that, while the U.S. government accepted Chile's prerogative to nationalize U.S.-owned firms, it maintained that such moves, when carried out, should involve adequate compensation of the private enterprises affected. See McElveen and Siekmeier (2014, p. 571).
[142] These words were spoken not long after the Chilean government's copper nationalization had become effective (on July 16, 1971), ahead of a Chilean-government-time-limited period of negotiation on compensation. See McElveen and Siekmeyer (2014, p. 656).

Allende's own Marxist outlook and various specific steps and postures taken by his administration, including its attitudes toward U.S. businesses, his government's policies were regarded by observers as part of the overall ongoing contest between the United States and the USSR.

Both sides of the geopolitical contest saw Chile in such terms. Garthoff (1994, p. 541) remarked: "The Soviet leaders… staked much hope [on] Salvador Allende's Chile." Similarly, Gouré and Rothenberg (1975, p. 97) noted that "Moscow attributed particular importance to the… election of Allende to the presidency of Chile," and they quoted an August 1971 item in the USSR state media as having stated that Allende's election was "second only to the victory of the Cuban revolution in the magnitude of its significance as a revolutionary blow to the imperialist system in Latin America."

Whereas, to the Soviet authorities, Chile was seen as demonstrating the possibility of a democratic transition to a Communist state, in the United States it was a source of concern as a possible spearhead for revolutionary governments in Latin America. Within the U.S. government, the likelihood that Allende would move domestic arrangements toward those associated with collectivist socialism was foreshadowed, with a CIA report of July 30, 1970, stating: "An Allende administration would proceed as rapidly towards establishment of a Marxist-Socialist state as the circumstances permitted."[143] This analysis also emphasized possible geopolitical repercussions: "Allende… would almost certainly take harsh measures against U.S. business interests in Chile and challenge U.S. policies in the hemisphere. The hostility of Allende and his allies towards the U.S. is too deeply rooted to be easily changed. On key international issues, which involved any kind of an East-West confrontation, an Allende administration would be openly hostile to U.S. interests or at best neutral."[144]

As these comments implied, the global contest with the Soviet Union helped motivate the steps that the Nixon Administration took to stop Allende. Its attempts included covert efforts to influence Chile's election results; then attempts to stop Allende's swearing-in after his election victory; and, after Allende's swearing-in, actions intended to undermine the government and to support its replacement, including removal by undemocratic means.

[143] In McElveen and Siekmeier (2014, p. 125).
[144] In McElveen and Siekmeier (2014, p. 121).

The introductory text of the U.S. State Department volume that provided historical documents related to U.S. policy toward Chile over 1969–1973 noted that the period from 1970 to 1973 saw administration efforts during 1970 "to block Allende, either by constitutional means or by military coup" and that the Allende years saw the administration implement a "policy to destabilize the Chilean government while simultaneously strengthening ties with the Chilean military."[145] As part of this process, as Garthoff (1994, p. 357) observed, covert U.S. government action in Chile, which spanned the period from 1963 to 1973 and had included the financing of Allende's political opponents in the 1964 and 1970 Chilean elections, was stepped up from 1971 onward.

Various departments of the U.S. government were involved in the internal administration discussions of Chile, including the U.S. Treasury, whose head John Connally sent a memorandum to Nixon on August 10, 1971, expressing concern that Chile's practice of incomplete- compensation nationalizations of U.S.-owned firms might "become a blueprint" for other countries (McElveen and Siekmeier, 2014, p. 656), and who produced a further memorandum on January 15, 1972, in which Connally provided suggestions to the president regarding what measures would further the administration's aim to "keep maximum pressure on Chile" (McElveen and Siekmeier 2014, p. 757).

In the Department of Defense, as detailed by Kuehn (2019), during the first Nixon term G. Warren Nutter was one of those receptive toward the idea of a Chilean military coup to remove Allende—although, in the event, the actual Pinochet coup occurred in September 1973, after Nutter was out of government. Nutter was, of course, a one-time dissertation student of Friedman's. His job in the Nixon Administration lay, however, outside economics, and so he was—unlike George Shultz, for example—not one of the figures in the administration with whom Friedman was in close contact over the 1969–1973 period.

Friedman's decision to travel to post-coup Chile and anticipations of the controversy

Friedman's personal involvement regarding events in Chile came when he accepted an invitation from Harberger to accompany him on a trip to the country that took place in late March 1975.[146] Harberger himself would later publicly confirm that Friedman's going to Chile was "largely at my urging" (*Wall Street Journal*, December 12, 1976*a*).

[145] McElveen and Siekmeyer (2014, p. V).
[146] See Friedman and Friedman (1998, pp. 398–399).

The trip to Chile had been announced in that country's press in December 1974 (Farrant, 2020, p. 124). The fact of the forthcoming trip was therefore publicly known, and on the basis of the resulting reaction Friedman knew in advance of the trip that his going to the country would certainly be controversial. In mid-March 1975, he remarked publicly, albeit in the low-circulation outlet of his audio commentary series (Instructional Dynamics Economics Cassette Tape 166, March 1975): "I have a great problem with many friends of mine who always say: 'Well, now, how can you go to places like Brazil and Chile, when they are military dictatorships?' Well, going to 'em doesn't mean approving of their political system. But, on the other hand, given their political system, it is certainly desirable that it operate as best as possible in an economic sense. And, more important from our point of view, you ought to be willing to learn from all kinds of regimes, whether they be totalitarian or not. They have lessons to teach us. And, as scientific observers, I don't believe we ought to throw away the opportunity to get evidence on things we're interested in."[147]

This commentary articulated a number of the arguments that would feature in Friedman's many post-March 1975 defenses of his trip. The notion that going to a country or being involved in improving its economic system did not connote "approving of their political system" would recur in Friedman's many remarks on Chile during 1975−1976, and in later years too. Another statement—"you ought to be willing to learn from all kinds of regimes"—reflected the same line of argument that he advanced in his February 1975 correspondence with academic Gerhard Tintner, who, due to the controversy over Chile, would turn from one of Friedman's longtime friends to a determined critic, and who wrote privately to Friedman to criticize his willingness to make the trip (see Farrant, 2020, pp. 112−118). Friedman's February 1975 statement to Tintner that "I shall be going entirely as an observer" (quoted in Farrant, 2020, p. 113) lined up with his March 1975 taped commentary above putting himself in the category of "scientific observers," as well as his remark in the summer of 1977 that his trip to Chile had been motivated by the fact that "as a scholar I wanted to see what an honest-to-God inflation was like" (*Reason* magazine, August 1977, p. 25) and his likening, in a discussion that he made earlier in 1977, of his trip to

[147] As was implied by these remarks and those (quoted in Chapter 4 above) that he made regarding Brazil, Friedman did not put great stock in using the "totalitarian" and "authoritarian" terms as a means of distinguishing between different types of dictatorships. Outside economics, some defense hardliners, including Brian Crozier (1978, pp. 200–201) and, most famously, Jeanne Kirkpatrick, did utilize these terms to distinguish between Communist and some military dictatorships and only called the former "totalitarian." In practice, however, Friedman did make a version of this distinction between authoritarian regimes and Communist regimes (an early example being his commentary in Instructional Dynamics Economics Cassette Tape 137, January 4, 1974, with later examples considered below), even though he was not strict in making terminological distinctions between them.

natural scientists' wish to go to countries that featured the natural phenomena that they studied (*Stanford Daily* (Stanford University), January 31, 1977, p. 1).

At cross-purposes with this observer status was his indication, already made ahead of the trip, that he wanted, as he put it in the March 1975 commentary quoted above, to help in the process of making the country "operate as best as possible in an economic sense" and that his dialogue in Chile would include articulation of his economic diagnoses and related policy recommendations. As part of this process, Friedman's trip would include meetings with members of the government. Indeed, in early 1981 he described communicating "the latest findings about [inflation's] cause and cure" as well as the case for a market-based economy as a reason his interlocutors wanted to speak to him in Chile, and he indicated that this was true both of his meetings with both government and non-government figures (*Newsweek*, January 12, 1981).

Although Friedman had clearly been forewarned to some extent of the controversy that would be associated with his trip to Chile, he likely believed that—as had basically occurred with respect to his 1973 visit to Brazil—he would be able to ride out initial criticism of his trip and obtain acceptance of the message that his trip, and discussion of the country's economic arrangements, did not connote an endorsement of the political regime.

Nevertheless, on account of the Pinochet regime being the subject of widespread condemnation and related news coverage, Friedman likely knew that the Chile trip would be more controversial than that he had made to Brazil. Although he did not anticipate the size of the backlash that would occur, in making the arrangements concerning the trip to Chile, he and Harberger took steps that Friedman evidently believed, erroneously as it turned out, would separate him in public perception from the Chile junta. First, the funding of the trip did not come from the government: Harberger would note that the trip was financed "under the auspices of a private Chilean foundation" (*Wall Street Journal*, December 10, 1976*a*), while Friedman variously described the trip as being made possible "under private auspices" (*California* magazine, October 1984, p. 78), "under the auspices of a private foundation" (*Newsweek*, January 12, 1981), "under the auspices of a private bank" (*Meet the Press*, NBC, October 24, 1976, p. 3 of transcript), "under the auspices of a private Chilean bank" (*Newsweek*, June 14, 1976*b*), "at the behest of a private foundation," and "under the auspices of the Banco Hipotecario."[148] These various descriptions

[148] The final two quotations are from Friedman (1977k, p. 491) and Friedman and Friedman (1998, p. 399), respectively.

were consistent with one another, as the foundation in question was one financed by the named Chilean bank or bank holding company (Edwards and Montes, 2020, p. 110).

A second step that Friedman took to try to forestall the trip being seen as an endorsement of the regime was to turn down the offer by two universities to give him honorary degrees during his trip (*Wall Street Journal*, December 10, 1976*a*). His grounds for rejection were that the universities received government funds: "I refused to accept the degrees, because I did not want to seem to be expressing approval of the political system in Chile."[149]

These steps at making the trip one not perceived as associating Friedman with the government would be to little avail, in view of the fact that he met junta members, including Pinochet, during the trip and the additional fact that the government was, in early 1975, contemplating economic proposals advanced by a number of the Chilean one-time University of Chicago economics students. Friedman was also traveling to Chile during a time when the U.S. government's covert operations in the country were being disclosed. Friedman's hope to avoid a major controversy in these circumstances, Paul Samuelson would suggest, demonstrated that "Milton is the most naïve fellow in the world" (*Sunday Times* (London), December 12, 1976).

Public appearances in Chile

"While in Chile, I gave a series of public lectures, some of which have been published," Friedman remarked in a 1977 defense of his trip. "I also talked to a variety of groups of people."[150] A detailed account of Friedman's public engagements and the content of his speeches is provided in Edwards and Montes (2020).[151]

In their accounts of their March 1975 time in Chile, both Friedman and Harberger would stress that Friedman's public lectures included the delivery at two universities of his lecture "The Fragility of Freedom," a version of which Friedman later also gave in the United States in a talk at Brigham University, Utah, in December 1975.[152] In his late-1976 defense of the trip and

[149] Friedman (1977k, p. 491).
[150] Friedman (1977k, p. 491).
[151] Importantly, their coverage includes press interviews. In addition, a collection, issued by the bank that had hosted him, of some of Friedman's public remarks was published in Chile in Spanish: see Friedman (1975k).
[152] Although Edwards and Montes (2020, p. 114) treat the Utah speech's contents as something that has to be inferred from other sources—such as variants of the speech that appeared in *Encounter* (Friedman, 1976c) and in Feldberg, Jowell, and Mulholland (1976)—the Utah speech itself actually appeared in print twice in the United

Friedman's participation in it, Harberger stressed that the Friedman lecture "characterized the present government of Chile as one which was denying and curtailing freedom in many important ways, and expressed the hope that in the near future Chileans would once again enjoy a full measure of political and intellectual liberty." (*Wall Street Journal*, December 12, 1976*a*.) Harberger's discussion also indicated that Friedman met privately with opponents of the junta during his trip—a point that Friedman's own accounts also brought up on a number of occasions.[153]

Friedman's meeting with Pinochet

Friedman's trip also included numerous meetings with members of the government. He knew well in advance of the trip that, at a minimum, the government officials whom he would be meeting would include former University of Chicago students now having economic positions in the regime (*Reason* magazine, August 1977, p. 25). Such meetings took place, but, in addition, Friedman during his trip met with members of the ruling junta. In particular, on March 21, 1975, he had a 45-minute meeting with Pinochet, with Harberger, L*ü*ders, and others also in attendance.[154] Friedman, in an *aide memoire* written shortly afterward, noted that, in recommending sharp monetary and fiscal restriction as well as free-market reforms, "I was probably just repeating what others had said" to Pinochet.[155] As already indicated, the junta was, at that time, considering the adoption of economic measures recommended, in good part, by some of the Chilean-based University of Chicago-trained economists. Friedman also voiced at the meeting the view that economic liberalization would promote forces of political liberalization (Edwards and Montes, 2020, p. 110). This position lined up with a major theme in many public statements he had made on economic and political freedom. The linkage that he perceived, as outlined in those statements, will be discussed further below.

In his 1998 memoirs, Friedman acknowledged that his having this meeting provided an "iota of substance" to the characterization that his critics would give of him of being a Pinochet adviser.[156]

States during 1976: see Friedman (1976j, 1976k). Of these, the latter Friedman reference was a printing of the speech in the *Congressional Record* instigated by Senator Barry Goldwater.

[153] See, for example, *Reason* magazine, August 1977 (p. 25), *Newsweek*, January 12, 1981, and Friedman (1977k, p. 491).

[154] See Friedman and Friedman (1998, p. 591) and Edwards and Montes (2020, p. 110).

[155] See Friedman (1975l).

[156] Friedman and Friedman (1998, p. 399).

Post-trip correspondence with Pinochet

At their meeting, Pinochet asked Friedman to write his recommendations down in a post-trip letter. Friedman did so, in a letter dated April 21, 1975.[157] Friedman would make statements in later years that—with the important exception of the negative subsequent U.S. media commentary that it generated—"I have no regrets" about the fact that he had gone to Chile (*Business Week*, November 1, 1976, p. 75) and that he "would not hesitate to do it again" if he had his time over (*Reason* magazine, August 1977, p. 25). He further indicated that "I do not regret the advice I gave there" (*Business Week*, November 1, 1976, p. 75). With regard to the April 1975 letter, however, it may well have been a different story. He likely started to acquire doubts very quickly about whether he should written it. Friedman's concern about it coming into the public record was indicated in correspondence that Friedman wrote to his former student, U.K. financial journalist Samuel Brittan. In the letter to Brittan dated August 21, 1975, Friedman enclosed the letter of recommendations regarding Chile but suggested that, as it had been a letter addressed to Pinochet directly, it might be preferable for Brittan to destroy the enclosure after reading it.[158]

This vantage point of August 1975 was an early time in the controversy and, in subsequent years, Friedman found that he had to discuss his involvement with Chile over and over again. Indeed, his 1998 memoirs with Rose Friedman included not only a twelve-page chapter on Chile, but also a further twelve pages, in small print, about Chile, comprising the authors' Appendix A.[159] That appendix, although it collected several of Friedman's key replies to critics during the height of the controversy in the mid-1970s, also underlined a key weakness and inaccuracy of his early rebuttals. As detailed below, Friedman's rebuttals stated on multiple occasions that his contacts with the Chilean government had been wholly contained within his March 1975 trip to Chile. But the appendix provided the hitherto unpublished April 1975 letter to Pinochet, as well as Pinochet's reply of May 16, 1975.[160]

The Friedman letter to Pinochet had put in written form the policy recommendations that he had

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[157] See Friedman and Friedman (1998, pp. 591−594). Schliesser (2010, p. 195) provides a timeline of events on Chile and Friedman in 1973−1976 that is useful but that contains an error in giving the letter to Pinochet as being dated April 25, 1975, instead of April 21.
[158] Letter from Milton Friedman to Samuel Brittan, August 21, 1975 (p. 2), Milton Friedman Papers, Hoover Institution archives (Box 21, Folder 33).
[159] See Friedman and Friedman (1998, pp. 397−408, 591−602).
[160] See Friedman and Friedman (1998, p. 594).

made while in Chile and so was linked to that trip. But its existence rendered unequivocally false the blanket statements—which he would make in 1976, in particular—that his contacts with the Chilean government had been limited solely to the trip.

More specifically, the fact that the trip had been followed up by correspondence undermined one of his specific defenses: that, as he put it in 1984, the trip had been his "only involvement in the Chilean business" (*California* magazine, October 1984, p. 77). "I had not seen any Chilean official before that; I have not seen and have had no contact with Chilean officials afterwards," he remarked on television (*Meet the Press*, NBC, October 24, 1976, p. 3 of transcript). In a number of replies to criticisms (for example, *Newsweek*, June 14, 1976*b*), he similarly described the March 1975 trip as "my only contact with Chilean governmental officials."[161] And in *Business Week* (November 1, 1976, p. 75), he stated: "I had no contact with people in Chile prior to the visit, and have had none since."[162]

Subsequently, in early 1977, Friedman remarked (*Stanford Daily* (Stanford University), January 31, 1977, p. 1), "I have had no contact with any official of Chile since then," and his interviewer seemed to take "then" as being March 1975. The following summer, he stated: "Since leaving Chile, I have had no advising or counseling contact with anybody in the Chilean government." (*Reason* magazine, August 1977, p. 25.)

In December 1977, in the high-profile venue of a *New York Times* interview, Friedman relayed the important piece of information that, post-trip, he had sent a write-up of his analysis of Chile to Pinochet and to the other individuals who had been at their meeting (*New York Times*, December 2, 1977). This was the letter that would see print in his memoirs in 1998.[163]

In some of his pre-December 1977 characterizations of the degree of his contact with Chilean officials, Friedman may have been trying to include the post-trip letter under the umbrella of his trip. But the existence of the letter meant that the categorical statements that he made before December 1977 indicating that his contact with the Chilean government had been wholly limited to his March 1975 travel to Chile were certainly not literally accurate.

[161] Friedman used identical wording in *Daily Telegraph* (London), June 13, 1977, except to say "government" rather than "governmental."
[162] Also quoted in *Edwardsville Intelligencer* (Illinois), November 3, 1976.
[163] Especially in view of the fact that it had multiple recipients, the existence of the letter-memorandum was very likely public knowledge before 1977, and reference to it may have occurred in the course of what the *New York Times* (March 21, 1976) characterized as the virtually-every-week references to Friedman in the Chilean press.

The initial post-trip backlash, April to December 1975

The initial public controversy in the United States concerning Friedman's trip spanned the period from April to December 1975. As this period opened, the Pinochet junta was being further ostracized in international circles, with its human-rights abuses receiving additional scrutiny. For example, the London *Times*—which had been very critical of the Allende government—editorialized that Pinochet's "internal political policies" were causing "Chile's economic isolation" in the international arena, and it added: "Evidence of torture, arbitrary arrest[,] and imprisonment without trial is now overwhelming." (*The Times* (London), April 19, 1975.)

The news about the Pinochet regime's ongoing abuses came against a background of greater details emerging about U.S. government involvement from 1970 to 1973 in undermining the Allende administration. On September 7, 1974, six months before Friedman's trip, it had already been revealed that in the previous April that the CIA's director William Colby had confirmed to a Congressional committee that $8,000,000 had been allocated in 1970 to 1973 for various U.S. operations against the Allende Government (Carruth, 1993, p. 702). The end of 1975 would see further details come to light, with the disclosure on December 4, 1975, that the U.S. government had spent $13,400,000 from 1963 to 1973 on Chile-related covert activities.[164]

Attention to Chile in the United States from 1973 to 1976 was therefore high because of the Pinochet junta's abuses and because of the revelations about U.S. government interference in pre-1973 political developments. But there was, from 1975 on, a further major factor drawing world attention to Chile: the fact that its economic policy changed sharply.

As Friedman would later note on numerous occasions, University of Chicago-associated economic policies against inflation, involving a major demand restriction, had not been implemented by the Pinochet junta in 1973 or 1974.[165] Rather, policies of this kind were introduced in the spring of 1975 (Van Overtfeldt, 2007, p. 350)—more specifically, around April and early May 1975 (*New York Times*, May 6, 1975; *Wall Street Journal*, November 4, 1975, p. 1). The package implemented, comprising severe monetary and fiscal restriction alongside free-market reforms, was unambiguously associated with the University of Chicago, as it was

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[164] See Caruth (1993, p. 714). The December 1975 disclosures, resulting from Congressional inquiries, are discussed further below.
[165] See, for example, *California* magazine (October 1984, p. 77), Friedman (1991b), and Friedman and Friedman (1998, p. 398).

designed in large part by a portion of these graduate students who later worked in economic posts in the government of the Pinochet junta. This group became widely referred to as "the Chicago Boys."[166] As Friedman's *aide memoire* of 1975 noted, the package was already being contemplated by the government while he was visiting the country. But the fact that its actual adoption came soon after his trip would compound the perception of a strong link between himself and the regime—and the related interpretation that the package was designed personally by himself.

Such interpretations started appearing in the U.K. and U.S. press in the spring and summer of 1975. The London *Observer* of April 27, 1975, stated: "This week, Pinochet's economic overlord, Jorge Cauas, announced a package of austerity measures, designed by the conservative U.S. economist Milton Friedman, to try to shock the economy out of the present raging inflation and save it from complete bankruptcy." In the *Houston Post* of July 14, 1975, columnist W.D. Bedell wrote that Cauas' "actions were reportedly based on advice received from Milton Friedman, the University of Chicago economist who visited Chile in March," while noting that "Friedman is said to have advised 'shock treatment' for the economy." Bedell later linked the policy change further to Friedman's visit by describing Cauas as "a disciple of Friedman" who "had already begun to put some Friedman theories of unshackled enterprise into practice in Chile and after the Friedman visit this process was stepped up" (*Houston Post* (Texas), November 17, 1975).

By the time of Bedell's second column, the controversy in the United States about Friedman's visit to Chile had risen in profile, in large part because of prominent coverage in the *New York Times*. Friedman would sometimes give a highly critical piece written by the newspaper's regular columnist Anthony Lewis (in the edition of October 2, 1975)—an article discussed further below—as what really launched the controversy (Instructional Dynamics Economics Cassette Tape 179, October 1975, Part 2; *Newsweek*, January 12, 1981). But he would later trace the onset of the U.S. controversy to another, earlier *New York Times* article—a front-page news

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[166] Friedman himself on occasion referred to "the so-called Chicago Boys" (*Newsweek*, January 25, 1982; Friedman, 1991b, p. 2; *Wall Street Journal*, January 8, 1991), and he also accepted and deployed the "Chicago Boys" terminology in Friedman and Friedman (1998, pp. 398, 408). Dornbusch (2001, p. 20) noted the fact that the term tended to be used in relation to the former students both by critics and defenders of the economic prescriptions with which they were associated.

As the present discussion concerns the controversy, in U.S. public debate during the 1970s, swirling around Friedman on the matter of Chile, a detailed discussion of the Chicago Boys or of Chile's economic policy and economic outcomes is certainly beyond the scope of this account. See Edwards and Edwards (1987) and Edwards and Montes (2020) for background and references on these subjects.

report of September 21, 1975.[167] This article had discussed Chile's current, 40-year-record, rates of unemployment and had observed: "The guiding light of the junta's economic policy has been Milton Friedman, a conservative economist from the University of Chicago who visited Santiago shortly before the 'shock treatment' program took effect. The 'Chicago Boys,' as the junta's economic advisers like to call themselves[,] slashed public spending, restricted bank credits[,] and slowed the printing of money." (*New York Times*, September 21, 1975, p. 30.) The following day, an editorial in the *New York Times* stated that "the economic chaos left by Dr. Allende's Marxist administration" had continued, and in this connection suggested that 20 percent unemployment and a continuing "fantastic inflation rate" should be the basis for judging that "after many months of applying Prof. Milton Friedman's monetary theories and harsh austerity programs," the measures had not succeeded (*New York Times*, September 22, 1975).

The surge in national attention toward Friedman on the subject of Chile in the late September/early October 1975 period had an effect on his day-to-day activities at the University of Chicago, as student protests arose from the controversy. He had experienced campus picketing in the past, notably when giving a talk in California in 1966/1967 year (*Fortune*, June 1, 1967, p. 131). But the University of Chicago in the late 1960s and early 1970s had been seen as a major academic institution that avoided many of the disruptions seen elsewhere, with the university's administration having established a harder disciplinary line against student protests (*The Ithaca Journal* (New York), March 4, 1970). Friedman had highlighted this aspect of the university's experience in a *Newsweek* column at the time (April 14, 1969), although the same discussion had expressed doubt about whether the protest-related violence seen on other campuses might be avoided at his own institution in the future. In early 1975—before his Chile trip—Friedman recounted that the University of Chicago had had only a limited experience with the student protest movement of the late 1960s and also implied that the United States' experience of major student protests was now long past (Instructional Dynamics Economics Cassette Tape 163, early February 1975).[168] Even in December 1975—by which time he had become the subject of sustained student protests—Friedman was referring to the protest era in the past tense when he referred to the "times of trouble during the late 1960s when many universities in this country were subjected to disturbances and disruptions."[169]

[167] Friedman and Friedman (1998, p. 401).
[168] In both the 1969 and 1975 discussions, Friedman credited the university's administration with stalling the momentum of the protest movement by swiftly applying penalties to those students who generated disruptions.
[169] Friedman (1976j, p. 561).

But as he said these words, U.S. campus protest activity was actually obtaining new vitality in the Greater Chicago area, in the wake of adverse responses to Friedman's visit to Chile. The University of Chicago's student newspaper, the *Chicago Maroon*, stated (October 3, 1975*a*, p. 1): "Left-wing campus and area organizations… have formed a united front to protest the involvement of University professors Milton Friedman and Arnold Harberger in policymaking for the ruling military junta in Chile." The article indicated that the new committee described its aim as to "drive Friedman and Harberger off the campus" (p. 3).

The same October 3 issue of the *Chicago Maroon* had multiple articles on the Chile controversy and the emerging local protest movement. On the same day, there was a campus rally attended by about 225 people (*Chicago Daily News*, October 3, 1975). There would be a string of student protests and other demonstrations on the University of Chicago campus in 1975−1976 against Friedman. Some of them were held outside his Price Theory classes (Gloria Valentine, interview, April 1, 2013). Another incident occurred on February 28, 1976, when the University of Chicago hosted a panel on the topic of government spending that also featured Northwestern University's Robert Eisner (in one of the last of the plethora of debates that Friedman had held over two decades with that locally based Keynesian). About six protesters attended and caused multiple interruptions (*Daily Northwestern* (Northwestern University, Illinois), March 1, 1976).

A measure of the degree to which Friedman, despite a long record of controversy, was unaccustomed to picketing and protesting until this point was brought out by the fact that, in 1975, Friedman included his home address in Chicago in his *Who's Who in America 1976* entry (Marquis Who's Who, Inc., 1976, p. 1080).

In the October 3 issue of the *Chicago Maroon*, Friedman provided for the newspaper's printing a private letter that he had written in July in defense of his trip.[170] In one of many underestimations that he would make of the scale of the controversy, Friedman suggested that his putting of this letter into the public record would be his only comment on the matter (*Chicago Maroon*, October 3, 1975*c*, p. 1).

The Friedman letter (*Chicago Maroon*, October 3, 1975*a*) included many of the arguments that he would make subsequently, including that the economic liberalization that he supported would

[170] Although the published version of the letter deleted the identity of the individual to whom he had addressed, the coverage of the letter in *Business Week* (January 12, 1976, p. 70) essentially identified the recipient as Gerhard Tintner. Later discussions such as Farrant (2020, p. 117) have confirmed this identity.

improve the lot of the populace and help speed the restoration of political freedom.

The campus controversy was reported in the city-wide Chicago press on October 3 and 4, 1975. The press coverage included remarks by Harberger in which he cited support for his former students (*Chicago Daily News*, October 4, 1975): "I am eager to be supportive as I can of my former students in their efforts to improve the economy of Chile or any other country. I won't be bulldozed into turning my back on men I perceive as honorable."

In contrast, Friedman was reported to be in Vermont and unavailable for comment. But he would soon realize that his intention to refrain henceforth from commenting on the matter was unsustainable, and he provided a long discussion of the controversy later in the month on his audio commentary series. Friedman's discussion recognized, as Harberger's had, that there was heterogeneity among their former graduate students on the matter of serving in the junta regime: "Some of them have become refugees from Chile under the present government, but some of them decided that they could contribute more to their country by staying and working within the government." (Instructional Dynamics Economics Cassette Tape 179, October 1975, Part 2.) Reflecting the continued national interest in the controversy, Friedman's *Chicago Maroon* letter was reprinted in the *Wall Street Journal* near the end of the month (October 27, 1975).

Although Friedman was, in this letter and elsewhere, at pains to stress that he was not the designer of Chile's new economic policy, he also made clear that he believed that that policy was appropriate.[171] "If they can hold on for a few more months, I have great confidence you'll see some good things from the economy." (*Wall Street Journal*, November 4, 1975, p. 37.) Friedman's general opposition to *U*-turns in times when monetary restriction was being applied against inflation at the cost of recession was something he had voiced in the U.S. context on numerous occasions. Urging that aggregate demand policy not change course in the more drastic economic situation that Chile experienced in 1975 was, therefore, consistent with Friedman's preexisting posture. But, combined with the fact that Chile was a dictatorship, Friedman's injunctions against *U*-turns likely reinforced critics' portrayal of Friedman as "Draconian," as the heading of a *Business Week* item on the matter put it (January 12, 1976). The *Business Week* article in question ended with a Friedman quotation on the Chilean demand restriction: "My only

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[171] Harberger, too, had stated (*Chicago Maroon*, October 3, 1975*c*, p. 1): "the government budgetary policy in the broad, and the monetary policy treated broadly, both in a general way correspond to what I believe and to what Professor Friedman was saying."

concern is that they push it long enough and hard enough" (p. 72), and this quotation was, not surprisingly, later highlighted by Naomi Klein (2007, p. 101).

Controversy in January-September 1976

The January 1976 *Business Week* article was an early entry in a major buildup over the course of 1976 in the American and U.K. media in the controversy over Friedman and Chile. By late October 1976, the accumulation of coverage had led him to complain about "the utter irresponsibility of American publications, including *Business Week*, in dealing with this." (*Business Week*, November 1, 1976, p. 75.) The specific *Business Week* item to which Friedman was referring was that in the January 12 edition. Although this long article did relay some of the points that Friedman had made in defending himself, it was mainly negative about both him and Harberger, suggesting that they "conducted themselves inappropriately by granting numerous public appearances and media interviews while in Chile," while expressing doubt about Friedman's contention (subsequently confirmed, as noted above) that he had called for political freedom during his public appearances (*Business Week*, January 12, 1976, pp. 70, 71).

The *Business Week* article also underlined the U.S. government's involvement in Chile's internal affairs during the Allende years. A hearing was held on December 4–5, 1975, by the U.S. Senate's Select Committee To Study Governmental Operations With Respect to Intelligence Activities on Covert Action in Chile, 1963−1973. The published volume of testimony was accompanied by a staff report. A report author, William G. Miller, in summarizing its conclusions, noted: "In the period 1970 through 1973, in order to prevent a Marxist leader from coming to power by democratic means, the United States worked through covert action to subvert democratic processes. The means used went far beyond those used in 1964 in money [spent], propaganda, and political manipulation. The means used were economic warfare, the encouragement of a *coup d'etat*[,] and military violence." Miller's testimony further recorded that much of the covert U.S. effort had been intended to tilt the balance in favor of a rival political party, but this move had backfired, because the junta had instead assumed permanent control of the country, instead of ceding rule to a multiparty political system.[172] Another staff member, Gregory F. Treverton, elaborated at the same hearing: "After the coup[,] the military junta moved quickly to consolidate its political power. Political parties were banned, Congress was put in indefinite recess, and censorship was instituted. Supporters of Allende and others

[172] Testimony of December 4, 1975, in Select Committee To Study Governmental Operations With Respect to Intelligence Activities, U.S. Senate (1976, p. 5).

deemed opponents of the new regime were jailed, and the military leader, Augusto Pinochet, indicated that the military might have to rule Chile for two generations."[173]

Garthoff (1994, p. 403) referred to "the suspicion of American involvement (later confirmed in part)" in the overthrow of Allende and the events leading up to it, and the Congressional inquiries contributed to this process of documenting the U.S. government's attempts to influence developments in Chile during 1970 to 1973. It was against the background of these revelations that Democratic presidential candidate Jimmy Carter, in the televised debate that he had with President Ford on October 6, 1976, named among the "kinds of things [that] have hurt us very much" in international standing "the destruction of elected governments, like in Chile."

The January 1976 article in *Business Week* pointed to a "new ingredient" that linked together the news stories of Chile's new economic policy and the U.S. policy against Allende. Specifically, the Senate committee's recent revelations included the fact that some of the CIA funds had involved the financing during the Allende years of critiques of government economic policies, with the Chilean economists who wrote these critiques included numerous University of Chicago graduates. In this environment, in late 1975 and early 1976 Friedman felt it necessary both to reject the notions that he was involved in the 1973 coup itself—"I had nothing to do with [the junta's] establishment" (*Chicago Maroon*, October 3, 1975*a*)—and that he had ever participated in any covert operations: "I have never had any knowing relationship with the CIA; nor, to my knowledge, have my colleagues," he remarked to *Business Week* (January 12, 1976, p. 70). Friedman felt that, although the *Business Week* piece quoted this denial, the article as a whole implied that he was lying about the matter, and he looked back on the report as having been "generally highly critical of Al [Harberger] and me an included the utterly fallacious allegation that we had 'uncomfortably close ties' with the Central Intelligence Agency."[174]

The issue of the junta came up in an interview that Friedman recorded in Chicago on May 11, 1976 and that was broadcast on U.K. television in mid-July. Interviewer Peter Jay asked a question that, in its formulation, took for granted that Friedman supported the junta's rule. "I have never given any public support to the political regime in Chile," Friedman shot back, adding reproachfully to Jay: "I beg your pardon." Friedman went on: "I have visited Chile for six days. While in Chile, I talked about the disease which Chile was faced with—namely,

[173] Testimony of December 4, 1975, in Select Committee To Study Governmental Operations With Respect to Intelligence Activities, U.S. Senate (1976, p. 17).
[174] Friedman and Friedman (1998, p. 402).

inflation. I tried to analyze it and talk about Chile. But never in Chile or outside of Chile have I supported the military junta. I have said that Chile was faced with a very difficult choice between—after the Allende regime, with inflation running at seven [or] eight hundred percent a year—was faced with a very difficult choice. And it came out—it was faced with a choice between two alternative totalitarian systems, both bad." The latter remark alluded to Friedman's view—which he had elaborated in the letter published early in the previous October—that Chile's economy had reached a condition that meant that the country was poised to become either a Communist totalitarian state or a military dictatorship.

In the United States, the controversy was raised in Friedman's regular media outlet of *Newsweek*, when a letter (June 14, 1976*a*) from a Minneapolis-based humanitarian group stated that it wished to express its members' "shock and dismay on learning Milton Friedman has been serving as an economic adviser to the Pinochet Chilean junta" and implied that Friedman should be dropped as one of the magazine's columnists. Friedman's reply acknowledged the trip and his having met with Pinochet while in Chile, but rejected the letter's description of himself as an economic adviser to the Pinochet junta, while adding: "I do not regard it as evil for an economist to render technical economic advice to the Chilean government to help end the plague of inflation." (*Newsweek*, June 14, 1976*b*.)

In a speaking appearance in London on August 31, 1976, upon being asked about Chile's economic program, Friedman exasperatedly articulated an observation that he would restate many times in subsequent years: "In the first place, let me make one point clear. I have not been guiding the economic policies of Chile!" As he would many times in later years, he expressed infuriation at being implied in the media and by critics to be "guiding the day-to-day policies of that government." He went on, however, to suggest that, although "I have not studied the recent statistics myself," it seemed that Chile's inflation had fallen substantially and that the period of recession associated with the new policies might have ended.[175] In a further talk given the following day, Friedman suggested that Chile's problem of a 20 percent per month inflation rate justified a shock-treatment policy rather than a gradualist policy: "the only sensible thing to do is a shock treatment, in which you make a very sharp move… and try to bring the inflation rate down to your long-term objective in a very short period."[176] He stressed that, nevertheless, he did not mean that this should involve trying to achieve a zero inflation rate immediately and that even a non-gradual or shock-treatment policy should, instead, consist of a multi-year program of

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[175] Friedman (1977i, p. 37), also in Friedman (1991a, p. 144).
[176] Friedman (1977i p. 45), also in Friedman (1978b, p. 70; 1991a, p. 151).

reductions in monetary growth and public spending.[177] But Friedman made clear that this recommended policy was still a major contrast with a gradualist program of the type he would recommend for a country like the United States.[178]

Intensified criticism, October-December 1976

On *Meet the Press* on October 24, 1976, Hobart Rowen—the *Washington Post* economics reporter and commentator who was more accustomed to asking Friedman about U.S. monetary policy topics—used the occasion of Friedman's appearance to bring up a major recent international incident. "Dr. Friedman, a few weeks ago in Washington, a man named Orlando Letelier, a former Chilean ambassador to the United States, and a former foreign minister under the Allende regime which preceded the existing military junta, was brutally killed." It would be confirmed that the Chilean government was responsible for Letelier's assassination. Letelier had been an outspoken critic of the junta, and Rowen noted that this criticism had extended to Chile's economic policy and to Friedman himself: "Just a few weeks before that, he had written an article for *The Nation* magazine [August 28, 1976] in which he condemned you for providing what you call technical advice to the Chilean government."[179] Rowen then asked Friedman to defend his role—in doing so, posing the question that was quoted early in this chapter. Owing to Letelier's activism on the economic dimension of the junta's policies, the uproar over the Letelier assassination escalated the public controversy over Friedman and Chile (see Edwards and Montes, 2020, p. 117)—a fact that Friedman acknowledged in his memoirs.[180]

Also giving impetus to the controversy in this period, of course, was the surge in attention toward Friedman generated by the October 14 announcement of his 1976 economics Nobel.

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[177] He had earlier stated that an approach to reducing inflation in which the authorities were "forced to stop it brutally—the way in which the Brazilians did in 1964" comprised too rapid a disinflation (*Newsday* (BBC2 television program), September 20, 1974, p. 5 of transcript). The parallels between Brazil's 1960s inflation experience and that of Chile in 1970s were, however, limited by the fact that, in annual data, Brazil's peak rate was about 86.6 percent, in 1964 (Krieger, 1974, Table 1, p. 44), while Chile's inflation was over 600 percent at the end of 1973 (Edwards and Edwards, 1987, Table 1–2, p. 10; see also Velasco, 1994, Table 8–2, p. 391).
[178] Friedman (1977i, p. 45), also in Friedman (1978b, p. 71; 1991a, pp. 152–153). In his letter to Pinochet, Friedman had suggested that "a shock program could end inflation in months" (Friedman and Friedman, 1998, p. 593). This was not necessarily incompatible with his public statements that a drastic program could nevertheless involve taking years to eliminate inflation though, if it was, it certainly was a simplification of the latter statements. Friedman seemed to have in mind getting annual inflation in Chile down to about 30 percent per year as an appropriate intermediate step, as he associated such a rate with conditions that Chile was long been accustomed to (*Stanford Daily* (Stanford University), January 31, 1977, p. 1).
[179] *Meet the Press*, NBC, October 24, 1976, p. 2 of transcript.
[180] Friedman and Friedman (1998, p. 402).

Two letters, each signed by two past Nobel Prize winners, were published in the *New York Times* criticizing the fact of the award to Friedman: one by George Wald (a 1967 medicine Nobel laureate) and Linus Pauling (a double Nobel laureate, in chemistry and peace), and one by David Baltimore and S.E. Luria, each of whom had won a medicine Nobel (*New York Times*, October 24, 1976*a*, *b*). These letters, which appeared on the same Sunday as the *Meet the Press* interview, were both dated ten days earlier, October 14. In his memoirs two decades on, Friedman commented sarcastically on the notion that the two letters were produced "independently" of one another and implied that it was likely that the two pairs of authors were in contact with each other when they decided to write their letters.[181] He was likely correct on this point. But it was hardly a startling practice for the generation of public letters of this kind to involve some degree of coordination.

One of the letters was worded in a manner that preempted one of Friedman's objections to the manner in which his role in the controversy had been characterized. While the Baltimore-Luria letter referred to Friedman as "a major economic adviser and supporter of the Chilean junta," the other letter, by Wald and Pauling, quoted Letelier as having described Friedman as "the intellectual architect and unofficial adviser for the team of economists now running the Chilean economy." Wald and Pauling's use in this manner of the qualifier of "unofficial" came after Friedman's June 1976 reply in a *Newsweek* letters column (June 14, 1976*b*) maintained again that he was "not now, and never have been, an economic adviser to the Pinochet Chilean junta." He would repeat this formulation in the London *Daily Telegraph* of June 13, 1977, and in October 1977 he took issue with protest leaflets that "referred to me as an economic adviser to the Chilean government."[182]

Friedman continued to eschew the "adviser" label in later years: "I have repeatedly denied this libel in print," he remarked near the end of the 1970s (*Human Behavior*, March 1979, p. 10), "I have never advised the government of Chile," he remarked in the mid-1980s (*California* magazine, October 1984, p. 77), and in the early 1990s he stated, "I never advised Pinochet" (CSPAN, November 16, 1991).[183]

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[181] Friedman and Friedman (1998, p. 403). Friedman also implied that the fact that the letters were both written on the day of his prize announcement suggested some kind of orchestration, but he did not go into detail regarding his speculation (p. 597).
[182] Friedman (1977k, p. 491).
[183] Similarly, in answer to a question posed by an audience member who was a Chilean refugee, Friedman had remarked during a speaking appearance in Glasgow: "I am not, and never have been, an adviser to the Pinochet regime. I spent six days in Chile, two [sic; three] years ago. I gave a series of public lectures. I met and talked with General Pinochet and a number of his advisers, just as I also met and talked with some of the political opponents of

The central point underlying Friedman's resistance to the "adviser" description was that he did not have, and had not had in 1975, a formal affiliation with the Chilean government. Related to this was the fact that, during his 1975 trip, including in his meetings with the junta, his activities did not involve remuneration from the regime—"I never got a penny from the Chilean government" was one way in which Friedman later expressed this aspect of the matter (CSPAN, November 16, 1991).[184]

These points made Friedman well positioned to reject implications that he was an official adviser. And he did so, as when he stated (*Reason* magazine, August 1977, p. 25): "I emphasize that I was not then and I never have been an official adviser in any way to the [Chilean] government." Friedman similarly rejected the label of formal adviser: Harberger had remarked earlier in the controversy (*Chicago Daily News*, October 4, 1975): "I can assure you that Milton is not serving as a formal adviser to the Chilean government," and two years later Friedman himself remarked more generally: "I have not been willing and I am not willing to adopt any position as a formal adviser to the Israeli government—or any other government." (*The Australian*, October 27, 1977.)[185] In this same period of October 1977, he observed: "I have always refused to be a formal economic adviser to the U.S. government. I have been an informal adviser to anyone who wants to listen to what I have to say."[186]

But Friedman could not deny that he had expressed views on appropriate economic policy to the Chilean government in the spring of 1975. And his remarks published in the *Chicago Maroon* in October 1975 stating that "I do not regard giving advice on economic policy as immoral" may have been mainly a general statement intended to encompass employed economic advisers to dictatorships, but it may also have been intended to cover his own trip, to Chile, and he himself

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the regime. At no time have I ever endorsed the Pinochet regime in Chile. You have my deepest sympathy for the tragedy that has overtaken your country." (*The Scotsman* (Edinburgh), April 22, 1978.)

[184] Friedman's focus on this point may have been partly motivated by the fact that Lawrence R. Klein had largely escaped criticism for providing economic analysis for the economic policymakers in Brazil's dictatorship that probably was remunerated by the government. Both Lawrence Klein and news coverage had distinguished the provision of his analysis from what Friedman had provided in Chile in 1975 partly on the ground that the economic analysis that Lawrence Klein had provided had been of a technical character (*Business Week*, January 12, 1976, p. 70). Friedman had stressed that he considered the analysis that he had supplied in Chile in 1975 was also technical, in focusing on how to bring down inflation.

[185] Similarly, he stated later in 1977: "I have never been an economic adviser to any country, including the United States." (*New York Times*, December 2, 1977.)

[186] Friedman (1977k, p. 491). In fact, there was a degree of formality in the 1970s in Friedman's manner of providing advice on the U.S. monetary policy side, as he was an occasional attendee of the Federal Reserve Board's outside consultants' meetings through 1978 and was a Federal Reserve Bank of San Francisco visiting scholar in 1977. The latter position will be discussed in the next chapter.

would later remark that, when he was in Chile in 1975, he was "giving them advice."[187] Likewise, earlier, on television during the peak of the controversy, he referred to "the advice I gave to the Chileans" as having been "technical advice in the sense of laying down an analysis of inflation" of the kind he had "given… in many places" (*Meet the Press*, NBC, October 24, 1976, p. 3 of transcript). In light of such acknowledgments, he could concur with characterizations to the effect that he had relayed advice, even though his provision of advice was not on an ongoing basis.

Friedman therefore did not wholly eschew the "adviser" description, insofar as it was applied to anyone who had been in the activity of giving advice. He rejected its typical application to himself in the context of Chile, however, because his critics often imputed to himself an insider status consisting of an ongoing, and considerable, role in within-government economic-policy discussions. But, as the Wald-Pauling letter to the *New York Times* showed, critic could help forestall Friedman's outright rejection of the adviser label by calling him an "unofficial adviser." The use of the formulation (or the similar one "informal adviser") made it hard for Friedman to claim literal inaccuracy in the label of himself as an adviser. Rather, he continued to stress that his interaction was contained to 1975 and also focused on the fact that he had not been paid by the Chilean government.

These points found expression in Friedman's later emphasis on indicating that he had not been a "consultant" to the Pinochet junta.[188] Harberger had earlier said of Friedman and himself in their 1975 trip (*Wall Street Journal*, December 10, 1976*a*): "we were not there as consultants to the government." Friedman likewise later stated, "I was only there six days, [and] I was not an official consultant or anything like that" (*The Observer* (London), February 17, 1980, p. 35). In early 1991, he observed, with regard to the question being put to him of the kind, "How do you justify having served as consultant to Gen. Pinochet when he was the dictator of Pinochet?," "The short answer [is], 'I never did.'" (*Wall Street Journal*, January 8, 1991.)

The Stockholm trip and the Nobel events

By late 1976, it had become routine for Friedman's scheduled public appearances to lead to him

[187] Friedman (1982d, p. 61).

[188] Friedman had sometimes been characterized as having been a consultant. For example, a *New York Times* report (February 20, 1976, p. 47) stated of Friedman and Harberger: "They have engaged in economic consulting activities in Chile and have encountered criticism for doing so."

facing Chile-related demonstrations: Al Harberger would recall that they were now occurring "every place he went practically, including when he received the Nobel Prize—there were demonstrations in Stockholm against him." (Arnold Harberger, interview, April 12, 2013.)

During their week in Stockholm in December 1976 for the Nobel festivities, the Friedmans had a 24-hour police guard, on the Swedish authorities' advice (*Newsweek*, January 12, 1981).[189] Although, after Friedman delivered his Nobel lecture late in his stay in Stockholm, the content of his lecture (on the subject of inflation and unemployment) did receive worldwide media coverage, many of the headlines in the days leading up to his lecture instead concerned the protests and questioning that he was facing on the matter of Chile.

"Milton Friedman is in Stockholm to accept his Nobel prize," a *Wall Street Journal* editorial supportive of Friedman began (December 10, 1976*b*). "He spent two hours at a press conference [on] Tuesday [December 7] not explaining his economic theories but defending himself against accusations of being somehow responsible for dictatorship in Chile."

The editorial was referring to the fact that, in a cramped conference room in the building of the Royal Swedish Academy of Sciences, Friedman had appeared before a hundred members of the media, many of them asking hostile questions. "You people have such a distorted idea of what's going on," Friedman remarked angrily as the questions piled up. He remarked in the session: "I do not believe in Chile more than I believe in dictatorship in Russia. I am for a free society." (*Boston Globe*, December 8, 1976.)[190]

Another comment that he made at the press conference—"My first ambition is to survive all this" (*Chicago Daily News*, December 8, 1976)—did not sound like a remark coming from someone who was enjoying himself—and so seemingly underlined the pall that the protests and media criticism were clearly casting over Friedman's participation in the Nobel events. But, in making this observation, he was also referring to the fact that he was now beholden to the frenetic and prearranged agenda that his Swedish hosts were putting on him and the other Nobel winners. Friedman was able, in the period following the week in Stockholm, to reflect on pleasant aspects of the formalities of the Nobel events in which he and Rose Friedman had

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[189] See also Friedman and Friedman (1998, p. 447), who indicated that police vehicles accompanied the car that took them from Stockholm's airport and that they had two bodyguards during their trip.
[190] Another report, possibly giving its own rendition of the first of these sentences, quoted him as saying: "I don't like Chile's dictatorial government any more than that of Russia." (*Chicago Daily News*, December 8, 1976.)

participated, as well as recounting the protest activities.[191]

Likewise, the images of Friedman that appeared in the media during the week in Stockholm brought out some lighter moments—including him sharing a laugh with the year's Nobel literature prize recipient. Saul Bellow. But the press and television coverage also produced a stream of pictures of Friedman looking angry or grim in response to protests or hostile media questions, as well as of himself and Rose Friedman being accompanied by security protection.

The centerpiece of the official festivities in which Friedman participated was the conferring, at a ceremony on December 10, of the Nobel awards to the 1976 recipients. When Sweden's King Carl XVI Gustaf was bestowing the medals and came to Friedman, an audience member—described in a later article, apparently quoting Friedman, as a "young man who had sneaked in on his father's ticket" (*Stanford Daily* (Stanford University), January 31, 1977, p. 1)—interrupted the event: this "single demonstrator in white tie and tails jumped up," a news report noted, and called out words of protest related to Friedman and Chile (*The Knoxville News-Sentinel* (Tennessee), December 11, 1976). As the ceremony resumed following the interruption (which had generated a glowering expression, captured by the cameras, on Friedman's face) and the protester's removal, a brief, off-the-cuff remark by the king, expressing relief at the limited nature of the actual disruption, suggested that the monarch may have been warned of the possibility of a larger-scale disruption of the ceremony.[192] There were an estimated 2000 demonstrators outside the building in which the awards were being conferred (*The Knoxville News-Sentinel* (Tennessee), December 11, 1976).

In an interview that appeared in print a couple of days after the ceremony, Friedman remarked of the widely reported criticisms of himself: "The Chilean smear is really disgraceful." He also insisted that the advocates, including economists serving in Chile's government, of monetary control and free-market reforms should not be tarred with the same brush as Chile's junta and that he would himself not be disavowing them: "There are old students of mine down there, and I'll be Goddamned if I'm going to turn my back on them." (*Sunday Times* (London), December

[191] See Instructional Dynamics Economics Cassette Tape 205 (December 1976, Part 2), Friedman (1977j), and Friedman and Friedman (1998, pp. 446–459). The fact that the protests inevitably featured prominently in these accounts was reflected in the Hoover Institution's summary when excerpting Friedman and Friedman (1998): "Milton and Rose D. Friedman recall what it was like when Milton received the Nobel Prize in 1976: The Nobel Committee was gracious enough, but the demonstrators in Stockholm were another matter." (*Hoover Digest*, Fall 1998, p. 7.)

[192] See the footage in *The Power of Choice*, PBS, January 29, 2007. This documentary also has excerpts of Friedman's December 7 press conference.

12, 1976.)

Friedman's criticism of the military dictatorship in and after 1975

A frequent allegation against Friedman in relation to Chile, and one to which he particularly objected, was that because many of its economic policies were ones that he broadly favored, he refrained from criticizing the junta's affront to democracy and violations of human rights. James H. Street, a professor of economics at Friedman's alma mater of Rutgers University, wrote in the *New York Times* (January 4, 1977), "He is only mildly critical of the present Chilean regime." This allegation persisted beyond Friedman's lifetime: for example, Filip (2020, p. 98) claimed that "the brutal regime of Augusto Pinochet was neither criticized nor condemned by Friedman," and Beckett (2015, p. 42) stated that "Friedman neither publicly supported nor condemned the repression in Chile," while, about a year after Friedman's death, television news show host Keith Olbermann spoke of "Friedman's friend, Chilean dictator Augusto Pinochet" (*Countdown*, MSNBC, November 29, 2007).

The pervasiveness of the allegation that Friedman endorsed the repressive nature of the political regime, or was silent about it, was a basis for Arnold Harberger's contention that "there are few economists who have been more unjustly criticized than he in connection with Chile." Harberger cited (as he previously had in December 1976, as noted above) in refutation of this allegation the fact that, while in Chile, Friedman gave "lectures outlining his principle, which he fully believed, that economic freedom *leads to* political freedom. And so he was not silent about his desire to see freedom restored in Chile, political freedom." (Arnold Harberger, interview, April 12, 2013.). Edwards and Montes (2020, p. 129) correspondingly observed that while Friedman's remarks in Chile included assessments "that the political and economic conditions were very negative under President Salvador Allende," it was also the case that "he publicly stated that political and economic freedom had to go hand in hand" and that they did not do so currently.[193]

Similarly, post-trip, Friedman made numerous statements that Chile was not free and had a repressive regime. These statements were often coupled with criticism of other dictatorships in the world, as in the Stockholm remarks above. They encompassed Pinochet's Chile

[193] The concluding two paragraphs of the Edwards and Montes (2020) paper give distinct, and mutually inconsistent, summaries of the content of the remarks that Friedman made in the course of his 1975 trip. Of these summaries, that in the penultimate paragraph, just quoted, is the one consistent with their preceding analysis.

nevertheless. Speaking in Australia shortly after leaving Chile, he stated that Chile was a country in which "a free government was destroyed by inflation" (*The Australian*, April 2, 1975) and stated that it had seen the "destruction of a democratic society by the reaction of a military in taking it over."[194] In a talk in May 1975 given in the United States, Friedman went through the Allende years and added that in the present day "they do not have a free society in Chile." (*Valley News* (Lebanon, New Hampshire), May 8, 1975.) In his *Newsweek* column six months later (November 17, 1975), he referred to the "destruction of personal freedom in Chile." On television the following month, Friedman stated (*The Open Mind*, PBS, December 7, 1975, p. 8 of transcript): "But if you take the road that we have been on, we are heading towards a destruction of our free society and towards a totalitarian society. We are unfortunately headed down the route which Chile has already taken." A few days later, in his December 1975 speech in Utah that was a version of the "Fragility of Freedom" speech that he had delivered in Chile, he referred to "Chile, which has lost its freedom and which today is governed by an authoritarian regime."[195]

The following year, in *Newsweek* (June 4, 1976*b*), Friedman referred to 'my sharp disagreement with the authoritarian political system in Chile." In the new *Business Week* article that appeared after the controversy intensified, he stated, "I did not then and do not now condone the regime in Chile." (*Business Week*, November 1, 1976, p. 75.) In a mid-1977 letter in the U.K. press that was fairly closely modeled on the letter that had been published by the *Chicago Maroon* and the *Wall Street Journal* in 1975, Friedman remarked: "Let me stress again, I do not approve or condone the regimes in Chile or Brazil or Argentina any more than I condone the regimes of Yugoslavia or Russia or the other Communist states. I would fervently wish their replacement by free democratic societies." (*Daily Telegraph* (London), June 13, 1977.) In a *Reason* interview that appeared in the same summer, Friedman stated: "I did not, in Chile or out of Chile, indicate any support of the political policies of the junta. It is a military government, it is an undesirable form of government." (*Reason* magazine, August 1977, p. 25.)

Similarly, in later years, Friedman remarked, "I deplore the loss of liberties in Chile" (*The Guardian* (London), March 1, 1980), and: "I object to the dictatorship in Chile. I expressed these sentiments in Chile when I gave a public speech on the fragility of freedom. I have never endorsed the political policies of the junta." (*The Spectator* (Hamilton, Ontario, Canada), July

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[194] Friedman (1975i, p. 18).

[195] Friedman (1976j, p. 562; 1976k, p. 31517). In Friedman (1976c, p. 8), this passage appeared with the words "lost its freedom" rather than "lost freedom."

15, 1980). In the early post-junta period, he remarked: "I have nothing good to say about the political regime that Pinochet imposed. It was a terrible political regime."[196]

Friedman's defenses of his 1975 trip

As has already been indicated, Friedman also did not regard support for Chile's economic policy as a blanket endorsement of the regime: "The economic policy, which I commend, can be separated from the political situation, which I deplore." (*New York Times*, May 22, 1977.)[197]

One of the points that Friedman stressed in making this distinction was that his interaction with policymakers and government officials of dictatorships did not begin with Chile. His pre-1975 travels had included visits to various countries in Europe, Asia, and the Middle East that were not democracies. These, however, had uniformly been less commented upon trips. In part, this was clearly because, in Chile, he met the head of government: in his other trips abroad in the period before 1975, it had been uncommon for Friedman to meet the heads of government or of state, whether of democracies or dictatorships. Of the visits to democracies, one exception had been his meeting Prime Minister Edward Heath during Friedman's visit to the United Kingdom in September 1970. But it was only in the early 1980s that meetings with heads of government became a more common feature of Friedman's travels to democracies abroad.[198]

Among the dictatorships that Friedman visited in the period before 1975 were the USSR in 1962 and Yugoslavia in 1962 and 1973.[199] He did not, however, meet these countries' heads of government on these trips.[200] The meeting with Pinochet in Chile in 1975 therefore did not have parallels with Friedman's pre-1975 trips to dictatorships.

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[196] Friedman (1991b, p. 2).

[197] Friedman made this remark in the course of a letter that he wrote in rebuttal to one of the pairs of Nobel laureates who had criticized his award. Although this follow-up correspondence was subsequently published in the *New York Times,* the Friedman portion of it was addressed to the Nobel laureates, so it was a letter but not (as it is cited by Kuehn, 2019, p. 32) a "letter to the editor."

[198] On his trip to Australia that immediately followed his trip to Chile, there had been arrangements made for Friedman to meet Prime Minister Gough Whitlam—whose office ultimately indicated that, on account of time constraints, a meeting was not possible. See Friedman and Friedman (1998, p. 430).

[199] On the Yugoslavia trips, see especially Friedman and Friedman (1998, pp. 292–294, 423).

[200] The absence of a meeting between Friedman and its dictator Marshal Josip Tito in his trips to Yugoslavia was an item stressed by Farrant (2020, p. 117) as weakening the parallel that Friedman sought to make between his trips to Yugoslavia and to Chile. In a separate criticism of Friedman's parallel with his trip to Yugoslavia, Farrant (2020, p. 117) goes on to imply that Friedman was being untruthful in his statements, which were numerous, that the content of what he said in Yugoslavia was like what he had said in Chile. Instead, without formally citing any Friedman remarks delivered in Yugoslavia, Farrant suggests that they "provided relatively general analyses" pertaining to international monetary matters. Farrant's imputation of untruthfulness on Friedman's part does not seem well

Friedman had, nevertheless, engaged with senior government officials in some of his pre-1975 travels to undemocratic countries: for example, a study that he worked on in 1970 while briefly visiting South Korea (a country that, during parts of the decade of the 1970s, was nominally democratic—but was ruled by the military throughout) contained in its acknowledgments the statement: "I am indebted to Governor Jin Soo Suh of the Bank of Korea."[201] As far as the Yugoslavia visits was concerned, he noted of the most recent (that in early 1973), "in that case, [I] was sponsored by the central bank of Yugoslavia, which is a government agency."[202]

The numerous occasions on which Friedman made references to Communist countries in his discussions regarding Chile were part of his conveying his disapproval of dictatorships of any form. With respect to the controversy over his visit to Chile, the references were also used to highlight the fact that he believed the reaction to his travels to have been asymmetric: the trips to Communist countries had not generated a comparable uproar, and this dichotomy, he suggested, implied a "double standard" on the part of his critics (*Chicago Maroon*, October 3, 1975*a*). Friedman underlined this argument at his December 1976 press conference in Stockholm: "I am being attacked for spending six days in Chile in March last year. But there is a double standard here. When I went to Yugoslavia, I was not attacked for that." (*Boston Globe*, December 8, 1976.)

Further using the Yugoslavia example, Friedman pointed to the fact that he had discussed economic matters with the governments of that and other countries had not led him to receive the label that he had received in much commentary on Chile: "When I go to Yugoslavia, no one describes me as an adviser to the Communist Yugoslav government." (*The Spectator* (Hamilton, Ontario, Canada), July 15, 1980.) Friedman also emphasized the consistency of the economic analysis and prescriptions across forums—"I always say the same thing in public as I say in

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founded. As it happens, the published lecture given in Yugoslavia did *not* confine itself to general analysis: as indicated earlier in this chapter, Friedman (1973d) was specific on a matter concerning domestic monetary analysis by insisting that the quantity theory of money still held in an open economy. But more crucially, it was a hallmark of the guest lectures and talks that Friedman gave around the United States and in various other parts of the world during the 1970s that he would deliver remarks on both monetary analysis and market economics—so the printed record of the former does not preclude the possible existence of the latter in the case of the Friedman Yugoslavia visits. George Macesich, Friedman's host for much of his time in Yugoslavia, affirmed that, at the Yugoslavia speaking events, Friedman's coverage encompassed both "their [Yugoslavia's] particular economic system and the role of money" and that, on an occasion when Friedman was due to speak at a university in Yugoslavia, an opponent of free-market economics locked the seminar rooms in order to try to prevent the Friedman presentation (George Macesich, interview, May 28, 2013).

[201] Friedman (1971d, p. 846).

[202] Friedman (1977k, pp. 491–492).

private" (*Stanford Daily* (Stanford University), January 31, 1977, p. 1) and across countries—"I gave a series of public lectures in Chile of exactly the same kind that I have given in Yugoslavia" (*California* magazine, October 1984, p. 77).

Friedman believed that disapproval of dictatorships should not be a bar on visiting them: "I do not regard visiting any of them as an endorsement. I do not regard learning from their experience as immoral." (*Chicago Maroon*, October 3, 1975*a*; also in *Daily Telegraph* (London), June 13, 1977.) With regard to the Communist countries, it had, indeed, been official U.S. government policy even in the explicitly adversarial conditions of the late 1950s—well before the détente era discussed later in this chapter—that interaction with those countries, such as by U.S. academics who communicated their thinking on matters to counterparts and officials in the East bloc, should not be precluded and was positively desirable. Foy Kohler, a senior U.S. diplomatic official in the 1950s and 1960s, recalled the U.S. negotiations in 1958 to encourage "cultural, humanities, and informational programs which would have the effect of creating at least a small opening for American influence on the closed Soviet society."[203]

It had also been common by the mid-1970s to have standing official relationships on economic matters between the United States and other democracies with dictatorships. Examples of this in the cases of the OECD and the IMF were given earlier in this chapter. But there were also numerous examples of academic economists and economic-research institutions in the West having relations with members of the governments that were dictatorships. One example—again related to the non-Soviet bloc but Communist country of Yugoslavia—was the fact that Dimitrje Dimitrijevic of the Yugoslav National Bank was a founder member of the Editorial Board of the *Journal of Monetary Economics*, as was indicated in the board membership list on the cover page of January 1975's premiere issue of that journal.

With regard to the Soviet bloc proper, trips by academic economists to bloc countries, either for conferences sanctioned by the ruling Communist government or in visits to official policymaking bodies, had indeed become common by the 1970s. The academic economists who made these trips would likely overwhelmingly have denied, as Friedman did regarding his own trips to dictatorships, that their visits or interactions signified an endorsement of the regime or its repressive practices.[204]

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[203] Kohler (1979, p. 29).
[204] For example, Zvi Griliches, of Harvard University's economics department, observed that he had given "a number of lectures in Moscow at the invitation of the USSR Academy of Sciences" and that he would have taken

Many of the visits that academic economists made to the East bloc in order to discuss economic theory and policy were to the USSR. But there were also many visits to the Soviet satellites.

A notable example was Hungry. This was a country that Friedman never himself visited before the liberation of Eastern Europe but was one to which he had ancestral ties. During the 1970s and 1980s, Hungary played host to multiple gatherings attended by Western economists. For example, as recorded in Goodhart (1975, pp. 90, 97), the Econometric Society had a meeting in Budapest in the later part of 1972. A couple of years later, the prime minister of Hungary opened the Fourth World Congress of the International Economic Association, which was held from August 19 to 24, 1974, in Budapest (see Machlup, 1976). Still further along, in developing their critique (see Chapter 13 below) of Friedman and Schwartz's *Monetary Trends*, David Hendry of Oxford University and Neil Ericsson of the Federal Reserve Board prepared a paper for presentation at the European Meetings of the Econometric Society, to be held in Budapest in September 1986 (Hendry and Ericsson, 1986).

Friedman, whose trips to the Soviet bloc were sparing, did not attend any of these particular events. But he was very supportive of the notion that such events should proceed, as Friedman, too, believed that the trans-system dialogue should be welcomed and that academics of both blocs should participate. Indeed, he himself was involved on two occasions—during the 1960s and in September 1970—in a regular "meeting between economists of the Eastern European countries and of the Western countries," the 1970 meeting being held in Venice. Friedman noted: "The Eastern European countries represented included Hungary, Romania, Czechoslovakia, Poland, East Germany, Yugoslavia…"[205]

During 1975−1977, however, Friedman felt that his critics were not seeing his 1975 visit to Chile in the same spirit of continuing communications channels open. "I visited Chile, whose government I do not approve. How else can we get ideas to them?," he remarked in early 1977 (*Los Angeles Herald-Examiner*, February 11, 1977). He indicated that he regarded maintaining a dialogue as appropriate to "break down barriers between countries" and contrasted this approach favorably with an alternative posture of "intellectual isolation" (*New York Times*, May 22, 1977). He pressed this point in his lengthy letter in the London *Daily Telegraph* of June 13, 1977: "how are we to promote an interchange of ideas? How are we to penetrate the iron curtains that

exception if this had led to others labeling him "a supporter of the Brezhnev regime." (*Harvard Crimson* (Harvard University), November 1, 1976.)

[205] Instructional Dynamics Economics Cassette Tape 58 (October 4, 1970).

separate so many countries? How are we to bring hope and intellectual and moral stimulus to the brave people who are seeking to restore freedom in their own countries?"

A further dimension of Friedman's view of the matter of visiting dictatorships and of establishing an interchange of ideas was that he did not regard it as inappropriate to provide economic advice regarding those countries' economic policy if the prescribed economic measures were intended to improve the well-being of the populace. As the Communist countries were tied to an economic system that he believed locked them into low living standards, the scope for them to accept advice from Western economists (at least those who were not subscribers to Marxist economics) was limited.[206] Even so, Friedman welcomed the possibility that East bloc economists could be exposed to "economists who have a great deal of interest in the price system and free markets—in the capitalist system." Meeting "those who are favorable to a market economy" might ultimately encourage economic liberalization in those countries, he believed (Instructional Dynamics Economics Cassette Tape 58, October 4, 1970).

Friedman therefore shared the widespread view that academic contacts with countries that had oppressive governments could help the process of political liberalization in those countries, as well as the fairly widely held view that improving the living standards of undemocratic countries was a desirable objective. But he also subscribed to what he called a "special case" of the latter position (*Chicago Maroon*, October 3, 1975*a*) in believing that, over time, economic liberalization would be a democratizing force while simultaneously improving living standards.

This position, which underlay his statement that "economic improvement would contribute both to the well-being of the ordinary people and to the chance of movement toward a politically free society" (*Daily Telegraph* (London), June 13, 1977), will be examined further shortly.

[206] With respect to the issue of whether it was *desirable* to encourage economic advance in the Soviet bloc, the Western world's governments did not have a uniform and consistent view over time. It would probably be accurate to suggest that a concerted attempt to use either trade sanctions or restrictions on access to Western funds was not really attempted by the United States over most of the 1950s and 1960s, that its Western European allies were generally disinclined to take such measures, and that there also existed a sizable body of thought across the West (both within and outside Western governments) that held that improvements in Soviet households' living standards might help ease East-West tensions by inclining the USSR to become less expansionist and less fervent about world revolution. Friedman's own position on these issues was more consistent than official U.S. government policy, in the sense that he was not enthusiastic about trade sanctions and, when, in 1972, official U.S. policy became one of overtly encouraging trade with the USSR, he supported such trade but not on non-commercial terms. See the discussion below titled "Trade Credits and the Soviet Union" for a detailed analysis.

Friedman's objection to the focus on himself

The lengthy defenses—such as the mid-1977 example just quoted—that Friedman felt the need to produce bore testimony to the extent to which media coverage was linking himself and current Chilean economic policy. The impression of a major, continuous personal contribution by himself that emerged from many accounts was a source of continual frustration to Friedman. He would complain from 1976 onward that his critics were erroneously ascribing to him an ongoing and major involvement in Chilean economic policy: "people are saying that from my office in Chicago… I am guiding the Chilean economy," he remarked during his *Meet the Press* appearance (October 24, 1976, NBC, p. 3 of transcript).[207] Friedman made similar comments on many other occasions in subsequent years—often including in these remarks the sarcastic remark that he presumably conducted such guidance by extrasensory perception, as he was not in phone contact with Chilean government officials.[208] Shortly after his relocation to California, he declared emphatically that he was not going to accept the premise of what was being said about him. "I'm not going to take any responsibility for policies of the junta." (*Stanford Daily* (Stanford University), January 31, 1977, p. 1.)

Another major feature of Friedman's reaction—though one that he spoke about publicly to a much lesser degree at the time—was his unhappiness at the fact that the perception of a general University of Chicago/Chile link was centered on him—when other department members (most notably, Harberger) had actually been far more immersed in the study of Chile and in various discussions over the years of Chilean economic policy than he had been.[209] This frustration found expression in Friedman's 1998 memoir account, in which he indicated that his own celebrity had led to the focus of protests being put on himself, rather than on Harberger. Harberger did not disagree with this assessment, although he added that "I had my share of demonstrations" to face (Arnold Harberger, interview, April 12, 2013).

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[207] Earlier, he had remarked at one of the protest-interrupted organizations: "I am not an expert on Chile. I do not get daily reports from anyone," adding sarcastically, "except the Spartacus Youth League" (the group that was regularly protesting against him in the University of Chicago area) (*Daily Northwestern* (Northwestern University, Illinois), March 1, 1976).

[208] See, for example, Friedman (1977j, p. 7), *Reason* magazine (August 1977, p. 26), and *The Times* (London), August 15, 1978, as well as in his in retrospectives on the 1970s in *Jerusalem Post*, November 6, 1987, and *Forbes*, August 17, 1992 (p. 42).

[209] Friedman and Friedman (1998, p. 403). This was a widespread assessment, and Rachel McCulloch observing (interview, October 3, 2013) with regard to Chile: "He [Harberger] was very involved. Whereas Friedman was marginally involved, I would say." See also McCloskey (2003).

Harberger, as indicated above, defended Friedman in a number of interviews and writings that appeared in the U.S. press in 1975 and 1976. As also noted previously, he did so in part by making the point that Friedman was not involved in running the University of Chicago side of the exchange program that had led to there being many Chilean economics graduates.

Another friend of Friedman's who defended him in the press was the nationally syndicated conservative columnist William F. Buckley, Jr., with whom Friedman had been on warm terms since the 1960s. In Buckley—whose defense of Friedman appeared in a column in December 1977, by which time Chile-related anti-Friedman demonstrations had become well established as a routine occurrence—Friedman had a high-profile supporter, but not one who was likely to change the views of many of Friedman's critics.[210]

Friedman and Buckley were both part of generally Republican-party favoring circles, but, as Friedman once noted, Buckley in the 1950s had come to prominence "in a different corner" of those circles from his own libertarian and free-market corner (*Reason* magazine, June 1995, p. 37). Buckley had, particularly in the 1950s and 1960s, taken a hardline conservative-Republican and strongly anti-Communist viewpoint, rather than an outlook founded on adherence to economic liberalism and older traditions of political liberalism. In domestic policy, Buckley had over the years defended illiberal arrangements imposed, nationally or within U.S. states, by the majority on the rest of the population: notably, he had coauthored a book praising Senator Joseph McCarthy late in the era of McCarthyism (see Buckley and Bozell, 1954) and had also opposed many of the civil-rights steps taken by the three branches of the federal government from 1954 to 1964. In foreign policy, Buckley had been an advocate of a U.S.-led toppling of Fidel Castro in Cuba and had opposed Nixon's bridge-building with China (*Record American* (Boston), February 25, 1972, p. 2).

Although Buckley would subsequently repudiate many of these positions, the fact that he had once held them did not situate him well to defend Friedman on liberty-related issues and on Chile. Consequently, Buckley's suggestion that it was "enraging to think that someone as tenderly concerned about human rights as Milton Friedman" should be characterized as favoring political oppression (*The Jersey Journal* (Jersey City, New Jersey), December 6, 1977)—while a suggestion that had merit in itself—was likely undercut by Buckley's own record of having taken what even he, in retrospect, recognized as illiberal and narrow-minded perspectives on numerous

[210] In a previous column, Buckley had quoted Friedman's *Chicago Maroon*/*Wall Street Journal* published letter regarding his Chile trip (*Milwaukee Sentinel* (Wisconsin), December 2, 1975).

key national and global issues.

Friedman's views on inflation and the controversy

In the mid-1980s, an interviewer suggested to Friedman: "There was a great deal of criticism, principally by Anthony Lewis in the *New York Times*, when you went down there [to Chile]. He asked whether you had any compunction about trying to prop up such a bloody regime." (*California* magazine, October 1984, p. 78.)

Although Lewis was certainly a key voice finding fault with Friedman with regard to Chile, his criticism was not, in fact, one that maintained that the policies that Friedman recommended would prop up the regime. The criticism that being part of the process of facilitating economic reforms would, in the case of a dictatorship, strengthen a country's economy and thereby bolster the government' standing—in turn possibly delaying the achievement of democracy—has certainly been advanced on the matter of having dialogues with dictatorial regimes, and so it figures into the ethical considerations concerning such dialogues.[211] But it did not correspond to Lewis' position. What Lewis advanced was, instead, a different criticism. Lewis' discussion (*New York Times*, October 2, 1975) was strongly predicated on the notion that Chile's, and any, economy would be permanently worsened by the kinds of policies that were associated with Friedman's name.

More specifically: Although Lewis' coverage of economic policy measures and of Friedman in his column were brief, they relayed a view that (*i*) that Friedman was idiosyncratic among economists in believing that the measures that he recommended against inflation would work, (*ii*) that they would not work and would, instead, produce permanent economic slump alongside continued high inflation, and (*iii*) that these measures of the kind that Friedman recommended, with respect both to demand restriction and to increasing the role of market forces, would not be acceptable in a democracy and no were inextricably tied to politically repressive regimes like Chile's. Each of these three items will be considered in turn.

With regard to Friedman's analysis of inflation, he had made major strides in getting acceptance of it in the U.S. economics profession by the mid-1970s. His focus on a monetary diagnosis and cure for inflation was, therefore, no longer idiosyncratic. The distinction between Friedman and

[211] For example, it is in this context that Ruger (2011, p. 57) discusses Friedman going to Chile in 1975.

the U.S. economics mainstream on the analysis of inflation had narrowed very considerably and would continue to do so over the late 1970s. "Twenty years ago, when I was at Yale, Milton Friedman was considered a crackpot," Richard Rosett, dean of the University of Chicago's business school, noted after Friedman won the Nobel award (*Business Week*, November 1, 1976, p. 75). During the 1950s, Friedman's then-widely-shunned viewpoint on monetary policy's importance had been a key factor in producing this perception. "He once was a small voice in the profession," noted a report on Friedman's activities on the day of the announcement of his Nobel award. The article quoted Friedman saying in a speaking appearance in Flint, Michigan, that day that "there has been a very substantial change in monetary thought in the last 20 years" and that "some of the more extreme views in Keynesian analysis are dead." In contrast, Friedman noted, "Many ideas I have fostered are being used." (*The Flint Journal* (Michigan), October 15, 1976.)

On the matter of inflation being a monetary phenomenon, this process of acceptance continued beyond 1976 and became consolidated, and, very late in his life, Friedman replied, "I think that is a correct statement," when presented with a quotation from Bernanke (2004a, p. 5) stating that Friedman's views had become embedded in modern monetary analysis (*The Charlie Rose Show*, PBS, December 26, 2005, p. 21 of transcript).

With regard to achieving disinflation, Friedman insisted that, as an analytical and empirical proposition, "It's not what I advocate that matters—there is only one way to do it." (*St. Louis Globe-Democrat*, December 16, 1977.) The original revival of monetary policy in the 1950s, Friedman observed in testimony in 1959, had reflected a recognition that monetary policy needed to be deployed: "*No country succeeded in stemming inflation without adopting measures that made it possible to restrain the growth in the stock of money*."[212] The ranking of monetary policy and fiscal policy as aggregate demand tools had also changed by the 1970s. Hart and Brown (1951, p. 98) had stated—reflecting a view to which Friedman himself had once himself previously subscribed: "The personal income tax is one of the strongest weapons in the anti-inflation arsenal." A quarter century later, partly reflecting the influence of Friedman's research as well as experiences like the 1968 tax increase, this view had much less acceptance in the U.S. economics profession.

After the monetary policy/fiscal policy debate had receded, the monetary view of inflation did

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[212] Testimony of May 25, 1959, in Joint Economic Committee, U.S. Congress (1959a, p. 607).

not immediately prevail in the United States. Instead, in the country's economic debates over the 1970s, it still had to contend, particularly in policy circles, with cost-push views. But Friedman's view that inflation was a monetary phenomenon would predominate in the United States by the end of the decade.

In higher-inflation countries such as many in Latin America, it had, likewise, become evident that the actions that were effective against inflation involved measures that were designed to restrict aggregate demand and that entailed monetary restraint. Mikesell (1967, p. 3) observed: "The economic indicator which is most closely correlated with the rate of inflation in Latin America is the rate of increase in the money supply… This applies particularly to countries with high rates of inflation such as Argentina, Brazil, Chile, and Uruguay." Correspondingly, with regard to how to proceed with disinflation, when the University of Chicago's Tom Davis referred in Congressional testimony in May 1962 to "the anti-inflationary programs that we have encouraged Latin American governments to espouse," he was referring to stabilization packages—often associated with and recommended by the IMF—that involved demand restriction and that proposed to reduce monetary expansion substantially.[213]

The monetary nature of inflation across economies underlay Friedman's statement (*Daily Telegraph* (London), June 13, 1977): "The diagnosis is the same everywhere and so is the treatment." Of course, with regard to Chile, Friedman's recommended treatment, while entailing monetary restriction, did differ from that he recommended for the United States, because it involved massive initial steps down in nominal aggregate demand growth, as distinct from the gradualist approach that he recommended in instances of double-digit inflation rates. This drastic approach to disinflation was what he and others called "shock treatment"—a term that was not, as *Business Week* (January 12, 1976, p. 71) contended, originated by Friedman but instead was already prevalent in discussions of prior actual, or contemplated, Latin American economic-stabilization packages (see, for example, Krieger, 1974, p. 45).

In later years, this drastic approach to disinflation became known instead as "cold turkey," and it was analyzed in a textbook context by Dornbusch and Fischer (1987, pp. 525−527). Dornbusch and Fischer noted several analytical reasons why cold-turkey policies, rather than gradualism, might be appropriate in response to triple-digit (or still higher) inflation rates. This reasoning, which had featured in Friedman's discussions in the 1970s—as well as others' analyses that had

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[213] Testimony of May 10, 1962, in Joint Economic Committee, U.S. Congress (1962, p. 28).

appeared in the 1960s of the various stabilization packages in Latin America and elsewhere—had included the consideration that the experience of very high inflation had lowered both the length of nominal price contracts and the size of holdings of money balances and so made a more rapid restriction of aggregate demand less costly in terms of short-term loss of output and employment than it would be in an economy less accustomed to inflation. An additional, but related, consideration in favor of a large initial restriction of aggregate demand was what Dornbusch and Fischer (1987, p. 526) called a "credibility bonus." This factor—also embedded in Friedman's reasoning but highlighted especially in Sargent's (1982) analysis of several very large-scale pre-1950 disinflations and economic-stabilization packages—involved the possibility that, in extreme-inflation conditions, a very major change in policy stance toward restriction could lower inflation expectations quite quickly but a gradualist policy—the public's confidence in which might be undermined by a belief that a policy *U*-turn would occur in a future year—might not.[214]

Friedman's recommendations concerning Chile also differed from those he applied to the United States in the sense that he believed that Chile needed to engage in simultaneous major monetary and fiscal restrictions. Even in the case of the United States, Friedman believed that it was desirable that growth in federal government spending be restricted alongside monetary growth as part of a disinflation program. But he believed that monetary restraint would be able to generate U.S. disinflation even if the federal government had an unchanged fiscal policy stance.[215] With regard to Chile, however, he believed that this separation of monetary policy and fiscal policy was not possible. He maintained, as elsewhere, that the "one thing that's essential" to get inflation down in Chile was a reduction in monetary growth (*Stanford Daily* (Stanford University), January 31, 1977). But Friedman stressed—both in his public discussions and in his letter to Pinochet in April 1975—that money creation was closely linked to fiscal policy in Chile because the budget deficit was so large and was being monetized.[216] Accordingly, he believed that disinflation needed to include sharp fiscal restriction—in Friedman's recommendation,

[214] Sargent (1982) also strongly underlined the point that the hyperinflations that he studied had typically been cured by drastic simultaneous monetary and fiscal restriction. In so doing, he provided examples that contradicted the later presumption underlying *Business Week* (January 12, 1976, p. 70) and other commentaries that such policies were new and that the feasibility of their implementation was founded on their being a repressive political regime in force.
[215] For discussion of his views in this connection, see, in particular, Nelson (2020a, Chapters 7 and 10) and Chapters 6, 10, and 11 of the present book.
[216] See also Caputo and Saravia (2021, p. 214) on the relevant Chilean fiscal and monetary data for this period.

major government spending cuts.[217]

The view that a major monetary and fiscal restriction was needed to get inflation down in very high-inflation condition was more widely accepted among economists than the controversy over Friedman and Chile's economic policy might have suggested. For example, Tom Davis, Friedman's one-time colleague and now at Cornell University, was a critic of the 1975 Chile economic-policy package but indicated that he did favor budget deficit reduction, his proposed means of doing so being tax increases rather than spending cuts (*Business Week*, January 12, 1976, p. 71).[218] And Dornbusch and Fischer (1987, p. 530), in noting what experience showed was the best way "to reduce the inflation rate from several hundred percent per year to low double-digit per annum figures," suggested that, in contrast to traditional cold-turkey policies, it was worthwhile to include "wage and price controls as part of a complete package," as this feature could expedite the adjustment of inflation.[219] But they themselves also stressed the necessary condition for disinflation that "*the accompanying aggregate demand measures are taken*" (Dornbusch and Fischer, 1987, p. 530, emphasis in original).[220]

All told, with regard to what has been designated above part (*i*) of Lewis' 1975 critique, it is appropriate to conclude that Friedman's position that inflation was a monetary phenomenon and that disinflation required major monetary restriction was *not* a perspective special to him but had become, certainly over the later 1970s and beyond, a mainstream one.

Lewis' critique extended to the real implications of monetary and fiscal policy restriction. As indicated in item (*ii*) in the list above, Lewis' discussion implied that this restriction would create permanently high unemployment, along with a permanently deep negative output gap.[221]

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[217] For example, in his letter to Pinochet of April 21, 1975, he wrote (Friedman and Friedman, 1998, p. 592): "A cut in the fiscal deficit is the indispensable prerequisite for ending inflation." For examples of his corresponding public remarks, see Friedman (1977i, p. 46) (also in Friedman [1978b, p. 71; 1991a, p. 152]).
[218] The study of postwar experience of several advanced economies by Alesina, Favero, and Giavazzi (2019) found that, empirically, fiscal-consolidation measures that are centered on reducing government outlays involve fewer real output costs than those that rely heavily on tax increases.
[219] Friedman himself recognized (in discussing lower-inflation situations) the analytical merit of controls in these circumstances but doubted their having the intended beneficial effects. See Nelson (2020b, Chapter 15).
[220] Similarly, in discussing the 1980s "heterodox" policies of disinflation that Dornbusch and Fischer (1987) were considering, Diaz and Tercero (1988, p. 385) remarked that: "as the proponents of these policies have repeatedly emphasized, except in rather extreme cases the heterodox part of the program cannot substitute for the 'fundamental' adjustments (the short-run and structural reduction of the deficit)."
[221] He correctly implied that Friedman took the opposite view— but incorrectly implied that Friedman believed that an output recovery would occur through the channel of shifting income to businesses (*New York Times*, October 2, 1975).

Although Lewis was applying this argument to the case of Chile, it was one that would be applied to smaller-scale applications of monetary restriction to reduce inflation. For example, Nigel Lawson would recall that the restriction of aggregate demand in the first term of the Thatcher Government (in whose economic team Lawson had served) had led not only to recession but also to critiques that "implied that the recession would go on forever."[222]

When discussing the process of reducing inflation in any country, including Chile, Friedman consistently indicated that disinflations would involve a short-term decline in real output.[223] But he also believed that, once inflation was brought down significantly, noninflationary recovery (including a period when the output gap was closing while inflation continued to fall) and, ultimately, restoration of full employment could both occur. Consistent with the wide acceptance of the natural rate hypothesis, this view also became part of the U.S. economic mainstream. Bosworth, Dornbusch, and Labán (1994, p. 1) stated that, from 1976 in Chile, "recession was followed by several years of strong recovery" but that this development generated undue claims of success, as it was followed by the economic crash that started taking place in 1981−1982. Their conclusion was correct insofar as the pre-1982 economic expansion was misperceived as signifying a definite upturn in long-term productivity growth. But, even if it did not reflect a higher trend rate of potential output growth, the actual output growth seen in the pre-1982 period was consistent with a process of post-disinflation recovery of the kind predicted by the natural rate hypothesis. And as both Dornbusch's and Friedman's retrospectives concurred, the post-1981 economic crash largely reflected the repercussions of a 1979 policy change in Chile, when the government fixed the exchange rate and so imposed an additional and severe monetary restriction in the early 1980s.[224]

It is therefore apparent that what was called above Lewis' proposition (*ii*), which was a claim to the effect that demand restriction necessarily imposes permanently high unemployment, was one that ran against mainstream U.S. economic opinion by the late 1970s and that continued to do so in later years.

Item (*iii*) of the propositions embedded in Lewis' column—the contention that it was only in the presence of political oppression that the monetary-restriction and free-market policies that

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[222] Lawson (1991, p. 630). Lawson was, of course, using the term "recession" loosely to mean a negative output gap.

[223] He expressed this judgment specifically with regard to Chile on a number of occasions, including in his April 1975 letter to Pinochet (see Friedman and Friedman, 1998, p. 592).

[224] See Chapter 13 below.

Friedman recommended could proceed—was echoed in his newspaper about six months after the column. A news report (*New York Times*, March 21, 1976) on Chile's economic policy (both its demand-restriction and free-market aspects) took this proposition so much for granted that it treated it as a factual, rather than a disputed and contentious, point that with regard to "Mr. Friedman's theories… their application requires the continuation of a repressive political system." But it was a proposition that Friedman vigorously disputed.

In arguing against this proposition, Friedman stressed that, with respect to the issue of monetary restriction, countries, whether democracies or dictatorships, had invariably found over history that monetary restraint was the only effective means of getting inflation down.[225] On the issue of economic liberalization, too, he dissented strongly from what he would call "the myth that only an authoritarian regime can successfully implement a free-market policy" (*Newsweek*, January 25, 1982). In terms of recent examples, Friedman, in 1977, pointed out that, in the democratic world, some advanced economies like the United Kingdom and Italy were reducing the role of the public sector in the economy.[226] In providing a longer-term perspective on the same topic, he had earlier cited the United Kingdom in the nineteenth century as a democracy that had moved from a heavily state-controlled economy to one characterized by considerable economic liberalization (Instructional Dynamics Economics Cassette Tape 25, May 25, 1969).[227]

Friedman's position on Chile and the economic freedom/democracy link

As well as disputing the link between repression and his policy recommendations that his critics postulated, Friedman went further in arguing that the economic policies that he advocated would help pave the way for the restoration of democracy in Chile.

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[225] See, for example, his discussion in *New York Times*, May 22, 1977, as well as the discussion above.
[226] Friedman (1977k, p. 493).
[227] Peck (2011, p. xliv) stated that, in a 2000 interview, Friedman "sputtered, 'No, no, no, not at all. After all, Great Britain put Chicago theory in[to] practice in the nineteenth century […]'" (*Commanding Heights*, PBS website, 2000, p. 15; also in Ebenstein, 2012, p. 249) when it was put to him that Chile in the Pinochet era had been the first country to do this. The *Oxford English Dictionary* online (oed.com) defines "sputter" as "to speak or talk hastily and confusedly or disjointedly" and the similar word "splutter" as "to utter hastily or indistinctly" or "to talk or speak hastily and confusedly." Other than being a rapid-fire response, Friedman's 2000 answer did not fit these descriptions. "No, no, no, not at all" was standard Friedman speech. And the substance of Friedman's answer in 2000 concerning the nineteenth-century United Kingdom and the United States as examples of market liberalization was a repeat of points that he had made well before the junta came to power in Chile. In the aforementioned Instructional Dynamics Economics Cassette Tape 25 (May 25, 1969), he had cited the United Kingdom in the nineteenth century as providing "the most dramatic example in history" of a reduction in the degree of state intervention in the economy. (See also the discussion in the previous chapter.) Consequently, it seems inappropriate to judge that Friedman's answer in the documentary interview was the hasty, confused response that would justify characterizing it as sputtering.

In making this case, Friedman made numerous interventions, particularly in 1975 and 1976, on the link between economic freedom and democracy, including specifically in the context of Chile. As already indicated, he talked little publicly about the Allende's government during its 1970–1973 tenure. But he did refer on numerous occasions after the government's fall. In these discussions, Friedman characterized the situation in Chile in 1973 as poised to lead to either Communist dictatorship or authoritarian dictatorship. To a limited degree, his accounts referred to political steps that he thought Allende had taken that were in the direction of stifling political dissent: "Allende had, in large part, destroyed democracy," he remarked in one early commentary (Instructional Dynamics Economics Cassette Tape 166, March 1975). Suggestions of this kind were prevalent among critics of Allende within the U.S. government and in the U.S. and U.K. press.[228] But Friedman himself did not press this point strongly and, as indicated above, he acknowledged that the move to a dictatorial regime that did not tolerate dissent resulted from the Pinochet coup and did not occur under Allende.

Friedman instead concentrated on the economic conditions prevailing in Chile by September 1973 and suggested that these were liable to generate a situation of revolution and dictatorship. In so doing, he made a twofold economic critique of the Allende policies. First and more familiar was the very high rate of inflation: "If we really do have unrestrained inflation, we shall not end up with a democratic system," he observed, articulating what he believed was a general principle that applied across countries, including the United States (*Reason* magazine, June 1975, p. 93).[229]

The suggestion that super-high inflation rates could generate unrest that ended democracy was not special to Friedman. As detailed in the first part of this chapter, discussions of the political turmoil associated with inflation were prominent in the world media in the mid-1970s, and the scenarios considered included, as an extreme case, the possibility of coups.[230] One example of

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[228] For example, internal U.S. government memoranda during the Allende years referred to alleged instances in which the Chilean government was limiting the scope for political dissent (see McElveen and Siekmeier, 2014, pp. 543, 549). As far as restrictions on the press were concerned, the judgment of Senate staff member Gregory F. Treverton suggested some cases of pressure but concluded: "Freedom of the press continued in Chile until the military coup in 1973." (Testimony of December 4, 1975, in Select Committee To Study Governmental Operations With Respect to Intelligence Activities, U.S. Senate, 1976, p. 16.)

[229] Of course, he made many other comments to this effect: see Nelson (2009b, pp. 492–493; 2020a, Chapter 7).

[230] It had, of course, already featured heavily in discussions of Latin America even in the 1950s and 1960s, owing to the severe inflation experiences seen in the region over those decades. For example, Mikesell (1967, p. 29) observed: "It is not simply a coincidence that severe political crises, often resulting in the overthrow of governments, have occurred in periods of extreme price instability, although I am not suggesting that inflation provides the principal explanation for Latin American revolutions or dictatorships."

such analyses was a piece in *Time* magazine (April 8, 1974, p. 72) which stated: "The ultimate threat is that inflation will eventually weaken confidence in democratic governments and prepare the way for sharp, violent shifts to the radical right or left."

Second, Friedman believed that the expansion of the public sector's role under Allende had been proceeding in a manner that meant that the likely next step would have not been consented to by the citizens and that a Communist dictatorship would have been imposed to continue the expansion of the government's role. Consequently, he referred to "the Communist regime Allende was seeking" (*Chicago Maroon*, October 3, 1975*a*) and the "Communist totalitarianism that was in store for Chile" (*Daily Telegraph* (London), June 13, 1977). Similarly, in December 1975, Friedman suggested that the Allende government was "clearly seeking to turn Chile into a Communist dictatorship" and that Chile reached "the tipping point at which the willingness of the public to put up with increasing involvement in their own lives was exceeded." He considered that the situation was one in which Chile was poised to have either a "left-wing dictatorship" or "a counterrevolution with the military taking over." [231] It was, of course, the latter that occurred, with the Pinochet coup ending democracy in Chile.

In Friedman's controversial interpretation, therefore, the Allende Government's moves to give the public sector a large role had helped produce conditions, including an attempt at preempting private-sector activity on such a scale that it could be enforced only through coercion, that some loss of democracy was the next step, either in the form of a government crackdown or the forcible overthrow of the government.[232] "Chile had a choice of Communism under Allende or a military junta," he suggested (*Los Angeles Herald-Examiner*, February 11, 1977).[233] "There's no

[231] Friedman (1976j, p. 563; 1976k, p. 31517). Some form of the notion that a revolutionary situation that threatened democracy was created by developments through 1973 was expressed by many critics of the Allende government in the U.K. and U.S. press. It was also voiced by some other economists, although they did not necessarily subscribe wholly to Friedman's view of the process through which this occurred. For example, Velasco (1994, p. 397) stated: "If it was the left that delivered the lethal blow to Chile's traditional political-economic arrangements, it was the military government that buried the system for good."

[232] Griffiths (1982, pp. 68–69) reached conclusions about the implications of the growth of Chile's public sector in 1970–1973 that were fairly similar to Friedman's.

[233] Friedman's hypothesis that political freedom became more precarious as the share of public spending in output rose had appeared in his work on foreign aid in the 1950s (see Nelson, 2023). It acquired new prominence and controversy in 1975–1977 when he applied it to the cases of Chile and the United Kingdom (see Nelson, 2009b, pp. 492–494, as well as the previous chapter).

With regard to Chile specifically, Friedman implied that the alleged tipping point at which the share was high enough to undermine political cohesion may be 35 to 40 percent (*The Money Programme*, BBC2, December 10, 1976, p. 12 of transcript). This range left some ambiguity about whether he thought that the tipping point was reached in the year of the coup or, instead, earlier. In Friedman (1977g, p. 90), Friedman contended that the rise in the public sector's role prior to Allende had created economic problems, including inflation, that led to Allende's

doubt that Allende brought the coup on himself, but not necessarily through his inflationary policies," he later remarked. "It was because he was trying to turn the country into a standard Communist totalitarian state." (*Boston Globe*, April 3, 1983, p. 25.)

Friedman was emphatic, from the launch of the public controversy over himself and Chile, in advancing the argument that the economic policies that he recommended would be helpful in getting back to democracy. On the macroeconomic side, he felt that stabilization of the economy would make it more likely that Chile would "get rid of the junta" and return to being a democratic state (*Stanford Daily* (Stanford University), January 31, 1977, p. 1). Reductions in the extent of the country's economic problems would, he assessed, produce an atmosphere in which the junta would be under great pressure to relinquish power. "If there is a ghost of a chance of democracy being restored in Chile, it will depend on a considerable improvement in the state of their economy," he argued (*Business Week*, November 1, 1976, p. 75).

On the microeconomic or structure-of-economy side, Friedman expounded a position that was more special to himself, as it reflected his view of the economic-freedom/political-freedom linkage. He had long acknowledged that a substantial degree of economic freedom had existed in some very far from politically free counties (*Wall Street Journal*, April 30, 1962). With reference specifically to Latin American countries, Friedman granted that they had generated examples of a considerable role for private-sector economic activity coexisting with the suppression of political freedom (*Reason* magazine, December 1974, p. 12). He was therefore acknowledging that economic freedom did not have an instantaneous relationship with political freedom. But he believed that there was a contrast among dictatorships in important respects in their reversibility. As he repeatedly noted in his defenses of his Chile trip—and speaking, of course, before the fall of the East bloc in 1989−1991—the record suggested that Communist dictatorships had never given way to democracy (*Chicago Maroon*, October 3, 1975*a*; *Daily Telegraph* (London), June 13, 1977; *Newsweek*, January 12, 1981). In contrast, he observed, "Some juntas have led to a democratic form of government." (*Los Angeles Herald-Examiner*,

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election, and that both the loss of freedom under the junta had been preceded by reductions in freedom under Allende. For the most part, however, he judged the tipping point as having been reached when the public-sector share attained 40 percent. For example, he stated (*Newsday* (BBC television program), BBC2, November 9, 1976, p. 2 of transcript): "When government spending in Chile reached about 40 percent of the [national] income, that was the turning point. You had first the Allende threat and then the military takeover—a loss of freedom, tragic loss of freedom." Friedman also cited the figure of 40 percent as the tipping point for Chile in *Milton Friedman Speaks*, Episode 5, "What Is Wrong With the Welfare State?," taped February 23, 1978 (p. 25 of transcript). This share was reached in 1973, when the share of government spending to GDP was 44.7 percent (Edwards and Edwards, 1987, Table 2–2, p. 32).

February 11, 1977.) From a starting point of a military junta rather than a Communist dictatorship, there was, he perceived, "some chance of reversing things" and moving from such regimes back to democracy: "It might take five, ten, fifteen years to reverse, but it would be possible. The Greek Colonels got thrown out, after all." (*Daily Mail* (London), September 30, 1976.)

With regard specifically to economic policy, Friedman contended that permitting a large role for private economic activity would create forces that would undermine the rule of the Pinochet junta. Although, as he noted, military dictatorships often permitted private-sector economic activity, he believed it was common for them to endorse heavy government intervention in the economy and move to a centrally controlled economy over time. As, in Friedman's assessment, central economic control could not be extended on a truly large scale in a democracy, such moves in the direction of economic interventionism tended to make the dictatorship permanent. Correspondingly, in his analysis, Chile's junta could not coexist indefinitely with a free market economy: it would either have to give up its rule or centralize economic control (*Newsweek*, January 25, 1982).

On this basis, Friedman believed that economic liberalization would be a force making for political liberalization. He believed that the policies he favored would "promote[,] not retard[,] a movement toward greater liberalism and freedom" (*Chicago Maroon*, October 3, 1975*a*) and increase the "chance of movement toward a politically free society" (*Daily Telegraph* (London), June 13, 1977).

Dictatorships and the amount of permitted private-sector activity

Friedman's particular view that enhancements of economic freedom helped facilitate the process of political liberalization was, and remains, controversial. Unlike, for example, his propositions about inflation, it was not something on which he could invoke unambiguous, wide support across the economics profession.

Nevertheless, some of the building blocks of that view were empirical and historical propositions for which economic research over time did provide considerable evidence. For example, Friedman's position that military governments often showed inclinations to increase, rather than

decrease, state control of the economy was supportable by Latin American experience.[234] It was also undisputed that different dictatorships, even those outside Communist-run areas, had been associated with different attitudes to the use of market mechanisms and that a regime's attitude sometimes changed over time. There was, furthermore, considerable agreement among economists that reforms that allowed market mechanisms to operate to an increased extent had improved economic performance in countries, including in dictatorships. With regard to Spain, for example, General Franco had been dictator of that nation since 1936, but he changed economic policy notably in 1959, and de la Escosura, Rosés, and Sanz-Villarroya (2012, p. 45) reported their finding that "the 1959 Plan opened the way to a new institutional design that favored a free-market allocation of resources and allowed Spain to accelerate growth and catch up with Western Europe." They concluded: "Without the 1950s reforms and, especially, the 1959 Plan, per capita GDP would have been significantly lower in 1975 [when Franco died]."

December 1976 and afterward: an ongoing controversy

Friedman's week in Stockholm reached its culmination with his Nobel address of December 13, 1976, and he returned with Rose Friedman to the United States. Although he had complained about the tightly scheduled nature of his agenda in the Nobel Prize awards week, the resumption post-trip of Friedman's U.S. engagements underscored the fact that he had established a routine at home that involved its own hustle and bustle of near-continuous speaking engagements. Reflecting this situation, Friedman, having given his Nobel lecture on Monday, promptly moved from the international to the U.S. speaking circuit, and from economic-researcher mode to public-policy participant mode, by giving a talk at the Dupont Plaza Hotel in Miami on the evening of Wednesday, December 15, 1976, on "National Economic Policy and Financial Planning."

Looking back during this event on his just-completed week in Stockholm, Friedman optimistically attempted to cast the protest activity against himself as now being in the rear-view mirror: "I have never seen such a big mountain made over such a small molehill. I went to Chile a year and a half ago for six days under the auspices of a private Chilean foundation. I do not endorse the political actions of the junta, and going there implies no approval." (*Miami Herald*, December 16, 1976.)

[234] For example, Martinelli and Vega (2021, pp. 413–414) discuss the increase in economic intervention that occurred in Peru during the 1968–1975 military dictatorship.

During the following year, however, Friedman would be disabused of any perception that the controversy was fading away. The matter of himself and Chile continued to receive heavy press coverage—a fact reflected in his mid-1977 reference to the continuing "stream of comments in the media arising out of my visit to Chile" (*Daily Telegraph* (London), June 13, 1977). The year 1977 would also feature, at Friedman's public appearances, many more of what he had referred to as the "so-called protests" (*Stanford Daily* (Stanford University), January 31, 1977, p. 1). For example, an occasion when he made a lunch talk at the University of California, Berkeley, by the university's economics department, would see demonstrators disrupt the talk by heckling Friedman through the window of the presentation room, and a speech—one that was specifically concerned with the criticisms that he had received in connection with Chile—that Friedman gave at the Commonwealth Club of California (in San Francisco) involved thirty demonstrators confronting him outside the venue, giving out, to entering attendees, leaflets that accused Friedman of wrongdoing, and continuing their demonstration while Friedman spoke (*New York Times*, December 2, 1977).[235]

These particular protests, and others, took place in Friedman's new location, the Bay Area. But he also encountered demonstrations over this period when he left his new home city of San Francisco to fulfil scheduled speaking appearances in the rest of the country, and he faced further protests when he made trips abroad. One confrontation took place when, at the invitation of Niels Thygesen, Friedman visited Copenhagen in mid-1977. Thygesen recalled that "there was, in fact, a demonstration that interrupted our meeting with him at the Danish Economic Association." Thygesen recalled that the demonstration that Friedman "handled, in a way, quite nicely," only to give up—"he stopped his presentation after he had been interrupted a couple of times. We couldn't get him to resume, even though those who had disturbed the proceedings were removed." Thygesen observed that Friedman was simultaneously put off his stride by, and resigned to, incidents like the interruption. "He took it very calmly. He said, 'I'd like to discuss this' [that is, he told his hosts that he would have wished to continue the economic talk] but then he just stopped." (Niels Thygesen, interview, February 10, 2015.)

In his memoirs, Friedman suggested that the controversy, and in particular the protests, had faded by about 1981, before Chile's economic crash of the early and mid-1980s gave new impetus to criticism of the country's economic policy.[236] But that does not seem to be how Friedman saw things at the end of 1980, because he wrote a *Newsweek* column (in the issue of

[235] Friedman's remarks at the event were published as Friedman (1977k).
[236] Friedman and Friedman (1998, p. 405).

January 12, 1981) about the amount of demonstrations that he had faced and predicted that he would encounter more.

In 1990, Chile's junta was finally replaced by a democratic government. Official investigations into the junta's record of abuses reached a conservative estimate of 2095 deaths and 1102 "disappearances" as having been due to the Pinochet regime (Schliesser, 2010, p. 195). As already indicated, Friedman continued across the junta and post-junta periods to condemn the junta's record of repression and to insist that it was possible to take this stand while having favorable things to say about Chile's economic policies and performance.[237] During the 1990s, there was much favorable world commentary on Chile's macroeconomic record, and in early 1991 Friedman ventured to claim that Chile's return to democracy and its unfolding economic outcomes had led to a falling-away of negative verdicts on himself on the subject of Chile (*Wall Street Journal*, January 8, 1991).

In spite of generalizations like this, questions to Friedman on the matter continued, including at a November 1991 speaking event in Washington, D.C., when he defended his role and stated: "Chile was a case in which a military regime headed by Pinochet was willing to switch the organization of the economy from a 'top-down' to [a] 'bottom-up' performance and, in that process, a group of people who had been trained at the University of Chicago in the Department of Economics, who came to be called 'the Chicago Boys,' played a major role in designing and implementing the *economic* reforms. The real miracle in Chile was not that those economic reforms work so well. Chile is, by all odds, the best success story in Latin America today. The real miracle is not that those economic arrangements worked so well—because [after all] that's what Adam Smith [already] said. The real miracle is that a military junta was willing to let 'em do it. See, as I said to begin with, the principle of the military is 'from the top down,' [while] the principle of the market is 'from the bottom up.' Now, it's a real miracle that a military group was willing to let a bottom-up approach [to the economy] take over… I will say that that process led to a situation in which you were able to get an election which ended the military junta, and you now have a democratic government. You cannot cite any similar examples from the world of entirely socialist states. So I was not an adviser to Pinochet, I was not an adviser to the Chilean government, but I am more than willing to share in the credit for the extraordinary job

[237] See, for example, the Friedman (1991b) quotation given above.

that our students did down there." (CSPAN, November 16, 1991.)[238]

The period from 1975 to the early 1980s was likely that of peak protest against Friedman. But, as Friedman's long answer in 1991 answered, the controversy over himself and Pinochet-era Chile never really went away. Friedman was still taking questions on the issue as part of one of his final interviews in 2006 (*Rutgers* magazine, Fall 2006).[239]

As Friedman well understood, his standing was materially damaged, with his image, nationally and internationally, adversely affected due to members of the U.S. and global publics seeing, and in large numbers partially or fully accepting, the criticisms made about his trip to Chile and the related charges against him. Marc Nerlove observed of his former teacher and 1969−1974 colleague: "In any case, he was a lovely man in many ways. He was, unfortunately, tarred by some of his associations in later years." (Marc Nerlove, interview, September 26, 2013.)[240]

During the period of maximum protest activity against him and of media criticism—both of which were often very angry—Friedman himself responded angrily. With regard to the protesters in particular, he, in essence, rose to the bait: Especially in 1977, Friedman provided visceral, and eminently quotable, public comments about what he regarded as an unjust attempt to make him a pariah. When protesters attended and disrupted a public talk that Friedman gave at Cornell University in September 1977, Friedman referred to them *inter alia* as "these ruffians" (*Ithaca Journal* (New York), September 28, 1977).[241] Late in the year, he gave a newspaper interview, conducted at his and Rose Friedman's new San Francisco home, specifically on the topics of the Chile-related interruptions of his public speaking engagements and of the related controversy regarding his 1975 trip. In the interview, he referred to the protesters as "bums" and

[238] Friedman's statement that no fully socialist state had ever transitioned to democracy—a stock part of his discussions in the 1970s and 1980s—had, of course, been rendered obsolete by late 1991 by the prior two or three years' events.
[239] The specific example of this interview, appearing in the fall of 2006 (the period of Friedman's death) therefore provides an illustration of Applebaum's (2019, p. 268) observation that Friedman faced questions on the matter right through the rest of his life.
[240] Nerlove remarked of Chile: "I visited there in '74… [And] many of my former students, especially [one-time students] in my 'baby' [that is, introductory graduate University of Chicago] econometrics class, were in positions of great responsibility in that government… I certainly didn't seem to have gotten tarred by having gone there during that period… I learned what hyperinflation was really like in that visit." (Marc Nerlove, interview, September 26, 2013.)
[241] The event was taped for use in the planned *Milton Friedman Speaks* television series (discussed in the next chapter). Although the series was never completed and transmitted, the recording of the event (*Milton Friedman Speaks*, Episode 3, "Is Capitalism Humane?," taped September 27, 1977, released publicly as a videotape in 1980 and much later on DVD), although edited to remove the protesters' main interruption of Friedman's talk, clearly indicated that the protesters had had a marked effect on the course of the proceedings.

remarked of his encounters with the demonstrators: “Obviously, I don’t enjoy it. It’s a frightening experience to see these crazy kids, these kooks, with the madness on their faces.” (*New York Times*, December 2, 1977.)

Friedman’s interviewer recorded his demeanor as he made these sometimes pointed remarks as being calm. A noteworthy admission in the interview concerning the movement against him was Friedman’s matter-of-fact acknowledgment that the protesters had, to a degree, achieved their aim: they had wanted to stop him from making public appearances, and he had indeed cut down his speaking commitments. He conceded that, “if you’re going to be honest,” the conclusion was that it was not true that the demonstrations had not become a consideration in the planning of his schedule. “There is no doubt that if I know I’m going to face an episode like this, I’ll be more reluctant to accept an invitation to give a speech. More important, the people who offer invitations will be more reluctant to offer.” The protesters had succeeded in introducing into the consideration “a cost, both on the speaker and the host.” (*New York Times*, December 2, 1977.)

Over time, Friedman also became more outspoken about what he felt was the organized nature of the protest movement. In the *New York Times* interview just quoted, Friedman had also maintained that many of the protesters were “mindless puppets” and suggested that there was an underlying central force coordinating the protests. But he did not give voice to specific conjecture about what that force was.

In the 1990s and 2000s, however, Friedman was considerably more forthcoming about his suspicions on the matter, with one of his final interviewers summarizing his position as: “He’s also convinced that the protests were orchestrated by Communist propagandists in Moscow.” (*Rutgers* magazine, Fall 2006, p. 27.)[242] In a 2000 interview, Friedman suggested that, in the aftermath of the Pinochet overthrow of the 1970−1973 government, “The Communists… were going to discredit anybody who had anything to do with him. And, in that connection, I was subject to abuse, in the sense that there were large demonstrations against me at the Nobel ceremonies in Stockholm.”[243] In his late-1977 interview, he had cited as evidence of the orchestrated nature of the campaign the uniformity of the literature: “Demonstrators outside were the same kind of people carrying the same banners, and their leaflets were word for word the same as [at] the other [protest] places.” (*New York Times*, December 2, 1977.) In the same interview, he suggested that “the same kind of people” attended different demonstrations, and in

[242] See also Friedman and Friedman (1998, p. 402).
[243] *Commanding Heights*, PBS website, 2000, pp. 14−15 of transcript. Also printed in Ebenstein (2012, p. 249).

his memoirs he made the bold claim that he and Rose Friedman recognized "a few faces" that were the same in protests in the Greater Chicago area and at Cornell University and in the audiences during Friedman's 1975 lectures in Santiago.[244] In a later interview, he named the December 1976 Stockholm demonstrations and post-1976 protests outside the Friedmans' home in San Francisco as additional examples of occasions when he saw "the same faces" (*Rutgers* magazine, Fall 2006, p. 27).

Friedman's written replies to critics took for granted that he and they had a common aim of seeing Chile restored to being a "liberal democratic society" (*Chicago Maroon*, October 3, 1975*a*). His subsequent comments about the nature of the protests against him nevertheless clearly indicated, especially via the implication of USSR involvement, that he did not feel that this aim was shared by all protesters and by all the protest organizers.

But, with the abuses of the Pinochet regime widely documented from an early stage of its rule, there was bound to a be a critical mass of outcry in the Western world against the junta, even if the East bloc had stayed wholly uninvolved in the matter. It was also bound to be the case that, in Western opinion, there would be a considerable body of thought holding that the appropriate means of registering opposition to the Pinochet regime's infringements of human rights should include having academics eschew travel to Chile, and that their shunning of the regime should include having no contact with members of the Chilean government on the matter of economic policy. Consequently, Friedman was bound to face controversy as a consequence of agreeing to the trip to Chile and of then meeting Pinochet and other members of the junta regime, as well as because of the University of Chicago's training of economists who later worked for the junta government.

These were also bound to be issues that caused dissension and controversy among economists. Perennial considerations factored into the debate over Friedman's 1975 trip: as Laidler (2007, p. 6), the "ethical issues raised by this affair" pertained to long-debated matters facing economists and so contributed to a permanent controversy.

In his memoirs, Friedman criticized the reticence of members of the economics profession when it came to offering public defenses of him, particularly during the 1976 period of criticisms of himself.[245] He noted Zvi Griliches as having been an exception: the Harvard University

[244] Friedman and Friedman (1998, p. 402).
[245] Friedman and Friedman (1998, p. 403).

economics professor had spoken up for Friedman in the controversy in the main student newspaper, doing so very largely on the same grounds on which Friedman had defended himself (*Harvard Crimson* (Harvard University), November 1, 1976). Earlier, Friedman had been present in person when his longtime sparring partner Robert Eisner criticized efforts to disrupt Friedman's speaking events, with Eisner contending that this would "only arouse antagonism" toward the protesters among those observing the events (*Daily Northwestern* (Northwestern University, Illinois), March 1, 1976). But Friedman, in complaining about the absence of more widespread defenses of himself, was implying that there was very considerable agreement with him among economists during the mid-1970s—and that this did not become apparent during the furor over himself and Chile, in part because of other economists' concern about themselves becoming subject to the protest movement or to other public criticism.

As already indicated, on the matter of the economic propositions that Friedman advanced about inflation, including his tracing of the problem to monetary policy and his contention that removing a massive inflation required a simultaneous monetary-fiscal response, these did indeed, by 1976, command considerable agreement in the U.S. economics profession, and this agreement increased after 1976. With regard to his free-market views and prescriptions, the center of gravity in professional opinion was not, in 1976 or later, as wholeheartedly in favor of economic liberalism as Friedman himself was. But mainstream U.S. economists certainly did not subscribe to the strongly interventionist positions often articulated by many of Friedman's public critics in 1976 and later, nor would they be as quick as Naomi Klein (2007, pp. 304–305) was to associate Adam Smith's economics with lawlessness.

Against this backdrop of notable overlap of views with Friedman on economics, there was in the mid-1970s, and continued to be in subsequent decades, a diversity of views among economists on the appropriate relationship between U.S. academics and dictatorial regimes. Among the next generation of economists after Friedman, one of the most prominent, Joseph Stiglitz, became outspokenly critical of Friedman for having gone to Chile in 1975 (see, for example, *Rutgers* magazine, Fall 2006, p. 27). Nor was there uniformity among monetarists on the matter of whether Friedman should have taken the trip. Allan Meltzer supported Friedman on the issue. But during the late 1970s, the elder monetarist Clark Warburton opposed Friedman on the matter (Michael Warburton, personal communication, May 21, 2015).

In the wider public debate, the respective positions of Friedman and his critics remained essentially unchanged after 1975–1976. Friedman stressed that he aimed to improve economic

conditions in Chile and that such economic improvement would be a force for political liberalization. His critics, in contrast, did not accept Friedman's position that support for the direction of Chile's economic policy was compatible with opposition to its political arrangements. To these critics, Friedman and other U.S. economists should have refrained from a dialogue with Chilean officials on economic matters until a government that did not engage in repression of the populace had come into power.

TRADE CREDITS AND THE SOVIET UNION

In the 1950s and 1960s, Friedman made numerous comments on the Cold War but mainly limited himself to economic aspects of the U.S.-USSR conflict, including the implications of different economic systems for living standards and employment opportunities: see Nelson (2023). By the mid-1970s, however, Friedman was increasingly inclined to come out with public commentary that was focused on the national security—as distinct from the more purely economic—aspects of the East-West competition. During this period, he associated himself with the view—advanced by many "hawkish," or hardline, critics of the defense and foreign policies emanating from the Republican administrations and the Democratic-controlled Congress in this period—that the United States was not taking the measures needed to address growing Soviet military strength.

This assessment that there was a growing danger to the Western side in the evolving superpower balance, and related warnings that hardliners made about the geopolitical outlook, challenged the consensus prevailing in U.S. public discourse during the mid-1970s that the degree of East-West tensions, and the accompanying danger of world war, had been lowered considerably since the start of the decade. Friedman largely shared, particularly from 1975 onward, the hardliners' doubts regarding the prevalent favorable interpretation of many of the foreign policy developments seen in recent years. In so doing, he substantially endorsed the position taken by the administration's defense-hardliner detractors that the threat to the Western world posed by the USSR had increased during the first half of the 1970s, just when—largely with the administration's encouragement—the U.S. public perception of that threat was decreasing.[246]

But, as discussed later, Friedman also parted company with these hardliners—many of whom were, on account of their role in, or support for, past Democratic administrations, increasingly

[246] The perceived diminution of this threat was tied to the prevalent tendency during the 1970s to refer to the Cold War as a period in the recent past, rather than as an ongoing state of affairs.

coming to be labeled “neoconservatives”—on significant specific aspects of their critique of official U.S. policy.

Détente in the 1970s and U.S.-Soviet trade links

The notion, prevalent in the late 1950s, of an *economic* contest between the U.S. and USSR economic systems had largely dissipated by the 1970s: the USSR was widely acknowledged as set to remain well behind the United States in economic performance for the foreseeable future.[247] But the military and geopolitical contest between the superpowers continued. Against the background of this ongoing competition, the Nixon and Ford Administrations advanced policies of détente that were intended to reduce tensions between the two blocs. As will be detailed below, Friedman was of the view that the Soviet Union’s threat to the United States and its allies was not really diminishing during this period, and he expressed himself publicly on this matter in the mid-1970s even though doing so meant taking a stand on subjects well outside his own expertise. But he was also called upon to comment on an aspect of the détente policy that was more clearly within the discipline of economics: proposals to expand trade with the USSR.

Friedman, who would be a longstanding opponent of general economic sanctions on countries, was well disposed toward the notion of having trade relations with the Soviet Union even while that country remained a geopolitical adversary of the United States. In 1962, in *Capitalism and Freedom*, he indicated that travel to the USSR should be unrestricted.[248] Indeed, the Friedmans actually were in the Soviet Union for a couple of weeks in late 1962, after *Capitalism and Freedom*’s completion. But in the same year in *National Review* (May 22, 1962, p. 363), Friedman had provided what was, on his part, a very rare instance of an expression of sympathy with the idea of economic sanctions—when he suggested that trade barriers against USSR hurt the United States but may be worth imposing on the grounds that they probably harmed the Soviet Union more.

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[247] Even from the vantage point of 1970, Paul Samuelson could note: “Of course, we all know that Mr. Khrushchev is gone and that his words [of USSR economic catch-up by 1980] showed him not to have been a prophet. Far from the USSR having overtaken the United States, [it] still remains very far behind. Indeed, we still have about double the real GNP of the Soviet Union—depending upon how you calculate ruble purchasing power parity and dollar purchasing power parity. The experts, I think, would say that the Soviet Union has [aggregate output equal to] ⅓ to ⅔ [of the U.S. total], depending upon which calculation you make. And, if we split the difference, that means about one-half. Now, it is true that the Soviet Union [in the 1960s] showed a percentage rate of growth of its real GNP a little more than ours—but the remarkable thing is how little [an excess] that was.” (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 57, August 24, 1970.)

[248] Friedman (1962a, p. 8).

Even with regard to market economies, however, Friedman was not a subscriber to the position that a nation's economic growth rested crucially on the volume of its international trade. And, in the case of the Soviet Union, he felt that even if it had ample opportunity to trade with other nations, its collectivist internal arrangements would condemn its aggregate economy to stagnation. With the contribution that Soviet exports to or imports from the United States or other countries could make to USSR economic performance certainly assessed as marginal, Friedman became more definitely in favor after 1962 of free trade with the USSR, even though he would continue to assess the Soviet geopolitical threat to be very high. Accordingly, he explicitly included the USSR when he remarked in late 1974: "I would be in favor of abolishing all tariffs against all nations." (Instructional Dynamics Economics Cassette Tape 148, June 11, 1974.)

By the time of these remarks, the détente policy had been associated with trade agreements between the United States and the USSR, and the Ford Administration was proposing the next step of granting the Soviet Union most-favored-nation (MFN) trade status. Friedman went only partway in endorsing this proposal. He indicated that he was not opposed to the Soviet Union being treated symmetrically with other countries in ordinary goods trade with the United States and, in particular, that he agreed with the administration that a granting of MFN should not be conditional on securing USSR concessions outside the trade area. In particular, he agreed that MFN conferral should not be conditional on the USSR's government expanding the rights of its citizens, including the right to emigrate (Instructional Dynamics Economics Cassette Tape 148, June 11, 1974).

The sting in the tail in Friedman's support for the administration position was one that also applied to the U.S. government's existing détente-related trade agreements. Although these agreements had involved sales of American-made goods to the USSR, the purchases had in part involved some form of U.S. government-provided trade credits, under which the Soviet Union was able to borrow from the U.S. government the funds that settled the purchase. This practice was not altogether out of line with U.S. government-brokered trade agreements with other countries. But, to Friedman, the existing practice was itself a problem—being governmental subsidization of the transactions. With regard to the possibility of MFN arrangements applying to the USSR, he opposed them insofar as they helped continue to "make it possible… for the U.S. taxpayer to subsidize the Soviet Union." He urged that MFN should be granted together with the "strict specification that neither directly nor indirectly will the U.S. in any way subsidize [USSR-related] trade, either by guaranteeing credits, by insuring against expropriation, or by

direct subsidy or direct loans to the Soviet Union. Of course," he added wryly, "if we did that, the Soviet Union's enthusiasm for this measure would decline greatly." (Instructional Dynamics Economics Cassette Tape 148, June 11, 1974.)

This position—that there should be free trade in goods with the USSR, including sales of American agricultural products to the Soviet Union, but that the U.S. government should not extend trade credits or loans in this process—was one that Friedman reaffirmed during the Carter and Reagan years.[249] He was, most notably, an outspoken critic of President Carter's grain embargo against the Soviet Union (see the discussion of 1980's events in Chapter 10).

In the event, MFN status for the USSR did not go ahead in 1974−1975. In large part, this was because Congress made humanitarian concessions a *quid pro quo* of MFN, and the USSR refused to accept such linkage. That refusal meant that the U.S.-USSR Trade Agreement that the countries signed in 1972 did not come into effect (see Garthoff, 1994, pp. 509−513). Another factor making for the failure of the agreement was that one key trade deal of 1972 that had proceeded over the intervening period—sales of grain to the USSR—had proved to be highly favorable to the Soviet Union, because it received the grain at the prices prevailing before the 1972−1974 surge in world commodity prices. The trade issue had consequently become a politically negative factor for the Ford Administration—with Jimmy Carter, seeking the Democratic presidential nomination, using one campaign appearance to criticize "the so-called 'advantages' of détente," remarking in this connection that "every time we've had a major trade with Russia, we've lost." (*Houston Post* (Texas), December 12, 1975.)

To Friedman's disapproval, the "Great Grain Robbery" episode, as it became known, was added to the catalogue of cost-push explanations for recent high U.S. inflation—the suggestion being made that the sale had helped raise U.S. grain prices and that this price pressure had spread elsewhere. Walter Heller invoked the grain sale as one of the reasons for the post-1972 surge in U.S. inflation.[250] So did Arthur Burns. In a Congressional hearing (held on September 4, 1975) that had been convened on the economic effects of the grain sales, the Federal Reserve chairman stated that "concern about the effects of rising food prices on the overall rate of inflation is clearly warranted."[251]

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[249] For example, he did so in *Milton Friedman Speaks*, Episode 8, "Free Trade: Producer Vs. Consumer," taped April 27, 1978, p. 26 of transcript; in *Newsweek*, November 15, 1982; and in Friedman (1989d, p. 369).
[250] See his remarks quoted earlier in this chapter.
[251] In Committee on Agriculture and Forestry, U.S. Senate (1975, p. 3).

Almost a year earlier, concern about a new grain shortage had led the Ford Administration to cancel corn and wheat shipments to the USSR, overriding U.S. exporters' commitments to make about $500 million in sales. This action was taken on October 5, 1974, and was only partially reversed in a follow-up trade arrangement made two weeks later (Garthoff, 1994, p. 509). Friedman publicly criticized Ford for this intervention, believing that the transactions should have been allowed to proceed as contracted (*Chicago Tribune*, October 18, 1974). As for inflation, he noted that the grain deal—being adverse to the United States' terms of trade—may have led to a small shift up in the American price level—but that it had played only a trivial role in the overall behavior of inflation in the 1970s (*Newsweek*, May 12, 1975).

While opposed to such U.S. government restrictions that impeded regular U.S.-USSR trade, Friedman concurred with prohibitions of shipments to the Soviet Union of U.S.-produced goods of a military or strategic nature.[252] In the late 1970s, he expressed concern that "technology and equipment" exports to the USSR had gone too far since 1972.[253] He contended during the same period, however, that the items whose export should be prohibited on security grounds constituted, on the whole, "minor exceptions" to the general rule that U.S. trade with the USSR should proceed freely.[254]

Of course, a more general question was whether trade relations with the USSR could strengthen the Soviet economy and thereby put it in a position to build up its military forces still further. A declaration that this was what actually happened would be made by one U.S. foreign policy hardliner, who stated (Lee, 1980, p. 101): "In the 1970s, the Soviet military buildup has been greatly facilitated by trade, credits, and loans from the U.S. and its allies."

This assessment, however, was the kind of economic proclamation by U.S. defense hardliners that Friedman tended to regard as misconceived. As indicated above, Friedman did oppose U.S. government measures that were literal subsidies of Soviet commerce because they offered non-commercial terms to the USSR. Indeed, in one of his interventions in this period on foreign policy, Friedman in 1977 included trade credits, as well as government-guaranteed loans to the USSR and Eastern Europe, provided by the United States and its allies, together with U.S. foreign policy setbacks as steps that put the Western world closer to a future of USSR world

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[252] See, for example, Friedman (1989d, p. 368).
[253] Friedman (1977a, p. 26).
[254] *Milton Friedman Speaks*, Episode 8, "Free Trade: Producer Vs. Consumer," taped April 27, 1978, p. 13 of transcript.

domination.[255] Usually, however, Friedman was much more circumspect. He was, as already noted, not one who put great weight on trade as a driver of countries' real output growth, and he certainly did not see it as a means by which command economies could make major strides. With regard more specifically to the Soviet Union, while he acknowledged the size of the economy as a broad constraint on the regime's choices, Friedman was not a strong subscriber to the view that its decisions regarding defense spending had a continuous relationship with the country's total volume of production, and he viewed the Soviet government as having considerable scope to maintain or raise the defense share of output in the face of economic pressure.[256]

With respect to the capital account of the balance of payments, former U.K. diplomat Duncan Wilson (1975, p. 128) suggested that as part of its motivation for détente, the USSR sought injections of capital funds from the West as a means of expediting a move to an increased growth rate of aggregate output. On the surface, historical precedents seemed supportive of the notion that a capital inflow might well benefit a country's growth. In this connection, Mussa (1986a, p. 112) observed: "In earlier periods, direct purchases of foreign bonds by individual investors played an important role in private international capital movements. British investors, for example, helped finance railroad construction in the United States, Argentina, and tsarist Russia."

But Friedman had grounds for doubting that these precedents applied to the case of the United States and other Western nations lending to the USSR. He was himself often a proponent of the benefits that a country could accrue from a capital inflow, but his arguments in this connection were made in the context of private-sector-produced capital flows between market economies. In contrast, Friedman did not believe that an injection of foreign capital could be counted on to have a beneficial effect on Soviet economic growth: for he was impressed by the degree to which a command economy like the USSR's could, in its process of capital accumulation, allocate the capital to wasteful activities.[257]

In any event, and consistent with his opposition to the geopolitical competition being a grounds

[255] See Friedman (1977a, pp. 25–26).
[256] See Chapter 15 below.
[257] Consistent with this, the study of the Soviet economy by Hardt and Holliday (1973, p. 16) cited misdirection of capital expenditures as a reason why the Soviet Union's productivity level was far behind that of the United States despite the former country having much higher had a long record of higher ratios of investment spending to GNP. See also the analysis of Friedman's views on this matter in Nelson (2023).

for imposing general economic sanctions, Friedman did not oppose the private sector of the Western countries being a source of loans to the USSR (meaning, necessarily, the public sector of that country), provided that these were not subsidized by Western governments.[258] Similarly, when in the 1980s, although he felt that loans to the East bloc were taking away funds that would have been deployed more productively in the Western world, he did not regard it as appropriate for existing loans to the East bloc by U.S. commercial banks to be turned into an economic weapon against the USSR: he instead regarded it as appropriate for these lending relationships to continue, if the banks wished.[259]

The military strength of the Soviet Union and the United States

Friedman felt that the way to respond to the Soviet bloc in the 1970s was through strengthening of the Western, and especially U.S., military effort. As was discussed in Chapter 3, Friedman felt that the United States' status as a leading technological power should be put to advantage in the military area by a focus on capital *vis-à-vis* labor in defense policy. He was concerned that this was not happening in practice. In this connection, in a public letter of December 2, 1970, Friedman referred to the "current mood of the country and of Congress is starving the military for research funds… I have much sympathy with the view that the present climate of opinion is reducing too much of the funds available for military research."[260]

U.S. defense spending was falling in the early 1970s as a result of America's lowering of troop commitments to the Vietnam War, but Friedman's remarks were referring to a public mood (itself in good part a backlash against the nation's involvement in Vietnam) urging more general cuts in U.S. defense spending. He opposed this prescription. Friedman stressed that the contest with the Soviet Union had not ended—and was concerned that the decline in U.S. defense spending was liable to become excessive, especially in view of the fact that the USSR's defense spending had been rising in recent years and this increase was set to continue. "Our military strength, *vis-à-vis* Russia, not only has been declining, but [it also] looks as if it's going to decline much further," he observed in a television appearance taped near the end of Nixon's third year in office (*Firing Line*, PBS, January 5, 1972, p. 6 of transcript).

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[258] See Friedman (1977a, pp. 25–26) and *Milton Friedman Speaks*, Episode 8, "Free Trade: Producer Vs. Consumer," taped April 27, 1978, p. 26 of transcript.
[259] Again, see Chapter 15 below for a detailed discussion.
[260] In Committee on Appropriations, U.S. House of Representatives (1971, p. 397).

Four years on, U.S. real defense spending had indeed declined further: in 1975, it was nearly 15 percent below its 1971 level, with an additional small fall occurring in 1976 (Council of Economic Advisers, 2011, Table B‒6, p. 197). Furthermore, the Soviet military expansion had continued during these years of falling U.S. military expenditure. Against this background, Friedman, in a column critical of rising government spending under President Ford, identified the defense area as one that warranted increases, in light of the Soviet Union's military buildup (*Newsweek*, February 9, 1976). He therefore implied that the course of defense spending in the United States in the 1970s had been the opposite of that warranted by the behavior of its principal geopolitical opponent.

This stance was part of a wider critical attitude that Friedman had adopted toward the conduct of East-West policy under the Ford Administration, including under U.S. Secretary of State Henry Kissinger. After having been critical of Kissinger's ventures into economic policy, Friedman started expressing increased doubts about Kissinger's stewardship of foreign policy, too.

III. PERSONALITIES IN INTERNATIONAL ECONOMIC POLICY AND GEOPOLITICS, 1975–1976

HENRY KISSINGER

In December 1972, at the tailend of President Nixon's first term and about ten days ahead of Friedman's traveling to Rochester, Minnesota, for his heart operation, one of that city's local newspapers put on its front page a wire item concerning a change in U.S. Cabinet arrangements. The article reported that Secretary of the Treasury George Shultz would have an expanded set of responsibilities in economic policy in the president's second term, along lines intended to allow Shultz to oversee the whole of U.S. economic policy (*Rochester Post-Bulletin* (Minnesota), December 1, 1972). The wire report interpreted this revamp of Shultz's role as giving him a status that was the counterpart in economic matters to that possessed by Henry Kissinger—at the time, the president's national security adviser—in foreign policy.

This parallel was very inexact. In particular, the independence of the Federal Reserve meant that U.S. economic policy was not amenable to being centralized to the same degree as decisions on U.S. foreign policy. But the reference that the article made to Henry Kissinger's role underscored the fact that, even before he was assigned the additional post of Secretary of State in August 1973, Kissinger was, along with the president, the leading foreign policy figure in the

administration. In this capacity, Kissinger had already established the globetrotting celebrity image that would subsequently lead Friedman to remark, in connection with the travels that came with his receipt of the Nobel economics award in 1976, "I felt to some extent like Henry Kissinger."[261]

From academia to the center of policymaking

In common with Friedman, Kissinger had a career background as a university professor—in Kissinger's case, at Harvard University. Also like Friedman, Kissinger had, as an academic, been able to include in his output research work, popular writings, and other commentary on public policy, while also engaging in occasional advice to U.S. presidential candidates. Indeed, during 1964, the year in which Friedman was an economic adviser to Barry Goldwater, Kissinger's role in the primaries portion of the campaign had been as a foreign policy adviser to Nelson Rockefeller, whom Goldwater defeated for the Republican presidential nomination. Following the primaries, in a pre-election display of unity on the Republican side, both Friedman and Kissinger were contributors to *The Conservative Papers*, a book published during the 1964 presidential campaign proper and featuring contributions by GOP-supporting public intellectuals.[262]

In 1968, both Friedman and Kissinger had advisory roles in the same presidential campaign—that of Richard Nixon. In this campaign, however, Kissinger's participation was heavy, while Friedman's was, in contrast, very modest. Consequently, they never met during 1968's campaign activities. Friedman and Kissinger in fact never encountered one another at all in the 1950s and 1960s, although Friedman had had correspondence with Kissinger in 1958 on a routine matter (when he sent Kissinger a letter of recommendation for a third party).[263] They were both guest speakers (at different sessions) for the annual conference for bank correspondents held by the First National Bank of Chicago at the Conrad Hilton Hotel, Chicago, on November 21–22, 1966.[264] Friedman did not actually meet Henry Kissinger until the 1980s, when George Shultz, by then U.S. Secretary of State, arranged a social occasion in California

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[261] Friedman (1977j, p. 7).
[262] See Laird and others (1964).
[263] Milton Friedman Papers, Hoover Institution archives (Box 29, Folder 18).
[264] Reflecting in part the ranking of their national fame at this point, Kissinger was perceived as the star speaker obtained for the event (*Chicago Tribune*, October 30, 1966).

that both Friedman and Kissinger attended.[265]

Kissinger, of course, was Shultz's most famous predecessor in the position, having been Secretary of State in 1973−1977 under Nixon and then Ford. Kissinger's years in that role were the culmination of a major divergence between his career trajectory and Friedman's. Friedman at the end of the 1970s would be described in one commentary as the "Henry Kissinger of economics," on account of the high profile that each had in their respective fields (*Arkansas Gazette* (Little Rock), October 6, 1979). But with regard to most U.S. government economic decisions, and certainly those in the monetary area, Friedman knew, and talked to, many key figures but was not himself a participant when it came to taking actions. Kissinger, in contrast, became immersed in U.S. policy formation. Both had become, by the late 1960s nationally known professors who published public-policy work alongside research, but, of the two, only Kissinger then moved from celebrity academic to full-time government service.

Kissinger became national security adviser at the onset of the Nixon Administration in 1969. Notwithstanding the job title, Kissinger's adviser position was a *de facto* policymaking role, as the 1972 press item quoted above implied. Notably, during Kissinger's tenure, Kissinger's contributions to foreign policy were often in practice more central than those of Nixon's first Secretary of State, William Rogers.

Hedley Bull's (1980, p. 487) characterization therefore applied to the whole of Kissinger's 1969−1977 tenure in the Nixon and Ford Administrations: Kissinger, "in stepping so effortlessly into the world of high policymaking, acted out the secret dreams of countless academic experts." Notwithstanding his disclaimers, it is highly likely that, in the field of economics, Friedman counted among those many experts who aspired to be a policymaker—even if, in his ideal world, the policymaking would largely involve enforcing and overseeing an automatic monetary policy rule.

As of 1973, Friedman seemed to like much of the substance as well as the style of the Nixon Administration foreign policy with which Kissinger had become closely associated. When, in June of that year, Friedman said that, notwithstanding his disapproval of Nixon's domestic economic measures, he felt that the president had done "a magnificent job in many areas," he likely had in mind foreign policy (*The Sun* (Baltimore), June 15, 1973, p. C16).

[265] Friedman may, however, have been an audience member at a Kissinger speech or speeches on occasions before this direct interaction.

The administration's negotiations with China likely played a part here. Although Friedman abhorred the rule of Mao, he was receptive to improving U.S.-China diplomatic and economic ties. Friedman wrote very little on China during the 1970s, but he clearly approved of the Nixon-Kissinger efforts, developed early in the decade, to establish more regular relations with China, and he reaffirmed this approval of a number of Nixon's "imaginative" foreign policy initiatives in later years.[266]

As of the first quarter of 1975, Friedman still seemed predominantly favorably disposed toward the Kissinger record, describing him as "great man" (*Newsweek*, March 31, 1975). From April 1975 onward, Friedman would turn to a more negative perspective on Kissinger's achievements, as discussed below. Even by March 1975, however, Friedman had reached the conclusion that there was an important area in which Kissinger lacked talent: economics.

Kissinger's lack of economic expertise

The judgment that Kissinger was no expert in this field was not novel to Friedman: his "critics accuse him of not knowing much economics," the *New York Times*' Leonard Silk observed (*Kansas City Star* (Missouri), February 14, 1974), while the general skepticism about Kissinger's command of economics was registered in such headlines as "Henry Is Right: World Economics Expert He Ain't" (*Arizona Republic* (Phoenix), April 18, 1974) and "Henry the Economist" (*Omaha World-Herald* (Nebraska), June 3, 1975).[267]

Doubts about Kissinger's command of economics were borne out in the unscripted question-and-answer portion of a National Press Club appearance that Kissinger made on February 3, 1975, on the subject of energy. In describing the domestic oil-price proposals that the administration had advanced, he cast the proposed rebate to consumers as one that implied that "the inflationary

[266] See Friedman and Friedman (1998, p. 388) for this quotation.

[267] Earlier, an analysis of some of Kissinger's past writings, as well as his absence from much of the August 1971 U.S. government talks on international economic policy, had been seen as indicating "Dr. Kissinger's almost obtuse disregard for matters economic" and that economics was his "blind spot" (*Financial Times* (London), March 20, 1973). It would seem on the surface that strong reinforcement of the judgment that Kissinger lacked expertise in economics received confirmation in the diaries of Arthur Burns, who worked with Kissinger in the White House during 1969 and subsequently in Federal Reserve-Administration meetings. But Burns' diary description of Kissinger as "a brilliant political analyst, but admittedly ignorant of economics" (in his November 26, 1971, diary entry, in Ferrell, 2010, p. 66) was undercut by the fact that, in the same passage, Burns rendered a verdict of "a no less confused amateur economist" on George Shultz—who certainly *was* a trained economist, but who was at loggerheads with Burns because of Shultz's opposition to incomes policy as an anti-inflation device (and who, in espousing a free-market solution to the energy problem in 1973–1974, would have a further major clash with Burns).

impact [of price decontrol] will be severely minimized, if not eliminated."[268] In so doing, Kissinger confused the concept of preventing an impact on inflation from coming from oil price decontrol with the concept of shielding consumer purchasing power from the measure.

This February 1975 speech was part of Kissinger's efforts as Secretary of State to become a force in economic policy. Eight months earlier, the London *Financial Times* (May 28, 1974) had already noted "Dr. Kissinger's avowed interest in giving the State Department a bigger say in foreign [that is, U.S. international] economic policy." U.K.-based economics commentator Malcolm Rutherford similarly referred, in early 1975, to "Dr. Henry Kissinger, the U.S. Secretary of State, who is now devoting much of his attention to economics" (*Canberra Times* (Australia), January 9, 1975).[269]

The issue on which Kissinger focused in attempting to establish an economic-policy role for himself was that of energy—in particular, dealings with OPEC. The Secretary of State's energy policy proposals would generate ire on the part of Friedman, who considered the quality of the economic analysis underlying them to be very poor and the outcomes that they would produce, if implemented, to be highly undesirable.

Political versus economic solutions

Crucially, Kissinger's involvement in this matter began in earnest during the initial embargo period (through March 1974) that occurred in the first stage of the oil shock. That entry point was significant, because the steps that Kissinger proposed in response to the embargo—which was clearly a political action, and not just an economic one, on the part of OPEC—were those he continued to stress when OPEC kept the world oil price high after it lifted its prohibition of petroleum sales to the United States.[270] In this post-embargo period, the oil shock remained, in

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[268] In Joint Economic Committee (1975h, p. 75).
[269] Rutherford noted that Kissinger had told reporters at a press conference in late 1974 "that no adequate theory exists to deal with the concurrent problems of inflation and recession." In so doing, Kissinger was taking the stance—common among non-economists, but anathema to Friedman and to many Keynesians, too—that economic analysis could not account for the phenomenon of stagflation. Even though he returned, after 1975, to avoiding coverage of economic matters, Kissinger repeated this stance on stagflation. He did so, for example, in a television appearance in early 1980, when he remarked: "there's no question that the economic performance of the United States over a substantial period of time has been unsatisfactory... a conjunction of low productivity, high inflation, high unemployment, and other factors that is totally unprecedented and for which our government have not found a theoretical or a practical tool." (*The Money Programme*, BBC2, February 3, 1980, p. 2 of transcript.)
[270] Consistent with this interpretation, Paul Samuelson (in Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 187 (Paul Samuelson series), [late] September 1975) characterized Kissinger as believing

the form of high oil prices, but had become basically an economic move by OPEC, rather than a political one. Kissinger's proposed policy responses, however, reflected a focus on international negotiation and on contingency planning. They therefore reflected a perspective that the oil shock was fundamentally a political problem rather than an economic one and, consistent with this posture, Kissinger told an international gathering of policymakers in September 1974, held at Camp David, that "we must respond to the political as well as the economic implications of current trends."[271]

During the embargo, Kissinger proposed cooperation among consumer countries to share a fixed supply of oil (*Financial Times* (London), December 19, 1973). In elaboration of this position, later in the embargo, Kissinger, in the opening address to a February 1974 energy conference, stated: "The United States declares its willingness to share available energy in time of emergency or prolonged shortages." (*The* [*Evening*] *Post* (Wellington, New Zealand), February 12, 1974). And even in the post-embargo period, Kissinger's thinking was oriented toward embargo scenarios rather than dealing with high prices without an embargo: for example, in his February 1975 talk he cited as a reason for adopting his proposals the need to be well situated "in the event of a new embargo."[272]

In both the pre- and post-embargo periods, Kissinger saw the matter of oil in terms of collective actions by governments: cooperation among the United States and its allies on energy policy, and international negotiation between consumer-producer relations. This contrasted with Friedman's view that the appropriate U.S. policy response to the oil price increase was to allow the price system to operate freely and so have the market generate forces making for new energy supplies and lower prices. He was accustomed to viewing the market system as impersonal, as it aggregated many individual demands and supplies and equilibrated them—often without direct interaction between the market participants. In contrast, the Kissinger conception of the oil price as something to be negotiated made it a foreign policy matter, with the world's energy solutions envisioned as flowing from discussions between senior members of different national governments.

Kissinger therefore came into the energy problem predisposed toward looking at it in terms

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that, in the energy area, "the main problem is to show the rest of the world that the U.S. can't be bullied by an oil boycott—that we mean business and are going to make ourselves independent of foreign supplies of oils."

[271] From his remarks, in Galpern (2012, p. 31), at a meeting of G5 officials held on September 28, 1974.

[272] Joint Economic Committee, U.S. Congress (1975h, p. 69).

different from those in which economists viewed it. Nevertheless, in what was seen at the time as helping to counter the impression that he was shunning economic analysis, Kissinger's February 1974 speech on oil invoked the concept of Pareto optimality (*Kansas City Star* (Missouri), February 14, 1974). This particular economics reference was, however, one that was unlikely to cut much ice with Friedman—in view of his preference for individual-market-based analysis of issues such as energy supply over general equilibrium-based application of the Pareto type to these matters.

Also in the February 1974 speech, Kissinger stated: "Our ultimate goal must be to create a cooperative framework within which producers and consumers will be able to accommodate their differences and reconcile their needs and aspirations." (*The* [*Evening*] *Post* (Wellington, New Zealand), February 12, 1974.) This was a position that Kissinger maintained as he continued to engage in energy policy in the no-embargo, but high-oil price, environment, of the rest of 1974 and 1975.

In contrast, Friedman believed that the demand-and-supply equilibration via market forces provided just such a "cooperative framework." In such a system, individual consumers' behavior, not consciously coordinated but guided by price behavior, would, in combination with supply incentives, produce pressure on price and supply that were in the needed directions. Kissinger instead believed, as he put it privately in September 1974, that "we should develop a coordinated response"—with his conception of coordination here again meaning that between governments: "the prices should be subject to political decisions by consumer governments."[273]

In a study discussed further below, Friedman's former student G. Warren Nutter (1975, p. 17) suggested that Kissinger's "limited exposure to the subject of economics" was evident in his concentration on personalities, particularly leading figures in government—rather than impersonal mechanisms—in the solution of problems. Nutter reached this conclusion after analyzing Kissinger's approach to political and military relationships. But, as already implied, evidence in favor of Nutter's thesis was very clear in Kissinger's approach to heavily economic matters such as oil prices.

One item in Kissinger's proposals was what, at the September 1974 meeting, he called "financial

[273] From his remarks at a meeting of G5 officials at Camp David, September 28, 1974, in Galpern (2012, p. 30).

solidarity" among oil-consuming nations.[274] Under his proposed arrangement, countries' governments would leave in what Kissinger variously called a "common trust fund" (Galpern, 2012, p. 42), a "solidarity fund" or a "mutual insurance fund" (Joint Economic Committee, U.S. Congress, 1975h, p. 69), intended as a means of helping finance the higher levels of member countries' current account deficits that were expected to result from the oil shock. This $25 billion fund was indeed set up in January 1975 (Joint Economic Committee, U.S. Congress, 1975h, p. 69). But it was rendered redundant by the success, discussed earlier in this chapter, of private-sector petrodollar recycling processes as a means of financing the consumer countries' current account deficits following the oil shock.

But it was another part of Kissinger's energy package—the proposed management of the oil market itself—that Friedman would seize upon and that would underlie Friedman's negative judgment regarding the Secretary of State's economic aptitude. In the September 1974 multi-country meeting of economic policymakers, Kissinger suggested that "we must demonstrate that major consumer countries are willing to protect their interests through consumer solidarity" (Galpern, 2012, p. 32). Kissinger's public-policy suggestions about how to implement such "consumer solidarity" in the world oil market would generate a Friedman critique.

Consumer solidarity proposals

Kissinger's proposal was essentially that the oil-consuming nations form a united front—in effect, a monopsonistic cartel—as a means of countering the oil-producing cartel of the OPEC nations and of allowing the world oil price to be subject to a negotiation process encompassing oil-producing and oil-consuming nations. Federal Reserve Chairman Burns was involved with Ford and Kissinger in designing these proposals. Burns endorsed the idea in Congressional testimony.[275] Reacting to this testimony the following day, Friedman added to his considerable stockpile of criticisms of Arthur Burns by taking issue with the idea of a consumer cartel. "He [Burns] recommended an agreement among oil-consuming nations to try to drive down the international price of oil. I don't believe producer cartels hold up, [and] I don't think consumer cartels hold up. I think that would be pure window dressing, that the market price of oil is, in any event, going to come down." (Instructional Dynamics Economics Cassette Tape 151, August 7, 1974.

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[274] From his remarks at a meeting of G5 officials at Camp David, September 28, 1974, in Galpern (2012, pp. 32, 42).
[275] See Burns' testimony of August 6, 1974, in the Joint Economic Committee, U.S. Congress (1974b, p. 291).

By the time of Burns' public endorsement of it, Kissinger had already been pushing the idea of consumer solidarity for several months. A number of the reservations that Friedman would articulate about Kissinger in response to this stance were anticipated, in general terms, by newspaper columnist Michael Padev, who commented after Kissinger, in the early post-embargo period, had delivered a speech on the world economy, including the oil crisis, to the UN General Assembly. "Dr. Kissinger has often jokingly told newsmen that he is not 'the world's greatest economic expert,'" Padev remarked. "His UN speech proves him absolutely right. It is a real pity that a man of superb intelligence and such penetrating understanding of international political problems should talk such nonsense about the world's economy—and the methods to improve it." (*Arizona Republic* (Phoenix), April 18, 1974.) As will be seen, about eleven months later Friedman would articulate sentiments concerning Kissinger very similar to those voiced by Padev.

The campaign for a joint consumer-country position to be established as a stepping stone to a meeting between consuming and producing countries was a major activity in which Kissinger engaged through mid-1975. His public interventions on the matter included another UN address on September 23, 1974, in which he highlighted his vision of oil prices as something to be negotiated: "both producers and consumers have legitimate claims," he observed, in calling for a "new understanding between consumers and producers."[276] Kissinger gave another speech on the matter on November 14, 1974, this time delivered in the city of Chicago to the University of Chicago Board of Trustees, and used it to outline a "Blueprint for Consumer Cooperation."[277]

In a summary of his University of Chicago speech that he sent to U.S. ambassadors, Kissinger indicated that he would make the case that "cooperation among the major oil-consuming areas… is the fundamental prerequisite for an effective program of action."[278] Again, this perspective went against Friedman's longtime position that the market system provided a means of cooperation between agents, including across countries, and that an explicit governmental program of action was not needed to generate such cooperation.

The element of Kissinger's energy policy initiatives that would give rise to a major backlash, on the part of Friedman and others, against the Secretary of State would be his willingness to contemplate, and even help enforce, high oil prices as part of a negotiated settlement with OPEC.

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[276] Galpern (2012, p. 28).
[277] Galpern (2012, p. 74).
[278] From Kissinger's telegram of November 14, 1974, in Galpern (2012, p. 76).

During 1974, Kissinger claimed to be interested in substantial oil price reduction. This was also the stance that he took in internal government discussions. For example, Kissinger suggested at an Oval Office meeting with President Ford on August 17, 1974 that "we have to find a way to break the cartel."[279] Likewise, Kissinger's November 1974 telegram to ambassadors explained his aim as being to "create the objective conditions for an eventual price reduction."[280] But his actual proposals indicated how much he was willing to compromise on this matter. Seeing the main problem as the danger of another embargo rather than high oil prices *per se*, and perceiving the pricing process as a matter of negotiation, he felt that OPEC would, in exchange for making guarantees on future supply, would have to be provided with a major concession: the consumer countries' explicit acceptance that much of the 1973−1974 oil price rise would not be reversed.

Furthermore, even if the world oil price were now to fall substantially, Kissinger wanted some of that decline prevented from reaching retail gasoline prices in the consuming countries. This wish arose from the fact that a prominent part of his proposals involved the consumer-country governments following policies aimed at promoting oil substitutes such as synthetic fuels. The development of oil substitutes, Kissinger believed, required keeping the petroleum price at home high enough to make them economically viable. "We do need some price protection," he insisted—meaning protection from eventualities in which domestic petroleum prices in the consumer countries fell by more than a specified amount.[281]

In effect, Kissinger was proposing an oil price stabilization scheme, possibly at quite a high price after tax. With respect to an ultimate agreement with producers, Kissinger had, by December 5, 1974, become amenable to arranging a constant world real oil price if the price was acceptable (Galpern, 2012, p. 86), and with respect to consumer countries' policies about prices at home, Kissinger stated, in a meeting on January 30, 1975, that "we could have agreement on a common overall price" of domestic price of oil across consumer countries.[282] Going public, in the speech on the oil crisis that Kissinger gave on February 3, 1975, he indicated that he did not want to return to a world of "[u]nconstrained consumption of cheap oil" and so, while he wanted oil prices down from their current "excessive" level, he also wished the consuming nations to put a

[279] Galpern (2012, p. 12).
[280] Galpern (2012, p. 76).
[281] From Kissinger's remarks at a meeting with U.S. Treasury officials on December 9, 1974, at the Secretary of State's office (Galpern, 2012, p. 88).
[282] From Kissinger's remarks in a meeting that he and Ford had on January 30, 1975, with U.K. prime minister Harold Wilson and U.K. foreign secretary, James Callaghan (Galpern, 2012, p. 126).

floor under the price of oil in their countries.[283] It would be a matter of further study to determine "the level at which prices should be protected," but it had to be "high enough to encourage the long-range development of alternative energy sources.[284] This would be part of an "eventual consumer-producer agreement."[285]

After this feature of the Kissinger energy plan was unveiled, members of Congress would label the plan "the Administration' Oil Floor Price Proposal" (Joint Economic Committee, U.S. Congress, 1975h).

It was against this background that Friedman made an intervention on the matter, in his *Newsweek* column of March 31, 1975. "Some people have a talent for economics; some, though highly gifted in other respects do not," he noted by way of introduction. Friedman then relayed his conclusion that, on the basis of "Henry Kissinger's pronouncements on economic matters," the Secretary of State fell into the latter category of individual. Friedman zeroed in on the consumer-cartel/floor-price proposal: if it succeeded, it would make the 1973–1974 oil price increase permanent by cutting off many of the demand and (non-OPEC) supply forces making for a decline in the oil price. Already resentful of U.S. government impediments to domestic oil price adjustment, Friedman objected to the Kissinger effort to move the oil market even further away from free-market conditions. As indicated earlier in this chapter, Friedman believed that, left to themselves, these forces would break OPEC and put the oil price below the pre-1973 price. The consumer cartel would probably not be assembled, Friedman judged, but if it did, the United States might be left "holding the bag" with the responsibility of enforcing the floor price when market forces were tending to drive the oil price below that floor. Friedman concluded the column: "Henry, do stick to politics." (*Newsweek*, March 31, 1975.)

By the time of Friedman's piece, Kissinger had also been criticized by Paul Samuelson in his own *Newsweek* column (March 24, 1975): "Recently I made headlines when I referred contemptuously to Henry Kissinger, 'boy economist,' with his new frenzy to guarantee permanently the high price of oil. The charge stands." The headlines to which Samuelson referred stemmed from a February 12 appearance in Gainsville, Florida, in which Samuelson reacted to Kissinger's floor-price speech of earlier in the month. "I think Kissinger should stay

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[283] In Joint Economic Committee, U.S. Congress (1975h, p. 70). Kissinger had stated bluntly in a meeting with Ford and French government officials on December 15, 1974: "We never thought that consumer solidarity would produce lower prices." (In Galpern, 2012, p. 95.)
[284] Joint Economic Committee, U.S. Congress (1975h, p. 72).
[285] Joint Economic Committee, U.S. Congress (1975h, p. 72).

in power politics," Samuelson had remarked on that occasion. "… Some people are tone-deaf to economics. Kissinger is not cut out to understand economics. He's promising us [via his proposals] a high price of energy for a long time to come." (*The Star-Press* (Muncie, Indiana), February 13, 1975.)

The Friedman and Samuelson remarks in February-March 1975 were notable not only for their common ground regarding Kissinger's lack of economic expertise but also for Samuelson's implication that oil prices would likely come down substantially, provided that Kissinger's plan was *not* adopted. That they both took this stance reflected the prevalence in economic commentary in the first half of 1975 of the belief (a belief that, as discussed earlier in this chapter, would turn out to be confounded) that the oil price might soon fall considerably from its post-shock value—even though most analysts did not see the price as likely to decline by as much as Friedman was predicting.

The high point of Kissinger's economic interventions was in May 1975, when he gave three speeches on world economic policy—one on May 13 in Kansas City, one in Paris on May 27 to the International Energy Agency (the multi-government body intended to respond to the oil shock), and a May 28 speech, also in Paris, to the OECD (*Omaha World-Herald* (Nebraska), June 3, 1975). Despite this continuing public activity in favor of the consumer-cartel idea, the administration's proposals in this area were already floundering. Part of the problem was internal. Notwithstanding the support that they had from Burns and Kissinger, there was, in fact, high-level disagreement within the administration about forming a consumer-cartel bloc, Secretary of the Treasury William Simon was known to have objections to the notion that were similar to Friedman's, with Simon reportedly critical of the idea of negotiating prices with OPEC rather than letting the market system operate (*Omaha World-Herald* (Nebraska), June 3, 1975).

But the idea was also failing to generate wide support in U.S. public opinion and among America's allies. Shortly after Simon's testimony in March 1975, the Ford Administration had tried and failed to advance the cartel idea through international forums and was judged as having been unsuccessful in what was described as its efforts to "lay the groundwork for a grand meeting of oil producers and consumers" (*The Courier-Journal* (Louisville, Kentucky), September 2, 1975). Specifically, attempts in May to set up a conference of oil producers and consumers had broken down (*The Courier-Journal* (Louisville, Kentucky), June 3, 1975). No

consumer-producer conference materialized in later months, either.[286] A Department of State telegram sent to Kissinger on October 23, 1975, in effect acknowledged the proposals' declining momentum: "fewer industrial countries now think that the solution can be found in negotiations with the producers than did six months ago."[287]

Along the way, the concessions that the administration had made in its attempts to attract greater support from other oil-consumer governments to its proposals were telling. At a meeting with President Ford and the Secretary of State on January 30, 1975, Prime Minister Harold Wilson had suggested to Kissinger that there might be a case for a general program of stabilizing world commodity prices, not just oil. At that time, Kissinger had replied negatively.[288] By September 1975, however, the administration was indicating its willingness to contemplate broad-based stabilization of global commodity prices (*The Courier-Journal* (Louisville, Kentucky), September 2, 1975). This move amounted to serious contemplation, at the U.S. official level, of the kinds of schemes to which Friedman had voiced opposition in U.K. debates on the matter in the mid-1950s.

Despite the absence of a real groundswell of support for a consumer cartel, the U.S. and other governments continued to go through the motions and produced an agreement at a conference held on January 30–31, 1976, in Paris of the 18-government International Energy Agency on a consumer bargaining position (Sobel, 1977, p. 30). But this energy conference's decisions lacked substance for reasons similar to those that Friedman had cited in downplaying the importance of the Jamaica exchange-rate conference held earlier in the month. The Jamaica conference had ratified a system (floating exchange rates) that the authorities were largely compelled to accept anyway, while the Paris conference gave the consuming countries a supposed bargaining stance (a $7 oil price) for a price-setting dialogue with the OPEC cartel that OPEC—which was for the moment secure in its ability to enforce a much higher world price—had no interest in commencing.

[286] At a late stage in this process, Paul Samuelson returned to the Kissinger plan by describing it in his audio commentaries: "You fight oligopoly by oligopsony. A monopoly of a few sellers is to be checked by the countervailing power of the monopoly of a few buyers. And so Dr. Kissinger is to get together with the Japanese and the Germans and the French and form a tight little buying cartel. Then… there's [planned] to be hard horse-trading and dickering at some international conference." (Instructional Dynamics Economics Cassette (Paul Samuelson series), Tape 187, [late] September 1975.) One deviation between the actual 1974–1975 negotiations and this characterization was that Kissinger had not, in fact, been able to get France to agree to his scheme, and France declined to become a member of the International Energy Agency.
[287] In Galpern (2012, p. 296).
[288] See Galpern (2012, pp. 126–127).

By this point, Kissinger's economic interventions had receded. He would later remark about his areas of expertise: "I have never claimed any great confidence in economics." (*The Money Programme*, BBC2, February 3, 1980 p. 3 of transcript.)

Promotion of new alternatives to oil

As noted, part of the rationale for the oil price floor in the package pushed by Kissinger in 1974 and 1975 was the intention to guarantee an ongoing price environment conducive to the promotion of new, non-oil, sources of energy. A related Ford Administration proposal was to create a new Reconstruction Finance Corporation (RFC) that would invest government funds in firms that were attempting to develop alternatives to oil. Nelson Rockefeller, vice president in the administration, was a proponent of this idea and, having failed to obtain Congressional acceptance of it while in office, tried to revive interest in it after he left office, by testifying in favor of it early in the tenure of the Carter Administration.[289]

In contrast, Friedman rejected the notion that government seed funding was needed to get the private sector involved in energy substitutes on the scale needed: provided that the oil price was decontrolled and the revenues associated with these prices were allowed to accrue to the private sector, he saw the market system as capable of generating adequate incentive to expand energy supply on a large scale. Friedman was also specifically opposed to reestablishing an RFC. He had acknowledged previously that the original RFC had proved useful in the 1930s as a means of official recapitalization of the banking sector. In the environment of the 1970s, however, Friedman was concerned that a revived RFC might lead to an extensive government ownership stake in the nonfinancial U.S. business sector, especially if the nationwide general price controls (whose return Friedman believed was a strong possibility—see Chapter 5 above) put many major firms in a precarious financial state.[290]

Friedman's growing disaffection with détente

The decline in prestige of the U.S. presidency that the Watergate scandal generated and the public backlash against President Ford after his September 1974 pardon of Richard Nixon helped produce a leadership vacuum that further raised Henry Kissinger's stature. The Secretary of State's high standing during the Ford years did go through some fits and starts: in October 1974,

[289] See his testimony of September 13, 1977, in Committee on Finance, U.S. Senate (1977).
[290] See his remarks in Friedman and Kristol (1976, p. 16).

for example, Friedman noted that Kissinger seemed to be losing his status as a media favorite (Instructional Dynamics Economic Cassette Tape 157, November 6, 1974), while in the same vein, in early 1975, Paul Samuelson observed that Arthur Burns seemed to have displaced Kissinger in achieving plaudits for statecraft—a turn of events that, Samuelson reported, had left the Secretary of State unhappy (*Newsweek*, March 3, 1975).[291] But, riding out these fluctuations, throughout the whole of the Ford presidency Kissinger retained his status as a leading national and international figure.

Friedman, however, was developing more reservations over these same years about the Kissinger approach to global international relations. From his starting point of having a high regard for Kissinger's stewardship of U.S. foreign policy, Friedman had greater doubts about it after March 1975, specifically in the area of East-West geopolitics. In this period, he no longer primarily limited his reservations regarding the Secretary of State, as he had in his March 1975 column, to Kissinger's grasp of economic analysis.

One key step along the way to Friedman's doubts about the Ford-Kissinger foreign policy was the fall of Saigon in April 1975—in effect, the confirmation of Communist victory in Vietnam, decisively superseding the apparent peace settlement of 1973. Although Kissinger blamed Congress, in withholding funds and authorizations, for a large part of the failure of the 1973 settlement to hold, Friedman made clear that he did not exclude the administration from culpability. Friedman was visiting the Pacific at the time of the fall of Saigon, and he came back to his own country gloomy about its international role: "When Britain gave up its task as a guardian of the world's peace, the U.S. took up that task," Friedman remarked on May 2, 1975, in a television commentary. "We are now giving up that task. Who can, or will, replace us?"[292] Friedman expressed the same sentiment in his audio commentary series during this period (Instructional Dynamics, Economics Cassette Tape 167, early May 1975).[293] In the same commentary, he concluded: "I believe, therefore, that the next five or ten years are going to be very dangerous years, not only in the Far East, but throughout much of the world."

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[291] Burns' rising prestige showed that Friedman's critique remained a minority view. In particular, cost-push views of inflation remained prevalent and, despite intensified debate in the media and Congressional circles on the matter, the general public remained disinclined to blame high U.S. inflation on Burns' prior actions. As Burns himself acknowledged shortly before the beginning of the Ford Administration (see his testimony of August 6, 1974, in Joint Economic Committee, U.S. Congress, 1974b, p. 281), the subject of monetary policy remained largely a mystery to the public.

[292] *CBS Morning News*, May 2, 1975, p. 13 of transcript.

[293] Friedman made a similar remark in early 1977, just after the end of the Ford Administration. See Friedman (1978b, p. 10).

The strategic nuclear balance

As these comments indicated, Friedman's concerns about the growing strength of the USSR went beyond the case of Vietnam or the future of the Asia-Pacific region and encompassed the United States' overall place in the general world power balance. By the start of the second half of the 1970s, Friedman was becoming skeptical about the policy of détente with the Soviet Union that had been launched by Nixon and Kissinger. In this rethinking, Friedman largely lined himself up with defense and foreign policy hardliners in the United States who were critical of the Kissinger approach to U.S.-USSR diplomacy.

The detente policy had much mainstream support in the United States, and a longtime critic of the Nixon presidency, Paul Samuelson, had praised détente in 1973 as an outstanding achievement of the administration (*Newsweek*, November 26, 1973). But Friedman, although not an outright critic of the administration's foreign policy through 1973, had a more reserved judgment than Samuelson on the policy moves associated with detente. As already noted, he was concerned from the start of the 1970s about the possibility of a precipitate decline in U.S. military spending, and on television in 1972 he had already spoken of the need to develop a consensus in favor of a "rebuilding of defense" after the Vietnam War commitments were wound down.[294] Furthermore, the possibility of World War III, while remote, was sufficiently prominent in Friedman's mind even in 1972 for him to mention it in an interview as one scenario (*Vision* (London), 1972, p. 44).

Friedman was also inclined to be wary about political agreements with Communist countries—one example of which was the Federal Republic of Germany's dealings in the early and mid-1970s with East Germany through its Ostpolitik policy, which Friedman judged to an initiative that "opens up all sorts of possible dangers" (Instructional Dynamics Economics Cassette Tape 117, March 14, 1973).

Over the period from 1972 to 1975, Friedman's exposure to defense hardliners who were critical of Kissinger also increased. This category of critics of the administration, as one observer put it, "accused [Kissinger] of being too easy on the Soviets" (*Omaha World-Herald* (Nebraska), August 19, 1975), particularly in the area of arms negotiations. Some of these hardliners were

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[294] *Firing Line*, PBS, January 5, 1972, p. 10 of transcript. In addition, in Instructional Dynamics Economics Cassette Tape 100 (May 31, 1972), Friedman expressed concern that, currently, it was likely that "the public reaction to any further military expenditures is negative."

longtime Republicans or Republican supporters. In this category, there were two, in particular, with whom Friedman was good friends. One of them, noted earlier in this chapter, was U.S. conservative columnist-publisher William Buckley, and another was Buckley's brother James, who in his capacity as a U.S. Senator from New York had opposed the Strategic Arms Limitation Treaty (SALT) agreement with the USSR that the Nixon Administration negotiated and that Congress approved 1972.

During 1972, Friedman also had occasion to have a dialogue with many neoconservatives, who were primarily Democrats or former Democrats.[295] The neoconservative movement, in common with longer-standing conservatives like the Buckley brothers, was very critical of the détente policies pursued by Kissinger, Nixon, and Ford. Conservatives and neoconservatives were, consequently, considerably aligned on matters of defense and foreign policy.

Friedman's growing reservations about Kissinger's superpower diplomacy had by 1976 led him to voice doubts about U.S. foreign and military policy in his *Newsweek* column. In what likely reflected, in part, his dialogues with neoconservatives, Friedman observed in his column that "[m]any experts believe that the Soviet Union is already militarily stronger than the U.S." (*Newsweek*, February 9, 1976.) A few months later, in sharp contrast to Kissinger's reference in 1973 to the Soviets as "our former adversaries" (*Time* magazine, July 30, 1973), Friedman bluntly referred to them as "our Communist enemies" (*Newsweek*, May 3, 1976). Friedman viewed the regional interventions made by the Soviet Union as part of a process that he described in the spring of 1976 as one in which the USSR was testing President Ford's resolve on the world stage (Feldberg, Jowell, and Mulholland, 1976, p. 44–45).

In the ongoing competition, a key issue beyond the matter of which countries had come into the Soviet orbit was that of the relative strength of the United States and the USSR as military superpowers. In this connection, a central part of the hardliner critique of Kissinger was that the steps that he and his subordinates took in their various negotiations with the USSR, especially those in the forum of the Strategic Arms Limitation Treaty (SALT) talks, were essentially confirming, and threatening to perpetuate, the United States' loss of clear-cut strategic (that is, intercontinental-weapon) nuclear superiority by conceding a numerical lead in certain aggregates of weapons, including missile numbers, to the Soviet Union.

[295] The occasion that led Friedman to his dialogue was soliciting comments on his paper "Capitalism and the Jews." Friedman later recalled that the comments on this paper had come from those who became "among the most prominent neoconservatives." (From his August 1982 remarks in Elzinga, 1985, p. 459.)

In this respect, it is important to note that, even though some of the neoconservatives (such as Eugene Rostow) had been officials in the 1960s Democratic administrations, the critique made by defense hardliners of the détente policy was one that encompassed 1960s and 1970s defense procurement and foreign policy decisions and did not pertain solely to the policy actions initiated by Kissinger. Notwithstanding the change in U.S. administration that occurred in 1969, the U.S. officialdom, and the defense and foreign policy circles generally, that, as discussed presently, underestimated the Soviet Union's nuclear buildup in the mid-1960s overlapped considerably—both in personnel and thinking—with the officialdom that negotiated arms-control agreements during (and after) the 1969−1977 Kissinger years in foreign policy. Correspondingly, both the 1960s Democratic administrations and the successor Republican administrations were accused by the hardline critics of underestimating the USSR's nuclear ambitions.

In particular, an issue that outside critics of the 1970s administrations stressed and on which they believed the 1970s administrations were too complacent—the loss of the U.S. nuclear-missile lead—had occurred during the 1960s. The Kennedy Administration had engaged in a major nuclear-missile construction program in the early 1960s in response to a perceived deficit in U.S. intercontinental missile numbers compared with the USSR fleet. But the near-simultaneous advent of satellite technology in the early 1960s would establish definitively that the USSR did not have large strategic nuclear missile numbers after all. And with the very sizable U.S. missile buildup already in motion, a period followed in which there was, as Lee (1977, p. 29) put it, a "widespread belief that the Soviets had opted out of the intercontinental missile competition" and the United States, not the Soviet Union, was regarded as poised to acquire a permanent nuclear lead.[296] Indeed, in a remark that would be continually hurled back at him in the 1970s and 1980s, Robert McNamara, Secretary of Defense in the Kennedy and Johnson Administrations, stated in 1965 that the Soviets "have decided that they lost the quantitative race and they are not seeking to engage us in that contest. It means there is no indication that the Soviets are seeking to develop a strategic nuclear force as large as ours…"[297]

Friedman himself had already expressed doubts during the 1960s about McNamara's record regarding choices on weapons procurement (Instructional Dynamics Economics Cassette Tape 10, January 1969). These doubts would have been reinforced when, as seems likely, Friedman became familiar with some of the many 1970s critiques penned by hardliners of McNamara's

[296] Similarly, Wohlstetter (1977, p. 133) remarked: "It was common in and out of government through the mid-1960s to hold that the Soviets… would not try to catch up."
[297] Among the many quotations of this statement was that given by Graham (1979, p. 75).

projection of Soviet missile strength. For, in fact, contrary to McNamara's postulate about likely Soviet behavior, the USSR not only did endeavor to catch up in missile numbers—it also did, in fact, do so, by engaging in a large-scale missile buildup over the middle and late 1960s. Holloway (1970, p 11) observed: "The pace and scale of this deployment surprised many observers who had thought in 1964 that the Soviet Union might be prepared to accept a position of strategic inferiority rather than incur the enormous costs of such a buildup." Contrary to initial administration expectations, the USSR built up its land-based strategic nuclear fleet of intercontinental ballistic missiles (ICBMs) during the 1960s on a scale that matched the American deployment. Ultimately, in fact, the Soviet Union's land-based missile silos were in excess of the U.S. total: by 1971, the Soviet Union had deployed over 1500 land-based intercontinental ballistic missiles (ICBMs), surpassing the size of the U.S. fleet, which had leveled off in the late 1960s at 1,054 missiles (Miller, 1982, p. 292; Zaloga, 2002, p. 241).

A parallel with the Keynesian-monetarist debate

In light especially of this development, and U.S. policymakers' acquiescence to it in the SALT agreement of 1972, the U.S. government perspective on USSR strategic power and how American diplomatic and defense policies should respond to it were subject to withering criticism during the 1970s on the part of hardliners outside the administration. The parallel between this critique of U.S. national defense policy that a group of individuals outside the administration made and the monetarist critique of U.S. economic policy in the 1960s and 1970s was not lost on Lawrence Freedman (1982, p. 41), who observed that "arms controllers appear as the Keynesians of strategic studies, accused of misguided idealism… In the dock some plead guilty and promise that they are now reformed characters… while others merely look crestfallen[,] wondering where it all went wrong."

The parallel is reinforced by the fact that, as already stressed, defense hardliners' criticism of Kissinger in the 1970s on the subject of the U.S.-USSR strategic competition focused largely on elements of commonality between U.S. government thinking on superpower relations between the 1960s and the 1970s.[298] In a parallel vein, monetarists applied similar criticisms to the macroeconomic policies of the Kennedy-Johnson administrations and to the 1971−1974, post-U-turn, Nixon Administration as well as to the Federal Reserve of both eras. And Friedman and other monetarists found plenty of fault with inflation policy in the Ford era, too, because,

[298] As noted, there was commonality despite the fact that the Democratic administration of 1961−1969 was one to which Kissinger was opposed in the presidential campaigns of 1964 and 1968.

notwithstanding the presence of such Friedman admirers as Alan Greenspan and William Simon in senior economic positions, nonmonetary interpretations of, and approaches to, inflation continued to emanate from the president and various other administration officials over 1974–1977, as was discussed in Chapters 3 and 5. Crucially, too, the nonmonetary view of inflation was expounded throughout the Ford years by the Federal Reserve leadership, thanks in large part to the continuation of Arthur Burns as the central bank's head.

In pursuing further this parallel between monetarists' criticism of U.S. economic policy and hardliners' criticism of U.S. foreign policy, it is straightforward to discern a very specific similarity between the two debates: that between the U.S. intelligence authorities' protracted underestimation of the USSR's missile buildup in the 1960s and 1970s and the U.S. economic authorities' long-lasting overestimation of potential output (and underestimation of excess demand) in the same decades. Even the terms in which these errors were described—although taking place in different social-science literatures—exhibited similarities in wording.

Simple juxtapositions of quotations from the two literatures—those of monetary economics and of strategic or defense studies—bear this out. For example, in the defense literature, Wohlstetter (1975, pp. 170, 179) observed: "For over a decade, beginning in the early 1960s, we [in the United States] systematically expected the Soviets to deploy fewer offense vehicles than they did… [W]e systematically underestimated the Soviet buildup." In the economic-research literature, Orphanides, Porter, Reifschneider, Tetlow, and Finan (2000, p. 119): "the output gap as measured recently lies almost uniformly above the contemporaneous estimates: The real-time estimates of potential output over this period were systematically overly optimistic." Again, in the defense literature, Luttwak (1985, p. 215) referred to "the systematic underestimation of the Soviet ICBM force from the mid-1960s onward, and Daniel O. Graham observed in mid-1975: "during the time period our projections were consistently underestimations of the Soviet effort."[299] In the economic-research literature, Woodford (2003, p. 93) noted the "systematic overestimation" of potential output in the 1960s and 1970s. A final comparison: In the defense literature, Wohlstetter (1977, pp. 124, 127) stated: "Our longer-term predictions about the Soviet strategic triad were under the mark for eleven years… [A] study of differences between predicted and actual number of [USSR] silos suggests both underestimation and *increasing* underestimation." Correspondingly, Orphanides (2000, p. 14) observed: "Comparing the real-time and original series on the output gap reveals systematic one-sided measurement errors."

[299] Testimony of July 21, 1975, in Joint Economic Committee, U.S. Congress (1975h, p. 97).

Orphanides (2000, p. 14) added that "error in the measurement of the output gap, although already substantial at the beginning of the sample in 1965, worsened significantly during the early and mid-1970s before gradually improving later on."[300] This is one respect in which the parallel between the two branches of systematic errors (in USSR missile buildups, on the one hand, and in U.S. output gaps, on the other) breaks down. Whereas U.S. government misestimates of potential output built up further after the early 1970s, the corresponding official misestimates of the Soviet missile fleet came to an end with the SALT agreement of 1972, under which both the United States and the Soviet Union agreed to numerical limits on their missile deployments. The particular agreement was for each side to hold their missile deployment at the levels reached in 1972. This was, as chief U.S. SALT negotiator Gerard Smith would recall, "the so-called freeze of 1972."[301] To the economics profession, the "freeze" of the early 1970s referred to either the Nixon wage-and-price freeze of August 1971 or to the attempted price freeze of 1973. To those in the strategic world, however, the freeze referred to the missile-number caps imposed on the superpowers by the 1972 SALT agreement.

Being a freeze, the SALT agreement that Kissinger and his team negotiated was a ratification of the existing situation, and so it confirmed the USSR's lead in missile numbers. The agreement set a ceiling on each sides' deployed ICBM fleet—of 1618 missiles for the USSR and of 1054 for the United States—and imposed maxima of submarine-launched ballistic missiles of 950 for the Soviet Union and 710 for the United States, along with other conditions (Committee on Foreign Relations, U.S. Senate, 1979c, p. 106; Miller, 1982, pp. 177−179). Friedman's off-the-cuff reaction to this corresponded to a common response in the United States to the headline numbers: they seemed "on the surface to permit Russia a superiority in both land-based and submarine-based missiles" (Instructional Dynamics Economics Cassette Tape 100, May 31, 1972).

Advocates of the SALT agreement could point out that the United States had a heavy lead in strategic aircraft (nuclear bombers capable of intercontinental delivery), which were not covered by the 1972 restrictions. They could also highlight the fact that the United States had a wide lead in technology as well as a large numerical advantage in the number of missile warheads. But concern remained that the technological and warhead leads might prove ephemeral. In the event, the number of warheads in the USSR arsenal did eventually largely catch up with the United States' total, even when bombers were included in the comparisons (see, for example, *Financial*

[300] See also Orphanides (2003, p. 644).
[301] Testimony of July 16, 1979, in Committee on Foreign Relations, U.S. Senate (1979b, p. 29).

Times (London), November 1, 1985; *Durham Morning Herald* (South Carolina), November 14, 1985). In technology, however, it continued to lag badly. Friedman's longstanding position that the Soviet system was not well positioned to adapt and innovate proved correct.

Hardliners' negative verdict on the details of the administration's arms agreements in the 1970s was accompanied by a more general critique of doctrine underlying policy and the attributions—in the hardliners' view, mistaken and overly benign—it made to Soviet military ambitions. Secretary of Defense McNamara had in the mid-1960s adopted an assured-destruction nuclear strategy under which nuclear war would be deterred by the knowledge on both sides that neither could win a war—rather, it would lead to unacceptable damage to both combatants (see, for example, Ball, 1980, pp. 179, 201; Freedman, 1981, pp. 245–248).

This strategy, expounded by McNamara and widely viewed as also underlying Kissinger's negotiations, was one in which both sides were assumed to subscribe to a doctrine of mutual destruction or mutual deterrence. That is, both sides were ascribed the view that nuclear missile buildups were not made with the aim of maintaining a state of stalemate between the two sides—and not of improving their own side's chances of military victory. Despite some heterogeneity of views within the U.S. government over the 1970s, this doctrine remained that governing U.S. arms negotiations in the 1970s. In contrast, hardliners questioned both whether the USSR should accept the mutual-destruction doctrine and whether it was wise for the United States to use it in its own strategy. Doubts about the mutual-destruction doctrine were expounded not only by neoconservatives but also by some other defense specialists—including Friedman's former University of Chicago departmental colleague, Martin Bailey (1972).

This is not a dispute on which Friedman dwelled during his years at the University of Chicago. Indeed, he had himself in the early 1960s taken for granted that nuclear war with the Soviet Union was synonymous with the destruction of the United States.[302] In the 1980s, however, now stationed at a body—the Hoover Institution—that had many foreign policy and defense experts of the hardline persuasion, Friedman became more interested in, and somewhat sympathetic toward, notions that nuclear war was something for which preparation was needed by defense planners rather than an event that would quickly lead to global destruction.[303]

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[302] See Friedman (1962a, p. 201).
[303] See Chapter 15 below.

Splits with neoconservatives on economics

Neither in his late University of Chicago years nor in his time from 1977 at the Hoover Institution did Friedman did not fall into line with neoconservatives and other defense hardliners on all their assessments of the Soviet Union and its competition with the United States. Indeed, he made clear that, in important respects, those critics shared the fault he attributed to their frequent target, Henry Kissinger, of being poor on economics. Even in August 1982, by which time many neoconservatives' positions on economics had moved considerably toward his own, Friedman still maintained: "I do not regard the neoconservatives as defenders of free markets. We must be very careful to distinguish [views on] foreign policy aspects from [those on] domestic policy aspects."[304]

These 1982 remarks were in part informed by Friedman's experience in the 1970s, when he had a low estimation of the economic understanding of the prominent neoconservatives. Perhaps most notably, Senator Henry Jackson was a major early neoconservative politician. Friedman's disdainful verdict on Jackson's economic views was analyzed in the previous chapter.

Friedman had even less occasion to be impressed by an economic proposition offered by the fringe of hardliners. Notably, a book-length critique of Kissinger coauthored by conservative activist Phyllis Schlafly suggested that the 1972−1974 commodity price explosion (and so, went the logic of these critics, the breakout in U.S. inflation after 1972) reflected the successful execution of a Soviet Union plan, which its improved strategic arsenal allegedly allowed it to carry out with impunity (see Schlafly and Ward 1975, pp. 605−607). The implication was that the USSR was masterminding Western economic difficulties, including high inflation, in conjunction with its efforts in negotiations with Kissinger to consolidate military advantages. To Friedman, in contrast, it was misguided to think that world commodity prices could be steered by the Soviet government. And, of course, he in any event regarded it as poor economics to attribute inflation in the United States and its allies to the rise in commodity prices.

A number of other hardliners had more gravitas than Schlafly in public debate but also made economic prescriptions and diagnoses with which Friedman sharply parted company. A call that frequently emanated from hardliners was for trade restrictions to be made a weapon against the USSR. Some hardliners favored a full-scale embargo, while many others sought to make freer

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[304] In Elzinga (1985, p. 55).

U.S.-USSR trade conditional of changes in the Soviet Union's internal or foreign policies. In particular, Senator Jackson was a major force in Congress' decision, discussed above, to link U.S. conferral of MFN on the USSR to specific undertakings by the Soviet Union regarding its own policies outside the area of trade. In contrast, as earlier noted, Friedman favored normal trade relations with the Soviet Union, limiting his opposition to trade to the narrow area of U.S. export of strategic technology.[305]

Friedman also disagreed with numerous defense hardliners on the sources of strength in the U.S. economy. For example, retired U.S. colonel R.D. Heinl Jr. granted (*New Hampshire Sunday News* (Manchester), May 3, 1970) that "the Russians have a gross national product less than half of ours" but qualified this by saying that U.S. GNP "includes such frivolities as the production value of Cadillac tail-fins." Similarly, neoconservative Richard Pipes (1976, p. 21) suggested: "Consumerism… leads to a decline in public spirit and an addiction to comfort that significantly diminishes the state's ability to mobilize the citizenry." In contrast, Friedman regarded the existence of consumer sovereignty in a market economy as a sign of strength. This was especially the case because the associated higher degree of productive efficiency raised total national output and so made any given defense effort a smaller strain on a country's resources.

Warren Nutter's critique of Kissinger

One foreign policy hardliner whose economic analysis Friedman did rate very highly was his first-ever Ph.D. student, Warren Nutter. The two of them had already, in the late 1950s through the mid-1960s, they had been on the same in the public debate on Soviet economic strength: both had been on the—ultimately successful—side of the debate that maintained that the USSR economy lacked dynamism.[306] By 1975, Nutter was more specifically focused on foreign policy issues. Nutter had behind him a spell (1969–1972) as an Assistant Secretary of Defense in the

[305] Some defense hardliners shared Friedman's view that trade with the Soviet Union should be normalized, other than exports specifically of military significance. Notably, former Ambassador to the USSR, Foy Kohler, remarked in August 1977 that "unquestionably it is essential that we do not give them technology that really would step up the military-industrial sector of their economy… I personally would like to see normal trade with the Soviet Union but I see no reason… [for] those massive credits that certainly Brezhnev had in mind and sometimes, at least, Nixon seemed to be voicing [support for].") (In American Bar Association, 1978, p. 10.) Similarly, earlier, in testimony in Committee on Foreign Affairs, U.S. House of Representatives (1974, p. 89), Kohler had stated that he supported USSR trade with the United States on the same terms as, but no better terms than, those applying to other U.S. trading partners. In addition, history professor Walter Laqueur, invited by Senator Henry Jackson to testify before what was essentially an anti-détente Congressional hearing on April 17, 1973, had earlier observed: "I don't think that the West should indirectly subsidize the Soviet defense budget [but] it should have trade with the Soviet Union." (In Committee on Government Operations, U.S. Senate, 1973, p. 21.)
[306] See Nelson (2023) for a detailed discussion.

Nixon Administration, and he regarded military developments both during and after his time in government with alarm. He was critical of U.S. conduct of the Vietnam War—blaming both the administration and Congress for what Nutter considered a sustainable stalemate embedded in the 1973 settlement.

Nutter, like Friedman, had been disaffected with official U.S. policy in this area in part by the change in the situation in Vietnam from negotiated settlement in 1973 to Communist victory in 1975. Nutter saw the lack of sizable Congressional support after 1973 as partly responsible for this outcome: he had been one of those unsuccessfully trying to secure financial support for the anti-Communist forces in the leadup to the fall of Saigon (Committee on Appropriations, U.S. House of Representatives, 1975, pp. 45, 73–74). But, like Friedman, he also blamed Kissinger and the administration for the April 1975 outcome and, in Nutter's case, his disillusionment was magnified by his own having been a member of the first Nixon administration. Nevertheless, in his book on Kissinger, Nutter's focus was not the Vietnam War but instead the overall U.S.-USSR world power balance—including the content of the nuclear agreements that Kissinger had negotiated with the Soviet Union.

In his critique of Kissinger, Nutter (1975, p. 23) passed a negative judgment on the SALT agreement, contending that it allowed "a sizable expansion of the Soviet arsenal." Furthermore, on the basis of a comparison of Kissinger's pre-1969 writings as an academic with the policies he propounded as a member of the Nixon and Ford Administrations, Nutter (1975, p. 12) argued: "Secretary Kissinger hails agreements that Professor Kissinger would have strenuously opposed."[307] Friedman's negative verdict on U.S. foreign policy of the mid-1970s was likely reinforced by the Nutter analysis.

Venturing into foreign policy commentary in ***Newsweek***

Friedman had remarked in early 1972: "I have no competence in the area of international affairs… which is why I've been keeping quiet about it."[308] This was a sensible self-denying ordinance. And, certainly, various statements that Friedman made in later years seemed to reaffirm it. In April 1975, for example, Friedman told a journalist that, because he was "not a

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[307] There had, of course, been previous analyses contrasting Kissinger's position after 1969 and the points of view expressed in his pre-1969 writings, including one in *Commentary* magazine (June 1974).

[308] *Firing Line*, PBS, January 7, 1972, p. 7 of transcript. (The transcript used "confidence" instead of "competence," but the latter word makes more sense in context and would be in line with Friedman's way of speaking.)

political expert," he would bypass the journalist's questions about how a Third World War figured as an eventuality.[309] In a 1976 interview (Rogge, 1976, p. 3), he stated that he did not think it was appropriate to comment publicly regarding topics on which he was not expert. And, in the spring of 1977, Friedman seemed to be conscious of the potential harm that such commentary could do to one's own standing, as he observed that the recent writings of neoconservative Irving Kristol on economics had started to undercut Friedman's confidence in the reliability of Kristol's statements on other topics (Instructional Dynamics Economics Cassette Tape 213, May 11, 1977).

Nevertheless, in 1976, in particular, but also in late 1977, Friedman went against the spirit of these injunctions, by producing commentaries, most notably in *Newsweek*, that dealt specifically with the foreign affairs of the United States. In a kind of parallel to Henry Kissinger's ill-fated ventures in 1974 and 1975 into U.S. economic policy, Friedman made forays into geopolitical punditry. The most sustained series of commentaries appeared in the second half of the spring of 1976. They were poorly judged and confirmed Friedman's status as a novice in the foreign policy field.[310]

In April-May 1976, after making trips in April to the minority-rule states of Rhodesia and South Africa, Friedman discussed the United States' approach to these countries. His various commentaries in this area—appearing in *Newsweek* columns (May 3 and 24, 1976), his audiotape series (Instructional Dynamics Economics Cassette Tape 189, April 1976), and on *CBS Morning News* (April 29, 1976) would do him little credit.

In his cassette commentary, he stressed the degree to which the private enterprise had provided a means by which South Africa's different races had worked together to build the economy—a perspective that definitely underplayed the extent to which South Africa's legal system restricted opportunities of the Black majority in the private sector and public sector alike. In his column on South Africa, Friedman opened by noting his continuing "abhorrence of the apartheid policies" practiced in that country. But he went on to catalogue the similarities between South Africa and the United States. In doing so, Friedman intended this to underline the need for a deeper appreciation of South Africa's situation. But taking this tack in his commentary could also easily have been interpreted as neglecting the great dissimilarity between the two countries that lay in the fact of minority rule in South Africa. In his CBS television commentary, Friedman pushed

[309] See Friedman (1975g, p. 11).
[310] Friedman's columns on South Africa would be sharply criticized by Rayack (1987, pp. 177–178).

the tenuous parallels even further, asserting, "Neither Rhodesia nor South Africa is an ideal democracy, just as we are not."[311]

In putting undemocratic countries like Rhodesia and South Africa on the same footing as the universal-franchise United States, Friedman was putting his own country in a bad light. Parallels between the United States and South Africa or Rhodesia of this kind were more associated with radical critics of the United States. The superficial comparisons that he made underlined the fact that Friedman was better suited to talking about Adam Smith rather than Ian Smith.

The foreign policy scenario that Friedman evoked in his foreign-policy-centered columns on the minority-rule southern African countries was that sanctions might "drive [them] into the arms of the Soviet Union" (*Newsweek*, May 24, 1976). He was particularly fixated on this possibility with regard to South Africa and focused on it not only in a 1976 column but also in a follow-up column, "South African Nightmare" that appeared in the first year of the Carter Administration (November 28, 1977). Angola had become a Soviet Union-dominated government in 1975, and Friedman suggested in the 1976 column that Kissinger had "learned nothing from Vietnam or Angola" (*Newsweek*, May 24, 1976) in the Soviet Union acquiring client states. In contrast to the cases of Vietnam and Angola, in which the USSR acquired satellites in the wake of military conflict, Friedman suggested that economic sanctions imposed by the West might prompt the apartheid government to form a voluntary alliance with the USSR. This far-fetched speculation was an example of what Hirsch and de Marchi (1990, p. 66) perceptively described, in another connection, of times when his appetite for debate "tempted Friedman into areas where he was not an expert." Friedman was now breaking the dictum that one should stick to talking about things that one knew about: he was devoting what was ostensibly an economics column to a foreign policy subject. Friedman was trying his hand at assessing the state of world affairs—and the commentaries that resulted from this attempt were not impressive.[312]

The basic position that Friedman took when discussing southern Africa was in keeping with a general reluctance—voiced previously in his opposition to limitations on U.S.-USSR trade, as discussed above—to use controls on trade, capital, and travel as weapons against countries whose policies the United States opposed. Friedman's basic position on southern Africa was

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[311] *CBS Morning News*, April 29, 1976, p. 7 of transcript.
[312] Friedman's thoughts on South Africa and the USSR were, however, taken sufficiently seriously by some defense specialists for his *Newsweek* column on the subject to be cited in the lead article of the 1977 edition of the prestigious readings collection *American Defense Policy*. See Pfaltzgraff (1977, p. 16).

also not unlike that taken by a variety of critics of the Ford Administration foreign policy on the Republican side, including Ronald Reagan.[313] Like Reagan, Friedman expressed the concern that the regime to which minority rule could give way would not be one practicing pluralistic democracy in the context of a market economy but, instead, a collectivist state. This fear was not altogether unfounded, as the subsequent case of Zimbabwe showed, although it proved far too pessimistic, indeed completely off base, with respect to South Africa.

But the question remains why Friedman felt qualified to write about these issues in the first place. A number of factors likely played a part. First, his very recent trips to southern Africa made him feel inclined to comment, even though his main television and magazine outlets had been given to him on account of his economic expertise and had not been envisioned as a vehicle for general foreign affairs commentary. Second—and a matter closer to his economic expertise—he had a strong dislike of economic sanctions, as already stressed. Third, Friedman likely felt that concern about the threat of world Communism had faded too much in recent years in the United States. He had called Marxists "the most extreme opponents of our system" in the original write-up of his Price Theory lectures in 1951—and he continued to use that description a quarter century later, in the revised edition of his *Price Theory* text.[314]

Some years before his 1976 visit to the area, Friedman had taken a degree of interest in the economics and politics of southern Africa, and he had raised the subject with his Rhodesia-born departmental colleague Stanley Fischer. Fischer found Friedman highly ill-informed on the subject. "Milton didn't know where I was from, and we were walking along. And he was busy expounding to me on South Africa and Rhodesia." Fischer's judgment on what Friedman had to say on this topic was: "Absolute crap." Fischer assessed that, on issues like this, Friedman picked up pieces of information from various sources and then followed an unfortunate practice in which "he chanced his arm on playing facts" by generalizing from the impressionistic and anecdotal evidence to which he had been exposed (Stanley Fischer, interview, August 30, 2013).

This occasion had been one of private conversation, but during the same period of the early 1970s it had also been observed (*Financial Times* (London), September 17, 1970) that Friedman was willing to talk publicly on matters beyond economics. For the most part, however, these interventions had involved the application of economic principles to areas of social policy

[313] See, for example, Reagan's radio commentary dated July 6, 1977, in Skinner, A.G. Anderson, and M. Anderson (2001, pp. 185–186).

[314] See Friedman (1951c, p. 72; 1976b, p. 199).

closely intertwined with economics or had considered aspects of defense or foreign policy that were amenable to economic analysis. His critiques of the draft and of trade sanctions were examples of his cross-disciplinary interventions. In contrast, when Friedman was emboldened by his 1976 visit to southern Africa to talk publicly about that subject, he was really venturing into geopolitics.

This was not his forte. With regard to political, cultural, and constitutional matters outside the United States, Friedman had little experience and, often, only superficial knowledge. Continental Europe was, as discussed in other chapters, one example of the limitations of his knowledge. Africa was another. His commentaries on South Africa exemplified this fact. Friedman's experience in news commentary and his extensive travels probably made him overconfident of his ability to speak authoritatively on world affairs. Instead of being authoritative, however, Friedman's foreign policy commentaries in the mid-1970s provided a demonstration of a statement he had made in another context: "I do not know that I have any insights, but I have opinions."[315]

The Soviet Union's combination of military strength and economic weakness

The reservations that Friedman expressed in the mid-1970s about the East-West military balance were contributions that dated better than his speculations about southern Africa. In talking about the global balance of power, he was correct in suggesting that Soviet military strength was expanding while U.S. defense spending was declining. And he was on the right track in the general point, though not in the specific examples he chose, in implying that the USSR was, during the 1970s, trying to expand its empire, notably in the Third World. The fact that these points, associated with hardliners in the era of détente, became more widely accepted after the mid-1970s in U.S. political discourse was reflected in the trajectory of U.S. military spending, which troughed in 1976 and expanded strongly from 1980 onward under Presidents Carter and Reagan.

An aspect of Friedman's perspective on the Soviet Union that also did him more credit in retrospect was that the fact, for all his concern about the geopolitical advances of the USSR, he repeatedly took heart in the United States' clear-cut economic advantages over the Soviet system. In the late 1950s through the mid-1960s, Friedman and Warren Nutter had been isolated

[315] Testimony of June 21, 1973, in Joint Economic Committee, U.S. Congress (1973, p. 139).

in stressing Soviet economic weakness. By the 1970s, in contrast, that perspective had become widely accepted.[316] The weakness of the level and growth rate of Soviet productivity had become increasingly recognized after the mid-1960s, and the fillip that the oil shock gave to the oil-exporting Soviet Union did not materially reverse the economic fortunes of the USSR or the East bloc.

The bad USSR economy, reinforced by the inflexibility of the Soviet government on domestic economic matters, would prove to be a trump card for the United States in the global competition by the early 1980s. In the intermediate period of the 1970s, however, the USSR's chronic economic problems coexisted with growing Soviet military strength.

Nevertheless, the worst-case scenarios sketched by Friedman, as well as of defense hardliners, in which the USSR became dominant outside Eastern Europe, did not materialize. Soviet dominion in Western Europe or in more of Asia was, in the event, avoided. The avoidance of this outcome in part likely reflected the compensating policy changes in the United States. But it may also have reflected hardliners simply being too dire in their assessments during the 1970s of the shift in the military balance between the USA and the USSR, as well as of the Soviet Union's likelihood of success in acquiring new client states.

The fact that the USSR's internal economic management was producing and guaranteeing the country's underlying weakness was one reason why Friedman was unenthusiastic about the use of economic or financial sanctions as a U.S. government weapon against the USSR or the East bloc. After 1982, the policies of the Reagan Administration largely refrained from such sanctions. On that score, the administration followed an approach that was closer to Friedman's prescriptions than it was to that prescribed by many neoconservatives.[317]

Confirming the intrinsic economic weaknesses associated with a planned economic system, the Soviet bloc would ultimately collapse, even in the absence of the systematic use of sanctions against it. As already noted, Friedman and Warren Nutter had stressed these weaknesses in 1956–1965, challenging the consensus of that period (a consensus later characterized as one in which "the Soviet Union was widely portrayed as 'ten feet tall'").[318] Although they had not claimed that this weakness was inevitably terminal for the bloc, their basic message that the East

[316] Again, see Nelson (2023). See also Chapter 10 below.
[317] See Chapter 15 below.
[318] Schroeder (1991, p. 92).

bloc economies were intractably stagnant became an ever more widely accepted characteristic of the European Communist economies, and particularly of the USSR, in Western analysis during the 1960s and 1970s.

The contrast between Soviet superpower status in the military field and its serious weakness on the dimension of domestic economic performance would be highlighted in the retrospective given by Michael Mussa in 2000, when he noted that "for forty-five years, the Soviet Union maintained effective control over most of central and eastern Europe," only to add: "But, under the stress of economic stagnation… this empire collapsed… and by 1992, the Soviet state split apart into politically independent republics."[319]

MICHAEL MUSSA

By the time he wrote these words, Michael Mussa, as chief economist and director of research at the International Monetary Fund, had been deeply involved for a decade in matters concerning Eastern Europe and Russia, as part of the process of integration of the former East bloc countries into the international economic system and their transition to market economies.[320]

The IMF years were Mussa's second spell working in the official sector. He had previously served as a consultant to, and then a member of, the Council of Economic Advisers for nearly three years of the second Reagan term.[321] These experiences had been preceded by a couple of decades as a graduate student and then teacher-researcher at academic institutions—mostly the University of Chicago.

Although Mussa did not receive his Ph.D. until 1974, his time as an on-campus graduate economics student had been much earlier—September 1966 to June 1971 (see Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1986, p. 62)—a period covering the heyday of increasing attention to Milton Friedman in national and international discussions of monetary policy. Mussa did not have Friedman on his dissertation committee. But his thesis—subsequently developed into book form as Mussa (1976a)—concerned domestic macroeconomic

[319] Mussa (2000, pp. 48–49).
[320] Kose, Prasad, and Terrones (2005, p. 31) noted that Mussa had served "at the Fund during the trying periods of the Asian and Russian crises" of the late 1990s.
[321] Mussa began as a consultant to the CEA in October 1985 (Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1986, p. 64). He then served as a member of the CEA from August 18, 1986, to September 19, 1988 (see *https://obamawhitehouse.archives.gov/administration/eop/cea/about/Former-Members*).

dynamics and cited a good amount of Friedman's work.[322]

A more momentous link between Friedman and Mussa would, however, come a little later, when their careers met at a tangent. Having been teaching at the University of Rochester, Mussa officially joined (as an associate professor) the University of Chicago on January 1, 1977.[323] This was exactly the same day on which Friedman ceased working at the university.

Although Friedman was leaving the university's economics department and Mussa was joining the business school, the synchronicity of Friedman's departure and Mussa's arrival was notable for its symbolism. Mussa's start-of-1977 arrival as a new University of Chicago was a prime example of the generational change underway in monetary economics at the institution. Just as Mussa was arriving, Friedman was exiting, after having been based at the University of Chicago for almost all of Mussa's lifetime: Mussa was born in April 1944, only a couple of years before Friedman assumed the position of associate professor of economics at the university.

Furthermore, as discussed below, Mussa was an important example of an economist whose approach to monetary analysis reflected, to a considerable degree, the heritage of Friedman's work in the area. In particular, in contrast to the flexible-price-based approach to inflation/output dynamics favored by a number of his University of Chicago colleagues in the late 1970s, Mussa advocated a focus on sticky prices that broadly coincided with Friedman's perspective and that anticipated the New Keynesian consensus that would characterize professional views on inflation in later decades.

Culmination of Friedman's years at the University of Chicago

Friedman had already decided by 1974 to leave the University of Chicago in the 1976/1977 year: "In three years Friedman will retire," a profile of him had noted (*Miami Herald*, November 24, 1974, p. 2F). In the event, the actual timing of his departure proved to be closer to two years after these words appeared. Friedman's presence at the university in the 1976/1977 academic

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[322] Specifically, Friedman (1956, 1957a, 1969a, 1970a): see Mussa (1976a, p. 306). The thesis was titled "A Metzleric Model of Macroeconomic Dynamics," and the book was an "extension" of the thesis (Mussa, 1976a, p. vii).
[323] See Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1986, p. 63). His later title at the university was the William H. Abbott professor of international business, Graduate School of Business (Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1984, p. III). He had become a full professor in 1980 (Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1986, p. 75).

year was curtailed by his taking leave in the second half of that year (in order to undertake activities on the West Coast and elsewhere that will be discussed in the next chapter).

The plan to leave the University of Chicago in 1976/1977 roughly corresponded to Friedman reaching the age of 65 (which Friedman would attain on July 31, 1977)—widely regarded at the time as a point at which economics professors usually moved to emeritus status. The Friedmans also, over the years, repeatedly gave a second reason for his departure: stepping away from the grading of exams and term papers.

These cited reasons for Milton Friedman's end as a University of Chicago teacher were, however, somewhat pat. For one thing, if it was really the case that age 65 was a wholly mandatory retirement age, then avoiding grading could not be legitimately cited as another reason for his retirement. In fact, reaching age 65 did not invariably mandate retirement. And as for grading, Friedman had the option of delegating this task, had he so desired.

Having been present when Friedman "expressed himself on the subject [of grading] more than once," Al Harberger's view was that things might have been different if Friedman had really viewed an arrangement not being involved in grading as viable: "if he had been able to continue teaching and not have to grade exams… he might have stayed on longer." (Arnold Harberger, interview, April 12, 2013.) Nevertheless, asked further about Friedman's reasons for leaving, Harberger cited as the central factor the wishes of Rose Friedman.[324] "Oh, I'm sure that the grading wasn't the only thing—though I'd say if you asked him what he was complaining about, it was that this was something that got under his skin a bit. But no, I don't think [it was decisive]. If I had to make a bet, I think Rose had a lot to do with the decision to go to California." (Arnold Harberger, interview, December 9, 2013.)

Rose Friedman's retrospective in 1998 appealed to the usually cited reasons: "We finally moved to San Francisco in 1977—although I cannot claim that it was because of my persuasive power. Milton had reached the age of retirement and was very tired of grading exam papers."[325] But in another 1990s-vintage account, Milton Friedman gave Rose's wishes a more prominent role: "When I reached the age of 65—I was, at that time, living in Chicago and teaching in Chicago—I decided I had graded all the exam papers I was going to grade. My wife grew up in Portland, Oregon, and she was in love with San Francisco. She tried to move us out here many times

[324] This was also the driver of the Friedmans' move cited in a profile in *Rutgers* magazine (Fall 2006, p. 27).
[325] R.D. Friedman (1998, p. 140).

during our life together, but she never succeeded until I decided I was going to retire from active teaching. Fortunately, the Hoover Institution at Stanford University offered me the opportunity to be a [research] fellow…"[326]

Robert Lucas indicated that "later on, I learned that there was some agreement between him and Rose that, after [he turned] 65, they were moving. I think Rose felt she had had enough of Hyde Park." (Robert Lucas, interview, March 12, 2013.) That Rose Friedman wanted to move to San Francisco was undisputed. "I love San Francisco," she would remark. "It's my favorite city." (*Straits Times* (Singapore), October 18, 1980.)

Milton Friedman would joke that, as the Bay Area was a key region of the United States in which public opinion traditionally strongly favored an interventionist public sector, San Francisco was an appropriate place to which to relocate, it being where the battle needed to be fought (*Rutgers* magazine, Fall 2006, p. 27). More seriously, he would cite a major attraction that the 1977 move had for him: "I could continue my research and writing, without doing any teaching."[327]

In this connection, it was especially significant that in December 1976, a few weeks before the relocation proceeded, Friedman indicated that, in the course of holding his University of Chicago position in recent times, he had been finding it harder to attain the tranquility most conducive to the activity of writing (*Chicago Daily News*, December 8, 1976).

There was some anticipation that Friedman's official retirement would be associated with a large step-down in his public profile. That was the expectation, or hope, expressed in a magazine that in the 1970s and 1980s tended to be one of Friedman's detractors: *Business Week*. Its reporter William Wolman wrote (in the edition of January 19, 1976) that "University of Rochester economist Karl Brunner… is likely to emerge as the leader of the monetarist school when Milton Friedman winds down his activities in retirement." Of course, Friedman's busy agenda in the decade after 1976 meant that the winding-down of his activities, as well as the ascendancy of Karl Brunner, that Wolman envisioned did not occur.

Closer to the mark than Wolman was a news item that gave a public airing to Friedman's post-University of Chicago plans. On Election Day 1976, it was announced that, starting in the

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[326] CSPAN, November 20, 1994, p. 10 of transcript.
[327] CSPAN, November 20, 1994, p. 10 of transcript.

following year, Friedman would be Senior Research Fellow at the Hoover Institution, Stanford University—a position discussed in detail in the next chapter. The report (*The Detroit News*, November 3, 1976), after noting Friedman's projected responsibilities at the institution, observed with tongue in cheek: "Other than those assignments, writing his weekly [sic; every three weeks] column for *Newsweek* magazine, and scholarly journal and book writing, he'll be taking it easy."

Friedman could not, however, be affronted at perceptions of himself as being retired, because he would indeed describe his status in these terms—especially in the initial post-1976 period after his University of Chicago activities ended. It would soon become apparent, in the course of his blitz of media and other public-policy activities in the late 1970s, that this characterization had to be taken with a grain of salt. Samuel Brittan (2005, p. 298) instead captured Friedman's post-1976 life reasonably accurately as "a working retirement." But as Robert Lucas (personal communication, November 7, 2013) observed of Friedman: "He defined 'retirement' as never having to grade papers again."

Departure of a celebrity

Although Friedman was not intending to withdraw from economics or from public life, he was, indeed, leaving his traditional location. It was consequently the case that, with the close of 1976, the University of Chicago economics scene was losing its biggest name.

Those familiar with Friedman's schedule knew that, although he had become synonymous with the University of Chicago, he was not there half the time. Friedman had long absences thanks to his spending six months a year at the Friedmans' "Capitaf" second home in Ely, Vermont—what Paul Samuelson had jokingly called Friedman's "Vermont hideout" (*Washington Post*, August 1, 1971). And, over and above these regularly scheduled absences, during the part of the year in which he was nominally a presence on campus Friedman actually had frequent getaways for one-off trips—such as that involving the Wisconsin appearance discussed at the start of the chapter. These excursions were, however, often high-profile and had the effect of reinforcing the association in the public mind between Friedman and the university—a situation exemplified by another short trip, this one to Dallas on October 16, 1975. One of the press pieces on this trip referred to "the University of Chicago's ubiquitous economist, Dr. Milton Friedman," while also noting that "he's become regarded as something of an institution" (*Dallas Morning News*, October 17, 1975). Another wrote of "Dr. Milton Friedman, nationally known economist from the University of Chicago… the bald little man with the big smile" (*Dallas Times-Herald*,

October 17, 1975). As Phil Donahue said to Friedman in one of the latter's appearances on Donahue's Chicago-based, but nationally broadcast, program (*Donahue*, NBC, September 30, 1975), Friedman had become a household name. Correspondingly, an undergraduate monetary-economics textbook that appeared during Friedman's final calendar year at the university observed that "his utterances receive wide circulation in the press."[328]

Friedman's celebrity status therefore made him more closely associated with the university than one might have gleaned from the modest amount of time per year he actually spent on campus. That status also associated him with the university as a whole—a situation Friedman recognized when he made remarks to the press about the University of Chicago as an institution: "The university stands for academic excellence and intellectual independence." (*Chicago Daily News*, December 2, 1975, p. 6.) More specifically, Friedman's high profile made him, to many, the face of the overall economics scene at the University of Chicago, even though he was really a member of only one part of that scene—the economics department—and specialized in a field, monetary economics or business-cycle macroeconomics, different from that pursued by most of the other economic researchers at the university.

Friedman's ubiquity meant that, although he had had no affiliation with the institution—the business school—that Mussa would join, he was popularly associated with it. Charles Nelson, a member of the school from 1969 to 1975, recalled that a newspaper "ran an article about the University of Chicago's School of Business, and how it was up and coming, and, you know, a rising star, *et cetera*. And I think over half the article talked about Friedman, and it never mentioned the fact that Friedman never had any appointment whatsoever with the school. You know, they focused on the superstar, from their point of view. And Friedman certainly had an enormous impact on the whole place—and that umbrella covered the business school, as well." (Charles Nelson, interview, September 9, 2013.)

Friedman also had a definite impact on the undergraduate part of the university. He had not been involved in undergraduate economics teaching in the quarter century after the early 1950s.[329] But

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[328] Thorn (1976, p. 257).

[329] William Dougan (interview, September 19, 2013) recalled, however, that Friedman did teach an undergraduate class in late 1976. Robert Lucas suggested that this may have been a not-for-grade course, possibly open to undergraduates, that he recalled Friedman teaching late in his stay at the university (Robert Lucas, personal communication, November 7, 2013).

The practice under which senior members of the University of Chicago's economics department did not teach undergraduate classes came as a letdown to some of the university's college students, who had expected to take classes taught by the most well-known department members, such as Friedman. "I think there were people who

Friedman was bound to have an impact on the undergraduate aspect of teaching at the university because of the detailed discussion of his work that took place in introductory texts and courses.[330]

Away from the references to his own work was treated, Friedman had by 1976 become interested in how the later nineteenth-century U.S. economy was treated in college and high school classes, including in history courses. He considered the period to have been one of considerable advance and was concerned that the courses, along with the media, had characterized the period as one in which the poor were made worse off (*Milwaukee Journal*, October 30, 1976).

As far as high school teaching of economics was concerned, Friedman's fame was also felt across the country: Gloria Valentine, Friedman's secretary at the University of Chicago in 1972–1976, found that some U.S. high school students, upon receiving class assignments that concerned Friedman's views, would take a shortcut in their research by phoning Friedman's office, asking to speak to him. Valentine, who was puzzled at the students' parents permitting their children to make long-distance phone calls of this kind, would rarely let the calls through, and she recalled reproaching some of the students for the familiarity that they assumed in referring to Friedman as "Milton" (Gloria Valentine, interview, April 1, 2013).

Ahead of his departure, Friedman participated in efforts to create a concrete legacy for himself at the University of Chicago and the economics department specifically. Plans were made for a "Dr. Milton Friedman Fund" to be provided to the department and intended to consist of a starting amount of $1 million. Risking appearing egotistical by soliciting donations to a fund that was named after himself, Friedman participated in events designed to raise the one million dollars, including the aforementioned talk in Dallas to a business audience (*Dallas Morning News*, October 17, 1975). The fund bearing Friedman's name went ahead but was low profile and proved to have a short life—petering out within a few years of his departure.

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dropped out of the major, fairly quickly, when they realized that," Philip Meguire, who was an undergraduate economics student in the mid-1970s, observed (interview, November 18, 2013). For the most part, however, "we accepted it as a reality. And the instructors that we did have were not bad." Gary Becker noted (interview, December 13, 2013) that, "there were separate people, who were not members of the regular department, who taught economics undergrad. We didn't have a very extensive program." With regard to senioe members of the department, I, when I came in, never taught undergraduates, an undergraduate class at Chicago. Friedman never did, Stigler never did… Ted Schultz never did, maybe Gregg Lewis did, I don't remember, [but] many of the people never taught it. So it was very different in those days."

[330] The previous chapter discussed the treatment of Friedman's work in undergraduate economics texts.

The next generation

Friedman nonetheless left a major imprint on the post-1976 world of economic research at the University of Chicago. He achieved this mainly in the same way in which his influence was felt in the economics profession at large: through the significance and legacy of his contributions to economic analysis.

For reasons largely unrelated to 1976/1977 being his last teaching year, Friedman's work and career were the subject of many retrospectives in this period. Milton and Rose Friedman themselves produced some of them. Milton Friedman wrote for the *Journal of Monetary Economics*—a periodical that, as already noted, was launched in early 1975—an article (on Homer Jones) that, in part, reminisced about his undergraduate years and what brought him to the University of Chicago. Rose Friedman wrote a long (ultimately, fourteen-part) series of articles in 1976–1977 on her husband's life and career, as well as their collaboration, in the Japanese English-language periodical *The Oriental Economist*.

Before the *JME* article appeared (in the November 1976 issue) and during the publication of the *Oriental Economist* series, Friedman won the Nobel prize—and this, of course, gave rise to a large quantity of articles dealing with Friedman's career and contributions. Rose Friedman herself inserted an instalment specifically concerned with the Nobel award into her *Oriental Economist* series.

The younger generation of monetary economists would not have been able to read economics journals or economics coverage in the media in 1976 without coming across multiple retrospectives on Friedman's career.

A significant number of prominent members of this generation spent some of the 1970s at the University of Chicago. As already indicated, Michael Mussa was a leading example, joining the University of Chicago at exactly the point at which Friedman left, and having been previously on campus through mid-1971. But the intervening period of Friedman's 1971−1976 closing years at the university had also seen him overlap with many members of the new generation of contributors to monetary economics—a generation born after he started publishing economics articles in 1935. Several of these individuals had left the university in 1975 or earlier, thereby preceding Friedman's own departure: these included Robert Gordon (born 1940), Charles Nelson (born 1942), Stanley Fischer (born 1943), and Robert Barro (born 1944). Of this group, Barro

and Fischer made brief reappearances, following their departures from the university, at Friedman's money workshop, as guest speakers in talks held during 1976: Fischer to give his paper "Corporate Supply of Index Bonds" on February 17, 1976, and Barro to present "Money and Economic Activity in the United States since 1869" on May 11, 1976.[331]

A still-younger generation was present on the University of Chicago campus at the tailend of Friedman's time. Joining the business school at the start of the 1976/1977 academic year, and thus overlapping on the campus with Friedman for one academic quarter, was Frederic (Rick) Mishkin (born 1951). Although "I didn't get to know him because he was getting ready to leave" (Frederic Mishkin, interview, June 18, 2013), Mishkin did encounter Friedman during their brief crossover at the university over the second half of 1976. Most notably, on November 2, 1976, Mishkin—who would later recall that, as a graduate student, "a major intellectual influence on my thinking was Milton Friedman and Anna Schwartz's *A Monetary History of the United States*" (Mishkin, 2007a, p. ix)—gave a paper at a rowdy session of the money workshop. He found that Friedman was not well disposed at all toward the argument that Mishkin was advancing in the presentation.[332]

This was one of the very last workshop sessions presided over by Friedman. Before he left for Stockholm with Rose Friedman on December 6, 1976, there would be four remaining workshop sessions on his watch: Thomas C. Heagy's presentation of "The Long Term Demand for Money in the U.S.—A Time Series Approach" on November 9, 1976; Reuven Brenner's talk on "Indexing and the Source of Changes in the Price Level" on November 16, 1976; Friedman's own delivery of his draft Nobel lecture on November 23, 1976; and Allen Drazen's offering of his paper "Asset Holding in an Overlapping Generations Model" on November 30, 1976. The appendix to the present chapter contains further details on Friedman's money workshop in its later years.[333]

Also among the new economists at the business school, having joined it in 1975, was Roger Kormendi (born in 1949). Before drifting away from research in the mid-1980s, Kormendi would produce several articles in monetary economics, as well as research on fiscal policy that would provoke Franco Modigliani's wrath in the 1980s in a manner reminiscent of Modigliani's

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[331] Information on workshop dates was obtained from the University of Chicago Library's archives.
[332] This had been issued as Mishkin (1976). For more details on this session, see Chapter 18 below.
[333] The Friedmans' arrival date in Stockholm was given in Friedman and Friedman (1998, p. 446−447) as the daytime of December 6, 1976.

reaction during the 1960s to the Friedman-Meiselman findings on fiscal policy. "Friedman last taught at Chicago the quarter he won the Nobel, one year after Roger started at Chicago," recalled Philip Meguire, who would be a student and longtime coauthor of Kormendi's. "They knew each other a tiny bit." (Philip Meguire, personal communication, April 29, 2011.)

Still another notable presence on the University of Chicago campus was someone who was visible to Friedman only as a face in the crowd but who would play a very major role in monetary economics in later years. Completing his undergraduate studies during Friedman's final years at the university, Michael Woodford (born 1955) was not majoring in economics but attended some of Friedman's 1975/1976 Price Theory graduate lectures out of curiosity (Michael Woodford, personal communication, November 6, 2013).

Friedman's teaching of Price Theory into 1976 reflected the anomaly—present through the mid-1960s and again a feature of the graduate school's teaching assignments since 1973—that he taught in an area different from the monetary field that had brought him national fame. Of course, originally his responsibility for teaching graduate price theory had not been an anomaly: microeconomics *had* been his main field when he started teaching at the university in 1946. And his contributions in this area during the later 1940s, especially those made in collaboration with L.J. Savage, had been the basis for his receipt of the Clark medal in 1951. The Friedman-Savage work on utility did still receive citations in the economic-research literature of the mid-1970s.[334] It was even mentioned in a *Business Week* article on gambling (August 4, 1975, p. 68). But it was Friedman's monetary research that was the overwhelming basis for his modern reputation in research and policy circles. It was this research on which the Nobel award was based, and it was Friedman's career since the late 1940s, focused on monetary analysis, rather than his whole career that was the grounds on which a 1976 textbook discussion noted: "He started his work amid ridicule and lived to see it become the leading rival of Keynesian economics."[335]

One of the oldest of the new generation of monetary economists, Robert Lucas (born 1937), was one of those who overlapped with Friedman in the University of Chicago's economics department in the last Friedman years: Lucas joined in 1974 as a visiting professor, with his appointment becoming permanent in 1975. Lucas was also one of those in his generation most

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[334] Unusually, Friedman also permitted his money workshop to hold a session related to that research, when Benjamin Eden presented "Time Perspective in Behavior towards Risk and the Economics of Gambling" on April 8, 1975. This paper was also related to some of Friedman's monetary work of the time (Friedman, 1977b).
[335] Thorn (1976, p. 257).

associated with the challenge to Keynesian economics and, furthermore, Lucas' own work in this area built on Friedman's monetary contributions in important respects. Nevertheless, Lucas emphasized with respect to his own move to the university that "I was drawn more" by the presence of "good theorists" in the department (Robert Lucas, interview, March 12, 2013.)[336]

This was natural: Lucas' research, although related to Friedman's, included numerous technical contributions that Friedman likely never read. Such work was very far from what Friedman's approach to research had become: Dennis Robertson's (1954, p. 671) description of Friedman as one of the "eminent mathematical economists," while fairly accurate at the time when Robertson offered it, could not, by the mid-1970s, seriously be regarded as still applicable. "I didn't come here to work with Friedman," Lucas further recalled of his move to the University of Chicago (interview, March 12, 2013). "I knew I wasn't going to be collaborating with him or anything. But he was a hugely stimulating guy to have around—that's for sure."

Michael Mussa and price stickiness

In 1977, following Friedman's departure, Lucas would take over the overseeing of the money workshop. Even in Friedman's final five or six years of running the workshop, the content of the papers presented at the workshop had heavily reflected Lucas' impact on research. Friedman could take considerable consolation in the fact that—even though the rational expectations revolution of this period was putting much of the analytical modeling and data analysis in his published work even further behind best practice—the research that emerged from the revolution provided support for many specific theoretical and empirical positions that he had taken.[337]

In this connection, Edward C. Prescott observed that "Lucas' program to make economics dynamic—aggregate economics, using dynamic economic theory to model that: that was a revolution." Friedman's work preceded this revolution: "It was almost sad that he didn't have all the firepower, the tools that are needed." Prescott noted that, all the same, "Friedman came up smelling of roses there in what he had supposed" because "a lot of Friedman's recommendations" and hypotheses that had appeared in Friedman's research of the 1950s and 1960s would receive support in the body of 1970s work that was generated in the new methodological environment of rational expectations and optimization-based macroeconomic

[336] In the same remarks, Lucas also cited an attraction of his initial move in 1974 that "Robert Barro [who would leave in 1975, as already indicated] was doing some good work."
[337] For a detailed discussion, see Nelson (2020b, Chapter 15).

models. (Edward C. Prescott, interview, February 16, 2016.)

The items that Prescott specifically cited were—with respect to the Phillips curve—the result that "there's no permanent tradeoff, that it didn't make economic sense;" the permanent income hypothesis which, in emphasizing intertemporal decisions, was "so important… [in relaying] the economics that people were saving for a reason;" and the lesson that, in setting monetary and fiscal policy, "you need to stick to rules that have good operating characteristics." Friedman, Prescott observed, had built that "intuition [supporting these propositions], without all those developments in dynamic general equilibrium, and using probability, recursive methods." (Edward C. Prescott, interview, February 16, 2016.)

Michael Mussa was an example of the younger-generation monetary economists whose perspective was shaped heavily by Friedman's work. Mussa would later describe himself as "a wobbly monetarist" (*Chicago Tribune*, June 26, 1985). The "wobbly" qualification likely came about because of the growing doubts in the United States about relationships involving money that arose from the major velocity movements seen after 1981. In addition, as discussed below, Mussa was never a strong proponent of the constant-monetary-growth rule.

Mussa was, nonetheless, highly sympathetic to many aspects of monetarism. Philip Meguire, who became a graduate student at the business school during the late 1970s, would later characterize the effect of Mussa's classes in these terms: "I was educated in the 1970s to be warm to monetarism." (Philip Meguire, personal communication, May 2, 2012.) Mussa's continuing positive attitude, in subsequent years, toward monetarism was reflected in his analysis of U.S. monetary policy over the 1980s in Mussa (1994). In contrast to numerous other retrospectives on U.S. monetary policy in the 1980s, this analysis gave the behavior of monetary aggregates considerable weight in judging monetary policy stance.

Mussa was also interested in Friedman's monetary work outside monetarism proper. Mussa (1976a, p. 306) had noted that Friedman's 1969 article "develops the concept of the 'optimum quantity' and 'optimum rate of growth' of money."[338] This particular Friedman paper would receive attention in the research of numerous University of Chicago economists other than Mussa's, with Lucas and Stokey (1983) being perhaps the most notable example. As stressed in Nelson (2020a, Chapter 9), the 1969 Friedman paper was a breed apart from Friedman's main

[338] In fact, a legitimate criticism of the Friedman (1969a) paper was that it really concerned optimal monetary growth and had misapplied the "optimum quantity" to this growth-rate concept.

monetarist writings. Relatedly, both in 1969 and subsequently, Friedman seemed anxious to make clear that the deflation rule derived in that article was not something that he prescribed or to which he put attached much practical applicability. Indeed, one of the methods that Friedman eventually used to separate himself from the deflation-rule prescription was to cite a paper by Mussa—specifically, Mussa (1977)—as the source of the derivation of the rule.[339]

The natural rate hypothesis, too, was part of Friedman's body of work that Mussa pursued. Although Mussa's (1976a) book did not actually cite Friedman's 1968 paper on the Phillips curve, in other work during this period, Mussa (1976b, p. 225) had defined and used the "natural rate of output" concept. Furthermore, on numerous occasions, Mussa credited Friedman with the natural rate hypothesis and stressed its importance. For example, Mussa (1980, p. I–96) observed, "The basic idea of the natural rate hypothesis was initially advanced by Milton Friedman (1968[a]) and Edmund Phelps (1968)," while Flood and Mussa (1994, p. 63) noted: "An essential idea that was introduced in Friedman's thinking and in other macroeconomic theorizing at that time was the 'expectations-augmented Phillips curve.'"

In contrast to his work on the optimum quantity of money, Friedman was very happy to be closely associated with his development of the natural rate hypothesis. But because it focused on aggregate-supply relations, this, too, was seen as a contribution somewhat separable from his monetarist body of work. In effect, Friedman acknowledged this separation himself in his Nobel lecture when he stressed that, in his discussion of Phillips-curve dynamics, he would not be taking a stand on what forces drove nominal aggregate demand.[340] A further complication was that widespread acceptance of the natural rate hypothesis among economists by 1977 disguised a considerable amount of dissent from Friedman's particular account of the Phillips curve.

In particular, as will now be detailed, among the monetary economists based at the University of Chicago in the decade after Friedman's departure, Michael Mussa would be a prominent believer in a sticky-price basis for the short-run nonneutrality associated with the Phillips curve. He was not the only economist making this case in the 1970s, but he marked himself in doing so from the University of Chicago.[341]

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[339] See Friedman (1987a, p. 18). This attribution may also have been, in part, an acknowledgment on Friedman's part that Mussa's derivation of the deflation rule was more rigorous than his own had been.
[340] Friedman (1977c, pp. 453–454).
[341] In addition to Mussa, there were a few other economists at the University of Chicago in the immediate years after Friedman's departure whose work stressed protracted and systematic (though ultimately temporary) effects of

There was considerable acclaim for and acceptance of the natural rate hypothesis by 1976, as Friedman's Nobel award confirmed. Nevertheless, the apparent consensus disguised underlying considerable deviations in views from those that Friedman had advanced concerning Phillips-curve relationships. Notably, the cost-push view of inflation was far from dead in policy circles. For example, about ten days before Friedman won the Nobel award, Phillip Lynch, speaking for the Australian government, in the address to the IMF discussed earlier in this chapter, stated that there was a "remarkable degree of consensus" that it was not an option to "buy less unemployment at the expense of higher inflation."[342] But Lynch's actual practice of anti-inflation policy, like that of policymakers in many other countries during the late 1970s, involved considerable reliance on attempts to hold down prices and wages by nonmonetary means. Policymakers in this era therefore tended to embrace a perspective closer to that of Arthur Okun, who saw inflation largely in cost-push terms despite accepting aspects of the accelerationist position, than that of Friedman.[343]

The view predominant among University of Chicago economists in 1975−1979 did seem superficially to be very close to Friedman's. A paper that Roger Kormendi presented to the money workshop on April 5, 1977, three months after Friedman's departure, began: "The cornerstone of modern macroeconomic theory is the so-called natural rate hypothesis."[344] Kormendi went on to cite Friedman, Phelps, and Lucas as having provided the basis for the hypothesis. Furthermore, there was little sympathy for cost-push notions of inflation, and on October 22, 1975, during Friedman's tenure heading the workshop, Lucas had sent a memorandum to workshop participants denouncing the cost-push perspective.[345]

Notwithstanding these elements of solidarity with Friedman's analysis of inflation, however, the specification of the Phillips curve and approach to macroeconomics propounded by Lucas and other key proponents of rational expectations—especially those associated with the University of

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monetary policy on real variables and who tended to favor a sticky-price-focused rationalization of the Phillips-curve relationship. These individuals included Mishkin, Jacob Frenkel, and Robert Nobay.

[342] In International Monetary Fund (1976, pp. 178, 180).

[343] See Chapter 5 above.

[344] Kormendi (1977, p. 1).

[345] As for Michael Mussa, his disdain for cost-push explanations of the 1970s inflation was evident in his Congressional testimony of November 1, 1983. In his written submission for the hearing, he referred to "the failure of the administration and the Federal Reserve to respond to the acceleration of inflation in 1977 and 1978." (In Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1984, p. 204.) Implicitly, this criticism was that the authorities should have responded by firming *aggregate demand settings* in these years (whereas, in the event, they mostly put off tightening—specifically, of monetary policy—until the second half of 1978). In both years, there *had* been an administration response to accelerating inflation, but it had taken the form of nonmonetary (and non-demand) measures against inflation. For details, see the next chapter.

Chicago—had parted company with Friedman by rejecting short-run nominal rigidity (in the form of stickiness of wages, prices, or both) as the source of short-run curvature of the Phillips curve and as a key reason for the nonneutrality of monetary policy with regard to the business cycle.

In contrast to Lucas, Prescott, and numerous other 1970s proponents of the application of rational expectations and microfoundations, Mussa was an advocate of the importance of long-lasting nominal stickiness. Like Friedman, he rejected the notion that rational expectations, in themselves, precluded the possibility of systematic effects of monetary policy on output: indeed, Mussa rejected the premise advanced by others that Friedman had grounded his own Phillips-curve analysis in adaptive expectations, contending instead: "In Friedman's original work, … expectations are forward-looking."[346] But Mussa granted that more work needed to be done in order to make the modeling of a sticky-price-based Phillips curve rigorous, and he proceeded to seek to make a contribution in that direction himself.

In a paper titled "Sticky Prices and Disequilibrium Adjustment in a Rational Model of the Inflationary Process" that Mussa presented to the money workshop on May 3, 1977, he provided his own attempt to reconcile the natural rate hypothesis, sticky prices, and rational expectations. The paper would ultimately be published in the *American Economic Review*, and, in it, Mussa (1981, p. 1070) would acknowledge that "serious theoretical difficulties are encountered in models that combine rational expectations with the notion that prices are sticky and respond slowly" to excess aggregate demand or supply but would go on to propose a means of generating this combination. In his model, Mussa (1981, p. 1074) observed, the price-adjustment equation "is similar to the 'expectations augmented Phillips curve' that has been widely employed in discussions of wage and price dynamics."

Mussa's model did not catch on and was far less influential than other attempts in the late 1970s and early 1980s to bring price stickiness into a rational expectations framework, notably those of Stanley Fischer, John Taylor, and Guillermo Calvo.[347] But, in contrast to many of his University of Chicago colleagues, Mussa had correctly anticipated that the economics profession's acceptance of the natural rate hypothesis and of rational expectations would ultimately not

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[346] Flood and Mussa (1994, p. 63). In a similar vein, Laidler (1997, p. 89) observed that in "Friedman's (1968[a]) application... to the Phillips curve, explicitly adaptive expectations were conspicuous by their absence."
[347] See, however, Clarida and Galí (1994) and McCallum and Nelson (1999) for late examples of studies that used elements of the price-setting specification that Mussa advocated.

involve eschewing an assumption of short-run price stickiness. Indeed, Mussa had foreseen the direction in which the profession would go as early as April 1975, when he remarked: "I think you can have relatively rigid wages and prices even in an environment in which there is no substantial amount of money illusion."[348]

Mussa would increasingly specialize in international economics and, in that context, he would become a champion of bringing the natural rate long-run restriction into the Phillips curve of open-economy models. He called in a conference in October 1980 for open-economy models to incorporate the hypothesis, especially in view of "the absence of any factual basis" for a long-run tradeoff in recent years' data.[349] He also made clear that he believed that the appropriate specification of the Phillips curve in these open-economy models was one involving incomplete short-run price adjustment—a message that he underlined in a famed later paper (Mussa, 1986b) in which he indicated that the time-series behavior of real exchange rates in the floating era was consistent with gradual adjustment of the price level to disturbances.

One point of contrast with Friedman lay in policy evaluations. Mussa would come to be known for "his outspoken and occasionally acerbic style" (*Financial Times* (London), March 8, 2001). But neither at the time nor later was he as trenchantly critical of 1970s Federal Reserve policy as Friedman was. Certainly, Mussa shared the judgment that monetary policy was systematically flawed over this decade. In 1983, for example, he would suggest that "the perceived long-run stance of U.S. monetary policy" during the 1970s had been one of excessive looseness.[350] And looking back on monetary policy in Arthur Burns' second term (1974−1978) as Federal Reserve chair, Mussa (1994, pp. 87, 95) noted the "significant accomplishment" of getting inflation down in 1975−1976 but nevertheless registered the negative judgment that the period from January 1975 was one of "inadequacy of the Federal Reserve's efforts to curb inflation." He was nevertheless not as viscerally critical about Burns' performance (on the criteria of both average performance and variability) as Friedman had been on many occasions—such as in October 1975, when Friedman remarked: "The slow growth of the last couple of months is indicative of the Federal Reserve's stupidity." (*Dallas Times-Herald*, October 17, 1975.)[351]

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[348] In Birnbaum and Laffer (1976, p. 157).
[349] Mussa (1980, pp. I–77 to I–78; quotation from p. I–77).
[350] From Mussa's testimony of November 1, 1983, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1984, p. 198).
[351] As indicated in the previous chapter, this temporary weakness was followed by a revival of monetary growth in 1976. In combination with the continuation of expansionary settings into 1977 (discussed below and in the next chapter), this was a posture of monetary policy that Friedman later largely blamed for the late-1970s surge in inflation. Mussa (1994, p. 89) argued with regard to developments in 1976 that "the evidence… does not indicate

More generally, Mussa did not share Friedman's skepticism about the practical scope for a central bank to pursue an output-gap-stabilizing, activist monetary policy in circumstances of a short-run nonvertical, and long-run vertical, Phillips-curve relationship. Mussa's greater confidence in the prospects for an activist policy in this situation actually stemmed in good part from a belief that he shared with Friedman—that price stickiness produced long-lasting effects of monetary actions on output. To Mussa, this gave policymakers increased leeway to arrive at policy actions that steered output closer to its potential value. In a paper prepared for a July 1977 conference, he viewed the key question with regard to the Phillips curve as "'how long is the long run?' If the long run is a year, then macroeconomic policy (with its lags and uncertainties) cannot be very useful. If the long run is a decade, then it may be useful to think in terms of a policy tradeoff between inflation and unemployment."[352] Mussa did not see as strong a propensity as that Friedman perceived for policymakers to behave in an unintentionally destabilizing manner. Also unlike Friedman, Mussa was willing to use, in his analysis of current economic policy, time series on the output gap that were derived, in part, from estimates of potential output.[353]

Evidently, therefore, despite his common ground with Friedman about the formulation of the Phillips curve and about the negative judgment about U.S. monetary policy as conducted over much of the 1970s, Mussa did not adhere to Friedman's comprehensive opposition to an activist central-bank reaction function.[354] This divergence in perspectives was reflected not only in Mussa's research writings but also in his willingness to spend long periods in policymaking.

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that the Federal Reserve was knowingly fueling the resurgence of inflation." Friedman's commentary during 1976 was consistent with this evaluation, but it came in a barbed form: Burns, he told an audience of financial market participants at an event in Boston, "has good intentions, I'm sure, in trying to get the inflation rate down to 1 percent. But we can't judge what will happen on good intentions. After all, Mr. Burns did not intend for inflation to go up to 12 percent." (*Boston Globe*, June 2, 1976.)

[352] Mussa (1979, p. 196).

[353] See his testimony of November 1, 1983, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1984, p. 220). It should be stressed that, by this vantage point of late 1983, it was typically the case that estimates of U.S. potential output had recognized the post-1973 productivity slowdown and so lacked much of the sizable error present in 1970s-vintage estimates.

[354] Mussa's perspective that the monetarist critique of the official sector had gone too far had a counterpart in the deterioration over time in his relations with one of the key monetarists, Allan Meltzer. Mussa, consistent with his background as a UCLA undergraduate student in 1962−1966 and his 1971−1976 appointment at the University of Rochester, would be a key participant in many of the Carnegie-Rochester conferences held from 1973 to 1989, and Brunner and Meltzer (1993, p. 47) cited Mussa (1986b), a paper prepared for the Carnegie-Rochester conference series, as a major study in the literature documenting price stickiness. But in the 1990s, concurrently with Mussa's tenure at the Fund, Meltzer would be a vocal critic of the IMF, and Meltzer and Mussa had ceased to be on speaking terms by 2000. Mussa was taken to be referring primarily to Meltzer when he remarked: "Despite recent criticism of the institution, the Fund is undoubtedly a highly efficient international organization." (*Financial Times* (London), March 8, 2001.)

Of these periods, Mussa's membership of the Council of Economic Advisers from 1986 to 1988 was notable as being one in which the CEA became more outspoken about the nature of the short-run Phillips-curve relationship. Although, as stressed by Romer and Romer (2002, pp. 33−34), key U.S. economic-policy agencies—including the CEA—largely embraced the expectations-augmented Phillips curve (including a vertical long-run form) from about 1979 onward, the Reagan Administration had a sporadic record when it came to acknowledging in its public statements that there were real economic costs in the short run associated with bringing inflation down.

Notably, the 1985 *Economic Report of the President*, for example, had acknowledged that the 1981−1982 recession had resulted from the restriction of aggregate demand that brought about the disinflation of the 1980s, but it claimed that this recession had been an "unexpected… part of the economy's transition to lower inflation."[355] This passage of the report would receive sharp criticism from Dornbusch and Fischer (1987, p. 500): they deemed it "simply not accurate" and contrasted the report's claim with the fact that in the early 1980s "[m]ost economists shared Friedman's view... that disinflation would bring a recession."

As noted, Mussa's affiliation with the CEA began, initially in the role of a consultant, in October 1985—well after the 1985 *Economic Report* had been produced and released. By the time of the production and appearance of the 1987 report, Mussa was a full CEA member and had much to do with the content of the report.[356] The 1987 report embedded what Dornbusch and Fischer would call "Friedman's view"—that Phillips-curve dynamics made a recession an inevitable part of reducing inflation.

The 1987 *Economic Report* referred unequivocally to the "inevitable economic cost of disinflation."[357] Instead of portraying a recession as being avoidable or unexpected in conditions of disinflation, the report portrayed the disinflation-recession link as a basis for seeking, and then consolidating, price stability: "The destructive sequence of business cycles with progressively rising inflation rates and interest rates, punctuated by severe recessions, has been broken. With appropriate macroeconomic policies, the U.S. economy need not suffer, once again, the painful

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[355] Council of Economic Advisers (1985, p. 29).
[356] Indeed, Mussa and CEA staff member Richard Clarida gave a briefing on the forthcoming report to U.S. Secretary of State George Shultz. (Remarks of Richard Clarida at the session of the Hoover Working Group on Economic Policy, Hoover Institution held on February 17, 2021.)
[357] Council of Economic Advisers (1987, p. 56)

process of wringing entrenched inflation out of the economic system."[358]

A talk with the U.S. president-elect

This assessment, published in early 1987, appeared just as the post-disinflation U.S. economic recovery was entering its fourth year. The events of the subsequent twelve months would underpin the assessment's validity, as 1987 would see a further period of reasonably low inflation, accompanied by the continuation of the U.S. economic expansion into a fifth year. This outcome contrasted with the most recent prior multi-year expansion, which had begun under President Ford in 1975 and then continued into the Carter years. That expansion had, at 58 months, failed to reach the five-year mark, and the 1987 *Economic Report of the President* would recall it as having "ended in a double crescendo of rising inflation and interest rates and falling economic activity."[359]

As detailed in the next chapter, Friedman had made a public warning in late 1976 of the dangers of pushing too hard on aggregate-demand policy to encourage the post-1975 economic expansion. This Friedman appeal was explicitly cast in terms of advice to President-elect Carter—who was being reported as receiving the opposite advice from his economic team

Friedman's warnings would prove prescient. The details of monetary policy decisions were beyond the president's control, but the tone set by the Carter Administration in its first year in office was certainly in the opposite direction of generating a consensus in favor of demand restraint. The holding off until 1978 and 1979 of monetary restriction that could have been applied in 1977 likely played an important part in producing the unfavorable configuration of economic data that President Carter would face by the start of 1980. The Reagan Administration in which Mussa would serve would likely not have come into being in the first place had a recession taken place earlier, in 1977 or 1978—bringing with it a correspondingly earlier, and lower, peak in inflation. Mussa certainly believed so, as was indicated in his remark: "One of the important consequences of the high inflation rate in '79 and '80 was Ronald Reagan."[360]

In late 1976, Carter was not altogether oblivious to the overheating danger. His awareness of it

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[358] Council of Economic Advisers (1987, p. 31).
[359] Council of Economic Advisers (1987, p. 30). On the length of the 1975−1980 expansion, see *https://www.nber.org/research/data/us-business-cycle-expansions-and-contractions*.
[360] From Mussa's testimony of June 6, 1985, in Treasury and Civil Service Committee, House of Commons (1985 p. 51).

was evident when, near the end of the year, the president-elect contacted Friedman.[361]

With the Nobel events having lasted until mid-December, the Friedmans were left with only the final two weeks of 1976 to finalize preparations for their beginning-of-year relocation. In the course of this hectic process, and with the Friedmans having gone to their Vermont home for the holiday season (Instructional Dynamics Economics Cassette Tape 205, December 1976, Part 2), Friedman received, on Christmas Day 1976, a surprise call from the president-elect.

Jimmy Carter asked to be put through to Milton Friedman and, assured that he had been, began: "I've wanted to talk to you, but first let me congratulate you on the [Nobel] prize." Unfortunately, Carter's staff had connected him not to Milton Friedman but to one of Friedman's numerous namesakes, in particular—Mr. Milton (Milt) Friedman, a member of President Ford's staff (*Detroit Free Press*, December 26, 1976).[362] Carter was, in due course, connected to *the* Milton Friedman—who, writing in his memoirs more than two decades later, described his conversation with Carter as having been a "pleasant talk" but relayed no details on its contents.[363]

Years earlier, and only about ten days after the conversation occurred, Friedman had provided a bit more of a glimpse into the exchange. "We talked casually for a few minutes... We had a nice pleasant conversation… Carter asked for some comment on the economy." (*San Francisco Examiner*, January 5, 1977.).

It would prove to be their only conversation—and Friedman did not find a convert. Once Carter took office in 1977, Friedman would before long find himself greatly at odds with the approach of the new president.

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[361] Carter had, however, missed an opportunity to mention Friedman in his third televised debate with President Ford, held on October 22, 1976. Carter had received criticism for having been willing to be interviewed by *Playboy* magazine. Responding in the debate to this criticism, the Democratic presidential candidate listed several celebrities who had been interviewed by *Playboy* in the past. His catalogue included William Buckley but omitted Friedman, notwithstanding Friedman's having won his Nobel award just eight days earlier and the fact that *Playboy*'s February 1973 issue had included a long interview with Friedman.
[362] Ford's staff member Milton Friedman had served as senior speechwriter and special assistant to the president (*The Detroit News*, February 5, 1976).
[363] Friedman and Friedman (1998, p. 459).

APPENDIX: FRIEDMAN'S WORKSHOP ON MONEY AND BANKING IN THE 1960S AND 1970S

Nelson (2020a, Chapter 10) discussed the launch of, and early years of, Friedman's Workshop on Money and Banking in the 1950s. This appendix provides the recollections of several participants in the workshop in the mid-1960s through 1976, the final calendar year in which Friedman ran the workshop. An area excluded from the following discussion is the workshop's dialogue on rational expectations: this was considered in Nelson (2020b, Chapter 15). Meiselman (1975, p. 295) characterized the workshop members in the 1950s as "a small and abrasive counterrevolutionary cell," having a perspective contrary to the Keynesian mainstream of the economics profession.[364] Meiselman, who was writing from the vantage point of 1972, added: "The ground has shifted so much on these issues that my students now find it difficult to believe that many of the debates of yesteryear really did take place." By the mid-1960s, this shifting of ground was well underway. In addition, by this point the money workshop had acquired a considerable reputation outside the university as a formidable forum for the discussion of monetary issues. The opportunity to present at the workshop was consequently one that many researchers who were located outside the university accepted with alacrity.

It was very typically the case, however, that the experience of actually giving a paper in the workshop was a considerable ordeal. Michael Darby recalled: "Occasionally, the visitors would get their feathers ruffled: they weren't used to it, and maybe they didn't believe the warnings." (Michael Darby, interview, October 15, 2013.)

Many of those who presented in the workshop did indeed look back on it as a grueling experience. Michael Parkin presented "Inflation and Unemployment in an Indexed Economy: Some Analytical Issues" in the workshop session of November 18, 1975. He recalled this event as arising because "there was an attempt by some people in Chicago, to recruit me." But when "I gave a talk in Chicago in Milton's workshop, I got absolutely brutalized." (Michael Parkin, interview, May 29, 2013.) Parkin recalled the workshop's format, which consisted of two hours of forensic examination of the paper under discussion, as putting the presenter through the

[364] Cagan (1976b, p. 155; p. 86 of 1978 reprint) provided a similar characterization of the reactions that researchers on money faced in the early to mid-1950s. Cagan's account, which emphasized the condescension that he faced in his early career when he indicated that his research used the quantity theory, has become a favorite article for authors to quote when conveying the flavor of economic opinion in the 1950s: see McCallum (1986a, p. 9; 2016, p. 56) and Meltzer (2009a, p. 17). An indication of the way in which professional opinion was changing by the mid-1960s was provided by Cagan's joining the economics department of Columbia University in 1966.

wringer.[365]

Stanley Fischer was a colleague of Friedman's in the economics department from 1969 to 1973.[366] Fischer recalled: "He also said in the first one [I attended], 'Now, we want you to bring your papers here so that we can discuss them and help you... So we don't want to see completed papers.'… And he said, 'You know, just bring your papers when they're imperfect.'" Fischer observed that he and J. Phillip Cooper of the business school heeded this guidance by scheduling a workshop presentation of a draft version of a joint paper.

The two presented "Simulation of Monetary Rules in the FRB-MIT-Penn Model" (later published as Cooper and Fischer, 1972) at the workshop on April 7, 1970. "So we wrote half the paper, and we arrived there. And after bleeding to death for about an hour, I swore that there was never going to be another incomplete paper from me in the money workshop, because there was no mercy shown to anybody there. I'm not sure whether it's a good way of doing it or a bad way of doing it. But as I say, economics at Chicago was tough: people were friendly, but once you got down to intellectual stuff, the atmosphere was very, very demanding." (Stanley Fischer, interview, August 30, 2013.)

Charles Upton, then of the University of Chicago's business school, co-presented a paper in Friedman's workshop in February 1975 (see Gould, Miller, C.R. Nelson, and Upton, 1978, p. 229). Upton recalled: "Milton always invited the speakers to have lunch before the talk. And those lunches tended to have sort of the atmosphere of the last meal. Because you knew the criticism [in the workshop] was going to be rigorous. It was not designed to prove that you're stupid, but, you know, Milton would open the workshop by saying, 'Any comments on page one?' You almost never got beyond page three, O.K.? It was brutal." (Charles Upton, interview, January 8, 2015.)

In Nelson (2020a, 2020b), it was repeatedly stressed that, in important respects, the University of

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[365] Two hours was the allotted length of time for the workshop, but Bennett McCallum—who attended the workshop "religiously" during the quarter in which he visited the economics department in 1975, and who presented what became McCallum (1976) at a workshop session on February 25, 1975—noted that "we might go two-and-a-half hours sometimes." (Bennett McCallum, interview, June 13, 2013.)

[366] Fischer joined the economics department, after graduation from MIT, in 1969, initially under a postdoctoral fellowship that included teaching responsibilities, before serving from 1970 to 1973 as an assistant professor in the department (American Economic Association, 1981, p. 145; Stanley Fischer, interview, August 30, 2013). See the next chapter for further discussion.

Chicago's economics department was not a Friedman-dominated body: many department members had interests different from Friedman's, and even in Friedman's field of money there were other power centers in the department. With regard, however, to Friedman's own turf of his money-and-banking workshop, his dominance of proceedings is not in question. Indeed, it was corroborated by the recollections of many workshop participants interviewed for this book. Fred Levin, a graduate student at the department in the early 1970s, observed that "if a professor was there, he wasn't treated any better than any of the students. It wasn't a joint workshop, with Friedman running it with other professors. No, it was Friedman's workshop." (Fred Levin, interview, March 10, 2014.)[367]

The page-by-page analysis of the paper was a distinctive Friedman's arrangement. "I actually think it was not the best way to run a workshop," observed Frederic Mishkin, who, as noted earlier in this chapter, presented at the workshop in late 1976. "I think that's one of the reasons it did change [after Friedman left]. You frequently got too bogged down in details before you got to the essence, and, frequently, you really didn't get to the key part of the paper." (Frederic Mishkin, interview, June 18, 2013.)

Robert Lucas (interview, March 12, 2013) noted of this aspect of the workshop format that, "as soon as Friedman left, we dropped it. I'd got into the habit of listening to presentations to decide whether or not I wanted to read the paper. So I did not like preparing in advance that way, and I had never done it."

But when Friedman was still at the university but happened to be away for spells, those running the workshop maintained his format. For example, during his visit to the university in the 1964/1965 academic year, Allan Meltzer ran the workshop in Friedman's absence and adhered to the Friedman format (see Chapter 16 below). Among the others who oversaw workshop sessions during periods when Friedman was away were David Meiselman (Robert Lucas, interview, March 12, 2013), Harry Johnson in the 1962/1963 academic year (William Poole, interview, April 30, 2013; see also H.R. Heller, 1989, p. 65), Lester Telser in 1967 (Thomas Simpson, interview, May 29, 2013; Arthur Laffer, interview, June 10, 2013), and Richard Zecher in the early 1970s (Richard Zecher, interview, September 3, 2013).

[367] One means by which the academics were put on the same plane as other participants was the rule, not always enforced, that every participant had to present a paper to the workshop at some point. "Friedman's idea was: If you're going to hand out all this hard criticism, you've got to stand up and take some punches yourself," Robert Lucas observed (interview, March 12, 2013).

In the Friedman era, it was usually the case that several other teachers affiliated with the economics department or with the business school attended the workshop. One of the most regular attendees was the business school's Arnold Zellner (Bennett McCallum, interview, June 13, 2013). Others who were very often present at the workshop during their years at the university were Martin Bailey (David Meiselman, interview, July 16, 2014), Arthur Laffer (Arthur Laffer, interview, June 10, 2013), and Robert Barro (see Barro, 2007, p. 133). Aaron Director also attended the workshop in its early years (Richard Timberlake, interview, September 10, 2014).

Harry Johnson was a participant in the money workshop in the 1960s and 1970s but was more closely associated with the university's trade workshop (*a.k.a.* the International Economics Workshop: see Harrod, 1970, p. 617, and McCulloch, 2010, p. 534). The trade workshop was regarded as quite different from the money workshop.[368] Robert Aliber observed that "there was a difference in the tone of the two workshops. The trade workshop was a little more collegial." (Robert Aliber, interview, May 1, 2013.) And Arthur Laffer observed of the money workshop: "The students in that workshop were far from objective. They were there to curry favor with Friedman, and if they felt that Friedman didn't like something, they ganged up on the person [presenting]." (Arthur Laffer, interview, August 11, 2014.)[369]

Other attendees of the money workshop, while concurring that it had a freewheeling atmosphere, viewed the give-and-take that took place in a more positive light. Richard Zecher observed: "And that's the reason it was called a workshop, I think, because it was not someone lecturing somebody else. It was very much interactive." (Richard Zecher, interview, September 3, 2013.) Michael Darby described the rationale for the workshop in these terms: "The idea was that it was a workshop; it wasn't a polite seminar where you went to present your work and have your ego massaged. That was viewed as the strength of Chicago—that people really cared about ideas, and if you *were* giving a paper there, you really would come out with something you could use to improve it—rather than, you know, have your ego stroked." (Michael Darby, interview, October 15, 2013.) William Dougan, a graduate student in economics at the university during the mid-1970s, stated that Friedman's "workshop was a true workshop. The idea was: We're here to help you write a better paper. And so the focus is on the paper; this is not about you educating us, although, with any luck, that happens [as well]." (William Dougan, interview, September 19,

[368] Nevertheless, the two workshops occasionally held joint sessions, including when Don Patinkin presented "Indexation in Israel" on October 13, 1975.
[369] See also the remarks by Laffer and Marc Miles quoted in Chapter 11 below.

2013.) Robert Nobay, who was a colleague of Friedman's during the latter's closing years at the university in the mid-1970s, said of the workshop, "it was fundamentally an important adjunct to supervision. So, we had a bunch of very active Ph.D. students who would take anyone to task." (Robert Nobay, interview, December 3, 2013.)

William Gibson, a workshop attendee in the mid-1960s, stated that "it was a highly charged and competitive atmosphere, but for good reason, and a lot of good stuff came out of it." (William Gibson, March 7, 2013.) Gerald Dwyer remarked: "The money-and-banking workshop was really focused on people's papers… The interesting thing was, it seemed like we saw a fair number of papers that were coming out in the *JPE*—after they were accepted, apparently. And they'd get torn apart." (Gerald Dwyer, interview, August 20, 2013.)

With respect to Friedman's interaction with students at the workshop, William Poole, who attended the workshop as a graduate student in the early 1960s, observed: "Milton was very gentle with students, and encouraging, unless he thought the student was just really way, way off base. I mean, he didn't pull any punches. He was polite, but he didn't hold back criticism. But he would work hard to get the student to understand why there was a mistake in the paper, and get the student to defend what he had done. And most of the time, obviously, Milton was correct, and the student was wrong." (William Poole, interview, April 30, 2013.) Ann-Marie Meulendyke, a graduate student in the late 1960s, recalled: "He would never put a student down, but he would put other professors down for what he saw as sloppy or careless thinking." (Ann-Marie Meulendyke, interview, April 29, 2013.)

As already stressed, one of Friedman's edicts was that workshop attendees had to have read the paper in advance. Bennett McCallum recalled that "you were just strictly supposed to have read the paper before you came, and you'd better not demonstrate that you had not read the paper. He looked very unfavorably on people who did that." (Bennett McCallum, interview, June 13, 2013.) Friedman practiced what he preached, himself reading the presented papers in advance. In this connection, Robert Nobay recalled that one presenter in the mid-1970s "produced the most turgid paper, which was absolutely full of matrix algebra and the like. And I gave up on page three or four. And it was pretty obvious that I wasn't alone… Bob Lucas said to me, 'I gave up on page 10.'" Nobay observed that "the only person who kept that [particular] seminar going" was Friedman. Nobay remembered Friedman making remarks to the presenter along the lines of, "'And on page 34, you've missed out this term, and you've got the square in the wrong

place,' et cetera… He was the only one I'm aware of who actually went right through the paper. So, you know, that was his professionalism." (Robert Nobay, interview, December 3, 2013.)

Friedman made no attempt at being a neutral chair of the workshop, instead voicing strong opinions on the paper being presented. For example, Mehrling (2005, p. 159) related an occasion when Friedman introduced a presentation by Fischer Black by observing: "We all know that the paper is wrong. We have two hours to work out why it is wrong." As it happens, the occasion for this statement cannot have occurred at the date of "sometime in spring 1972" that Mehrling (2005, p. 315) provided. It is also very likely that Black did not present the paper that Mehrling gave as what was under discussion at the workshop, but, instead, presented a different paper.[370] The statement itself nevertheless rings true as a Friedman remark. It likely occurred at Fischer Black's presentation at the money workshop held on November 21, 1972, of the paper "The Uniqueness of Price Level in Monetary Growth Models" (later published as Black, 1974).

A preamble of the kind that Friedman gave for Black's workshop presentation might well be regarded as constituting prejudicial comment. At the same time, however, it was hardly to be expected that Friedman was going to take a neutral stand on the anti-monetarist arguments that Black advanced in his work. Furthermore, the Friedman remark quoted by Mehrling is similar to the kind that leading Keynesians of Friedman's generation gave at seminars. In this vein, while Robert Hall noted that "Milton was famous for derailing the discussion," he added that "Samuelson had much the same effect at MIT." (Robert Hall, interview, May 31, 2013.) Another case in point is brought out by the following recollection that Christina Romer gave of a presentation of Romer (1986): "The first time I presented this research at MIT in 1984, Franco

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[370] As discussed in Nelson (2020b, Chapter 15) and Chapter 2 above, Friedman was in the Pacific and Middle East until the late spring of 1972, after which he was based in his second home in Vermont through the end of September. (See Rose Friedman's remarks in Friedman and Friedman, 1998, pp. 417–418—a chronology confirmed by the contemporaneous Instructional Dynamics Economics Cassette commentaries.) Prior to late September 1972, Friedman was in Chicago in the year only briefly, for his son's wedding earlier in September (Friedman and Friedman, 1998, p. 417; Gloria Valentine, personal communication, March 14, 2015).

Mehrling stated that the paper that Black presented was Black (1972). Friedman indeed knew of this paper: see Friedman (1977b, p. 413) and Friedman and Schwartz (1982, p. 604). But there was no confirmation in the acknowledgments or text of Black (1972) that that paper was presented at the money workshop. Indeed, there was no indication in Black (1972) that Friedman provided comments on an earlier draft and, as Friedman's work was discussed heavily in the study, it would seem likely that Black's revised paper would have made mention of feedback from Friedman, had Black received any. As indicated presently, the workshop incident related by Mehrling very likely pertained to a presentation of a *different* Black paper (confirmed in University of Chicago Library records as having been presented at the workshop), namely, what became Black (1974). This possibility is buttressed by the fact that Friedman himself noted that Black had presented results like that in Black (1972) "in a number of papers" (Friedman, 1977b, p. 413).

Modigliani stood up and said, ‘I don’t believe you, because if I did, I’d be very upset.’”
(*Business Week*, July 18, 1993, p. 85.)

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 8: Debates on Monetary Policy and Macroeconomic Stabilization, 1977 to 1978

Edward Nelson[1]
Federal Reserve Board
July 30, 2023

I. EVENTS AND ACTIVITIES IN DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1977–1978

A preamble to a March 1977 newspaper interview with Milton Friedman described him as "now on leave" from the University of Chicago (*U.S. News and World Report*, March 7, 1977). And in the March/April 1977 issue of *Society*—a public-affairs periodical published by Friedman's undergraduate alma mater, Rutgers University—he described himself as a citizen and taxpayer of Chicago.[2]

These characterizations were accurate, but they did not tell the whole story. When completing his tax returns around the time the *Society* article appeared, Friedman would certainly have fallen into the category of a citizen of Chicago, with his local and state tax liabilities reflecting his residence in that city during calendar 1976. And, in early 1977, he was, indeed, on leave from the Department of Economics of the University of Chicago. But it was also true that he was not coming back to the department. In fact, as of March 1977 Friedman had already left both the University of Chicago and the city of Chicago for good.

Milton and Rose Friedman relocated permanently to San Francisco at the very start of 1977—in so doing, missing one of the city of Chicago's worst winter months in recent history. They arrived in their new home city on Monday, January 4, and the following day Friedman started his

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[1] Email: Edward.Nelson@frb.gov. The views expressed here are the author's alone and should not be interpreted as those of the Federal Reserve or the Board of Governors. he author is grateful to the interview subjects for their generosity in providing useful information, as well as for comments provided by participants in a seminar (November 2013) at the University of California, Berkeley, at which some of the material in this chapter was presented, as well as to Charles Steindel for comments on an earlier draft of this chapter. See the Introduction in Nelson (2020a) for a full list of acknowledgments. The author regrets to note that, in the period since the research for this chapter was begun, eight individuals—Kenneth Arrow, David Backus, Gary Becker, Charles Brunie, David Lindsey, Allan Meltzer, Richard Muth, and Paul Volcker—whose interviews with the author are drawn upon below, have passed away.

[2] Friedman (1977g, p. 89). In addition, in *Newsweek* (March 21, 1977), Friedman described Senator Adlai Stevenson (D–IL) as one of his state's senators.

three-month spell as a visiting scholar at the Federal Reserve Bank of San Francisco (*San Francisco Examiner*, January 5, 1977).[3]

Visiting the Federal Reserve Bank of San Francisco

Friedman's spell as a visiting scholar at the Federal Reserve Bank of San Francisco bridged much of the gap between his thirty years at the University of Chicago and what, to his surprise, would prove to be an almost-30-year stretch at the Hoover Institution, starting in fall 1977. In a public statement around the time of Friedman's visitor appointment, the San Francisco bank's president, John J. Balles, observed: "Milton Friedman has altered the course of economic thinking and continues to contribute [to] and influence our fundamental judgments concerning the theory and practical applications of economics." (*American Banker*, January 18, 1977.)

Friedman's visit had arisen from an invitation that Michael Keran extended after Keran became research director of San Francisco's reserve bank in 1973. When seeing Friedman in the city of Chicago in the mid-1970s, Keran recalled, "I said, 'We have a visiting-scholar program at the San Francisco Fed. Would you be interested in participating?' And, to my surprise, he said, 'Yes.'" (Michael Keran, interview, March 7, 2013.)

Keran had, from a distance, been a student of Friedman's during the 1960s, having been guided into his dissertation research topic (the study of money and the business cycle in Japan) by Friedman when they met in Japan in 1963, and having then completed his doctoral dissertation at the University of Minnesota, with long-distance feedback from Friedman.[4] Prior to being hired as the Federal Reserve Bank of San Francisco's research director, Keran had been at the St. Louis bank's research department during the period in which the latter institution's monetarist research took off. Keran was hired to raise the San Francisco's research department's profile;

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[3] The press coverage in connection with the formal announcement of Friedman's retirement from the University of Chicago indicated that his visiting position at the Federal Reserve Bank of San Francisco would last about six months (*Tampa Tribune* (Florida), November 3, 1976). However, in Instructional Dynamics Economics Cassette Tape 204 (December 1976, Part 1), Friedman indicated that his stay would only be (in University of Chicago terminology) over the winter quarter. Consistent with this, in correspondence with Alan Walters (February 9, 1977), Friedman indicated that his period as visiting scholar would finish on March 31 (a dating consonant also with Friedman and Friedman, 1998, p. 559). His activities in the second quarter of 1977—during which the Friedmans were based in their Vermont residence—are discussed mainly in the next chapter.

[4] See Friedman and Friedman (1998, pp. 325–326). The dissertation, completed at the University of Minnesota in 1966, was in effect confirmed as work in the money-workshop tradition via its inclusion, in revised form (see Keran, 1970), in a University of Chicago Press book collection, alongside several Friedman-supervised University of Chicago Ph.D. dissertations (see Meiselman, 1970).

but he also had in mind making its research sensibility more monetarist. "When I left St. Louis, I said, "I'm hoping to go to convert St. Francis to the views of St. Louis.'" (Michael Keran, interview, March 7, 2013.) Already by 1975, Friedman had applauded the stamp that Keran had put on San Francisco's reserve bank by increasing the monetary-research content of its monthly review (Instructional Dynamics Economics Cassette Tape 175, September 1975, Part 1). Having Friedman work temporarily among the bank's economists dramatically underlined the bank's shift of interest toward monetarism.

According to Keran, the rapport Friedman developed with the Federal Reserve Bank of San Francisco's research-department staff was "incredibly good. I'd take him around when he first came, to introduce him to the staff and the research assistants. And [over time] he extracted more information about them, especially the research assistants, than *I* knew. He was very good at making people feel at ease, asking them a lot of questions about themselves, and generally making himself very agreeable. That was a skill I envied." (Michael Keran, interview, March 7, 2013.)

Keran also recalled Friedman being "inundated" with mail during his stay, in the wake of the publicity of the Nobel prize and ceremony just before Friedman's move to San Francisco. Though he had not yet formally moved to Stanford University's Hoover Institution, that was already Friedman's official postal address for business correspondence. His secretary Gloria Valentine, who had started employment at the institution, would bring Friedman's voluminous mail to downtown San Francisco for his inspection.

Research activities

The research project that primarily occupied Friedman during his time as a visiting scholar was an investigation of the properties of the monetary base. Although he had a longstanding preference for an M2-type definition of money, Friedman saw attractions in the monetary base as an alternative monetary concept. Most obviously, monetary targeting that was focused on the base could sidestep the monetary-control issues raised by targeting an aggregate that included bank deposits.

A further possible reason for preferring the base was empirical. In their *Monetary Statistics*, Friedman and Schwartz had noted from their impressions of other countries' experiences that some of the most severe recorded velocity problems had arisen from the behavior of bank

deposits in the wake of financial innovation (including the extension of interest payments on deposits, as well as the creation of deposit substitutes) and changes in banking and monetary regulations. They suggested the stock of currency as an aggregate that could be used for monetary analysis in such circumstances, as currency series tended to be insulated from those institutional changes that bore specifically on the behavior of deposits.[5] In follow-up work during the early years of the 1970s, Friedman's graduate student James Lothian had offered the monetary base as another aggregate that could be more robust to financial innovation than was the case for monetary totals that consisted of the sum of currency and commercial bank deposits (Lothian, 1973, 1976).[6]

Militating against these arguments was experience that suggested that M1- or M2-type aggregates might be more reliable than currency or the base in terms of their relationship to aggregate economic activity. This experience included the Great Contraction of 1929−1933. The rise in both the currency/deposit ratio and the reserve/deposit ratio (for the M1 and M2 definitions of money alike) during the banking panics of those years had made currency and the base misleading indicators for a time. Furthermore, as the 1976 Bach Report had noted, the payment of interest on reserves, if enacted in the future, would amplify the difference between currency and bank reserves and render it more difficult to predict nominal income on the basis of the behavior of their sum. More routinely, reserve-requirement changes could make the base a misleading indicator of monetary conditions if the base series was not adjusted for those changes.

It was this last consideration that Friedman investigated during his early 1977 researches. As he and Anna Schwartz later recalled (*Wall Street Journal*, December 20, 1993), their *Monetary History* "attributed the depth of the 1929−33 depression and the occurrence of the later 1937−38 recession to Fed mismanagement." The 1937−1938 recession, being preceded by the Federal Reserve Board's 1936−1937 moves to raise reserve requirements, had highlighted for them the importance in practice of reserve-requirement changes as a factor bearing on the quantity of money.

For the postwar period, the same consideration applied in a different way. The Federal Reserve had, as Friedman observed, followed an interest-rate stabilization policy that meant that reserve-

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[5] See Friedman and Schwartz (1970, p. 145).
[6] Friedman had referred to Lothian's work-in-progress in his consultant's memorandum to the Federal Reserve Board of June 9, 1971 (Friedman, 1971c, p. 10).

requirement changes were generally not allowed to have a powerful, 1930s-style, effect on the money stock. Rather, the Federal Reserve routinely offset its reserve-requirement changes with open market purchases or sales.[7] The fact that it did so, however, only underlined the differences between the base and the adjusted base (that is, the monetary base adjusted for changes for reserve requirements). The Federal Reserve Bank of St. Louis had, since the 1960s, provided a series on the adjusted base. In his 1977 investigations, Friedman compared the predictive performance, for variations in nominal income, of changes in the unadjusted monetary base with those of changes in the adjusted base.[8] He found that the adjusted monetary base performed far better. Friedman took this finding as suggesting that, in the case of the United States, one could not bypass the money-measurement problem simply by relying on high-powered money. Leaving aside the option, aired by Friedman and Schwartz, of treating currency as the monetary aggregate of interest, this finding implied that either the adjusted base or a currency-plus-deposits aggregate was needed as the measure of money.

Completion of this research project became a casualty of Friedman's busy schedule and waning research ambitions. A research article never came out of the project. Despite the enthusiasm of a coauthor at the Federal Reserve Bank of San Francisco, who worked on a draft of a paper that would lay out the project's findings, the writing-up of the project was never finished. The only major glimpse of the project that Friedman subsequently provided was his recollection of the project's results, which he gave in passing in the course of a floor discussion during an NBER Festschrift for Anna Schwartz, held in New York City in October 1987 (see Bordo, 1989, pp. 76–77).

Friedman had declared at the end of 1976: "My primary interest continues to be my scientific work."[9] This statement was not borne out by his subsequent activities. The year 1977 did see a number of research publications by Friedman, including multiple issuances of his Nobel lecture (discussed in the next chapter) and a written record of his January 1977 debate with Franco

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[7] It was the fact that open market operations could offset reserve-requirement changes, while also being more easily varied than those requirements, that underlay Friedman's preference for the latter tool for the purpose of monetary control. For example, in 1973 he observed: "it's worth emphasizing that there is absolutely nothing that can be done by changes in reserve requirements that cannot be done by open market operations. Open market operations are a more sensitive and effective tool. Changes in reserve requirements are a blunt instrument which are discontinuous in time and, on the whole, are a poor instrument for monetary management." (Instructional Dynamics Economics Cassette Tape 124, July 4, 1973.) See Nelson (2020a, Chapters 2, 8, and 10) for further discussion.

[8] In Instructional Dynamics Economics Cassette Tape 206 (January 1977), the first cassette commentary recorded from San Francisco, Friedman referred to this exercise as a current research project.

[9] Friedman (1977m, p. 241; p. 265 of 1992 reprint).

Modigliani, published as a special issue of the *Federal Reserve Bank of San Francisco Review* (see Section III below). But, after 1977, it became clear that he had put his days of prolific research output behind him. The year 1978 became the first since 1956 in which no article by Friedman appeared in an economic-research journal.[10]

Gary Becker firmly associated the change in Friedman's research output to his relocation. "Well, he didn't do much research after he left Chicago. If he had stayed at Chicago, he would have continued to do research. I mean, you can't stay at Chicago without doing research; otherwise, you'd better leave. The reasons he left were for Rose, and so on, who didn't like Chicago and wanted to live in San Francisco. And yes, he basically stopped serious research at that point; he may have finished up some stuff, [but] he basically stopped doing serious research." (Gary Becker, interview, December 13, 2013.)[11]

By moving physically west to California, Friedman had left the metaphorical Wild West that was the University of Chicago economics world. At the Hoover Institution, he did on rare occasions hijack a seminar that he was attending (see the next chapter for an example). But Friedman's routine of hosting and attending freewheeling research workshops that could be disruptive for presenters, and a gladiatorial experience for attendees, was now behind him. So too were occasions such as that in 1962 when, as an audience member, he had turned a lecture at the University of Chicago by Harold Wilson into an unscheduled debate between himself and the future U.K. prime minister. *Fortune* magazine (March 19, 1984, p. 21) would note, "Friedman says he still misses the intellectual climate—but not the weather—at the University of Chicago."

Thomas Sargent contrasted Friedman's new permanent location of the Hoover Institution (which is discussed in more detail below) with competitive economics departments like the University of Chicago's. "Hoover is much more decentralized and, you know, relaxed. And it's not an intense research place. Some people like that and thrive on it, and some don't." (Thomas Sargent, interview, January 24, 2014.)

In some respects, Friedman did indeed thrive in this environment. As subsequent chapters will detail, his Hoover Institution years, particularly through about 1992, saw Friedman engage in prodigious writing and continuing activity in public policy. On these dimensions, Friedman

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[10] Friedman (1978a) appeared in *Policy Review*, a public-policy periodical concerned with domestic-policy and national-security issues and published by the Heritage Foundation.

[11] For other reasons for Friedman's move away from research, see the discussion in Chapters 8 to 12 below.

remained a vibrant force past age 65, which he reached on July 31, 1977. But he did not have the engagement in research that would later characterize leading economists like Becker or James Heckman, who would, unlike Friedman, stay at the University of Chicago when they themselves reached their mid-sixties.

Influence in universities

Although Friedman was drifting away from participation in research, and was becoming a much less common sight at economic-research conferences, his impact on the profession was far from over. To be sure, his influence was not felt primarily through self-identified monetarist economists. There never really was a broad-based Friedman school in academia, located across major universities and represented by senior figures at those universities, of the kind there was for Keynesian economics. Instead, during his University of Chicago years, Friedman's following had a ragtag nature to it, with many of the senior economists sympathetic to monetarism not to be found at major universities but at smaller ones, as well as at Federal Reserve Banks and private-sector financial institutions.

And in 1977, although Friedman had just finished over two decades of overseeing graduate students working in the monetary field, it was rare to find such students of Friedman in prominent places at the "top twenty" U.S. universities. Certainly, there were a few cases. Notably, Phillip Cagan was an economics professor at Columbia University. But Cagan did not make an impact with his research of the 1970s and 1980s comparable with that he had generated with his research of the 1950s and 1960s. And Cagan tended, in any event, to take a much more softly-softly approach to argumentation than Friedman did. "He likes to be provocative," Cagan observed to the present author with regard to Friedman (Phillip Cagan, interview, January 13, 1992), making clear—by the way in which he said this—that that was not Cagan's own approach. Two Friedman students who had graduated in the early 1970s, Michael Darby and Benjamin Klein, were by late in the decade both situated at the University of California, Los Angeles—"one of the best regarded state universities in the country," the Friedmans would have occasion to note—and would become senior figures at the university, as well as among the most-cited researchers among his former doctoral students.[12] Darby, already well established in microeconomics, would remain prominent in monetary economics through the late 1980s, but Klein was moving away permanently from the monetary field by 1978.

[12] The quotation is from Friedman and Friedman (1980, p. 176).

In the late 1970s and the early 1980s, Friedman's monetary work made itself known to the next generation of economists. But it did not do so primarily via the promulgation of that research by his former doctoral students. Instead, others whom he had not taught or supervised would fulfill this function, through an activity from which he had now withdrawn: the teaching of graduate students in economics at U.S. universities. The positions Friedman took did end up becoming the subject of discussion for the next generation of economists, as his views on monetary matters would have a prominent place in graduate courses around the country. Furthermore, much of his work was presented favorably.

A particularly celebrated example of Friedman's success in influencing the thinking of the next generation took shape in the mid- to late 1970s at the Massachusetts Institute of Technology.[13] Friedman's longtime sparring partners at that institution—Modigliani, Paul Samuelson, and Robert Solow—remained vibrant presences. Indeed, by 1977, all three of them were maintaining a higher level of activity in the research sphere than Friedman. But, in large part, they had handed over teaching responsibilities for the core MIT graduate economics programs to a younger cohort in their department. In particular, Stanley Fischer, Friedman's 1969−1973 University of Chicago colleague, taught MIT's graduate monetary-economics course.

In 1969, Friedman had impertinently offered a generalization: What was received wisdom about monetary policy at the University of Chicago during one year became received wisdom at Cambridge, USA—but with a lag of five years (Instructional Dynamics Economics Cassette Tape 17, March 1969). Via Fischer and other new MIT professors like Rudiger Dornbusch, that rule of thumb became even more true from 1973 onward, as they turned prominent elements of Friedman's research output in the 1960s and through 1972 into part of the MIT economics mainstream. Through Dornbusch and Fischer, Friedman's positions—on the importance of the real/nominal interest rate distinction, the centrality of monetary policy for the behavior of aggregate demand and inflation, and cautions about overstating the power of fiscal policy—permeated MIT teaching and research.

Most strikingly, the natural rate hypothesis, which had challenged the Samuelson-Solow position on the inflation/unemployment relationship and which had given rise to Solow's (1969) counterattack against Friedman, was now treated as a valid critique of the original Phillips curve. Fischer (1979, p. 172) referred to the task of "trying to improve macroeconometric models… [to

[13] The interaction with, and posture toward, Friedman in Harvard University economics circles is considered in the next chapter.

take] account of misspecifications, such as those pointed out by Friedman (1968[a]) and Phelps (1967) when they discovered the role of expectations in the Phillips curve." And Dornbusch (1980, p. 162) referred to "the vantage point of modern Phelps-Friedman macroeconomics."[14] In the face of perspectives such as these from his newer colleagues, Solow (1982, p. 24) would acknowledge that "most macroeconomists" now accepted the Friedman-Phelps position on the Phillips curve, although he went on to confirm his own continued resistance to it.

Also, as Dornbusch and Fischer saw things, "a major stage in the acceptance of monetarism occurred when Friedman and Schwartz published their *Monetary History of the United States*."[15] "One of the big changes" on the teaching side at MIT, recalled Frederic Mishkin (interview, June 18, 2013), "was that Stan Fischer came when I was entering graduate school, and Stan was a huge fan of Milton Friedman's. [Though] that didn't mean he agreed with all of his policy prescriptions. So, to give you an example: When I was taking a course, my first course from Stan, he said that if you're serious about being a monetary economist, you have to read Friedman and Schwartz by your bedside every night: you have to have it [the *Monetary History*] at your bedside, and read it every night. And, in fact, I did. So, I think, with the advent of Stan coming on the faculty, that the appreciation of Friedman's work went up just tremendously… And, in fact, there was a big change in the kind of economics that was taught at MIT because of Stan's coming. It moved away from the strict Keynesian, Modigliani-Solow-type discussion of macroeconomics to ones where rational expectations was brought into the classroom [and] the emphasis was on long-run issues; and *Monetary History* was viewed as one of the classic works that had ever been written in monetary economics, and so forth. So there was really a sea change."[16]

A somewhat later MIT graduate student, Christina Romer recalled at a memorial event for Anna Schwartz in April 2013: "I remember vividly one meeting of our second-year monetary economics graduate class at MIT. The professor, Stanley Fischer, started the class by asking, "How do we know money matters?"… Finally, Stan shut down the discussion [and said]: 'Because Friedman and Schwartz showed us.'"[17]

"What happened is Stan, who was at Chicago maybe a little longer than I was, then went back to

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[14] Dornbusch had seemed receptive to a permanently-sloping Phillips curve as recently as Dornbusch (1973, p. 894).
[15] Dornbusch and Fischer (1981b, p. 551).
[16] See also Mishkin (2007a, p. ix).
[17] C.D. Romer (2013, p. 3). See also Ball and Mankiw (1994, p. 128) for a related recollection.

MIT, where he had gotten his Ph.D.," recalled Richard Zecher (interview, September 3, 2013).[18] "… He took a lot of the things he learned in Chicago, and he went back to MIT, and he sort of 'MIT'd' them." By "MIT'ing" the concepts, Zecher explained, he meant that Fischer "gave them a different slant; but they were some of the same basic ideas. And then he trained all these guys—all these guys that are running central banks now; all trained by Stan… Maybe we could say Milton's had a tremendous impact on the heads of central banks, through Stan Fischer at MIT." (Richard Zecher, interview, September 3, 2013.)[19]

Fischer's own characterization of matters was that his period at the University of Chicago "enabled me to combine MIT's analytics with the policy relevance that Milton Friedman typified" (quoted in Blanchard, 2005, p. 249).

In the decade to 1978—which for Fischer comprised roughly six years at MIT and four at the University of Chicago—he had had ample reason to become extremely familiar with Friedman's other positions on monetary matters: that is, those extending beyond the Phillips-curve work and the Friedman-Schwartz historical research. Fischer (2017b, p. 2) would describe his earliest publications in monetary economics as "research [that] was carried out when monetarism was gaining credibility in the profession." In this early period of Fischer's career, Friedman—who would later state that "Stanley Fisher is a good friend of mine, and I have a great deal of respect for his abilities" (*Moneyline*, CNN, January 23, 1998)—would be a major source of feedback on Fischer's work on policy rules with Phillip Cooper, during the period when both Cooper and Fischer were located at the University of Chicago (see Nelson, 2020a, Chapter 8). Fischer sat in on Friedman's graduate money course (see Blanchard, 2005, p. 249), and both Cooper and Fischer were regular attendees of Friedman's money workshop. "There could be no better complement to an MIT graduate education than being a member of Milton Friedman's money

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[18] Zecher in fact had a University of Chicago affiliation for a longer span of years (1968 to 1973) than did Fischer. But Zecher spent part of that time away from the university, taking a visiting position in Australia.

[19] Since Zecher wrote these words, some of the individuals who were MIT graduate students over the years that overlapped with Fischer's time at MIT have stepped down or retired. For example, Ben Bernanke left the Federal Reserve Chair position in 2014, Charles Bean ended his period as Deputy Governor of the Bank of England in 2014, Mario Draghi's tenure as ECB president ended in 2019, and David Wilcox retired as chief economist for the Federal Open Market Committee in 2019. However, the same span of years saw Philip Lowe move up from Deputy Governor to Governor of the Reserve Bank of Australia (in 2016)—a move that also saw Guy Debelle appointed Deputy Governor (a position that he held until 2022) after having been an assistant governor—and Stacey Tevlin become chief economist for the Federal Open Market Committee (in 2019). These individuals had also been MIT graduate students during the Fischer era. More recently, the nomination in February 2023 of Kazuo Ueda to be the next governor of the Bank of Japan has been highlighted as a new example of a Fischer student becoming a senior central banker (*Financial Times* (London), February 16, 2023).

workshop," Fischer remarked in 1986.[20] Upon moving to MIT in 1973, Fischer would develop MIT's own money workshop (see Blanchard, 2005, p. 250).

Finally, in his research using rational expectations models, Fischer produced results that were a closer heir to the Friedman tradition than the Lucas-Sargent-Wallace-Barro body of work could claim to be. This was because Fischer saw systematic monetary policy, and not just one-period monetary policy shocks, as mattering for output fluctuations (Fischer, 1977a; 1980, p. 213).

In light of the posture toward monetary issues that Fischer revealed in his writings, McCallum (1998, p. 309) would ask if there was anything that "differentiates Stanley Fischer from monetarists?" The label of "monetarist" was, however, was one Fischer rejected as a description of himself. And, indeed, Fischer did not agree with Friedman on many details of monetary analysis, while also parting company with Friedman on the desirability of the constant-monetary-growth rule. Furthermore, Fischer believed, with qualifications, in the validity of fiscal-multiplier analysis, while Friedman did not.[21] Rudiger Dornbusch was likewise in favor of much more active and, in practice, stimulative demand policies in the 1970s than the policies espoused by Friedman (see Section II below).[22] It remains the case, however, that, in forming their positions on macroeconomics, both Dornbusch and Fischer had assimilated many of Friedman's ideas on the effects of monetary policy on the economy.

Influence on textbooks

A shift similar to that observed at MIT was also occurring elsewhere in the later 1970s. It was

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[20] Fischer (1986, p. xi).

[21] Fischer also parted company with Friedman on the lineage that Friedman (1956) saw between his restatement of the quantity theory of money and earlier University of Chicago research on money. As a graduate student at MIT, Fischer was research assistant for Don Patinkin's work that critiqued Friedman's contention, and in the resulting paper Fischer was thanked in the opening footnote, for reading older University of Chicago Ph.D. dissertations on money (see Patinkin, 1969, p. 46). Fischer recalled that Patinkin, in assigning him that task, "really made me work for that footnote. It was a pleasure, incidentally; and there just wasn't a whole lot [in the theses] that you could possibly describe as what Friedman said was a Chicago tradition. Now, it wasn't clear to me why it was so important for him to *have* the Chicago tradition. You know, otherwise, he could have said, '*I* invented something,' which he may well have done." (Stanley Fischer, interview, August 30, 2013.)

[22] Miles (1979, p. 601) described Dornbusch as an international monetarist, and Fischer (1977b, p. 60) even referred to the "simplest monetarist model, due to Dornbusch (1973)…" However, these descriptions were written in an era when contributions to the literature on the monetary approach to the balance of payments were routinely described as monetarist. These contributions gave an important role to the money demand function in the analysis and made the price level endogenous. They were therefore closer to Friedman's monetarism than to the Keynesian open-economy models of previous years. But they tended to consider issues different from those that were the focus of most of Friedman's monetarist research, and they reached conclusions that often did not coincide with Friedman's own positions on open-economy matters.

felt not only in the content of graduate teaching, but also in undergraduate textbooks. One example of the latter was an intermediate undergraduate textbook on monetary economics published in 1977 titled *Money, the Price Level and Interest Rates: An Introduction to Monetary Theory*, by Gail E. Makinen, an associate professor at Wayne State University. The text's Chapter Nine was titled "The Monetary Theory of Milton Friedman." Makinen (1977, p. 232) began the chapter, "That a chapter in a textbook is devoted exclusively to an examination and explanation of the monetary theory identified with a single individual stands as a measure of his influence and importance."

However—paralleling the case of graduate teaching, discussed above—MIT provided the most notable example of undergraduate textbooks that felt the stamp of Friedman. The first edition of Dornbusch and Fischer's intermediate undergraduate textbook *Macroeconomics* appeared in 1978. The macroeconomic modeling in the latter part of the book, particularly Part III, was developed partly from Fischer's MIT graduate-course lecture notes, rather than from his undergraduate class (Richard Anderson, interview, November 14, 2013). The many respects in which the book as a whole reflected the influence of, and underlined the importance of, Friedman's macroeconomics have been discussed in Nelson and Schwartz (2008, p. 848) and Nelson (2020a, Chapter 1).[23] As the authors themselves put it in their text (Dornbusch and Fischer, 1978, p. 520): "Much of the analysis of this book would, a few years ago, have been considered monetarist." In this respect and others, the new textbook's analysis veered sharply from prior texts: Fischer recalled that, "certainly, what was in our book was new when it came out" (Stanley Fischer, interview, August 30, 2013).

The textbook was replete with mentions of Friedman. In discussing why this was the case, Fischer observed: "Milton was very active in a *lot* of debate on policy, and particularly pushing the money-supply view. And it was in the newspapers all the time. It was very prominent. Milton *was* one of the most prominent public economists. [Here] I don't mean public-economics

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[23] As detailed in Nelson (2020b, Chapter 15), the authors were critical of Friedman's discussion of lags in the effects of monetary policy, and they implied that he had not been consistent on the matter. Similarly, in an interview for this book, Fischer suggested that in Friedman's popular writings "there was always some damn lag that allowed you to say that everything—inflation, or growth, or whatever—was caused by money, but [one was] never sure that they were the same lags each time." (Stanley Fischer, interview, August 30, 2013.) It is suggested in Nelson (2020b, Chapter 15), however, that the Dornbusch-Fischer (1978, p. 526) criticism of Friedman along this lines may have overlooked the fact that in the early 1970s he explicitly changed his position on the amount of time it took for monetary policy to affect inflation, and that he subsequently stuck to this new, longer estimate.

In any event, the textbook's coverage of inflation became more monetarist over time, with the fourth edition stating (Dornbusch and Fischer, 1987, p. 638): "The answer to the question [of] whether inflation is a monetary phenomenon in the long run is yes."

economists; I mean, people whom the public knew about." In contrast, with regard to Paul Samuelson, Fischer observed, "Paul was not particularly interested in macroeconomics, as opposed to all the *other* things he was interested in. Paul Samuelson could have gotten the Nobel Prize in five or six areas; and Milton couldn't have." Fischer suggested that, instead, the question to ask with regard to the textbook was: "Why was there more Friedman than Modigliani?" The reason for this, Fischer said, was that "Modigliani was never as *prominent* in the public's view. I think Modigliani in many ways was as important an economist as Milton. But not for public policy." (Stanley Fischer, interview, August 30, 2013.)

Another undergraduate macroeconomics textbook, released the same year as Dornbusch and Fischer's and in practice the major competitor against it, was by another former Friedman colleague, Robert Gordon. Gordon (1978), also titled *Macroeconomics*, incorporated into its analysis the modifications to the standard macroeconomic model that were associated with Friedman: the permanent income hypothesis, the natural rate hypothesis, the real/nominal rate distinction, and heavy qualifications about the effectiveness of fiscal policy. Although Gordon was not a monetarist—and, indeed, would become extremely vehement during the 1980s in his public criticisms of particular monetarist ideas and recommendations—he consulted Friedman by correspondence on his 1978 edition's coverage of monetarism; and, as has been noted in Chapter 5, Friedman provided a public endorsement of that edition.

James Tobin's ongoing resistance

At Yale University, in contrast, James Tobin was a continuing source of resistance to the notion that Friedman's analysis should be incorporated into mainstream teaching and modeling. A recollection by Gregory Mankiw concerning Matthew Shapiro underlines this fact. At graduate school in the early 1980s, Mankiw recalled, "Matthew was a co-student with me at MIT. [As an undergraduate,] Matthew had gone to Yale, where he had worked under Tobin. He was Tobin's research assistant, I believe. And then he came to MIT, where he used to take courses with me, and we took courses from Stan Fischer. And I remember him saying this, years ago: One of the things that really struck him was a difference in attitude toward Milton Friedman between MIT and Yale at the time. When he was at Yale, he kind of grew up thinking that this guy Milton Friedman was some sort of crank at the University of Chicago; and then he went to MIT, and he found people speak about Friedman with a sort of reverence." Thus, in classes that Shapiro took, the manner in which Friedman was characterized underwent a transformation: over "a period of a

year, this guy had gone from being a crank to being one of the most revered members of the profession." (Gregory Mankiw, interview, September 24, 2013.)

Shapiro himself recalled that he was "trained as an undergrad in the Keynesian-monetarist controversies… Milton Friedman was treated adversarially, as having quite a different view from the James Tobin Yale Keynesian perspective." (Matthew Shapiro, interview, November 14, 2013). Shapiro was an undergraduate during the time when Tobin's remarks at a 1975 conference appeared in print—remarks in which he declared (Tobin, 1977b, pp. 763–764) that "monetarism is a religion, whose widespread following is a significant economic fact in its own right, whether or not its doctrines are true."

Though he wrote generously about Friedman when the latter received his Nobel prize (*The Economist* (London), October 23, 1976*a*), Tobin soon resumed hostilities. Barry Eichengreen, a graduate student at Yale University in the 1974–1979 period, remarked of Tobin's graduate monetary economics course: "As late as 1977/78, which must have been the second time I took the course… Friedman was in 'the empty chair,' metaphorically." That is, Friedman was an absent figure, against whom a good deal of the teaching was directed.[24] Eichengreen recalled that Tobin was "teaching a lot about points like '*Post hoc ergo propter hoc*?'—that came from 1970, so it was relatively recent [Tobin, 1970b]. He was concerned to show that there was no mechanical relationship between money and price or money and output; that there was of financial assets of which money was [only] one. And he presented Friedman as kind of the counterpoint to that. So what people were learning in other places in the second half of the 1970s, like rational expectations, came up in passing in the course—but was much less important than these debates about does money matter; how much does money matter; how much do we have to simplify the financial sector in order to figure out how much money matters." (Barry Eichengreen, interview, April 3, 2014.)

Another graduate student of the period, David Backus, concurred: "Friedman, I think that's true—he was the guy in the empty chair." (David Backus, interview, April 16, 2014.) However, Backus stressed that he found that Tobin was reticent when it came to comparing his views directly with those of Friedman: "I don't recall getting much insight into Friedman—or Lucas, Sargent, *et al*. Looking back, it was a pretty isolated world. We saw work by these guys, but

[24] Eichengreen's parallel here was with Clint Eastwood's use of an empty chair as though it was occupied by President Barack Obama when Eastwood laid out a critique of President Obama in a speech at the 2012 Republican party convention.

didn't know what to make of it." (Davis Backus, personal communication, April 6, 2014.) Backus indicated that, rather than from courses or from reading Tobin's written exchanges with Friedman, "most of what I got out of Tobin was just talking to him and Brainard; they were extremely generous with their time." Tobin had a research program that he was following," and "I would say what I lacked as a Ph.D. student was any sense of what this other stuff was… We [students] were just missing that line of thought. We just didn't understand what it was about." (David Backus, interview, April 16, 2014.)

Tobin's reticence on this score was evident in a discussion by Tobin and de Macedo (1983, p. 7) that took issue with the idea that "the commodity [goods] price level is an asset price, in the sense that its reciprocal is the real value of money." This was clearly a follow-up to the exchange that Friedman and Tobin had had in the November 1974 conference on monetarism. At that event, Friedman had contrasted his own position that the price of money was the inverse of the price level, with the "Keynesian or central banker" stand that the price of money was the interest rate.[25] This had provoked a reply from Tobin that Friedman's contrast reflected a superficial attitude to capital theory.[26] But the Tobin-de Maceo paper did not reference the earlier exchange or, indeed, mention Friedman at all.

Another echo of that Friedman/Tobin exchange on the interest rate occurred in Fischer's MIT graduate class. Shapiro recalled: "Stan Fischer was teaching; I guess it was the second year monetary course that Greg and I were sitting on as first-year students. And I can't remember exactly how Stan framed the question, but he basically asked, "What's the price of money?" And somehow, he made clear that the answers were either $1/P$ or the nominal interest rate. And he took a poll, and I was sitting in the front row, so I didn't see what everyone else was saying. So, apparently everyone in the class raised their hand for $1/P$ except me, (laughter) who, being a student of James Tobin, said R. Which kind of sums it [the Friedman/Tobin divide] up on theoretical points." (Matthew Shapiro, interview, November 14, 2013.)

James Tobin's tendency to avoid mentioning Friedman by name when articulating his critique of

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[25]See Friedman (1976a, p. 316). In *Newsweek* (August 23, 1976), the figure accompanying Friedman's column did have "The Price of Money" as the label for a figure depicting the short-term interest rate. But Friedman may not have chosen the label for this figure, which in any event also plotted the inflation rate. In Instructional Dynamics Economics Cassette Tape 110 (November 1, 1972), Friedman did slip at one point into calling the interest rate "the price of money." His preference was to think of the interest rate as the price of credit, although in Friedman (1971d, p. 855) he acknowledged that the interest rate's status as the opportunity cost of money gave some substance to viewing the interest rate as a price of money. See Nelson (2020b, Chapter 12) for further discussion.

[26] See Friedman (1976a, p. 316) and Tobin (1976, p. 335).

monetarism would again display itself in an article that appeared in the *Southern Economic Journal* in early 1978. The article was based on an address that Tobin had given in Atlanta on October 18, 1976 (a few days after Friedman's Nobel award had been announced). Tobin (1978a, p. 421) opened this address by indicating he would be concerned with the transmission mechanism—"the process by which monetary policies are transmitted into changes in expenditures for Gross National Product." In the subsequent exposition, he proclaimed that "[n]aïve calculations of Fisherian real rates of interest are very unreliable indicators of financial incentives for real investment" (Tobin, 1978a, p. 426). This was a theme that had been a basis for Tobin's criticisms of Friedman going back to 1966 (see Nelson, 2020b, Chapters 12 and 13). The quantitative implications of this criticism had been amplified in the 1970s—a period in which real interest rates on securities were persistently negative, while measures of q from the stock market suggested instead an extremely high real cost for U.S. businesses in raising funds for capital spending. So Tobin was concerned with making the argument that the economically-crucial real interest rates were high, during a time when Friedman was contending, in contrast, that the cost of borrowing was low in real terms. Yet Tobin's 1978 discussion of this matter made no mention of Friedman.

In the same address, Tobin made a different but also anti-monetarist point that "inside money is… more powerful stuff than outside money" with regard to the ramifications of monetary developments for aggregate demand (Tobin, 1978a, p. 434). Tobin was therefore maintaining a position he had taken in his review (Tobin, 1965) of the *Monetary History*. On that occasion, Tobin had argued that loan creation by commercial banks was more significant for aggregate demand than a commercial-bank or central-bank purchase of bonds because the loan's existence could directly give rise to flows, which might not otherwise occur, of private-sector spending on goods and services. Both Friedman and Schwartz had responded to this line of argument in their writings since 1965.[27] But Tobin's 1978 paper did not cite these writings and, in common with the rest of the article, did not mention Friedman at all. Friedman and Schwartz nevertheless recognized the paper as a critique of their own position on money and, in revising their *Monetary Trends* in the years ahead of its 1982 publication, would take the opportunity to include what was, in effect, a short reply to Tobin (1978a). Friedman and Schwartz noted that "Tobin does not refer to any empirical evidence to support such an interpretation of his conclusions," and they argued that Tobin's theoretical outline did not adequately distinguish between predictions for

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[27] See Friedman (1972a, p. 922) and Schwartz (1969, p. 9; p. 177 of 1987 reprint).

aggregate spending and predictions concerning the composition of spending.[28]

Tobin was less reticent during the late 1970s in naming Milton Friedman on those occasions when Tobin participated in economic debate in the public square. One instance in which naming Friedman proved unavoidable was Tobin's April 1977 joint appearance with Friedman on television. That debate is discussed later in this chapter. Another instance came in a *New York Times* op-ed by Tobin (November 20, 1977) that argued that Arthur Burns should not be reappointed head of the Federal Reserve Board. Whereas Tobin (1977b, p. 763) had made a general reference to "monetarist criticism from economists" as a factor framing recent years' Federal Reserve policy decisions, his op-ed was more specific that it was "the influence of Milton Friedman and other monetarists" that had led to the Federal Open Market Committee's monetary policy organized in terms of monetary targets. Tobin argued that, partly as a consequence, Burns had pursued too restrictive a monetary policy and should not be given a third term as Federal Reserve Chairman.

Tobin's piece did acknowledge, with considerable understatement, that Friedman himself was not "entirely satisfied" with the evolution of monetary policy under Burns. And in another article during the same period, Tobin (1977c) granted that Friedman did not think monetary policy was tight (and, indeed, had been warning of late that it was too loose). Tobin cited Friedman's *Newsweek* column of October 3, 1977, to this effect. From the column, Tobin (1977c) inferred that Friedman must believe the current rate of unemployment of about 7 percent was at or below the natural level. As it happens, modern estimates of potential output, constructed by the Congressional Budget Office, imply that the U.S. output gap actually was positive in 1977:Q3, so if this was Friedman's position, it was one supported in retrospect by the data. It is more likely, however, that Friedman—as of late 1977—simply believed that enough monetary stimulus was in the pipeline to produce an undershooting by unemployment of its natural rate in the period ahead. Indeed, U.S. unemployment kept declining well into 1979.[29]

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[28] Friedman and Schwartz (1982, p. 32). Elsewhere in *Monetary Trends*, however, Friedman and Schwartz (1982, p. 37) did make wholly positive remarks about another 1978 Tobin discussion (published as Tobin, 1980a), in which Tobin had taken issue with the emerging literature seeking microfoundations of money-holding in overlapping-generations models. They endorsed Tobin's position that the stock of money in such models served as a vehicle for household saving but did not have the more distinctive characteristics usually associated with monetary assets. McCallum (1983) endorsed Tobin's analysis for the same reason.

[29] In discussing the early period of the Carter Administration, Karl Brunner and Allan Meltzer would portray it as one in which monetary acceleration spilled over into inflation despite the existence of continuing slack (*Wall Street Journal*, February 7, 1983). Modern gap estimates suggest slack was mostly absent after 1977:Q2, so the coexistence of slack and rising inflation was probably less pronounced in 1977 than they suggested. However, the situation Brunner and Meltzer described is also consistent with representations of the monetarist view using a New

In Tobin's book *Asset Accumulation and Economic Activity*, which appeared in 1980 but was based on lectures given in January and March 1978 (Tobin, 1980b, p. vii), he did bring to an end at least one longstanding disagreement with Friedman. Tobin's opposition to the natural rate hypothesis had come to be perceived as being behind the times, and his retrogression in some of his analysis, such as Tobin and Buiter (1976), to the simple assumption of a constant price level came in for particular criticism (see Calvo, 1985, p. 95). In the early-1978 lectures Tobin indicated that he now accepted the natural rate hypothesis (see especially Tobin, 1980b, p. 39). This indication was consistent with, but more definitive than, his remark in Congressional testimony a year earlier: "There may not be a long-term trade off betweenn the two [unemployment and inflation]."[30] The endorsement of the natural rate hypothesis in Tobin's lectures would be noted upon when the lectures appeared in book form (see, for example, Lucas, 1981b, p. 560; Grossman, 1982, p. 136).

Tobin's acceptance of the Friedman-Phelps modification of the Phillips curve made itself felt also in his subsequent usage of the terminology given in the accelerationist and natural-rate approaches to the full-employment unemployment rate. Thus, Tobin (1983, pp. 512–513) referred to the nonaccelerating-inflation rate of unemployment (NAIRU), and Tobin (1985b, p. 606) referred to the natural rate of unemployment.[31] These analytical concessions did not, however, bring Tobin much closer at all to Friedman's perspective on inflation as far as practical policy prescriptions were concerned. Tobin continued to associate much of U.S. inflation with factors other than demand conditions: indeed, as of late 1977 he viewed the U.S. economy as featuring 17 percent excess capacity (Tobin, 1977c). Tobin perceived demand restriction as an inefficient and weak, and largely unnecessary, means of removing inflation—whose appropriate remedy he continued to see as, instead, largely lying in incomes policy.

Tobin's book of lectures also signaled that he was redirecting much of his energy in research debates away from criticism of Friedman and toward challenging rational expectations macroeconomics—whose arguments against activist stabilization policy and questioning of fiscal policy led Tobin (1980b, pp. 21, 36) to give it the label of "Mark II Monetarism." Consequently,

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Keynesian Phillips curve; for, in that case, inflation can pick up in anticipation of the output gap becoming positive in the period ahead.

[30] From Tobin's testimony of February 4, 1977, in Committee on Banking, Finance and Urban Affairs (1977a, p. 203).

[31] Tobin also used the latter term in Klamer (1984, p. 103) and in *The Economist*, April 27, 1985 (p. 26). In the latter discussion, Tobin emphasized that activist demand management could be compatible with a natural-rate framework.

when Robert Lucas published "Tobin and Monetarism: A Review Article," that highly-critical article focused primarily on the book's criticisms of optimizing rational-expectations models and of Ricardian equivalence (see Lucas, 1981). Lucas' book review did, however, pay tribute to the Friedman-Phelps development Phillips-curve analysis. And it blasted the empirical record of Keynesian economics, in a passage that Friedman would quote favorably in print in 1987.[32]

Tobin's disagreement with Friedman extended to microeconomic and public-policy issues. When others stressed the similarities of his and Friedman's welfare-reform proposals, Tobin would pour cold water on the notion of a convergence of views (see Nelson, 2020b, Chapter 13). Likewise, in the area of income taxation, Tobin stressed his differences with Friedman at a May 1976 conference. In the course of the conference proceedings, a Friedman *Newsweek* column (April 12, 1976) of the previous month—in which he had proposed that tax revenue and economic efficiency could be boosted by a large cut in income tax rates implemented in conjunction with a tightening of tax deductions—became a focus of discussion. In the resulting exchanges, Tobin concurred that the system of deductions needed overhaul but added: "If Friedman's choice—of either reduced rates or less revenue—were the only choice you offered me, I would support his proposal. I do not, however, believe that it is the only choice available." Friedman's proposal, Tobin suggested, would, in effect, "reward the people who have, by seeking and using all kinds of loopholes."[33]

Friedman was not an attendee of this conference. But it took place at a poignant location: the Hoover Institution, which Friedman would formally join about fifteen months after the conference took place. For, as indicated earlier in this chapter, in 1977 Friedman's years at the Hoover Institution began.

Friedman at the Hoover Institution

Biographical material supplied by Friedman over the years frequently gave his affiliation with the Hoover Institution in 1976—and sometimes specifically dated it to December of that year.[34] But this dating was not accurate. Soon after his relocation to California, Friedman did give a talk

[32] See Friedman (1987a, p. 13).
[33] From Tobin's remarks in Campbell (1977, p. 174).
[34] For example, the biographical sketch in Friedman (1976f) incorrectly implied that he taught at the University of Chicago while holding his Senior Research Fellow post at the Hoover Institution, and that in Friedman (1977i, p. 11) gave him as starting his Hoover Institution post in December 1976. Friedman's entry in Europa Publications Limited (1986, p. 822) described him as "Sr. Research Fellow, Hoover Inst. of Stanford Univ., 1976–."

at the Hoover Institution in January 1977 on the topic of his receipt of the Nobel award in economics.[35] But this contribution had been part of his participation in a conference. Friedman's employment proper at the Hoover Institution did not begin at that time, and he was not located at the Hoover Institution in the subsequent eight months.[36] His position as Senior Research Fellow at the Hoover Institution formally began only in the first (that is, the fall) semester of the 1977/1978 academic year. This would be a post he would hold for the rest of his life.

Friedman received his own spacious office in the Hoover Institution's building, with his University of Chicago secretary of the previous five years, Gloria Valentine, continuing as his secretary throughout Friedman's Hoover Institution era and being located in the outer office. Next along from Friedman's office, Rose Friedman also had her own permanent office (*Straits Times* (Singapore), October 18, 1980; Gloria Valentine, interview, April 1, 2013).

From the beginning, however, Friedman intended the Hoover Institution to be a base of operations, rather than a place at which he could be expected to be found on a five-day-a-week basis. Gloria Valentine recalled (interview, April 1, 2013): "There were sometimes meetings going on or seminars that he wanted to attend, and he would go to those; and he usually tried to get to the office once a week." The fact that Milton and Rose Friedman had moved into an apartment in San Francisco was significant here. Gary Becker (interview, December 13, 2013) noted with regard to Friedman's physical presence at the Hoover Institution that "he was not around a lot, because he lived in San Francisco, and if you know that area, San Francisco's not so close to Hoover: it's like a 45-minute to an hour drive, depending on traffic. So he was not there on a daily basis—not at all."[37]

But though Friedman was not as constant a presence at the Hoover Institution as his new affiliation might have suggested, it was certainly true that his move to Northern California's Bay Area was permanent. It signified a break from Friedman's former attitude which, Michael Keran

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[35] See Friedman (1977j).

[36] During his time at the Federal Reserve Bank of San Francisco, Friedman reluctantly agreed to pay one visit to the Hoover Institution, to see his new office. "I had to coax Professor Friedman to come down and see his new office at Hoover. The inducement was to see what a very nice office he had, far better than his University of Chicago office. He came to see the office just before he went to Vermont for the spring and summer [1977]." (Gloria Valentine, personal communication, October 4, 2014.)

[37] The *Wall Street Journal* (July 17, 1978, p. 27) referred to the "Hoover Institution in Palo Alto" as being "nearby" the Friedmans' apartment in San Francisco and suggested that Friedman's commute was short. In fact, the Hoover Institution was not close to the Friedmans' apartment (nor, incidentally, was the Hoover Institution located in Palo Alto, though it was near that city), and the commute was therefore not short, even for an automobile driver who exceeded the legal speed limits (as Friedman often did).

recalled (interview, March 7, 2013), was "that this is Lotus Land and you'll never get any real work done if you come to the West Coast." Richard Muth, who had served as an associate professor at University of Chicago's business school in 1959−1964, had been a professor of economics at Stanford University since 1970 (Blaug and Sturges, 1983, p. 279). Muth recalled of Friedman's own move, "it was funny because, before that time, whenever he saw me, when I used to go to Chicago but I'd gone to Stanford, he would ask: "How are things in Lotus Land?" So, when he first moved out here, I asked him the same question." (Richard Muth, interview, May 20, 2015.)

Stanford University and the Hoover Institution

The fact that Muth and Friedman were once more affiliated with the same university reflected the fact that Friedman's move to the Hoover Institution implied a move to Stanford University as well. In a form letter sent to Friedman's correspondents notifying them of his change of address, his new address was given as "The Hoover Institution[,] Stanford University[,] Stanford, CA. 94305."[38] And when Friedman's January 1977 talk on his Nobel award was published by the Hoover Institution, the booklet's cover identified that body as "Hoover Institution • Stanford University." Indeed, a wire report on the announcement of Friedman's appointment had highlighted the Stanford University part of his new affiliation, as the story was headlined: "Friedman To Take Post At Stanford" (*Christian Science Monitor* (Boston), November 4, 1976).

These descriptions were totally accurate. Hoover Institution was indeed part of Stanford University, and, consistent with this, Friedman was issued a university identification card. Nevertheless, it deserves underscoring that, particularly in the era of the 1970s, the Hoover Institution was perceived as somewhat separate from the university. And Friedman's appointment was not a joint one. Unlike such subsequent leading macroeconomists at the university like Michael Boskin, Robert Hall, and John Taylor, he did not have a dual Department of Economics/Hoover Institution affiliation. Indeed, in a sharp reaction to a hostile profile of him that appeared in the magazine *Human Behavior*, Friedman included among his list of the article's errors the description of himself as "a professor of economics at Stanford." "I have no formal connection with the Department of Economics at Stanford," Friedman pointed out (*Human Behavior*, March 1979, p. 10). It was accurate to call him Professor Friedman, but only because he held an emeritus professorship at the University of Chicago: "an inactive and

[38] Unsigned form letter from Milton Friedman's office, dated December 15, 1976, in Arthur Burns papers, Gerald Ford Presidential Library.

uncompensated" professorship, he noted. He did not owe the title to his Stanford University affiliation, as the Hoover Institution did not confer professorships.[39]

Stanford University's Department of Economics members of the late 1970s and the 1980s, like Paul Evans (a member of the department in 1976–1984) and Kenneth Arrow (who rejoined Stanford University in 1979), did not remember Friedman attending the department's seminars or striking up much of a relationship with the overall department (Paul Evans, interview, February 26, 2013; Kenneth Arrow, interview, December 7, 2013). Indeed, as of the late 1970s, Friedman and the Department of Economics at Stanford University had had a background that was not very conducive to a close relationship: "he had a checkered history with them," Gary Becker recalled (interview, December 13, 2013). In particular, when the department had sought to hire Friedman in the mid-1960s, it had turned down Friedman's request that an offer be made to Becker as well. This inaction had helped sour Friedman's attitude to the offer, which he declined.[40]

Indeed, it warrants emphasis that, when Friedman joined the Hoover Institution, its relationship with the rest of the university when Friedman joined it was far different from that prevailing in the first two decades of the twenty-first century. In 1977 and 1978, it would still be some time before a host of senior members of the Department of Economics held joint affiliations with the Hoover Institution or when, as now, graduate students from the economics department are a regular sight in the Hoover Institution's corridors.

In the 1960s and 1970s, and into the 1980s, the Hoover Institution was much more self-contained, for the most part lacking extensive interaction with the broader university. This tendency was reinforced by the fact that many in the regular social-science departments of the university viewed the institution with suspicion, because ideological persuasion figured explicitly in the basis for its existence (*Wall Street Journal*, June 15, 1984). To be sure, the tensions between the Hoover Institution and the wider university were more severe for social sciences other than economics: for example, the Hoover Institution's foreign-policy and national-

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[39] In addition, although Friedman sometimes made statements (see, for example, *Glasgow Herald*, October 20, 1980, and *Australian Business Monthly*, October 1993, p. 54) liable to create the impression that his work still involved grading papers or giving classes, neither of these was part of his duties after 1976 (aside from guest lectures at Stanford University, in the case of teaching). (The *Chicago Tribune* of April 2, 1978*a*, likewise said of Friedman that "he now teaches at Stanford University," which he did not. Similarly, three years later the *Boston Globe* of April 1, 1981, p. 39, incorrectly stated that Friedman "now teaches at the Hoover Institution.")
[40] For more details, see Nelson (2020b, Chapter 12).

security specialists typically took a more straightforwardly "hawkish" posture on United States/Soviet Union relations than that usually found among international-affairs experts in the political-science and history departments of Stanford University and elsewhere. But the Department of Economics' membership and Hoover Institution's economists also tended to have something of an arms-length relationship until the 1980s.

The relationship between the university and the Hoover Institution improved after the 1970s. Even in 1987, however, Friedman described the relationship between the overall university and the institution as one of "armed neutrality"—and did not, himself, stimulate good relations by adding that he would like the Hoover Institution to have "nothing to do with Stanford" (*Stanford Review*, November 1987). The more convivial relationship between the university and the institution, especially between its economists, that developed in later years make such statements now seem very jarring.

Notwithstanding the tension between Hoover Institution and the university in Friedman's first decade or so there, the number of economists with joint Hoover Institution/Department of Economics affiliations gave Friedman some degree of connection with the department over time. So did the presence of old friends in the department like Richard Muth: "I saw as much of him during that period, I think, as I ever had." (Richard Muth, interview, May 20, 2015.) And through such means as their visiting the Hoover Institution and their attending conferences that it held, Friedman developed extensive interactions with some more junior economists whose permanent Stanford University affiliations were not with the Hoover Institution. Notable among these members of the younger generation was Ben Bernanke—whose regular affiliation from 1979 to 1985 was with Stanford University's business school, but who visited the Hoover Institution in the 1982/1983 year. The upshot of these interactions was that, by the time the Friedmans wrote their memoirs, Milton Friedman could cite the proximity to the Stanford University economics department as an advantage of his Hoover Institution affiliation.[41]

Friedman's attendance of Department of Economics seminars remained extremely rare, however. Thomas Sargent was based at Hoover Institution for part or most of each year from 1985 to 2002 and was, in addition, closely related to Stanford University's economics department, sometimes with a formal affiliation.[42] With regard to Friedman's attendance at the economics department's seminars, Sargent observed, "I think I saw him at one seminar." However, Sargent noted that,

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[41] Friedman and Friedman (1998, p. 563).
[42] See *http://www.tomsargent.com/personal/resume.pdf*.

additionally, "we had seminars at Hoover; he came to some of those." (Thomas Sargent, interview, January 24, 2014.)[43] Indeed, though Friedman himself moved further toward public policy and away from research in his Hoover Institution years—especially after his and Anna Schwartz's completion of their final book, *Monetary Trends*—he continued to supply comments on much of the monetary research sent to him by correspondents. All told, Friedman's presence at the institution contributed to a shift noted by Michael Boskin, in which the Hoover Institution's contribution to research had improved over the previous decade, from being in a poor state in 1974 (*Wall Street Journal*, June 15, 1984).

During the period in which he accustomed himself to the new environment in 1977 and 1978, Friedman's appearances at the Hoover Institution were somewhat more frequent than would later be the case. An incentive to come into the office in that period lay in the fact that, during the 1977/1978 academic year, the Hoover Institution had a large amount of visiting researchers who shared a University of Chicago or monetary-economics background with Friedman. Among these were Karl Brunner and Allan Meltzer, as well as Robert Barro and Michael Darby.[44]

The Carter presidency and U.S. macroeconomic policy in 1977–1978

With regard to the national scene, at the start of 1977 Friedman entertained some hopes about the economic policy likely to be pursued by Jimmy Carter, who would take office as U.S. president on January 20. As noted in Chapter 5, during the 1976 presidential election campaign Friedman had indicated he preferred Ford to Carter (and Ronald Reagan to either of them). Furthermore, Carter's principal economic adviser in that campaign had been Lawrence Klein, the pioneering Keynesian model-builder and past sparring partner of Friedman. Under Klein—and in contrast to Friedman's own perception of the appropriate prescription for the U.S. economy—Carter's presidential campaign had put considerable emphasis on the need for economic stimulus. Klein had specifically indicated: "We feel the Fed has been miserly in dealing out money. There

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[43] John Maynard Keynes' life played a role in giving rise to an instance in which Friedman involved himself in the economics seminars of Stanford University's economics department. For the occasion in October 1991 when Keynes' biographer Robert Skidelsky was visiting the Hoover Institution, Friedman contacted Stanford University's economics department to encourage the scheduling of an additional Skidelsky talk, in its own seminar series (Gloria Valentine, interview, April 1, 2013; Lord (Robert) Skidelsky, interview, November 26, 2013).

[44] Brunner (1978, p. 649) stated: "The first draft of the paper was prepared [during Brunner's period] as a visitor at the Hoover Institution (Stanford University) during the winter 1977−8." Meltzer was also at the Hoover Institution in this year (Allan Meltzer, interview, April 21, 2013; Committee on Banking, Finance and Urban Affairs, 1978c, pp. 107−108). For Barro's and Darby's positions as Hoover Institution in National Fellow in 1977/1978, see Barro (1978, p. 569), as well as their respective online CVs: *https://scholar.harvard.edu/files/barro/files/vita_0518.pdf?m=1525874087* and *https://www.anderson.ucla.edu/Documents/areas/fac/strategy/MICHAEL%20R.%20DARBY%20CV.pdf*.

should be a more expansionary policy." (*The Sunday Sun* (Baltimore), July 25, 1976, p. K9.)

Shortly after the election, Friedman had observed in a television interview: "I believe that the best thing for this country will be if Mr. Carter can rise above his advisers, including Larry Klein, who I like and respect but don't agree with."[45] Around the same time, Friedman was under the impression that Arthur Okun would likely figure among the administration's economic personnel: he welcomed this, on the grounds that Okun was "much less extreme [and] much more balanced" than other Keynesians such as Lawrence Klein (Instructional Dynamics Economics Cassette Tape 202, November 1976, Part 1). In the event, Okun did not join the administration, while Klein did not take an official post either.

Friedman would point to someone other than himself whose economic prescriptions Carter should heed. This was the U.K. Prime Minister James Callaghan, who had declared in a Labour party conference speech of September 28, 1976: "We used to think that you could just spend your way out of a recession and increase employment by cutting taxes and boosting Government spending. I tell you, in all candor, that that option no longer exists, and that insofar as it ever did exist, it only worked by injecting bigger doses of inflation into the economy, followed by higher levels of unemployment as the next step." Friedman quoted this statement many times (see Nelson, 2009, p. 84), including in his December 1976 Nobel lecture.[46]

The notion that Callaghan had provided words for Carter to live by informed the title of Friedman's *Newsweek* column of December 6, 1976, titled "To Jimmy from James."[47] In that column, Friedman provided what turned out to be a prophetic characterization of what would ensue if the United States in 1977 pursued expansionary economic measures: "If Mr. Carter tries to put his advisers' policies into effect and succeeds in doing so—including getting the Federal Reserve System to speed up substantially the rate of monetary growth—there might be a sudden spurt in the economy and a quick reduction in unemployment. However, these good results would be temporary. By 1978 or 1979, inflation would be back in double digits and wage and

[45] *Wall Street Week*, Maryland Public Television, November 5, 1976, p. 11 of transcript. The transcript of this appearance was largely reproduced at the time in the *American Banker* (November 22, 1976). In addition, the host of *Wall Street Week*, Louis Rukeyser, used his syndicated newspaper column to summarize Friedman's position on Carter and his advisers (see *The Evening Bulletin* (Philadelphia), November 15, 1976).

[46] See Friedman (1977c, p. 460).

[47] The title carried the incorrect implication that the U.K. prime minister was more formal in his rendering of his first name than was President-elect Carter. In fact, Prime Minister Callaghan was widely referred to in his home country as Jim Callaghan. (Friedman would belatedly refer to Callaghan as "Jim Callaghan" in Friedman, 1984b, p. 29.)

price controls would be in place or in contemplation. By 1980 at the latest, unemployment would be rising sharply. As Machiavelli might say: what a way to face the 1980 election!"

In a talk he gave in Chicago during this period, Friedman suggested that the U.S. economy would face "deep and real trouble" unless Carter proved to have "the ability to rise above his campaign promises" (*Chicago Tribune*, November 18, 1976).

Friedman was to be disappointed by the fact that the Carter Administration actually pressed ahead during 1977 with advocacy of stimulus to aggregate demand. Although the administration did abandon its early proposal for a tax rebate (see Section II below), its economic policy continued to be oriented toward prompt elimination of the perceived high level of economic slack in the economy.

Another economic move on Carter's part was the announcement, on April 15, 1977, of a variety of measures intended to reduce inflation from 6 percent to 4 percent by the end of 1979 (*Boston Globe*, April 16, 1977; Nelson, 2005). As this package consisted of a series of measures intended to alter specific prices and involved no monetary policy action, it might have been expected to provoke a wholly negative reaction from Friedman. But Friedman's response to the package—which he gave in the course of a debate on April 18 with James Tobin on public television's *MacNeil/Lehrer Report*—was disarmingly benign. Friedman noted that the package was, in essence, directed at relative prices and could not be expected to achieve its stated aim of reducing inflation. But, evidently wearing his "public choice theory" hat (see the next chapter), Friedman took the president's team as understanding the monetary nature of inflation but as obliged to announce an inflation package for public-relations purposes. The president had not included monetary measures in his package, Friedman said, but that signified a recognition that the executive branch lacked authority over monetary policy. Furthermore, Friedman stressed, the fact that Carter had not proposed broad wage and price controls was to be applauded. Accordingly, Friedman observed: "I want to commend President Carter." In addition, Friedman gave a backhanded compliment to the inflation package, which was "excellent because it tries to little to do so little of the wrong things."[48] To Friedman, the package amounted to window dressing and so, though not beneficial, it was largely innocuous.

In making this judgment, Friedman took something for granted that he often did in analyzing

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[48] *MacNeil/Lehrer Report*, PBS, April 18, 1977 (*American Banker*. April 21, 1977, p. 6).

Arthur Burns' statements.[49] This was that policymakers ascribed the same degree of importance to monetary policy in inflation's determination that he did. As has already been indicated in previous chapters, by the mid-1970s, Friedman regarded the monetary view of inflation as so strongly confirmed that he tended to assume that others accepted that view. For example, in April 1978, when confronted with a recent analysis of inflation by Michael Blumenthal, U.S. Secretary of the Treasury, that seemed to line up with wage-push views of inflation, Friedman proclaimed that "Secretary Blumenthal knows as well as you and I do that inflation does not come from trade unions."[50]

Even by this time, however, considerable evidence had accumulated in support of the conclusion that the Carter Administration did not adhere to a monetary view of inflation. In April 1978, after a year in which inflation had failed to fall below 6 percent, President Carter—who once described inflation as the result of "attitudes and habits" (quoted in Kemp, 1979, p. 97)—gave a new major speech on inflation. The president's emphasis continued to be on a nonmonetary approach to disinflation. Carter stated: "Reducing the inflation rate will not be easy, and it will not come overnight. There are no easy answers. We will not solve inflation by increasing unemployment. We will not impose wage and price controls. We will work with measures that avoid both extremes." (*Kansas City Star* (Missouri), April 11, 1978, p. 2A.)

The "measures" that Carter actually advanced in this talk consisted of wage guidelines, alongside an indication that firms should keep price increases in line with the cost increases implied by the wage guidelines. This package, of course, amounted to a revival of a Kennedy-Johnson-era measure against inflation. A year earlier, in his television appearance alongside Friedman, Tobin had lamented Carter's failure to introduce guidelines in his April 1977 inflation announcement. As Carter later would, Tobin had portrayed guidelines as an alternative to a strategy of demand restriction: "guideposts, or some other direct attempt to reduce the inflation rate and to break the hard-core momentum of the wage-price spiral that we've inherited from the past, would be preferable to the solution of keeping the economy stagnant, with… a lot of excess capacity for a prolonged period of time."[51] In reply, Friedman disputed Tobin's contention that guideposts had

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[49] And as he would again—for example, in *Newsweek*, October 3, 1977, when Friedman discussed a recent speech by Chairman Burns.
[50] *Milton Friedman Speaks*, Episode 8, "Free Trade: Producer Vs. Consumer," April 27, 1978, p. 28 of transcript.
[51] *MacNeil/Lehrer Report*, PBS, April 18, 1977 (*American Banker*. April 21, 1977, p. 6). Similarly, earlier in the year, Tobin had qualified his tentative acknowledgment of the long-run relevance of natural-rate type results both by affirmation of the standard position (shared by Friedman) that there was a short-run unemployment/inflation tradeoff and by the contention (rejected by Friedman) that "there is another trade-off between trying to reduce inflation by incomes policies of various kinds as against trying to reduce it by holding the economy down for a long period of

worked in the United States in the 1960s. Friedman went on to provide a challenge: Could Tobin nominate another historical instance in which incomes policy had, he judged, been effective? Tobin declined to accept the challenge, prompting Friedman to remark that "you're evading my question, Jim. I want to know if you can cite a single example of a successful wage-and-price guideline policy, other than the one that you were involved in." Tobin insisted that the 1960s U.S. precedent was sufficient: "That's the one I want to copy."[52]

By April 1978, however, Carter's own embrace of guidelines was motivated by the perceived success of a different example: recent years' wage-oriented incomes policy in the United Kingdom. Ironically, therefore, Carter did adhere to Friedman's suggestion that he should pattern his approach on that of Prime Minister James Callaghan. But instead of taking a leaf from Callaghan's monetarist-inspired September 1976 *speech* on aggregate demand management, Carter was emulating Callaghan's *actual practice* of taking a nonmonetary approach to the control of inflation. Judgments concerning the success of Callaghan's incomes policy would be revised drastically when U.K. nominal wage growth and inflation surged in early 1979. As of April 1978, however, a look at the U.K. experience might have suggested that the decline in inflation in the past couple of years was testament to the value of incomes policies.

Friedman's reaction to the administration's April 1978 measures was pointed.[53] "President Carter's [anti-]inflationary package is like Hamlet without the Prince of Denmark… Inflation is not caused by trade unions, business interest[s], consumers, or oil sheiks… Inflation is a disease that has been around 1,000 years and, in all that period, only one medicine to cure inflation has been found: to hold down the rate of monetary growth and hold down governmental spending." (*Manhattan Mercury* (Kansas), April 27, 1978, p. A6.) The president's package, he later added, was "a reworked ball of nonsense" (*Bluefield Daily Telegraph* (West Virginia), May 3, 1978). In September 1978, Friedman would express frustration at Carter's failure to make reference to monetary policy on the occasions when the president discussed inflation—a tendency that Friedman suggested set Carter apart from every other "high government official in any Western country."[54]

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time with low rates of monetary growth" (February 4, 1977, testimony, in Committee on Banking, Housing and Finance, U.S. House of Representatives, 1977b, p. 203).

[52] *MacNeil/Lehrer Report*, PBS, April 18, 1977 (*American Banker*. April 21, 1977, p. 7).

[53] The administration had earlier in this year foreshadowed that it would be focusing its approach to inflation on voluntary wage-price guidelines—an approach Friedman had criticized as window dressing" (*Chicago Daily News*, February 21, 1978).

[54] Friedman (1978c, p. R–182).

This tendency continued in the following month. In October 1978, with U.S. CPI inflation approaching 9 percent, Carter announced yet another anti-inflation package, in which he broadened his wage-price guideline proposals. The deterioration in inflation performance over the previous eighteen months was reflected in the fact that, whereas in April 1977 Carter had sought to get inflation down to 4 percent in 1979, now he merely hoped to bring the inflation rate in 1979 back down to about 6 to 6.5 percent (*Financial Times* (London), October 25, 1978).

The devices in Carter's October 1978 package advanced to achieve this goal included voluntary wage guidelines, voluntary price guidelines, and a "real wage insurance" scheme (*Omaha World Herald* (Nebraska), October 25, 1978). The last of these proposals, which the administration continued to promote into 1979 (see Committee on Ways and Means, U.S. House of Representatives, 1979, and Romer and Romer, 2002b, p. 62), would provide tax relief, for those employees who adhered to the wage guidelines, if realized inflation proved to be high in relation to the guidelines. It was therefore a variant of the "tax-based incomes policy" idea of which Friedman was so derisive (see Chapter 5 above). As it required Congressional approval that was not forthcoming, the real-wage-insurance idea was not enacted (Biven, 2002, p. 189). However, the Carter administration continued its wage-price guidelines beyond 1978. Friedman was concerned that, ultimately, the administration's advocacy of guidelines could "soften the ground for another dose of wage and price controls" of the mandatory variety (*Chicago Daily News*, February 21, 1978).

II. ISSUES RELATED TO DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1977–1978

CROWDING OUT AND RICARDIAN EQUIVALENCE

Underlying the Carter Administration's approach to inflation in 1977−1978 was the view that high inflation did not reflect a problem of excessive aggregate demand, nor did the cure for inflation lie in restriction of aggregate demand. Indeed, as the administration saw things in these years—particularly during 1977—the U.S. economy had too little, not too much, aggregate demand. This attitude shaped the administration's early moves in the area of fiscal policy.

As of late 1976, Friedman seemed confident that Carter would discard his pre-election commitments to introduce stimulative measures. Friedman thought he observed in Carter's post-election press conference the start of a "gradual withdrawal from his pre-election promises" to

stimulate the economy—a process that he believed would see a "neatly-staged retreat from those positions" (Instructional Dynamics Economics Cassette Tape 203, November 1976, Part 2). As for the policies that Carter should actually follow, Friedman stated: "The advice that I would give to Jimmy Carter is exactly the advice I would give to Gerald Ford, and exactly the advice that I would give to anybody else: What this country needs is less government spending, not more; what we need is to let people spend more of their own money instead of first sending it to Washington, taking a commission off and then sending a small part back..." (*Donahue*, NBC, November 24, 1976.) In particular, Friedman denied the need for stimulus, arguing that the economy was "pretty strong… [W]e're in a good expansion."[55]

On taking office, however, the Carter Administration proceeded with plans to stimulate the economy. In a discussion of the history of U.S. fiscal measures designed to affect aggregate demand, Romer and Romer (2010, p. 764) conclude: "Tax actions… were common in the early postwar era but virtually disappeared after 1975." This is an accurate description of *implemented* tax measures in the quarter-century after 1975. However, it is notable that, early in its existence, the Carter Administration *attempted* to enact a prominent demand-stimulating tax measure.[56] Shortly after taking office, Carter proposed implementing a $50-per-household tax rebate. This rebate (more precisely, a transfer, one proposed to be disbursed even to those who lacked any tax liability) was advanced to be put into effect in the second quarter of 1977 (*New Republic*, May 7, 1977, p. 11).

Friedman had poured scorn in his first *Newsweek* column for 1977 on "the implicit assumption of so much current talk… that a tax cut or an increase in government spending is 'stimulative'" (*Newsweek*, January 10, 1977, p. 59). When the rebate proposal was formally advanced a few weeks later, Friedman was, not surprisingly, strongly negative in his reaction (*Los Angeles Times*, February 10, 1977, p. 21; *New York Times*, February 28, 1977; *U.S. News and World Report*, March 7, 1977).[57]

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[55] *Wall Street Week*, Maryland Public Television, November 5, 1976 (*American Banker*, November 22, 1976, p. 6).
[56] This measure—which, as discussed below, was not enacted—aside, the first two years of the Carter Administration did see implementation of a couple of packages that contained some tax-cutting element and that were motivated in part by a wish to stimulate aggregate demand. See the coverage of the Tax Reduction and Simplification Act of 1977 (enacted in May 1977) in Romer and Romer (2009, pp. 61−62) and of the Revenue Act of 1978 (enacted in November 1978) in Biven (2002, p. 199), Romer and Romer (2009, pp. 64−65) and Eizenstat (2018, pp. 314−320).
[57] He briefly covered the rebate plan in his *Newsweek* column (April 11, 1977). Friedman also criticized the plan on the daytime talk program *Dinah*! (Dinah Shore show) on March 23, 1977.

This negative reaction had two branches: one that pertained to a case in which the tax rebate was monetized; the other that referred to the likely effects of the rebate if it turned out to be a "pure" fiscal policy measure—that is, if it was not accompanied by extra money creation.

As far as the monetary side of the tax proposal was concerned, it is important to bear in mind that—at this stage of his understanding of U.S. historical regularities—Friedman still regarded the typical pattern as one in which budget deficits were accommodated by monetary policy (*Financial Times* (London), January 6, 1977; *St. Louis Globe-Democrat*, December 16, 1977; *Newsweek*, April 24, 1978). This background made him think it likely that a rebate *would* prompt an increase in the U.S. money stock. That this was not an unreasonable expectation, whatever the historical evidence said, was underlined by the fact that Federal Reserve Chairman Arthur Burns—although he was critical of the rebate proposal—indicated that the Federal Reserve might allow the money stock to rise, on a one-time basis, with the tax rebate (*Daily News* (New York), March 23, 1977). Furthermore, Burns had confirmed that he intended to follow policies consistent with Carter's announced goals of 6 percent real GNP growth in 1977 together with an unemployment rate of around 6 to 6.5 percent by year-end, compared with the value of about 7.5 to 8 percent believed to be prevailing when Carter was elected (*Omaha World Herald* (Nebraska), November 24, 1976; *Los Angeles Times*, February 4, 1977). In the event that it was monetized, Friedman said, the tax rebate would be inflationary.

For the case in which the federal borrowing arising from the rebate was not monetized, Friedman saw the measure as unlikely to be effective in stimulating aggregate demand. This was, of course, the same position that he had taken on previous instances when the federal government had attempted countercyclical fiscal measures—including the Ford Administration's tax rebate in 1975.

The way in which Friedman in 1977 articulated his skepticism about fiscal policy is notable, in light of what has subsequently been said of his views. In a February 2010 blog entry, J. Bradford DeLong declared that while "Friedman was a critic of fiscal policy," it was a violation of "Milton Friedman's model" to argue that the ineffectiveness of fiscal policy reflected the notion that attempts at fiscal stimulus amounted to "taking money from one place and giving it to another place."[58] This confident statement by DeLong should be juxtaposed against Friedman's actual words, given in response to the Carter rebate proposal: "How can the government

[58] Bradford DeLong, J. Bradford DeLong blog, February 22, 2010.

stimulate the economy by taking money out of one pocket of the public and putting it into another pocket?" (*U.S. News and World Report*, March 7, 1977.)[59]

Crowding out

What were the economic mechanisms that Friedman saw as working against the effectiveness of fiscal-stimulus measures like a tax rebate? The answer to this question was something on which his views were shifting in the late 1970s. The empirical evidence had long convinced Friedman that fiscal measures on their own were, by and large, ineffective in influencing aggregate demand. But his conception of the fundamental reasons why this was the case was changing over this period—from one emphasizing crowding out, to one stressing Ricardian equivalence.

"Crowding out" summarized the process in which fiscal actions that boosted disposable income or public spending had their effect on aggregate demand largely offset by interest-rate-raising effects of the accompanying increase in government borrowing and the resulting downward pressure on interest-elastic private spending. By 1977, crowding out was a well-established part of public discourse on macroeconomic management in the United States. For example, Snellings (1976, p. 6) noted: "Some well-informed observers fear the 'crowding out' of private borrowers that was so much-talked-about last year." Much of the credit for the popularization of the concept went to President Ford's Secretary of the Treasury, William Simon, who had triggered media and Congressional discussions of crowding out by suggesting in 1975 that it was a likely implication of actual U.S. federal deficits.[60]

Ahead of Simon, however, Friedman had promoted the notion of crowding out for many years in research and public-policy discussions. For example, in November 1968, he observed: "The state of the government budget has a considerable effect on interest rates. If the federal government runs a large deficit, that means the government has to borrow in the market, which

[59] Friedman employed similar imagery in *Los Angeles Times*, February 10, 1977, p. 21, and in *New Guard*, April 1977, p. 6.
[60] For example, a *Wall Street Journal* editorial (March 13, 1975) titled "Crowding Out" stated that Simon "sounded the first guns in this debate," while financial columnist Sylvia Porter contended (*Detroit Free Press*, August 20, 1976) that the crowding-out "theory [was] promoted by Treasury secretary William Simon." Later, a textbook discussion (Wonnacott and Wonnacott, 1979, p. 255) associated Simon with a belief in the practical relevance of crowding out, while Silk (1982, p. 237) even asserted that Simon "became famous for inventing 'crowding out.'" Simon's own principal exposition of crowding out was in testimony of March 3, 1975, in Committee on Ways and Means, U.S. House of Representatives (1975, pp. 24–25). See also his op-ed in *Washington Star* (Washington, D.C.), May 4, 1975.

raises the demand for loanable funds and so tends to raise interest rates."[61] It was against the background of such statements that Benjamin Friedman had occasion in 1978 to refer to "the 'portfolio crowding out' emphasized by Milton Friedman."[62]

In discussing the most recent calls for fiscal activism, Friedman's *Newsweek* column of January 10, 1977, cited the failure of the 1968 tax increase to stop inflation as evidence of the unreliability of fiscal measures in affecting aggregate demand. That 1968 episode had, indeed, been a considerable watershed in creating support among economists for Friedman's skepticism about fiscal policy. But, although retrospectives tended to point to the temporary character of the fiscal measure as the reason for its apparent failure, Friedman himself had in 1968 had appealed to reverse-crowding-out, rather than his own permanent income hypothesis, as the basis for predicting that the tax increase would fail to brake aggregate demand.[63]

Just as in his analysis of the 1968 tax surcharge, Friedman's critiques of President Carter's proposed tax rebate largely cast its likely ineffectiveness in terms of crowding-out mechanisms. He granted that the rebate would, if enacted, lead to "extra expenditures by consumers" (*U.S. News and World Report*, March 7, 1977). But the associated government borrowing, he contended, would lower aggregate U.S. saving (*Newsweek*, April 11, 1977) and reduce U.S. businesses' investment expenditures (Instructional Dynamics Economics Cassette Tape 206, January 1977, Part 1).

The permanent-income/Ricardian equivalence alternative

In 1977, just as in 1968, it fell to a Keynesian, Robert Eisner, rather than to Friedman, to emphasize the permanent income hypothesis, rather than crowding out, as the reason for the likely ineffectiveness of the proposed fiscal measure. On February 7, 1977, in testifying about the rebate proposal to the House of Representatives' Committee on Ways and Means, Eisner

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[61] From his remarks in Friedman and Heller (1969, p. 50). Early public statements in which Friedman emphasized the upward pressure on interest rates generated by unmonetized fiscal deficits included his remarks in Samuelson and others (1952, p. 389; also in Joint Committee on the Economic Report, U.S. Congress, 1952a, p. 1299) and his Congressional testimony of January 31, 1952 (see Joint Committee on Economic Report, U.S. Congress, 1952b, especially pp. 334–337) and March 1952 (see Joint Committee on Economic Report, U.S. Congress, 1952c, p. 690).
[62] B.M. Friedman (1978a, p. 598).
[63] In particular, he did so in his 1967 discussions of the matter, which are cited and discussed in Nelson (2020b, Chapter 13). In addition, in Friedman and Heller (1969, p. 50), he said that the decline in interest rates following the introduction of the 1968 tax surcharge was "precisely what we had predicted and what our analysis leads us to predict."

observed: "I have to recall to you the theory of Milton Friedman, who points out in his permanent income hypothesis that people's spending is most influenced by the long run, or permanent, income, and not by temporary income changes."[64] Eisner noted that the 1968 tax surcharge had been associated with a reduction in the saving ratio and the 1975 tax rebate by a surge in the ratio—patterns that suggested that consumers rode out the changes in taxes.[65]

Buttressing Eisner's position was something that Friedman acknowledged on television in April 1977: the fact that "interest rates are relatively low," as they were in nominal terms (compared with the recent past) and especially real terms (compared with most of twentieth-century U.S. history)—in the mid-1970s. He acknowledged that this fact created a problem for the use of crowding-out story as the explanation for why the fiscal deficits already recorded in the mid-1970s might not have been stimulative, on net, for private aggregate demand.[66]

Thus, as between portfolio crowding out and the permanent income hypothesis, the latter seemed to have considerably more appeal by the late 1970s in understanding the limitations of taxes as a demand-management tool. Furthermore, in 1978, the permanent income hypothesis was given a modern cast by Robert Hall's (1978b) derivation of the consumption function in an infinite-horizon model. Although Hall treated real interest rates as constant in his analysis, research of around the same vintage by Boskin (1978) provided evidence that saving (and so consumption) significantly depended on the real interest rate—a position that Friedman had long advocated.[67] Thus, this period saw the basic ingredients of the New Keynesian IS function—a permanent-income infinite-horizon consumer model and interest-elastic consumption (as well as investment) expenditures—laid out in research studies. This approach to modeling private behavior affirms that measures such as tax rebates will fail to affect aggregate demand. And it does so because of the permanent income hypothesis, and in particular a particular corollary of that hypothesis: what in the late 1970s came to be known as Ricardian equivalence.

Moving toward acceptance of Ricardian equivalence

The puzzle is why, as the architect of the permanent income hypothesis, Friedman himself did

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[64] In Committee on Ways and Means (1977, p. 498).
[65] Similarly, Franco Modigliani, in testimony at another hearing the same day (see Joint Economic Committee, 1977, p. 158) suggested that the 1975 tax cut had primarily been saved. However, as discussed in Chapter 4, in his later research, such as Modigliani and Sterling (1986), he went back to a more traditionally Keynesian position that the 1968 and 1975 tax changes had indeed sizably affected household spending.
[66] *MacNeil/Lehrer Report*, PBS, April 18, 1977 (*American Banker*, April 21, 1977, p. 10).
[67] On the last point, see Nelson (2020a, Chapter 5).

not in 1977 deploy the hypothesis when he criticized the Carter rebate proposal. It was not for lack of understanding of the implications of the permanent income hypothesis for temporary tax measures. Those implications—which were embedded in Ricardian equivalence—had been spelled out by Barro (1974). As mentioned in Chapter 4 above, Barro had presented this formal outline of Ricardian equivalence at Friedman's money workshop at the University of Chicago in 1973. In the course of the discussion of Barro's paper, Friedman eventually sided with Barro on Ricardian equivalence against the skeptical reaction expressed by other participants, notably Gary Becker, in the workshop (Barro, 2007, p. 133). And before this, in 1969, Friedman had affirmed in 1969 that the notion of an infinite horizon might be the right setting in modeling a household, via his statement that it was irrational not to care about one's heirs.[68]

Furthermore, even before Barro's formalization of the idea, the notion of Ricardian equivalence had run through many discussions of fiscal policy. In 1977, the concept's venerable status in economics analysis was underlined by O'Driscoll's (1977) pinpointing an exposition of the hypothesis made by David Ricardo. But even in the quarter-century to 1974, Ricardian equivalence had been a recurring feature of theoretical discussions of the effects of fiscal policy. Barro (1974, p. 1096) had himself offered a number of quotations to this effect. In addition, Modigliani (1964a, p. 483) had referred to the hypothesis as a familiar one: he had observed that it was a widespread proposition that in conditions of certainty, perfect credit markets, and infinite horizons, "tax finance and loan finance must have identical effects," and in particular the "consumption pattern would be the same in both cases." And Patinkin (1967, p. 11) suggested that "whether government interest-bearing securities… [are] a net asset of the private sector" was an issue "increasingly discussed in recent years."

In addition, and notably, an extended analysis of pre-1974 discussions of Ricardian equivalence by Elmendorf and Mankiw (1999, pp. 1643−1644) drew attention to a passage in the second edition of Patinkin's *Money, Interest, and Prices* (see Patinkin, 1965, p. 289) that implied that Patinkin had first learned of the idea of Ricardian equivalence indirectly from Friedman— via Patinkin talking to Carl Christ, who had attributed the idea to Friedman). [69] The textual evidence,

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[68] Friedman (1969a, p. 23).
[69] Elmendorf and Mankiw very strongly implied that Patinkin must have learned of the hypothesis during the year in which he, Carl Christ, and Friedman were all located at the University of Chicago—that is, the 1946/1947 academic year. However, the evidence cited by them gives little support for this speculation, for the Patinkin passage in question was not in the original, 1956, edition of Patinkin's *Money, Interest, and Prices*; rather, it first appeared in the 1965 edition (with Beard and Johnson, 1974, specifically noting it as an addition). In fact, one can go further and locate Patinkin's digestion of the idea as occurring in 1957.

however, indicates that Patinkin actually meant that he was exposed to the idea due to its presence in Christ's (1957) article (a review of the first edition of *Money, Interest, and Prices*). In that review, Christ mentioned the tax-anticipations idea and stated that he'd been alerted to that idea by "a discussion with Milton Friedman" (p. 349).[70]

Friedman had also recognized the Ricardian-equivalence scenario in print when, in 1970's *Monetary Statistics*, he and Schwartz had noted that government debt would not count as part of individuals' (net) real wealth if "individuals are regarded as treating the obligation to pay taxes to finance interest payments on government debt as a liability."[71]

By the early 1970s, Friedman was, in fact, including Ricardian-equivalence mechanisms in his list of reasons for expecting little response of total spending to arise from fiscal stimulus.[72] He did this most notably in his contribution to the 1972 *Journal of Political Economy* symposium on monetary theory. It was largely on the basis of this 1972 contribution that Roley (1981, p. 22) suggested that "Milton Friedman also appears to rely on the ultrarationality assumption [that is, Ricardian equivalence] in describing the ineffectiveness of bond-financed fiscal policy."[73] But as he had indicated in a 1970 discussion (Instructional Dynamics Economics Cassette Tape 61, November 18, 1970), the actual outcome Friedman anticipated in the wake of a tax-cut-induced budget deficit was a mixture of an interest-rate rise and Ricardian reactions (in which, in effect, the tax cut proceeds were used to buy bonds) and Friedman's 1972 article was consistent with this, as it had still counted downward pressure on interest rates as one effect of a tax increase—so it was not actually, at this point, ruling out crowding-out channels entirely.[74] Similarly, Tobin (1976, p. 335) was jumping the gun somewhat when he pointed to a Friedman *Newsweek* column

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[70] Carl Christ was an associate professor at the University of Chicago in 1955−1961 (American Economic Association, 1981, p. 96), so there were abundant opportunities for Christ to talk about fiscal policy with Friedman after 1946. (Patinkin, 1965, p. 288, actually cited Christ, 1957, but did not make it completely clear that his discussion on page 289 of Carl Christ's views was also a reference to the 1957 Carl Christ article.) The fact that Patinkin (1967, p. 11) associated the discussion of the Ricardian-equivalence idea specifically with Mundell (1960) reinforces the likelihood that Patinkin did not perceive Friedman as having floated the notion at a point very much earlier than the early 1960s.

[71] Friedman and Schwartz (1970, p. 135). They continued to acknowledge that government debt entered official measures of net wealth (see Friedman and Schwartz, 1982, p. 277). But this was just a factual statement about the statistics, with Ricardian equivalence casting doubt on the conceptual validity of the statistical classification.

[72] In addition to the items discussed presently, see Friedman's remarks in Instructional Dynamics Economics Cassette Tape 91 (January 26, 1972).

[73] In addition, Gowland (1983, p. 61) referred to "the Chicago doctrine that individuals discount their future tax liabilities." This might simply be a reference to Barro (1974), but it could also be an allusion to the statements supportive of Ricardian equivalence made over the years by Friedman (as well as by Lucas, 1981).

[74] Correspondingly, Benjamin Friedman (1978) had specifically cited Friedman (1972a) as having advanced the crowding-out position.

(January 27, 1975*a*), as having made Ricardian equivalence “a central argument” against fiscal policy as a stabilization device. For a subsequent Friedman *Newsweek* column (May 12, 1975), though acknowledging as a special case that in which new bond issuance by the federal government “can be regarded as advance receipts for future taxes!,” affirmed that a tax cut would in practice tend to raise interest rates, as it would not be fully saved.

What seems to have been a stumbling block preventing Friedman’s putting Ricardian equivalence front and center, both in these discussions and again in his 1977 critique of the rebate, is that he was not quite ready to accept an infinite-horizon approximation as the baseline assumption for the analysis of the private sector’s reaction to fiscal measures. As of 1977, he was still inclined to view the evidence as balanced in favor of the presumption that U.S. households did not fully offset changes in public-sector saving with their own saving decisions. He was likely broadly sympathetic at this stage with the work in the area by Martin Feldstein. Feldstein had argued that Social Security taxes reduced U.S. private capital formation (see Feldstein, 1974b—work Friedman mentioned in Instructional Dynamics Economics Cassette Tape 174, August 1975, Part 2) and who had debated Barro on that issue.[75]

Even in early 1977, Friedman was, however, exhibiting further moves in the direction of empirical acceptance of Ricardian equivalence. This was evident when he stated that a bond-financed tax cut raised future interest payments of the government, but without specifically claiming that a tax cut pushed up interest rates (*Newsweek*, January 17, 1977; *U.S. News and World Report*, March 7, 1977). The shifting of Friedman’s views on the matter was also evident in the contrast between his statement in February 1974 that increased government borrowing “will mean still higher interest rates” (*Chicago Tribune*, February 6, 1974, p. 7) and his statement in September 1977 that non-monetized federal deficits amounted to “taxes that will be imposed on wealth to pay back interest on the debt or the debt itself” (*Lubbock Avalanche-Journal* (Texas), September 29, 1977).

[75] See Barro and Feldstein (1978). Friedman’s sympathy with the Feldstein results did not signify a rejection of rational expectations. Indeed, Feldstein pointed out (especially in Feldstein and Pellechio, 1979, p. 362) that he was resting his theoretical basis for rejecting Ricardian equivalence not on consumers being irrational but, instead, on the empirical importance of deviations from an infinite-horizon framework. (In the case of Social Security specifically, the opposite, Barro, position was that households—having infinite horizons—would make saving plans that offset the effects on nationwide saving, and on the allocation of resources across generations, associated with payroll taxes and other Social Security arrangements. This behavior, in turn, would lead capital formation to be unaffected by Social Security arrangements. See especially Barro and Feldstein, 1978, and Boskin and Kotlikoff, 1985.) In time, however, Friedman would become more comfortable with the infinite-horizon baseline, and so he moved away from the Feldstein side to the Barro side of the Ricardian-equivalence debate.

In a short span of years after 1978, Friedman would move to, essentially, complete acceptance of the Ricardian equivalence proposition as the baseline hypothesis. Friedman had a vigorous informal debate with Robert Hall on the consumption function in conversation at the Hoover Institution during 1978/1979 (Levis Kochin, interview, April 23, 2013).[76] Not only would he become persuaded by the analytical arguments put forward by Barro and Hall; but also, he came to be further convinced that the real-interest-rate/budget-deficit relationship in the United States in the 1970s had not, in fact, borne out the predictions implied by the crowding-out story that he and others had expounded.[77]

In sum, although doubts concerning the power of fiscal policy were a perennial feature of Friedman's analysis from the early 1950s onward, it was only after the 1970s that the permanent income hypothesis really achieved pride of place as the basis for his doubts. He would then believe, as he had before, that moving money from one pocket to another was not stimulative. But he would come to see this experiment in terms of shifting funds between a pocket in the present and a pocket in the future.

President Carter's abandonment of the rebate proposal

President Carter withdrew the tax rebate proposal in mid-April 1977 (*Boston Globe*, April 15, 1977). Dornbusch and Fischer (1978, p. 546) observed that the abandonment of the rebate idea was partly because of good readings on U.S. economic activity. They added, however, that a consideration weighing against the rebate was "the question of whether transitory tax cuts or rebates are really effective in stimulating aggregate demand." Indeed, another textbook account—Gordon (1978, p. 487)—maintained that the rebate would likely have been saved. Gordon therefore largely endorsed the Ricardian-equivalence account, in this case. However, notwithstanding the respect with which the economics profession, by this stage, already treated the permanent income hypothesis, there remained widespread adherence to an old-Keynesian belief that the rebate would have been powerful. For example, in the period preceding the plan's withdrawal, the Wharton econometric model's forecast had predicted that the rebate would boost consumption and output in the second and third quarters of the year (*Daily News* (New York), March 25, 1977). And, after the plan's withdrawal, Tobin (1978b, p. 20) lamented the opposition the rebate had received and claimed that the "rebate in 1975 had been instrumental in launching the recovery." Robert Solow, while conceding that doubts about the effectiveness of

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[76] Kochin was a National Fellow at the Hoover Institution in 1978/1979: see *https://econ.washington.edu/file/2154/download?token=ZwqbbCcm.*
[77] see Chapter 9 below

rebates had grown in the economics profession, suggested that "a $50 rebate might be spent rather quickly" (*New Republic*, May 7, 1977, p. 13.)

Solow criticized the "infirmity of purpose" behind the rebate's withdrawal (*New Republic*, May 7, 1977, p. 13). Indeed, when she came to office as Prime Minister of the United Kingdom two years later, Margaret Thatcher viewed Carter's reversal on the rebate as a lesson on what not to do, on the grounds that backing down from a publicly-stated position amounted to a lack of resolve (Young, 1991, p. 129). In 1977, however, Friedman offered a more favorable interpretation of the president's move than that embedded in these assessments. "I think it speaks extraordinarily well for President Carter's capacity to adjust his plans to circumstances," Friedman remarked shortly after the rebate's cancellation. "It's very, very hard for any of us to admit that we're wrong, and to withdraw from a position in which we have gone." (Instructional Dynamics Economics Cassette Tape 211, April 1977, Part 1.)

Subsequently, it became rare for Friedman to speak so highly of President Carter. The April 1977 praise arose partly because Friedman took Carter's move as an acceptance that stimulation of aggregate demand was no longer appropriate. It would, however, become clear that this was not the case. In his commentary on the rebate's withdrawal, Solow implied that demand-stimulating measures were still needed (*New Republic*, May 7, 1977, p. 13). The Carter Administration shared this view, and it continued to support expansionary measures after April 1977, in both the fiscal sphere and the monetary sphere.

THE SECOND MONETARY EXPLOSION

As recounted in Chapter 5, in the year to December 1976, CPI inflation in the United States was slightly below 5 percent. This rate, the lowest since 1973, implied a decline in inflation of over seven percentage points from the peak in late 1974. That considerable achievement set the tone for much economic commentary in late 1976 and into 1977—in which it was suggested that, although no further decline might be in prospect, the reduction in inflation seen during 1975 and 1976 would, essentially, be maintained. For example, one financial commentator wrote in late 1976 (*Bankers Monthly* (November 15, 1976, p. 2): "Inflation is likely to remain in the 5½ percent range over the coming year." Once, in early 1977, inflation had shot back above 6 percent, the same commentator remained sanguine, observing (*Bankers Monthly*, July 1977, p. 2): "Price inflation has slowed predictably from its unsustainable, weather-induced first-quarter pace. Consumer prices should advance at a rate no faster than the 5½–6 percent official estimate

in the second half of this year." Over in *Forbes* magazine, an analysis of inflation had recently been very optimistic about the prospects for inflation, with the article concluding: "Inflation, where is thy sting?" (*Forbes*, May 1, 1977.)

A dissenter from this consensus was Milton Friedman. He was far less optimistic. To Friedman, it seemed that a sharp and lasting *rise* in U.S. inflation was likely to occur in the coming years. In mid-1976, he proclaimed that the decline in the U.S. inflation rate had either come to an end or very shortly would (Instructional Dynamics Economics Cassette Tape 192, June 1976, Part 1). Speaking about six months later, in early December 1976, Friedman predicted that inflation would trough around February 1977 (Instructional Dynamics Economics Cassette Tape 204, December 1976, Part 1).

Friedman went further, making quantitative predictions. In that early December 1976 commentary, for example, he asked, "What will be the rise in the CPI over the next expansion? … [T]he prediction I came out to was an average rate of rise over the next expansion of 7 to 9 percent."[78] He specified this prediction as pertaining to the period from February 1977 to early or mid-1979. A month later, Friedman observed (*San Francisco Examiner*, January 5, 1977): "I'm hesitant to give any number [but] the probability is for 7 or 8 percent at the end of 1977 or in 1978." On television in mid-April 1977, Friedman said of inflation, "I expect it's going to step up in the next year or two to 7 percent or 8 percent."[79]

Around October 1977, with the accrual of several months' monetary data having given Friedman the material with which to extend his forecast, Friedman predicted that the overall average rate of inflation from mid-1977 to mid-1979, he suggested, would average 7 to 9 percent. He added that it was "now too late to do much about the rate of inflation over the next couple of years," as it was going to reflect monetary policy actions that had already been taken. [80]

Friedman had also specifically indicated that a return of inflation to the double-digit range was in prospect for 1979 and 1980 (*Newsweek*, October 3, 1977).

In February 1978, Friedman predicted that the peak of inflation in 1979 or 1980 could well

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[78] Friedman also gave the 7 to 9 percent number in a keynote talk at the Bay Area Council 1977 outlook conference on January 25, 1977. See *Contra Costa Times* (Walnut Creek, California), January 26, 1977.
[79] *MacNeil-Lehrer Report*, PBS, April 18, 1977 (*American Banker*, April 21, 1977, p. 6).
[80] Friedman (1977a, p. 23).

exceed 12 percent and would come in the wake of rates of 7 to 9 percent in 1978.[81] Friedman elaborated in April 1978 that double-digit M2 growth during 1977 had led him to judge that "inflation from February 1977 to October 1979 will average something like 7 to 10 percent… [and] no sustained reduction in inflation can be expected before mid- or late 1979." (*Newsweek*, April 24, 1978.) The following month, in an appearance at the Economic Club of Detroit, Friedman made other specific predictions. The United Press International report on this speech opened by observing: "Economist Milton Friedman says the U.S. economy will fall into a major recession by 1980 after a period of double-digit inflation next year." The article quoted him specifically saying that inflation "probably will be eight or nine percent by the end of the year—and may be into double digits in 1979" (*Kenosha News* (Wisconsin), May 19, 1978).

And Friedman would be vindicated. These various predictions—which differed in the vantage point at which they were made, numerical specificity of the forecast, and the time frame to which the forecast referred—would have in common the property that they proved accurate. Friedman's pessimistic picture of how inflation would evolve over the rest of the 1970s would indeed be realized.

Having crossed above 6 percent in February 1977, the twelve-month CPI inflation rate did not go below 6 percent again until August 1982. In May 1978, it moved above 7 percent, in September 1978 above 8 percent, at the end of 1978 it hit 9 percent, and during 1979 it passed through the 10, 11, and 12 percent barriers, moving above 13 percent at the end of the decade. In terms of averages, Friedman's prediction of 7 to 9 percent inflation, which Friedman had predicted first for February 1977 to mid-1979 and later for 1978 alone, was met for both periods: the average quarterly annualized inflation rate was 8.6 percent for 1977:Q1−1979:Q2 and 8.9 percent for 1978. In contrast, as 1978 approached professional forecasters' consensus prediction for that year's inflation rate was about 6 percent (*U.S. News and World Report*, August 7, 1978, p. 17; *Wall Street Journal*, October 1, 1979).

Long-term interest rates

Friedman's accurate prediction that the late 1970s would see a sharp rise in inflation and, indeed, a renewed period of double-digit inflation contrasted sharply with the assessments of inflation prospects underlying trading in the bond market. In the final quarter of 1976, the ten-year

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[81] *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," February 6, 1978, p. 24 of transcript.

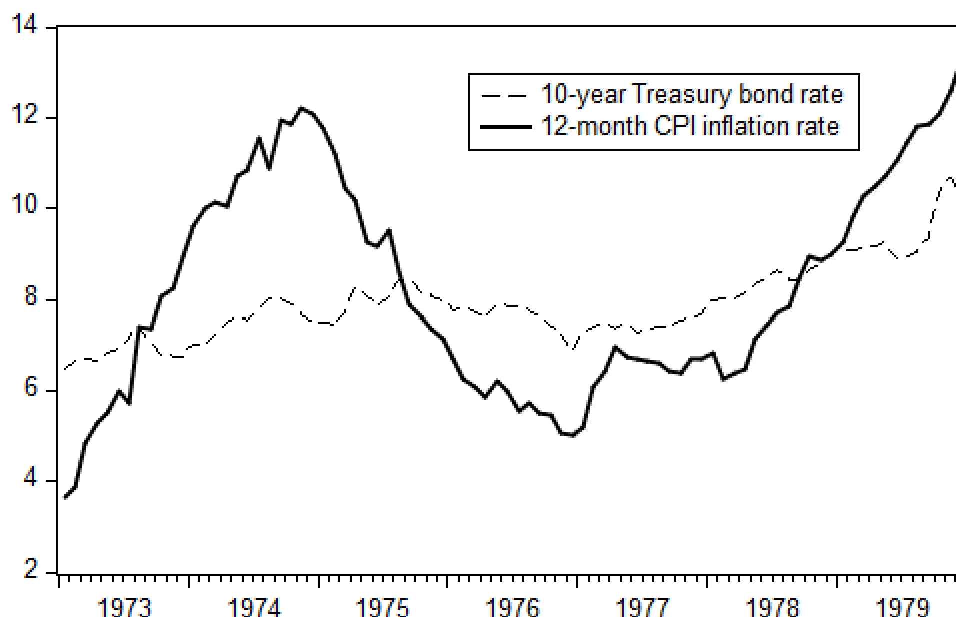

Figure 1. Ten-year Treasury bond rate and twelve-month CPI inflation rate, January 1973 to December 1979.
Source: Federal Reserve Bank of St. Louis' FRED portal.

Treasury bond rate averaged 7.2 percent. Financial commentator William Hummer contended during this period (*Bankers Monthly*, November 15, 1976, p. 2) that "medium-term and long-term yields could stay in their current range or decline a bit further." Several months later, Hummer's forecast seemed to have panned out: he observed (*Bankers Monthly*, July 15, 1977, p. 2) that "as summer arrived, bonds were trading near their highest price levels in 3¼ years…" The inflation expectations embedded in these bond rates were correspondingly placid, being in mid-single-digits (see Levin and Taylor, 2013, p. 222). Therefore, the behavior of longer-term Treasury securities reinforced the consensus that what inflation trouble lay ahead would be considerably less severe than that seen earlier in the decade.

Friedman observed in late 1976 (Instructional Dynamics Economics Cassette Tape 204, December 1976, Part 1): "To stick my neck out, I find it hard to believe that long-term bond yields will not rise this coming year, and yet, as I say, I was wrong on that a year ago and maybe I'm wrong now." Longer-term interest rates were indeed about 80 basis points higher in December 1977 than they were in December 1976—but this increase was concentrated in the

final quarter of the year. In fact, before October 1977, every month of the year had recorded a bond rate lower on average than in the corresponding month in 1976. As he explained in his December 1976 commentary, Friedman's basis for believing that rates would increase had been that the late-1976 values were justified only if the inflation rate in the next several years stayed around 4 to 6 percent, while "monetary growth for the past few years does not justify a rate anything like that low."

Yet even with the late-1977 increases in the bond rate, it is clear that the markets *did* expect inflation far below that actually seen in the late 1970s. The monthly average for the rate on ten-year Treasury bonds did not cross 8 percent until February 1978 and did not pass 9 percent until December 1978. See Figure 1.

The monetary basis for Friedman's predictions

Why did Friedman forecast inflation so differently from (and, as it turned out, better than) others in these years? The answer is that the behavior of M2 growth over this period provided a good guide to future inflation developments, at a time when other approaches fell short. He traced the second round of double-digit inflation, that in 1979–1980, to the second monetary explosion of 1976–1977.

As far as Friedman was concerned, the path of future inflation was provided by the pattern of monetary growth, and by the roughly two-year lag from monetary growth to inflation that he had found to characterize the historical U.S. data. As was discussed in Chapter 5, after being surprised by how restrained monetary growth was over 1975, Friedman had sounded the alarm in 1976 about the fresh surge in the money stock. Late in that year, he was drawing parallels between the high monetary growth in 1976 and the rapid rate of monetary expansion in the early 1970s that had preceded the 1973–1974 outbreak of U.S. inflation (Instructional Dynamics Economics Cassette Tape 190, May 1976, Part 1). When high monetary growth continued, Friedman was prompted to observe at the start of 1977 that there had been "unduly expansive [monetary] policy for the last six months" (*San Francisco Examiner*, January 5, 1977).

The evolution of the second monetary explosion in the 1970s is depicted in Table 1, both for the old definition of M2 used in the 1970s and for the modern M2 definition adopted in the United States in 1980.

Table 1. Annualized percentage quarterly growth rates of M2, 1976–1978								
	Quarter of year							
	Q1		Q2		Q3		Q4	
Year	Old M2	New M2	Old M2	New M2	Old M2	New M2	Old M2	New M2
1976	10.5	13.0 [13.3]	10.0	12.7 [12.6]	8.9	11.3 [11.1]	12.6	15.2 [15.3]
1977	10.9	13.7 [14.3]	9.0	11.2 [11.2]	10.1	9.6 [9.5]	7.9	9.7 [8.5]
1978	7.0	7.5 [7.7]	8.4	7.5 [7.7]	9.8	8.2 [7.7]	8.5	9.5 [7.7]

Source: Simpson (1980, Table A2, p. 113) for both old and new M2. The square brackets give calculations using FRED data on new M2.

It was on the basis of this monetary explosion that Friedman made his prediction of a renewed, late-decade surge in U.S. inflation.[82] Friedman's initial predictions in 1976 and 1977 of inflation of 7 to 9 percent in the years ahead were evidently arrived at by from subtracting about 3½ or 4 percent average real growth from the 10.5 to 12 percent rate of monetary growth prevailing since mid-1975. But as, during late 1977 and the first part of 1978, he moved into predictions for 1979, he made a somewhat less mechanical adjustment. He reasoned that the inflation of the 1970s had imparted a slight upward trend into M2 velocity, so that a percentage point or two had to be added to inflation projections that were based on (old) M2 growth (*Newsweek*, August 20, 1979; Charles H. Brunie, interview, July 15, 2013). That factor partly accounted for why, in early 1978, Friedman was offering a 12 percent inflation prediction for mid-1979—even though he was not predicting still-higher monetary growth than that observed so far.

Continuing along this line of argument, Friedman predicted at a briefing to the Oppenheimer and Company financial group in mid-1978 that inflation would likely peak in the fourth quarter of 1979, and that "it would be a miracle if inflation peaked below 10 percent, and 10%−12% or 10%−13% would be more likely" (quoted in *Wall Street Journal*, October 1, 1979).

Friedman was on fairly solid ground predicting an extra couple of points of inflation over that suggested by the patterns of M2 growth. From his participation in the Bach Committee (see

[82] Other judgments on this period that have stressed 1976 as the year in which the resurgence of U.S. inflation originated included Brunner (1980a, p. 26), who contended that the Federal Reserve launched new round of inflation via its actions in 1976, and Romer and Romer (2004, p. 140), who saw monetary policy in the late Burns period as very loose: they argued, on the basis of the behavior of real short-term interest rates, that "at the end of 1976, policy became dramatically more expansionary."

Chapter 5) and his related deliberations with the Federal Reserve, Friedman knew that the traditional, commercial-bank-centered M2 definition was on the way out and would be replaced by a broader M2 series that included thrift institutions' deposit liabilities. Although that new M2 series was not yet published in 1978, its behavior could largely be gleaned from the behavior of what in 1978 was officially called M3 (and was published by the Federal Reserve Board). Friedman would therefore have known, from M3's behavior, that the new M2 concept had consistently been growing a couple of points more per year in 1976 and 1977 than had the old (and, before 1980, still official) M2 series. Until the most recent years, a mean-reverting velocity had been a good approximation with regard to the old M2 aggregate, so monetary growth (minus output growth) and inflation behaved similarly, on average: see Chapter 2 above, as well as Friedman's observations in *Newsweek*, April 24, 1978. It could be conjectured that the new M2 series would inherit this historical property of a roughly stable velocity.[83] That being the case, it followed that old M2 velocity had acquired an upward trend of 2 percentage points or so.[84] Consequently, a reduced-form approach of the kind Friedman used, based on past monetary growth and a simple velocity assumption, Friedman had been able to forecast successfully the United States' second bout of double-digit inflation in 1979–1980.

Cost-push views in officialdom

"What's needed is not the knowledge; it's the will."[85] With this statement in February 1978, Friedman displayed his tendency, already mentioned in this chapter, to view policymakers as sharing his own, monetary, perspective on inflation's causes. Starting from this premise of a common economic model with policymakers, he attributed the monetary authorities' generation of inflation, and their failure, so far, to stick to plans to disinflate, to a lack of resolve and to acquiescence to outside pressures. However, for the late Burns years of 1976 to 1978, as for the prior six years, the evidence is strong that Friedman was incorrect: lack of knowledge was a problem, after all. In particular, Burns continued to adhere to a nonmonetary view of inflation and, insofar as he thought that monetary policy mattered for inflation, he mistakenly believed that the Federal Open Market Committee (FOMC) was providing *anti*-inflationary pressure.

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[83] It was subsequently shown to possess this property (specifically, stationary behavior) by Engle and Granger (1987), Small and Porter (1989), Rasche (1990), and Hallman, Porter, and Small (1991).
[84] This upward trend could be interpreted as a shift by households away from holding bank deposits in the wake of the higher inflation of the 1970s. The fact that new M2 velocity was stationary could then be interpreted as reflecting the fact that the flight away from bank deposits was in large part toward thrift accounts, as well as to the money-fund accounts (included in the modern M2).
[85] *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," February 6, 1978, p. 24 of transcript.

An indication that these were Burns' positions is provided by the transcript of the June 1977 FOMC meeting, which includes an exchange between Burns and the president of the Federal Reserve Bank of Boston, Frank Morris. The subject of the exchange was the long-term bond rates. As discussed above, bond rates were quite low. By way of explanation for this state of affairs, Morris remarked: "I think there is an expectation that we are not going to have a sharply rising trend in interest rates. That also shows up in the Treasury bill futures markets. The Treasury bill futures yields are going down, reflecting an anticipation of that." Chairman Burns asked Morris: "You think this behavior of long-term interest rates reflects, in part, confidence in the Federal Reserve's policies?" Morris replied: "I think so, yes." "Well, that's been my own judgment, but it's a hard thing to be sure of," Burns concluded.[86] Although Burns was cautious in his interpretation of what was driving bond rates, he clearly believed that it was appropriate to be confident that FOMC policy was noninflationary.

Potential problems on the inflation front, as Burns saw it, arose from elements other than in the monetary sphere. In fact, although Burns was at odds publicly with the Carter administration over much of 1977, an emphasis on cost-push factors was common ground between himself and the Carter administration team. Meltzer (2009b, p. 919) suggested the contrary, by juxtaposing the views of Carter's advisers against Burns' statement (on July 29, 1977) that inflation cannot go on without "monetary nourishment."[87] Burns' full remark, however, was: "For our part, we at the Federal Reserve know that inflation ultimately cannot proceed without monetary nourishment." This was an acknowledgment that monetary restraint was necessary for long-run price stability, but it did not imply that such restraint was sufficient for obtaining this condition, and it was consistent with nonmonetary factors impeding the achievement of price stability.[88] Furthermore, the statement was predicated on the—in retrospect, badly-informed—notion that monetary policy was already, in 1977, contributing to the reduction of inflation.

In line with his cost-push perspective, Burns regarded the revival of inflation in 1977 as occurring despite a continuing considerable amount of the slack in the economy (see Nelson,

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[86] From the FOMC meeting of June 21, 1977 (Federal Open Market Committee, 1977a, pp. 17−18).
[87] Meltzer cited this statement as being from the "Carter papers, Jimmy Carter Library." Actually, there is nothing unique to the Jimmy Carter Library when it comes to the availability of this statement. It was from Burns' opening testimony at a Congressional hearing and was printed in the *Federal Reserve Bulletin* at the time. For this published version of the passage in question, see Burns (1977a, p. 728).
[88] More specifically, if cost-push factors had a positive mean in the long run, as adherents to cost-push views of inflation maintained, monetary growth rates that would otherwise be consistent with price stability would be associated with continuing inflation.

2005, and DiCecio and Nelson, 2013, pp. 406, 411).[89] Impressed by the speed of the national economic expansion in the late-1976/early-1977 period, Burns had opposed Carter's tax-rebate idea (*Los Angeles Times*, February 4, 1977; *Daily News* (New York), March 23, 1977). But, as noted, he had signed up to a 6 percent real growth for 1977, and he had endorsed the administration's proposal to bring the unemployment rate down from by about 1.5 percentage points from the end of 1976 to the end of 1977. Viewed in this light and in conjunction with Burns' other statements, the concern that Burns voiced during 1977 about stimulus boosting the inflation rate indicated a speed-limit view of inflation dynamics—and did not signify agreement with Friedman's judgment that the United States was poised to cross into excess demand territory (see DiCecio and Nelson, 2013, pp. 410–412).

Phillips-curve approaches

It was noted in Section I that, by the late 1970s, the Friedman-Phelps work on the Phillips curve had been accepted by many economists. These economists eschewed pure cost-push views of price behavior and instead saw inflation in terms of an expectational Phillips curve relationship, with a sizable response of inflation to the output gap. Those economists who did model inflation using such a Phillips curve were capturing in their projections and policy analysis the aggregate-demand-to-inflation connection that Friedman was using in the reduced-form, monetary-growth-based, inflation forecasts he gave during 1977 and 1978. In principle, therefore, they could, like him, have predicted the double-digit inflation of the end of the decade. In practice, this did not happen.

A key reason for this, discussed further below, is that, in the late 1970s, output-gap estimates continued to be heavily mismeasured—rendering unreliable inflation projections that were based on the output gap. Whereas a cost-push approach would be in error by giving a zero weight to the output gap when a sizable weight was warranted, a Phillips-curve approach might appropriately give a sizable weight to the output gap; but, in the late 1970s, such an approach might use an output-gap estimate that suggested a deeply-negative gap, when it was actually

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[89] In addition, like Friedman, Burns approvingly quoted U.K. Prime Minister Callaghan's criticism of past inflationary policies. He did so, for example, at an FOMC meeting held a couple of weeks after Friedman quoted in his column (see Federal Open Market Committee, 1976, p. 40). The context in which Burns recited this quotation suggested that he felt that monetary policy was exercising restraint and that the fault in current stabilization policy lay in the danger was that fiscal policy would become overstimulative. Similarly, Burns (1977c, p. 4) portrayed Callaghan's statement as an indictment of past approaches—"policies for stimulating employment on which we have relied in the past"—from which he distinguished the modern-day formulation monetary policy in the United States and elsewhere.

close to zero or even positive. That this was not merely a hypothetical problem in the late 1970s is indicated by Orphanides' (2003, p. 645) Figure 2, in which an official output-gap estimate of the time is always negative in 1977 and 1978, while the revised "final" output gap is nearly always positive in those years.

But in principle, as stressed in earlier chapters, take-offs in inflation such as that observed in 1977 could have led modelers to revise their output-gap estimates promptly and get their inflation projections back on track. However, this process did not occur. The flawed output-gap estimates were, in 1977 and 1978, largely retained. The rise in inflation was, for the moment, primarily attributed to temporary factors; and the chance to project accurately the double-digit inflation of 1979 was missed.

Indeed, even Rudiger Dornbusch who—as described in Section I of this chapter—had done much, with Stanley Fischer, to integrate monetarist ideas about economic behavior into mainstream economics teaching, was inclined to view the output gap as sizable, and monetary ease as appropriate in 1977 and 1978. In the Congressional testimony of March 7, 1978, that was discussed in Chapter 1, Dornbusch stated: "The first priority then should be continued expansion in aggregate demand."[90] To this end, he called for reductions in interest rates: "Interest rates should *not* be allowed to rise further and, indeed, a rollback is desirable."[91] Dornbusch saw the stimulus he recommended, along with other measures he was urging, as consistent with keeping inflation at 6 percent.[92]

Dornbusch and others—including presumably the bond traders who set their years-ahead inflation projections so low during 1977 and into 1978—may have been misled into thinking that inflation in 1977 was overstated by the cold winter weather of early 1977 and by the year's exchange-rate decline, rather than perceiving that the rise in inflation was overwhelmingly due to overly-easy monetary policy.

A fundamental reassessment among policymakers, commentators, and market participants of output-gap estimates and inflation expectations would not occur until 1979. With this reassessment would occur a rethinking of whether monetary policy was, as Friedman had

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[90] Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1978b, p. 42).
[91] From Dornbusch's written submission in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1978b, p. 53).
[92] From Dornbusch's spoken testimony of March 7, 1978, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1978b, pp. 42–43).

insisted, been much too loose in 1976–1978. Dornbusch himself was in the vanguard of this rethinking. An early glimpse into his revised thinking came in the fact that, in contrast to Dornbusch's 1978 assessment that interest rates had risen too much already, Dornbusch and Fischer (1979, p. 5) would observe that, during the economic recovery that had started in 1975, interest rates had taken a long time, by historical standards, to pick up. Later, Dornbusch (1993, p. 332), in looking at the double-digit inflation of 1979, observed, "Arthur Burns… was responsible for that situation." In so doing, Dornbusch lined himself up with the Friedman interpretation that the double-digit inflation in 1979 originated in overly-loose monetary policy in the late Burns years.[93]

It is not clear that Phillips-curve-based forecasts, even when using revised data, would have done quite as well as Friedman did in predicting the double-digit inflation of 1979. For example, Stock and Watson's (1999, p. 323) Figure 4 suggested that such projections implied that inflation would be well below 10 percent in 1979 and into 1980. This miss perhaps reflects intractable problems with measuring the output gap or the unemployment-rate gap and in pinning down those gaps' dynamic relationship with inflation. Certainly, however, Phillips curves using the revised gap series would have accurately predicted a sharp rise in inflation.[94] Once again, therefore, the theme emerges of major problems with measuring the output gap—and how Friedman was able to insulate his predictions from these problems.

Output-gap estimates and official views

By 1977, there was considerable realization that the output gap had been overestimated *in the past*. In the case of the United Kingdom, for example, Laidler (1976c, p. 87) observed that "the use of macroeconomic policies to maintain levels of employment too high to be viable in the long run has been an important source of inflationary pressure." What was less appreciated, in both the United Kingdom and the United States, was that such overestimation *remained* a major practical problem in the late 1970s.

Instead, as of 1977, it seemed that the authorities had now put their house in order and

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[93] A drastic reassessment of the appropriate posture of monetary policy in the late 1970s would also be evident in Dornbusch's analysis of the United Kingdom. Whereas Dornbusch and Fischer (1980a, p. 25) estimated that the U.K. output gap was −9.6 percent in 1977, Dornbusch would later assess that the U.K. output gap was zero in that year (*Financial Times* (London), July 24, 1985).
[94] Reflecting this, the simplest accelerationist Phillips curves do well in explaining U.S. inflation's behavior during the 1970s (see the references discussed in Chapter 4).

appropriately revised their estimates of potential output. The Carter Administration's ambitious economic growth target for 1977, and the perception of a large amount of slack that motivated the target, might have been even stronger, had not estimates of potential output not been substantially revised down. Downward adjustments to estimates of potential GNP had led the Council of Economic Advisers in January 1977 to issue revised estimates of the output gap in recent years, with the result that the output gap during the trough in 1975 was reduced (that is, made less negative) by about 4 percentage points (Orphanides, 2003, p. 655). Likewise, the Federal Reserve Board in early 1977 substantially lowered the estimate of potential production that underlay its estimates of capacity utilization (*Financial Times* (London), January 14, 1977). It turned out, however, that the scale of the change in the aggregate-supply picture that had occurred in recent years was so great that, even with these revisions, the economy's sustainable-output path remained substantially overestimated.

A crucial problem here was that, as yet unbeknownst to policymakers, from 1973 onward the potential growth rate of the economy had dropped from close to 4 percent to about 2.5 percent. In contrast, policymakers in 1977 continued to work on the assumption of trend growth close to its pre-1973 rate: Arthur Burns, for example, during 1977 gave potential output growth as 3.5 percent (Burns, 1977b, p. 361). The belated recognition of this productivity-growth slowdown is considered further in Chapters 8 and 9.

The difficulties in estimating the output gap arising from the changing potential-growth picture were compounded by large revisions to actual output data.[95] Another factor that continued to cloud the picture of the true output gap was, of course, the rise during the 1970s in the natural rate of unemployment. This matter is discussed further in the discussion titled "Franco Modigliani" in Section III below.

The combination of inaccurate initial output data and inadequately-revised potential-output estimates meant that output-gap mismeasurement continued to be a serious problem in 1977. Though at the time perceived as being about 7 percent (Orphanides, 2003, p. 645), modern estimates of the output gap in 1977:Q1, Carter's first quarter in office, suggest that output was less than 2 percent below potential by that point. Furthermore, the gap was narrowing rapidly, and it was positive in each quarter from 1978:Q2 through the end of the decade.

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[95] See Orphanides (2003), as well as the discussion in Chapter 4 above of the revisions to the decline in output during the 1973–1975 recession.

In marked contrast to these subsequent assessments, James Tobin, in his April 1977 television debate with Friedman, had declared that the United States had inflation "not because there's too much money chasing too few goods. Quite the contrary. We have excess capacity, and a large amount of unemployment."[96] Similarly, in officialdom, Carter's Chairman of the Council of Economic Advisers, Charles Schultze, remarked in September 1977: "Ample resources are available to permit further expansion…" (*Daily News* (New York), September 14, 1977.) In the same month, the Federal Reserve Board's Governor Charles Partee testified that "sizable unused resources exist in this and other economies" (Partee, 1977, p. 891; also quoted in *American Banker*, October 3, 1977, p. 15).

As stressed in Chapter 5, Friedman was not ahead of the pack in recognizing the shift in the behavior of potential output. In 1978, for example, his estimate of noninflationary monetary growth seemed to be predicated on potential growth of about 4 percent (*Newsweek*, April 24, 1978). Only in 1979 did he seem to embrace a potential growth rate of about 3 percent (*American Banker*, June 12, 1979, p. 3)—by which time the Carter Administration had brought down its official estimates of potential growth to such a rate. Nor did Friedman and others on his side of the debate on stabilization policy show much sign of detecting how badly the output statistics themselves had been underestimated: Lucas and Sargent (1978, p. 49), for example, referred to the United States as having had a "depression" in 1973−1975. Friedman, as we have seen in Chapter 5, did sound warnings early on about the rise in the full-employment unemployment rate. But this was only one reason for exaggerated gap estimates in the 1970s.[97]

The monetarist side nonetheless became well known for eschewing output-gap estimates in their policy prescriptions and macroeconomic analysis (see Wonnacott and Wonnacott, 1979, pp. 331−334, for an early discussion). This characterization was justified, on two dimensions.[98]

First and most obviously, they stressed policies, notably the constant monetary-growth-rule, that did not respond to estimates of the level of the output or unemployment gaps.

The second respect in which monetarists in the late 1970s distanced themselves from output-gap estimates was that their view of the inflation process suggested a picture of the excess-demand

[96] *MacNeil-Lehrer Report*, PBS, April 18, 1977 (*American Banker*, April 21, 1977, p. 6).
[97] The best sign Friedman gave during this period of a recognition of a secular productivity-growth slowdown was to point to the robust growth of employment since 1975 as an indication that the U.S. economy had had a good recovery from the 1973−1975 recession, (*Oakland Tribune* (California), January 27, 1977).
[98] In addition to what follows, see the discussion in Nelson (2020a, Chapter 8).

situation that was at variance with the extant output gap estimates. This was a point on which some members of the monetarist school were explicit during 1977 and 1978. The economic analysts at Citibank, for example, noted that official estimates of slack could not produce inflation rates of the kind observed in recent years and concluded (*Citibank Monthly Economic Letter*, October 1977, p. 4): "This indicates that either there is less slack in the economy than the conventional indexes would have us believe[,] or that the existence of slack has less of a damping influence on the inflation rate than it used to." The monetarist position was squarely in line with the first of these interpretations, with David Laidler (1978b) challenging a prominent report to the OECD by McCracken and others (1977), which had reported mid-1970s troughs of the output gap as being in double-digit percentages for the United States and other major countries. Laidler pointed out that the inflation of the 1970s suggested a condition of excessive demand—not the chronic excess supply that had been the report's characterization of the decade. For his part, Friedman's emphasis on excess demand had been implicit in his predictions from late 1976 of a sharp rise in inflation over the rest of the decade.

Friedman, therefore, by no means denied the output gap/inflation link in his own analysis. On the contrary, the reduced-form monetary-growth/inflation link in his framework arose as an outgrowth of the structural dependence of inflation on excess demand. Consequently, as indicated above, Friedman's prediction that rapid monetary growth would be followed by inflation embedded the notion that, along the way to producing inflation, monetary ease would generate positive output gaps. But his lack of confidence in estimates of these structural relationships, and in empirical measures of resource gaps, led him to the shortcut of direct monetary-growth/inflation relationships. This reduced-form approach served him especially well during the late 1970s.

Other reduced-form forecasts

We have seen, therefore, that Friedman's inflation predictions did well because they captured the relationship between aggregate demand pressure and inflation without using unreliable estimates of slack to predict inflation. Could econometric models have done as well as he did, if they had forecast using available macroeconomic data and not used gap estimates? It is difficult to know the answer to this question definitively, as this was a time when other reduced-form approaches to forecasting inflation were hard to come by. Autoregressive integrated moving average (ARIMA), or Box-Jenkins, time-series forecasting methods were available and had become widely used but, by construction, these approaches projected inflation based on its own past

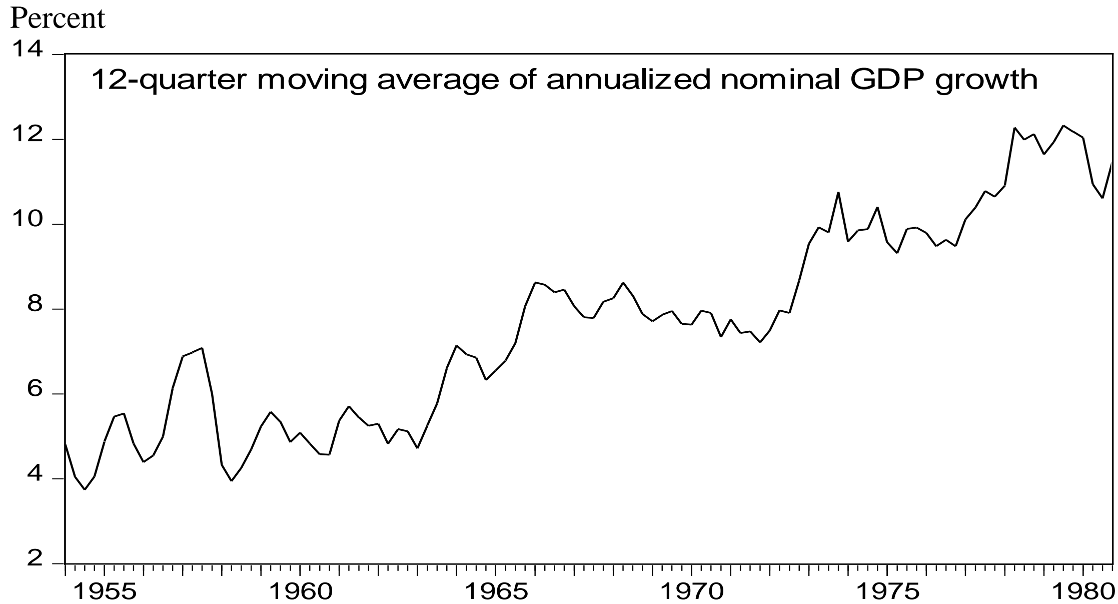

Figure 2. Twelve-quarter moving average of annualized growth in nominal GDP.
Source: Computed from quarterly data, Federal Reserve Bank of St. Louis' FRED portal.

behavior—and so they supported the notion that the moderate inflation pattern of 1976 would continue. The vector autoregression (VAR) approach to forecasting was just getting started: Sims' (1980) pioneering article on the subject appeared in working-paper form in 1977 (see Sargent, 1979) and applications of the VAR approach were only beginning to appear in print in 1977–1978—for example, Sargent (1978).

One reduced-form approach that likely would have done about as well as Friedman's forecasts of inflation using monetary growth was not prevalent at the time. This would be to predict inflation using nominal income growth. Friedman took for granted that rapid nominal income growth would occur in the course of monetary ease being transmitted into inflation—and that, with the different lags involved in the reaction of output growth and inflation to monetary policy, nominal income growth would most likely take off ahead of inflation. This regularity certainly was observed again in the late 1970s. Jerry Jordan noted in 1978 (*American Banker*, October 23, 1978*b*) that nominal income growth in the prior three years had averaged over 11 percent, "the highest rate for any three-year period in over 30 years." This pattern is confirmed by modern data on nominal GDP growth. A three-year average of annualized quarterly nominal income growth (see Figure 2) shows that, in the late 1970s, aggregate demand growth reached a new plateau that was steeply higher even than the previous high of the mid-1970s. The three-year

averaging process also shows also how ephemeral the restrictive policies that led to the 1973–1975 recession proved to be.

Therefore, the pattern of nominal income growth, like that of M2 growth, provided an early warning of the late 1970s resurgence of inflation. Because nominal income growth typically lagged monetary growth over this period, however, monetary growth, and increases in M2 in particular, provided a signal about the rise in inflation that was ahead of the signal provided by nominal income growth.

In light of episodes like the late 1970s, Gordon (1985, p. 49) offered the generalization: "Inflation in the long run is always and everywhere an adjusted-nominal-GNP phenomenon."[99] Friedman would largely endorse this notion, himself remarking in 1985, "Inflation tends to depend on the average rate of growth of nominal income… over a considerable period."[100] However, Friedman regarded tracing inflation to monetary growth as a more fundamental exercise. Furthermore, nominal GDP growth can be very high and not lead to inflation, as occurred in 1983–1984. In this later period, rapid nominal GDP growth was noninflationary because the economy was absorbing considerable resource slack. That is, the mid-1980s featured a state of the economy that many nonmonetarist economists, largely erroneously, had perceived as prevailing during 1977–1978: rapid growth in aggregate spending that was noninflationary, as it was justified by the amount of slack in the economy.

Monetary targets and the federal funds rate

The failure of the Federal Reserve to meet its monetary targets, particularly by the criterion of M2, was what for Friedman lay behind the monetary explosion that had generated faster growth in nominal spending and the revival of inflation in the United States.

In late 1976, Friedman lamented the fact that the Federal Reserve's announced intention of a gradual slowdown in monetary growth had not been manifested in its actual performance (Instructional Dynamics Economics Cassette Tape 204, December 1976, Part 1). A little later (in (Instructional Dynamics Economics Cassette Tape 208, February 1977, Part 1), after noting that Burns had adjusted down the target range, Friedman vented: "But all he's changing are the

[99] Gordon defined "adjusted nominal GNP" as nominal GNP growth minus the long-run rate of potential output growth.
[100] Friedman (1985a, p. 52).

targets—not what's actually *happening*!" Similarly, Friedman observed late in 1977 that "while the targets are going down, actual monetary growth is going up" (*St. Louis Globe-Democrat*, December 7, 1977). He summed matters up in September 1978 with the observation that the "targets for monetary growth set by the Fed have consistently declined in the past two years in order, the Fed says, to foster a gradual reduction in inflation, but actual monetary growth has proceeded along a rising, not a declining[,] trend."[101]

These characterizations were borne out by the fact that the Federal Reserve's M2 target range had stepped down from 8½–10½ percent growth in 1975 to 7½–10½ percent in 1976, but the outcomes over the target periods were 9.6 percent and 10.9 percent, respectively (Argy, Brennan, and Stevens, 1990, p. 54).[102] Monetary growth was then 9.8 percent in 1977.[103] Furthermore, the pattern for M3—which as suggested above, Friedman by this stage likely considered a more meaningful monetary aggregate, as it signaled the behavior of the in-progress redefined M2 series—was considerably worse: the Federal Reserve overshot its targets in 1975, 1976 and 1977, and monetary growth was above 11.5 percent in every year (Argy, Brennan, and Stevens, 1990, p. 54; Bernanke and Mishkin, 1992, p. 190).[104] Monetary growth on the M2 measure did, however, decline substantially in 1978 (see Table 1 above and the discussion titled "G. William Miller" in the next section).

Friedman also expressed exasperation at Burns' statement that monetary growth had not been excessive in 1976 (Instructional Dynamics Economics Cassette Tape 208, February 1977, Part 1). The FOMC did raise the federal funds rate in 1977, by about 200 basis points in total: it averaged 4.65 percent in December 1976, 6.56 percent in December 1977. Furthermore, testifying in mid-year about the rising funds-rate profile, Burns attributed the increase to FOMC moves to restrain monetary growth. But the Chairman also pointed to the stability of long-term bond rates as implying that the FOMC tightening had been sufficient (Burns, 1977a, p. 724).

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[101] Friedman (1978c, p. R−182). In this assessment, "the past two years" may have been a reference to 1976 and 1977.
[102] Bernanke and Mishkin (1992, Table 1, p. 190) recorded a similar rise. The data for old M2 in Simpson (1980, Table A2, p. 113) show a sharper move up, from 8.4 percent in 1975 to 10.9 percent in 1976.
[103] See Argy, Brennan, and Stevens (1990, p. 54) and Simpson (1980, Table A2, p. 113). (In Bernanke and Mishkin, 1992, p. 190, the reported M2 growth for 1977 had an evident typographical error, with 9.8 percent being incorrectly reported as 3.8 percent.)
[104] The pattern for M1 behavior differed from that of the broader aggregates. But, from mid-1976, M1, too, was largely characterized by growth in excess of the FOMC targets (Paulus, 1980, pp. 103−104). Indeed, during 1977, when M2 growth was high but formally within the Federal Reserve's generous target range, M1 growth was in excess of its own target band (*American Banker*, October 3, 1977, p. 3).

Indeed, the fact that long-term rates were quite low likely reflected not only unrealistically optimistic inflation expectations on the part of markets, but also downward pressure on real interest rates from still-easy monetary policy. In 1975, Burns had implied that monetary policy had a negligible effect on (real and nominal) long-term rates in the short run. By February 1977, he was willing to concede that long-term rates responded to movements in the federal funds rate "to a degree" (*American Banker*, February 10, 1977), but even in the FOMC meeting the following month, Burns insisted "there's only one interest rate over which we have a high degree of influence, and that is the federal funds rate."[105] A different outlook on the Federal Reserve's short-run influence on interest rates might have led Burns to view low long-term rates in mid-1977 as a symptom of monetary policy being too loose.

The failure of the monetary-growth targets to be met, together with the Federal Reserve's continuation of an operating procedure centered on setting the federal funds rate, led Friedman to conclude in September 1978 that "the major effect of monetarism has been on the language in which policy is expressed rather than on the content."[106] He would have been reinforced in this view by the observation of the aforementioned Federal Reserve Bank of Boston's president, Frank Morris, a couple of months earlier (*Wall Street Journal*, July 17, 1978, p. 1): "Most of us have learned a great deal from Friedman and the monetarists, but few of us accept the entire body of monetarist doctrine. I believe that monetarism may have peaked and that it now may be on the wane."

Monetary targeting and the law

Notwithstanding his dissatisfaction with Federal Reserve policy over this period, Friedman could at least point to some further legislative successes for his own approach to monetary policy. The requirement that the Federal Reserve state and pursue monetary growth targets, already initiated by Congress in 1975, was put on a more formal footing with the passage of an amendment to the Federal Reserve Act, signed by President Carter in November 1977.[107] Friedman later noted that this legislative move had, in essence, made a monetary-growth rule part of United States law.[108]

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[105] From Burns' remarks at the FOMC meeting of March 15, 1977 (Federal Open Market Committee, 1977b, p. 31).
[106] Friedman (1978b, p. R–185).
[107] The legislation, as signed and passed, is available at *https://fraser.stlouisfed.org/title/1040*. In the legislation's text, the amendments to the Federal Reserve Act it contained were separately and officially termed the Federal Reserve Reform Act, and that terminology would later be used by Federal Reserve Chairs (see, for example, Miller, 1978b, p. 185 [p. 1 of typescript version]; Bernanke, 2013, p. 7).
[108] See Friedman (1995, p. 175). Friedman also briefly referred to this 1977 legislation in Friedman (1982a, p. 107) and Friedman and Friedman (1985, p. 162).

As Friedman acknowledged, however, the new statute essentially repeated the wording of Congress' 1975 reporting requirements for the Federal Reserve's monetary targets.[109] It followed that the 1977 legislation amounted to a continuation of arrangements that Friedman had already concluded had not led to a material change in the substance of U.S. monetary policy. Indeed, Burns himself had remarked in an interview: "Our objective in life is not to hit the target, but [to secure] the best possible performance of the economy." (*New York Times*, January 4, 1976.)

Friedman wanted something more binding. Influenced by the analysis of a former student, Robert Weintraub, who worked as a staffer for the Congressional supervisors of the Federal Reserve, Friedman became persuaded that nothing short of a constitutional amendment imposing a monetary rule was needed to make the rule stick.[110] This idea was in harmony with Friedman's strong, if notably uncritical, interest in the public-choice literature. The idea led to the Friedmans' drastic, and hardly practical, proposal in 1980 (in the book version of *Free To Choose*) of a constitutional amendment that would consecrate a constant-monetary-growth rule for the United States—one that would apply, initially at least, to the monetary base.[111]

Other Congressional initiatives

Another legislative change in the 1977−1978 period that had a bearing on the Federal Reserve's targets was the Full Employment and Balanced Growth of 1978, also known as the Humphrey-Hawkins Act, which became U.S. law in late October 1978.[112] The Humphrey-Hawkins legislation further formalized the reporting process regarding the Federal Reserve's targeting of monetary aggregates. And it made the announced target ranges pertain to the whole year—rather than being routinely readjustable at every FOMC meeting.[113]

Other aspects of the legislation also had implications for monetary policy. But exactly what these implications were was a matter of differing interpretation, both at the time and

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[109] This included the resolution's expression of a dual mandate of maximum employment and price stability for the Federal Reserve. See Clarida (2021).

[110] See Friedman's (1995, p. 175) discussion, which cited Weintraub (1978) in support. See also Chapter 5 above.

[111] See Friedman and Friedman (1980, p. 308) as well as Friedman and Friedman (1985, p. 99). (On Friedman's growing interest in monetary-base-oriented rules during the 1980s, see Chapter 10 below.) Friedman (1985a, p. 61) himself acknowledged that a constitutional amendment to impose constant monetary growth was "highly unlikely."

[112] The bill was passed in its final form by Congress on October 15, 1978, and approved by President Carter on October 27, 1978 (see *https://fraser.stlouisfed.org/title/1034* and *https://www.presidency.ucsb.edu/documents/acts-approved-the-president-week-ending-friday-96*). DeLong (1997, p. 271) gave a date of 1977 as when the act was passed and signed. This date was not correct.

[113] See Chapter 4 above, as well as Paulus (1980, p. 103) and G. William Miller's testimony of February 21, 1979, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1979a, p. 8).

subsequently. The Humphrey-Hawkins Act called for the federal government to pursue full employment. In itself, this was not a new statutory requirement for either the federal government generally or the Federal Reserve specifically. The latter institution already had an assigned goal of maximum employment alongside a price-stability goal.[114] The act, as passed, was merely a statement of goals (and, in view of existing legislation on the books, really just a reaffirmation of goals).[115] It did so with some numerical specificity, however, calling for a 4 percent unemployment rate to be achieved by the 1980s (as well as inflation below 3 percent).[116] In view of this, some observers, both then and later, saw the Humphrey-Hawkins Act as setting employment goals inconsistent with price stability. DeLong (1997, p. 271), for example, viewed the Act as symptomatic of the policies that produced the 1970s inflation outcomes and as helping to encourage those outcomes (a characterization somewhat difficult to square with the fact that, as discussed in the next section and in Chapter 9, monetary policy *tightened* in both 1978 and 1979).

Closer to the time of the law's passing, Blinder (1983, p. 70) had reached a similar conclusion to DeLong. Blinder judged that the "Humphrey-Hawkins type definition of high employment" involved an unemployment goal far below the natural rate and so would be inflationary if used as the basis for monetary and fiscal policy settings. Later, however (and when he was himself a policymaker), Blinder (1994, p. 336), in essence, withdrew this criticism. He instead cast U.S. law as specifying an economic-stabilization goal for the Federal Reserve—but one that did not oblige the authorities to set a target for unemployment that was below the natural or full-employment rate of unemployment.

In the event, the Federal Reserve's own posture over the years from 1977−1978 onward was similar to that of Blinder (1994). The interpretation on which FOMC policymakers put the Humphrey-Hawkins and the other laws giving it an employment mandate was that they set an unemployment-rate goal but one that should evolve as policymakers' estimates of the natural rate

[114] It did so through the aforementioned amendment to the Federal Reserve Act in 1977: see, for example, Bernanke (2006b). (However, the implication in the same Bernanke speech that the Federal Reserve did not have a legislated price-stability goal *until* 1977 is at variance with how many pre-1977 policymakers, as well as numerous commentators including Friedman, interpreted existing statutory goals. For a discussion, see Nelson, 2020b, Chapter 12.)
[115] DeLong (1997, p. 271) incorrectly took the act as introducing a dual mandate of price stability and full employment for the first time.
[116] See *https://fraser.stlouisfed.org/title/1034* and *Newsweek*, March 5, 1979.

improved.[117] In addition, both in the years leading up to the act and after its passing, the Federal Reserve leadership was vocal on the point that the full-employment unemployment rate was higher than it had been in the past.[118] In so doing, they indicated, sometimes explicitly, that nonmonetary policies should be responsible for lowering that rate and that it was not appropriate for monetary policy to try to attain below-normal unemployment rates.

For his part, Friedman had vehemently opposed early versions of the Humphrey-Hawkins bill. These drafts had actually required the federal government to engage in large-scale public-sector hiring, in the event of the unemployment rate reaching values perceived as excessive, in order to attain the bill's specified unemployment target. Friedman had spoken out strongly against this measure in his *Newsweek* columns—both in 1974, when it was being mooted in Congress, and in 1976, when it was specifically associated with the Humphrey-Hawkins bill (*Newsweek*, September 2, 1974; *Newsweek*, August 2, 1976).

Friedman's *Newsweek* analyses considered two cases. In the first case, the extra public expenditure involved in the new federal hiring would not be monetized by the Federal Reserve. In such an instance, the hiring "would not reduce unemployment but simply add to government employment," a circumstance that would make "us all poorer" because the public employment would direct resources less efficiently than the market system (*Newsweek*, August 2, 1976). Contentions that public hiring added to overall output and employment, Friedman suggested in his 1974 column, were based on a "neglect of indirect effects" (*Newsweek*, September 2, 1974). His 1976 column had zeroed in on the "folly" in the economic analysis guiding Senator Hubert

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[117] The other laws included the Employment Act of 1946 and the aforementioned Federal Reform Act of 1977, both of which specified a "maximum employment" objective. On the fact that the maximum-employment remit has been interpreted by officialdom as referring to a long-run sustainable employment concept, see, for example, Judd and Rudebusch (1999) as well as Blinder (1997, p. 4). On this matter, Blinder added that "the FOMC has never officially adopted the natural rate Phillips curve as part of its intellectual framework." This situation in effect changed in 2012. However, even in a framework not based on the natural rate hypothesis, policymakers may regard it as desirable, on price-stability and other grounds, to avoid economic overheating and may interpret maximum-employment goal as something below a physical maximum. These positions were in fact those that prevailed in policy circles in the United States throughout most of the postwar period. For example, Karl Bopp, Federal Reserve Bank of Philadelphia, cautioned against "policies of overfull employment and inflation" and saw monetary policy as appropriately directed at promoting "maximum sustainable use of available resources" (Bopp, 1970, pp. 15, 16).

[118] For example, like Friedman and others, both Chairman Burns and his successor cited higher unemployment benefits as a factor pushing up the full-employment unemployment rate. Burns' own way of articulating this point was to observe that "there are always some people who get along very well on unemployment insurance and food stamps" (*Washington Star* (Washington, D.C.), November 25, 1976). (This remark bore out Burns' earlier observation—in *People Weekly*, April 14, 1975, p. 18—that "I am not much of a diplomat." Notwithstanding this, in the 1980s, Burns did become a senior U.S. diplomat.) On Chairman Miller's observation on the benefits/unemployment link, see Miller (1978a) and the discussion in Chapter 4 above.

Humphrey. Humphrey—who, during his spell away from the U.S. Senate in the 1960s, had been on the 1964 and 1968 presidential tickets, and so had been on the opposite side to Friedman in those campaigns—was moved to reply in *Newsweek*'s letters pages to Friedman's 1976 column. In that response (*Newsweek*, October 11, 1976*b*, p. 8), Humphrey cited the existence of economic slack and implied that the slack would not be absorbed unless the public sector grew: "The more than 7 million workers now unemployed attest to the failure of the private economy to absorb these workers." This reply would have grated with Friedman both in its implication that measured unemployment invariably recorded economic slack and that additions to government spending, for an unchanged monetary policy, provided a means of adding to overall economic activity.

The second case considered in Friedman's two *Newsweek* columns was that in which the fiscal expansion associated with public-employment measures was accommodated by Federal Reserve monetary easing. Friedman added that, in this case, new government demand would not be completely at the expense of "private demand[,] and so could create new jobs" (*Newsweek*, September 2, 1974). But he stressed that the end result would be to add to inflation, not to real economic activity.

Although Humphrey's bill contained price-stability as well as employment goals, and in his 1976 reply to Friedman he had affirmed that the "choice is not… between full employment and inflation," it was clear that Humphrey did not share Friedman's perspective on the demand/inflation linkage. Rather, in Humphrey's assessment (*Family Weekly*, July 3, 1977): "In recent years inflation has been the result of OPEC pricing policy, poor harvests, underutilization of machinery and equipment, high interest rates[,] and noncompetitive pricing by big business." Like so many who took a nonmonetary perspective on inflation, Humphrey saw stagflation as a vindication of cost-push views: "we have seen that as unemployment went up so did prices, and as inflation came down [after 1974] so did unemployment" (*Family Weekly*, July 3, 1977).

This clash of viewpoints on the inflation/unemployment relationship had been evident earlier, on October 20, 1975, when Friedman and Humphrey had a face-to-face encounter at a special hearing of the Joint Economic Committee in the city of Chicago. When, during the question-and-answer portion of Friedman's testimony, Humphrey observed that "unemployment produces inflation under the current situation," Friedman replied: "I don't believe that's quite correct."[119]

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[119] In Joint Economic Committee (1975d, p. 63).

The exchange between Humphrey and Friedman also concerned specifically the Humphrey-Hawkins bill. In the course of the discussion, Friedman stated his objections to the economic-planning and public-employment aspects of the bill. These objections led Humphrey to retort: "You are just as stubborn now as when I first met you. And, by the way, you are very effective."[120]

In the event, the version of the Humphrey-Hawkins bill that went into law—nine months after Humphrey's death—was considerably watered-down and lacked the public-employment provisions that Friedman had criticized. Friedman himself quickly became reconciled to the Humphrey-Hawkins Act, devoting a *Newsweek* column to the subject of "Implementing Humphrey-Hawkins" (*Newsweek*, March 5, 1979). In his 1976 rebuttal, Humphrey had complained: "Nowhere in his article does Professor Friedman even indicate that he supports full employment as a goal." In fact, Friedman had a lengthy record of endorsing full employment as a policy goal (see Nelson, 2008; 2020a, Chapter 8). His objection was instead to activist full-employment *policies*, which he proposed to replace with rules that would promote conditions of full employment without targeting output or the unemployment rate directly.

In his 1979 column, Friedman squared the low numerical unemployment target value in the new law with his own natural-rate framework by treating Humphrey-Hawkins as an injunction to lower the unemployment rate through improvement of the supply side of the economy, alongside adherence to fiscal and monetary rules that "would provide a more stable economic environment" (*Newsweek*, March 5, 1979).[121]

Federal Reserve transcripts

Still another area of monetary policy in which legislators were active in the later 1970s was that of Federal Open Market Committee policy communications.

As of the mid-1970s, the Federal Reserve adhered to the following practice with regard to FOMC meetings: In addition to regularly releasing, after a multi-week interval, minutes of the monthly meetings, it published transcript-like lengthy records (called the Memoranda of

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[120] In Joint Economic Committee (1975d, p. 63).
[121] The column argued that the policy rules that could implement the law's goals would be put in place or encouraged by a proposed constitutional amendment that Friedman had recently been involved in drafting. On this draft amendment, see Chapter 10 below.

Discussion) of the meetings.[122] During the Burns era, these transcript documents were issued with a roughly five-year lag (so, for example, those for the year 1966 were available by early 1972).[123]

In April 1975, however, Representative Wright Patman—who, the previous January, had been dislodged by Henry Reuss as chair of the House of Representatives' Committee on Banking, Currency, and Housing, but who still possessed oversight powers as chair of its subcommittee on monetary policy—formally requested that Chairman Burns hand over the as-yet-unreleased transcripts for meetings from 1971 to 1974.[124] Burns declined, on the grounds that a multi-year lag in the release of the transcripts was needed to foster a mood of candor and give-and-take in FOMC meetings (see Lindsey, 2003, pp. xii–xvii).

In the face of such Congressional pressure for early release of the transcripts, as well as a separate move being made in the courts to compel expedited release of a variety of FOMC documents (such as the meeting minutes), the FOMC announced in March 1976 that it would stop preparing the Memoranda of Discussion (Lindsey, 2003, p. xi).[125] By making this move, it seemingly signified that it would not keep transcripts or audio recordings for its meetings from mid-1976 onward (*Wall Street Journal*, December 20, 1993; Lindsey, 2003, pp. 8–10).
It was against that background that, in 1976–1977, Stephen Neal, the new chair of the U.S. House of Representatives' subcommittee on monetary policy (Patman had died in March 1976), solicited responses from experts, and held hearings, on the subject of maintaining and making public the FOMC meeting transcripts.[126] Friedman's own contribution was a letter to Neal dated

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[122] These publications described the meeting's exchanges in the third person. But they provided detailed, chronological accounts of individual participants' contributions to those exchanges. Their transcript-like quality was reflected in Warburton's (1976) characterization of them as "mechanically reported verbatim records."
[123] See Federal Open Market Committee (1971, pp. 90–93).
[124] Patman apparently did not apparently request the 1970 transcripts, whose public release was due within a year.
[125] On the legal actions made against the Federal Reserve at the time, see *Washington Star* (Washington, D.C.), November 25, 1977, Goodfriend (1986, pp. 65–78), and Lindsey (2003, pp. 4–24), as well as the capsule account by Friedman and Schwartz in the *Wall Street Journal* of December 20, 1993.
[126] The published volume to result from these inquiries was *Maintaining and Making Public Minutes of Federal Reserve Meetings*. This was a misleading title, as the FOMC already did continue to maintain minutes (formally titled the Record of Policy Actions and Minutes of Actions) for its meeting, and these documents continued to be regularly released from the mid-1970s onward without interruption. The Neal investigation, like the Patman request, had been concerned with whether transcripts (*a.k.a.* the Memoranda of Discussion), not the minutes, should be maintained (and be released in an expedited manner). Friedman himself, like Burns and many others, referred to the Record of Policy Actions as the meeting minutes notwithstanding his 1976 practice (discussed presently) of referring to the transcripts (strictly speaking, the long, but third-person, description of the meeting) as the minutes. (On Friedman's usage of "minutes," see Nelson 2020b; for Burns' usage of this terminology, see Lindsey, 2003, p. xiii. Burns' successor, G. William Miller, also followed this practice: see his April 1978 testimony quoted below.) The element of validity in calling the transcript-type long policy-meeting descriptions the "minutes" is that the

October 2, 1976. This letter remarked that Neal's questionnaire "touched a very sensitive nerve in my particular case. When Anna Schwartz and I were writing our *Monetary History of the United States* we were denied access to the minutes of the Open Market Committee by the Federal Reserve System at that time."[127] (Here, by "minutes" Friedman meant not the minutes, but instead what were, as noted above, known by the 1970s as the Memoranda of Discussion—that is, the published versions of the meeting transcripts.) Friedman recalled how he and Schwartz, in writing their *History*'s account of monetary policy in the 1930s, had been able to compensate for the absence of these documents for FOMC meetings (as well as the meetings of the predecessor committees to the FOMC) by using former Federal Reserve Bank of New York president George Harrison's publicly-available papers. These papers, deposited with Columbia University, had included internal Federal Reserve documents, including detailed policy-meeting memoranda.

In his letter, Friedman made the point that the start of the Federal Reserve's practice of releasing same-year FOMC minutes had begun in 1967, while its practice of releasing FOMC transcripts after a five-year lag had begun in 1964.[128] Prior to these changes, the minutes for the FOMC meeting in a given year had been released only in the following year's Federal Reserve Board *Annual Report*, while transcripts had had no official release at all. Friedman suggested that the likely catalyst for these mid-1960s increases in FOMC disclosures was the release of the Friedman-Schwartz *Monetary History*. The implication was that the Friedman-Schwartz study provided impetus for enhanced FOMC meeting documentation: both because their work had highlighted the limited amount of releases the Federal Reserve had made to date on FOMC deliberations; and also because the Federal Reserve hoped that issuance of its historical records would prompt other researchers to produce rival studies that disputed the *Monetary History*'s mostly-negative verdict on the Federal Reserve's historical monetary policy performance.[129]

Other accounts have pointed to different reasons why the FOMC started to release these

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Federal Reserve's name for the Memoranda of Discussion before 1967 was the "Minutes" (see Danker and Luecke, 2005, p. 176, and *https://www.federalreserve.gov/monetarypolicy/fomc_historical.htm*).

[127] Friedman (1976g, p. 201). See also *Wall Street Journal*, December 20, 1993, and Friedman and Friedman (1998, pp. 233–235).

[128] To be more specific: In late August 1964, the Federal Reserve Board announced that a duplicate of the FOMC meeting transcript-minutes for the years from 1936 to 1960 would be deposited at the National Archives and made available there. See Yohe (1965, p. 352). In practice, copies became available in various other publicly accessible locations too.

[129] See also *Wall Street Journal*, December 20, 1993.

documents in the 1960s.[130] Whatever the reasons, however, it remains the case that Chairman Burns was being accurate when he remarked in his 1975 response to Patman that, as things stood, the FOMC regularly issued a "copious body of information" concerning its decisions, separate from that in the transcripts.[131]

True, the Federal Reserve had, by 1975, acquired a reputation for obfuscation that was now hard for it to shake. For example, *Business Week* (April 28, 1975) had editorialized that the "Fed itself is sticking to the traditional policy of saying little, and what it says comes out in circumlocutions that require translation and interpretation." But, in fact, the FOMC minutes were not vacuous. They contained material information about the FOMC's thinking, and the Federal Reserve Board staff of the time prided themselves on keeping accurate records of the meeting.[132] Furthermore, these releases could be taken in conjunction with the large number of speeches by Burns and other FOMC policymakers, the Congressional testimony of Burns and other Board members, as well as other material in the *Federal Reserve Bulletin* and in the Board's annual reports. The sum of these items provided contained ample material for ascertaining policymakers' doctrine and strategy and the motivation for FOMC decisions during the 1970s—and they were used for this purpose by researchers such as Poole (1979) and Romer and Romer (1989, 2002).

From the mid-1970s onward, Friedman was heavily influenced by the public-choice literature (see Nelson, 2020a, Chapter 8, as well as the next chapter). This perspective made him extremely disdainful from about 1975 on when it came to much of the Federal Reserve's public descriptions of its policies. With regard to strategy and doctrine, Friedman would, as already indicated, contend that the monetary policymakers understood the monetary nature of inflation. Correspondingly, he implied that their public appeals to cost-push explanations amounted simply to disinformation.[133]

But it seems that, instead, Friedman was under a mistaken impression when he supposed in the later 1970s that Burns and other policymakers no longer subscribed to cost-push theories. Their actual rejection of such theories did not occur until a little after the Burns era. With regard to

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[130] See Nelson (2020b) on these alternative accounts.
[131] From Burns' letter of June 3, 1975, to Patman, as reproduced in Lindsey (2003, p. xvi).
[132] For example, Robert Holland, at different times a staff member and governor of the Federal Reserve Board, wrote that as "secretary of the Federal Open Market Committee during the year 1972, [I] was responsible for an accurate rendition of what took place at its meeting." (*New York Times*, September 1, 1974.)
[133] A similar position was taken later by Meltzer (2009b).

individual policy decisions, Friedman would lament in 1988 that FOMC policy documents told him the policy actions take but not "the policy that produced these actions" (*Wall Street Journal*, April 15, 1988). This conclusion, however, was flawed by a failure to consider a wider body of official policy materials the Federal Reserve had put on the public record, including the policy minutes. Romer and Romer (1994b, p. 81) found that these materials were informative about "the purpose of the policy" underlying FOMC behavior in 1987–1988. This was the very period for which Friedman in 1988 had complained Federal Reserve public documents were unrevealing about the consideration driving monetary policy.[134]

In contrast to his jaded post-1974 attitude toward FOMC communications, Friedman was more on track earlier when, in 1970–1974, he indicated that he understood the thinking behind Federal Reserve decisions but that he disagreed with that thinking. And with regard to the reaction function, in the late 1960s and, in the early 1970s, both he and Samuelson indicated they saw systematic aspects in the Federal Reserve's reaction function; again, in Friedman's case the key objection was disagreement with the reaction function followed.[135]

Specifically, before 1975 Friedman's reservations stemmed not from a notion that the Federal Reserve's reaction function could not be ascertained, but instead on the fact that the reaction function followed did not give rise to a predictable pattern for monetary-growth; in addition (though relatedly), he objected to the absence of an articulated, announced long-term plan by the Federal Reserve. He favored a different instrument (reserves or the monetary base), with a changed implied reaction of interest rates to the state of the economy than prevailing previously, alongside public announcement of target rates of monetary growth. And once the Federal Reserve adopted formal monetary targets in 1975, neither the instrument choice nor the reaction function seemed to Friedman best suited to achieve those targets.[136]

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[134] See the coverage of 1987-1988 monetary policy on pp. 81–82 of Romer and Romer's (1994b) section, "Monetary Policy Since 1987." Their analysis drew on the Record of Policy Actions (the then-name for the FOMC meeting minutes), which had been reprinted in the same Federal Reserve Board (1988) *Annual Report* that Friedman had criticized as vacuous.

Friedman and Schwartz's own work had demonstrated that the public documents of the Federal Reserve were revealing about the reasons for policy decisions. This is brought out by the fact that for a quotation from the Federal Reserve regarding the reason for the 1936 decision to increase reserve requirements, Dewald (1975, p. 154) cited "Federal Reserve technical memorandum quoted in Milton Friedman and Anna J. Schwartz, *A Monetary History of the United States*…" The quotation (indeed used by Friedman and Schwartz, 1963a, p. 526) actually came from the Federal Reserve Board's annual report, not an internal memorandum.

[135] For documentation of these points, see Nelson (2020a, Chapter 8; 2020b, Chapter 15) and Chapter 2 above.

[136] In this later period, Friedman layered onto his technical objections to the Federal Reserve's reaction function criticisms that reflected his acceptance of the public-choice literature's arguments. For example, he argued that

Rather than positing a relationship between transcripts and disclosure of the reaction function, Friedman's 1976 letter focused on the transcripts' historical value. He called for the Federal Reserve's creation of Memoranda of Discussion to be resumed and for their release to be two years, rather than five years, after the meetings.[137] This two-year suggestion implied he likely had some sympathy with the Burns position that a too-rapid release of the transcripts would limit the candor of the meeting discussions. It is also notable that, even during his post-1974 era of enhanced cynicism regarding the reliability of public statements by the Federal Reserve, Friedman did not himself argue that rapid (that is, a same-year, and perhaps near-immediate) transcript release was necessary for clarity about the reaction function. In contrast, in his own letter to Neal, Robert Lucas (1976a) argued that immediate release of the FOMC transcripts was necessary, in order for the FOMC's reaction function to be known by the public.

The Burns FOMC was not really very secretive about its reaction function, as it did relate policy-rate decisions to the state of the economy. It is true that, as already noted in Chapter 5, Burns did not want to disclose his intentions or the FOMC's and tensions regarding *future* interest rates. But the disinclination to engage in discussion about interest-rate prospects (that is, about the interest-rate values that are most likely in future periods) is, in principle, separate from transparency about the reaction function (that is, about the mapping between current monetary policy decisions and the state of the economy). And, as it turned out, the FOMC transcripts for the later Burns years did not turn out to be particularly forthcoming on policymakers' assessments interest-rate prospects. They were instead marked by much the same tendency to avoid discussion of the future rate decisions as that Burns exhibited in public. As Burns said on the record in 1977, "I won't talk about the future. I don't know what the future will be." (*Wall Street Journal*, October 7, 1977, p. 33.)

In the Burns era, as in other periods, the transcripts supplemented and fleshed out information on the FOMC's decision-making. But they were not a unique or even essential source for grasping the thrust of Committee thinking. Rather, they supplemented voluminous material that was in the public record far earlier than the transcripts. Indeed, the FOMC transcripts for the second half of the 1970s, as eventually released, verified that parts of the public record that Friedman and subsequent writers sometimes dismissed as mere disinformation for public consumption—such as the Federal Reserve's continued appeal to cost-push explanations for inflation, and

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central-banking and commercial-banking interests were opposed to reforms of monetary-control procedures, and that the political environment was a force working against implementation of a noninflationary monetary policy.

[137] Friedman (1976g, p. 202).

Arthur Burns' contention that monetary policy was not overexpansive in 1976 and 1977—actually reflected the positions genuinely taken by policymakers, as they also articulated them in their internal deliberations.

The largest missing element among the information released publicly by the Federal Reserve in the 1970s consisted of high-frequency releases about FOMC decisions. The actual policy decision made at any FOMC meeting was typically not subject to a formal public disclosure until the release of the meeting minutes some weeks later (although financial observers were usually able to glean the essence of the decision well ahead of that time, by observing the open market operations taken by the Federal Reserve after the meeting). Friedman's 1976 letter had called for ending this situation, by having the FOMC's policy directive be made public immediately after the FOMC meeting.[138] The problem of the absence of immediate postmeeting official information was not resolved until 1994, when, under Alan Greenspan, the FOMC started the practice of releasing a policy-decision statement immediately after each meeting.

Although this gap in information prevailed throughout his tenure, in other respects Arthur Burns seemed to take to heart—in his later years as Federal Reserve Chairman—the criticism that the Federal Reserve was insufficiently transparent. A few weeks after *Business Week* (April 28, 1975) editorialized that "Arthur Burns should stop playing a man of mystery," the *Washington Post* (May 22, 1975) published an article titled "Arthur Burns Goes Public," which highlighted his recent agreement to give a television interview for NBC's *Meet the Press*. Burns followed this with other public media engagements, including an appearance of ABC's own Sunday morning news show, *Issues and Answers* (January 18, 1976), a long interview with *U.S. News and World Report* (May 17, 1976), and press conferences in November and December 1976 (*Washington Star* (Washington, D.C.), November 25, 1976, and December 6, 1976).[139]

Furthermore, with regard to the FOMC, Burns in 1975 and 1976 presided over moves to more-rapid release of the meeting minutes. Under his tenure, the lag between FOMC meetings and meeting minutes' public disclosure went down from the ninety days introduced in 1967, to 45 days, then to about a month—so that, by the time of new FOMC meeting, the minutes for the previous meeting were already publicly available (*Washington Star* (Washington, D.C.), November 25, 1977, p. A–6; Lindsey, 2003, pp. xi, 14–17).

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[138] Friedman (1976g, p. 202).

[139] It was earlier reported (*Washington Post*, May 22, 1975) that Burns was considering having a press conference every six weeks as a permanent arrangement—a proposal with which he did not ultimately proceed. (Burns had also held press conference on rare occasions before 1975—see, for example, *Seattle Times*, October 15, 1973.)

These efforts at greater transparency would, however, be enormously overshadowed by a countervailing action on Burns' part, taken in 1976 and disclosed in 1993, that would put paid to any prospect that he would be remembered as being forthcoming about monetary policy. As noted above, in 1976 the FOMC strongly implied, via its announcement that it would discontinue its Memoranda of Discussion, that it would no longer be keeping meeting transcripts.[140] In fact, this was an untruthful impression. The Federal Reserve continued to make, and keep, transcripts of FOMC meetings from 1976 for internal purposes.[141] Indeed, Burns took his own copies of the transcripts from his tenure with him when he left the Federal Reserve in 1978, and these were included in the papers he subsequently deposited with the Gerald Ford presidential library (see Lindsey, 2003, p. 13). Friedman and Schwartz were among those criticizing the Federal Reserve when, in 1993, the fact of the withholding from the public of the existence of the transcripts became widely known (*Wall Street Journal*, December 20, 1993). The practice of releasing transcripts five years after an FOMC meeting was subsequently restored, and the Federal Reserve also published the meeting transcripts from the 1976–1993 period.[142]

The end of the Burns era

Burns' second term as Federal Reserve Chairman was due to expire at the end of January 1978. When Jimmy Carter was elected, Friedman speculated that, if Carter chose not to reappoint Burns, the most likely nominee for the Chair position was Arthur Okun (Instructional Dynamics Economics Cassette, Tape 202, November 1976, Part 1). However, a year later, Okun publicly ruled himself out of consideration for the position (*Washington Post*, November 18, 1977).

[140] Similarly, Chairman G. William Miller later stated: "Those Memoranda of Discussion, of course, have been suspended recently; I hope that they will be reinstated, so that there will be a historical base for analyzing decisions." However, he also said that he was still "learning about" the FOMC meeting process, and he may not have known that meeting discussions were still being transcribed—and that what had been suspended was (only) the preparation of a public version of the transcripts. (The quotations are from Miller's testimony of April 10, 1978, testimony, in Committee on Banking, Finance and Urban Affairs, 1978b, p. 127.)
[141] This usage appears to have been limited to the creation of the public minutes, which then became the main record of the meeting consulted, rather than for regular referencing in place of the minutes. Consistent with this, Federal Reserve Chairman G. William Miller said to a Congressional questioner on April 10, 1978: "the minutes that I have [of the February 1978 FOMC meeting] are the same that you have" (in Committee on Banking, Finance and Urban Affairs, 1978b, p. 125).
[142] The 1970s Memoranda of Discussion up to 1976 that had not been released by 1976 had already been released on the five-year-lag schedule. For example, writing in 1978, Poole (1979, pp. 478–479) used the Memoranda of Discussion through 1972 (though referring to them as "*FOMC Minutes*") used the Memoranda of Discussion through 1972, while in the same proceeding Jordan (1979, p. 497) noted that the coming years would see the public release of the Memoranda of Discussion for 1973, 1974, and 1975. (Those prepared for early 1976 were released as well.)

By this time, Friedman had actually come to the view that Carter would likely decide to reappoint Burns, despite the public airing of several disagreements between the president and the Federal Reserve leadership. Having Burns at the head of monetary policy, Friedman reasoned, meant that the administration could be critical of the Federal Reserve, rather than being associated with its policies.[143] In mid-December 1977, Friedman assessed that the "chances are good" that Burns would be reappointed (*St. Louis Globe-Democrat*, December 16, 1977). But on December 28, 1977, Carter announced that he would be nominating a new candidate, G. William Miller, for Federal Reserve Chairman (*Dallas Morning News*, December 29, 1977).

Friedman expressed doubt that Carter's decision would have much bearing on events. "On very few occasions in the past," he remarked in reaction to the Miller nomination, "has the name of the man who is chairman of the Fed made much difference." (*New York Times*, December 30, 1977, p. D3.)[144] "Policy is unlikely to change," Friedman observed (*Journal of Commerce* (New York), December 30, 1977). In particular, he had already indicated that he expected inflationary policies to continue whether Burns was reappointed or not.[145]

Shaping Friedman's judgment was the tenure of Arthur Burns. Speaking before the Miller nomination was announced, Friedman alluded to his 1969–1970 euphoria over Burns' initial nomination and appointment (see Nelson, 2020b, Chapter 15) when he noted that "the name of the man who is Chairman of the Federal Reserve Board makes far less difference than at one time I thought it did."[146]

For the moment, the perception of Burns among news and financial commentators lined up with the characterization given in a *Wall Street Journal* article (May 4, 1977) that "Mr. Burns is perhaps the nation's best-known inflation fighter." In contrast, shortly before the announcement that Burns would not be reappointed, Friedman would describe him as having presided over "a Fed which is promoting inflation" (*St. Louis Globe-Democrat*, December 7, 1977). Friedman saw the esteem in which Burns was held by others as par for the course, and he would later declare that "[n]o major institution in the United States has so poor a record of performance over

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[143] Friedman offered this scenario both in Instructional Dynamics Economics Cassette Tape 208 (February 1977, Part 1) and a talk at the Commonwealth Club of San Francisco on November 11, 1977 (see Friedman, 1977k, p. 494).
[144] Friedman made a similar remark in Friedman (1982a, p. 103).
[145] See Friedman (1977k, p. 494) and *St. Louis Globe-Democrat*, December 7, 1977.
[146] Instructional Dynamics Economics Cassette Tape 215 (January 1978; recorded in December 1977).

so long a period yet so high a public reputation as the Federal Reserve."[147] However, with regard to Burns specifically, Friedman's verdict on the Chairman's record would, in the years after 1977, become much more widely accepted. For example, in 1987 the *Financial Times* judged that Burns had "enjoyed starry-eyed respect on Wall Street for longer than his track record probably justified."[148]

Contributing greatly to these retrospective judgments—as well as buttressing the negative verdict on Burns that Friedman was voicing during 1977—was monetary policy's behavior during Burns' final year or so as Federal Reserve Chairman. In an assessment written a little while before Burns left office, Friedman stated: "Monetary policy during 1977 was not in my opinion conducted effectively." The new monetary explosion, Friedman said, meant "we have, for the fourth time in fifteen years, paid the cost of a recession to stem inflation and then thrown away the prize by starting off on a new inflationary path."[149]

The same letter criticized what Friedman called the Federal Reserve's "futile attempt to control interest rates." Indeed, Friedman saw the Federal Reserve's reliance on the federal funds rate as a major factor behind the resurgence in monetary growth. Although Burns' FOMC had raised the federal funds rate during 1977, Friedman believed that the Federal Reserve had been too slow in doing so and had thereby permitted rapid monetary growth.[150]

Such a pattern largely repeated what had been observed earlier in Burns' tenure. For example, a research article published in 1977 on the Federal Reserve's federal funds rate reaction function found, on the basis of a sample period of December 1970 to December 1974, that "the Federal Reserve has been willing to move its operating instrument by only relatively small magnitudes, on average, in response to undesired growth in money" (DeRosa and Stern, 1977, p. 218). This regularity was prevalent not only in the first half of the 1970s but over almost the whole decade. In the 1980s, Friedman would note that the two years after formal monetary targets were introduced in 1975 did not lead the Federal Reserve to abandon stabilizing interest rates in the

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[147] Friedman (1984a, p. 24). Friedman made a related observation in *Newsweek*, May 2, 1983, as well as a near-identical remark in the *Wall Street Journal*, April 15, 1988.
[148] This judgment appeared in an editorial of May 26, 1987, a month before Burns' death.
[149] Friedman (1978d, p. 156).
[150] Jerry Jordan likewise judged (in *American Banker*, March 24, 1983, p. 10) that the FOMC "resist[ed] rising market interest rates in 1977 and 1978 by rapid injections of money and credit." It happens, as discussed below, that this characterization applies more to 1977 than to 1978, at least if M2 (rather than M1, the aggregate that Jordan tended to prefer at the time) is used as the measure of money.

short run.[151] The retrospective judgment of David Lindsey, Friedman's former student and a Federal Reserve Board staffer in the later Burns years, affirmed that this stabilization pattern had been at the expense of longer-run monetary and macroeconomic stability. "The results for inflation in the 1970s suggested that the FOMC had acted 'too little too late,'" Lindsey remarked in a 2003 Federal Reserve Board staff memorandum.[152]

It was a severe disappointment to Friedman that Burns' FOMC did not genuinely change the Federal Reserve's operating procedures and had, instead, maintained the federal funds rate as its operating instrument. Burns opposed a move to a reserves-control system to the end: in a September 1977 letter, the Chairman maintained that "technical adjustments in our procedures… would not improve matters" (quoted in *American Banker*, October 3, 1977, p. 3). To Friedman, this position was symptomatic of Burns' having become a "captive of and spokesman for the bureaucracy he supposedly commands" (*Newsweek*, July 24, 1978).

Indeed, the issue of monetary control ranked close to the topic of incomes policy as a source of strain between Friedman and Burns. One of the senior staff at the Federal Reserve Board of the time, and a former Friedman student, Stephen Axilrod, observed of Burns and Friedman, "Their very close friendship, I think, kind of broke up because they didn't agree on the money supply… Milton became really ticked off. I can't remember all the letters I wrote to him, over Burns' signature, on this subject, but there were a lot of them—maybe three; and, on this subject, three is a lot."[153] Axilrod, however, traced Burns' resistance to changes in monetary-control arrangements less to the staff influence than to the fact that "Burns, essentially, did not believe at all in money." (Stephen Axilrod, interview, April 24, 2013.)

Paul Volcker, who observed the Burns/Friedman relationship first from the vantage point of the U.S. Treasury (from 1971 to 1974) and then (from August 1975) as president of the Federal Reserve Bank of New York, observed that "they were deadly enemies. Though it was funny, because Friedman was a student of Burns. But Burns was very unhappy with [Friedman's

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[151] See Friedman (1982a, p. 108) and Friedman and Friedman (1985, p. 95).
[152] Lindsey (2003, p. x).
[153] In addition to direct correspondence with Friedman about monetary-control issues, Burns had to write or commission Federal Reserve replies to Congress' expressions of interest in Friedman's proposals in this area. Chapter 4 above gave one such example: the exchange stemming from Friedman's November 1975 Congressional testimony. An earlier example occurred in 1971, when Burns wrote a letter (dated May 3) to William Proxmire, who had asked for a response to Friedman's *Newsweek* column (May 3, 1971), "Money Explodes." As was his custom by this point, Burns made mininal reference to Friedman: he mentioned him fleetingly at the start of his three-page, single-spaced letter (see Burns, 1971b).

outlook]… I'm trying to reconstruct a personal relationship insofar as I knew it, and I didn't know it all that much. But I did know that, intellectually, they'd become enemies." (Paul Volcker, interview, October 16, 2013.)

During 1977, Burns did affirm the desirability of slowing the growth in the money stock over time, as already indicated. However, in response to a statement by Allan Meltzer, who had said, "The Fed simply tried too long to hold down short-term interest rates," Burns defended compromising between stability in short-term interest rates and achievement of the monetary targets. He maintained that, in seeking a reconciliation between these goals, "I'm a quasi-monetarist" (*Wall Street Journal*, October 7, 1977).

With the announcement of Burns' departure, Friedman seemed relieved that he would no longer have to criticize Burns on current monetary policy matters. He opined that Burns could "render greater service" as a private citizen than as a policymaker (*Journal of Commerce* (New York), December 30, 1977). He looked forward to Burns having more time to participate in public discourse on nonmonetary economic issues, such as the role of the public sector, on which Friedman felt Burns had spoken eloquently while Chairman (*Newsweek*, January 9, 1978*a*).[154]

Friedman's relationship with Burns, already considerably improved by 1978 over its state earlier in the decade, would subsequently move back toward its previous state of amicability. Gloria Valentine (interview, December 5, 2013) noted that in her initial years as Friedman's secretary Burns was "angry with Professor Friedman about his criticism of the Fed," and she observed that, after leaving office as Federal Reserve Chair, "Arthur Burns came and visited Hoover, and he and Friedman talked; it was as if they had never had a problem." Burns and Friedman would work together on Republican candidate Ronald Reagan's economic team during the 1980 presidential election campaign (see Chapter 10).

In these circumstances of restored relations, Friedman was reluctant after 1978 either to criticize Burns' record or to recount in detail his previous criticisms of Burns—though he occasionally did both of these things.[155] This self-denying ordinance was likely reinforced by the reality that,

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[154] Friedman also had some favorable things to say regarding Burns' statements concerning monetary policy. See the discussion titled "G. William Miller" in the next section.

[155] Silber (2012, p. 150) notes that Friedman did not mention Burns in his *Newsweek* column of February 19, 1979, which criticized the monetary policy of the 1970s. However, most inaccurately, Silber (pp. 150, 151) suggests that Friedman's columns *never* held Burns accountable for the inflation of the 1970s, a claim that overlooks many of Friedman's columns (and other commentaries) during Burns' tenure. (The earliest Friedman *Newsweek* column that

once appointed U.S. Ambassador to the Federal Republic of Germany in mid-1981, Burns was in no position to respond publicly to criticism, as well as by the fact that Friedman largely supported the Reagan Administration foreign policy that Burns had been assigned to help pursue. When, however, the subject of monetary policy in the 1970s came up in his research or policy writings, Friedman repeated his earlier criticisms, and he would remain willing to name Burns on occasion in that context.[156]

Friedman's continuing wish to put distance between his and Burns' views on monetary policy showed up in other ways, too. When Burns left the Federal Reserve in early 1978, he joined Washington, D.C.'s American Enterprise Institute (AEI), with which Friedman was already affiliated. Later in 1978, the AEI released a massive book collecting a large number of Burns' speeches as Federal Reserve Chairman.[157] The only mention of Friedman in the book (Burns, 1978) was a listing of him, ahead of the contents page, on the AEI's Council of Academic Advisers. The listing was routine, but it did give the appearance of Friedman's participation in approving the book for publication. In fact, the book could not meaningfully reflect the AEI advisers' input, as it was simply a reprint of published speeches. And these speeches included several Friedman had criticized over the years, including the notorious Burns (1970) advocacy of incomes policy that had triggered their initial rift (and which had not even been previously printed in the *Federal Reserve Bulletin*).

In a past era, during Burns' years of NBER seniority, two large books coauthored by Friedman had each contained, at the NBER leadership's insistence, an appendix consisting of a rebuttal to the contents of the main text penned by a NBER director. At the AEI, Friedman's advisory position was somewhat analogous to that of the NBER directors, as both these positions implied some role in giving advice on publication approval. Notwithstanding this parallel, there was no possibility that a Friedman reply to Burns' monetary views would appear in Burns' AEI book. This would not have been acceptable to Burns. Nor would Friedman have been well disposed toward writing one, in view of his inclination to let sleeping dogs lie when it came to his previous arguments with Burns on monetary policy. But the Burns speech compendium

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Silber considers was written after Burns' successor had been named.) For many counterexamples, see Nelson (2013a; 2016; 2020a, Chapter 5).

[156] For example, Friedman named Burns (as well as Martin and Miller) in his indictment of monetary policy since the early 1960s, for example in the *Free To Choose* television series (U.S. version, Episode 9, "How to Cure Inflation," March 7, 1980, p. 6 of transcript), *Newsweek*, May 2, 1983, Friedman (1984a, pp. 27, 55), and Taylor (2001, p. 105). See also Chapter 11 below.

[157] Beyond these, the book also included one speech delivered in 1969 and another given in September 1978.

appeared when Friedman, who was in any event in the process of cutting his ties to the East Coast, was becoming increasingly worried that the appearance of his name as an academic adviser in AEI books might give the appearance of approval of or consultation on the contents—when he certainly had not been for books such as Burns (1978), and in contrast to the past he no longer had the time or disposition to review AEI books, if asked. Against this background, Friedman relinquished his affiliation with the American Enterprise Institute, including his advisory position, in 1979.[158]

A glimmer of praise for monetary policy in the late phase of Burns' tenure was discernible from a remark Friedman made in a *Newsweek* column in 1981. Although his 1978 article on Burns' imminent departure had observed that "the actual rates of monetary growth, as determined by the Fed with Arthur Burns as chairman, have risen rather than decreased" in recent years (*Newsweek*, January 9, 1978*a*, p. 53), Friedman would subsequently grant, in his *Newsweek* of column of June 15, 1981, that monetary growth had begun to slow in late 1977.

This finding was based on the modern (post-1979) definition of M2. However, Friedman had earlier made a similar judgment on the basis of the old definition of M2 and had indicated that monetary growth started coming down in October or November 1977 (*Newsweek*, April 24, 1978; *The Register* (Orange County, California), December 23, 1979, p. E11).[159] This result is also evident in Table 1 above. That table shows that, in the case of both old and new M2, monetary growth at annualized quarterly rates moved out of double digits during 1977. For the twelve-month growth rate, the shift out of double digits was somewhat later: in January 1978, new M2 was 9.8 percent above its level a year earlier—high, but the first single-digit rate recorded since June 1975.

Later studies by Sims and Zha (2006) and Bianchi, Lettau, and Ludvigson (2018), utilizing modern econometric techniques and variously drawing on monetary-growth and interest-rate data, have affirmed Friedman's position that the switch away from an inflationary monetary policy began in late 1977—earlier than the oft-cited October 1979 date.

Thus, at the tailend of Burns' tenure, the Federal Reserve had begun to end the second monetary explosion. This policy tightening would continue under Burns' successor.

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[158] See Friedman and Friedman (1998, p. 344).

[159] In the same vein, Friedman in the *Chicago Tribune* (April 2, 1978, p. A7) gave the period of very rapid monetary growth as the year 1976 and early 1977.

III. PERSONALITIES IN DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1977–1978

G. WILLIAM MILLER

It seemed to some observers that G. William Miller had come from nowhere to be in charge of U.S. monetary policy: the first *Newsweek* issue to appear following his nomination described him as an "unknown quantity" (*Newsweek*, January 9, 1978*b*). But Miller had, in fact, established a profile on national economic issues for some years. The statements that Miller (as head of Textron, Inc., based in Providence, Rhode Island) made in these contributions did indicate a few areas of agreement with Friedman on economic issues. For example, Friedman had long been an advocate of relaxation of travel and nonmilitary trade with the USSR and China (see Chapter 10). So too was Miller: he had made headlines in 1966 when he obtained permission from the State Department to visit China (*The Detroit News*, July 13, 1966), and in 1970 Miller arranged a visit the USSR to encourage commercial relations (*Tampa Tribune* (Florida), May 5, 1970). But there were also areas of major disagreement, as discussed below.

Miller, who had been head of Textron since 1960, remarked in 1974: "I love the job and have lots of fun doing it. One problem, though: I don't see how I can get a promotion." (*Boston Globe*, February 4, 1974.)[160] President Carter would remedy this problem by nominating Miller to a leading position in national economic policy, and Miller was confirmed by the U.S. Senate as the new Federal Reserve Chair in March 1978.

Miller and macroeconomics: 1969 to March 1978

Miller did not have Burns' background of formal economics training, and he did not claim otherwise. "I cannot profess to have the qualifications to step in[to] Dr. Burns' shoes," Miller remarked alongside Carter and Burns at the press conference announcing his nomination (*Dallas Morning News*, December 29, 1977). A couple of months later, in a Congressional hearing held about a month after Miller was sworn in, a legislator began haltingly, "Dr. Burns—Dr. Miller,"

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[160] Although he was head of Textron throughout the 1960–1978 period, the formal positions Miller occupied in the firm varied somewhat over time. He was chief executive officer 1968–1978, president 1960–1974, and chair 1974–1978. He had joined the firm in 1956 and been its vice president from 1957 to 1960 (Europa Publications Limited, 1986, p. 1095).

promoting Miller's reply, "Neither Dr. Burns nor Dr. Miller is here!"[161]

Miller was, however, not new to monetary policy, as he had been serving for many years on the board of directors of the Federal Reserve Bank of Boston. This was not a *bona fide* policymaking position. It was nevertheless a senior (though part-time) role—one that involved receiving briefings on the economy and monetary policy, as well as consultations with the bank president (who, being an FOMC meeting participant, actually *was* a policymaker). Duesenberry (1983, p. 126) contended that, notwithstanding Miller's success in the private sector, it was really his Federal Reserve Bank of Boston service that put him in the running for the Federal Reserve Chair position. Duesenberry also suggested that, during this service, Miller had been schooled in Federal Reserve System practices by the bank's president, Frank Morris. As already indicated, Morris believed in the late 1970s that the tide was turning away from Friedman's ideas; consequently, it is likely that the impression conveyed to Miller of Friedman's economics from Morris' schooling was far from favorable.

In his capacity as a director, Miller also attended some of the Federal Reserve Bank of Boston's research events. As early as June 1971, for example, Miller was an attendee of the bank's conference on consumer spending and monetary policy. This event attracted not only Keynesian luminaries like Duesenberry, Modigliani, Tobin, and Garder Ackley, but also Friedman's former students Phillip Cagan, David Meiselman, Richard Selden, and Beryl Sprinkel, as well as his former teacher, Homer Jones.[162]

Furthermore, as a board member, Miller interacted with Robert Solow, who overlapped with him as a director. Solow recalled, "I was the only economist on that board, other than Frank Morris." Consequently, Solow recalled, "I took it on myself to explain economics and to explain the role of the staff—the research staff of the Fed, and their findings, and all of that—to the rest of the board... So that was the role I played." With regard to his impressions of Miller from these interactions, Solow observed, "I thought he was very sharp, and very smart. I don't think he understood economics very well... But in terms of IQ… [and] just pursuing a line of thought, an argument, I thought he was pretty impressive." (Robert Solow, interview, July 7, 2014.)[163]

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[161] From the hearing of April 10, 1978, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1978b, p. 129).
[162] See Federal Reserve Bank of Boston (1971, pp. 327–328).
[163] In January 1977, Miller favorably referred in public to Solow's macroeconomic writings (Miller, 1977, p. 341).

During the leadup to 1978, Miller made a fair number of contributions to public discussions of economic stabilization policy and of price-setting. These statements indicated that, however unhappy Solow was with the quality of Miller's economic analysis, that analysis had even more substantial differences with Friedman's perspective on economics.

An early example is provided by a statement Miller made in early 1969. Although he himself was the head of a conglomerate, he called for an inquiry into conglomerate mergers (*The Detroit News*, January 31, 1969). This move was perhaps an endorsement of the view—prevalent at the time—that increased concentration of industries was among the factors driving high inflation. The notion that Miller subscribed to cost-push views of inflation is reinforced by his comments he gave in a *Business Week* guest op-ed in 1974 (an article earlier discussed by Romer and Romer, 2004, pp. 155−156). In this piece, Miller did not specifically claim what he called "controlling the aggregates—the supply of money and net federal spending" could not reduce inflation. But he contended that there were alternative means of achieving disinflation that did not entail short-run output losses. In addition, although Miller's analysis criticized direct controls on wages and prices, it proposed a mixture of control and decontrol on other dimensions: for example, for firms, Miller proposed introducing more tax incentives and lower regulation, but he suggested introducing selective controls on consumers and removing, for two or three years, labor unions' option to go on strike, in favor of a compulsory arbitration system (*Business Week*, October 5, 1974).

Miller's economic views also got a measure of national exposure via a speech on the macroeconomic scene that he delivered in Pittsburgh in January 1977. The periodical*, Vital Speeches of the Day*, decided to publish the speech in its March 1977 issue. In the same issue, *Vital Speeches* printed a talk, "The Future of Capitalism; The Intellectual and the Businessman," given at Pepperdine University, Los Angeles, California, on February 9, 1977. The author of this speech was Milton Friedman.[164]

Another occasion over this period that saw Miller acquire a high profile in economic-policy discussions was when he testified in February 1977 to the U.S. House of Representatives' Committee on Ways and Means. Miller did so during the committee's hearings on the new

[164] See Friedman (1977l) and Miller (1977). Miller (1977) was previously analyzed by Romer and Romer (2004) and is touched on below.

administration's tax proposals.[165] In this testimony, he spoke in favor of the Carter Administration's proposed increase in the investment tax credit, but he added his own favored measure of region-specific rapid depreciation allowances for business investment. The contrast with Friedman's stand on this matter is notable. By this time, Friedman had warmed to the idea of an investment tax credit, in light of the apparent logjam facing other ways of reducing corporate taxes (Instructional Dynamics Economics Cassette Tape 197 August 1976, Part 1, and Tape 207, January 1977, Part 2). But the specific arguments Miller invoked for his tax-credit package were not ones on which Friedman looked favorably. Miller stated: "I prefer not to reduce corporate taxes generally, because then the reduction can simply go into the corporation's coffers and stay there."[166] In contrast, Friedman *did* want general corporate tax reductions, while Friedman's emphasis on monetary policy and aggregate demand made him unsympathetic to the view that whether a corporation saved or invested the proceeds from a tax cut was decisive for the path of aggregate demand.

Likewise, Friedman would have been unsympathetic with Miller's suggestion, in the February 1977 testimony, that the prominence of investment spending in the rise of U.S. aggregate demand during 1961−1965 was a major factor in allowing the economic expansion of those years to be noninflationary. The "established merits of capital spending as a means of creating jobs without unleashing inflation," Miller contended, made it appropriate to "suggest that there was a correlation among the economic growth, full employment, price stability[,] and expanded fixed investment."[167] In contrast, Friedman had stressed on numerous occasions (and would continue to do so) that forces that added to potential output, including capital spending, were typically minor factors in determining the course of inflation compared with monetary growth.

At his January 1978 confirmation hearings nearly a year later, Miller's portrayal of what monetary policy could do paralleled Burns'. Monetary restraint, Miller said, was a *necessary* part of controlling inflation, so "over time, working on inflation will mean lower [growth in]

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[165] See Committee on Ways and Means, U.S. House of Representatives (1977). When Miller's nomination was announced, the *Irish Times* (Dublin) (December 30, 1977) referred to a "scouring of recent economic pronouncements by Mr. Miller" as having unearthed "a speech in Pittsburgh last January" (Miller, 1977), but it did not mention this testimony. (Romer and Romer, 2004, also cited and analyzed Miller, 1977, but not the 1977 Congressional testimony.) The testimony may have become little noticed because Miller was identified only as William Miller, not G. William Miller, in the hearings volume.
[166] From Miller's testimony of February 7, 1977, in Committee on Ways and Means, U.S. House of Representatives (1977, p. 370).
[167] From Miller's written submission in Committee on Ways and Means, U.S. House of Representatives (1977, p. 332).

money aggregates." However, he maintained that such monetary restraint on its own could not be expected, even by 1985, to restore price stability—it was "a mistaken hope to believe the Federal Reserve on its own could accomplish this, because the economic environment in which we live is made up of many factors."[168] Like Burns, Miller did not regard monetary restriction as sufficient to remove inflation.

In sum, although—shortly before Miller was formally confirmed as chairman—President Carter described Miller as having an "excellent understanding of economics in every respect," there was plenty in Miller's economic analysis that was inconsistent with an emphasis on monetary policy as an influence on aggregate demand and—especially—on inflation.[169] His positions were not very different from those of his predecessor or of many, perhaps most, academic economists at the time.[170] But they were very far indeed from Friedman's.

Miller on inflation control: 1978

Miller's first Congressional testimony as Chairman was on March 9, 1978 (the day after his swearing-in). His opening testimony affirmed that the FOMC intended its strategy of lowering monetary growth over time—policy settings that would "prove consistent with… a gradual winding down of inflation over the longer run."[171] But he included among "policies beyond the province of the Federal Reserve" included "this nation's ability to find a way to reduce the upward wage-price pressures that continue to plague our economy."[172]

In statements given later in the year, Miller continued to characterize inflation's causes—and, to a large degree, its control—as being outside the sphere of monetary policy. For example, when speaking at an October 1978 conference on productivity, Miller (1978c, p. 21) stated: "Monetary policy can be used to restrain inflation, but without coordination with other economic policies, it

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[168] From Miller's testimony of January 24, 1978, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1978b, p. 43).
[169] The quotation is from Carter (1978a), which records remarks the president made on March 3, 1978.
[170] Romer and Romer (2004, p. 155) argued that the 5 to 5.5 percent natural unemployment rate they inferred from Miller's (1977) speech as "much lower" than that espoused by the Federal Reserve, and specifically Arthur Burns, in that period. However, a rate of about 5 percent for the full-employment unemployment rate was a common view in officialdom in 1977. As discussed in DiCecio and Nelson (2013), Romer and Romer's analysis of Burns' 1977 statements took him as believing that the economy was on the verge of overshooting potential output in 1977—when actually, as noted above, it seems that Burns' concerns in that year were about too-rapid growth creating inflation through bottleneck mechanisms. Like others, Burns in 1977 believed that notable slack existed.
[171] Miller (1978b, p. 188; p. 9 of typescript version).
[172] Miller (1978b, p. 189; p. 12 of typescript version).

offers substantial dangers of its own." He contended that recent years had seen "a cycle of intense upward pressure on costs and prices," as U.S. workers tried to maintain their real incomes, and he argued that dollar depreciation had further pushed up inflation.[173] Earlier in the year, he had said of President Carter's wage guidelines: "If businesses do not cooperate ... then we will have high rates of inflation."[174]

These statements, it should be stressed, did not amount to a dismissal of the importance of ending inflation. Miller actually stressed this as an imperative. For example, in April 1978, he testified: "I am in favor of making inflation our No. 1 priority in terms of domestic policy."[175] Rather, the above-quoted statements amounted to a different assessment on Miller's part from that of Friedman regarding the issues of how inflation arose and the appropriate means of removing it.

Friedman on Miller

On May 11, 1978, Friedman met Miller in Washington, D.C., when the Federal Reserve Board governors conferred with their panel of consultants. Friedman had been attending these consultants' meetings periodically since the panels were launched in 1965. This would, however, be the final such meeting Friedman attended. Friedman had become disillusioned with the meetings.

One reason Friedman was critical of these "meetings of so-called academic consultants" as a basis for a dialogue with the Federal Reserve concerned the role that the staff economists were assigned in them. Only Federal Reserve Board policymakers, and not the Board staff, interacted with the consultants during the formal proceedings of these events. The fact that, as Friedman

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[173] Miller (1978c, p. 23). The prominence of developments in the exchange value of the dollar in discussions of U.S. inflation in 1977–1978 is further considered in the next chapter.
[174] From Miller's testimony of June 29, 1978, in Joint Economic Committee (1978a, p. 122).
[175] From Miller's testimony of April 10, 1978, in Committee on Banking, Finance and Urban Affairs (1978b, p. 135). Similarly, a little earlier in the *Chicago Tribune* (April 2, 1978*a* (Section 2, p. 8), Miller had stated: "I hope that we have the courage to make inflation our highest priority for domestic economic policy right now."

Romer and Romer (2004, p. 155) referred favorably to previous retrospectives that had suggested that "it was apparent before Miller was appointed that he was primarily concerned about employment and growth and, hence, would run [an] inflationary policy." However, as discussed below, neither the very important Romer-Romer analyses of Miller's tenure as Federal Reserve Chair, nor other detailed accounts including that provided below, support the notion that Miller pursued deliberately inflationary policies or even that he loosened monetary policy. Furthermore, as both the quotation just given and those below indicate, Miller was not a subscriber to the position that inflationary policies were a successful means of achieving a boost to the economy, and he put stress on the need to remove inflation.

put it, “the many Federal Reserve personnel [attending] sat around the sides of the Board Room, where the meeting was invariably held, without participating,” made these occasions more stilted than the various meetings in which Friedman had participated at the Federal Reserve Banks of Chicago, St. Louis, and (most recently) San Francisco.[176] At these district-bank events, both policymakers (the bank presidents) and research staff had been Friedman’s interlocutors.

A deeper reservation Friedman had about the Federal Reserve Board panel meetings pertained to whether the events really amounted to meaningful consultations. This reservation had made him reluctant to continue accepting invitations to join the panels. Although he had attended one Board consultants’ meeting a year from 1971 to 1975, and he then appeared at the April 1976 meeting that rolled out the findings of the monetary-statistics committee (on which he had served), Friedman was absent from the subsequent panels that convened in October 1976, February 1977, and November 1977. “Milton stopped coming,” Stephen Axilrod recalled (April 24, 2013). “I asked him about it once. He said, ‘Well, I had no influence.’”

However, after making this observation to Axilrod, Friedman did accept the invitation to the May 1978 event, the first consultants’ meeting to be held under Miller. As of then, and for some time beyond, Friedman held out hope that Miller would be more receptive than Burns had been to the operational and strategic changes in monetary policy that Friedman was recommending. Nevertheless, by the time the May 1978 panel convened, Friedman’s negative perspective on the consultants’ meetings had firmed. Friedman later complained that “the choice of the particular consultants invited to attend seemed designed to guarantee offsetting and contradictory advice, leaving the Fed free to pursue its own devices.”[177] The May 1978 meeting would not have struck him as an exception to this pattern.[178] This meeting of the Board of Governors and the economic consultants had two sessions: one in which a monetarist (William Poole) analyzed monetary control, with Paul Samuelson as a discussant; and another in which a Keynesian (Arthur Okun)

[176] The quotations in this paragraph are from Friedman (1982a, p. 105). The information he conveyed here was not correct in two respects. First, some of the later consultants’ meetings, including the May 1978 meeting, were held in Dining Room E in the Federal Reserve Board’s Martin Building (which was opened in 1974), instead of the Board Room in the Board’s Eccles Building. (The Board Room would have been the more memorable location, as consultants would be seated at the same long table used for FOMC meetings.) Second, by the late 1970s, the panel convened at these meetings was formally referred to as the Economic Consultants rather than academic consultants, as an early innovation in Arthur Burns’ tenure as Board Chairman had been to bring in nonacademic private-sector economists as panel members.
[177] Friedman (1982a, p. 105).
[178] The information that follows is based on Federal Reserve Board records.

gave a presentation on current monetary policy, with Milton Friedman as his discussant.[179]

As he concluded his discussion of Okun's presentation, Friedman "said he was never coming again," recalled David Lindsey, the former Friedman student who, on that day, was among the many staff economists in the audience for the meeting.[180] "He welcomed Miller as the new chairman and said, 'You know, I've been coming to these for a long time and giving advice, and they never listen to me, and I'm not coming again.' And Paul Samuelson said, in response, 'I consider it an honor to be invited to these [meetings]; and I'm happy to give my opinion, whether you take it or not, and as long as you keep asking me, I'll keep coming back.' So, Samuelson, who I disagreed with mostly, was quite generous and cordial whereas Friedman [on this occasion] was a little bit of a jerk, I thought." Friedman did stay for the rest of the meeting, as well as the subsequent luncheon with the staff and the Board governors; so "he didn't get up and stomp out; but it was virtually like that." (David Lindsey, interview, May 2, 2013.)[181]

Despite the tone he had conveyed with his appearance at this meeting, Friedman in 1978 saw reasons for optimism about Miller's ascension. Ahead of the May meeting, he had said to Stephen Axilrod that policymakers "might pay attention now" to monetarists' suggestions now that Miller was Chairman (Stephen Axilrod, interview, April 24, 2013). And a week or so before the consultants' meeting, Friedman had given a negative retrospective on Arthur Burns' performance as Federal Reserve Chairman and added, "Let's hope it will be different with Miller."[182] Asked later in the month to provide an assessment of Miller, Friedman said, "His

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[179] Although known, thanks to Poole (1970), for having provided theoretical support for the interest rate as a monetary policy instrument, William Poole in the late 1970s and early 1980s was an advocate of a reserves-based operating procedure: he judged in Poole (1979, p. 479) that "Burns' failure to reform Federal Reserve operating procedures… was responsible for many of the policy mistakes of the era." Consequently, he would have advocated a position at the consultants' meeting one opposite to that of his discussant, Samuelson. As for Arthur Okun, he had criticized the increases in the federal funds rate in Burns' final year in office (*Washington Post*, November 18, 1977) so, on this and other grounds, the position he voiced at the discussants' meeting would have differed greatly from that of his discussant, Friedman.

[180] Those staff members did not include Janet Yellen, who was an economist at the Federal Reserve Board in 1977−1978 (see *https://www.brookings.edu/experts/janet-l-yellen/*). Her absence from the audience may have resulted from limitations on staff attendance of the meeting or, alternatively, it may be that, by May 1978, she had already left for the United Kingdom to start a new position at the London School of Economics.

[181] Thomas Simpson—like Lindsey, a Ph.D. graduate of the University of Chicago who worked at the Federal Reserve Board in this period—similarly recalled Friedman relaying the same message to him on the day of the event. "He said, 'You know, I've gotten invitations to these things. I used to come because I've got people would listen to me and it would make a difference. But,' he said, "I've come to realize that it's kind of a waste of my time to come here, and I'm not so sure I want to participate.' He said, "The only reason I came here today is that you got a new chairman, and I was curious, in terms of meeting him.'" (Thomas Simpson, interview, May 29, 2013.)

[182] *Evening Gazette* (Worcester, Massachusetts), May 3, 1978.

statements to date are excellent. However, the real test is performance, and it is too early to judge that."[183]

It is evident from this last remark that Friedman had been left with a favorable impression by some of Miller's statements since his nomination—perhaps those in which Miller had indicated that the Federal Reserve could not permanently reduce nominal interest rates or the unemployment rate through money creation.[184] Also likely to have struck Friedman favorably was the fact that Miller made remarks about the need for sustained monetary restraint in pursuit of the goal of price stability. In taking this position, Miller mirrored Burns, of whom Friedman had said (*St. Louis Globe-Democrat*, December 7, 1977): "Burns' statements are excellent."[185]

As indicated above, however, Miller's statements prior to joining the Federal Reserve indicated that he was also well-disposed toward cost-push views of inflation. Furthermore, Miller's statements as Federal Reserve Chairman would make clear that he continued to hold a nonmonetary view of inflation.[186] One statement by Miller, in July 1978, that "monetary policy cannot do the job alone," would particularly draw Friedman's ire: Friedman was still quoting it in the 1980s.[187] That this statement would have been anathema to Friedman was certain; but the statement amounted, in essence, to a repeat of remarks Miller had made at his January 1978 confirmation hearings and his March 1978 first Congressional testimony as Chairman (both quoted above). Friedman may have largely overlooked these early statements and may have worked on the presumption that Miller's views concerning inflation and monetary policy were

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[183] *Newsweek*, May 29, 1978. As these remarks were presented by *Newsweek* as part of a joint question-and-answer session with Paul Samuelson for the magazine, they may have been given when both the economist-columnists were in Washington, D.C., for the Federal Reserve Board consultants' meeting held, as noted above, on May 11, 1978.
[184] On monetary growth and unemployment, for example, Miller had testified on March 9, 1978 (the day after his swearing-in) that "[m]acroeconomic policies will not be able to produce the reduced level of unemployment that all of us seek as a national goal without unleashing a degree of inflation that would be self-defeating. In fact, we could unleash inflationary forces that would bring us right back to high unemployment. So I think you are absolutely correct in saying that if we rely upon macroeconomic policies, we are going to have enormous difficulties and perhaps not achieve our unemployment goal." He was responding to a question that referred to a stated administration goal of a rate of 4 to 4.4 percent unemployment by 1983. (Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1978b, pp. 118–119.) Miller's warnings against the dangers of holding nominal interest rates down included page 153 of the same testimony and his remarks of June 29, 1978, in Joint Economic Committee (1978a, pp. 126–127).
[185] For example, Burns had said (*Family Weekly*, July 3, 1977): "Too often in the past we have lacked the courage to stay long enough on a monetary and fiscal path that will lead to noninflationary economic growth. We cannot afford to backslide once again." Even when making this admonition, however, Burns did not suggest that removing inflation would require a period of recession or slow growth, as Burns envisioned the appropriate policies as "sustaining the expansion now in progress" and, indeed, "significantly reducing the high level of unemployment."
[186] On this matter, as well as the items noted here, see Romer and Romer (2002b, pp. 31–32) and Nelson (2005).
[187] See *Newsweek*, May 2, 1983, and Friedman (1984a, p. 55).

close to his own. But Miller's subsequent affirmation as Federal Reserve Chairman of a nonmonetary perspective toward inflation would have led Friedman to abandon that presumption. In September 1978, Friedman complained that in the face of rising inflation, "discussion about appropriate policy tends to concentrate either on income policies or fiscal policy."[188]

Another area of disappointment for Friedman was that of monetary policy operation. At the time when Miller was nominated, Friedman said that Miller's first act, once confirmed, should be to establish a committee to investigate Federal Reserve operating procedures, with a view to a major reform: "its techniques for controlling money creation are obsolete" (*New York Times*, December 30, 1977, p. D3). Several months into Miller's tenure, Friedman still hoped that Miller's background in the business world might incline him to be innovative with regard to monetary policy procedures—and in particular to initiate a move from reliance on a federal funds rate instrument to the use of a bank-reserves instrument (*Newsweek*, July 24, 1978). The Miller FOMC, however, retained the federal funds rate as its operating instrument.

A "long-range program"

In 1977 and 1978, Friedman saw another example having been recorded of a pattern he had observed on several previous occasions since 1962: the United States abandoning demand restraint. Against this backdrop, Friedman laid increased stress on the need for a multi-year program of disinflation. In an April 1977 television appearance, for example, he stated: "I think the most effective cure for inflation is a slow, steady attempt to bring it down over a period of four or five years."[189] Around the same time, he noted the advantages of "a steady course on the path of government which can be known in advance by people in the marketplace" (*Saturday Evening Post*, May/June 1977, p. 16).

A year later, Friedman gave a specific version of this proposal: a *Newsweek* column (of April 24, 1978) called for steps down in M2 growth each year, accompanied by restrictions on federal government spending that would limit the adjustment to the monetary restriction required of the private sector.[190]

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[188] Friedman (1978c, p. R–185).
[189] *MacNeil/Lehrer Report*, PBS, April 18, 1977 (*American Banker*, April 21, 1977, p. 6).
[190] Similarly, in a speech given in Scotland a few days after the *Newsweek* article appeared, Friedman (1978e, p. 7) observed: "What is needed is for the authorities to announce long in advance, and by that I mean years in advance, what is going to be the course of the monetary aggregates…"

On May 9, 1978, Chairman Miller replied to a request by Representative Dawson Mathis (D–GA) to react. Miller wrote: "In the last section of his article Dr. Friedman asserts that 'We need a long-term program dedicated to eliminating inflation.' I agree wholeheartedly."[191]

This was true, in general terms: in the previous month, for example, Miller had observed that a "high priority should be the implementation of a strong anti-inflation program."[192] The difference was great with Friedman on specifics, however. This was brought out by Miller's next observation, "Monetary policy has a critical role to play, but it cannot alone bear the whole burden of combating inflation."

In an interview a couple of months later (*U.S. News and World Report*, August 7, 1978), Miller reiterated and elaborated on his opposition to Friedman's proposed long-range plan. Miller argued that the "Federal Reserve is limited in what it can do about inflation" (p. 18) and cited factors that he thought were introducing more inflation, including a Social Security tax increase legislated for January 1, 1979, which "gets right into prices and is very inflationary" (p. 19). Miller said of Friedman's plan (p. 19): "I don't think it would work. It's a nice, neat way to set the dial and go home, but I don't think the world is made up that way. I don't think that's the way monetary policy works." Rather, "monetary policy has to take account of oil boycotts and wheat deals and famines and wage settlements."

Miller added in this interview, "What Dr. Friedman suggests is in a sense workable, but I think the volatility of interest rates and the impact on the real economy would be so disruptive as to be counterproductive." Friedman's *Newsweek* column proposal, however, had aimed to reduce the impact on the real economy of disinflation by reducing monetary growth in stages—by 1 percentage point per year—as well as by limiting the burden of adjustment on the private sector through restraint on federal spending. And as it happened, however, the monetary growth rate for 1978 for M2 that Friedman had recommended in his column—8 percent—was not very

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Such gradualist programs, of course, had been made by Friedman in some form for years. Stanley Fischer—who, as Section I discussed, participated in Friedman's money workshop for several years starting in 1969/1970—observed (Fischer, 1981, p. 35): "Advocates of gradualism have argued since 1970 that the best way out of inflation is for the growth rate of the money stock to be reduced over a period of four to five years to the noninflationary range of 3 to 4 percent." (Fischer added that there had since been "Fed acceptance of the argument." As indicated presently, however, the Federal Reserve's leadership, although it was agreeable to the notion of a gradual slowdown in monetary growth, did not endorse convergence to a completely noninflationary rate of the kind associated with M2 growth of about 4 percent.)

[191] *Congressional Record*, July 18, 1978, p. 21530.

[192] *Chicago Tribune*, April 2, 1978*b* (Section 2, p. 8).

different from the actual M2 growth generated for the year under Miller.

However, Friedman's proposal had featured a continuing downward path for monetary growth from 1978 to 1982, and the endpoint of this proposed path had drawn criticism from Miller in his May rebuttal. In that letter, Miller argued that "the 4 percent rate of growth in M2 that Dr. Friedman sets as a goal for 1982 would be the lowest rate of growth in that aggregate since 1960, except for the 'credit crunch' year of 1969."[193] The comparison with 1969 was not entirely appropriate, because 1969's 4 percent monetary growth rate had been attained suddenly, rather than as part of a multi-year, phased reduction. A 4 percent growth rate acquired in a gradualist manner could be expected to involve less of a crunch than a sudden shift to that rate.

The fact that Miller objected even to a gradual achievement of this 4 percent rate suggests a possible parting of company with Friedman on the appropriate definition of price stability. Friedman and the Federal Reserve probably did not differ too much in their assessments of longer-term M2 velocity behavior. It was widely accepted that M2 velocity had been roughly trendless since the late 1950s at least through the mid-1970s (see the discussion in Section II above), though there was some doubt about whether this was the case in the most recent years.[194] If Miller took potential output growth to be about 3.5 percent, and this assumption is combined with a constant or gently rising longer-run pattern for M2 velocity, Miller's objection to 4 percent M2 growth might have reflected a judgment that the appropriate longer-run inflation objective was about 2 percent rather than around zero. Such a judgment would be consistent with Romer and Romer's (2002) position that the Federal Reserve's (and U.S. government's) economic objectives have not changed appreciably over the decades.

Friedman's ultimate target of 4 percent monetary growth, in contrast, reflected his preference for roughly zero inflation. Evidently, Miller opposed a goal of a monetary growth rate consistent with zero inflation, even if that rate was approached gradually—perhaps because he believed that a 1 or 2 percent inflation rate was more conducive to orderly relative-price adjustment and so to macroeconomic stability.[195]

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[193] Miller letter of May 9, 1978, in *Congressional Record*, July 18, 1978, p. 21530.
[194] As noted above, however, it is likely that both Friedman and the Federal Reserve viewed the M2 concept that was likely to have a stable velocity in the future as being the prospective redefined, and broader, M2 series, rather than the existing reported M2 series.
[195] If this was the basis for the rejection of a zero inflation target, it was in keeping with arguments in Tobin (1972) and that Friedman had considered in Friedman (1958, p. 252; p. 182 of 1969 reprint) (and expressed sympathy with at some earlier point: see Rees, 1970, p. 237), although he felt that other considerations outweighed this argument.

Table 2. U.S. inflation in 1978				
	Annualized quarterly inflation rates (percent)			
	1978:Q1	1978:Q2	1978:Q3	1978:Q4
CPI	7.1	9.4	9.6	9.6
GDP deflator	5.9	7.9	7.0	8.5
Source: Computed from the Federal Reserve Bank of St. Louis' FRED portal. Quarterly averages of monthly data are used for the CPI.				

Friedman was invited by Congressman Mathis to write a rejoinder to Miller's letter. In this riposte, dated June 8, Friedman remarked that he "had hoped that a new chairman who as a businessman has had to face facts and correct error" might involve a more open-minded response

to criticism, but that he did not see evidence of this in Miller's letter. "Unfortunately, Chairman Miller's comments on my proposal for an announced five-year policy of monetary deceleration are strictly in the Federal Reserve tradition of blandly dismissing all criticism by undocumented assertion." Friedman argued that Miller's rejection of rules-based policy came despite the "highly defective" historical record produced under a U.S. monetary policy not based on rules. "I need not repeat the litany of failure documented fully in Anna Schwartz's and my *Monetary History of the United States, 1867–1960,* nor remind you of the Federal Reserve's contribution to both inflation and recession in the period since that covered in our book—including the credit crunch of 1969 that Chairman Miller refers to[,] as well as that of 1966."[196]

The blame-game and Miller

The choice, discussed above, between zero and 2 percent inflation had become a distinctly hypothetical one in the late 1970s. Inflationary pressures were again becoming severe, and the inflation rate shot up in 1978 (Table 2).

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[196] From Friedman's letter of June 8, 1978, in *Congressional Record*, July 18, 1978, p. 21530.

Policymakers were caught off-guard by this development. Secretary of the Treasury Blumenthal had said in the summer of 1977 (*American Banker*, August 2, 1977): "We have a little too much inflation… Inflation is now coming down gradually." And in remarks in March 1978, President Carter gave the inflation rate for the second half of 1977 as 4 to 4.5 percent, assessed the current underlying inflation rate as 6 to 6.5 percent, and reaffirmed his goal as getting inflation materially below 5 percent (Carter, 1978). Speaking in August (by which time inflation had risen substantially), Carter said (*Business Week*, August 21, 1978, p. 100) that "it has been running up at a rapid rate caused by unanticipated food price increases" but suggested that "the inflation rate curve [will] top out at the end of this year and then perhaps start to go down." In the same vein, Chairman Miller stated at roughly the same time (*Iron Age*, August 21, 1978, p. 20) that "both the consumer price index and the GNP implicit deflator should show some improvement in the second half of the year." Instead, both deteriorated (see Table 2 for the modern data), and both series would not peak until 1980.

The poor inflation record in 1978 and 1979, and Miller's departure in 1979 from the Federal Reserve for the U.S. Treasury—at a time of adverse sentiment about the economy in financial markets and the U.S. community—would permanently damage his reputation. Miller was also vulnerable to a loss of standing because he came to the Chairman position lacking deep Congressional support. In contrast, Burns was held in high regard by Republican members of Congress—*Newsweek* (January 9, 1978*b*) had observed that Carter had "sacked a conservative icon" by not reappointing Burns—and by many Democrats. Burns' champions saw him as vigilant against inflation, with a letter titled "Why Burns Should Stay" by Senators John Sparkman (D−AL) and Jacob Javits (R−NY) pointing to low longer-term interest rates as a demonstration of Burns' success in containing inflation expectations (*Wall Street Journal*, December 22, 1977). In contrast, as we have seen, Friedman thought Burns had stirred up a surge in inflation, and Friedman's 1978 criticism of G. William Miller amounted essentially to the position that the new chairman was not making a daring break from the Burns regime.

By the mid-1980s, with Paul Volcker having made a success of the job of Federal Reserve Chairman, the negative verdict on Miller was so entrenched that even a labor-economics textbook included a problem-question that gave a 1979 quotation from Burns and asked the reader to try to make a "coherent statement" of the logic behind "Miller's assertion" (Ehrenberg and Smith, 1985, p. 331).

Although Miller was destined to be blamed for making inflation worse and not responding effectively to it, neither the stance of monetary policy during his tenure, nor the direction in which he took FOMC decisions, really justifies this verdict.

First, with regard to the overall stance of monetary policy under Miller: Although writers on monetary policy often follow Friedman in taking inflation in year t as largely determined by events in years $t-1$ and earlier, it seems that this practice becomes rarer when it comes to discussing the behavior of inflation under Chairman Miller. Very often, the fact that inflation was high and rising under Miller is taken as evidence that *Miller*'s monetary policy was inflationary (examples include DeLong, 1997, pp. 271–272, and Levin and Taylor, 2013, p. 238).

An alternative interpretation is, however, available and is consistent with Friedman's commentaries. Under this interpretation, the second peak of inflation in the 1970s reflected the aftereffects of the easy monetary policy of 1976 and 1977. Indeed, as documented in Section II, the surge in inflation that occurred from 1977 onward was predicted successfully by Friedman on the basis of data up to 1977. On this interpretation, the inflation behavior from 1978 to 1980 was largely set in place by monetary developments before 1978, and so it cannot be viewed as indicative of an easy monetary policy stance in those years.

Second, on the direction in which monetary policy went under Miller: The fact that monetary policy tightened under Chairman Volcker is sometimes taken as having the corollary that it did *not* tighten under Miller (see, for example, Goldfeld, 1990, p. 200, and Rotemberg, 2013). In fact, as will now be discussed, monetary policy *did* tighten notably under Miller.

The Miller tightening

That monetary policy tightened appreciably under Miller was stressed by Friedman at the time, but not often by later analysts (exceptions include Romer and Romer, 1989, 1990, and Nelson, 2005). However, M2 growth visibly fell in 1978—see Table 1 above—and, in later years, Miller would emphasize his success in hitting M2 targets and the fact that most of his tenure witnessed a firming of policy (see Biven, 2002, p. 143). Furthermore, the federal funds rate rose over his period in office, and the real short-term interest rate was positive for a considerable part of his tenure (see Figure 3). In addition, narrative evidence of a Miller tightening underlay the

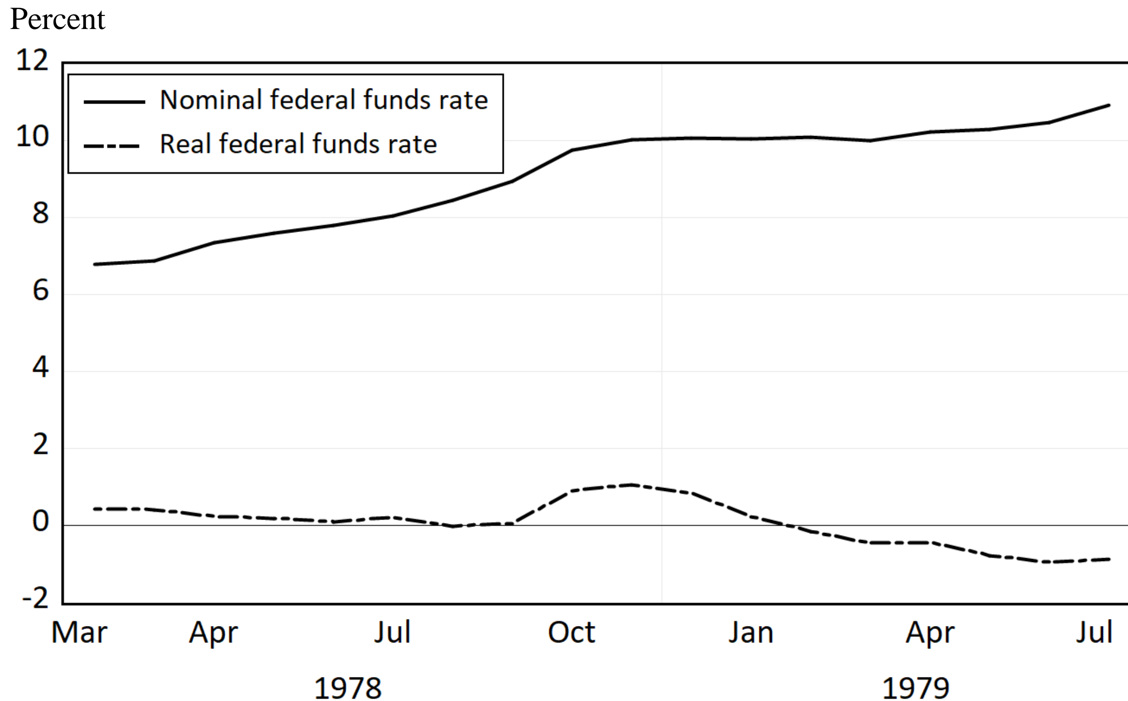

Figure 3. Nominal and real federal funds rate, March 1978 to August 1979.
Source: Federal Reserve Bank of St. Louis' FRED portal. The 12-month CPI inflation rate is subtracted from the nominal federal funds rate to obtain the real rate.

inclusion in the "Romer dates" of an FOMC tightening episode in August 1978 (see Romer and Romer, 1989, p. 136; 1990, p. 161).

The weight of the evidence therefore suggests that monetary policy moved decidedly toward tightening under Miller. This may seem discordant with the fact that Miller's views on monetary policy and inflation were quite similar to Burns'. As Miller, like Burns, doubted that inflation would respond to a negative output gap and he emphasized nonmonetary influences on inflation, he would seem *a priori* unlikely to undertake a monetary policy tightening. But he did so. Why?

One explanation—that Miller tightened under pressure from the administration—can be ruled out. DeLong (1997) highlighted Miller's tenure as one in which the administration urged the Federal Reserve to adopt a tighter monetary policy. But such a situation describes only a fleeting period during 1979 when the administration and Miller disagreed about the timing of monetary policy firming (see Chapter 9).

During the Federal Reserve's tightening in 1978, in contrast, the administration spoke out against tightening. For example, President Carter stated in an August interview (*Business Week*, August 21, 1978, p. 102): "The Federal Reserve Board doesn't consult with me before it takes any action. I deplore the rapid increases in interest rates that have occurred this year... My hope is that for the rest of this year we will not have any additional increase in interest rates. So whatever the Board can do to prevent increases in short-term rates would certainly be constructive." The Federal Reserve raised the federal funds rate further shortly after Carter made this statement.[197]

As for Friedman, his reckoning of monetary policy stressed the political environment as a factor shaping overall monetary policy stance—but not specific administration pressures to loosen monetary policy. He held the Federal Reserve, not Carter, responsible for monetary policy, though he faulted Carter for espousing a nonmonetary account of inflation's sources.

An unforced error on Miller's part that contributed both to the lasting perception that he was both an ineffective Chairman and inclined to loosen policy occurred when, at a meeting on June 30, 1978, of the Federal Reserve Board, he not only voted against a discount-rate increase but was also defeated by other Board governors in the vote (*Wall Street Journal*, July 21, 1978). Paul Volcker—being Federal Reserve Bank of New York president rather than a Board governor at the time—was not a participant in the Board vote but was disconcerted by it: "he got outvoted at one point; and I don't think he realized the symbolic, or whatever, significance of that. You know, people thought he wasn't very important because [they judged that] you got outvoted and therefore didn't have much influence. I think he made a mistake in the way he handled that, but I think the criticism—it wasn't so much criticism, but the [overall] feeling—that he was a weak chairman was a little bit unfair." (Paul Volcker, interview, October 16, 2013.)

This episode contributed to what the *Financial Times* (August 3, 1978) reported as "questions… raised about Mr. Miller's judgment," due to his "appearing to take a softer line on… the outlook for interest rates than much of Wall Street believes desirable." It deserves underscoring, however, that the FOMC under Miller *did* raise the federal funds rate, both before and after this discount-rate vote.[198] Notwithstanding that reality, retellings or inaccurate recollections of the

[197] This produced the *Washington Post* headline "Fed Moves To Raise Funds Rate" (August 29, 1978).
[198] Indeed, in midyear a commentary had observed that Miller "has overseen a sharp tightening of short-term interest rates" (*Kansas City Star* (Missouri), June 21, 1978), and *Time* magazine's cover, titled "The Inflation Fighter: Federal Reserve Chairman G. William Miller," for its July 17 edition, portrayed Miller as a gunfighter entering a saloon.

discount-rate-vote episode likely contributed to the post-1979 misconceptions that Miller either loosened monetary policy during his tenure or that he suffered defeats in FOMC votes as chairman.[199]

The main reasons for the Federal Reserve tightening in 1978, notwithstanding its nonmonetary view of inflation and administration pressure not to tighten, are likely twofold. First, although voicing skepticism that economic slack (excess *supply*) could *reduce* inflationary pressure, Miller, like Burns before him, did accept that excess *demand* could *add* to inflationary pressure, and so he was anxious to tighten by an amount that prevented output from overshooting its potential value: "we are up to a capacity point where I am worried," he remarked in August (*Iron Age*, August 21, 1978, p. 20). Earlier in the summer, he explained that, as he saw it, the emergence of excess demand would superimpose new inflationary forces upon those already present from the cost side, so that "we are at a point where we must be very careful not to trigger demand-pull inflation along with our present cost-push inflation."[200] With the perception that the gap was closing, the warranted rate of both real and nominal income growth declined, so Miller concurred—notwithstanding his opposition to an explicit multi-year proposal of the kind Friedman had laid down—that "[w]e need to set the general target of squeezing down the monetary aggregates" (*Iron Age*, August 21, 1978, p. 20).

A second key factor that made the Federal Reserve well disposed toward policy tightening in 1978 was the depreciation of the U.S. dollar exchange rate, discussed in the next chapter. Though the FOMC's tightening on this occasion was partly based on a rationale that Friedman deplored (that is, import-price-push views of inflation), it helped produce policy settings he preferred (that is, a further move away from the second monetary explosion of 1976−1977).

The tightening of monetary policy during 1978 was not a drastic squeeze: as *Citibank Monthly Economic Letter* observed (November 1978, p. 3): "We haven't yet had the sort of credit squeeze that pushes interest rates far above their usual relationship to inflation." But it was a material firming, and Friedman recognized, and stressed, the fact that monetary policy had tightened during 1978 under Miller. Indeed, as will be discussed in Chapter 9, by early 1979 Friedman was worried that Miller may have already tightened too much.

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[199] Miller explained his vote as reflecting a view that the discount-rate increase was excessive in relation to the increases in market interest rates that the FOMC had so far generated: see Miller's testimony of July 28, 1978, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1978a, p. 37).
[200] From Miller's testimony of July 28, 1978, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1978a, p. 60).

FRANCO MODIGLIANI

The beginning of this chapter discussed how Friedman began his time living in California with a three-month spell as a scholar-in-residence at the Federal Reserve Bank of San Francisco. One of the key events that the San Francisco bank convened during this stay was a debate on the evening of January 26, 1977, between Friedman and Franco Modigliani.[201]

The bank's research director at the time, Michael Keran, recalled of the event: "We organized that. I was the moderator, if you will; those two people needed one… We had to hire a large conference room outside the Fed [building] to handle all the people who wanted to come to hear it." (Michael Keran, interview, March 7, 2013).

Modigliani had recently completed a year as president of the American Economic Association and, the previous September, had delivered his presidential address at the association's annual meeting. That address (Modigliani, 1977), which has been discussed at various points earlier in this book, had been notable for a containing an acknowledgment—at the time, almost alone among major Keynesians born before 1930—of the likely validity of the Friedman-Phelps critique of the Phillips curve. In this connection, Modigliani (1977) was the first in the list of references that McCallum (1982, pp. 7−8) gave when documenting the point that "a number of influential researchers in the Keynesian tradition have in recent years expressed agreement with the NRH [natural rate hypothesis]."[202]

Indeed, of the four major university-based Keynesians against whom Friedman was often pitted—Modigliani, Samuelson, Solow, and Tobin—Modigliani was the one who gave the earliest signs of acceptance of the natural rate hypothesis. His address was given in 1976, while Tobin and Samuelson did not provide their own endorsements of the hypothesis until 1978−1980, and Solow remained a skeptic.[203] Because of this and other material in the

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[201] See Friedman (1977d) and Friedman and Modigliani (1977). (For the specific date of the event, see *Oakland Tribune* (California), January 27, 1977.) Dimand (2018, p. 10) incorrectly implies that the Friedman/Modigliani January 1977 event was not examined in the research literature on Friedman until 2016. In fact, however, Nelson (2007)—which appears to be, by a considerable margin, the most-cited research article specifically about Friedman to appear since his death—not only discussed this event but also updated the empirical evidence that Friedman offered in his exchange with Modigliani. Subsequent pre-2016 discussions of the Friedman-Modigliani event include Nelson and Schwartz (2008) and Nelson (2008, 2009); earlier, it was cited in Mayer's (1998) analysis of aspects of the Keynesian/monetarist debate.

[202] In addition, although Modigliani (1977) was mostly critical of the rational expectations literature, McCallum (1979a, p. 66) perceived in the address an acknowledgment of the Lucas (1976b) critique.

[203] See Nelson (2020b, Chapter 13) and the discussion earlier in the present chapter.

Modigliani presidential address, Friedman was able to cite it, in a paper he wrote for the September 1978 Mont Pelerin Society meeting, as a demonstration of the ground that Keynesians were giving to his position.[204]

However, Modigliani's acceptance of the natural rate hypothesis was highly qualified. In contrast to subscribers to the Friedman-Phelps Phillips curve, Modigliani seemed very enamored of the position, prevalent both in policy circles and among Keynesian economists, that the response of the output gap to economic slack had become negligible. In 1975, for example, he stated: "There is little evidence that increasing unemployment to 9 percent from 6 percent gives anything in the way of reducing inflation." He also stated that measures that raised the unemployment rate in the short run constituted an "unacceptable" way to fight inflation (*Kansas City Star* (Missouri), February 25, 1975).

That is, like Arthur Okun—another Keynesian economist who had sometimes been portrayed as a convert to accelerationist or natural-rate ideas—Modigliani seemed to believe that *if* a Phillips curve still described inflation dynamics, that Phillips curve was of the Friedman-Phelps kind; but that, most likely, a Phillips-curve relationship, in the sense of a responsiveness of inflation to the output gap, did not actually hold until full employment was reached.[205] Similarly, like Okun, Modigliani attributed inflation in the 1970s largely to cost-push causes. For example, Modigliani (1977, p. 7) contended that inflation after 1973 "was driven primarily by an exogenous price shock rather than by excess demand."[206] And he voiced support for a variant of Okun's idea of a tax-based incomes policy to fight inflation.[207]

Most of the cost-push aspects of Modigliani's ideas were, however, absent, from Modigliani's presidential address. The address instead focused on revisiting the Keynesian/monetarist debate on policy rules in light of the experience of the United States in the first half of the 1970s. The January 1977 event in San Francisco consisted of a re-presentation by Modigliani of this presidential address, followed by Friedman as a discussant, with a back-and-forth exchange between them completing the session.

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[204] Friedman (1978c, p. R–183).
[205] On Okun's views, see Chapter 4 above.
[206] In this vein, Modigliani held (in Friedman and Modigliani, 1977, p. 21) that "anybody who looks at the evidence must conclude that what happened in 1974 is primarily an explosion of prices, due to the impact of food and oil."
[207] See Okun and Perry (1978, p. 282).

The January 1977 debate

"I was prepared to meet him," Modigliani recalled in August 1982 of his debate with Friedman, adding that he went into the debate aware that Friedman was "tough and very fast and dangerous" (quoted in Klamer, 1984, p. 120). Furthermore, by the mid-1970s Modigliani regretted the time and effort he had put into the "AM/FM" debates in the 1960s (Charles Steindel, personal communication, September 9, 2015).[208]

These debates had evidently not led to the clear-cut and widely-acknowledged victory over Friedman that Modigliani had hoped for.[209] Nor, for that matter, had Modigliani's (1964b) critique of Friedman's view on rules; despite being published in the *Journal of Political Economy*, this article had neither generated a written exchange with Friedman nor prevented increased interest in the constant-monetary-growth rule. Instead, what prevailed by 1977—as Modigliani put it on July 30 that year—was that economists were living in "these days of rampant monetarism."[210]

Modigliani's (1977) address contained an extensive critique of the constant-monetary-growth rule, not only on grounds of principle but also via the deployment of what Modigliani argued was empirical evidence that the rule did not perform well in the United States. Like many Keynesians of the time, Modigliani perceived a greater conversion by U.S. policymakers (before 1979) to Friedman's prescriptions than had actually occurred. But Modigliani (1977, p. 11) went further in this direction by implying that the constant-monetary-growth rule had been instituted informally under Arthur Burns. Modigliani held that U.S. monetary growth had been stable from 1971 to 1974. He contended that this had resulted in outcomes inconsistent with monetarists' predictions: the stability of monetary growth in the 1970s had been associated with sizable

[208] Charles Steindel was a graduate student at MIT in 1973−1977 (Charles Steindel, interview, December 3, 2015), and Modigliani was his coauthor (see Modigliani and Steindel, 1977) and thesis advisor. Steindel discussed the AM/FM (Ando-Modigliani versus Friedman-Meiselman) debate with Modigliani after coming across the debate during his graduate-student years. On that debate, see Nelson (2020b, Chapter 12).

[209] Modigliani had begun work on the critique that evolved into Modigliani and Ando (1965) in the 1960/1961 in response to an early version of the Friedman Meiselman study. Niels Thygesen, who collaborated with Ando and Modigliani during this early stage of their development of a rebuttal, recalled: "Ando and Modigliani were clearly upset by the work of Friedman and Meiselman. They thought it was much oversimplified—which it was, of course, in a sense—[and] very provocative. And yet, they were very respectful, clearly, of the way Friedman proceeded. And I remember statements—Modigliani saying, 'If we write something about this paper, we have to keep it absolutely watertight, because otherwise, Milton will tear us apart afterwards.' So there was a mixture of great respect for him, but also irritation that he was simplifying things too much." (Niels Thygesen, interview, February 10, 2015.)

[210] From the remarks published as Modigliani (1979, p. 330).

swings in aggregate output. The same was true of another period of stable monetary growth that Modigliani cited: that from 1953 to 1957 (see Modigliani, 1977, pp. 11–12).

In the comment on Modigliani (1977) that he delivered during the January 1977 debate, Friedman stressed that monetary growth had not been stable in the 1971–1974 period when judged by his preferred aggregate, M2.[211] Modigliani's demonstration had used M1.[212] On this basis, he challenged Friedman at the Federal Reserve Bank of San Francisco event, with reference to 1971–1974: "I defy you to find any other period in which, for a period of that length, you get that low level of variation."[213]

After consulting the data, Friedman did just that. Whether judged by M1 or M2, monetary growth had been more stable in the early- to mid-1960s—when the economy displayed considerable stability—than in 1971–1974. Modigliani conceded this point.

A second concession from Modigliani during the seminar was his acknowledgment that Friedman had not—as Modigliani had claimed in his presidential address—rested his natural-rate-hypothesis exposition in 1968 on the assumption of competitive and atomistic labor and goods markets.[214]

Notwithstanding his concessions of these two points in the course of his exchange with Friedman, Modigliani maintained the two original claims in the published version of his presidential address, printed in the *American Economic Review* in March.

Perhaps Friedman should have rebutted the point regarding monetary variability earlier. For

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[211] See Friedman (1977d, pp. 15–16). For 1953–1957, Friedman applied the same argument, combined with the point that the start of the FOMC's bills-only policy in 1953 had meant that the effective money supply was more variable over that period than was the measured money stock.
[212] Friedman had himself noted that the level of M1, and to a lesser extent that of M2, had from 1971 through early 1974 displayed a tendency to return to a particular trend line (Instructional Dynamics Economics Cassette Tape 144, April 17, 1974; Instructional Dynamics Economics Cassette Tape 147, May 30, 1974). However, in considering this period, he also stressed (both at the time and subsequently) that there were considerable—and, as he saw it, unnecessary—fluctuations in monetary growth within each year.
[213] From Modigliani's remark in Friedman and Modigliani (1977, p. 20).
[214] For Modigliani's acknowledgment that Friedman did not base his 1967 presidential address on perfect competition, see Friedman and Modigliani (1977, p. 19). (Modigliani went on to argue that because agents respond to price signals in Friedman's model, it was *de facto* a perfect competition model; but see Chapter 8 of Nelson, 2020a, as well as Chapter 2 above, for critical analysis and refutation of this argument.) For Modigliani's acknowledgment that the early to middle 1960s provided a counterexample to his position that the 1970s (and 1950s) provided the most-stable periods of monetary growth, see the footnote written by Modigliani that appeared in Friedman (1977d, p. 16), as well as Modigliani (1986, p. 37).

prominent Keynesians had been pointing to the years to 1974 as one of stable monetary growth in critiques appearing publicly as early as mid-1975—as part of what were described as "the Okun-Modligiani charges" against monetarism (*New York Times*, July 14, 1975). On that occasion, Friedman had been quoted saying of Modigliani and Okun's policy prescriptions, "They've talked that way year after year and they are wrong." He added, "The worst thing in the world that the Fed could do is to follow their advice and run a policy geared to holding down interest rates…. Those guys [Okun and Modigliani] are fine-tuners. If the Fed adopted my kind of policies, they would be out of business." But it was instead Friedman's former student Beryl Sprinkel who was quoted replying to the specific allegation that the first half of the 1970s had seen stable monetary growth—an allegation to which Sprinkel responded: "There is no way to interpret the 1969 to mid-1974 period as one of stable monetary growth." (*New York Times*, July 14, 1975, p. 39.)

In the event, as we have seen, Modigliani pressed ahead with this interpretation, though in January 1977 he would concede that the 1960s did provide an example of a more stable monetary-growth pattern than that seen in the years to 1974.

Just as the January 1977 debate saw Modigliani voice more concessions to Friedman than those he actually made in print, at that event he also qualified his criticisms of the constant-monetary-growth rule. "One of Modigliani's points," Keran recalled, "was that Friedman's [constant-monetary-growth rule] approach would have been great if he were in Italy; but that, here in the United States, it's too crude a thing to work; that, if you have a lot of inflation and a lot of disorganization in the markets, the monetarist approach might work well, but not in the subtle environment of the U.S. economy... I remember that comment quite strikingly." (Michael Keran, interview, March 7, 2013.) "Unlike Milton, I happen to think there are some things the [U.S.] government can do very well," Modigliani observed eighteen months after the January 1977 debate (*Wall Street Journal*, July 17, 1978, p. 27).

With regard to that debate, Keran judged that Friedman was the winner on the day. "In terms of Modigliani, I can still see the flash in his eye when he wanted to counter a point that Milton made. So he had a lot of passion. Milton was very analytical and calm, cool, and collected. And so, I think—I might be biased on this, but that was my impression—that Milton came across stronger." (Michael Keran, interview, March 7, 2013.) However, Modigliani was satisfied with his own performance: "I think that I did fine" (Klamer, 1984, p. 120).

Debating resource slack and stabilization policy

In the afternoon ahead of their evening debate, Friedman and Modigliani met the local media to discuss the national economic situation (*Oakland Tribune* (California), January 27, 1977). This appearance revealed disagreement about the current policy prescription. Friedman had already recommended to the authorities that they "[l]et the recovery proceed… at a moderate pace" (*Newsweek*, December 6, 1976) and exercise fiscal and monetary restraint, rather than increase stimulus. Indeed, as discussed in Sections I and II above, in the late-1976/early-1977 period he had already concluded that policy settings were already too loose, and that the stance of monetary policy was promoting a resurgence of inflation.

Modigliani, in contrast, recommended stimulus. His recommendation, it is true, was tempered by an upgraded estimate of the full-employment unemployment rate. He had already granted in his presidential address that more-generous unemployment benefits had raised the baseline unemployment rate.[215] Five years earlier, he noted at the media appearance, his nominated full-employment goal would have corresponded to a rate of 4.5 to 5 percent. "[But] now, I'm around 6 percent. I am responding to the changed composition of the figures." (*Oakland Tribune* (California), January 27, 1977.)

To be more precise: Modigliani's estimate of the full-employment unemployment rate was probably about 5.8 percent. This value can be inferred from the fact that, in the same January 1977 media event, he gave the amount by which current unemployment exceeded that rate as 2 percentage points. Modigliani's assessment of the value of the full-employment unemployment-rate was therefore higher than that being used by the Carter Administration, which accepted the value of 4.9 percent that it had recently inherited from the Ford Administration (see Biven, 2002, p. 202). Modigliani's estimate was also close to the value of around 6 percent that would be judged in retrospect to be the appropriate estimate of the natural rate of unemployment in the late 1970s. But, notwithstanding his fairly realistic estimate, Modigliani recommended an amount of fiscal and monetary expansion that was inappropriate. His recommendation was unintentionally overstimulative because, as already indicated, it rested on then-prevailing estimates of potential economic growth. These estimates suggested, erroneously, that output needed to grow by more than 3.5 percent a year just to make headway into the amount of resource slack.

[215] See Modigliani (1977) and the discussion in Chapter 4 above.

Friedman, in contrast, cast doubt on the 2 percent unemployment-gap estimate that Modigliani cited. With regard to unemployment-rate targets, he had already remarked in a November 1976 television appearance that "we are only led down a false trail by trying to believe that, somehow or other, a powerful government can pick a number out of the air—like 3 percent or 4 percent or 5 percent—and produce policies which will lead to this result."[216] Although Friedman regarded the 6 percent rate cited by Modigliani as more realistic, he did not want to center the formulation of economic policy on *any* numerical estimate of the full-employment unemployment rate. He had said in November, "we ought not to regard the [unemployment] numbers which are generated in that way [by the statistical agencies] as calling for massive policies which can do more harm than good."[217] With regard to the amount of resource slack, Friedman stated in the January 1977 media appearance: "I question whether the margin is big enough to make it possible for the government to pour out money without threatening another surge of inflation."[218]

Indeed, as already indicated, by early 1977 Friedman believed at this point that an inflation surge was already in motion. Modigliani told reporters at his joint appearance with Friedman that it was feasible for President Carter to preside over declines in both the inflation rate and the unemployment rate during his presidency. Friedman, in contrast, gave the reporters his estimate of a rate of 7 to 9 percent inflation for the next couple of years (*Oakland Tribune* (California), January 27, 1977). As has been stressed in this chapter, this was a prediction that was vindicated.

Revisiting the 1977 debate

About a decade after their 1977 exchange, Friedman and Modigliani both had occasion to revisit it in print. Modigliani's book *The Debate Over Stabilization Policy*, based on lectures given in 1977, was belatedly published in 1986. The book alluded to the Federal Reserve Bank of San Francisco debate with Friedman, which had taken place not long before the lectures.[219] However, little of Friedman's work since 1970 was mentioned and, like Tobin, Modigliani was now

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[216] *Wall Street Week*, Maryland Public Television, November 5, 1976, p. 4 of transcript (*American Banker*, November 22, 1976, p. 4).
[217] *Wall Street Week*, Maryland Public Television, November 5, 1976, p. 4 of transcript (*American Banker*, November 22, 1976, p. 4). See also Nelson (2020a, Chapter 8).
[218] Around the same time, he expressed objections to "the usual kinds of statements about how there [is] so much slack in the economy… that there was no danger of restimulating inflation" (Instructional Dynamics Economics Cassette Tape 207, January 1977, Part 2).
[219] See Modigliani (1986, p. 37).

concentrating his fire on the rational-expectations critiques of activist stabilization policies, rather than on Friedman's writings.

Modigliani (1986) did, however, indicate (as had Modigliani, 1977) that the monetarist movement had made valid criticisms of the early vintages of Keynesianism. Modigliani had himself long set himself apart from these variants of Keynesianism. Indeed, Friedman had noted in the San Francisco debate in 1977 that "I've always thought that Franco, insofar as you use these terms, has always been a monetarist, in very important ways."[220] Friedman had particularly pointed to Modigliani's (1944) paper, which—at a time when Friedman himself was only beginning to shake off extreme Keynesian views—had relegated instances in which monetary policy was ineffective in altering nominal income to a special case, allowing an important role for the money stock in other circumstances. Of course, once he became a monetarist, Friedman would go further than this and would reject the empirical relevance of the liquidity-trap case.

In his *New Palgrave* dictionary entry on the quantity theory of money, which appeared a decade after Modigliani (1977) was published, Friedman paid Modigliani the compliment of quoting that presidential address—specifically, its observation that there were "no serious analytical disagreements between leading monetarists and leading nonmonetarists."[221] But Friedman immediately added that "there remain important differences on an empirical level." [222] Even this qualification understated the divide between Friedman and Modigliani, however. For one thing, different empirical judgments can imply different analytical frameworks: Modigliani's insistence in 1977 on the prominence of cost-push shocks as drivers of inflation, for example, amounted to a rejection of Friedman's monetary view of inflation. For another, by the 1980s Modigliani was apparently not well disposed toward seeing his differences with Friedman as capable of being resolved empirically, as he claimed that Friedman "has a mission and seems to be willing to sacrifice some intellectual honesty for that" (Klamer, 1984, p. 120). Modigliani did not provide any specifics underlying this claim. His remark may have reflected the disdain that leading Keynesians often had for Friedman's popular writings, which they felt were oversimplified.[223]

Though its final version saw print in 1987, Friedman's *New Palgrave* entry was initially drafted in late 1985. That period also saw the fulfillment of a prediction that he had made concerning

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[220] Friedman (1977d, p. 12).
[221] Modigliani (1977, p. 1), also quoted in Friedman (1987a, p. 13).
[222] Friedman (1987a, p. 13).
[223] See Chapter 2 above.

Franco Modigliani. In 1981, Friedman had predicted that Modigliani would win a Nobel award in economics for his work on consumption behavior.[224] This came to pass in October 1985.

[224] See Wallechinsky, Wallace, and Wallace (1981, p. 418).

Milton Friedman and Economic Debate in the United States, 1973−2006
Chapter 9: Debates on International Economic Policy, Regulation, and Aggregate Supply, 1977 to 1978

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July 30, 2023

I. EVENTS AND ACTIVITIES RELATED TO DEBATES ON INTERNATIONAL ECONOMIC POLICY, REGULATION, AND AGGREGATE SUPPLY, 1977−1978

"Still a good year," was the title of Paul Samuelson's *Newsweek* column for January 9, 1978, in which, in the wake of recent news—including the announced departure of Arthur Burns as head of the Federal Reserve—he reevaluated the United States' economic prospects for the twelve months ahead. Among the features of his projection that confirmed that "it should be a good year for the American economy," Samuelson cited the likelihood there would no recession in 1978 and the prospect that the coming twelve months would, instead, see real output growth at or above its potential rate.[2] Yet the year 1978 would not be remembered as a period of economic success for the United States. Dornbusch and Fischer (1984, p. 556) would classify it as part of the "poor economic performance of the 1969−1983 period," and in mid-2014 Richard Fisher, who had been a senior official in the U.S. Treasury during the Carter years, would look back at the U.S. economy "in 1978, when things were pretty awful."[3]

These adverse retrospectives did not stem from the year's performance on the criteria of real growth and the unemployment rate. On the contrary, by these metrics, the year was one of the best in the decade of the 1970s. Milton Friedman had remarked in late 1977 that, with regard to real economic activity, the "immediate outlook is good" and that it looked like the U.S. economic expansion "should have a year or so to go"—and those assessments, like the similar Samuelson predictions given a couple of months later, were borne out by the subsequent data for

[1] Email: Edward.Nelson@frb.gov. The views expressed in this paper are those of the author alone and do not necessarily reflect the views of the Board of Governors of the Federal Reserve System or its staff. The author regrets to note that, since the research underlying this chapter was begun, four of the individuals whose interviews with the author are quoted below—Francis Bator, Victoria Chick, Franklin Fisher, and Paul Volcker—have passed away.
[2] *Newsweek*, January 9, 1978*c*, p. 53 (also reprinted in Samuelson, 1983a, p. 133).
[3] Federal Open Market Committee (2014, p. 225). Fisher was an assistant to U.S. Secretary of the Treasury Michael Blumenthal in 1978−1979. See *https://www.federalreservehistory.org/people/richard-w-fisher*.

1978.[4] The U.S. unemployment rate in December 1978 was 6 percent, down from 6.4 percent a year earlier, while modern data on U.S. real GDP show a very high 6.7 percent growth in the four quarters to 1978:Q4.

The behavior of productivity growth, it is true, provided a more mixed picture about the country's real economic performance. The four-quarter growth of U.S. labor productivity would turn out to be 2.7 percent in 1978:Q4—in itself, certainly a good outcome. But this followed growth of only 0.7 percent growth in the four quarters to 1977:Q4—an unusually weak reading, by the standards prevailing up to that time, in a year of economic expansion.[5] The cumulative evidence arising from data for 1977 and 1978 put paid to initial optimism regarding the United States' post-1973 productivity growth rate. Initial figures for calendar 1976 had shown 4 percent productivity growth in that year, the best since 1964 (*Daily News* (New York), February 26, 1977). In the wake of such information, William Nordhaus—a member of the Carter Administration's newly assembled Council of Economic Advisers (CEA)—had testified in June 1977: "In general, after correcting for cyclical movements, the economy's productivity grows at a fairly steady rate…" Nordhaus had granted as the "one major exception" to the steady growth a one-time decline in the level of U.S. productivity in 1974, in the immediate wake of the oil embargo.[6] The net effect of national productivity readings in 1977 and 1978 would, however, be to force a reassessment by the CEA of its position that the productivity trend had not changed after 1973—and that reassessment led the Carter Administration in early 1979 to make sharp downward revisions to its estimates of post-1973 U.S. potential output.[7]

The most prominent aspects of national performance casting a pall over 1978's macroeconomic record were, however, to be found instead in the behavior of nominal variables. One of these variables—inflation—has been discussed at length in the previous chapter. U.S. inflation got

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[4] The quotations are from Friedman (1977a, p. 23). The occasion was the year's "Stanford/Hoover" conference—held in Los Angeles, which he visited in the first week of November 1977 (*Los Angeles Times*, November 7, 1977).

[5] See "Nonfarm Business Sector: Real Output Per Hour of All Persons, Percent Change From Quarter One Year Ago, Quarterly, Seasonally Adjusted," *https://fred.stlouisfed.org/series/PRS85006151*. Modern vintages of *annual-average* data suggest that labor productivity (output per hour) in the nonfarm business sector rose 2.8 percent in 1975, 3.5 percent in 1976, and 1.5 percent in both 1977 and 1978. See Council of Economic Advisers (2018, Table B−16, p. 551).

[6] From page 98 of Nordhaus' written submission for his testimony of June 1, 1977, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1977a).

[7] For further discussion, see Orphanides (2003) and Chapter 11 below. Nordhaus himself would later find (see his 2004 paper) that labor productivity growth fell from 2.27 percent in 1959−1973 to 1.34 percent in 1973−1995 (p. 27) and that, with regard to the slowdown after 1973, "for a five-year window, the slowdown is largest for the 1978–82 period, centered on 1980" (p. 6).

severely worse during the year. This result, though in keeping with Friedman's public warnings during 1977, was in marked contrast to a January 1978 memorandum to President Carter, written by two of his domestic advisers, that had stated: "We see no sign that inflation is heating up again, or is likely to do so over the next two years."[8] Double-digit inflation—which Samuelson's January column had given only a 1-in-10 chance—was barely avoided in 1978, and Friedman accurately foresaw (*Newsweek*, April 24, 1978) that a recession would occur as policymakers reacted to the breakout (although the recession did not start until 1980—later than he predicted).

Another major source of discontent about economic developments in 1978 was one that likely especially shaped Richard Fisher's impression of that year's events, due to his having been located at the U.S. Treasury: the depreciation of the U.S. dollar nominal exchange rate.

U.S. dollar depreciation

Although Friedman would state on multiple occasions in the mid-1980s that the flight from the U.S. dollar "had begun in 1978," this was not an accurate recollection.[9] The concentrated period of dollar depreciation would come to be regarded as comprising October 1977 through October 1978 (see, for example, Abrams, 1979, p. 21; P. Wonnacott, 1982, pp. 3–4). The period of dollar weakness was actually already underway during summer 1977 (see, for example, Laffer and Miles, 1982, p. 405), before speeding up considerably later in the year—a pattern evident in Figure 1. The summer declines in the exchange rate had led Chairman Burns to state in testimony in late July 1977 that the Federal Reserve was "deeply concerned about the dollar."[10] Indeed, that early decline in the exchange rate was the subject of widespread media commentary and had prompted Friedman to devote a column to the matter at the end of the summer (*Newsweek*, September 5, 1977).

Cumulatively, the U.S. dollar effective exchange rate index fell nearly 14 percent in the eighteen months from April 1977 to October 1978. This depreciation paralleled, albeit on a smaller scale, the nearly 24 percent depreciation that the pound sterling had experienced two years earlier (that

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[8] Memorandum (January 16, 1978) to President Carter by Stuart Eizenstat and Charles Schultze. See Biven (2002, pp. 199, 308) for the quotation and date, respectively.
[9] He would say this in Friedman (1983a, p. 28; 1984a, p. 28; 1985, p. 53).
[10] From Burns' testimony of July 26, 1977, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1977d, p. 70). For an example of international media coverage of the dollar depreciation, see *Yorkshire Post* (Leeds, U.K.), August 4, 1977.

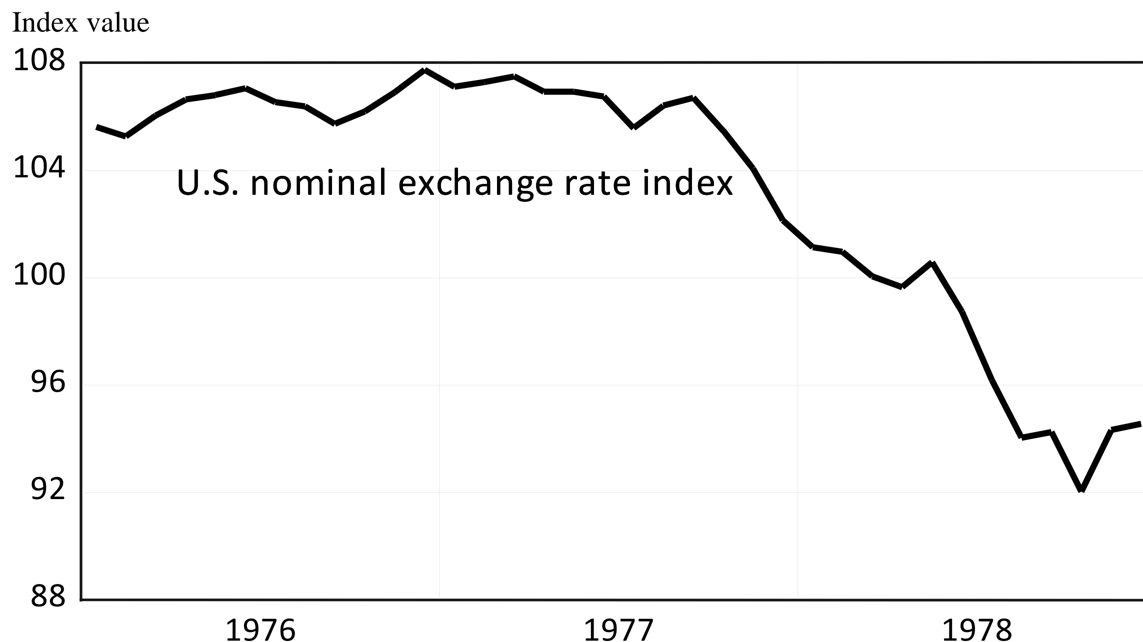

Figure 1. The U.S. nominal exchange rate, January 1976–December 1978.
Source: Trade Weighted U.S. Dollar Index: Major Currencies—Goods (Index, March 1973 = 100), Monthly, Not Seasonally Adjusted, FRED portal (*https://fred.stlouisfed.org/series/TWEXMMTH*).

is, in the period April 1975-October 1976).[11] This protracted decline in the U.K. exchange rate, which had eventually led to a Friedman *Newsweek* column (October 11, 1976*a*) on "The Pound Crisis," occurred alongside a major public debate on economic policy in the United Kingdom—one characterized by a high degree of media interest in Friedman.[12] His many appearances during the second half of 1976 on U.K. commercial and state-owned television stations and in its national press were a source of irritation to *Labour Monthly*—a periodical that, despite its name, had little sympathy with the ruling Labour government and was instead in favor of revolutionary socialism. 'In this situation," stated its lead article as 1976 closed (January 1977, p. 3), "it is no wonder that the arch-reactionary American economist, Professor Milton Friedman, should be shooting his mouth off with a maximum of publicity in our press and TV on the dire fate facing Britain...."

[11] An index of the effective nominal exchange rate for the U.K. pound sterling registers a value of 171.99 in April 1975 and of 130.95 in October 1976. Its low value for the decade of the 1970s was recorded in November 1976, at 130.7. See *https://fred.stlouisfed.org/series/NNGBBIS*.
[12] Book-length accounts of this episode, which mainly focused on its fiscal-policy aspects and did not particularly highlight Friedman's role in the public side of the debate, were Burk and Cairncross (1992) and Dell (1991). On Friedman's prominence in the public discussion, see Nelson (2009a, pp. 81–96; 2009b, pp. 490–494).

Friedman's profile was also very high in 1977 and 1978 during the United States' own exchange-rate trauma. A decade previously, *Washington Post* economics writer Hobart Rowen had suggested that, in terms of economics, the year 1967 "seems to have been the year of Milton Friedman" (December 31, 1967). But the year 1977 would prove to make a stronger claim for this label. The very first day of the year would see, unusually, a two-month-old instalment of *Meet the Press* repeated in the Chicago area, in a near-primetime television slot (*Chicago Daily News*, January 1, 1977). This consisted of Friedman's most recent appearance on the program, and its rebroadcast would occur on what was virtually Friedman's last day living in the city of Chicago.[13] And, near the end of the year, another newspaper economics columnist, J.A. Livingston, would observe that "Americans have been Friedmanized" (*Kansas City Star* (Missouri), November 21, 1977).

Livingston, like Rowen a decade earlier, had made his remark in reference to the national attention given to Friedman's emphasis on the role of monetary factors in domestic economic behavior.[14] In particular, Livingston was lamenting the extent to which commentary in the United States regarding the behavior of real economic series was preoccupied with tracing such behavior to movements to the money stock. As it happened, this tendency was more true of the financial world than of policymaking: it is detailed below how the Carter Administration's economic team often seemed to center their analysis of aggregate spending behavior on *fiscal* policy, rather than on monetary policy. In the area of international economic arrangements, however, the notion that the United States had been "Friedmanized" by 1977 had considerable plausibility. Friedman's work on floating rates had been cited by the *New York Times* (October 15, 1976*b*) as his greatest influence on national policy, and the publicity created by the dollar depreciation of 1977 to 1978 underlined the fact that, over the preceding years, international monetary arrangements had moved in a direction that Friedman had pointed toward.

For his part, speaking in April 1978, Friedman contended that the "brute force of events," rather than the analytical case for flexible exchange rates he had laid out twenty-five years earlier, had given rise to the new international monetary arrangements. But he did grant that the theoretical analysis of floating exchange rates had helped establish a climate of opinion in which the floating-rate system could settle in (*The Listener* (London), April 27, 1978, p. 528).

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[13] In some listings, the replay of the program was given the title *Milton Friedman Meets the Press* (*Register-Star* (Rockford, Illinois), January 1, 1977).
[14] Perhaps inspired by coming across Livingston's column, U.K. financial writer Patrick Sergeant voiced a related observation-cum-complaint about the situation prevailing in his own country, in a column that asked the question: "Is Money Really All That Matters?" (*Daily Mail* (London), November 26, 1977).

Events during 1977 and 1978 in the country in which Friedman made these remarks—the United Kingdom—bore this observation out. The U.K. pound sterling had floated in panicked circumstances in June 1972. But, having caught their breath, the U.K. authorities' considered view was that a float was preferable to the alternatives available. Specifically, in both October 1977 and December 1978, the U.K. government consciously decided to float, or to keep floating, the pound sterling, and their decision was based on the domestic-monetary-control/policy-autonomy arguments that had permeated Friedman's 1953 paper on floating and his subsequent contributions on the subject. The 1977 decision ended several months in which the authorities had tentatively restored a fixed sterling/dollar exchange rate, while the 1978 decision saw the U.K. authorities decline to join the European Economic Community's new exchange-rate mechanism (ERM), under which it was proposed that, within major parts of Western Europe, exchange rates would be fixed.

As an enthusiastic proponent of floating rates, Friedman was, *de facto*, a leading spokesperson for the post-1973 system. Accordingly, although his judgment, as expressed at the September 1978 Mont Pelerin Society meeting in Hong Kong, was that a floating-rate system was "here to stay for the indefinite future," he was also aware of, and sensitive to, doubts about the system and efforts to replace it.[15] The European Economic Community's ERM was one such effort in the direction of curbing the floating-rate system. But the ERM was not due to start until 1979, and it would not involve an attempt to fix rates of exchange against the dollar. In the meantime, during 1977 and 1978, Friedman encountered doubts in his own country about the merits of floating rates. The U.S. dollar's large depreciation gave rise to criticism on the part of the U.S. government and outside commentators of the foreign exchange market's judgments.

In the face of these reactions, Friedman first affirmed the need for a more relaxed attitude toward price movements in the foreign exchange market. In the early stages of the dollar depreciation, he blasted "the exaggerated attention paid by the news media to trivial changes in exchange rate" (*Newsweek*, September 5, 1977). And when, over the course of the subsequent six months, the downward trend in the dollar exchange rate became consolidated, Friedman emphasized the degree to which the move was explicable in terms of fundamental economic behavior, rather than being capricious. In particular, in February 1978 Friedman observed: "The dollar is weak because our inflation rate has been higher, and our anticipated rate of inflation is higher, than the

[15] The quotation is from Friedman (1978c, p. R–181).

inflation rate of Germany, of Switzerland, of Japan."[16] This contrasted notably with Arthur Okun's declaration a couple of weeks earlier that the depreciation "is *not* the result of above-average inflation rates" (*Washington Post*, January 29, 1978; emphasis in original).

As a general proposition, the notion that exchange exchange-rate movements in the floating-rate era could be explained satisfactorily would come under challenge—with Meese and Rogoff (1983) documenting the result that much of the movement observed in the 1970s could not be well accounted for by proposed empirical models. The simplest version of these models—one in which real exchange rates were constant, so that purchasing power parity (PPP) alone drove nominal exchange-rate variations—was one of the first casualties of the floating-rate era. Friedman's former University of Chicago colleague Jacob Frenkel reported at a conference in June 1980: "One of the striking facts concerning the relationship between prices and exchange rates during the 1970s has been the dismal performance of the predictions of the simple versions of the purchasing power parity doctrine."[17]

Friedman was not, however, an adherent to strong versions of PPP. He had consistently taken the position that real shocks struck individual economies and the world and that the real exchange rate needed to adjust in response to these shocks. Furthermore, the fact that real exchange rates varied considerably after 1973—and even the fact that these movements were often hard to trace to fundamental economic driving forces—did not mean that it was not sensible to expect, other things equal, that a higher pace of price rise in one country than abroad should lead to a commensurate depreciation of that country's exchange rate. U.S. inflation over the 1977−1978 period exceeded the rate prevailing in some key trading partners, so several points of the double-digit depreciation in that period could be legitimately attributed to inflation-rate differentials.

As the authorities saw it in a Federal Reserve/U.S. Treasury announcement in early 1978, official action was now required to "check speculation and reestablish order in the foreign exchange markets."[18] Against this background of growing disillusionment in U.S. officialdom about what floating rates were delivering—including what officials considered to be an excessive decline in the dollar—trends emerged during 1977−1978 in U.S. policy developments unfavorable to

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[16] *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," February 10, 1978, pp. 35−36 of transcript. See also his remarks in *Chicago Tribune*, April 2, 1978*a*.
[17] Frenkel (1980, p. 145).
[18] Statement of January 4, 1978 (issued by the Board of Governors of the Federal Reserve System and the U.S. Treasury), in Federal Reserve Board (1978, p. 60).

Friedman's position. In particular, these trends heightened the prospect of a move away from, rather than cementation of, a system Friedman favored: that is, one in which leading economies' exchange rates were market-driven, trade and capital accounts were liberalized, and home economies' aggregate demand policy focused on domestic objectives.

In these two years, Friedman critiqued a variety of explicit administration initiatives in the international-economic-policy area that proceeded along a path contrary to his own recommendations. These included the continuation, in President Carter's first year, of the policy of occasional official interventions in the foreign exchange market; the administration's attempts to coordinate aggregate-demand policies across major economies; and a concerted intervention package, the "dollar rescue" of November 1978. In addition, the prospect arose that further measures in international economic policy would be undertaken: direct intervention by the administration in the underlying components of the current and capital accounts of the balance of payments, via increased restrictions on trade and foreign-exchange transactions. These were measures that Friedman feared were around the corner, as they seemed to fit in with the direction in which international economic policy was going.

Friedman's interventions in 1977 and 1978 in the various areas of international economic policy are now elaborated on, together with related issues in international economics that came up during these years.

Foreign exchange intervention

The existing world exchange-rate system "satisfies no one," Friedman stated in September 1978.[19] In his case, the source of dissatisfaction was the fact that the system was not a clean float but, instead, involved official interventions with foreign exchange intervention by the public sectors—interventions that he considered to be the "only thing wrong with the present system" (*Chicago Tribune*, April 2, 1978*a*, p. 8). In particular, with regard to U.S. exchange-rate policy, Friedman observed: "The Federal Reserve Board ought to keep out of the foreign exchange markets completely—and allow the dollar to find its own level." (*Chicago Tribune*, April 2, 1978*a*, p. 8.)[20]

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[19] Friedman (1978c, p. R−181).
[20] The Federal Reserve Board was only one of the U.S. policy entities involved in these operations. Foreign exchange operations were undertaken by the Federal Reserve Bank of New York, required approval by the Federal Open Market Committee, and in 1978 involved the Exchange Stabilization Fund of the U.S. Treasury (see Federal Reserve Board, 1978, p. 60, and Abrams, 1979, pp. 15−16). As already indicated, the January 1978 announcement

At the start of 1978, Friedman had been invited to testify to Congress on issues concerning exchange rates. Scheduling conflicts led him to decline this offer. But the invitation, received in a letter dated January 13, evidently played a role, along with two Federal Reserve Board press releases that were collected in the January 1978 issue of the *Federal Reserve Bulletin*, in galvanizing Friedman into writing a new *Newsweek* column (January 30, 1978) on foreign-exchange intervention.[21] The first of the Federal Reserve Board releases, already noted, had announced that the Federal Reserve and the U.S. Treasury would from this point on take a more regular role in intervening in the exchange market (Federal Reserve Board, 1978, p. 60), while the second release had reported details on Federal Reserve income for the year 1977 (Federal Reserve Board, 1978, p. 63). This income statement reported "a loss of $146 million in foreign exchange transactions."

Friedman's column noted that official intervention had been justified by the aim to rein in unjustified short-term market moves. Echoing a passage of his 1953 article, Friedman argued how to evaluate the validity of this intervention: "In general, there is a simple test: whether it makes or loses money." (*Newsweek*, January 30, 1978.)[22] In particular, if the authorities purchased dollar assets and sold foreign assets when the U.S. exchange rate was under untoward downward pressure, it should enjoy an augmentation in the value of its foreign-exchange portfolio once the selling pressure had passed. Friedman reported the bottom line by year since 1971 of the Federal Reserve's foreign-exchange record. This showed a loss for each year, culminating in the aforementioned $146 million loss in 1977.[23] Friedman noted that the losses cumulated to $550 million since 1971 and argued that this implied a negative verdict on the

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of increased exchange-market intervention by the U.S. authorities had been in a joint statement by the Federal Reserve Board and the U.S. Treasury.

[21] Friedman would enclose a copy of the *Newsweek* column, together with an older column and remarks written specifically for the congressional proceedings volume, in his letter of reply to the invitation to testify. See Friedman (1978d).

[22] The passage this echoed was the following: "It would do little harm for a government agency to speculate in the exchange market provided it held to the objective of smoothing out temporary fluctuations and not interfering with fundamental adjustments. And there should be a simple criterion of success—whether the agency makes or loses money." (Friedman, 1953a, p. 188.)

[23] Argy (1994, p. 400) mistakenly suggested that, in making these tallies, Friedman was "drawing heavily" on Dean Taylor's (1982) study. This assertion was incorrect: Friedman was simply using published, official, Federal Reserve figures. Taylor's study first materialized only later (with its working paper circulated as D. Taylor, 1980), and it actually came at Friedman's suggestion. "Milton originally suggested looking at official foreign exchange intervention. He [then] came to a seminar I gave on the topic [when Taylor was visiting the Hoover Institution as a National Fellow around 1979/1980] and gave feedback." (Dean Taylor, personal communication, November 14, 2019.) See also D. Taylor (1982, p. 356).

United States' foreign exchange intervention.[24]

Federal Reserve Board Governor Henry Wallich responded almost immediately, in Congressional testimony of February 6, 1978, at the hearing to which Friedman had been invited but which he had turned down attending. As often occurred in the case of public statements by national central bankers, Wallich's testimony responded to Friedman without mentioning him by name. To the initiated, however, Wallich's reference to Friedman was nevertheless unmistakable: "One test, which has sometimes been proposed, of whether actual intervention operations serve [their intended purpose]… is the degree to which intervention is profitable."[25] Wallich did not strongly dispute the validity of this test—but argued that the Federal Reserve had, in fact, passed it. His basis for this conclusion was the reliance on a narrower measure of Federal Reserve foreign exchange profit and losses than that Friedman had used—one excluding the losses the Federal Reserve had incurred by disposing of investments it had inherited from the Bretton Woods era. It was true that this narrower measure did show profits in each year from 1971 to 1977. However, the same measure went on to show a very sizable loss in 1978, after the Federal Reserve's step-up in intervention that year—making intervention over 1971–1978 loss-making overall, even on this narrower measure (see Abrams, 1979, p. 21).

The debate on Friedman's "simple test" for the validity of foreign-exchange intervention got a further fillip a few years later. Friedman in his 1978 *Newsweek* column had stated that he was not aware of comparable reported figures on profits and losses from exchange interventions by the authorities of the United Kingdom, Japan, and the Federal Republic of Germany. He subsequently suggested an investigation of the record of these countries to a former student of his, Dean Taylor, whose strongly-worded article on the matter appeared in the April 1982 issue of the *Journal of Political Economy*. Dean Taylor's article opened with the relevant quotation from Friedman's 1953 article then immediately stated his own conclusion that central-bank intervention by the central banks of Canada, France, Germany, Italy, Japan, Spain, Switzerland, the United Kingdom, and the United States during the floating-rate era had been "a dismal failure when judged by Friedman's profit criterion."[26] The Taylor piece also alleged a lack of transparency in reporting intervention data on the part of several central banks, with the article containing caustic remarks about the difficulties he had encountered in gleaning the relevant

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[24] He repeated this verdict, and the $550 million figure, in *Chicago Tribune*, April 2, 1978*a*, and in *Milton Friedman Speaks*, Episode 8, "Free Trade: Producer Versus Consumer," April 27, 1978, p. 18 of transcript.
[25] Wallich (1978, p. 89).
[26] D. Taylor (1982, p. 356).

information from several countries' reported data.[27] Dean Taylor concluded his study with the parting shot that "Friedman's (1953[a]) prediction has proven accurate" in foreshadowing that official actors would not possess superior judgment to that of private-sector participants when it came to the warranted movements of exchange rates (D. Taylor, 1982, p. 367).

In response to Dean Taylor's piece, the official sector swung into action, with the cross-central-bank Working Group on Exchange Market Intervention being set up at a June 1982 summit in Versailles (Bank of England, 1983, p. 384). Subsequently, various items appeared in research and policy outlets defending exchange-market intervention. For example, a Bank for International Settlements working paper by Helmut Mayer and Hiroo Taguchi (1983) offered empirical evidence pointing to the success of intervention, while also providing a scattershot set of arguments challenging Friedman's profit criterion as the basis for evaluating the validity of intervention. And the Bank of England, in effect, published a reply to Dean Taylor in the September 1983 issue of its official organ, the *Bank of England Quarterly Bulletin*. This rebuttal was notably explicit, citing Dean Taylor's work in a footnote to the abstract, and, very unusually for official Bank statements of the time, referring to Friedman by name and quoting his exchange-rate article. Further along in the decade, the Federal Reserve Board undertook detailed scrutiny both of the notion that profits should be a success-criterion for intervention and of the profit-and-loss record of central banks during the floating-rate era. The culmination of this investigation was the research study by Leahy (1995), which argued strongly on analytical grounds that the success (in stabilizing the exchange rate) of official foreign-exchange intervention and the degree of profitability of that intervention had no necessary relationship.[28] The case made in this study was sufficiently compelling that Bordo, Humpage, and Schwartz (2015, p. 21) indicated that it was the basis for their own acknowledgment that the success of official intervention as a force stabilizing exchange rates should not be evaluated by a profitability criterion.

It is notable, however, that despite the challenge to the generality and the validity of profitability as a criterion for judging the policy success of interventions, Leahy (1995) also stressed that, once data for the 1980s were included, more evidence had accumulated that foreign exchange rate intervention by the U.S. authorities had actually been financially profitable. The stress laid

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[27] D. Taylor (1982, pp. 357–358).

[28] See especially Leahy (1995, p. 833). Earlier, a different but related aspect of Friedman's (1953a) discussion—specifically, his challenge to the notion that private-sector speculation was destabilizing for an asset price—had been challenged on analytical grounds by Murray Kemp (1964, pp. 264–265). Kemp articulated this challenge in specific reference to the elaborated version of the argument given in Friedman (1960b).

on this outcome reflected the fact that Friedman's profit criterion retained intuitive appeal, even if it was now shown not to be a universally valid metric basis on which to judge whether intervention secured policymakers' aims.

Balance of payments deficits and current account deficits

By the time the aforementioned Leahy study appeared in the mid-1990s, the practice of making foreign exchange intervention with the aim of altering the external value of the dollar had essentially stopped (Bordo, Humpage, and Schwartz, 2015, p. 15). By this stage, monetary authorities, at least in the English-speaking world, had become much more reconciled to, and comfortable with, floating exchange rates. As already indicated, this outcome largely reflected acceptance of Friedman's point about the monetary-autonomy benefits conferred by floating. In the area of international arrangements, it also reflected implicit acceptance of an observation Friedman had reaffirmed in 1977 that, under a float, "there literally is no such thing as a 'balance-of-payments problem." (*Newsweek*, September 5, 1977.)

When Friedman had made this argument in 1953, it had been greeted with scorn by Frank Hahn, who claimed that the notion that a free-market exchange rate could deliver a zero overall balance of payments was "unproven assertion," that floating would allow domestic economic policy to deliver a desired level of (nominal) income an "unproven (and indeed almost completely unanalyzed) assumption," and mocked Friedman's essay for the way in which "Britain's postwar balance of payment problems, by the way, are, without the slightest use of analysis or facts, ascribed to fixed exchange rates!"[29]

Yet Friedman's position that a floating exchange rate would eliminate balance-of-payments deficits was far less controversial than Hahn implied. It was little more than an expression of the truism that, under a pure floating regime, the exchange rate adjusts to ensure that the flows of payments into a country net out to zero. So when, in 1978, a financial commentary stated that "the U.S. has a very large trade deficit, a large current account deficit, and an overall balance-of-payments deficit" (*Dallas Morning News*, February 3, 1978), in listing the three deficits in quick succession, it did not give a very informative picture. The last of the three deficits listed—the balance-of-payments deficit—was, in truth, different in nature from the first two, because it owed its existence to the fact that central banks were not allowing exchange rates to float

[29] Hahn (1954, p. 400).

completely cleanly. Insofar as the U.S. authorities were generating the balance-of-payments deficit by holding up the U.S. dollar through interventions, Friedman regarded the deficit as wholly undesirable.[30] Insofar as the deficit was being generated by foreign central banks' interventions, his feelings were more mixed. Friedman wished that they would not undertake these interventions, though he did not think the United States could stop them from doing so (*Chicago Tribune*, April 2, 1978*a*, p. 8). But he saw a bright side: insofar as they did try to hold their currencies up against the dollar, these countries were not putting their own taxpayers' funds at risk, not U.S. resources (*Newsweek*, September 5, 1977). And attempts by the rest of the world to make investments in the United States (and provide foreign goods) at a stronger exchange rate for the United States than the market would deliver amounted to a subsidy to the United States.[31]

With regard to the other deficits listed in the report—those on the trade and current accounts of the balance of payments—matters were more complicated. Many other than Friedman saw these deficits, particularly the current account deficit, as what should be properly viewed as key drivers of the exchange rate. In this spirit, the early (summer 1977) depreciation of the U.S. dollar was believed to be have been both welcomed and encouraged by Secretary of the Treasury Blumenthal—and as being seen by him as an appropriate means through which a narrowing of the U.S. current account and trade deficits would be brought about (*Detroit Free Press*, August 22, 1977; *Financial Times* (London), November 2, 1978*a*). Later, Board Governor Wallich, though arguing that the dollar depreciation had been excessive, suggested (in his aforementioned February 1978 testimony) that the widening of the trade and current account deficits had been a "major factor in the dollar's recent weakness in the exchange markets."[32] Along the same lines, in the research world, a paper "Exchange Rates and the Current Account" by Dornbusch and Fischer (1980b) postulated that a correctly-functioning floating exchange rate system should deliver values that tended to eliminate current account imbalances.

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[30] The absence of those central bank operations that are formally categorized as foreign exchange market interventions is not a sufficient condition for the overall balance of payments to have no surplus or deficit, that is, for the balance for official financing (or, roughly speaking, the change in a country's gross official foreign exchange reserves) to be zero. The monetary authority may engage in other operations or transactions that are not designated intervention but that are conceptually similar to intervention and that may produce a nonzero balance for official financing. See Bank of England (1983, p. 384) and Leahy (1995, p. 424).
[31] On this second point, see Friedman's remarks in *Chicago Tribune*, April 2, 1978*a*, p. 8, and *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, p. 36 of transcript.
[32] Wallich (1978, p. 87). Similarly, a *Financial Times* analysis (November 2, 1978*a*) would claim: "The timing of the depreciation of the dollar can be directly linked to the deterioration of the current account of the U.S. balance of payments during the first half of 1977."

Friedman was on the same wavelength as these analysts in a couple of respects: like them, he did believe that exchange-rate depreciations tended to narrow trade and current account deficits; and like them, he favored explaining the 1977–1978 exchange-rate depreciation in terms of economic fundamentals. He nevertheless parted company with them, as he was not well disposed toward making trade or current account imbalances central to the explanation of exchange-rate behavior. Indeed, as discussed in previous chapters, Friedman's argument for floating exchange rates did not rest on any claim that a floating rate would deliver a zero balance, or even necessarily a diminished imbalance, in the current account portion of the balance of payments.[33]

Friedman's belief, articulated in the 1960s, was that the generation of a sustained widening of current account deficit might be part of the proper operation of a country's floating exchange rate, was something he maintained in the 1970s and would develop further in the 1980s. He granted that a current account deficit might require correction in some circumstances (*Newsweek*, September 5, 1977). He did not, however, see that situation as very apposite for the discussion of conditions prevailing in the United States in 1977–1978.[34]

Rather, what he had in 1953 called the "fundamental adjustments" that required a move in the dollar lay elsewhere. "The dollar is weak not because we are importing oil but because we are producing pieces of paper, namely money," Friedman remarked in April 1978.[35] Following policies that stabilized the internal purchasing power of the dollar would mean "we could forget about its external value," he suggested (*Newsweek*, September 5, 1977), and the associated domestic policies, by lowering and then eliminating inflation, in any event provided the only true means of arresting the depreciation (*Chicago Tribune*, April 2, 1978*a*, p. 7).[36]

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[33] He was therefore not favorably disposed toward characterizations such as that Sylvia Porter made: "our balance of payments is drowning in red ink" (*Detroit Free Press*, August 22, 1977). In this statement, Porter was referring merely to the trade-account subcategory of the balance of payments. That said, Friedman himself did occasionally use "balance of payments deficit" as a (very poor) shorthand for the deficit in the current account of the balance of payments (see, for example, *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, p. 36 of transcript).

[34] See also his remarks in *Milton Friedman Speaks*, Episode 2, "Myths That Conceal Reality," taped October 13, 1977, pp. 32–33 and 36 of transcript.

[35] *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, p. 35 of transcript.

[36] In an appearance on NBC's *Meet the Press* program, Alan Greenspan similarly assessed that participants in the foreign exchange market "are essentially saying that they have very little confidence in the future purchasing power of the dollar—which is essentially a signal that an anti-inflationary policy, a credible anti-inflationary policy, is mandatory for this country" (quoted in *American Banker*, April 24, 1978).

Depreciation, inflation, and the trilemma

U.S. officials *did* see a strong relationship between dollar depreciation and U.S. inflation—but not the one that Friedman emphasized. On the contrary, instead of tracing the exchange rate's course primarily to price-level developments—and ultimately to monetary policy—they invoked a depreciation-induced price-level spiral as part of their *non*monetary accounts of the inflation process.[37] This approach, already used by Arthur Burns after the 1971 and 1973 dollar declines, was affirmed by him from an early stage of the 1977–1978 depreciation. "If the dollar depreciates in foreign exchange markets, that releases forces that tend to raise our price level," Burns stated in July 1977.[38] And when the depreciation intensified, he cited it as one of the factors tending to raise U.S. inflation (see Nelson, 2005). Burns' successor William Miller similarly testified in April 1978: "The decline of the dollar since last September will add about three-quarters of 1 percent to the inflation rate in this country by the end of this year."[39] Miller subsequently continued on this theme, maintaining: "A weak dollar introduces inflation into our economy." (*U.S. News and World Report*, August 7, 1978, p. 19.)

These were arguments to which Friedman had taken exception in discussions of floating exchange rates for twenty-five years. And in an appearance on *Meet the Press* appearance in November 1978, he affirmed: "Arithmetic is one thing, and economics is a very different thing… The great confusion in this area is to confuse particular prices with prices in general."[40] As discussed in Chapter 3 above, Friedman did allow for some lasting implications on potential output of changes in the terms of trade (of the kind that depreciations would induce). But, holding constant this effect arising on the supply side, he expected depreciations to amount to relative-price movements that would over time leave the aggregate price level and its rate of growth unchanged.

The notion that depreciation generated inflation—despite being one of the cost-push explanations that Friedman first challenged in his writings—proved hard for economists, other commentators, and those in policy circles to shake off. Even as central banks accepted the monetary nature of inflation, on the one hand, it was still commonplace to find, on the other

[37] Volcker (1978, p. 8) did acknowledge that higher relative inflation rates acted to promote depreciation. But, as indicated below, he went on to stress depreciation as a factor *driving* inflation.
[38] From Burns' testimony of July 26, 1977, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1977d, p. 70).
[39] From Miller's testimony of April 10, 1978, in Committee on Banking, Finance and Urban Affairs (1978b, p. 136).
[40] *Meet the Press*, NBC, November 12, 1978, p. 4 of transcript.

hand, official statements taking for granted that exchange-rate depreciation generated inflation and that, conversely, appreciation tended to lower inflation. However, an acceptance that these two propositions were not really compatible, and a statement in favor of the notion that depreciations were not a clear-cut source of inflationary pressure (on net), appeared at the official level in 2007 in a speech by a Federal Reserve Board governor. Specifically, Mishkin (2007b, p. 3) noted the relevance, in judging the implications for inflation of movements in the dollar (and in import prices), "the argument made by Milton Friedman… [that a]ny pattern of relative-price changes is compatible with a particular level of the inflation rate. What determines the overall inflation rate is not [the] relative prices [of] one category of goods and services[,] but rather the balance between overall demand and supply in the economy, which ultimately is influenced by monetary policy."

It is also possible to detect another important sign of moves away from the import-price-push conception of inflation (implicit in the view that depreciations produce inflation) in the increasing acceptance over time of Friedman's "trilemma" concept. The proposition of a "trilemma," already discussed in Chapter 4 above, holds that a central bank can choose its country's inflation rate under a float. The trilemma is therefore antithetical to a view that, for given settings of aggregate demand policy, a floating exchange rate is itself a source of inflationary or disinflationary pressure. Over time, it has become more widely understood that the securing of monetary policy autonomy under a floating rate is of practical relevance and is not merely a condition obtaining under restrictive conditions.[41] Along these lines, two notable figures who moved away from the import-price-push position to the trilemma position were Maurice Obstfeld and Paul Volcker. Early in his career, at a conference in Fontainebleau, France, in July 1982, Obstfeld (1985, p. 276) claimed that, for floating-rate countries, "the policy autonomy predicted… never really materialized. Instead, the inflationary effect of exchange rate depreciation emerged as the new external constraint on the conduct of mon policy." In contrast, thirty-five years later, Obstfeld was a strong advocate of the validity of the trilemma (see Obstfeld, Ostry, and Qureshi, 2017). Similarly, Paul Volcker (1978a, p. 8) had contended that "depreciating currencies… exaggerate inflationary forces." But Volcker would later state that "the trilemma and the dilemmas and so forth are all true." (Paul Volcker, interview, October 16, 2013.)[42]

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[41] For example, such autonomy is fully consistent, as discussed in Nelson (2020d), with comovement of various asset prices across countries. The claim by Fahrer and Shori (1990, p. 5) that monetary policy cannot deliver a chosen inflation rate under a float if there is international capital mobility was certainly not correct.
[42] Meltzer (2009b, pp. 1010–1011) argued that the period after 1978 was when Volcker increasingly discarded cost-push aspects of the analysis of inflation.

Policy coordination and the locomotive theory

As of the late 1970s, however, the resilience of import-price-push views of inflation showed that key aspects of the trilemma notion were still encountering resistance in U.S. officialdom. Furthermore, another aspect of the U.S. authorities' attitude to economic policy in this period worked against the notion of policy autonomy under an exchange-rate float. This was the Carter Administration's encouragement of international macroeconomic policy coordination.

What was sometimes called the "locomotive theory" implied that the engagement of countries like Japan and the Federal Republic of Germany in expansionary aggregate demand policies would make it easier for the U.S. authorities to increase demand, because the U.S. trade deficit was less likely to deteriorate sharply in response to economic expansion at home if the United States' major trading partners were expanding too. A concerted cross-country stimulation of demand was an approach pushed by the Carter Administration at the May 1977 G7 economic summit in London, after whose first session Secretary of the Treasury Blumenthal said of the output-growth aims of the member countries: "We have our targets. We expect to meet them. We will do what is necessary to meet them." (*Kansas City Star* (Missouri), May 7, 1977.) The matter was pressed further at the G7 summit in Bonn held in June 1978—by which time the dollar depreciation had made the U.S. participants still more interested in commitments being made by Germany and Japan to expansionary measures (see Eizenstat, 2018, pp. 210–215).

Friedman was not well disposed toward these arguments. Asked in 1977 whether Germany and Japan were "mainly to blame for not pushing their recovery along as fast as they could have done?," Friedman answered: "No." (*Euromoney*, October 1977, p. 21.) The United States should not complain: "Each country is mostly to blame for its own problems." (*Euromoney*, October 1977, p. 25.)

Friedman addressed the "locomotive" theory more specifically at around the time of the Bonn summit. He agreed that, in an ideal situation, all economies would be operating continuously at a high level. But he also implied that some cyclical fluctuations were unavoidable. And, subject to the acceptance of this reality, Friedman threw doubt on the notion that economies should have closely synchronized business cycles.[43] "Surely, the best situation is for some countries to be in the expansion phase… while others are in the recession phase. In that way, the various booms

[43] As already indicated, Friedman also doubted a key rationale (trade-balance considerations) for the United States' support for simultaneous expansion.

and recessions would at least to some extent average out on a world scale." (*Newsweek*, June 12, 1978.)

In opposition to this argument, those in policy circles at the time could have pointed out that pronounced negative output gaps were prevalent across advanced nations in late 1976—with McCracken and others (1977, p. 339) reporting that an output gap of minus 8.1 percent existed in the aggregate of nine OECD countries as of the second half of 1976—in which case substantial economic expansion by the major economies seemed warranted.[44] This basis for expansionary policies proved to be fragile: it would subsequently be undermined by later revisions to historical output-gap estimates. In the meantime, however, the Carter Administration succeeded in gaining Japan's and Germany's agreement to undertake expansionary measures. The main measures undertaken, however, went in the direction of easing fiscal policy, particularly tax cuts (Eizenstat, 2018, p. 214), rather than monetary policy. Consequently, they were, from Friedman's perspective, were unlikely in practice to boost nominal or real aggregate demand very much in those countries.

Paul Samuelson, often a disarmingly outspoken supporter of Friedman on exchange-rate matters in the 1960s, disagreed with Friedman on the appropriate domestic economic-policy prescriptions for the United States both in that in that decade and in the 1970s. But, with regard to the policy-coordination issue that was so prominent in economic discussions in 1977 and 1978, he once again showed his solidarity with Friedman on matters of international economic policy. Speaking at a conference held in the first week of October 1978, Samuelson remarked: "I have heard people say that we have to have coordinated policies with floating exchange rates. That is what we *don't* have to have." If Germany wanted to orient its aggregate-demand policies on fighting inflation, Samuelson added, "that is its own business under a properly running floating exchange rate."[45]

Yet, less than three months later, *New York Times* economics columnist Leonard Silk would declare Friedman and Samuelson to have had their views on exchange rates refuted. Silk's

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[44] The same 1977 report gave a negative output gap that was near or in double digits in late 1976 in the case of Germany and Japan (see McCracken and others, 1977, pp. 84, 86).

[45] In Hinshaw (1981, p. 86). For a contemporary report on this conference, see *Los Angeles Times*, October 9, 1978. Although the Federal Republic of Germany's stance remained largely one of floating the mark against the dollar, by this point Friedman had criticized the Bundesbank for interventions it had made during 1977 and 1978 aimed at supporting the U.S. dollar and forestalling mark appreciation (*The Telegraph* (Brisbane), December 12, 1977; *Wall Street Journal*, August 28, 1978).

assessment was that what he considered to be 1978's biggest piece of economic news—the U.S. government's announcement the previous month of a dollar rescue package—had occurred because of widespread agreement that the case for floating rates "argued by many economists as different ideologically as Milton Friedman and Paul Samuelson" had been undermined by recent years' experience (*Fort Lauderdale News and Sun-Sentinel* (Florida), December 31, 1978).

The November 1978 measures

This announcement, issued on November 1, 1978, had been a joint statement by Federal Reserve Chairman Miller and Secretary of the Treasury Blumenthal indicating that coordinated Federal Reserve-Administration actions would begin immediately, with the aim of shoring up the foreign exchange value of the U.S. dollar (*Financial Times*, November 2, 1978*b*).

Among the measures was an end to the incremental official operations undertaken in the foreign exchange market in favor of a larger-scale series of interventions (*Financial Times*, November 2, 1978*b*; Eizenstat, 2018, p. 330). With regard to measures like this, Friedman was more skeptical about the likely impact of the package on the economy. As a rule, Friedman believed that the effects—on the exchange rate and domestic nominal spending alike—of foreign exchange operations by a country's central bank or treasury were lasting if they were unsterilized—that is, if they were allowed to affect the aggregate level of the monetary base. Such non-sterilization created scope for the operation to be associated with reactions of a variety of domestic financial series, including domestic asset prices and monetary aggregates. His longstanding position was that sterilized foreign exchange intervention worked for a time on the exchange rate, then its effect faded out (*Newsweek*, October 11, 1976; *Chicago Tribune*, April 2, 1978*a*, p. 8).[46] This attitude has been borne out by later work: for example, Svensson (2001, pp. 279–280) stated that "most empirical work on the effect of sterilized foreign-exchange interventions indicates that [their exchange-rate] effect is small or even negligible." Friedman considered that the intervention component of the November 1978 measures would, by itself, have a negligible impact on the dollar and the U.S. economy (*Meet the Press*, NBC, November 12, 1978, p. 5; *Newsweek*, January 8, 1979).

[46] As Friedman pointed out (*Newsweek*, January 8, 1979), another measure in the 1978 package—an effort to generate a capital inflow into the United States by new issuance of U.S. government bonds abroad—was bound to have an offsetting effect on the exchange rate in the longer term as repayment (in foreign currency) of the loans produced an outflow. At the time, the U.S. authorities saw merit in this measure in part because it connoted confidence by the U.S. government that the exchange rate would not be lower in the future, when the bonds were redeemed (see Eizenstat, 2018, p. 330).

Some observers saw the dollar package as important in signifying that President Carter was willing to countenance a recession. Alan Greenspan, now a private-sector economist, stated that the measures "increase the risks of recession" (*Kansas City Times* (Missouri), November 2, 1978, p. 1A). Indeed, Paul Samuelson saw the package as having made a 1979 U.S. recession very likely (*Newsweek*, January 1, 1979; *Financial Times* (London), August 6, 1979, p. 16), and Otto Eckstein declared, "We are changing our forecast [for 1979] to 'recession.'" (*Detroit Free Press*, November 2, 1978, p. 19A.) Such commentary was influenced particularly by the extent to which the announced measures were perceived as implying a move to a more restrictive policy toward aggregate demand at home. With regard specifically to the implications of the package for monetary policy stance, an anonymous Federal Reserve Board source was quoted as saying at the time of the announcement: "There's no question that the higher interest rates go, the tighter money gets, the higher the risk of recession." (*Detroit Free Press*, November 2, 1978, p. 19A.)

To Friedman, the domestic-policy aspects of the package were crucial—both if the package was going to arrest the depreciation and if it were going to have the restrictive effects on aggregate demand that Samuelson envisioned. In this connection, Friedman observed that what would really shore up the dollar were "credible measures" to reduce monetary growth (*Newsweek*, January 8, 1979).

In evaluating whether the package would really go in this direction, Friedman emphasized (see *Newsweek*, November 20, 1978) that two of the items in the November 1 announcement—a discount-rate rise (to 9.5 percent) and a reserve-requirement increase—were measures whose overall effect on monetary conditions could be offset by open market operations—and, in the case of reserve-requirement changes, often routinely had been offset in this fashion in the past.[47] The actions with regard to these specific monetary policy instruments did not necessarily have any implications, on net, for the monetary base or broader measures of the money stock (*Meet the Press,* NBC, November 12, 1978, p. 5 of transcript).[48]

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[47] Friedman had been persuaded already that reserve-requirement changes had been used in monetary policy announcements because they were perceived as a dramatic way of signaling future, more substantive policy moves. The reserve-requirement change certainly was interpreted by commentary at the time as being dramatic, with a *Financial Times* analysis (November 2, 1978*c*) claiming that it figured as being "potentially [among] the most important" of the announced measures and categorically stating that the "withdrawal of $3 billion of bank liquidity represent[s] a major tightening of credit" that would bolster the Federal Reserve's credibility.

[48] The reserve-requirement increase was perhaps especially unlikely to turn out to be a genuinely restrictive measure. In dollar terms, it was the largest increase in reserve requirements undertaken up to that time (*Financial Times* (London), November 2, 1978*d*). But it was a 2 percent *supplementary* reserve requirement on *large* time deposits—not an increase in regular reserve requirements on commercial banks' retail deposits (Miller, 1978d, p. 944; Judd and Scadding, 1979, p. 36). The new requirement burden therefore pertained to wholesale bank deposits

However, Friedman did not rule out the possibility that a genuine monetary tightening might emerge from the package. Of the various announced moves, there was one element that would certainly work in the direction of making monetary policy tighter —even though, curiously, Friedman omitted it from his list of monetary actions in the package (*Newsweek*, November 20, 1978). This was an increase of 25 basis points in the FOMC's federal funds rate target—from 9.5 percent to 9.75 percent (Federal Open Market Committee, 1978, p. 958). This modest increase, taken together with the previous moves up in the federal funds rate, likely helped contain monetary growth in 1978. As discussed in the previous chapter, M2 growth in 1978 was decidedly below 1977's rate and in the ballpark of Friedman's recommended rate for 1978 of 8 percent. Friedman would, as discussed in the next chapter, conclude in early 1979 that a decided monetary policy tightening had indeed occurred in recent months.

On the day of the November measures, President Carter stated that they had been taken because dollar depreciation "threatens economic progress at home and abroad and the success of our anti-inflation program" (*Detroit Free Press*, November 2, 1978, p. 1A).[49] U.S. policymakers' actions therefore reflected the emphasis on dollar depreciation in their analysis of inflation—and so were a manifestation of their continued adherence to cost-push views. However, unlike so many supposed cost-push factors, the exchange rate was at least conceded by policymakers to be a variable that could be influenced by monetary policy. Thus, the nonmonetary view of inflation held by policymakers during 1978 did not stop them from taking certain monetary actions in response to the rise in inflation. As they saw it, a declining dollar was introducing an extra cost-push element into inflation, and, by fortifying the exchange rate through monetary and other measures, they could limit the inflationary pressure arising from this source.

Trade barriers and foreign exchange controls

During 1977 and 1978, Friedman saw the U.S. government's international economic policy as deviating from what he regarded at its ideal configuration not only because it was advancing

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that were outside M2—whether the M2 considered is the old definition prevailing in 1978 or the modern definition introduced after 1979. Consequently, the monetary impact of this move—as judged by the repercussions for the behavior of M2—depended on whether the reserve-requirement increase on non-M2 deposits had the effect of making increases in M2 more difficult. For example, for a given volume of reserves, a higher requirement on non-M2 deposits might make overall reserves scarce and create downward pressure on M2. But if the Federal Reserve expanded total reserves in line with the higher volume of required reserves, such pressure would not materialize.

[49] Similarly, the record of the FOMC's related deliberations described the rescue package as "a broad government program to strengthen the dollar in foreign exchange markets and thereby to counter continuing domestic inflationary pressures" (Federal Open Market Committee, 1978, p. 957).

measures at international macroeconomic policy coordination in the U.S. authorities' interventions in the foreign exchange market intervention, but also because it was making it more likely that new restrictions would be instituted on transactions related to the trade and capital accounts of the U.S. balance of payments.

Of these, restrictions on the capital account were a possible policy change he feared that was not, in fact, really aired or pursued by the Carter Administration. Certainly, the notion of foreign exchange controls as a macroeconomic policy tool was in the air in economic discussions in this period, thanks in large part to Tobin (1978c, 1978d). Tobin, a one-time virtual ally of Friedman on matters concerning international economic policy, had become disenchanted with floating exchange rates. He now proposed, as a means of organizing the process of intervention, a network of taxes on international capital transactions (an idea subsequently known as the "Tobin tax"). Such a measure would be a streamlined version of the various foreign exchange controls that monetary authorities had used to pursue distinct exchange-rate and demand-management policies. Toward the end of 1978, Friedman suspected that the Carter Administration would want to strengthen the dollar while stimulating aggregate demand and suggested that Carter might introduce foreign exchange controls to do this (*Newsweek*, September 18, 1978).

In the event, of course, the administration instead launched the November 1978 dollar package. The dollar subsequently rallied but, a few months into the new year, as the exchange rate started to falter again, Friedman conjectured that Carter might impose foreign exchange controls sometime later in 1979 or in 1980 (*Daily Telegraph* (London), March 21, 1979; *Financial Times* (London), March 21, 1979). Again, this scenario was not realized, and in 1979 the administration was instead willing to along with a considerable monetary policy tightening as the main means of supporting the dollar.

With regard to trade barriers, President Carter had stated in April 1977: "I am very reluctant to restrict international trade in any way."[50] Nevertheless, the years 1977 and 1978 saw the administration introduce new "orderly marketing agreements" (negotiated quotas on imports) on sales to the United States of footwear produced by Japan and of television equipment produced

[50] In Pater (1978, p. 33), sourcing Carter's remark to *U.S. News and World Report*, April 11, 1977, p. 94. Similarly, Fred Bergsten, assistant secretary of the Treasury on international affairs in the administration stated that "import controls are not a very efficient way to preserve jobs" (in Pater, 1978, p. 31, citing *U.S. News and World Report*, August 8, 1977, p. 25).

by Japan and the Republic of Korea.[51] The administration was also under pressure to introduce new restrictions on steel imports, amid allegations that the low prices of steel imports reflected the dumping of steel on the U.S. market by foreign producers.[52] Speaking in Pittsburgh in October 1977, Friedman declared that protecting jobs in the steel industry would not bolster the overall level of employment in the United States. He also indicated that there was too much activity to stop dumping already: "As a nation, we're stupid to have antidumping laws. They mean that, if someone wants to give us a gift, we should refuse it." (*Minneapolis Tribune*, October 11, 1977.)

In the pre-floating rate era, when confronted with concerns about U.S. producers being hurt by competition, Friedman had frequently invoked the benefits to U.S. consumers that arose from a better terms of trade and the corollary of lower-than-otherwise prices having to be paid for imports (see, for example, Instructional Dynamics Economics Cassette Tape 73, May 10 1971). He had, in particular, used this argument when advocating floating exchange rates in the face of claims that a floating-rate system might lead the United States' trading partners to resort to artificial devices to increase their market share. Back then, Friedman had suggested that, if a country took advantage of a system of more flexible exchange rates to engineer competitive depreciations, the United States should welcome the consequent availability of low-price goods.[53] He continued to make this point in the floating-rate era.

The U.S. public was fairly used to opposition to import restrictions and arguments for free trade being made by economists. However, it was less accustomed to hearing criticism of U.S. trade restrictions when, as with anti-dumping laws, those restrictions were being taken as countermeasures against trading partners' alleged own interference with an ideal free-trade system. Friedman therefore received a stunned reaction, marked by nervous laughter, from his audience when he remarked during an April 1978 speech on free trade: "As a consumer, all I can say is: The more dumping, the better."[54]

By this point, Friedman had given an exposition of his argument in *Newsweek* in a column

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[51] See *Dallas Morning News*, February 5, 1978, *New York Times*, June 18, 1978 (p. 12), National Archives of the United States (1980, p. 167), and Biven (2002, p. 230).
[52] See Biven (2002, p. 230). This eventually led the Carter Administration to introduce, effective on February 15, 1978, a so-called trigger-price system, which imposed restraints on the importation of steel at prices below a specified value. See Carruth (1993, p. 733).
[53] See Friedman and Roosa (1967, p. 118). See also page 91 of the same source, as well as Friedman (1962a, p. 72).
[54] *Milton Friedman Speaks*, Episode 8, "Free Trade: Producer Vs. Consumer," taped April 27, 1978, p. 15 of transcript.

provocatively titled “In Defense of Dumping.”[55] The column once again emphasized the benefits to U.S. consumers arising from lower-priced imports.[56] But, having highlighted this point, Friedman’s column transitioned to arguments against restrictions on imports that applied irrespective of whether the restrictions were being imposed as a result of evidence of dumping.

In particular, Friedman restated his oft-used argument that restrictions on imports into the United States would tend to deliver the visible retention of jobs in specific sectors of the home economy, but their counterpart was the dispersed—and so, in effect, invisible—losses of jobs in the rest of the U.S. economy.[57] *Aggregate* employment would consequently not be boosted, on net. Friedman added that restricting imports actually lowered overall living standards: it lowered the home country’s potential output, by making employment less productive and by worsening the terms of trade.

With regard to trade policy generally, the Carter Administration prided itself on the progress it made in negotiating reciprocal reductions in trade barriers (see Eizenstat, 2018, p. 214). Friedman, however, favored a more forthright approach. In his April 1978 speech, he expressed impatience and skepticism regarding the method of securing liberalization via negotiation between countries on reciprocal reductions in trade barriers. In so doing, Friedman echoed sentiments that he had previously expressed about this method.[58] His speech called instead for a unilateral program under which the United States would eliminate barriers to imports entirely over a five-year period—the phased nature of the reductions being designed to allow time for affected industries to adjust gradually.[59] This prescription, which again echoed those he had made in the part, would be reaffirmed in print by Rose Friedman and himself in the 1980 book version of *Free To Choose*.[60]

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[55] Although Friedman may not have known of the precedent, this article’s title had been used previously. See *Irish Times* (Dublin), August 19, 1972.
[56] A textbook discussion that likewise cautioned that “there are powerful reasons to resist the [anti-]dumping argument for protection” was Parkin (1996, p. 904).
[57] He had previously made this argument in reference to steel in his October 1977 remarks in Pittsburgh and at Utah State University shortly thereafter (*Milton Friedman Speaks*, Episode 2, “Myths That Conceal Reality,” taped October 13, 1977, p. 37 of transcript). See also Friedman (1962a, p. 72).
[58] See, for example, Friedman (1962a, p. 73) and his remarks after the completion of the so-called Kennedy Round in *Newsweek*, July 17, 1967.
[59] See *Milton Friedman Speaks*, Episode 8, “Free Trade: Producer Vs. Consumer,” taped April 27, 1978, pp. 17−18 of transcript. See also *Manhattan Mercury* (Kansas), April 27, 1978.
[60] See Friedman and Friedman (1980, pp. 50–53). For Friedman’s earlier prescriptions along the same lines, see, for example, Friedman (1958b, pp. 514−515 [pp. 89–90 of 1987 reprint]; 1962a, p. 74) and *Wall Street Journal*, April 30, 1962.

The assignment issue

Not long before his death in 2002, Rudiger Dornbusch published a retrospective on his time as a graduate student at the University of Chicago in the 1960s. Although Dornbusch's recollections did not seem to be accurate with regard to every detail, one of his pithy characterizations was accurate enough: "For Friedman [the] open economy was a short topic: flexible exchange rates—fully flexible—and free trade. What else was there to say?"[61]

Friedman's open-economy prescriptions were, indeed, straightforward. There was, however, a sense in which they were not as self-contained as Dornbusch implied, as they fit into a broader framework of prescriptions regarding policy assignments. In 1969, speaking at a high level, Friedman described the policy configuration most appropriate for the United States to achieve its macroeconomic goals as consisting of a floating exchange rate to achieve external payments balance, the use of monetary policy to secure price stability, and the setting of fiscal policy to reflect community decisions regarding the amount of resources commanded by the public sector (Instructional Dynamics Economics Cassette Tape 12, January 1969). In effect, this description amounted to an assignment of instruments—albeit one in which the exchange rate would be a market price instead of a policy instrument proper, and fiscal policy would be given little role in the control of aggregate demand.

Public statements by U.S. policy officials in 1977 and 1978 would underline the extent to which, notwithstanding the adoption of floating exchange rates and a degree of influence of monetarism on domestic policy developments, the authorities' views concerning the appropriate assignment continued to differ from Friedman's perspective.

This divergence was evident in the multi-government communiqué that came out of the May 1977 London summit.[62] Although its statement, "Inflation does not reduce unemployment," seemed compatible with Friedman's views, the conflict with his own position was revealed in its observation: "Our most urgent task is to create more jobs while continuing to reduce inflation." As was noted in a monetarist analysis of the communique in Citibank's economic newsletter, the communiqué seemed perfectly consistent with policies oriented toward management of nominal income growth Δx, and possibly also with undertaking such management by focusing on monetary growth Δm (although—as discussed in the previous chapter, as well as below—the

[61] Dornbusch (2001, p. 22).
[62] For the text of this communiqué, see, for example, *Morning Advocate* (Baton Rouge, Louisiana), May 9, 1977.

Carter Administration remained preoccupied with *fiscal* policy's role in demand management). But it was with regard to the link to the connection between the control of nominal income and achieving price stability that the communique really broke decisively with Friedman's views. The Citibank analysts observed that "it's there that the resemblance [to monetarism] ends. Inflation, according to the official thinking that animates the communiqué, is not determined by monetary policy but by such political and physical forces as frosts, earthquakes, strikes, harvests, trade-union strength, and energy prices." (*National City Bank of New York Monthly Economic Letter*, June 1977, p. 14.)

The communique had stated that inflation was "one of the major causes" of unemployment.[63] Like the similar position prevalent in the Ford Administration (see Chapters 3 and 4), this amounted to taking inflation as being—at times when output was believed to be below potential—a largely exogenous process. Inflation (π) in turn was perceived as siphoning off real purchasing power—in effect, rises in π were perceived as crowding out the real growth Δy achievable in any given rate of nominal income growth $\Delta x = \Delta y + \pi$. Friedman was willing to countenance this way of thinking about inflation when contemplating sudden price shocks, but he rejected it as a manner of viewing the longer-term behavior of inflation. And he eschewed the implied conclusion for policy that efforts should be made to reduce inflation by devices other than those involving aggregate demand and that this approach would increase immediately the scope for real growth. Instead, Friedman viewed a period of aggregate demand restriction—entailing a period of slow growth in real and nominal incomes—as being needed for inflation to be brought down, with appreciable real growth possible once inflation was defeated.

Friedman continued to be an active debunker of interpretations of inflation that listed as causes of inflation many of the nonmonetary factors that Citibank had recited. In November 1977, for example, Friedman remarked of recent years' experience: "The increase in petroleum prices has been a negligible cause of higher rates of inflation."[64] A year later, in response to the blame that President Carter and Chairman Miller had been putting on recent increases in basic food prices, Friedman replied: "Why is that people point to food prices as a cause of inflation, but I have seen

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[63] *Morning Advocate* (Baton Rouge, Louisiana), May 9, 1977. In the same vein, in January 1978, the first annual report to be issued by the Carter Administration would cast the mission of economic policy as being to "*contain and reduce the rate of inflation as we move toward a fully employed economy*… [R]educed inflation would substantially enhance our chances to maintain a strong economic expansion." (Council of Economic Advisers, 1978, p. 5; emphasis in original).

[64] *Milton Friedman Speaks*, Episode 6, "Money and Inflation," taped November 7, 1977, p. 35 of transcript.

nobody point to the sharp decline in the cost of computers... as a cause of deflation?"[65] Rather, he declared—using a formulation he had used for many years—inflation came "from Washington and nowhere else" (*Meet the Press*, NBC, November 12, 1978, p. 2 of transcript).

Wage-push and the role of unions

U.S. officials were, of course, not remiss in citing domestic sources of inflation. But, in doing so, they largely pointed away from the District of Columbia and instead cited, in particular, autonomous wage pressures across the country. In this, they were supported by Arthur Okun, who gave testimony in March 1977 suggesting that nominal wage growth and inflation were settling back to their normal relationship with each other and that the former was stuck at 8 percent.[66] As discussed in the previous chapter, President Carter was prompted by wage-push, as well as profit-push, views of inflation to introduce wage-price guidelines in April 1978. In making the case for this approach, Secretary of the Treasury Blumenthal remarked at a press conference that "we must work together" against inflation by adhering to the administration's nonmandatory incomes policy. And when articulating the need for the policy's success, he used the "π-squeezes-out-Δy" line of thinking closely associated with nonmonetary views of inflation: if "the program fails, the impact of increasing inflation on the economy… will be severe" (*Detroit Free Press*, April 13, 1978).

Although depreciation and food price movements shaped policymakers' accounts of the course of inflation in 1978, the behavior of wages remained a key part of their account of the longer-term path of inflation, with Blumenthal maintaining during the depreciation period: "We see no evidence that the inflation rate has been jolted upward more than temporarily." (*Daily News* (New York), May 13, 1978.) The following November, CEA chairman Charles Schultze argued for the guidelines as a means of reducing inflation "over a period of time," while contending that measures of this sort "had some effect in the '60s, and they have had an effect in other countries more recently—in Britain for example" (*Arizona Republic* (Phoenix), November 6, 1978).

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[65] *Meet the Press*, NBC, November 12, 1978, pp. 4–5 of transcript.

[66] See the written portion of Okun's testimony of March 16, 1977, in Committee on the Budget, U.S. Senate (1977, p. 91). See also the spoken portion of Okun's testimony of April 6, 1977, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1977c, p. 13), in which he repeated this claim while adding that "prices are closely geared to direct costs plus a percentage markup." As indicated in previous chapters, the Federal Reserve leadership had endorsed wage-push theories of inflation for several years—a trend continued by Chairman G. William Miller in 1978 (see Miller, 1978c).

The previous chapter detailed the derisory comments Friedman had regarding, first, James Tobin's urging in 1977 of new wage-price guidelines and, then, the administration's introduction of guidelines in 1978. Just as these years saw Friedman reject direct measures to restrain nominal wage growth as a means of fighting inflation, they also saw him reaffirm that increases in nominal wages were not a source of inflation. Governments were not taking responsibility for inflation when they said that it was "produced by grasping unions," he remarked in a November 1977 speech and, similarly, in a *Newsweek* column the following April, he criticized the tendency to blame inflation on "labor-union intransigence."[67]

In rebutting these interpretations of the inflation process, Friedman highlighted a couple of aspects of wage and price determination. For one thing, as discussed in Chapter 3 above, Friedman stressed that nominal wage growth and inflation need not move together for prolonged periods and that monetary policy's effect operated partially via its influence on variations in the markup. For another thing, he stressed the endogeneity of nominal wage growth and, in particular, its dependence on aggregate demand. As in previous decades, he remained skeptical about the scope for U.S. labor unions to raise aggregate nominal wages, rather than change the division of wages across industries. In a talk on the labor market in September 1977, he affirmed that "the effect of the higher pay of [members of] unions is lower pay for all other workers," arguing that subsequent studies had been in line with his findings to this effect in the research he had done in the 1950s.[68] And it was still the case that those unions that did possess significant market power faced economic limits: "even the powerful trade unions have to recognize the facts of life… The belief that somehow unions have infinite power to impose their will is a myth." (*San Jose Mercury News* (California), February 13, 1979.)

Having exempted the unions from responsibility for inflation, however, Friedman also cast doubt on some of the benefits unions claimed for themselves. "What protects the worker is not the union. What protects the worker is the existence of alternative sources of employment. The true protection [provided for] the American worker has been a vigorous private enterprise system in which the great number of workers have more than one potential employer," Friedman remarked in an interview for a 1978 television documentary on labor unions.[69] From the perspective of

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[67] The quotations are, respectively, from *Milton Friedman Speaks*, Episode 6, "Money and Inflation," taped November 7, 1977, p. 3 of transcript, and *Newsweek*, April 24, 1978.

[68] *Milton Friedman Speaks*, Episode 13, "Who Protects the Worker?," taped September 29, 1977, p. 7 of transcript. With regard to the earlier research, see Friedman (1951b) and the discussion in Nelson (2009b, p. 478; 2020a, Chapter 10).

[69] *In Search of the Real America: The State of the Unions*, WGBH Boston, June 12, 1978, p. 10 of transcript.

unions, this was obviously a far less palatable conclusion than Friedman's message about unions and inflation. However, it was not too different a perspective on the labor market from that expressed by Christina Romer thirty-five years later: "basic economics shows that competition between employers for workers can be very effective at preventing businesses from misbehaving." (*New York Times*, March 3, 2013.)

Prior to the 1970s, Friedman had also endorsed broadening antitrust legislation to cover the behavior of unions and not just of firms.[70] His various statements over the years about unions' limited power indicated that he, nevertheless, saw considerable limits on the degree to which antitrust legislation could be relevant to unions. For the most part, Friedman did not consider an individual union to be a monopoly. He opposed laws keeping workers from joining a union or collectively bargaining with the employer as a union.[71] Furthermore, in an economy in which all workers were unionized, he did not believe that this generated a condition of monopoly, provided that there were multiple unions: "If all workers were unionized, then it would be a fight of one union against the other."[72]

What emerges from these various Friedman remarks on the labor market is that the antitrust laws regarding unions he had in mind were ones that would prevent actions taken by unions to combine in the exercise of their power: for example, via the pooling union strength into a peak union body that could direct industry-wide or economy-wide action across industries covered by different unions. And as indicated in Chapter 5 above, after 1973 Friedman was not in favor of wide-ranging antritrust laws. Perforce, his earlier recommendation that antritrust be widened to cover unions had now been superseded.

In this new context, Friedman's position regarding unions was that "I am only arguing against governmental immunities and special privileges."[73] In particular, both before and after his change in position regarding antitrust, Friedman opposed government laws or regulations that strengthened unions *vis a vis* the rest of the workforce—for example, by requiring union membership, or by insisting that firms that were in government contracts hire labor for the contracted project exclusively from unions. He preferred an approach in which the public sector

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[70] *Speaking Freely,* WNBC, May 4, 1969, p. 16 of transcript. See also Friedman (1952, p. 7; 1962a, p. 132). See also Instructional Dynamics Economics Cassette Tapes 37 (November 5, 1969) and 71 (April 7, 1971).
[71] See, for example, his remarks in *The Jay Interview*, ITN, July 17, 1976.
[72] *In Search of the Real America: The State of the Unions*, WGBH Boston, June 12, 1978, p. 11 of transcript.
[73] *In Search of the Real America: The State of the Unions*, WGBH Boston, June 12, 1978, p. 13 of transcript.

took a neutral role regarding the relationship between private-sector employers and employees.[74] In addition to raising objections to laws that specifically favored unions, Friedman had suggested in his 1950s and 1960s writings that damage to property or other violence that arose in protests by unions did not necessarily give rise to government prosecution when such prosecution would arise if comparable actions arose in other contexts.[75] He reaffirmed his concern about "differential enforcement of the ordinary laws" in this connection in his 1970s discussions (*The Jay Interview*, ITN, July 17, 1976). The example he gave in 1977 was that of a car being overturned: this did not seem to give rise to prosecutions in cases when it arose from union members' actions in the heat of a dispute with employers.[76] Friedman also criticized cases in which the government tacitly sanctioned, if only through inaction, the practice of unions taking disputes beyond the domain of the affected firm, particularly through the device of mass picketing. Correspondingly, he had praised President Ford for vetoing of a bill that would have put restrictions on how far-reaching union picketing could be (*Newsweek*, January 19, 1976).[77]

Friedman was, however, opposed to laws sometimes introduced, particularly by governments in U.S. states, to try to outlaw the "closed shop"—that is, the practice in which a firm agreed to an arrangement in which all its employees were required to join a specific union. He indicated in the 1960s that the closed shop emerged from a voluntary agreement and should be legal.[78] Friedman reaffirmed this position in 1977, while adding the proviso that the closed shop should not arise from, or be enforced by, government edict.[79]

"*Capitalism*"

Of course, to the most politically radical and self-proclaimed proponents of workers, labor's

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[74] For documentation of this and the previous sentence in the text, see, for example, *Milton Friedman Speaks*, Episode 13, "Who Protects the Worker?," taped September 29, 1977, pp. 13, 21 of transcript.
[75] See Friedman (1951b, p. 215), in a passage reproduced in Friedman (1962c, p. 161;1976b, p. 165).
[76] *Milton Friedman Speaks*, Episode 13, "Who Protects the Worker?," taped September 29, 1977, p. 14 of transcript. Friedman had given the same example in *Speaking Freely,* WNBC, May 4, 1969 (p. 16 of transcript), Instructional Dynamics Economics Cassette Tape 182 (early December 1975), and *The Jay Interview*, ITN, July 17, 1976.
[77] See also Friedman's remarks on *CBS Morning News*, CBS, January 9, 1976, p. 23 of transcript. For his remarks on mass picketing, see Friedman (1951b, p. 215), in the passage reproduced in Friedman (1962c, p. 161;1976b, p. 165). In addition, in 1976, Friedman remarked: "I don't believe it [collective bargaining] is [a restraint of trade] at all. If a group of workers get together and they would like to have some single person bargain for them, that's not an act in restraint of trade. What's in restraint of trade is when they set up pickets and prevent other people from going to work." (*The Jay Interview*, ITN, July 17, 1976.)
[78] Friedman (1962a, p. 116). In the same vein, conservative columnist John Chamberlain (*The Meriden Journal* (Connecticut), June 3, 1965) and Allan Meltzer (2004, p. 198) implied that Friedman's stance on the labor market most likely included opposition to "right to work," or anti-closed-shop, legislation.
[79] *Milton Friedman Speaks*, Episode 13, "Who Protects the Worker?," taped September 29, 1977, p. 34 of transcript.

interests would not be well served either by market forces, as Friedman suggested, or by strong unions, but by the displacement of the market system itself. This sentiment found expression in a mainstream U.S. economic journal when Leonard Rapping—a one-time member of Friedman's Price Theory class who, in his own account "became a Friedmanite" and whose research had defended and developed natural-rate ideas, but who subsequently described himself as having "jettisoned Chicago economics"—was asked to contribute a paper to the *American Economic Review*.[80] The tone of this self-labeled "radical critique" was set in its first sentence: "Capitalism by its very nature is an exploitative system." (Crotty and Rapping, 1975, p. 791.)

Despite the sharp divergence in viewpoints that had emerged between the two of them by 1977, one thing Friedman and Rapping continued to have in common was a willingness to use the term "capitalism." During 1977, Friedman used the term "capitalism" in the title of two major talks: a setpiece speech titled "The Future of Capitalism," delivered at both the University of Reno, Nevada, and Pepperdine University during the first half of February, and one on "Is Capitalism Humane?" on September 27 at Cornell University.[81] And, of course, fifteen years earlier, he had published a book titled *Capitalism and Freedom*.

It is worth looking into the apparent alacrity with which Friedman—on these occasions at least—used the term "capitalism."[82] Thygesen (1977, p. 82) suggested that the title *Capitalism and Freedom* was provocative. This might appear surprising, as the title of *Capitalism and Freedom* certainly seems less provocative—in the sense of embedding a particular point of view—than the title of Hayek's *The Road to Serfdom* was in 1944 or is today. The reason the title might have seemed provocative in 1962 is that it signified the endorsement *by an advocate of free markets* of the term "capitalism."[83] That term had been promulgated by the Communist or Marxist political and economic literature, and in the 1950s—as well as subsequently, as the above quotation from Crotty and Rapping (1975) brings out—it had tended to be used in the West by critics of the market system and by advocates of greater government intervention (among them non-

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[80] The quotations are from Rapping's May 1982 remarks in Klamer (1984, pp. 220, 227).

[81] For various records of these speeches, see Friedman (1977f, 1977l, 1978b, 1983b) and *Milton Friedman Speaks*, Episode 3, "Is Capitalism Humane?," taped September 27, 1977. See also the discussion in Chapter 7 above.

[82] This discussion of the term "capitalism" is an expanded version of one that originally appeared in the drafts available publicly in 2015–2019 of Nelson (2020b, Chapter 11).

[83] Bennett McCallum wrote in an email to the present author (August 18, 2008): "I think that it is possibly true that free-market supporters avoided the term 'capitalism.' In fact, I still think of it as a rather poor term. Free markets help everyone (on average), not primarily capitalists."

Communist critics such as social democrats).[84]

In light of this background of past usage, many Western commentators in the early postwar decades evidenced reservations about using the term "capitalism," seeing it as a concession to the anti-market side. For example, Hayek (1944, p. 77) had treated the term as a label that critics of the market used.[85] And when, in the early 1970s, Soviet commentaries on Western society began avoiding the term "imperialist" and also started to take "capitalist" out of rotation in favor of references to the "other social system," this was seen as the adoption of more-measured language on the part of the USSR (Leites, 1973, p. 34).

Friedman himself was aware of the problem of adopting Soviet terminology. In 1957, in reference to the use of economic-planning terminology in the United States, he observed that it was "another one of those terms we have borrowed from the Communists."[86] Indeed, in 1947, in describing non-planned economies, he had opted for the term "free-enterprise exchange economy." And his 1952 statement of position was called "Free Enterprise in the United States" rather than "Capitalism in the United States."[87]

Friedman nevertheless warmed to the term "capitalism" during the 1950s. In a 1953 radio appearance, he said that it was used as both a hopeful and a pejorative word (NBC, 1953, p. 1). In 1954, he made a facetious reference to the usage of "capitalist" as a negative term.[88] And in 1958, as discussed in Nelson (2020a, Chapter 10), Friedman used "Capitalism and Freedom" as the title of a book chapter he contributed.[89]

Too much significance should not, however, be read into Friedman's choice of the title

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[84] The term "capitalist" may, however, have been in greater usage in mainstream economics literature in the United Kingdom than in the United States. For example, R.J. Ball (1964, p. 177) referred to the "developed capitalist economies" of the Western world, and Kavanagh and Walters (1966, p. 94) to the "capitalist countries." To some extent, the term was accepted also in the U.S. economic literature, too. For example, Phillips, McManus, and Nelson (1937, p. 120) referred to the "capitalistic process of production." As discussed presently, one of the contexts in which the term was in more wide use was in referring to firms' capital-raising rather than to the market system as a whole.

[85] However, Hayek later edited a book titled *Capitalism and the Historians* (Hayek, 1954).

[86] From Friedman's March 26, 1957, testimony, in Special Committee to Study the Foreign Aid Program (1957, p. 129).

[87] See Friedman (1947, p. 407; also 1955c, p. 362) and Friedman (1952).

[88] Specifically, in a BBC radio appearance (September 29, 1954, p. 11 of transcript), Friedman referred to "the good old days of so-called capitalist exploitation."

[89] Friedman (1958c). Nonetheless, in another publication the same year, Friedman also opted for the terminology "free enterprise market system" (Friedman, 1958d, p. 22).

Capitalism and Freedom. One pro-market book, of which Friedman was aware by the early 1960s, had used "capitalism" in the title as far back as 1940.[90] Furthermore, in the summer of 2008 Anna Schwartz suggested to the present author that the overriding consideration behind the title *Capitalism and Freedom* was the need for a catchy and descriptive title. The use of the term "capitalism" in economic analysis also had a University of Chicago pedigree. For example, Frank Knight, although he had once observed that Marx "made capitalism a household word" (Knight, 1928, p. 119), himself used the term frequently.

It is, nevertheless, noteworthy that Friedman did not, on the whole, regard "capitalism" as a term to be shunned. Claire Friedland, who was located at the University of Chicago (first as a graduate student and subsequently as a collaborator on research with George Stigler) in the years in which Friedman composed and finalized *Capitalism and Freedom*, remarked that "of course, what he really meant was 'free markets,' or the system in which the entrepreneur is the important actor. And somebody might have said to him, 'Capitalism has a bad name.' And he might have said, 'I don't care. That's what we call it.'" (Claire Friedland, interview, October 27, 2014.) Friedman's employment of the term "capitalism" in his book's title and text, combined with the eventual success of his book—its cumulative sales making it one of the University of Chicago Press' highest-selling titles—helped shift the term's status from one that free-market advocates eschewed to one that many of them embraced.[91]

It is worth recording, however, that Friedman nonetheless displayed some backtracking after 1962 in his use of the term "capitalism." In 1965, for example, he instead deployed the term "free competitive enterprise system."[92] A *Newsweek* column later in the decade (June 24, 1968) used "free markets and private enterprise" rather than "capitalism," and Friedman referred on another occasion to the "free enterprise market mechanism" (Instructional Dynamics Economics Cassette Tape 2, November 1968). Although Friedman referred to the "capitalist system" in the early 1970s, he did so partly in the narrow context of describing the manner in which firms raised funds for investment, not in the broad sense of the market economic system.[93]

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[90] This was Carl Snyder's *Capitalism the Creator* (1940) which Friedman and Schwartz (1963a, pp. 413, 692) cited, and which, like *Capitalism and Freedom*, laid out a monetary interpretation of the Depression.
[91] A more recent example of a free-market advocate's acceptance of the term "capitalism" was Meltzer (2012).
[92] Friedman (1965b, p. 7). Rose Friedman also used this term (see R.D. Friedman, 1976, p. 29). Other terms Friedman used included "free enterprise systems" (Friedman, 1961d, p. 534), "free enterprise/competitive enterprise system" (Instructional Dynamics Economics Cassette Tape 70, March 29, 1971), "private enterprise exchange economies" (Friedman, 1976b, p. 6), and "market system" (*St. Louis Globe-Democrat*, December 9, 1965).
[93] See Instructional Dynamics Economics Cassette Tape 72 (April 21, 1971). This usage of the term "capitalist" is also prevalent in the economic-research literature. See, for example, Woodford (1988).

Friedman's ambivalence about the term "capitalism" emerged again when in a debate with Irving Kristol in the mid-1970s. Replying to Kristol, Friedman referred to "the system… that he describes as capitalism and I describe as free enterprise."[94] And even though Friedman's 1977–1978 speeches noted above prominently used the term "capitalism," it was the case that, subsequently, "capitalist" and "capitalism" appeared only infrequently in the book version of *Free To Choose* in 1980.[95] That said, the early 1980s seem to have ushered in another period in which Friedman seemed comfortable with the using the term "capitalism," much as he had been twenty years earlier.[96]

Capitalism and imperfect competition

Occasions on which Friedman appeared comfortable with the term "capitalism" or "capitalist" included those when he contrasted market arrangements with socialist arrangements. A prominent example of this was his September 1977 "Is Capitalism Humane?" speech noted above.[97]

Friedman did have one major source of hesitancy in using the term "capitalism" in making such contrasts—a reservation he noted in his 1977 talk on "The Future of Capitalism." He was concerned that the word "capitalism" was not particularly useful in singling out properties

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[94] From Friedman's remarks in Friedman and Kristol (1976, p. 22).

[95] Friedman *had* used the term liberally in "Capitalism and the Jews," a piece that, like *Capitalism and Freedom*, likely had a deliberately provocative title. Friedman gave this paper in lecture form as "Capitalism and the Jew" at the Washington Hebrew Congregation in Washington, D.C., on December 11, 1966, and as "Capitalism and the Minority Groups" at the Twelfth Annual Park Synagogue Forum in Cleveland in March 1971 (*Evening Star* (Washington, D.C.), October 22, 1966; *Cleveland Press*, March 8, 1971). (The existence of these early versions indicates that the dating by Vallois and Chassonnery-Zaïgouche, 2021, dating of the Friedman's iterations on the speech as starting in 1972 is not warranted.) He then presented a revision, with the "Capitalism and the Jews" title, at the Mont Pelerin Society meeting of 1972 (see Rose Friedman's remarks in Friedman and Friedman, 1998, p. 335), as a lecture at the University of Chicago in October 1976, and to the Fraser Institute in Canada in August 1982, before publishing a little-revised version in 1984 (Friedman, 1984e; see also Friedman, 1985b).
In the talk, Friedman contended that Jewish culture, in the face of stereotypes linking Judaism with business success, had tended to react to the stereotypes by generating intellectual traditions that he felt were unduly hostile to capitalism. Friedman's analysis of the historical and cultural issues involved was, however, criticized as superficial by conference participants on a number of the occasions on which he gave the talk (see, for example, Frankel, 1983, 1985, and Friedman's own account of the reaction in Block, Brennan, and Elzinga, 1985, p. 459, as well as his remarks in Friedman, 1985c).

[96] See especially Friedman's (1984a, p. 47) matter-of-fact reference to "the United States, Britain, or other capitalist countries." See also his other remarks given around the same period in *California* magazine (October 1984, p. 79) and his endorsement of Brookes (1982).

[97] Other discussions in which Friedman used "capitalist" or "capitalism" in reference to the market system when juxtaposing it against planned-economy arrangements included Instructional Dynamics Economics Cassette Tape 58 (October 4, 1970), and *Newsweek*, March 5, 1979.

special to a market economy—as the operation of planned economies, too, involved accumulation of capital.[98] Subject to that qualification, however, Friedman did regard "capitalism" as a useful catch-all term for the market economic system when contrasting it with centrally planned economies.

In particular, Friedman was willing to use "capitalism" as a term for the market system as it existed in practice. He stressed that, in reality, any market system observed in practice was really an approximation to an idealized conception of capitalism, because a sizable state sector existed in any real-world system.[99] Nor did he regard capitalism and perfect competition as necessarily synonymous. Although Leonard Rapping would imply that some 1960s University of Chicago teachers and graduates had come to take perfect competition as applying literally to the U.S. economy (see Klamer, 1984, p. 221), he did not specifically apply this criticism to Friedman. And, indeed, it did *not* apply to Friedman, as stressed in Nelson (2020a, Chapter 9). Friedman was not under any illusions that conditions of perfect competition well described actual market behavior right across the U.S. economy. But he also stressed that the elements of imperfect competition seen in practice were amenable to analysis using the standard tools of economics. He also underlined that the advanced economies did exhibit to a large degree the key theoretical features of a market system, including resource allocation and decisions based on a freely operating price system, private property, and voluntary exchange.[100]

Friedman remarked in an interview in June 1978 that economists' understanding of imperfect competition had advanced in recent years—specifically through improvements in the analysis of the operation of cartels.[101] He was probably referring to research he had seen regarding the decision-making process of the OPEC cartel. Nevertheless, his observation had more general application, because the years leading up to Friedman's remark had been a vibrant period for research on imperfect competition. Two items published in 1977 are particularly notable in this connection, though they may not have actually especially registered with Friedman at the time.

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[98] See *Milton Friedman Speaks*, Episode 3, "Is Capitalism Humane?," taped September 27, 1977 (p. 22 of transcript) and Friedman (1978b, p. 1). Much earlier, in April 1966 remarks (Friedman, 1966c, p. 33 of 1970 printing), Friedman had cited the same grounds as the basis for his contention that "capitalism" was a "highly misleading" term.

[99] See *Milton Friedman Speaks*, Episode 3, "Is Capitalism Humane?," taped September 27, 1977, pp. 14–15 of transcript, and Friedman (1983b, p. 87).

[100] See, for example, *Milton Friedman Speaks*, Episode 3, "Is Capitalism Humane?," taped September 27, 1977, pp. 5–6 and 14–15 of transcript, and Friedman (1983b, pp. 85–87).

[101] *Human Behavior*, November 1978, p. 31. (The interview was conducted on June 26, 1978, the day after the Friedmans' fortieth wedding anniversary. See *Human Behavior*, November 1978, p. 30.)

The first was Dixit and Stiglitz's article in the June 1977 issue of the *American Economic Review* article developing and further formalizing monopolistic-competition models. In the same journal just three months earlier, Franco Modigliani (1977, p. 6) had asserted that Friedman's 1968 exposition of the natural rate hypothesis had required a perfect-competition, one-good model, as well as a homogeneous labor input. Presumably Modigliani believed that implicit assumptions of this kind were necessary in order to Friedman to carry out the consideration of aggregate goods prices (and aggregate wages) in which he engaged. In discussing Modigliani's paper early in 1977 (see the previous chapter), Friedman had rejected the notion that his analysis implied or required a single good, homogeneous labor, or perfect competition. Dixit and Stiglitz (1977) in effect provided a framework that definitively refuted Modigliani's contention. They showed that a model in which there was a multiplicity of firms, each producing a different good and possessing some pricing power over their product, was amenable to an aggregate model in which there was a single, composite good. Subsequently, in examining the implied imperfect-competition macroeconomic models, Blanchard and Kiyotaki (1987) showed that the natural rate property prevails in both the short run and the long run of the flexible-price version of these models, as well as in the long run of the sticky-price version.[102]

A second important contribution on imperfect competition in 1977 was the monograph *Oligopoly and the Theory of Games*. Although the book restricted itself to theoretical analysis, its applicability to the analysis of the world oil price was not lost on readers, and some later research on OPEC (for example, Marshalla and Nesbitt, 1986, pp. 2, 7, 21) would have occasion to cite the book for its formal analysis of cartels while also quoting from Milton Friedman's popular writings on the first oil shock.

Oligopoly and the Theory of Games, published by North Holland, had been written by James W. Friedman. James Friedman, no relation to Milton Friedman and twenty-six years younger than him, had followed doctoral studies in economics at Yale University with five years (1963–1968) as an assistant professor in the department.[103] James Friedman was therefore located at Yale University during the period in which the macroeconomic debate between James Tobin and Milton Friedman took off, and he happened to possess a combination of their names to boot. As James Friedman was a microeconomist, one might expect that the link between James Friedman and the Tobin-Milton Friedman debates was limited to his Yale University background and the

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[102] See also Erceg, Henderson, and Levin (2000) for a generalization of this approach that featured Dixit-Stiglitz aggregates of heterogeneous supplies of labor input.
[103] See American Economic Association (1981, p. 152) and Blaug (1986, p. 290).

coincidence of names.[104] This, however, proved not to be the case. From 1977 onward, James Friedman would be repeatedly cited in the literature that pursued the themes that had featured in the Milton Friedman-James Tobin debates up to that point: on whether fiscal policy was powerful, on the case for policy rules, and on natural-rate models of inflation and real economic activity. What is more, the authors tending to cite James Friedman's name would be on the side of the debate that was adverse to Tobin's economics. For his part, Milton Friedman, largely out of the research debate by this point and not well versed in its technical side, would never cite James Friedman. But James Friedman's work was invoked by several other, newer-generation macroeconomists, who in important respects carried on the pro-monetary rules, pro-natural-rate-hypothesis, and anti-fiscal-activist positions that Milton Friedman had championed.

In particular, a celebrated paper on the intertemporal analysis of policy rules, "A Positive Theory of Monetary Policy in a Natural Rate Model," by Barro and Gordon (1983a), as well as the authors' (1983b) companion paper, contained no Milton Friedman citation, but both papers cited J.W. Friedman (1971) because of its application of dynamic game theory to the notion of time consistency.[105] In addition, Barro and James Friedman, who were colleagues in the economics department at the University of Rochester for several years, wrote a paper together in 1977 that generalized the Ricardian-equivalence proposition concerning fiscal policy.[106]

The Barro-J.W. Friedman paper, appearing in the August 1977 issue of the *Journal of Political Economy*, was immediately followed in the same issue by an article on a similar subject. This abstract of this article (Mohabbat and Simos, 1977, p. 851) referred to "Friedman's contention" and indicated that the results presented would "overwhelmingly support Friedman." These formulations underlined the fact that—to many in macroeconomics in this era—"Friedman"

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[104] James Friedman's Yale University period did, however, involve an affiliation with the Cowles Foundation. The foundation's seminars during this era were regularly attended by all affiliates even when the seminar topic was outside their field (William Brainard, personal communication, February 25, 2021). Of the Cowles Foundation seminars that James Friedman likely attended in the period 1959−1968, the main two that concerned topics associated with Milton Friedman's research were one given by A.W. Phillips (on stabilization policy) on February 28, 1963, and one given by Allan Meltzer (on money, credit, and interest rates) on January 22, 1965. See *https://cowles.yale.edu/cf-seminars*. There were also some other talks concerned with the key monetary and financial issues facing specific countries.

[105] In a similar manner, Blanchard and Fischer (1989a, p. 621) cited J.W. Friedman (1977) on time consistency, while Canzoneri and Henderson's (1991) book-length study of monetary policy contained multiple James Friedman citations and none of Milton Friedman (who was, nevertheless, implicitly acknowledged via the authors' practice of referring to James Friedman by his full name in the text—see their page 4). In addition, because Milton Friedman had his own body of work in price theory, the two would sometimes both receive citations in microeconomics texts. For example, Cowell (2006, p. 477) cited J.W. Friedman (1971) and Friedman and Savage (1948).

[106] James Friedman was at the University of Rochester from 1968 to 1983 (Blaug, 1986, p. 290), a period encompassing Barro's first spell at the institution (1975 to 1982).

could only mean one person, and that person was Milton Friedman. In this atmosphere, it is not surprising that the uptick of citations, in macroeconomic contexts, of James Friedman's work would on occasion see him confused with Milton Friedman. For example, in Currie and Levine's (1993) book on macroeconomic policy coordination and rules, James Friedman was cited in the text (p. 251), but this mention showed up in the book's index entry for Milton Friedman (p. 423). The confounding of names also sometimes went the other way, with the bibliography in a chapter of Thomas Sargent's *Macroeconomic Theory* text referring (1987, p. 131) to "Friedman, J. (1968) 'The Role of Monetary Policy.'"

The strong attention given to James Friedman in both the microeconomic and macroeconomic literatures was reflected in his inclusion in the second edition of *Who's Who in Economics* in 1986. In contrast to the first edition, in which he had not appeared, James Friedman now received an entry—one appearing right ahead of Milton Friedman's.[107] But it can also be discerned that by the mid-1980s—perhaps in view of the proliferation of references to him in the macroeconomic articles appearing on what could broadly be considered the "Milton Friedman side" of debates on monetary and fiscal policy—James Friedman had decided to call time on the drawing of connections between himself and Milton Friedman. James Friedman published a new monograph, *Oligopoly Theory*, in 1983.[108] In the opening pages of the book, James Friedman lashed out at his near-namesake. After contrasting Milton Friedman's views on economic-research methodology with those of Tjalling Koopmans, James Friedman declared: "The reader is entitled to know that my view is more closely aligned with that of Koopmans."[109]

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[107] See Blaug (1986, pp. 290–291).

[108] The new James Friedman book appeared just as he was starting a new position as a professor of economics at Virginia Polytechnic Institute (see Blaug, 1986, p. 290). This was an institution in which Milton Friedman's son David had served as an assistant professor in the period from 1976 to 1980 or 1981 (see American Economic Association, 1981, p. 152, and *http://www.daviddfriedman.com/Academic/Academic.html*). David Friedman had received a Ph.D. in physics in 1971 from the University of Chicago and had then been a research associate in physics in 1971–1973 at Columbia University. However, as noted in Chapter 5 above, he had written a popular book in economics in 1973—one Milton Friedman in June 1978 described as "a damned good defense of that [near-zero-government] position" (*Human Behavior*, November 1978, pp. 31–32)—and, at around the same time, moved into teaching economics. Rose Friedman judged of herself and her husband that "we made a mistake" in urging on David Friedman to pursue a career in a field other than economics: "we brainwashed him; we weren't very subtle." She confirmed that upon David Friedman's move into economics, "he has such enthusiasm for his work now," while adding: "He's a wonderful teacher, just like his father." (*The Anchorage Times* (Alaska), June 10, 1977.)

[109] J.W. Friedman (1983, p. 16). Earlier, with regard specifically to microeconomics, James Friedman had exhibited signs of criticism of Milton Friedman's outlook. Although Milton Friedman often likened information generated by historical experience to the outcomes of natural experiments, he was skeptical of the feasibility of true (laboratory-type) experiments as economic-research tools, even in microeconomics. For example, Wallis and Friedman (1942, p. 181) had declared that "it is probably not possible to design a satisfactory experiment for deriving indifference curves," while Friedman (1955a, p. 908) had referred to "the difficulty of making experiments in economics." Twenty-five years after the latter remark, James Friedman opened a book review in the *Journal of Political*

Nevertheless, James Friedman had shown notable affinity with Milton Friedman, including on methodological matters, in the aforementioned Barro and J.W. Friedman (1977) article, particularly in its concluding section (pp. 848–849) titled "Human Capital." The general subject matter of that section was in itself closely associated with Milton Friedman. But the detailed content of the section had the effect of deepening the connection to Milton Friedman's work. Specifically, Barro and James Friedman treated the stock of human capital in the economy as capable of being represented in macroeconomic analysis by a single aggregate—one that could be included in the economy's production function. As will now be discussed, simply by accepting the notion of an aggregate stock of capital—whether composed of human capital or machinery—this paper was part of a tradition that implicitly supported a key aspect of Milton Friedman's methodological position. And, in taking this stand, the authors were also, in effect, taking sides (and taking the same side as Milton Friedman) in one of the more heated debates on aggregate-supply issues to take place in economic research in the 1960s and 1970s.

The capital controversy

The year 1977 saw a new research journal appear in the United Kingdom: the *Cambridge Journal of Economics*. At the time of its creation, this journal was strongly associated with Cambridge University's tradition of harder-line Keynesian economics, as expounded especially in the postwar period by Joan Robinson and Nicholas Kaldor. Francis Cripps recalled of Robinson's attitude during the 1970s toward younger-generation figures at the university, like himself and Wynne Godley: "she was very supportive… She kind of wanted to believe that the new generation would carry on the tradition and carry on the fight." (Francis Cripps, interview, January 22, 2015.)

To subscribers to this tradition, the "fight" was both against monetarists and against mainstream American Keynesians of the Paul Samuelson type. Reflecting this attitude, the inaugural year of the *Cambridge Journal of Economics* included both an article by Cripps (1977), challenging Friedman's monetarist propositions, and one on stabilization policy by Robinson and Wilkinson

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Economy by observing, "Most of us have encountered the remark that economics is not an experimental discipline," while going on to contend that there was "an active tradition going back more than three decades which belies this assertion." (J.W. Friedman, 1980, p. 1064.) He later noted that his own "[e]arly work centered on laboratory experimentation, which I believe to be a neglected, [and] potentially very illuminating[,] method of [carrying out] empirical investigations." (Blaug, 1986, p. 291.) In May 2010, James Friedman attended a special seminar on the making of experimental economics, and in one exchange he built on comments made by Alvin Roth regarding the spur that scorn toward experimental economics had given to the new field. Roth had referred specifically to the "negative impetus" generated by Wallis and Friedman (1942). See Svorenčík and Maas (2016, p. 88).

(1977), in which the authors took Paul Samuelson to task for having promoted what they saw as a watered-down version of Keynesianism (making him, as they put it, one of the "bastard Keynesians").[110]

The criticism given in the *Cambridge Journal* of Samuelson, however, extended beyond stabilization-policy matters. The journal's first year of articles also included a critique by Pasinetti (1977) of Samuelson's contributions to capital theory. Pasinetti's article was contributing to a debate that, by 1977, was of long standing. The debate was widely known as the "Cambridge-vs.-Cambridge" debate, owing to the fact that it arrayed Cambridge University against MIT, whose location was of course in the Greater Boston-area city of Cambridge. The debate specifically pitted MIT economists—including Samuelson, Robert Solow, and Franco Modigliani—against leading figures in Cambridge University's economics world—among them older-generation Keynesians such as Robinson and Kaldor.[111]

In the United States, the debate particularly occupied Paul Samuelson. This had been especially the case during the 1960s, and his concern with the debate showed though in some of the revised editions of his undergraduate textbook. Samuelson's text would eventually contain material that, in the judgment of Gregory Mankiw, amounted to "nothing that you would ever want to teach to a freshman," and that reflected Samuelson's research interests at his time of writing (Gregory Mankiw, interview, September 24, 2013). In this connection, Mankiw pointed to Samuelson's coverage of the "reswitching" controversy—"reswitching" being an alleged property of the production function that was given considerable attention in the Cambridge-versus-Cambridge debate. One example of Samuelson's textbook coverage of reswitching is provided by the eighth edition of Samuelson's textbook—the first edition for the 1970s—in which Samuelson's discussion included a subsection titled "Alternative Theories: Repudiation of Aggregate Production Functions" (Samuelson, 1970, p. 734).[112]

"Of course, Paul Samuelson was fighting on two fronts," Christopher Bliss—watching the debate from Cambridge University—observed with regard to developments in the second half of the

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[110] See Robinson and Wilkinson (1977, p. 7). Of course, Robinson had used this pejorative term before 1977, too.
[111] Among the contributions to the debate were the Solow and Samuelson contributions discussed below as well as Samuelson and Modigliani (1966). For overviews of the debate, see, for example, Blaug (1975), Bliss (2010), Burmeister (1980), and Harcourt (1972). The more recent retrospective by Baqaee and Farhi (2018) is notable for casting the debate, particularly its coverage of aggregate production function, in terms of implications for the construction and specification of modern macroeconomic models.
[112] In addition to this subsection, Samuelson included a discussion of reswitching on page 590 of his text, and it was the latter discussion that was referenced in the book's index entry on "reswitching."

1960s. "He was fighting, on the one hand, across the Atlantic against Cambridge, England, on the production process; and he was also fighting on his back against Milton Friedman from Chicago. And I think, probably, that the second battle was the one he cared most deeply about." (Christopher Bliss, interview, January 2, 2015.)[113] Of the two debates, however, the Cambridge-vs.-Cambridge debate accounted for a greater proportion of Samuelson's research activities, and he acknowledged that he spent much time "pondering over the economic ideas of Joan Robinson and Nicholas Kaldor at Cambridge University" (*Boston Globe*, February 26, 1967).

By 1977, Milton Friedman was fighting on two fronts, too. He had, of course, been engaged in debate with MIT-based economists for several years. With regard to Cambridge University economists, his relationship had evolved over the previous quarter-century: from disagreement coupled with collegial relations in the 1953−1966 period, to more clear-cut acrimony thereafter.

After he had visited Cambridge University in the 1953/1954 year, Friedman had served, from 1956 to 1966, as coeditor of the Cambridge University Press' flagship line of *Cambridge Economic Handbook* monographs. In conferring on Friedman this position—which was once held by Keynes himself, and which Frank Hahn would fill in the 1972−1982 period—the university's economists may have regarded the appointment of an American as a coeditor as a necessary recognition of the postwar shift of the center of economic research from the United Kingdom to the United States.[114] In this connection, Friedman and his fellow editor themselves referred to the "change to Anglo-American editorship."[115] Furthermore, during Friedman's tenure the U.S. editions of contributions to the *Handbook* were published by The University of Chicago Press, with a house advertisement describing the series as one that "brings the best minds in American as well as British economics to focus on the most crucial theoretical aspects of economic science."[116]

A factor that had likely worked in favor in Friedman's attainment of this position was his admiration for Alfred Marshall. For Friedman's coeditor on the series, C.W. Guillebaud, was Marshall's nephew. In his writings of the late 1930s, Guillebaud defended Marshall from

[113] Samuelson himself observed in mid-1969: "The central issue that is debated these days in connection with macroeconomics is the doctrine of monetarism." (Samuelson, 1969, p. 7.)
[114] Friedman himself would discuss this shift on a number of occasions, including in Instructional Dynamics Economics Cassette Tape 17 (March 1969). A list of the successive editors of the *Handbook* series appeared in Gale (1982, p. ii). A difference between Friedman's tenure and that of Keynes and Hahn was that each of the latter two served as sole editor, rather than coeditor.
[115] Guillebard and Friedman (1957, p. vi).
[116] See University of Chicago Press (1964).

accusations of error, as Friedman would a decade later.[117] Another relevant factor was that many economists at Cambridge University admired Friedman's consumption work, which he had worked on during his fellowship at the university in the 1953/1954 academic year and had presented at a number of venues in the United Kingdom during that year.[118]

On economic subjects other than the consumption function, Cambridge University economists may also have felt that Friedman was someone with whom they could have amicable disagreement. Later, however, the disagreement between Friedman and key economists from Cambridge University became far from amicable. His relations with key Cambridge University figures would rapidly deteriorate at the start of the 1970s, a development marked by the appearance in 1970 of sharp critiques of Friedman's monetary economics by both Kaldor and Robinson.[119] Indeed, starting essentially at this point, it would rapidly become clear that, from the perspective of the leading economists of Cambridge University, Friedman was *the* principal adversary—a situation that made Friedman's 1956−1966 service as part of Cambridge University Press's economics editorial team all the more incongruous in retrospect.[120] Nicholas Kaldor, with whom Friedman had been friendly in the 1950s, would emerge as an especially prominent U.K. critic of Friedman and monetarism.[121]

Friedman himself made clear his own posture toward Cambridge University in January 1977

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[117] See Guillebaud (1937a, 1937b).
[118] As noted in Nelson (2020a, Chapter 3), Cambridge University's Nicholas Kaldor was an exponent of a version of the permanent income hypothesis. David Newbery, a student at Cambridge University in the mid-1960s, recalled the permanent income hypothesis being taught in a favorable light in economics classes. (David Newbery, interview, October 10, 2014.) By that stage, the university's department included Frank Hahn, who would go on record about his admiration for *The Theory of the Consumption Function* (see Hahn, 1990b). The fact that Friedman presented his consumption-function work in seminar appearances around the United Kingdom in the 1953/1954 academic year was recalled by Alan Walters, who saw one such presentation, in conversation with the author (May 11, 2002).
[119] See Kaldor (1970) and Robinson (1970).
[120] Although a detailed discussion of the U.K. version of the Keynesian-monetarist debates is outside the scope of this book, one notable feature of that debate that deserves mention here is that Friedman galvanized key Cambridge University economists into uniting against him. For, while Frank Hahn and James Meade were more sympathetic with the MIT side of the Cambridge-vs.-Cambridge debate than they were with Joan Robinson's position, the 1970s and 1980s saw Hahn and Meade figure, like Kaldor and Robinson, as major U.K. opponents of monetarism. As discussed in Chapter 1 above, Hahn (1971) essentially began hostilities with Friedman via an extremely negative review of *The Optimum Quantity of Money*. Friedman was affronted by the review and promptly ended his annual practice of sending Hahn a Christmas card. In recalling Hahn's review and Hahn's recounting to Sargent of Friedman's reaction, Thomas Sargent (interview, March 26, 2014) noted that Friedman's discussion of natural-rate and other theoretical issues, as manifested in the new and reprinted chapters in the book, "was loose, so, Friedman was making a guess about outcomes from some models which he hadn't quite written down. But it was a pretty damn good guess. (*laughter*) And Hahn wasn't flexible enough to notice that, so he just jumped on him."
[121] See Nelson (2009a, 2009b) for a detailed account.

when he joked that one should expect a wait of a quarter-century before a Cambridge University economics professor acknowledged an error in their analysis.[122]

The latter-day relationship between Friedman and key Cambridge University economists was evident in 1977 not only in this remark and the aforementioned contents of the *Cambridge Journal of Economics*, but also in a a sharp exchange of letters on the causes of inflation that Friedman and Kaldor had in the London *Times* (see Nelson, 2009a, p. 99). In the same year, Rose Friedman observed: "Those of us who came out of there [the University of Chicago] did not pick up the Keynesian philosophy, [so] we didn't have the same attitudes as they did at both Cambridges." (*The Anchorage Times* (Alaska), June 10, 1977.)

Against this background, it might be expected that Milton Friedman's reaction to the Cambridge-vs.-Cambridge debate would be a desire that both sides lose. Such an expectation, however, would be misplaced: Friedman was firmly on the U.S. Cambridge side of the debate. For one thing, he held the key figures at MIT in higher esteem than he did their counterparts at Cambridge University. Friedman clearly, if condescendingly, conveyed this judgment in 1969 when, after offering his generalization (noted in Chapter 6 above) that what was received wisdom about monetary policy at the University of Chicago in one year became received wisdom at Cambridge, USA, five years later, he added the further generalization the same wisdom only became accepted in Cambridge, United Kingdom, only after an additional several years' lag (Instructional Dynamics Economics Cassette Tape 17, March 1969).

On top of his more favorable attitude toward the economic viewpoints at MIT than those at Cambridge University, there were several specific considerations involving economic substance that made Friedman favor the economic analysis on the U.S. side of the reswitching debate. One of these was that Friedman's position that economic growth was determined by real factors and was enhanced by reliance on market mechanisms put him very much on the U.S. end of the debate. The MIT side of the two-Cambridges debate was taking a stand in defense of the validity and coherence of neoclassical models derived from optimizing behavior, and so that side was upholding an approach to economics that Friedman largely endorsed.[123] The U.K. critique of those models, in turn, was seen as pointing toward the desirability of less reliance on market

[122] Friedman (1977j, p. 3). This remark opened an evening talk, also available today in downloadable audio form, that Friedman gave to the Hoover Institution's conference on income distribution on January 28, 1977.
[123] Eatwell (1983) drew a more explicit connection between Friedman's economics and the issues in the reswitching debate, arguing that Friedman's natural-rate-of-unemployment concept became invalid if the Cambridge University economists' critique of neoclassical models was accepted.

mechanisms and greater governmental intervention in the investment process.[124]

In addition, Friedman had a stake in a specific aspect of the debate—the component of the debate that occasioned Bernanke (1987, p. 203), Jorgenson (1995, p. 7), Lucas (1988b, p. 36), and Paul Romer (2015) to refer to it. This was the issue—already alluded to above—of whether it was legitimate to use the concept of an aggregate capital stock in macroeconomic analysis, or whether, instead, the heterogeneity of capital equipment across firms and sectors rendered the concept of an aggregate capital stock inadmissible.[125]

This matter had been a central concern even in the earliest published contributions in the debate, such as Robinson (1956) and Solow (1955). The Cambridge University side of the debate denied that the aggregate capital stock was a viable concept.[126] The MIT side of the debate, in contrast, embraced analyses in which the concept of an aggregate capital stock was used. As one of the workers with Simon Kuznets on the formation of national accounts aggregates, Friedman was firmly aligned with the MIT side of this divide.[127] In 1970, Friedman matter-of-factly reaffirmed the validity of an aggregate-capital-stock concept, observing that in an analysis of aggregate output behavior over long periods, "the capital stock would have to be included and treated as an

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[124] That the MIT side of the debate was one that seemed to lend itself to more market-oriented policies than did the arguments associated with the Cambridge University side was noted by Harcourt (1972, p. 3) and Mirrlees (1973, p. p. xvii).

[125] The discussion that follows focuses on this issue, rather than the matter of reswitching. The latter was of course a very important aspect of the debate, but it was not an aspect to which Friedman's body of work had much connection.

[126] An example of a statement on this point by the Cambridge University side was Kaldor and Mirrlees' (1962, p. 174) contention that "under continuous technical progress and obsolescence, there is no way of measuring the 'stock of capital.'" Samuelson (1970, pp. 734–735) pointed to the issue as one on which Kaldor and Joan Robinson were united in their disputes with neoclassical economists.

Kaldor had also been involved in the *previous* generation's debates on capital theory—notably a debate centered on the views of Frank Knight. In this earlier period, Kaldor had some interaction with Friedman on the matter. This interaction was reflected in Kaldor's (1937, p. 208) observation: "I am indebted to Mr. Milton Friedman, of the National Resources Committee, Washington, for helping me to understand Knight's argument..."

[127] Robert Solow invoked Kuznets' construction of estimates of the aggregate capital stock when teaching at Oxford University in the 1968/1969 academic year. Solow presented a version of lectures on economic growth that would subsequently be presented at Warwick University and published as Solow (1970). In his Oxford University lectures, Solow, lightheartedly alluding to the Cambridge-vs.-Cambridge debate, stated that K in the production function could be regarded as standing for "Kuznets," observing that whatever Kuznets meant by K, that was what Solow meant. (Christopher Gilbert, interview, November 21, 2014, and personal communication, January 19, 2015.) Kuznets' work on national accounts had long been tied to an acceptance of the notion of an aggregate capital stock, and some of this work put a spotlight on capital-stock estimates—notably the Kuznets (1937, 1939) studies undertaken when Friedman was his assistant. However, Kuznets' primary study of the U.S. economy's capital stock was one of his later works, Kuznets (1961), which Friedman and Schwartz (1963b, p. 54; 1982, p. 602) would cite.

endogenous variable, presumably defined by an integral of past investment."[128]

The question of the viability of the notion of an aggregate capital stock went hand-in-hand with that of the validity of the aggregate-production-function concept. And, in defending the latter concept, Robert Solow made arguments that put him very close to the Friedman outlook on methodology. In particular, Solow (1966b, pp. 1259–1260) observed: "I have never thought of the macroeconomic production function as a rigorously justifiable concept. In my mind it is either an illuminating parable, or else a mere device for handling data, to be used so long as it gives good empirical results, and to be abandoned as soon as it doesn't, or as soon as something better comes along."[129] Duncan Foley, a junior MIT department member starting around the time this Solow article appeared, observed of Solow, "he has a tremendous theoretical intuition. His philosophy of theorizing is not to go for any kind of general models, or general point of view, like general equilibrium. It's to make very specific models of very specific kinds of problems. And I don't remember much challenge [from Solow] to Friedman's point of view at the level of methodology." (Duncan Foley, interview, October 2, 2014.) Solow confirmed that "I thought there was a lot of sense in Milton's methodology"—while adding: "I think—and this was a tendency Milton had—he pushed it too far." (Robert Solow, interview, December 2, 2013.)[130]

Prior to the appearance in print of Solow's just-quoted defense of aggregate production functions, Paul Samuelson had sketched Solow's position on the matter—doing so in an article (Samuelson, 1962b) that has sometimes been viewed as the paper that really put the reswitching debate into high gear (see Jorgenson, 1995, p. 7). In the same article, Samuelson himself also endorsed the treatment of the aggregate production function as a parable, or empirical approximation, observing (Samuelson, 1962b, p. 193): "It is the case, I believe, that Robert

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[128] Friedman (1970a, p. 218). Likewise, in Friedman (1966d, pp. 80–81), he had indicated that in some contexts it was desirable to treat consumption and capital goods as different, but he took for granted the validity of aggregating capital equipment into a single good.

[129] Consistent with Solow's 1966 account of his longstanding position, in 1955 he had written: "For many purposes it is remarkably useful to assume that there exists only one physical commodity which can either be consumed or used as capital in the production of more of itself." (Solow, 1955, p. 101.)

[130] Solow elaborated: "And in particular, the part of positive economics, in Milton's terms, that struck me as way too far was the notion that you really don't have to bother discussing the realism of assumptions—that the only thing that matters is the implications of the theory, not its assumptions. And I think that that's both logically and practically silly, nonsense; in fact, wrong, in the sense that the assumptions of a theory are among its implications, so, logically speaking, if you're testing all the implications, you will be testing the assumptions. And secondly, there are implications of any theory that are so close to the assumptions, that if they go wrong, if they're contradicted, the assumptions are *de facto* contradicted. If they're confirmed, the assumptions are *de facto* confirmed." (Robert Solow, interview, December 2, 2013.) Solow was also highly critical of some of the analogies and examples Friedman cited in support of his methodology, in particular the one involving leaves and sunlight. (Robert Solow, interviews, December 2 and 31, 2013.) (See Friedman, 1953b, p. 20.)

Solow and I have pretty much the same general views in this matter, having arrived independently and together at the same general conclusions."

Samuelson's endorsement, in the context of production-function analysis, of a position on aggregation that closely corresponded to that espoused by Friedman in his 1953 methodology article, contrasted somewhat with his opposition to Friedman's methodological position in other contexts (such as Samuelson, 1963, 1983b). It may be that the notion of the aggregate production function as an approximation found favor with Samuelson because it allowed for structural macroeconomic modeling, and that his main reservations about the Friedman methodology pertained to instances in which it was used to justify reduced-form modeling. In any event, both Samuelson and Solow invoked a Friedman-style defense of the procedure of using the concept of an aggregate production function.[131] Little wonder, that in a 1970 conference in Israel that concerned itself heavily with the reswitching debate, the approach of postulating the existence of an aggregate production function was described as "Friedmanesque" (Mirrlees and Stern, 1973, p. 355).

Although it is clear that Friedman's sympathies were with the MIT side of the Cambridge-vs-Cambridge debate, and notwithstanding his hopes to write more on capital theory at some stage, Friedman sat out the debate.[132] "Which I think was an act of wisdom on his part," Robert Solow observed (interview, December 2, 2013).[133] This judgment was shared by Victoria Chick, who, from her position in the U.K. academic scene, also stuck to monetary research. Chick avoided the capital theory debate because "if it captured you, you were really snared for years." With respect to Friedman, Chick remarked: "He didn't get into it, and he was wise not to, I think. As I say, it can swallow you up." (Victoria Chick, interview, January 13, 2015.)[134]

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[131] In a later contribution, in a paper marking the 1976 passing of his one-time University of Chicago teacher Paul Douglas, Samuelson implied that the Chicago School had a stake in the Cambridge-versus-Cambridge debate because the side consisting of "Kaldor and neo-Keynesians" had challenged empirical evidence in favor of the Cobb-Douglas production function (Samuelson, 1979, p. 936).

[132] He did make in print what was, in effect, a minor acknowledgment of the existence of the debate in 1972 when adding "(England)" for clarification after referring to "Cambridge" (see Friedman, 1972a, p. 936).

[133] Edwin Burmeister, whose graduate work at MIT and subsequent career would include heavy involvement in the Cambridge-vs.-Cambridge debate, observed: "I don't think people at Chicago had much interest in that. Milton didn't. The only person who really had much interest in it was Jim Heckman." Asked to account for Heckman's interest in the debate, Burmeister's answer was straightforward: "he's interested in *everything*." (Edwin Burmeister, interview, November 20, 2014.) Earlier, when visiting the University of Chicago in the early 1960s, Joan Robinson had attempted with little success to get the university's economists, including Friedman, excited by the controversy (David Laidler, personal communication, September 28, 2015). See also Silk (1976, p. 52) on this visit.

[134] By focusing on matters concerning aggregation of the production function and of the capital stock, the preceding discussion has not covered in detail the "reswitching" property of production functions (the notion that there might be widely differing levels of output at which a particular combination of productive inputs is the profit-maximizing

Travel, bases, and publications

Although he was now a Californian, at certain points during 1977–1978 Friedman was in fact in fairly close physical proximity to Cambridge University in the United Kingdom, and at other points during the same years to Cambridge, Massachusetts. He made trips to the United Kingdom in September 1977 in November 1978, for appointments in London, as well as a visit in April 1978 that was centered on a talk Friedman gave in Scotland.[135] And he was in New England for extended periods in both 1977 and 1978, owing to the fact that he and Rose Friedman continued, in the initial years after their January 1977 relocation to San Francisco, to have a home in Vermont and to use it as their base for much of the spring and summer period. Consequently, during the mid- to late April 1977 period he and Rose Friedman taped a commentary, for his Instructional Dynamics Economics Cassette series, while staying in Vermont (Instructional Dynamics Economics Cassette Tape 211, April 1977, Part 1). The Friedmans were still in Vermont in early May and in the early days of June.[136] Their schedule involved them spending further time there in July (*The Anchorage Times* (Alaska), June 10, 1977). Likewise, the Friedmans were located in Vermont during May and June 1978 (*Los Angeles Times*, May 15, 1978, p. 3; *Omaha World Herald* (Nebraska), June 19, 1978; *Human Behavior*, November 1978, pp. 26, 30).

The Friedmans interspersed their time in Vermont and San Francisco with numerous trips to other parts of the country. Some of the better-publicized trips occurred in 1978—those, discussed in the next section, in connection with the tax-limitation movement. Among their

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one, when another combination is profit-maximizing in the case of intermediate levels of output). The Cambridge University side of the debate often regarded Samuelson's (1966) acknowledgment (following his prior expressions of doubts), in a *Quarterly Journal of Economics* symposium, that reswitching could occur, as having been a devastating concession on his part. Eatwell (1982, p. 45), for example, stated that the 1966 exchanges "culminated in an admission by the high priest of orthodoxy, Paul Samuelson, that … something is logically wrong with neoclassical theory." On the U.S. side, however, Burmeister (1980, pp. 130, 154) and Robert Solow (in *Times Literary Supplement* (London), March 14, 1975) suggested that cases of reswitching had little bearing on the applicability of standard neoclassical analysis of the production process." The successes on some counts of the U.K. side of the debate, along with U.S. economists' predominant view that reswitching could be accommodated in the standard framework, were reflected in Clark's (1987, p. 107) observation: "The smoke has disappeared from the battlefields of the Cambridge on capital theory, but the reasons for the conflict have not evaporated, nor have textbook expositions of the neoclassical parables been radically altered."

[135] With regard to these trips, see *The Listener* (London), April 27, 1978 (a record of a talk, broadcast in the United Kingdom and delivered at Strathclyde University Business School, Glasgow, Scotland: on this talk, see also Valentine, 1987, p. 545, and Friedman, 1978h), Friedman and Friedman (1998, p. 476), and Moore (2013, p. 351).

[136] See Friedman and Friedman (1998, p. 473) on the Friedmans being in Vermont in early May. See also the Vermont dateline in Friedman's (1978g) preface dated May 2, 1977. On Friedman's continued stay in Vermont in June, see *Philadelphia Inquirer*, June 3, 1977.

other travels within the United States was one to the state of Alaska in June 1977. This trip, the Friedmans' first trip to Alaska (*The Anchorage Times* (Alaska), June 10, 1977), featured a talk by Friedman, delivered on June 9, on both current economic issues and longer-term ones (including Friedman's observation—one perhaps actually better suited to a mainland U.S. audience—that "this country had the good fortune of having two oceans fall on each side of us").[137] Other travels included visits in February 1977 to Malibu and Los Angeles in California and to Reno, Nevada; to Dallas for a talk on April 2, 1977 (Valentine, 1987, p. 544); and a further trip to Los Angeles in November 1977.

Prior to the last of these visits, Friedman had begun what would turn out to be over a dozen speaking engagements in various parts of the country over an eight-month period starting in September 1977.[138] These talks would be for the *Milton Friedman Speaks* series. This series was initially conceived as being, in their videotaped form, the basis for a television series—an idea abandoned by the late 1978 in favor of the *Free To Choose* series, made in 1979–1980 and discussed in the next two chapters.[139] One of the talks in the series involved a return on Friedman's part to the University of Chicago for a public lecture, dealing mainly with the workings of the price system, on October 3.[140] Although Friedman arrived at these events mostly through air travel, he indulged his fondness for driving when it came to arriving in Logan, the city in which he had a speaking engagement at Utah State University.[141] The Friedmans reached Utah by spending two days driving through the Middle Far West of the United States, including the states of Nebraska and Wyoming.[142]

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[137] Friedman (1977e, p. 9). On the specific date of this event, see Valentine (1987, p. 545).
[138] For the list of engagements, see Friedman and Friedman (1998, p. 604).
[139] Friedman and Friedman (1998, p. 473).
[140] *Milton Friedman Speaks*, Episode 1, "What Is America?," taped October 3, 1977.
[141] The speech was on October 13, 1977. See Friedman and Friedman (1998, p. 604) and *Student Life* (Logan, Utah), October 14, 1977, p. 1.
[142] See *Milton Friedman Speaks*, Episode 2, "Myths That Conceal Reality," taped October 13, 1977, p. 26 of transcript. The meal stops on road trips like this could be a source of exasperation for Rose Friedman. James Friedman, having become a full professor in 1972, observed in the *Journal of Political Economy* in late 1976 that "those with a disposition to become professors also have a disposition to like such things as… varied food." (J.W. Friedman, 1976, p. 1374.) This was an attribute that Rose Friedman likely felt was lacking in Professor Milton Friedman. Gloria Valentine noted that "Professor Friedman liked junk food" and observed that, in recalling their pre-1977 travel routine to Valentine, "Mrs. Friedman mentioned that when they traveled between Chicago and Vermont, that there were nice restaurants that she wanted to stop in, but he stopped [instead] at McDonalds. He preferred [going to] McDonalds, or Burger King, or Kentucky Fried Chicken." Later, when working at the Hoover Institution, Friedman took note of the McDonalds located at the Stanford shopping center. On one occasion, a number of colleagues stopped by Friedman's office at the Hoover Institution, and "they were going out to lunch. Later, I found out that they had to go to McDonalds, because that's where Professor Friedman wanted to go. (*laughter*)." (Gloria Valentine, interview, December 5, 2013.)

Visits to Continental Europe also characterized these years. They included a brief trip to the Netherlands, one made so that Friedman could give an address at a Mont Pelerin meeting in Amsterdam on April 15, 1977.[143] Later, starting around mid-June 1977, the Friedmans went on several weeks of international travel that, though culminating in a visit to Israel, started off with trips to Switzerland and Scandinavian countries (*The Anchorage Times* (Alaska), June 10, 1977). This itinerary included a return to Sweden little more than six months after Friedman's Nobel lecture, with his new visit involving a delivery of his "Future of Capitalism" setpiece speech to the Federation of Swedish Industries. Friedman also visited Denmark, in a trip that included an engagement at a meeting of the Danish Economic Association.[144]

Although Friedman had put time into working on issues facing Continental Europe when he was there in 1950, he had done little to consolidate those early ties, and Niels Thygesen, Friedman's host in Denmark, recalled: "Friedman didn't have many links in Europe. He came here, of course, occasionally… [including] to my country, because at the time he got the Nobel Prize he was invited, [and] in several other places in Europe. But he was never there for long."[145] Thygesen noted that "I had met Friedman briefly in the United States a couple of times, I think, at the American Economic Association meetings in the 1960s, but the first time I really met him" was during the Denmark trip of 1977. "We had invited him to Copenhagen, and he came here, not exactly at the time of the Nobel Prize, but the following year. And then I had, of course, some discussions with him." The topics they discussed included the content of Thygesen's (1977) article "The Scientific Contributions of Milton Friedman." The *Scandinavian Journal of Economics*, a journal closely linked to the formal process of the Nobel economics award, followed a practice of publishing materials connected with each year's winner, including a commissioned article on the laureate's contributions. By the time of their meeting, Thygesen's article on Friedman was already in print, and Thygesen recalled that, in their discussion, "he was not particularly complimentary about my piece (*laughter*)… He thought there were a couple of errors in it—I don't remember how significant they were. I thought he overdid it a bit—Friedman was not a very generous man. He would tell you, straight away, if there was even a small detail that he didn't agree with. But I had a nice couple of days with him." (Niels Thygesen, interview, February 10, 2015.) This had indeed been the case of overreaction on the part of Friedman, whose work and positions Thygesen had skillfully encapsulated in his article.

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[143] See Friedman (1984f, p. 42). This event took place just after Friedman completed his time as a visitor to the Federal Reserve Bank of San Francisco. See *Oakland Tribune* (California), April 18, 1977, p. 12.
[144] For more details concerning this event, see Chapter 7 above.
[145] Friedman eventually did make longer trips to Continental Europe in 1990 and 1997.

With the Nobel lecture set for the *Journal of Political Economy*, Friedman did not offer it to the *Scandinavian Journal of Economics*. That journal did publish in late 1977 his article "Time Perspective in Demand for Money," in late 1977.[146] This was a light revision of a 1972 manuscript. It was Friedman's final paper in monetary economics to be mainly theoretical in content, and it was also the last paper he published in the 1970s in an economic-research journal.

With regard to another outlet for Friedman's research, a news report that appeared just after he moved to San Francisco stated: "The economist, who has written almost 20 books, is now busy on another." (*San Francisco Examiner*, January 5, 1977.) This was his *Monetary Trends* with Anna Schwartz. This book remained unfinished over the 1977−1978 period, although progress toward completion took place in those years. "What do I do?" Friedman asked himself in the spring of 1977, before answering: "What I always do—writing, thinking on problems connected with the field of money and the operation of monetary policy." (*Oakland Tribune* (California), April 18, 1977, p. 12.)

The impression of a new book on monetary matters actually appearing in 1977 was created a few months later by Hobart Rowen's observation during the summer that "Friedman, in a new book, talks of 'decades' of simultaneously rising inflation and unemployment." (*Washington Post*, July 24, 1977, p. G1.) The book in question, however, consisted simply of the issuance by London's Institute of Economic Affairs in pamphlet form of Friedman's Nobel lecture, already published in the USA as an article in the June 1977 issue of the *Journal of Political Economy*.[147]

The U.K. reprint of Friedman's Nobel lecture was subtitled *The New Dimension of Politics*, and, in some retrospectives that appeared in the years that followed, it was indeed the political or policy-formulation aspects of the lecture that came to be seen as foreshadowing future developments in monetary analysis. Bernanke (2006b, p. 4) suggested that "Milton Friedman once again was in the vanguard on this issue." "Friedman laid out the modern argument" for price stability, Bernanke contended, by supplementing the argument that Phillips-curve dynamics meant that the output gap would revert to zero irrespective of the inflation rate with renewed stress on the damage to aggregate supply arising from high inflation. In this way, Friedman had made the case for orienting monetary policy toward the achievement of zero, or low single-digit,

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[146] See Friedman (1977b).

[147] See Friedman (1977p). Another Friedman pamphlet-book had been published by the Institute of Economic Affairs (IEA) at the start of 1977. This item—Friedman (1977i)—consisted of his talks in London in August-September 1976 on Kenneth Galbraith's economics and on proposals to move toward smaller government. Material in this book was discussed in Chapter 6, and further coverage of it appears in the next chapter.

inflation, in addition providing impetus to research on the losses in potential output that were associated with high inflation—so that, Bernanke concluded, "by the late 1970s, even economists who were not part of Friedman's monetarist circle were beginning to study and acknowledge the costs to the economy associated with high inflation."

And with regard to the appropriate policy arrangements, Thomas Havrilesky, a longtime U.S. monetary economist, saw the longer-term message of Friedman's monetary work and the 1970s literature as being of a piece. Havrilesky recounted in 1995: "Milton Friedman about 40 years ago and intensified by the rational expectations revolution in the 1970s and 1980s, and supported by recent empirical work, a stream of economic research indicates that insulating the central bank from political pressure will improve inflation performance."[148] Havrilesky's characterization elided Friedman's own opposition to central bank independence. But it is true that both Friedman's 1977-published lecture and his early work were amenable to arrangements that isolated monetary policy from short-term political pressures—with Friedman's particular prescription involving the assignment to the central bank of a policy rule attuned to price stability.

Another article regarded in retrospect as impressing upon economists the case for central bank independence was one that appeared immediately after Friedman's in the June 1977 issue of the *Journal of Political Economy*: Finn E. Kydland and Edward C. Prescott's (1977) "Rules Rather Than Discretion: The Inconsistency of Optimal Plans." As its title suggested, this article pointed specifically toward the desirability of price-stability-oriented policy rules rather than recommending central bank independence *per se*. The subsequent literature, however, came to see central bank independence as conducive to avoidance of the scenario of a long-run inflationary policy that Kydland and Prescott had derived. Indeed, in a twentieth-anniversary retrospective on Friedman's Nobel lecture, Carl Walsh would argue that "Friedman was amazingly prescient" in his lecture—Walsh's grounds being that Kydland and Prescott (1977) had been in the spirit of the call Friedman had made in his Nobel lecture for better positive economic analysis of policymaking in order to understand why inflationary outcomes arose.[149]

As it happened, Kydland and Prescott (1977) departed from what Friedman had in mind in his Nobel lecture, as they postulated that the central bank aimed to maximize social welfare by pursuing economic aims. Friedman, in contrast, had become enamored of the public-choice

[148] March 16, 1995, testimony, in Joint Economic Committee (1995, p. 48).
[149] Walsh (1997, p. 1; p. 220 of printed version).

approach—which implied, as he acknowledged, a "more cynical" perspective on policymakers than that associated with an approach based on social welfare maximization (*Financial Times* (London), January 6, 1977), with a focus on shorter-term and special-interest political pressure rather than economic tradeoffs.[150] In addition, Kydland and Prescott held that the monetary authority purposely sought a positive output gap, whereas Friedman did not ascribe such an overemployment aim to policymakers when analyzing hypothetical or historical policies.[151]

There was, however, an important aspect that the Kydland-Prescott and the public-choice approaches had in common—one that would seriously mar much of Friedman's policy analysis during the 1970s, especially in 1977 and 1978. They took policymakers as well informed about the structure of the economy and, particularly, the connection between their monetary actions and the behavior of inflation. Inflation was consequently seen as a deliberate choice by the authorities, and policymaker reluctance to restrict demand in the face of high inflation was seen as deliberate acquiescence to and encouragement of that inflation. These conclusions—valid enough analytically, when applied to the policymakers in the public-choice and Kydland-Prescott models of monetary policy decisions—were of very limited applicability in the understanding of the reasoning behind real-world, U.S. policymaker choices in the 1970s. Friedman's statements during the early Carter years underlined the difficulties involved in applying the public-choice literature's approach to actual policy.

Public-choice analysis—and applying it to Jimmy Carter

As the above discussion has suggested, the tone of Friedman's commentaries on current policy developments was uneven during 1977 and 1978. It had, basically, two major strands, and they were in conflict with one another.

As part of the *first* strand, Friedman made numerous commentaries—informed by his exposure to the largely-theoretical investigations of policymaking being made in economic research, notably those in the public-choice vein—that took for granted a consensus in policy circles that inflation was a monetary phenomenon. That is, policymakers *chose* inflation consciously and knew that disinflation entailed monetary restriction. In these commentaries, Friedman likewise tended to assume that there was wide agreement that a move to economically-liberal microeconomic policies would improve prospects for employment and economic growth in the

[150] On Friedman's interest in this literature, see the discussion that follows as well as Nelson (2020a, Chapter 9).
[151] See, for example, Nelson (2020a, Chapter 9; 2020b, Chapter 11) and the previous chapters of this volume.

medium and longer term. From the perspective of these commentaries, the principal obstacle to a major change in policy settings lay in perceived short-term political costs of the change.

The second strand of commentaries appeared over the same years of 1977 and 1978, but less frequently than the first. In this second strand, Friedman took both high inflation and the tendency for the federal government to engage in interventionist policies in specific sectors to be the result of officialdom's acceptance of a different economic model from his own (and, of course, an incorrect one). From this perspective, a doctrinal shift—effected by either an overhaul in the conceptual framework guiding existing policymakers, or a change in the personnel making policy in favor of those possessing a different conceptual framework—would likely be required to generate a major policy change. Recalculation of political costs of different policies would, these lines of thinking suggested, not be the primary reason for the change.

As will be seen, of the two interpretations of economic policy reflected in these different strands, it was the second that actually better captured policy decisions under President Jimmy Carter and the reasoning underlying his policies. Friedman's considerable reliance on the first, and flawed, interpretation of contemporary U.S. policymaking would lead him to numerous mispredictions of the direction in which Carter would go on economic policy during 1977 and 1978. These mispredictions were a corollary of the fact that Carter and his advisers really did adhere to an older economic framework, one that was largely antithetical to Friedman's.[152]

Crucially, Friedman overemphasized political, as opposed to economic, assessments as factors guiding the formulation of U.S. economic policy in the 1977−1978 period. Friedman's tone during Carter's time in office was set very early, in the January 1977 debate with Franco Modigliani discussed in the previous chapter, when he affirmed the need for a "positive science of politics."[153] He went on: "Franco, myself and all the rest of us have tended to follow the attitude: Well, now, what we need to do is to figure out the right thing. If only we can tell them what the right thing to do is, then there's no reason why able, well-meaning, well-intentioned people should not carry out those ideas." Yet, Friedman suggested, it was something other than the right policy that emerged in practice, and it wasn't "an accident that that happens. It happens for very systematic, explicit reasons."[154] Similarly, thirteen months later Friedman lamented the

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[152] For documentation of this point, see, in addition to the discussion that follows, the previous chapter, as well as Christina Romer (2005), Romer and Romer (2002b), and Nelson (2005).
[153] Friedman (1977d, p. 18).
[154] Friedman (1977d, p. 18).

lack of application until recently of economic research on "the political arena" that saw the participants as "pursuing their own interests" instead of social welfare.[155] And, as already indicated, his Nobel lecture of December 1976 had pointed to the need for such a research agenda, while also mentioning James Buchanan, Gordon Tullock, and Kenneth Arrow as among the authors who had already made headway on the subject matter.[156]

Friedman's interest in modeling policymakers along the lines of the public-choice literature impaired his own analysis of current policy developments. He was aware—indeed, he had talked at length on the subject—that, since 1970, U.S. policymakers had embraced nonmonetary views of inflation. He also knew, from his experience in the 1960s, that academic views on economic policy often took years to permeate policy circles: thus, officialdom in the Kennedy-Johnson years had given fiscal policy primacy over monetary policy at a time when academic work was proceeding apace at reversing the ranking. Friedman was also aware that, his influence notwithstanding, much influential economic opinion in U.S. academia remained different from his own, particularly in the area of policy prescriptions. Indeed, the lack of consensus in 1977 among macroeconomists on the right setting of stabilization policy was evidenced by his and Modigliani's different policy prescriptions in January 1977, and by the continued advocacy during the year of demand stimulation alongside incomes policy on the part of Okun, Tobin, and others.[157]

These considerations should have led Friedman to suspect that the White House and the Federal Reserve in the late 1970s would still be inclined to embrace nonmonetary views of inflation, even though the monetarist arguments against those views were coming to prevail (though were not yet the clear-cut winner) in academic discussions. The public-choice perspective, however, encouraged a line of thinking in which policymakers were assumed to understand the basic structure of the economy and optimized in light of that structure as well as the prevailing political pressures. That perspective tended to lead Friedman further toward a tendency he had already sporadically displayed in past discussions of Burns, Nixon, and Ford: to take for granted that policymakers understood that inflation was a monetary phenomenon.[158] In contrast, the

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[155] *Milton Friedman Speaks*, Episode 5, "What Is Wrong With the Welfare State?," taped February 23, 1978, p. 10 of transcript.
[156] Friedman (1977c, p. 460).
[157] See Chapters 5 and 7 above.
[158] Thus, in *Newsweek* (March 5, 1979), for example, he would write that "it has been politically profitable for the powers that be to produce inflation." Also noteworthy in this connection was Friedman's insistence in 1978 (see the previous chapter) that Secretary of the Treasury Blumenthal did *not* really believe in wage-push views of inflation.

record of U.S. policymakers' statements and actions suggested that such an acceptance had not actually taken place at all—either in the first half of the 1970s or in 1977–1978.

At the time of Jimmy Carter's inauguration in 1977, Friedman, believing that politicians had drawn the correct lessons from the economic problems of the first half of the decade, expected Carter to be led by his own long-term political interests to support restraint in aggregate demand. This view tended to make Friedman to be more optimistic about the likelihood that economic policy would progress rapidly toward a framework he favored. For it suggested that, if support for that framework came to prevail in public opinion, policymakers would tend to adopt it whatever their own feelings about its merits. In this vein, Friedman would cite examples of policymakers before the 1970s—including Winston Churchill, Dwight Eisenhower, and Lyndon Johnson—who came to office associated with particular positions on social or foreign policy yet had followed diametrically opposite policies on taking office.[159]

In the 1970s, Richard Nixon had provided another such example, this time in the area of economic policy, with his imposition of wage-price controls. Public opinion had, in the early 1970s, helped shift Nixon away from policies Friedman favored. Furthermore, President Nixon's post-1971 economic experience, Friedman believed, had helped teach politicians that expedient macroeconomic policies were no longer good politics.[160] Friedman declared in

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[159] In February 1978, Friedman contrasted Churchill's opposition to dissolving the British empire with his acquiescence with that development as a postwar prime minister (*Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, p. 33 of transcript). (See, however, A. Roberts, 2018, pp. 945, 950, for an argument that the United Kingdom's process of decolonization, although not reversed, was not formally extended to more countries during Churchill's 1951–1955 period in office. On that reading, Friedman's interpretation of Churchill's record was not strictly incorrect.) Earlier, in Instructional Dynamics Economics Cassette Tape 203 (November 1976, Part 2), Friedman had noted Eisenhower's warnings against the military-industrial complex as president despite his military background, Johnson's enaction of civil rights reforms despite his opposition as a legislator to earlier reforms in this vein, and President Nixon's extensive negotiations with China and the USSR, notwithstanding his reputation as being hardline on foreign policy. (For other discussions Friedman gave along the same lines, see *The Evening Bulletin* (Philadelphia), November 15, 1976, and *Milton Friedman Speaks*, Episode 2, Myths That Conceal Reality," taped October 13, 1977, p. 25 of transcript.) Friedman cited all these examples as evidence that experience in office often led policymakers to change their position, with trends in public opinion and the advice of staff in government often playing important roles in shaping the positions that policymakers took in office. (Friedman's view to this effect were also reflected in his assessments of the influence of economists' consensus on the policies of successive presidents—see Nelson, 2020a, Chapter 10)—and in his doubt that Barry Goldwater would have been able to enact his election campaign promises if elected—see Nelson, 2020b, Chapter 12.)

[160] In *Newsweek*, October 16, 1978, Friedman indicated that Nixon himself had acknowledged that imposing wage-price controls was a mistake. As he noted, this acknowledgment had appeared in the former president's newly published memoirs—see Nixon (1978, pp. 520–521)—from which Friedman drew his column title, "The Politics of Economics" (a phrase that appeared in Nixon, 1978, p. 522). Although Friedman was not mentioned in these memoirs, which appeared in May 1978, Friedman cited them in Friedman and Friedman (1998). In the United

November 1977 that he perceived a "very strongly detectable change in the attitudes of the public at large" in which both the government's role in producing inflation had become better understood and in which there was a greater willingness to take measures against it.[161]

In this light, and as indicated in the previous chapter, Friedman believed that Carter would not proceed with the traditional economic policies he had largely espoused during the 1976 election campaign. Indeed, as of early 1977 Friedman believed that it was "very likely" that there would be a recession in 1978 or 1979, and he reiterated his view that Carter wanted, or should want, such a recession (Instructional Dynamics Economics Cassette Tape 209, March 1977, Part 1). He expressed this judgment again in his speech in Alaska in June 1977.[162] A recession at such a point in Carter's term, Friedman believed, would leave the president able to go to the polls in 1980 in conditions of economic recovery and declining inflation.

Toward the end of Carter's second year in office, Friedman acknowledged that the president had only "occasionally" seemed to have shown interest in what Friedman saw as this enlightened intertemporal approach (*Newsweek*, September 18, 1978). Instead, especially in 1977, the president and other senior officials had shown faith in measures—stimulus packages to be enacted immediately and against the background of an economy well into recovery, encouragement of the holding-down of market interest rates, and incomes policies and related measures against inflation—that Friedman saw as delivering dubious short-term benefits, while increasing longer-term output instability and forfeiting the achievement of price stability.

Friedman had overestimated the resilience of the ideas concerning stabilization policy that had guided policy earlier in the 1960s. "We have followed very bad policies," he observed in 1978 (*Meet the Press*, NBC, November 12, 1978, p. 2 of transcript). But it took him well beyond 1978 to return definitively to his earlier position that "[b]ad economics makes bad policy" (*Newsweek*, May 12, 1975) and to the corollary that economic policy mistakes in the past had been guided by faulty economic analysis.

The contradictory pattern marking Friedman's interpretation of policy developments in

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States, the memoirs were titled *RN: The Memoirs of Richard Nixon*. For the release of the book in the United Kingdom, however, the main title *RN* was dropped, as Nixon's initials were liable to be confused with the acronym for the Royal Navy. The citation of Nixon's book in Friedman and Friedman (1998, pp. 630, 646) used its U.K. title, rather than the *RN* title used by the American publisher they listed.

[161] *Milton Friedman Speaks*, Episode 6, "Money and Inflation," taped November 7, 1977, p. 27 of transcript.

[162] See Friedman (1977e, pp. 6–7).

1977–1978 was reflected in two different statements he made in a single interview—with NBC news in November 1978. On the one hand, he perceived "a very cheerful picture from the long run, because the real thing I think needs emphasis is that the American people are waking up to what the situation is, and they are going to make it politically profitable for the people in power to change policy in the right direction." (*Meet the Press*, NBC, November 12, 1978, p. 9 of transcript.) This statement was on the right track in foreshadowing a major policy change ahead but missed the fact that what would really drive the change was a doctrinal shift at the policymaker level.[163] On the other hand, in the same interview he stated: "I believe we are set on a road that is almost sure to lead to mandatory wage and price controls, probably early in 1980."[164] This statement was accurate in seeing that as of late 1978, policymakers' thinking about economic relationships was not too different from the thinking that had led to controls in 1971. But it was off-base in presupposing (as did his speculation during the same period that President Carter would impose foreign exchange controls) that the Carter Administration would be unwilling to countenance restriction of aggregate demand in the vicinity of a presidential election year. In fact, once finally persuaded that such restriction was necessary for disinflation, the administration went along with that direction of policy.

In the areas of microeconomic policy and the size of government, Friedman likewise fluctuated between the view that a move to economic liberalization would occur irrespective of the politicians in charge and one in which Carter was a strong major force in the opposite direction. As the United States entered the late 1970s, Friedman saw public opinion as a factor on his side. "Surely it is a healthy thing that the public at large should recognize the defects of governmental arrangements," Friedman said in a television commentary in May 1976. "That is an essential first step toward eliminating the failures—toward cutting the government down to size." (*CBS Morning News*, May 25, 1976, p. 6 of transcript.) Although, Friedman observed in November 1976, Carter had shown himself during the campaign to be a "fairly typical New Deal liberal" (Instructional Dynamics Economics Cassette Tape, 202, November 1976, Part 1), he envisioned circumstances in which Carter would follow policies contrary to New Deal precepts.

A good deal of Friedman's commentary during 1977 and 1978 was in this vein. In November 1977, he ventured to suggest that public-sector actions government were no longer seen as the natural way to address social problems.[165] "People are realizing that government projects are

[163] See especially the next chapter.
[164] *Meet the Press*, NBC, November 12, 1978, p. 8 of transcript. See also *Newsweek*, November 20, 1978.
[165] *Milton Friedman Speaks*, Episode 6, "Money and Inflation," taped November 7, 1977, p. 27 of transcript.

backfiring," he added in December. "The calls for government solutions are declining." (*St. Louis Globe-Democrat*, December 16, 1977). He noted during 1978 that this shift was also occurring among some intellectuals and opinion leaders, such as the neoconservatives, who had a prior record of favoring government intervention.[166]

Yet the same years also saw expressions of pessimism on the part of Friedman regarding the prospect of a move to freer markets. In April 1977 he remarked, "I am innately optimistic, but my mind forces me to be pessimistic about the future of liberty and freedom," adding: "We are on a downgrade." (*Oakland Tribune* (California), April 18, 1977, p. 12.) And, disappointed by how economic policy took shape over 1977, he remarked early the following year: "The policy of the Carter Administration is a policy of continuing and extending governmental controls."[167] The administration's proposed changes to energy policy, discussed under the heading "James Schlesinger" in Section III below, likely formed the clinching evidence to Friedman of this tendency. In addition, the Carter Administration secured increases both in the minimum wage and in Social Security taxes in 1977–1978.[168]

Deregulation and inflation policy

A countercurrent over the same years was the interest that the Carter Administration displayed with regard to some aspects of deregulation. Most notably, Alfred E. Kahn, the Carter Administration's chairman of the Civil Aeronautics Board, facilitated the passage in 1978 of the Airline Deregulation Act (Baumol and Blinder, 1978, p. 501). At the time, Kahn was described in the press as "sometime professor in economics (of the Milton Friedman school of thought), current Chairman of CAB and ranking with Sir Freddie Laker as one of the leading figures of aviation today." (*Interavia*, October 1978, p. 920.)

But, having achieved acclaim in his work in the aviation area, Kahn was promptly moved by Carter into macroeconomic policy—an area in which Kahn, a passionate proponent of cost-push

[166] See Friedman (1978i), as well as Friedman (1978b, p. 20).
[167] *Milton Friedman Speaks*, Episode 15, "The Future of Our Free Society," taped February 21, 1978, p. 17 of transcript.
[168] President Carter signed, on November 1, 1977, a law introducing a phased increase in the minimum wage (Carruth, 1993, p. 733). With regard to the Social Security tax rate, Barro and Redlick (2009, p. 48) find that the average marginal rate rose in 1979, after five years of rough stability, from 4.3 to 6.8 percent, then to 7.2 percent in 1980 and 8.4 percent in 1981. Despite himself having presided over increases in Social Security taxes, President Ronald Reagan highlighted the Carter Administration's prior increases in those taxes. In his first televised debate with Democratic presidential candidate Walter Mondale, on October 7, 1984, Reagan referred to the "Social Security tax [increase] of '77"—which he labeled "huge."

ideas, was decidedly *not* of the Milton Friedman school of thought. Kahn was simultaneously given two posts—adviser to the president and chairman of the Council on Wage and Price Stability—with Carter proclaiming that Kahn would be "my new partner in controlling inflation in this country" (*Dallas Morning News*, October 26, 1978). Friedman lamented the development. Kahn, Friedman observed, had done "a remarkable job," producing "spectacular results" via his promotion of deregulation.[169] But the motivation to assign Kahn, who had no monetary policy authority, responsibilities supposedly associated with the control of inflation was misguided. In administering incomes policy, Friedman remarked, Kahn would henceforth actually act to "stifle the forces of the market," and insofar as Kahn invoked deregulation in the context of fighting inflation, he would be propounding something that was "sheer delusion" (*Newsweek*, November 20, 1978).[170]

The administration's economic insularity

In this connection, it deserves emphasis that, to a very important extent, the Carter Administration economic team operated in a "bubble"—being resistant to, and in some ways oblivious to, the changes in thinking that Friedman had engendered in research circles. The Carter economic officials largely adhered to an economic framework closer to that of the Kennedy-Johnson Administration than that of best-practice macroeconomics of the late 1970s. Friedman complained (*Newsweek*, November 20, 1978) that the Carter Administration seemed to have learned nothing from history on economic matters—but it was also true that its team also appeared to have absorbed only a portion of the changes in professional thinking that Friedman had helped engender in economics over the preceding two decades.

Carter's advisers were, it is true, aware of the permanent income hypothesis and its warning against the effectiveness, in the management of aggregate demand, of fiscal policy-induced changes in transitory income. But, in making the case for the proposed 1977 tax rebate, Carter's

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[169] *Newsweek*, November 20, 1978. See also Friedman and Friedman (1980, p. 200). Friedman had stressed the desirability of airline deregulation on a number of occasions, including in a talk in Detroit in early 1971 (*Detroit Free Press*, March 9, 1971). He would praise the 1978 deregulation as having given rise to lower prices and more air traffic and as having allowed airlines to act more like private-sector firms than they had previously, although he criticized the fact that U.S. airports remained government-run (see Friedman, 1989c, p. 571).

[170] Around the same time, Meltzer (1979, p. 1039) likewise referred to "the vulgar error of recommending deregulation or efficiency gains to reduce inflation." Correspondingly, in a retrospective assessment, Lawrence Summers has referred to the approach to anti-inflation policy dominant in the Carter Administration from October 1978 to the early summer of 1979 in the following terms: "The whole [']Alfred Kahn as inflation czar['] futility was about a foolish conflation between competition and inflation policy." (Lawrence H. Summers, twitter, December 26, 2021.)

economists argued in internal memoranda that the hypothesis' prediction had been refuted by experience earlier in the decade (see Biven, 2002, pp. 63−64). As far as another Friedman contribution—the natural rate hypothesis— was concerned, the CEA's Charles Schultze had observed the debates on the expectational Phillips curve at close hand by seeing empirical Phillips curves debated at successive panels of the Brookings Institution economics group. He came to regard the Friedman-Phelps side as having won the debate (see Schultze, 1996, p. 34) and his late-1970s statements confirmed his rejection of a simple Phillips curve.[171] But the implications for stabilization policy of a unitary coefficient on expected inflation in the Phillips curve were, in effect, nullified by the Carter Administration's predilection toward adherence to a strongly cost-push view of inflation.[172]

All told, the administration's first two-and-a-half years in office were characterized by economic views that rated fiscal policy higher in importance than monetary policy and that downplayed the significance of monetary policy for aggregate demand and inflation alike. "The inflation we are now experiencing has nothing to do with either the money supply or with government spending, according to Carter's advisers," was the hostile, but basically accurate, assessment of one political commentator (*Arizona Republic* (Phoenix), March 7, 1978), while Stuart Eizenstat, a domestic-policy adviser to Carter, would later concede that "[i]t took too long for the president's key economic advisers… to come to the realization that in a high-inflation environment, we had

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[171] On May 14, 1976, Schultze had testified: "Most of the economics profession would agree that holding the rate of adult unemployment at 3 percent would lead to inflation. There is, within the profession, division of opinion about whether the resultant inflation would be high but steady rate or an ever-accelerating rate." (In Committee on Labor and Public Welfare, U.S. Senate, 1976, pp. 142−143.) Although this particular testimony did not arbitrate between the two views, Schultze subsequently indicated that he had himself rejected the simple-Phillips-curve view, when he stated that "pushing the unemployment down through 6 [percent]" via aggregate-demand policy would "in no way under present circumstances [be] likely to speed up the rate of inflation." (From pages 25 and 21, respectively, of his testimony of January 11, 1977, in Committee on Banking, Housing and Urban Affairs, U.S. Senate, 1977c.) This judgment entailed a rejection of the simple Phillips curve's implication that reducing the amount of slack in the economy implied (*ceteris paribus*) a higher inflation rate. In the same vein, on April 13, 1976, Schultze had remarked that economic expansion "could continue at a good clip to get you down to that level of unemployment [6 percent] without reaccelerating inflation." (In American Society of Newspaper Editors, 1976, p. 70.)

[172] This meant that the Carter Administration economists discounted the role of the output gap in the Phillips-curve equation and played up the role played by changes in the intercept and/or the disturbance term. See the preceding discussion, as well as the accounts in Chapters 5 (under "Arthur Okun") and 7 above. Taylor (1997) and Romer and Romer (2002b, pp. 60–63) were highly notable early discussions of the late-1970s U.S. policy atmosphere. Both these discussions appropriately stressed that the Carter Administration's suggestion that negative output gaps did not make for sizable disinflationary process and that this factor was an obstacle to a realization of the imperative of a fundamental shift in monetary policy before 1979.

Note, however, that this chapter's analysis specifically does *not* attribute a belief in a long-run Phillips-curve tradeoff to the Carter Administration's economic team. Such an attribution (made to them by Meltzer, 1981, p. 40, and Goutsmedt, 2022) is not consistent with the view that the Carter team actually espoused.

given too little emphasis to monetary policy."[173]

The state of affairs as of 1978 has been elegantly captured by Brad DeLong as reflecting "the intellectual gulf between Cambridge and Washington" on macroeconomics as of 1978. By "Cambridge," DeLong referred to the MIT-originated macroeconomics textbook of Dornbusch and Fischer (1978). As discussed in the previous chapter, the Dornbusch and Fischer textbook reflected the fact that, as DeLong put it, the "first Chicago School by and large won the day" , in new-generation macroeconomics textbooks of 1978. The first Chicago school of the 1970s, in DeLong's classification scheme, being the positive-economics component of Friedman's macroeconomics—in which monetary policy is central means by which the government can affect nominal aggregate demand and prices, and in which prices are sticky and the output effects of monetary policy dissipate only slowly—as distinct from what DeLong calls the "second Chicago School" of the Lucas rational-expectations work, prices are sticky and the output effects of monetary policy dissipate only slowly. In contrast to this Cambridge-U.S. younger-generation acceptance of Friedman's macroeconomics, the Washington economists influencing the Carter Administration largely adhered to a 1960s-style Keynesianism.[174]

The mindset in the Carter Administration was so tied to an older orthodoxy that, even though Jimmy Carter had shown a personal interest in Friedman's economic views when they got in contact at the end of 1976, his economic team seemed to ignore Friedman's public statements in the early years of the administration. For example, Eizenstat (2018, p. 279) asserts that in 1977 "the harsh medicine of recession… [in order] to lower inflation.... was a policy that no one advised, inside or outside the administration." But, as we have seen, by June 1977 Friedman was, in fact, offering this advice to the president from outside the administration.[175] And also in June 1977, the administration's William Nordhaus (working on energy policy) stated: "I think there are no advocates around, or very few, for complete decontrol of [prices of] old and new oil

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[173] Eizenstat (2018, p. 352). Eizenstat goes on to state that the administration gave too much emphasis to fiscal policy in inflation control. Most of this overemphasis was in 1979, when it turned to fiscal policy restriction as a device against inflation. In 1977 and 1978, the administration gave heavy weight to fiscal policy's influence, but it did so in the context of trying to use stimulative fiscal policy to boost output.

[174] And even Dornbusch, despite considerable convergence to Friedman on analytical issues by 1978, was still differing sharply from Friedman in the early part of the year in his diagnosis of the U.S. economy—with Dornbusch seeing considerable slack and little chance that inflation would pick up. See the previous chapter.

[175] To be more specific: In early 1977, Friedman was urging a gradual economic recovery to take place in the context of a renewed slowing down in monetary growth (see the previous chapter). However, in mid-1977 he was indicating that a midterm recession might be in the president's interest, as that would likely provide a better set of economic conditions in the 1980 election year. By mid-1978, he regarded a recession in 1979 or 1980 as necessary in order to bring inflation down from the double-digit rates that he was already predicting.

and [of] gas."[176] In fact, the latest Nobel winner in economics—Friedman—had advocated that policy repeatedly, in his *Newsweek* column and elsewhere.

Ronald Reagan and the coming 1980 presidential contest

Nordhaus had also not mentioned Ronald Reagan, even though Reagan, too, was a prominent advocate of energy price decontrol.[177] Nordhaus may have regarded Reagan as no longer a major political figure, in light of his 1976 primary defeat at the hands of Gerald Ford.[178] As discussed in Chapter 5, following the November 1976 election Friedman, too, saw the Reagan presidential candidacy as something to be regarded as strictly being in the rear-view mirror. Consistent with this attitude, in the following year he emphasized factors different from specific candidates in hopes for achievement of a policy change. "It's nice to elect the right people, but that isn't the way you solve things," Friedman argued. "The way to solve things is by making it politically profitable for the wrong people to do the right things."[179] These remarks were a counterpart to the sentiment held by Friedman at the time, one noted above: that the introduction of policies he favored did not, and perhaps would not, have to wait for the ascendancy of policymakers who were naturally sympathetic to those policies.

But, as also indicated above, President Carter's adherence to largely interventionist policies was something that had to give Friedman pause in his downplaying of the identity of policymakers. Friedman's assessment had been faulty in a further respect: Reagan had not put presidential aspirations behind him. In an interview on *60 Minutes* a month after the election, Reagan contended that he could have won the election if he had been the GOP candidate.[180] In another television interview five months later, Reagan was asked: "In 1980, you will be 69 years old.

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[176] From Nordhaus' testimony of June 1, 1977, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1977a, p. 117).
[177] It was noted in the press during 1977: "Reagan has been an outspoken advocate of removing all federal price controls on oil and natural gas as a means of stimulating domestic energy production." (*Boston Herald American*, December 4, 1977. See also the discussion titled "James Schlesinger" in Section III below.) Friedman himself mentioned Reagan's support for energy-price decontrol in *Saturday Evening Post* (May/June 1977, p. 20) and in *Newsweek*, October 16, 1978.
[178] Thanks to his having defeated Reagan in 1976, former president Ford was as of 1977 was formally still the national leader of the Republican party.
[179] *Milton Friedman Speaks*, Episode 6, "Money and Inflation," taped November 7, 1977, p. 25 of transcript. Friedman had made a similar comment in his appearance on *The Open Mind*, PBS, May 31, 1977, pp. 14–15 of transcript, and in *Milton Friedman Speaks*, Episode 1, "What Is America?," taped October 3, 1977, p. 26 of transcript.
[180] See *Manchester Union Leader* (New Hampshire), December 6, 1976, and *Dallas Morning News*, December 9, 1976.

Are you washed up as a presidential candidate or a candidate for any other public office?" "I don't know," Reagan replied. "I have given no thought to 1980 in advance, and that would be up for the people to decide, if age is going to be a factor." (*Meet the Press*, NBC, May 1, 1977, p. 4 of transcript.) This reply understated the amount of preparation that Reagan was already engaged in. A retrospective two years later assessed that, in effect, "Reagan never stopped running for president" after 1976 (*Detroit Free Press*, March 15, 1979).[181]

Furthermore, the prospect of a successful campaign against Carter was improving. This development was contrary to expectations. Notwithstanding the fact that Carter's election victory in 1976 had proved to be narrow, he had entered office among widespread expectations that he would be reelected in 1980. At this early stage of his tenure, the damage to the Republican party arising from the Watergate scandal and the poor national economic performance of 1973–1975, combined with the tendency for incumbent presidents to be reelected, seemed to put Carter very much in the box seat when it came to securing a second term. Immediately after the 1976 election, *Time* magazine had noted that, although "the Republicans' fairly good showing in races for the Senate, House, and governorships gave them hope for the future," its next presidential victory was unlikely to be just four years away: "Probably not for 1980—unless Jimmy Carter turns out to be a singularly inept President, suffering foreign reversals, mismanaging the domestic economy, and defaulting on his many reform promises." (*Time* magazine, November 15, 1976.)

From the perspective of Friedman and many others, Carter's performance in office largely conformed to the negative scenario that *Time* had painted as a remote possibility. As noted earlier, Friedman saw merit (and longer-term electoral benefit) in a Carter default on some 1976 promises. But Friedman felt that Carter, to a large degree, did not actually depart sufficiently from his election-year economic stance. In particular, and notwithstanding his withdrawal of the tax-rebate proposal in April 1977, Carter continued to favor stimulative economic policies after the election and did so through late 1978.[182] And, separate from the issue of Carter's specific election promises, the overall tone emanating from his administration did little to consolidate his electoral support. Although it was the second half of Carter's term (1979 to 1981) that became remembered in retrospect as featuring the bulk of Carter's problems in foreign policy and

[181] In a similar vein, a November 1977 news report had been titled "Reagan Committee Is Staffed, Waiting" (*Milwaukee Journal*, November 29, 1977).
[182] See the previous chapter, as well as the discussion above of the Carter Administration's international-coordination initiatives.

economic policy alike, he had already had a steep decline in popularity over 1977 and 1978, and prospects for a 1980 Republican presidential victory heightened. Ronald Reagan's maintenance of a basic campaign organization after 1976 had therefore stood him in good stead.

As it happened, during 1977, though he indicated that "I earn my living out on the speaking circuit, doing a radio commentary and a once-a-week newspaper column" (*Meet the Press*, NBC, May 4, 1977, p. 4 of transcript), Reagan was for a time actually a nominal colleague to Friedman of sorts, as he had been named an honorary fellow at the Hoover Institution (*San Francisco Chronicle*, May 4, 1977). However, Reagan's appointment, and most of his activities in connection with it, occurred before Friedman really took up his own Hoover Institution post in the fall of 1977. Reagan did not live in the Bay Area, and he had an assigned office at the Hoover Institution only fleetingly.[183] Friedman therefore saw little of Reagan in the year after the 1976 presidential election and was not privy to the former governor's enduring presidential ambitions. This situation began to change on November 6, 1977, when Friedman and Reagan both attended an evening awards ceremony in Los Angeles (*Los Angeles Times*, November 7, 1977). At the event, the Friedmans were struck by the response generated by a throwaway remark by Milton Friedman about the failure of Ronald Reagan's 1976 presidential bid. Nancy Reagan's reaction, in particular, made the Friedmans aware of the resilience of the Reagans' White House ambitions, and it pointed toward the likelihood that there would be a new Reagan campaign in 1980.[184]

II. ISSUES RELATED TO DEBATES ON REGULATION AND AGGREGATE SUPPLY, 1977–1978

THE TAX REVOLT

In late 1977, Ronald Reagan was quoted as saying that the Republican party now had "the best issue we've had in a long time—and it's one on which a majority of working Americans will agree with us. The issue is taxes." (*Kansas City Star* (Missouri), December 18, 1977.) Reagan had accurately identified a policy area—that of tax cuts—that would figure heavily in his campaign for president in 1980 and even more so among the economic policy steps taken during his presidency starting in 1981. For the immediate year ahead of 1978, however, neither Reagan

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[183] Martin Anderson recalled that Reagan "had an office there [only] for a short period of time." (*Booknotes*, CSPAN, August 16, 1992.)
[184] See Friedman and Friedman (1998, p. 389).

nor the contest for president in 1980 dominated the terms of the U.S. debate on taxes. This debate instead proceeded primarily in the areas of state politics and Congressional exchanges. Each of these areas was marked by a standout development in 1978, on which discussion was centered.

As far as state politics was concerned, the major development was on June 6: the Californian electorate's passage by referendum—following a multi-month election-style campaign on both sides of the debate—of "Proposition 13." This measure capped the property tax rate at 1 percent of the value of a house, while limiting the amount by which the property's assessed value could be raised in any year (*Columbus Dispatch* (Ohio), June 15, 1978*a*). In effect, the measure amounted to a large cut in state property taxes: ahead of its passing, the proposed change was seen as likely to cut California's property tax intake by roughly 60 percent, from about $12 billion to $5 billion (*New York Times*, June 4, 1978, p. 1).

In the area of national politics, 1978 saw electoral and legislative debate—carried out through Congressional hearings and legislative sessions, the media, and the promotion of the bill by Republicans in the campaign for the midterm elections in November 1978—on the Kemp-Roth bill (due to Republicans Congressman Jack Kemp, of New York state, and Senator William Roth, of Delaware). This bill—originally introduced in mid-July 1977—proposed across-the-board reductions, of about 30 percent, in individual and corporate federal income tax rates.[185]

A syndicated article, used as the basis for many U.S. newspaper items during the early weeks of the year, foreshadowed the state-level dimension of the 1978 tax debate. In the January 1978 syndicated article, Friedman himself was quoted as remarking: "There's an uproar across the country. The undercurrent against taxation has been nothing less than astonishing." (*Vineland Times Journal* (New Jersey), January 9, 1978.) One of the versions of the article, in the edition of January 11, 1978, in Wisconsin's *Eau Claire Leader-Telegram*, was titled "State Tax Revolt"—and so used the "tax revolt" formulation that would come to encapsulate the debate. This term also appeared in various reports around the country on California's approaching Proposition 13 referendum, such as "National Tax Revolt Is in the Making" (*Sunday Plain Dealer* (Cleveland, Ohio), June 4, 1978) and "Tax Revolt Comes to a Vote" (*St. Petersburg Times* (Florida), June 4, 1978). Friedman, too, used the term in his talk in Scotland in April 1978, after the Proposition 13 vote had already been scheduled: "The public at large is restive, a

[185] On the date of the bill's introduction, see Kiefler (1979, p. 13).

tax revolt is brewing everywhere."[186]

In the "Yes" campaign for California's Proposition 13 campaign, the major public face on the side of its advocacy was Howard Jarvis. Jarvis was president of an apartment owners/landlords association (*Wisconsin State Journal*, June 4, 1978, p. 1), and he had, with Paul Gann, advanced the constitutional amendment and obtained the requisite amount of voter signatures for it to be put to a state-wide vote. On the national stage, however, it was Friedman who was the most eminent figure on the side of the tax revolt at the time of the vote on Proposition 13. For example, a news service report in late May. stated that "economist Milton S. [sic] Friedman" was the "most prominent" supporter of Proposition 13 (*Florida Today* (Brevard County), May 29, 1978, p. 12A).[187] Likewise, an Associated Press report on the tax-revolt campaign described Friedman as "[o]ne of the movement's most eminent ideologists" (*St. PetersburgTimes* (Florida), June 4, 1978, p. 19A), and the *New York Times*' report of the same vintage (June 4, 1978, p. 55) stated: "The movement has its high priest, Milton Friedman, the Nobel Prize-winning economist." About a week later, in lamenting the passage of Proposition 13, John Kenneth Galbraith was quoted regarding Friedman's role in the case for a "Yes" vote: "He's been the guru." (*The York Dispatch* (Pennsylvania), June 12, 1978.)

Friedman had been involved in a number of activities over the first half of 1978 associating himself with the tax-revolt movement. When Proposition 13 was confirmed as going ahead in a state-wide vote, Friedman produced a *Newsweek* column (April 10, 1978) in which he indicated his support for its passage in April 1978 and noted his own status as a founding member of the National Tax Limitation Committee (one—but probably not the single most prominent—groups formed to push for tax reduction).[188] He made a return to the greater Chicago area to be the showcase speaker at the committee's first national conference on May 21 in Lincolnshire, Illinois (*Register-Republic* (Rockford, Illinois), May 22, 1978).[189] And although Friedman was mainly away from California during the Proposition 13 campaign, he gave numerous media interviews arguing for a "Yes" vote and appeared on radio and television advertisements in the California area, urging passage (*Los Angeles Times*, May 15, 1978; *New York Times*, June 4, 1978, p. 55; *The York Dispatch* (Pennsylvania), June 12, 1978). An article on tax limitation, derived from his

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[186] Friedman (1978e, p. 13). See also the version of the same remarks in *The Listener* (London), April 27, 1978, p. 528.

[187] In fact, as the *New York Times* (October 15, 1976*b*) noted of Friedman, "he has no middle name."

[188] Another group, the National Taxpayers' Union, was likely more prominent.

[189] As the account in the *New York Times* put it (June 4, 1978, p. 55), Friedman "was the main drawing card at the Chicago [area] meeting."

May 21 speech, was scheduled for summer publication in the Heritage Foundation's *Policy Review* periodical, and once Proposition 13 was passed, Friedman rapidly revised the draft, which promptly appeared in condensed form in the *San Francisco Chronicle* on June 10 and subsequently in other newspapers.[190] Friedman also wrote a separate *Newsweek* column (June 19, 1978) on the passing of Proposition 13.

Friedman's association with the tax-revolt movement was reflected in the issuance, later in 1978, of a new book: The Fisher Institute, a Texas-based small-press publisher of free-market books, collected Friedman's Policy Review article and sundry other items under the title *Tax Limitation, Inflation and the Role of Government*.[191] A quickie paperback book on the tax revolt by Engelmayer and Wagman (1978) also included Friedman's name on its cover as a selling point.[192]

Yet Friedman's image as the leader of the tax revolt proved highly ephemeral: it had dissipated by the time these books appeared. Indeed, it was out of date even in mid-July 1978 when a Wa*ll Street Journal* article on Friedman described him in its headline as a "tax revolutionary" (July 17, 1978). In the course of summer 1978, Friedman had in effect been overshadowed by other, more emphatic proponents of tax cuts. The emerging popular/public-policy movement of "supply-side economics," consisting of various figures in journalism, economics, and politics,

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[190] For the condensed version of the article, see *San Francisco Chronicle*, June 10, 1978, *Lincoln Journal-Star* (Nebraska), June 12, 1978, and *Columbus Dispatch* (Ohio), June 15, 1978. (The last of these items traced the article to Friedman's May 21 speech.) Friedman's *Newsweek* columns were typically finalized a week or more before publication. So his publication in the *San Francisco Chronicle* on June 10, of an item on which he signed off during the June 7–9 period, represented one of the most rapid moves of his writings into print.

[191] This book (Friedman, 1978b) included, alongside Friedman (1978a), his February 1977 "The Future of Capitalism" speech and reprints, with some omissions, of three Institute of Economic Affairs pamphlets published in the United Kingdom in the 1974–1977 period (two of which had actually already seen print in the United States). The Fisher Institute finalized the book in October 1978 (see Friedman, 1978b, p. IV). This slender book may have not really been available for purchase until early 1979: the meager number of reviews of the book received started to appear early in that year (see *Dallas Morning News*, January 26, 1979; *Lake Geneva Regional News* (Wisconsin), February 1, 1979). These early reviews also indicated that there had been both paperback and hardback versions of the book and that it was available primarily through mail order, rather than bookstores. Macroeconomists who cited the book during the first two decades after it appeared included Robert Hetzel (1997, p. 55) and Alan Stockman (1982, p. 18).

[192] This contained a reprint (see pp. 264–269) of Friedman (1978a). The inclusion of this item justified the claim on the book's cover regarding the book's contents: "Including Milton J. [sic] Friedman[,] Nobel Prize winning economist[,] on tax limitation." As its reproduction in this book and its original appearance in *Policy Review* indicated, Friedman (1978a) amounted to a piece of public-policy popular writing, rather than a research contribution. The article did, however, subsequently receive citations in the economic-research literature—an early example being the reference to it in Bennett and DiLenzio (1982, p. 18), who also patterned the title of their article on that of Friedman's. Even before this, the *Journal of Economic Literature* (1979, p. 765) had included Friedman (1978a) when listing recent research on domestic monetary and fiscal matters.

including Congressman Kemp and Friedman's former colleague Arthur Laffer, had become the main group articulating the tax-cut cause. And they did so in a manner that made cuts a more central policy aim than Friedman did, while also making stronger predictions regarding the effects of tax cuts than Friedman himself could support.

The end result was that, even though Friedman remarked publicly in a speech in California as early as March 1976 that there should be a "taxpayers' revolt across the country, seeking a constitutional amendment that would set a limit to the fraction of income that can be taxed away" (*Stanford Daily* (California), March 11, 1976, p. 19), he would end up being remembered as far less central figure in the state and national tax-cutting movement of 1978−1981 than such individuals as Reagan, Jarvis, Kemp, Laffer, and the *Wall Street Journal*'s editor, Robert Bartley. And the tax revolt would be associated with a brand of economics (supply-side economics) separate from his own.

In retrospect, it is evident that the *New York Times*' description, on June 4, 1978, of Friedman as the high priest of the tax-revolt movement marked the peak of his profile in the movement. The path that subsequent developments would follow would be presaged by the appearance, on the same day (June 4, 1978), of an article in the *Atlanta Constitution* that—in making the case for tax cuts—expounded the "Laffer curve" to its readers. The *New York Times* itself, in an anti-tax-cut editorial on June 19, would concern itself with the Laffer curve—and the editorial would label Laffer "the leading academician of the tax-cut movement," while not mentioning Friedman once.[193] And when Walter Heller wrote a *Wall Street Journal* piece (August 7, 1978) criticizing proposals for a large federal tax cut, he directed his fire at the supply-side movement, particularly Kemp and Laffer, and did not refer to Friedman at all.[194]

Diverging from other tax-cut proponents on the analysis of tax cuts, and now rarely present in

[193] At the time, Laffer was a professor at the University of South California, having left the University of Chicago's business school in 1976. Although, by 1978, Laffer had mainly moved to public-policy activity in his non-teaching responsibilities and was not primarily engaged in contributing to the economic-research literature, he did have a book review published in that year's August issue of the *Journal of Political Economy*. See Laffer (1978).

[194] A largely favorable account of supply-side economics (including using that terminology) that appeared in the press at the time was by Canadian economics columnist Peter Brimelow (in *Financial Post* (Toronto), July 29, 1978). Brimelow, who would publish extended interviews with Friedman in later years, did not mention him in this column on the tax-cut debate. Although Brimelow's column was critical of Laffer, it praised Kemp as well as Jude Wanniski, formerly of the *Wall Street Journal*, whose book (Wanniski, 1978) Brimelow labeled a "highly sophisticated discussion." Brimelow also endorsed the position taken by many supply-siders that Robert Mundell's past writings had laid down "the supply-side academic foundations." (A recent example along these lines was Lawrence Kudlow's statement: "Mundell combined with Art Laffer at the University of Chicago to invent the supply-side policy mix." See *Kudlow*, Fox Business Channel, April 5, 2021.)

Washington, D.C., Friedman would fade as a key figure in the tax-revolt movement. His dissent from major claims of the movement was a focus of his *Newsweek* column of August 7, 1978—a column that, as discussed below, supported a large federal income tax cut while, in effect, taking issue with much of the rationale for it offered by Kemp, Roth, and Laffer.

Furthermore, supply-side figures, including Kemp, Laffer, and the *Wall Street Journal* editorial page, would often revel in emphasizing their disagreement with Friedman on a variety of macroeconomic points. Some of these points of disagreement would have very little to do with tax cuts *per se*.[195] With regard to tax cuts, however, Friedman's own writings and statements indicated that there indeed existed a threefold rift between himself and the emerging leaders of the tax revolt: on the appropriateness of directing tax cuts at the property tax, as Proposition 13 had done; on whether a boost to tax revenues should be regarded as a likely effect of income-tax-rate cuts, as major proponents of the Kemp-Roth bill were claiming; and the status of tax limitation *vis a vis* public-spending limitation as the key measure that would improve U.S. aggregate supply.

As will be seen, these disagreements with the main messages of the tax revolt had been put on record by Friedman by early 1978. But he repeated them over the course of the year. His disagreements with tax-revolt leaders were notable, as, unlike Heller, they came from someone whose anti-Keynesian position might be expected to make him treat those of a supply-side persuasion as allies. Over time, Friedman's critical comments regarding the cases being made for tax cuts achieved wide attention—in effect, achieving the pre-digital-era equivalent of going "viral."

Property taxation and the mix of taxes

During the Proposition 13 campaign, Friedman was cited, including by Jarvis, as having predicted that the measure's tax cut would promote not only the construction industry, but also total employment and economic activity in the state (*Ithaca Journal* (New York), June 1, 1978; *Wisconsin State Journal* (Madison), June 4, 1978, p. 1). The attribution to Friedman was accurate enough. Indeed, as far back as 1972, he had called for a tax cut in California and indicated that such a move would boost the jurisdiction's level of employment (*Pacific Business*, September/October 1972, p. 8). It was, however, not the case that Friedman was an enthusiast

[195] Major examples of such disagreements—in monetary policy and international economics—are discussed in other chapters, rather than in the discussion that follows.

for the specific concern of Proposition 13: cutting property taxes. As Friedman put it in the leadup to the vote, "it's not what I'd most prefer to see" (*New York Times*, June 4, 1978, p. 55).

Notwithstanding his support of Proposition 13, Friedman doubted that the initiative shifted the structure of taxes in a desirable direction. "There's a sense in which all taxes are antagonistic to free enterprise—and yet we need taxes," he had remarked in February before the ballot was scheduled.[196] And in balancing these considerations, Friedman did not think property taxes should be high on the list of taxes to cut, and he accordingly wrote in April that among the "many defects" of Proposition 13 was the feature that it sought to reduce "only the property tax, which is by no means the worst tax." (*Newsweek*, April 10, 1978.)

Indeed, on multiple occasions in the years up to 1978, and again in the months after the passing of Proposition 13 in June that year, Friedman had suggested that, among taxes, property taxes were among the imposts that had the least distortionary effect on the allocation of resources. "[T]he property tax… is by no means the worst tax," he wrote in his post-vote *Policy Review* piece.[197] In his taped commentary series eighteen months earlier, Friedman had gone so far as to say: "The property tax in the United States is probably the best tax we have on the book[s]." (Instructional Dynamics Economics Cassette Tape 204, December 1976, Part 1.) These statements indicated that he believed that the theoretical virtues of property tax that he had earlier acknowledged largely applied to property taxes in practice.[198]

Friedman therefore had reservations about the form in which Proposition 13's architects, and subsequently California's voters in passing the measure, had expressed their resistance to higher taxes. In considering the recent backlash against taxes, Friedman acknowledged that the property tax had been an important catalyst. But the unpopularity of the property tax, he believed, did not really reflect economic damage due to the tax but, instead, the fact that it was paid in the form of an often-substantial single check to the government (as opposed to taxes paid via withholding at source, via instalments of provisional payments, or through the purchasing of goods at prices inclusive of taxes).[199] Another factor, highlighted in the media at the time (for

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[196] *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978, p. 25 of transcript.
[197] Friedman (1978a, p. 7). Also in Friedman (1978b, p. 14).
[198] Speaking at a more abstract level, Friedman had still earlier stated (Instructional Dynamics Economics Cassette Tape 37, early November 1969), "There is no tax that I know of which has less adverse effect on anything than taxes on [the value of] pure land." On that occasion, he cited the characteristic that the tax's imposition would likely leave unaffected the equilibrium quantity of land and would instead be felt only in the sale price and rental value of land.
[199] *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978, p. 25 of transcript.

example, *Today* (Brevard County, Florida), May 29, 1978, p. 1A, and *St. Petersburg Times* (Florida), June 4, 1978, p. 18A), was that the pace of increase in many Californian homeowners' tax liabilities had stepped up sharply in the period up to 1977, as rising house prices had led to very sharp consecutive-year hikes in property tax assessments.

Henry George's argument, in his book *Progress and Poverty* (1879), in favor of property tax had left a strong impression on Friedman.[200] "In my opinion, and this may come as a shock to some of you, the least bad tax is [on] the unimproved value of land, the Henry George argument of many, many years ago," Friedman told an audience in early 1978. Furthermore, Friedman suggested that he might prefer the George scheme even to the flat-income-tax scheme he had proposed in *Capitalism and Freedom*.[201]

Friedman's endorsement of the property tax did indeed have shock value—to judge by the extent to which he was subsequently quoted on the subject. In particular, a set of Friedman comments that appeared in a syndicated newspaper article in late 1978 (see *The Times Recorder* (Zanesville, Ohio), October 5, 1978) included the remark: "In my opinion, the least bad tax is the property tax on the unimproved value of land—the Henry George argument of many, many years ago.

As already indicated, that endorsement became an example of a quotation that went "viral" even before the age of the worldwide web. The sentence would be quoted far and wide, including in the United Kingdom's *Journal of the Royal Society of Arts* in January 1984 and in Gregory Mankiw's *Principles of Microeconomics* textbook (2004, p. 168). Friedman reaffirmed his enthusiasm for the principle of land tax in later years—observing, for example, in April 1981: "I am in favor of a greater use of property tax, land value taxation."[202]

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[200] Friedman mentioned George's argument in the aforementioned commentary in Instructional Dynamics Economics Cassette Tape 37 (early November 1969).
[201] *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978, p. 25 of transcript. However, in comments appearing later in 1978, discussed further shortly, Friedman indicated that "my ideal system… would contain an income tax" alongside the property tax (*The Times Recorder* (Zanesville, Ohio), October 5, 1978). He referred in 1981 to "eliminating many kinds of taxes and giving an important role to [taxing] site value." (In Friedman, Porter, Gruen, and Stammer, 1981, p. 33.)
[202] In Friedman, Porter, Gruen, and Stammer (1981, p. 33). In the same discussion, however, Friedman mentioned (pp. 33, 39) practical problems that would be associated with the implementation of property tax and warned against "carrying site value taxation to its full conceivable possible extent" (p. 33). Likewise, in New Zealand some days after these remarks, Friedman (1981b, p. 16) observed, somewhat tentatively: "One can make an argument for… a tax on the site value of land." A quarter-century later, shortly before his death, Friedman named the property tax as a type of tax that found favor with him (*San Jose Mercury* (California), November 5, 2006).

The national tax revolt

The fact that the California debate had been focused on property taxes bolstered the criticism that the tax revolt was being generated by parochial (and well-to-do) interests rather than by popular feeling. However, there was macroeconomic substance behind the reaction against taxes in the United States during the late 1970s—a fact evident when considering the behavior of the federal tax burden during the decade. The average marginal tax rate in the United States, whether measured as the overall rate or the federal individual income tax rate, had declined in 1971 by about 1 percentage point (Barro and Redlick, 2009, Table 1, p. 48). Subsequently, however, as Barro and Redlick (2009, p. 10) recorded: "A period of rising federal income-tax rates prevailed from 1971 to 1978, with the [overall average marginal rate] increasing from 22.7 percent to 28.4 percent."

By the time the rate was reaching this level, there was evidence of an emerging change in attitude toward tax reduction in national political discourse. As the Friedmans would recall in an account written in the mid-1980s, once Proposition 13 succeeded in getting the "taxpayer revolt… thrust into national prominence," the associated change in the political climate started "making it politically profitable to try to reduce taxes."[203]

This development at the national level could, in turn, be seen as in part being the cumulative effect of more gradual changes in public opinion that had taken place in the course of the decade. In early 1971, Friedman had wondered why tax cuts were not more politically popular in the United States.[204] Over the 1970s, however, as the decided increase in federal tax rates took place, he detected a change. He highlighted what he saw as an early sign in this direction: the 1972 electoral defeat of Illinois' Republican governor Richard Ogilvie, who had increased state income taxes. Ogilvie's fate, Friedman believed, would discourage President Nixon from proposing tax increases (*Evening Times* (Trenton, New Jersey), November 28, 1972). Although, in a further development in state politics, 1973's California Proposition 1—jointly constraining taxes and public spending—had been defeated, Friedman in early 1975 ventured the judgment

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[203] Friedman and Friedman (1985, p. 44).

[204] In Instructional Dynamics Economics Cassette Tape 66 (January 27, 1971), he observed: "Why is it… that cutting taxes is not a politically attractive proposition…? … [Y]ou would think offhand that the most attractive thing a politician could do would be to promise to cut taxes." In Instructional Dynamics Economics Cassette Tape 67 (February 10, 1971), Friedman added that "it's a mystery to me why you have citizens of Chicago anxious to have the federal government impose a dollar of taxes on them so that they can get back 90 cents [worth of government spending]—and yet that's the way they behave."

that policymakers were underestimating the reaction that middle-income taxpayers would give to the increase in the tax burden they had incurred due to the increased size of the public sector (Instructional Dynamics Economic Cassette Tape 163, February 1975, Part 1). And, after the passing of Proposition 13, he suggested that "the political market may have changed" in favor of lower taxes and smaller government budgets (*Newsweek*, June 19, 1978).

The focus of the national tax revolt would be successive versions of the Kemp-Roth tax bill—a version of which would, as noted, ultimately become law as the Reagan tax cut of 1981. Although he supported both the Kemp-Roth bill in 1977–1978 and its later legislated version, Friedman would quickly dispute the economic rationale for the bill and so would create the second of the major rifts between himself and supply-siders.

He and the supply-siders would, nevertheless, have common ground during these years in being critical of the Carter Administration's position on taxes.

The Carter administration and the federal tax system, 1977–1978

With regard to the federal tax system, there had been some promising signs, from Friedman's perspective, in Jimmy Carter's public statements. Carter's reference during the 1976 primary campaigns to U.S. tax arrangements as "a disgrace to the human race" (*San Antonio Light* (Texas), April 28, 1976) closely matched Friedman's own observation in the same period that the American tax apparatus was "an ungodly mess" (*People Weekly*, April 5, 1976). Carter's statement would be favorably quoted by the Friedmans even in the mid-1980s, well after he had left office.[205]

Furthermore, Friedman had long focused on the removal of a myriad of tax deductions as a key part of a program of desirable tax reform, and he had reemphasized this in a *Newsweek* piece (May 12, 1976). Likewise, Jimmy Carter's criticism of tax deductions via his imagery of a "three-martini lunch," had, the *Detroit Free Press* later editorialized (August 14, 1978), "succeeded in dramatizing the need for reform, simplification and equalization" of the tax system. In this vein, Carter's then-adviser Lawrence Klein had remarked during the 1976 campaign: "We're looking for a total overhaul of the system, eliminating many present loopholes, injustices, shelters, and deductions." (*The Sunday Sun* (Baltimore), July 25, 1976, p.

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[205] See Friedman and Friedman (1985, p. 65). (That discussion cited *New York Times*, June 11, 1976, another place in which the Carter quotation appeared.)

K7.) A few weeks into his time in office, on February 10, 1977, Carter remarked: "The next move will be comprehensive income tax reform. This will be headed by the Secretary of the Treasury, Mike Blumenthal. And before the end of this year, we will be ready to recommend to the Congress a comprehensive, overall tax reform proposal."[206]

No such major streamlining of the tax code materialized in Carter's tenure, and the Friedmans would later contrast another, earlier occasion when Carter promised "comprehensive tax reform" with his actual record as president.[207] To Friedman, this outcome was politically inevitable. As he saw it, the *quid pro quo* of removing deductions should be across-the-board cuts in tax rates. And he did not see the two sides of politics agreeing to do this—largely because each feared that the other side would renege by later reintroducing either higher rates or a system of widespread deductions. Consequently, around the time when a *People Weekly* interview with Friedman characterized him as wanting to reform taxes, Friedman made his own pessimism about the matter clearer via a *Newsweek* column titled "Tax Reform: An Impossible Dream" (April 12, 1976). In a speech delivered in February 1978, Friedman reiterated his view that major tax reform was not currently a realistic goal.[208]

Carter did sign into law a bill titled the Revenue Act of 1978 that ostensibly was the culmination of his administration's 1977−1978 reform efforts.[209] But Carter himself had great misgivings in signing the bill, in part because of his awareness that had did not actually meaningfully remove tax loopholes.[210] Consistent with Friedman's analysis, and in contrast to the political climate that would be associated with Reagan's ascendancy, an obstacle to reform was that the Carter Administration was not, in the end, very interested in lowering marginal tax rates—and so had little to offer in exchange for securing the removal of loopholes. The new law did, however, reduce the capital gains tax. Friedman had cited the prevailing capital-gains-tax rate as one of the least controversial cases of a high rate lowering both tax revenue and the taxed activity (*Newsweek*, August 7, 1978). In contrast, the capital-gains-tax cut's inclusion gave President Carter's misgivings when he signed the bill into law (see Eizenstat, 2018, pp. 316–318).

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[206] See Carter (1977a).
[207] Friedman and Friedman (1985, p. 65) observed that Carter "pledged 'comprehensive tax reform'—a pledge… that he was unable to redeem."
[208] *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978.
[209] See Romer and Romer (2009, pp. 64−65).
[210] Carter's reservations, however, also involved fear that the tax cut would exacerbate inflation. Although Eizenstat (2018, p. 318) describes such fears as "prescient," they also exemplified the thought patterns of the administration, noted in Section I above, that focused on fiscal policy rather than monetary policy as the main policy measure capable of influencing aggregate demand.

Correspondingly, the administration had little interest in cutting other tax rates associated with investment income. This was an attitude that would prove politically costly. A selling point made regarding the Kemp-Roth bill was that it would lower the top income tax rate from 70 to 50 percent (see, for example, *National Post* (Toronto), July 29, 1978). Friedman himself stated with regard to federal income tax that "the maximum rate is 70 percent" (*Newsweek*, April 12, 1976).[211] Similarly, the tax cut enacted in the first Reagan term, and modeled after Kemp-Roth, was often said to have lowered the top marginal rate on income from 70 to 50 percent. These statements were true, but only because the tax rate on *dividend and interest income* was 70 percent. The top marginal rate on *labor income* had actually been lowered to 50 percent by law in 1969.[212] The proposed Kemp-Roth tax bill and the actual 1981 Reagan tax cut therefore did not move the top rate on labor income any lower than the rate already in force since the early 1970s.[213] But although, early in the life of the Carter Administration, Secretary Blumenthal had publicly criticized the 70 percent top marginal rate on investment income and stated that the administration might recommend bringing the rate down to 50 percent, it did not proceed to do so, and so it left office with the top marginal rate on investment income still at 70 percent.[214] By allowing the two top income tax rates (on labor income and investment income, respectively) to be so different, the administration underestimated the political damage associated with a headline top income tax rate of 70 percent and the political capital this gave its critics who could, as already indicated, make the technically-true claim that the Kemp-Roth bill proposed to lower the top marginal tax rate on income from 70 percent to 50 percent.

The administration also underestimated the extent to which opinion had turned against tax systems so graduated that they included a 70 percent rate on any class of income. Indeed,

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[211] Friedman also gave 70 percent at the top income tax rate in Friedman (1977i, p. 50) (see also Friedman 1978b, p. 75; 1991a, p. 155), in *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978 (p. 14 of transcript), and in the condensed version of this lecture that appeared exactly a year later in *San Francisco Chronicle*, February 6, 1979. However, in debating Friedman on television in 1980, Peter Jay referred (*Free To Choose* (U.S. television version), PBS, Episode 5, "Created Equal," February 15, p. 12 of transcript) to the U.S. federal tax system's "maximum personal rate [of] fifty percent"—here referring to the top marginal rate on labor income.

[212] Barro and Redlick (2009, p. 11) give the lower maximum earned-income marginal rate as 60 percent, introduced in 1971. However, the 60 percent rate prevailed only briefly. The end of the year saw the maximum rate actually brought down to 50 percent, completing a reduction (from 70 percent) brought into law in 1969 (see *Federal Reserve Bulletin*, June 1973, p. 401, and Surrey, 1973, p. 280).

[213] As Brooks and Eckstein (1978, p. 51) observed: "Kemp-Roth would cut the top rate from 70 to 50 percent, though it would not cut the top rate on 'earned' income at all." This fact was obscured in much other contemporaneous discussion.

[214] Blumenthal had stated: "When the marginal tax rate gets up to 70 percent, people say, 'What's the point?'" He added that efforts to avoid the 70 percent rate involved decisions to deploy savings "in strange ways… that are not socially useful." (*Washington Star* (Washington, D.C.), June 19, 1977, p. A1.)

Blumenthal actually stated that an aim was to make the tax system "somewhat more progressive" (*Kansas City Times* (Missouri), January 12, 1978). In contrast, Friedman's judgment that the tax system was already too graduated (*Newsweek*, December 19, 1977) proved to be a sounder assessment of the direction of economic and political thinking in the country.

More generally, the administration did not really reduce taxes, despite passing a tax-cut bill. Before Carter took office, Friedman had warned that, as the federal income tax scales were not indexed, a major cut in recorded tax rates would likely not forestall an increase in the rates taxpayers faced (*Newsweek*, January 17, 1977). Upon seeing Carter's income tax proposals, Friedman's verdict was that they, indeed, did not fully offset bracket creep and the administration's other tax increases: "the bottom line is that *President Carter and the Congressional leadership are proposing to raise, not lower, the total tax burden on the American people*."[215] The factors he cited in this late-1977 column were, in the event, magnified by the take-off in U.S. inflation during 1978 and by the fact that the tax-cut bill was not finalized until late in the year. Consequently, Blumenthal's stated intention of "permanent, across-the-board tax cuts" (*Kansas City Times* (Missouri), January 12, 1978) was, in effect, not met, and the administration found itself perceived as in the anti-tax-cut corner of the debate.

Federal tax rates and revenue: the debate over the economics of Kemp-Roth

It was in this atmosphere that the Congressional leadership of the Republican party had endorsed the Kemp-Roth bill and its proposal to cut federal income taxes by approximately 30 percent (*Manchester Union Leader* (New Hampshire), October 6, 1977). The Republicans were in the minority in both houses of Congress, and the bill, as yet, lacked much bipartisan support. It therefore had no chance of reaching President Carter for his signature (or veto) during the 1977−1978 Congress. But it featured heavily in the 1978 Congressional election campaign.

In critiquing the Kemp-Roth proposal during this campaign, newspaper columnist Tom Wicker noted that its advocates were appealing to the "Laffer curve," and, in particular, the notion that a large cut in tax rates might generate *higher* federal tax revenues (*Detroit Free Press*, August 24, 1978).[216] The Laffer curve—a concept that Arthur Laffer had reportedly first advanced during a dinner conversation in July 1974 (see, for example, *The Plain Dealer* (Cleveland, Ohio), July 17,

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[215] *Newsweek*, December 19, 1977. Emphasis in original.
[216] Another newspaper columnist in this period who criticized the Republicans for embracing the Laffer-curve idea was Richard L. Strout (*Detroit Free Press*, July 20, 1978).

1978; Blinder, 1981b, p. 83)—figured heavily in arguments regarding the case for a large tax cut that took place in the *Wall Street Journal*'s editorial pages in the second half of the 1970s (see Laffer and Seymour, 1979).

Friedman was largely absent from the *Wall Street Journal* exchanges on tax during this period, even as his old sparring partner Walter Heller was deeply involved in it. By the later months of 1978, however, the Laffer curve had become sufficiently pervasive in U.S. public debate that policy-oriented academic economists felt obliged to refer to it: for example, three 1960s-vintage members of the Council of Economic Advisers—Heller, Gardner Ackley, and Otto Eckstein—all discussed the Laffer curve explicitly in submissions to an August 1978 Congressional inquiry on the Kemp-Roth bill, while even Robert Lucas, at an October 1978 conference on rational expectations and economic policy, referred to "Arthur Laffer's influential 'Laffer curve.'"[217] Friedman himself gave Laffer his due when, in April 1981, he mentioned "the famous Laffer curve."[218]

The Laffer-curve idea not only galvanized supply-side supporters of a tax cut. It also provoked, over time, a strong negative reaction in public commentary—to such a degree that by the late 1980s, endorsement of the notion that tax cuts generated higher revenues had become something perceived as undermining the credibility of those making the case for tax cuts, including the Reagan tax cut (see, for example, M.K. Evans, 1983, p. 108). In this vein, Lawrence Lindsey (1985, p. 25) referred to "the derision with which the Laffer curve has been treated in much of the profession." Indeed, the account of 1980s economic policy by Reagan's former adviser Martin Anderson (1988, pp. 151−157) contained an extended discussion making the case that neither officials in the 1980 Republican presidential campaign nor the Reagan administration itself endorsed the notion that a broad-based income tax cut would raise the federal government's tax revenue, on net.

Ironically, however, in the pre-Reagan years the notion that broad-based tax cuts might give rise to an increase in tax revenues had picked up support from a wide variety of commentators—not only various supply-side figures, but also old-style Keynesians, and, with qualifications, from Milton Friedman.

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[217] See Committee on the Budget, U.S. House of Representatives, and Committee on the Budget, U.S. Senate (1978, pp. 3−13, 49−50, 104−110) and Lucas (1980, p. 204).
[218] In Friedman, Porter, Gruen, and Stammer (1981, p. 39).

The Laffer-curve idea before 1977–1978

At the level of principle—as distinct from its relevance for the analysis of actual or proposed major tax cuts—the Laffer curve had a long history in economic thinking. Indeed, Robert Hall (1999, p. 48) took exception to the "Laffer curve" label—his grounds being that "Arthur Laffer was hardly the first to recognize that the elasticity of revenue with respect to taxes depends on the elasticity of income with respect to taxes."[219] Likewise, Blinder (1981b, p. 81) suggested that "the analytical foundations of the Laffer curve were in fact established centuries ago." In documenting this statement, Blinder (1981b, p. 83) pointed to statements made in 1774 and 1844, the latter being by Edmund Burke.[220] Adam Smith's *Wealth of Nations* would also be quoted as articulating the Laffer-curve idea.[221]

In the twentieth century, the proposition that tax cuts increase revenue was central to Sir Josiah Stamp's 1922 book *Wealth and Taxable Capacity*, which included (p. 117) a specific argument that a cut in marginal tax rates would so boost private sector incentives and economic activity that it would raise overall tax revenue. Nor was the Laffer-curve notion something that figured in U.S. debates on tax policy starting only in the 1960s or 1970s. For example, the supply-siders of the 1970s emphasized the prominence that Laffer-curve ideas had acquired in public discussions of U.S. tax policy five decades earlier, during the 1920s.[222] The argument had also appeared in debates during the 1950s, with Herbert Stein observing (*Financial Times* (London),

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[219] Different views have been expressed regarding how obvious the point is: Blinder stated that the arguments for the Laffer curve "require no economic analysis at all" (1981b, p. 81). (It would be more accurate to say that the foundations require only standard assumptions that economists make. The endpoints of the curve—in which zero tax rates generate zero tax revenue and 100 percent tax rates wholly remove incentives to engage in market activity and so, likewise, generate zero tax revenue—indeed require very little economic analysis. But the derivation of a smooth curve between the two points requires economic assumptions.) Lucas (1980, p. 204) took the position that the Laffer curve, as Laffer had articulated it, did involve a rudimentary amount of economic theory.

[220] See also Canto, Joines, and Webb (1981, pp. 77–79) and Laffer and Seymour (1979, p. 5).

[221] Specifically, Canto, Joines, and Webb (1981, p. 77), Lawrence Lindsey (1985, p. 21), and van Ravenstein and Vojbrief (1988, p. 205) quoted Adam Smith (1776, Book V, Chapter 11, p. 414). Smith had suggested that both greater evasion and reduced consumption might mean that high excise tax rates generated less revenue than moderate tax rates. (Like numerous discussions of the revenue/rate matter prior to the twentieth century, Smith was focused on taxes on purchases of specific goods rather than income taxes.) Smith's emphasis on changes in the degree of evasion—as a channel through which revenues were related to the tax rate—anticipated a major theme in Friedman's discussion of the issue.

[222] The supply-side activist and journalist Jude Wanniski had pointed (as would Goolsbee, 1999, p. 1) to Andrew Mellon's 1920s writings for their advocacy of the position that a tax-rate cut would raise revenue (see Committee on the Budget, U.S. House of Representatives, and Committee on the Budget, U.S. Senate, 1978, pp. 4–5), while Jack Kemp (1979, p. 59) would quote President Coolidge endorsing the notion that lower tax rates generate higher revenues.

February 1, 1954) that “many Congressmen operate on the convenient theory that reductions in tax rate increase the revenues.”

For the period beyond the 1950s, however, the controversy would be elevated as the debate on the likely reaction of tax revenues could be raised by a tax cut became a more central aspect of the discussion of major tax cuts. Eventually, both prominent Keynesians and some in the supply-side camp on the issue would both deny having endorsed the empirical validity of the notion that broad-based tax cuts generated higher revenue. In fact, such endorsements had appeared prominently on both sides in 1977−1978—particularly in the context of drawing lessons from the experience of the Kennedy-Johnson tax cut.

Keynesians and the rates/revenue link

On the Keynesian side, Walter Heller would provide fuel for the advocates of the Kemp-Roth tax cut in the course of testifying in early 1977 in favor of the Carter tax-rebate proposal.[223] When Heller was asked about the effects of the 1964 tax cut on federal receipts, he testified: “Did it pay for itself in increased revenues? I think the evidence is very strong that it did.”[224]

Heller’s answer was seized upon by supply-siders and much quoted by them.[225] Heller himself would repudiate his statement, and in the early 1980s he would claim that it had been an “exuberant” off-the-cuff remark (Heller, 1981, p. 17). In fact, however, the remark was not an isolated one: Heller had actually made the same claim on multiple occasions prior to 1977. Indeed, he had in the past included higher revenues as both an *ex ante* rationale for, and the actual effect of, the Kennedy-Johnson tax cut. For example, in a 1965 interview with the London *Sunday Times*, Heller had stated that “by boosting national output and income, the tax cut would generate larger local, state and federal revenues in the long run.”[226]

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[223] This was the proposal discussed in the previous chapter.
[224] From Heller’s February 7, 1977, testimony, in Joint Economic Committee (1977, p. 161). This was an answer to a question by Senator Javits—not, as Heller (1981, p. 17) misremembered it, an “answer to a leading question by the late Senator Hubert Humphrey.”
[225] See, for example, Paul Craig Roberts (1984a, p. 80; 1984b, p. 86). Although he was one of the supply-siders who played up Heller’s statement, and the one who highlighted it in the *Wall Street Journal* (August 1, 1978), Paul Craig Roberts would later (in *Wall Street Journal*, December 18, 1985) criticize the *Journal*’s editorial writers for damaging the public perception of supply-side economics by associating it closely with the Laffer curve.
[226] *Sunday Times* (London), June 20, 1965. Likewise, Heller (1966, p. 113) had stated: “The upsurge of tax revenues flowing from economic expansion would finance higher levels of local, state, and federal spending than we would have had without the tax cut’s stimulus.” In addition, Congdon (1988, p. 41) noted that the list of benefits of the tax cut that Heller provided in January 1963 included higher revenues.

President Kennedy himself had invoked this argument; in a speech of December 14, 1962, in New York City, widely cited by supply-siders in later years, Kennedy had stated, "an economy hampered by restrictive tax rates will never produce enough revenue to balance our budget... In short, it is a paradoxical truth that tax rates are too high today and tax revenues are too low—and the soundest way to raise the revenues in the long run is to cut the rates now."[227] Likewise, in his contribution the following month to the 1963 *Economic Report of the President*, Kennedy wrote: "Let me make clear… reducing taxes is the best way open to us increase revenues."[228]

And, after the 1964 tax cut was implemented, it became a standard part of Keynesian accounts that one result had been higher overall federal revenue and that future tax cuts might produce the same result.[229] During the 1970s, Heller was not the only leading Keynesian who gave credence to the view that a tax cut was likely to pay for itself. While serving in his position as a Carter adviser, Lawrence Klein had stated (*The Sunday Sun* (Baltimore), July 25, 1976, p. K7): "We could have lower tax rates, actually, but no overall loss of revenue to the government, because, hopefully, the expanding economy under Carter will generate more tax dollars."[230] And in April 1975—ironically, in a conference that Arthur Laffer was co-organized—James Tobin had stated: "If the fiscal stimulus is allowed to have the effect of increasing output and increasing incomes, etc., recovery itself will generate saving and additional taxes to offset and finance that fiscal stimulus."[231]

Where Keynesians like Heller, Klein, and Tobin did differ with the supply-siders was how the tax cut would generate extra revenue. The supply-siders emphasized the reaction of labor and other inputs to lower tax rates and viewed the response of revenues as reflecting an upsurge in both actual and potential output. It is true that the effect of potential output of tax cuts was by no means wholly absent from the case that the Kennedy Administration had made for tax cuts. Kennedy had himself referred in the 1963 *Economic Report of the President* to the

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[227] *Commercial and Financial Chronicle* (New York), December 20, 1962, p. 2553 (see also Kennedy, 1962). This Kennedy statement would be quoted, sometimes with minor misquotations, in many subsequent discussions of supply-side economics and its alleged connection to Kennedy's economic views. See, for example, *New York Times*, September 18, 1984, *USA Today*, April 24, 2003, Laffer, Moore, and Tanous (2008, p. 56), and Kudlow and Domitrovic (2016, p. 110).
[228] In Council of Economic Advisers (1963, p. IX). This, too, was a Kennedy quotation later used to justify the Kemp-Roth tax cut. See, for example, *The Union Leader* (Manchester, New Hampshire), October 31, 1980.
[229] For example, *Life* magazine editorialized (June 4, 1965) that the 1964 tax cut "helped stimulate so much new productive activity that federal revenues are already higher at the new rates than they were at the old."
[230] Gordon (1978, p. 487) utilized a similar argument, contending that the slack in the U.S. economy in 1976 made candidate Carter's promise to cut taxes, raise government spending, and balance the budget internally consistent.
[231] In Birnbaum and Laffer (1976, p. 50).

"unrealistically heavy burden of taxation," for example.[232] And his Secretary of the Treasury, Douglas Dillon, testified in February 1963: "With lower marginal rates there is, quite naturally, a greater stimulation to effort…There is certainly a great deal more stimulus to put in extra effort if you can retain 35 cents out of every dollar of extra funds than if you only retain 25 cents. That is the reason for lower marginal rates. I think that is a very simple thing to understand."[233]

Nevertheless, the supply channel was not given the most important weight as the means through which the main Kennedy-Johnson economists and other Keynesians saw the cuts as generating revenues.[234] In their assessment, the economy's supply response was likely to be modest, especially in the short run.[235] The principal basis for expecting higher economic activity and hence higher revenues was the reaction to tax cuts of aggregate demand: the boost to private-sector spending arising from the tax cut, and the associated reduced shortfall of output below potential. It was this line of reasoning that had led Heller to maintain that the manner in which the tax cut raised economic activity (and so tended to lead to some or all of the revenue loss associated with lowering taxes to be recouped) was that it allowed aggregate demand curve to intersect with a higher-output position of an existing aggregate-supply curve. That is, as Heller saw it, the tax cut "drew on 'aggregate supply' capacity that already existed."[236]

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[232] In Council of Economic Advisers (1963, p. XIII).
[233] From Dillon's testimony of February 6, 1963, in Committee on Ways and Means, U.S. House of Representatives (1963, pp. 593−594). See Romer and Romer 2009 (pp. 38−39) for detailed documentation of the point that the Democratic party administrations of the 1960s cited stimulation of aggregate supply as a major reason for a tax cut. Heller (1982b, p. 76) acknowledged that the argument that aggregate supply would be improved had been an ancillary basis for the 1964 tax cut and confirmed that it had exerted a favorable effect on labor supply.
[234] See also Heller (1981, p. 15; 1982a, p. 287; 1982b, p. 76), and the attributions to Heller in Kemp (1979, p. 60) and Paul Craig Roberts (1984b, p. 85). Even the much-repeated Kennedy statements (regarding boosting longer-run output and revenues via a tax cut) had a considerable demand-side or Keynesian dimension embedded in them. Under the views prevailing at the Council of Economic Advisers in the early 1960s, monetary policy and price adjustment could not be counted on to deliver full employment, even over a long horizon, as long as the full-employment federal budget was in a state of what the CEA regarded as an unduly large surplus. The tax cut was designed to lower this full-employment budget surplus. See Council of Economic Advisers (1963, pp. 69−81) and Kennedy's own remark in the same volume (p. XIV) as well as Kennedy (1962).
[235] For example, in a letter to U.S. legislative-committee chairs dated August 7, 1978, Lawrence Klein referred to referred critically to "Arthur Laffer's claims [and] those who have written along similar lines in the past few weeks." (Committee on the Budget, U.S. House of Representatives, and Committee on the Budget, U.S. Senate, 1978, p. 118.) Later, Klein observed (*Chronicle-Telegram* (Elyria, Ohio), March 29, 1982): "We can find some effects of tax rates on labor supply or on savings, but they are small, slow and sometimes not statistically significant. This is hardly a basis for forming a grandiose scheme of public policy." Likewise, Walter Heller, although granting (in *Wall Street Journal*, July 12, 1978, p. 47 of 1979 reprint) that the 1964 tax cut was "well-designed to boost work incentives," Heller suggested that "these benign effects on the supply side work slowly," and Heller (1981, p. 17; 1982a, p. 287) referred to the "long-delayed supply stimulus" of a reduction in income tax.
[236] *Wall Street Journal*, July 12, 1978 (p. 47 of 1979 reprint).

Heller elevated the degree of disagreement between himself and the supply-siders when, in 1978, he retracted his prior statement (actually, statements) that the 1964 tax cut had paid for itself. His revised view was that that tax cut had generated substantial revenues—but well short of an amount that would have made the cut self-financing (*Wall Street Journal*, August 7, 1978).

Friedman on tax rates and revenues, 1962–1976

Friedman's own position on the 1964 tax cut's economic effects was discussed in Nelson (2020b, Chapter 12). As far as the specific implications of the cut for tax revenues were concerned, Friedman's position implied a mixture of agreement and disagreement with the Keynesian interpretation. Friedman, like Heller, saw much of the year-to-year movement in real national income in the 1960s as reflecting changes in aggregate demand, rather than as being instigated by movements in potential output. A further element of agreement with Keynesians was that he was hesitant to put great emphasis on substantial short-run responses of labor or other input supplies. Certainly, Friedman regarded supply responses, including to price signals, as important at the business-cycle frequency.[237] He nevertheless viewed long-run elasticities of factor supplies as significantly larger than the short-run elasticities.

Friedman, however, parted company drastically with Heller, Tobin, and L.R. Klein in not sharing their confidence that a large tax cut, by itself, could generate an appreciable boost to nominal and real aggregate demand in the short run. He did not regard the 1964 tax cut as establishing that such a boost occurred, as it had been a case of *monetized* fiscal expansion. The economic outcomes that followed the 1964 tax cut therefore failed to establish that an unmonetized tax cut of the same magnitude would have appreciably boosted nominal spending (and thus tax revenues).[238] The upshot was that Friedman would *agree* with Heller's ultimate position that the 1964 tax cut had not generated sufficient tax revenue to justify regarding the cut as self-financing. But he would *depart from* Heller's still-high estimates of the extra aggregate spending (both real and nominal) on goods and services produced by the tax cut.[239] Friedman

[237] See Nelson (2020a, Chapter 7).
[238] Similar assessments of the 1964 tax cut were made by Paul Evans (see Nelson, 2020b, Chapter 12) and Gregory Mankiw (see *Wall Street Journal*, September 29, 1983).
[239] The same was true of the prospective change in aggregate demand that would arise from the Kemp-Roth bill. Heller wrote in 1978 that if enacted, the bill would produce "a tidal wave of increased demand and… roaring inflation." (*Wall Street Journal*, July 12, 1978, reprinted in Laffer and Seymour, 1979, p. 49.) In contrast, of course, Friedman did not see a tax cut or other fiscal policy measures as likely, by themselves, to produce such an outcome.

believed that both Keynesians and supply-siders exaggerated the extent to which a tax cut, by itself, could give rise to higher flows of real spending in the short run.

Furthermore, in instances in which increases in tax revenues *were* associated with tax cuts in practice, much of the revenue increase was properly attributed, in Friedman's view, to the monetary policy response. This was so even if the tax cut operated on real activity through supply responses. Absent monetary accommodation of the tax cut, the supply-side improvement in output associated with a tax cut would tend to shift the split of increases in nominal income—rather than increase nominal income itself. As tax collections tended to be geared heavily toward nominal incomes, an unmonetized tax cut would likely have a limited effect on nominal spending, nominal income, and nominal tax receipts. For this reason, even a supply-side-focused account of the tax cut's effects really required, in Friedman's view, monetary accommodation as part of the mechanism making for higher tax revenues.

Friedman nevertheless did see scenarios in which *a tax reform package that included reductions in income tax rates* could give rise to a prompt increase in federal tax revenues. The mechanism he envisioned for this result to be achieved did not rely on immediate large labor-supply responses to the tax-rate cuts and could occur even if the reform left potential output basically unchanged. The key aspect of the reform was that emphasized above: a tradeoff of lower rates for elimination of most tax deductions. The result, Friedman believed, would be an increase in tax revenue.

Friedman's case for tax-rate cuts that would raise revenue was outlined in *Capitalism and Freedom*.[240] Friedman contended in that discussion that a streamlined income tax system in which there would be flat rate (above a tax-free threshold) of about 24 percent, while tax deductions would be strictly limited to work expenses, would make unprofitable the use of tax shelters—as the maintenance of these shelters involved, in effect, paying a marginal rate of income above the proposed flat marginal rate. More income would come to be classified as taxable income, and overall revenues would go up.

There was a difference in this picture of higher revenues from that underlying the arguments that

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[240] See Friedman (1962a, pp. 175−176). See also his later expositions of this argument in *Newsweek*, April 22, 1968, and *Playboy* (February 1973, pp. 60, 62; reprinted in Friedman [1975a, pp. 21−23; 1983b, pp. 36−38]). The *Playboy* discussion referred (p. 62; see also Friedman, 1975a, p. 22; 1983b, p. 38) to work being done on the matter at the Brookings Institution (meaning primarily the efforts of Joseph Pechman—who would ultimately become much more associated than Friedman was with proposals to reduce tax rates in exchange for removal of deductions).

would be most associated with the supply-siders. Specifically, Friedman emphasized heavily-reduced tax avoidance—and so more income being *recorded in tax returns* in the category of taxable activity—rather than relying on a *sharp rise in economic activity* as a result of the tax cut. Federal tax revenue, in Friedman's view, would rise in the immediate wake of the reform. even if output and potential output were unchanged in the short run. As it happened, Friedman *did* see lower tax rates accompanied by elimination of loopholes, and the more efficient tax system, as boosting potential output—once longer time horizons were considered. But his own rendition of the cuts-raise-revenues argument did not rely on a very large immediate output-supply response (in the form of a shift in the aggregate supply curve).

The proposal, like *Capitalism and Freedom* itself, attracted limited attention at the time, although a *Wall Street Journal* editorial (November 22, 1963) noted that Friedman had found a 23.5 percent flat income tax (accompanied by strict limits on deductions) would generate as much revenue as the existing income tax system and might produce more. Friedman indicated in 1969 that he continued to believe that a 24 or 26 percent flat-rate income tax would generate greater revenues than the existing federal system (Instructional Dynamics Economics Cassette Tape 15, February 1969). Correspondingly, he assessed that a still lower rate would maintain the existing level of tax revenue: over the late 1960s and into the mid-1970s, Friedman assessed this revenue-neutral tax rate to be about 18 percent (*Chicago Tribune*, March 2, 1969) or even 16 percent.[241]

Friedman particularly stressed the reported high marginal rates (especially the 70 percent top rate) as capable of being reduced with little revenue loss: "the high rates raise no revenue," he observed in 1970.[242] "Very few people pay taxes in the higher brackets," he added later, "largely because of the loopholes we've heard so much about."[243]

It was, however, still another reaffirmation of the flat tax proposal, in a *Newsweek* column of April 12, 1976, that attracted somewhat greater attention—and perhaps particularly caught the

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[241] On the 16 percent number, see *Firing Line*, syndicated, January 8, 1968, p. 21 of transcript, *Playboy* (February 1973, p. 62; reprinted in Friedman, 1975a, p. 23; 1983b, p. 38), and Friedman (1977i, p. 50; reprinted in Friedman, 1978b, p. 75). The last of these items was based on remarks Friedman made in September 1976. (The remarks, although given in London, were made specifically in reference to the U.S. federal tax system.)

[242] Friedman (1970b, p. 438). Similarly, in Instructional Dynamics Economics Cassette Tape 15 (February 1969), he had remarked that "the high rates yield no revenue" and in *Newsweek*, April 22, 1968, he had described the high rates as being "almost pure window dressing." Consistent with these judgments, Splinter (2020, pp. 1006−1007) finds that "in 1962, the top [U.S. federal marginal income tax] rate applied to fewer than 500 tax returns and, despite a top statutory rate of 91 percent, the top 1 percent [of income earners'] average federal income tax rate was only 16 percent."

[243] *Playboy* (February 1973, p. 60). Also in Friedman (1975a, p. 21; 1983b, p. 36).

eye of Ronald Reagan.[244] In this column Friedman noted that, between 1929 and 1972, the number of U.S. taxpayers reporting themselves as earning over a million dollars or more in income had been stagnant—an outcome suggesting widespread understatement of income for tax purposes and heavy recourse to tax loopholes.[245] He proposed that either simply abolishing all marginal income tax rates higher than 25 percent and doing nothing to deductions, or, instead, streamlining available deductions and imposing a flat income tax rate of 16 percent, would generate greater tax revenue. Notably, the published column was interspersed with subheadings "LOWER RATES… HIGHER REVENUE."[246]

Reagan on tax rates and revenues, 1977−1978

Columnist Tom Wicker accurately predicted in 1978 that Ronald Reagan was likely to include a Kemp-Roth-style tax cut in his program, should he run for president again (*Detroit Free Press*, August 24, 1978). Notwithstanding Reagan's support for Kemp-Roth and Jack Kemp's own enthusiasm for Laffer-curve ideas, it has become a matter of controversy whether the economic thinking of the Reagan campaign and administration actually relied on Laffer-curve arguments. Specifically, and as already indicated, Martin Anderson, an adviser to Ronald Reagan both before and during his presidency, would later question whether anyone closely linked to the Reagan economic team ever suggested that a large tax cut like Kemp-Roth would promptly yield higher tax revenues. Indeed, Anderson (1988, p. 151) referred to "the myth that Reagan and his key economic associates believed that large tax cuts would produce more revenue."[247]

In considering this same question, the discussion below will narrow it to Reagan specifically and to the 1977−1978 period. It will be seen that Reagan did himself did invoke the argument that tax-rate cuts would raise revenues—and that he cited Friedman as one inspiration regarding this point.

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[244] Friedman's *Newsweek* column was highlighted by Allen Wallis in a conference on income distribution held at the Hoover Institution in May 1976: see W.A. Wallis (1977, pp. 145−146). In the subsequent floor discussion James Tobin, like Wallis, seemed to endorse the Friedman position that reduced tax rates would yield more revenue via the reduced recourse to tax loopholes—but indicated he would prefer other tax reforms (see Campbell, 1977, p. 174).
[245] In Instructional Dynamics Economics Cassette Tape 48 (April 15, 1970), Friedman referred to an earlier occasion on which he carried out this exercise. In both cases, the judgment he reached was that reporting millionaires had not shown the scale of increase in number since 1929 that one would expect from the growth in U.S. national income and in the population.
[246] Friedman also referred to this column in Friedman (1977i, p. 50) (see also Friedman, 1978b, p. 75; 1991a, p. 156), in remarks delivered in the summer of 1976, and reaffirmed that his proposed tax "change would yield *more* revenue."
[247] See also Anderson (1988, p. 153).

Anderson conceded that, in an October 8, 1976, newspaper column, Reagan did give weight to the hypothesis that tax cuts might generate higher tax revenues, and he cited the federal tax cuts of the 1920s and 1960s in that connection. Indeed, the column was bullish on this point, and it was titled "Income Tax Cuts Raise Revenues" in one outlet (*San Diego Union* (California), October 8, 1976). But Anderson contended that this Reagan discussion was an isolated instance and one in which Reagan did not definitively claim that the tax cuts in question generated higher revenues. However, contrary to Anderson's contention, the 1976 column was not a single example. For example, in a September 1978 newspaper column, Reagan maintained that the tax cuts of the Harding, Coolidge, and Kennedy (really, Johnson) presidencies had all raised federal tax revenue (*Manchester Union Leader* (New Hampshire), September 5, 1978).

For the purposes of this book, however, the most interesting example of Reagan's deployment of the tax-cuts-generate-revenues argument occurred in a talk on October 18, 1977, that Reagan composed for his regular radio commentary series. In this broadcast, "Taxes," Reagan stated: "Economists like Paul McCracken of the University of Michigan, Milton Friedman [of] University of Chicago, Arthur Laffer of USC, Allan Meltzer of Carnegie Mellon University, and Arthur Burns, Chairman of the FR [i.e., Federal Reserve] Board, have each made it clear that government can increase its tax revenues and create the jobs we need *without* inflation by lowering the tax rates for business and individuals… Twice in this century, in the 1920s and the early Sixties, we cut taxes substantially, and the stimulant to the economy was substantial and immediate."[248]

Thus, not only did Reagan see the case in which tax cuts raise Treasury revenues as empirically relevant, but those with whom he associated the proposition included not only Laffer but also Friedman. Furthermore, Reagan believed that the argument applied to "something like the Kemp-Roth bill" (*Manchester Union Leader*, September 5, 1978).

In 1977 and 1978, however, Friedman would sharply separate himself from those claiming that higher tax revenues would result from the tax cut envisioned in the Kemp-Roth bill.

Disputing revenue claims about Kemp-Roth—Friedman's second break with the supply-siders

[248] In Skinner, A.G. Anderson, and M. Anderson (2001, p. 274). Actually, of course, Friedman had left the University of Chicago by the time of Reagan's broadcast—as would be brought home to Reagan when, as noted above, he subsequently saw the Friedmans in Los Angeles in November 1977.

We have already seen that Friedman did not subscribe to the view that large tax cuts, in isolation, triggered such a vigorous increase in *aggregate demand* as to make the cuts self-financing. Nor, as noted above, was it likely that Friedman viewed potential output as strongly responsive, in the short run, in response to tax cuts, so he parted company with the familiar supply-side rationale for tax cuts. It is nevertheless true, as Reagan suggested, that Friedman did see conditions under which a lowering of tax rates could be expected to generate higher revenue—and he regarded these conditions as achievable in practice. But he did not see the Kemp-Roth proposal as achieving them.

Friedman made this evident in his *Newsweek* column of August 7, 1978, titled "The Kemp-Roth Free Lunch." The column's title virtually duplicated that of Walter Heller's *Wall Street Journal* op-ed, "The Kemp-Roth-Laffer Free Lunch," to which Friedman did not refer but which had appeared in print on July 12, 1978, about two weeks before he wrote his column. Friedman presumably avoided repeating the "Laffer" part of Heller's title both in order to concentrate on the proposed tax cut and because he was disinclined to set off a direct exchange in print with Arthur Laffer. Nevertheless, without invoking Laffer's name, Friedman's column did in effect address the Laffer-curve arguments being made in 1978 for the Kemp-Roth tax cut proposal.

The analysis in the column largely concerned the empirical relevance of the proposition that the Kemp-Roth tax cut would shift the United States to a higher point on the Laffer curve. Friedman had already indicated his judgment on this proposition in November 1977, when he had stated that "I don't go along with Mr. Kemp's reasons" for the Kemp-Roth bill.[249] Now, nine months later, he made the same point in his *Newsweek* column. Kemp and Roth, he observed, had argued "that lower rates will so sharply increase incentives to work, innovate, and invest that national output will rise apace and, as a result, so will the tax base and government revenue." Incentives would be increased, Friedman granted, and there would also be some reduced tax avoidance. But, in claiming that overall tax revenue would increase, Friedman concluded, "their argument falters… [as] the deficit would, at least initially, go up." (*Newsweek*, August 7, 1978.)

His column did not lay out in detail the basis for this judgment. But that basis could be gleaned from previously-expressed Friedman positions: a tax cut like that in Kemp-Roth would not generate a large enough supply response (whether measured by output, potential output, or hours worked) in the short run to pay for itself, nor—as it would still leave many marginal rates well

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[249] *Milton Friedman Speaks*, Episode 6, "Money and Inflation," taped November 7, 1977, p. 37 of transcript.

above 25 percent—would its dampening effect on the private sector's incentive to exploit tax loopholes be of a size great enough to boost overall tax receipts.

In December 1979, Friedman reaffirmed his *Newsweek* judgment on the Kemp-Roth proposal. He contended that, although there were some types of taxes might "have such great disincentive effect" that a lower rate would lead to increased tax revenue, "I do not believe that is true for the tax system as a whole" (*The Register* (Santa Ana, Orange County, California), December 23, 1979, p. A11).

In addition, Friedman felt that Kemp, Roth, and other supply-siders were, by concentrating on taxes, neglecting the extent to which the size of the public sector, however financed, was a factor holding down the level of potential output.

Taxes and measuring the cost of government: Friedman's third break with the supply-siders

Friedman's focus on government spending, rather than taxes, as the key budgetary total that needed to be restrained moved him away from the center of the tax revolt, while also putting him at further odds with supply-siders. Correspondingly, the supply-siders, due to their concentration on tax reduction, would be more prominently and enduringly identified with the tax revolt than Friedman would be.

"The crucial matter is not the size of the deficit, but the level of government expenditures," Friedman had remarked in 1966 (*Saturday Review*, March 5, 1966, p. 69). This was a point that he upheld over the following four decades, during which he would underline it repeatedly. Developments beyond the 1960s would change the manner in which Friedman expounded the point: as the momentum underlying the tax revolt built up, the variable he contrasted with spending would often be taxes, rather than deficits. In this vein, in early 1975, both in his *Newsweek* column and on television, Friedman laid out a formulation that would become a very familiar maxim on his part: the true tax on the American people was the amount the government spent, not the amount of that spending whose financing was associated with explicit tax revenue. He observed in his column that "the true cost of government is measured by government spending, not by receipts from explicit taxes" (*Newsweek*, January 27, 1975*a*, p. 27), and he reinforced the message in a television interview a few weeks later when he remarked that "the real tax all of us pay is what government spends" (*Wall Street Week*, Maryland Public Television, February 7, 1975, p. 19 of transcript).

The rationale underlying Friedman's stand was that even government spending not financed by taxes represented a diversion of resources from the private sector, and so it (along with regulations) constituted "levies" on the private sector that shifted income from producers of goods and services to non-producers (*Newsweek*, August 19, 1974). One of the ways in which Friedman illustrated this point was by citing the behavior of the private sector's real disposable earnings. Its tepid behavior in the decade through mid-1974 had contrasted with the strength of real income per person.[250]

And, as was discussed at length in Chapter 6, Friedman suggested that the growth of the public sector might ultimately bear adversely on economic growth. Consequently, in the face of growing public spending shares, "my long-run concern is that we're headed toward stagnation, sharply reduced economic growth rates." (*Washington Star-News* (Washington, D.C.), July 15, 1974, p. A9.) In the 1970s, as this situation seemed to be realized, Friedman downplayed the degree to which tax cuts, alongside an unchanged volume of government spending could, by itself, provide much relief to the private sector. Budget deficits were financed either by money creation or government debt issuance, and each of these financing measures amounted to one form or another of a tax on wealth.[251]

Friedman felt so strongly on this issue that he was increasingly pressing this issue in discussions with academics. On one important occasion, this reached a situation of talking at cross-purposes with his interlocutor. Robert Barro was a visiting fellow at the Hoover Institution in the 1977/1978 academic year.[252] Barro would, as discussed in Chapters 6 and 8 above, usually be regarded as on the same page as Friedman on matters of fiscal policy. The Ricardian equivalence argument that he formalized (Barro, 1974) could be regarded as essentially confirming that the size of government spending is the better longer-run measure of the tax burden. Subsequently, and similarly in keeping with Friedman's position, Barro (1978, p. 576) would criticize Buchanan and Wagner's (1977) proposed constitutional amendment limiting the budget deficit as not addressing the pressure of the public sector on resources because it was unlikely to limit government outlays. But another Barro analysis of roughly the same vintage—

[250] Again, see Friedman's column in *Newsweek*, August 19, 1974.
[251] In the 1980s, Friedman would qualify this view somewhat by indicating that he believed that public debt issuance, while comparable with a tax on some dimensions, likely had less adverse effects on resource allocation than did taxes on income (*Wall Street Journal*, December 14, 1988). This complemented other arguments he had made for permitting temporary budget deficits.
[252] See National Bureau of Economic Research (1981, p. 10).

"On the Determination of the Public Debt"—initially attracted Friedman's ire.[253]

Barro recalled: "You know, I have a later paper that came out in '79 about tax smoothing, which was also [like Barro, 1974] in the *Journal of Political Economy*, that modified the Ricardian equivalence to allow for some public finance like optimal tax considerations, [according to which] the timing of taxes did matter, and there was kind of an optimal time pattern. I gave a seminar at Stanford, I think it was in 1978. It was after Milton had left Chicago. And I was giving that paper—which he should have liked. But he thought I was saying something different from what I was saying, and he just completely trashed the paper. He basically wouldn't let me get past page six. Because he kept saying the cost of government is the expenditure and the resources that are used up, not really the taxes *per se*—which is kind of a Ricardian-equivalence argument. My argument [in Barro, 1979] was not really at odds with that position, except that it said that there were tax distortions and that the timing mattered somewhat." (Robert Barro, interview, June 4, 2013.)

The new Barro paper took the path of government spending as given and looked at the appropriate mix of taxes over time. This was a valid exercise, and its findings were actually in keeping with Friedman's longstanding preference for tax-rate schedules that were stable over time. In particular, the analysis was in keeping with arguments regarding the optimal setting of fiscal policy in the presence of the permanent income hypothesis. But, although the paper's analysis was consistent with Friedman's larger vision, the fact that it did not treat government spending as a choice variable provoked a strong reaction from Friedman when Barro presented the paper at the Hoover Institution economics seminar.

Friedman's behavior at the seminar verged on disruptive; he kept stressing his refrain that the overall tax burden could not genuinely be eased unless government spending itself was lowered. Barro did not disagree and tried to point out that his analysis was concerned with the separate issue of achieving the least distorting mix of explicit taxes and other financing tools against the background of a given size of the public sector. "So you can think about an optimal pattern and therefore the optimal way of choosing fiscal deficits. But Milton thought I was saying something different. And he'd obviously not read beyond page seven, so he was determined that I was not

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[253] The seminar likely took place during the first three months of 1978. The published version of the paper appeared in October 1979 (Barro, 1979), and Barro presented the paper at a Cowles Foundation seminar at Yale University on March 30, 1979 (see *https://cowles.yale.edu/cf-seminars*). Barro, however, already had a manuscript version of the paper in December 1977 (see Barro, 1978, p. 580), and he also circulated a working-paper version in 1978 (see Brennan and Buchanan, 1980, p. 13).

going to get beyond that in the seminar. And he succeeded in that." (Robert Barro, interview, June 4, 2013.) Friedman missed this message—perhaps because he no longer followed his University of Chicago-era practice of studying papers in detail before their presentation—and his behavior at the seminar prevented Barro from getting to the paper's main results in the seminar.[254] "This was very puzzling. I mean, George Stigler happened to be there [at the seminar], and Bob Hall, and it was a very strange situation… But what Bob Hall said to me after that seminar was, 'I've seen seminars that ended up like this and [that was when the paper] didn't go anywhere. 'But,' he said, 'I've never seen it happen before with a good paper'—that's what Hall told me at the time." (Robert Barro, interview, June 4, 2013.)

Friedman's stress on public spending, rather than taxes, as the magnitude that better captured the public sector's absorption of the economy's resources necessarily put him at odds with the tax-limitation movement. But this tension produced less of a conflict in aims in the state-level debates. Existing requirements for approximate balance of U.S. states' budgets, and the absence of money creation as an option available to the state governments, meant that limitations on tax revenues did put an automatic limit on public spending.

In light of the tight link, at the state-government level, between tax limitation and spending link, California's Proposition 13 campaign in 1978 saw both the "No" and "Yes" campaigns taking for granted that state government spending would be restricted by the reduction in tax revenue. On the "No" side, the stand taken was captured in an Associated Press headline, "Officials in California Fear Nightmare If Taxes Slashed" (*Minneapolis Star*, June 1, 1978), and after the result, John Kenneth Galbraith suggested that once California's voters saw the repercussions for public services of Proposition 13, they would blame Milton Friedman and wish he had never moved from Chicago (*The York Dispatch* (Pennsylvania), June 12, 1978). On the "No" side, Friedman characterized claims that school closures or cuts to emergency services would result from passage of Proposition 13 as "scare tactics" (*San Francisco Sunday Examiner and Chronicle*, May 14, 1978). "There isn't a person in California who could say with a straight face that 10 percent [of California's state government spending] couldn't be cut," he remarked a

[254] Barro observed of the 1979 paper: "this was distinguishing taxes by their timing and that the timing matters in the same way for thinking about different types of taxes in the sense of how much deadweight loss there is. So it's a kind of second-order argument." (Robert Barro, interview, June 4, 2013.) As already indicated, by December 1988 Friedman's discussions of taxes and deficits seemed to have incorporated a version of this argument. This could be seen as a generalization, to include the choice between debt and other forms of financing, of Friedman's position that "some taxes do more harm than others… [so] the method by which we collect taxes could be rearranged so as to have a less adverse effect on incentives and production." (In Friedman, 1978a, p. 11. Also reprinted in Friedman, 1978b, p. 18.)

couple of weeks before the vote (*Dallas Morning News*, May 22, 1978). Friedman made a similar remark in the column he wrote in the wake of the referendum result, while expressing satisfaction that the "Yes" vote had withstood a "massive campaign against Proposition 13 by the Establishment… [T]he public refused to be bamboozled this time" (*Newsweek*, June 19, 1978). While rejecting the notion that basic public services were threatened, Friedman did relish the prospect that Proposition 13 would bear down on the size of California's public sector. Before the vote, he had suggested that the measure represented "the best chance we have to control government spending" (*San Francisco Sunday Examiner and Chronicle*, May 14, 1978), and he also noted: "Anything which reduces the amount of money available for government is a good thing."[255] In his *Newsweek* column in April, he approvingly quoted a letter that had appeared in the *San Francisco Chronicle* that had stated that it was not irresponsible to want the state public sector "severely disrupt[ed]," in the sense of creating pressures to reduce its size.[256]

The *San Francisco Chronicle* letter and Friedman's quotation of it were noted in the account of the tax revolt by one of the architects of Proposition 13, Howard Jarvis, in his own account of the tax revolt (see Jarvis and Pack, 1979, p. 124). However, that account took issue with Friedman by criticizing measures oriented directly on restriction of government spending, such as 1973's Proposition 1. Such measures, Jarvis argued, failed to convey benefits to taxpayers as clearly as tax-limitation measures did (see Jarvis and Pack, 1979, pp. 39, 187). Friedman himself acknowledged that one attraction to voters of tax-limitation measures was that they provided more visible gains than spending limitations.

Jarvis would remark with regard to the Proposition 13 campaign: "Having Friedman in our corner was a big plus."[257] Following the passage of Proposition 13, Friedman continued to provide some public support to Jarvis' efforts, including via being one of those appearing in a paid television program, *The Howard Jarvis National Tax Revolt*. This program, broadcast on most of the major NBC-affiliated television stations in the United States on September 26, 1978, resulted from the purchase by Jarvis' faction of the anti-tax movement of airtime in a prime-time slot (*Boston Herald*, September 23, 1978).

Over the same period, however, Friedman was also going his own way. He joined in taking credit for the Proposition 13 victory, but in doing so, Friedman cast the campaign as a follow-up

[255] In Jarvis and Pack (1979, p. 107).
[256] *Newsweek*, April 10, 1978, quoting *San Francisco Chronicle*, March 7, 1978.
[257] Jarvis and Pack (1979, p. 107).

to the 1973 and 1976 referendum campaigns for spending-limitation in which he had been involved. The difference between Proposition 13 and the earlier campaigns that he highlighted was the result: previously, he had been "unaccustomed to being on the winning side" (*Los Angeles Times*, July 27, 1978). Both before and after the Proposition 13 vote, Friedman made no bones about the fact that he was more excited about progress in spending-limitation amendments to state constitutions than those aiming to limit taxes. In this vein, in April Friedman affirmed that Proposition 1's joint spending-tax limitation measure in 1973 had been "a far better measure" than Proposition 13 (*Newsweek*, April 10, 1978). In that commentary, at the May 21, 1978 National Tax Limitation conference, and in his June *Newsweek* column of June 19 (written after Proposition 13 had been passed), he highlighted the spending-limitation amendment that had been passed by Tennessee voters on March 7.[258] His summer 1978 *Policy Review* piece "The Limitations of Tax Limitation" made clear, as the title suggested, that he still believed that government-spending limitation, rather than restrictions on tax rates, remained the best approach to follow, including in the area of constitutional amendment.

And on June 18, Friedman gave testimony to a hearing of the Nebraska state legislature in support of a proposed constitutional limitation on spending (*Alliance Times-Herald* (Nebraska), June 19, 1978). He did so by telephone, to a hearing that included not only the committee members but about 400 audience members, many of whom found Friedman's testimony, relayed through a speaker mechanism (*Lincoln Journal* (Nebraska), June 19, 1978, p. 1). In a move that underlined the difference between his own policy preferences and those of Proposition 13's designers, Friedman at the same hearing opposed two other proposed amendments (also being considered at the hearing) that would impose limits on property taxes (*Lincoln Journal* (Nebraska), June 19, 1978, p. 1).

In his testimony, Friedman suggested that limitations should apply at all levels of government (*Omaha World-Herald* (Nebraska), June 19, 1978). Friedman himself was involved in what he hoped would be an imminent next step of change at the national level. *The Economist* of August 12, 1978 reported: "Mr. Milton Friedman, the monetarist who backed Proposition 13, recently announced the appointment of a 'blue-ribbon' panel to draft an amendment to the constitution limiting federal spending and deficits." Specifically, as part of his involvement in the National Tax Limitation Committee, Friedman was a key member of a body that concerned itself with drafting a federal constitutional amendment. There were 28 others in this drafting group,

[258] On Friedman's remarks at the May conference, see *Register-Republic* (Rockford, Illinois), May 22, 1978.

including Paul McCracken. Friedman headed a press conference on the matter, held on July 26, 1978, in Washington, D.C. (*Los Angeles Times*, July 27, 1978).

At the press conference, Friedman declared that "people are simply fed up" at the size of the public sector (*Salt Lake Tribune* (Utah), July 27, 1978). Asked if the amendment would limit the president's and Congress' scope to address national problems, Friedman gave the contrarian response: "I certainly hope so. The purpose of this amendment is precisely to limit the power of government with respect to the percentage of income it can extract." (*Los Angeles Times*, July 27, 1978.) Although, at the press conference, there was a suggestion that the drafting of an amendment might be complete by the end of the year (*Salt Lake Tribune* (Utah), July 27, 1978), in the event the draft amendment was not finalized and announced until 1979.

Even when drafting was incomplete, Friedman was already talking up the amendment on a *Meet the Press* appearance of November 12, 1978: the U.S. government would "have a limited total amount of money… [then] decide how best to spend it."[259] He indicated he had been fortified by the latest development at the state level—the success of a spending-limitation referendum in Michigan at the elections earlier in the month, in contrast to the October 1976 result in the same state.[260] Making the rounds in favor of the national spending limitation amendment once it was drafted, and advocacy of its subsequent incarnations, was something to which Friedman devoted a great deal of his time in 1979 and the first half of the 1980s.

In the meantime, he had not given up on achieving restriction on the size of the public sector through the more mundane route of seeking change by legislative action on the U.S. budget. As Robert Barro's early-1978 seminar experience showed, Friedman was concerned with driving home his point that it was government spending that measured the pressure of the public sector on national resources. Friedman's discussions during 1977 and 1978 of budgetary proposals advanced by President Carter and by members of Congress also reflected this emphasis. "The real tax load which government imposes on the public is the total amount government spends," Friedman remarked in a 1977 interview.[261] The following February, he remarked in Pasadena, in his talk on tax reform. that "the real tax is what government spends, not what it takes in in the form of so-called 'taxes.'"[262] A couple of weeks later, in a speech given to the National

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[259] *Meet the Press*, NBC, November 12, 1978 p. 7 of transcript.
[260] *Meet the Press*, NBC, November 12, 1978 p. 6 of transcript.
[261] *New Guard*, April 1977, p. 7. Friedman expressed the same sentiment in *Newsweek*, January 17, 1977, and in *U.S. News and World Report*, March 7, 1977, p. 20.
[262] *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978, p. 9 of transcript.

Association of Manufacturers, he reaffirmed: "The true tax that the American people pay is what government spends."[263] Friedman stressed this point yet again at his May 21 talk to the National Tax Limitation Committee and in its published version in *Policy Review*.[264]

Furthermore, it was, he increasingly emphasized during 1978, not until government spending was cut that the genuine tax burden in the United States could be regarded as having been reduced. He was suspicious of proposed tax cuts both because they did not reduce the spending-to-output ratio or even necessarily the share of taxes in output. "Neither the Republicans nor the Democrats at the moment are proposing to reduce taxes," he remarked in February 1978.[265] At the tax-limitation conference three months later, he reminded the audience that both Republicans and Democratic politicians had promoted the historical growth in federal government spending (*Omaha World Herald* (Nebraska), May 22, 1978). And, with regard to what was in prospect, Friedman maintained that, by the criterion he used, neither President Carter nor Congress was really offering a reduction in taxes (*Dallas Morning News*, May 22, 1978). Friedman did not exempt the Kemp-Roth proposal from this criticism. Kemp-Roth, he insisted, was not a genuine tax-cutting bill because it did not entail a cut in government spending.[266]

It was at the time of his late-July launch of his federal spending-limitation project that Friedman addressed the Kemp-Roth bill in his *Newsweek* column. As already indicated, the column in question (August 7, 1978) was critical of the invocation by the bill's sponsors of the Laffer-curve argument—which Friedman did not regard as empirically relevant for the broad-based cut being proposed. But, beyond his doubts about the applicability of a Laffer-curve-based revenue increase to the Kemp-Roth proposal, Friedman was critical of the way in which Kemp and Roth were portraying higher government spending as achievable due to higher revenue. For the legislators to argue that "lower tax rates can make possible higher government spending," Friedman remarked, was to advertise, as a benefit of the bill, something—higher public spending—that should not be encouraged. In the same vein, he came back to his mantra: tax cuts not occurring alongside commensurate reductions in government spending did not really relieve the tax burden. If implemented, he suggested, the Kemp-Roth bill would reduce explicit taxes—

[263] *Milton Friedman Speaks*, Episode 15, "The Future of Our Free Society," taped February 21, 1978, p. 18 of transcript. Also quoted in *American Banker*, April 24, 1978.
[264] See *Omaha World-Herald* (Nebraska), May 22, 1978, and Friedman (1978a, p. 11), also in Friedman (1978b, p. 18–19).
[265] *Milton Friedman Speaks*, Episode 15, "The Future of Our Free Society," taped February 21, 1978, p. 18 of transcript.
[266] See Friedman (1978a, p. 11). Also in Friedman (1978b, p. 18).

but, absent a cut in federal government spending, the implicit taxes associated with greater deficit spending would go up. A full-fledged tax reduction, Friedman insisted, could be regarded as achieved only when government spending was reduced.

Nevertheless, Friedman came out strongly in favor of the Kemp-Roth proposal. Partly this was because he saw the supply-side stimulus that would arise from the cut as desirable, even though it would take time to emerge. But the primary basis for his support of Kemp-Roth lay elsewhere—the "starve the beast" argument. The tax cut would indeed reduce revenues for the period ahead, he suggested—but this would be a desirable result of the cut. The federal government, unlike its state counterparts, had wide legal and institutional scope to undertake ongoing public borrowing in the face of a revenue shortfall. But Friedman regarded the political environment as creating a practical limit to federal borrowing. Consequently, a lower overall level of federal tax receipts would create pressure to reduce government spending. Hence, for Friedman, an immediate tax cut offered the best chance of a lower path of government spending in future periods.

STARVE THE BEAST

A higher tax intake, Friedman remarked in his summer 1978 *Policy Review* article, "fosters a higher level of government spending. That is why I am in favor of cutting taxes under any circumstances, for whatever excuse, [or] for whatever reason."[267] Likewise, in his August 7 *Newsweek* piece he remarked that, as limits existed on the "tolerable deficit" that U.S. policymakers perceived they could run, "the only effective way to restrain government spending" lay in tax cuts—which, by limiting federal revenue, promoted economies in public expenditure.
This "starve the beast" view of fiscal policy amounted to a positive analysis of the historical source of U.S. government spending. It also had, in Friedman's case, a particular normative dimension, as it served as a basis of some of his prescriptions for limiting public spending.[268]

Several modern-day pieces of writing on the "starve the beast" position have taken the two 1978 statements noted above as the benchmark Friedman references on the matter. For example, Moghaddam (2012, p. 595) and Alexander James (2015, p. 240) cite Friedman's *Policy Review*

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[267] Friedman (1978a, p. 12), reprinted in Friedman (1978b, p. 19).
[268] As will be seen, John Kenneth Galbraith and Robert Eisner shared Friedman's positive analysis but, as supporters of high levels of public spending, they saw that analysis as a basis for refraining from tax cuts.

article, while Bartlett (2007, pp. 8–9) quotes both that article and the August 7 *Newsweek* column. None of these three retrospectives gives any indication that Friedman voiced the "starve the beast" argument publicly before 1978.[269] Indeed, all three of them give the opposite impression. Bartlett (2007, p. 6) further implies that public statements by John Kenneth Galbraith, appearing in the second half of the 1960s, were the "oldest expression" of the position that tax revenue drove spending the second half of the 1960s, and that they long preceded Friedman's own voicing of this hypothesis.

As will be shown below, these retrospectives are incorrect on the chronology of Friedman's adherence to the starve-the-beast argument. By 1978, he had, in fact, been subscribing to that argument for over fifteen years. And his earliest public statements on the matter preceded the Galbraith publications that Bartlett cites. As Friedman noted in November 1977, it was "long ago" that he "concluded that I am in favor of reducing taxes at any time under any circumstances for any excuse, that that's the only effective way to exert pressure on govt spending."[270]

The evolution of Friedman's position on spending and taxes

As of 1958, Milton Friedman's position was that the federal government's spending and taxes should both come down—but that the lowering of taxes should be made conditional on the delivery of expenditure reductions. It would be "highly desirable to reduce" the scale of the U.S. government budget, Friedman remarked in a 1958 talk. But he added that, as long as public spending remained at current levels, "it has been a good thing" that tax levels had been as high as they were.[271]

By the time of Friedman's delivery of this speech—November 1958—he likely had already had a conversation that year with Arthur Burns, in which Burns urged him to think in terms of the political and legislative mechanisms connecting increases in federal tax revenue and increases in

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[269] Another study specifically concerned with "starve the beast"—Hageman, Arnold, and Sutton (2009)—incorrectly argues that Friedman did not articulate the view that government spending drove deficits any earlier than 2003. They also wrongly assert (p. 27) that (as of 2009) Paul Krugman had written more on the matter more than anyone else "in the public press" (i.e., the print and internet media). They reach this conclusion on the basis of their citation of four *New York Times* items by Krugman. In fact, Friedman's contributions to the press on the subject certainly exceeded this number. (The authors also, incidentally, incorrectly state on the same page that Krugman was a past member of the three-person Council of Economic Advis[e]rs to President Reagan." He was not, although he served on the CEA's *staff* during part of Reagan's first term.)

[270] *Milton Friedman Speaks*, Episode 6, "Money and Inflation," taped November 7, 1977, p. 37 of transcript.

[271] Friedman (1958d, p. 19). In the same vein, Friedman (1957b, p. 90) took it for granted that tax increases reduced the budget deficit.

federal government spending. As Friedman later recalled (Instructional Dynamics Economics Cassette Tape 129, September 13, 1973), Burns tried to persuade Friedman to support a tax cut to stimulate the economy. Friedman was ill-disposed toward fiscal-policy activism and believed, at this stage of his thinking, that budget deficits tended to be monetized.[272] Consequently, he resisted Burns' case. Burns sought Friedman's support by taking a political-economy angle: Would not a tax cut put pressure on Congress to cut federal government spending? Again, Burns was unsuccessful in bringing Friedman round to his side. "I did not buy that at that time," Friedman recalled of the political-economy argument. But he would have second thoughts after 1958.[273]

Burns' advocacy of tax cuts to restrain public spending

Burns' support in 1958 for a tax cut (though not the argument that it would put downward pressure on public spending) was noted in Stein (1969, p. 335). Burns does not seem to have taken the "starve-the-beast" line often in his public writings. Burns (1968a, p. 315 of 1969 reprint) stated that "federal taxes have in fact grown just about as rapidly as expenditures" over 1946 to 1967, but he sourced the former rise to the increase in government spending, which had "naturally resulted in much higher taxes." A clearer-cut statement was Burns' (1964, p. 30) observation regarding the tax cut of 1964: "One probable result is some curbing of the growth of federal expenditures."[274]

Notably, by 1968−1969, Burns had firmly shifted to the position that increases in government spending made tax increases appropriate. In this connection, Burns supported the 1968 tax increase (Burns, 1968b, p. 3) and he contended that "unless we bring government expenditures under better control... further increases in the level of taxation may become unavoidable."[275] Indeed, it was Burns' later position that taxes did, and should, adapt to spending behavior, rather than his previous starve-the-beast posture, that informed comments he made in 1978 regarding the Kemp-Roth bill. The Committee on Ways and Means of the House of Representatives had canvassed the reactions of many prominent economists across the country—Friedman notably

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[272] Once more, see Friedman (1958d, p. 19). See also Nelson (2020a, Chapter 8).
[273] The quotation is, again, from Instructional Dynamics Economics Cassette Tape 129 (September 13, 1973).
[274] Also in the 1960s, Burns stated that the tax system "weakens the incentive to create and produce" (*Cleveland Press*, August 24, 1960) and (in a *U.S. News and World Report* interview cited in *First National City Bank Monthly Letter*, January 1969, p. 8) he suggested that it would be desirable to cut marginal tax rates every year.
[275] See Burns' remarks (given on December 2, 1969) in Burns (1978, p. 264).

not among them—to the Kemp-Roth tax-cut proposal.[276] Very few favored it unconditionally. Many opposed it, while Arthur Burns indicated he was in favor of the tax cut on the condition that it would be explicitly accompanied by an equal or larger cut in government spending: "if we now had a massive tax reduction that was not accompanied by a large reduction in governmental spending, the chances are that the effects would be serious; that is, that the rate of inflation would be magnified."[277]

But, whereas Burns during the 1960s had moved closer to straightforward fiscal-policy orthodoxy, including a more standard perspective on tax cuts, Friedman in the same decade had gone the other way. He had, belatedly, been won over by Burns' prior argument that tax cuts had merit as a means of encouraging government spending.

Friedman's adoption of the starve-the-beast position

"I personally have shifted views," Friedman acknowledged in 1972. "I used to say that we should first get spending down, and then cut taxes. And I now say, let's get taxes cut under any and all circumstances… because that's the only way you'll get spending down."[278] The position—known in later years as the "starve-the-beast" view, although Friedman seldom used that phrase—became a trademark Friedman argument.[279] In Congressional testimony on October 6, 1969, Friedman succinctly encapsulated both the positive and normative economic dimensions of the "starve-the-beast" position on the source of growth in government: "I do not believe that the level of federal spending or appropriations is independent of the level of tax receipts. In fact, I believe that the only effective method of cutting down government spending is by cutting taxes."[280]

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[276] This volume—Committee on Ways and Means, U.S. House of Representatives (1978b)—was one of two books issued by congressional committees during 1978 (the other being Committee on the Budget, U.S. House of Representatives, and Committee on the Budget, U.S. Senate, 1978) that were made up of solicited submissions by economists giving their views on the Kemp-Roth bill and related proposals. Friedman was not one of the economists contributing to either volume. Nevertheless, his views on taxes were aired to a congressional committee in 1978, thanks to the fact that Milwaukee tax expert Arthur E. Friedman's testimony of January 16, 1978 (in Committee on Ways and Means, U.S. House of Representatives, 1978c, p. 415) gave two extended quotations from "Milton Friedman's article," specifically the Friedman *Newsweek* column of December 19, 1977. Having read out and endorsed these quotations, Arthur Friedman noted: "Incidentally, Milton Friedman is not related to me."
[277] In Committee on Ways and Means, U.S. House of Representatives (1978b, p. 28).
[278] *Firing Line*, PBS, January 5, 1972, p. 11 of transcript.
[279] Nelson (2009a, pp. 64–65) was an earlier analysis of the development of Friedman's views on the taxes/spending nexus.
[280] In Joint Economic Committee (1970a, p. 828). The prominence of the starve-the-beast component of Friedman's testimony was brought out in the headline given by the *Chicago Tribune* to its coverage of the testimony: "Cut Spending By Tax Slash—Friedman" (October 7, 1969).

That Friedman would take this position seemed jarring to many, as it was Keynesian economists who were perceived as the economists more accepting of budget deficits. Indeed, *Newsday* (November 5, 1968) had, as a means of contrasting him with the Keynesian stand on budget deficits, erroneously asserted that "Friedman is a pay-as-you-go man." But Friedman, while skeptical about the demand-boosting properties of budget deficits, had come to see deficits (or the prospect of them) as a potential restraint on the growth of public expenditure, and as therefore possessing what he saw as a desirable property.

The move to the starve-the-beast position marked something of a culmination of the evolution of Friedman's position on the roles of taxes. Kenneth Boulding's 1945 monograph *The Economics of Peace* had viewed the Keynesian revolution as having had the salutary effect of "get[ting] out of our heads that the primary purpose of taxation is to 'raise money for the government to spend,'" and of, instead, instilling the realization that "the true purpose of taxation [is] as the most powerful weapon in our hands for the stabilizing of money prices and incomes."[281] As discussed in Nelson (2020a, Chapter 3), during the Second World War Friedman had subscribed to the Keynesian view of taxes' function that Boulding described.

But Friedman's postwar research had dispelled his faith in the capacity of fiscal policy to stabilize (or, for that matter, destabilize) the behavior of prices. This research had also cast doubt on the importance of fiscal policy for the cyclical behavior of output. Having therefore eschewed what Boulding saw as the modern way to think about taxes, Friedman shifted, via his adoption of the starve-the-beast perspective, to the view of taxes' role that Boulding had treated as *passé*: that taxes financed spending. And if taxes financed spending, Friedman thought, control of taxes could be a device for controlling public expenditure. "I used to believe that we should first have fiscal responsibility, in the sense of getting rid of deficits before we reduce taxes," Friedman remarked in February 1972, before adding that this was something he no longer believed (Instructional Dynamics Economics Cassette Tape 93, February 23, 1972).

When did this shift occur? As already indicated, some accounts erroneously date Friedman's move to propounding the notion that tax revenues drive spending to as late as 1978. And, although some accounts have recognized his adherence to the starve-the-beast position was in force well before the decade of the 1970s, the precise point between 1958 and 1969 at which Friedman became a subscriber to the position has become a murky issue in the research

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[281] Boulding (1945, p. 165).

literature, and an erroneous, later date has been nominated. In this connection, Romer and Romer (2009b) cite a 1967 Friedman *Newsweek* column (August 7, 1967) as his earliest use of the starve-the-beast argument.[282] In that column, Friedman had remarked, "If taxes are raised in order to keep down the deficit, the result is likely to be a higher norm for government spending." Friedman suggested that, further along the line, a fiscal deficit would then reemerge once spending reached a still-higher level, with fresh pressure to raise taxes then materializing.

Aside from this discussion, the year 1967—when a federal tax increase seemed to be in the offing, and in which Friedman had his newly-acquired outlet of a *Newsweek* column to discuss fiscal policy proposals—was one in which he made several other expressions of the starve-the-beast perspective. In a television appearance on January 10 discussing President Johnson's State of the Union address, Friedman stated that "the main effect of the tax hike will be, not to cut the budget deficit, but to raise spending" (*Sun News* (Las Cruces, New Mexico), January 12, 1967). At a Stanford University business-school conference in February 1967, he observed: "I don't believe it is wise to increase taxes, because the main result will be an increase in government expenditures. We're now spending too much, [and] taxes already are too high, not too low."[283] In his *Newsweek* column of October 30, Friedman remarked that the course of taxes "may affect the level of government spending."[284]

Prior to these 1967 interventions, however, the starve-the-beast argument had already been attributed to Friedman in the London press on the basis of a talk that Friedman gave in the city of Chicago on November 22, 1966 (reported in *Chicago Sun-Times*, November 23, 1966). In that talk (subsequently excerpted in *The Banker*, January 1967), Friedman had indicated that he "strongly opposed" a tax increase. "Postwar experience," he observed on the same occasion, "has shown that Congress will spend whatever the tax system will raise, plus a little more. Hence, I believe a tax rise would simply foster a higher level of government spending." The *Financial Times* writer Harold Wincott drew attention to this Friedman statement by quoting it in

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[282] Ebenstein (2007, p. 167) also used this source. In stating that Friedman laid out the argument "as early as 1967," Ebenstein overlooks the fact that Friedman had, as noted below, been making the argument since the very early 1960s. Similarly, Rayack (1987, p. 188) implied that Friedman's use of this argument began only in 1967. The citation of Friedman's (1972c) *Economist's Protest* collection by Anderson, William, Wallace, and Warner (1986, p. 631) also indicated that they believed Friedman's starve-the-beast position originated in 1967 or later. The *Economist's Protest* collection included his 1967 columns endorsing that position, as well as a later one (July 15, 1968) in which Friedman had likewise stated that advancing the cause of tax cuts was "the only way to put an effective ceiling on federal spending."
[283] Reported the same day in *Oakland Tribune* (California), February 9, 1967.
[284] This aspect of Friedman's October 30, 1967, column was noted by Brill (1968, p. 4).

his column of January 3, 1967.

An observation in an article published in late 1967 by the junior U.K. Treasury economist, Stephanie Edge, provided a further indication that Friedman had used the starve-the-beast argument for some years. Edge (1967, pp. 202–203) stated that it was "*well known* that Friedman thinks that government expenditure is, to some degree, a positive function of tax revenue" (emphasis added). In documenting this point, Edge cited Friedman and Meiselman's 1964 *Review of Economics and Statistics* article. A footnote in this Friedman-Meiselman article had indeed stated that "since World War II… Congress seemed willing to spend whatever the tax system would raise plus a little more."[285] Friedman and Meiselman went on to cite Lewis (1964, p. 250) for empirical evidence on the matter.

The year 1964 does appear to be the earliest point at which Friedman made the starve-the-beast argument *in his writings*, not only in his article with Meiselman but also in his piece for the *New York Times Magazine* (October 11, 1964, p. 133), in the context of the Goldwater presidential campaign. He had also remarked in an interview early that year (*Freedom School Newsletter* (Colorado Springs, Colorado), March 1964, p. 4) that a tax "cut is desirable… to get the government smaller." But Friedman had deployed the argument earlier in the 1960s in other public statements and in conversations. An allusion to Friedman's views on the topic appeared in an October 8, 1962, *Financial Times* op-ed by Paul Samuelson, in which Samuelson referred (p. 8) to "the group who want to cut taxes in the hope that this will put pressure on the government to cut its expenditure."

Comparisons with Parkinson

The starve-the-beast argument had been made at length, some time before Friedman expounded it, by C. Northcote Parkinson, a writer who had achieved major success in the United Kingdom and the United States through a series of writings providing humorous social commentary. Parkinson (1960) had offered, as "Parkinson's Second Law," the regularity that "expenditure rises to meet income," and he attributed the twentieth-century increase in U.K. public spending to the growth in the central government's tax revenue.

Although they did not mention Parkinson in their 1964 joint article, both Friedman and

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[285] Friedman and Meiselman (1964, p. 375).

Meiselman later acknowledged Parkinson's Second Law. For example, the March 1971 issue of the U.K. magazine *Management Today* reported (p. 146): "The fiscal theory of the monetarists has prompted Meiselman to propound 'Parkinson's Law of the Fisc.' This semi-serious law holds that the level of government spending rises to meet the available revenue."[286] In his *Newsweek* column of July 12, 1976, Friedman mentioned and quoted Parkinson (1960). And, on several occasions, Friedman credited the starve-the-beast argument to Parkinson. One such instance was in September 1974, when Friedman used the term "Parkinson's Law" to mean the Second Law.[287] Another was in his 1978 article on tax limitation in which Friedman stated: "I believe along with Parkinson that government will spend whatever the tax system will raise plus a great deal more."[288] A 1977 book chapter on U.S. social security was another discussion in which Friedman invoked Parkinson.[289] In that analysis, Friedman labeled the proposition that spending rises to meet revenue "Parkinson's Law," and he indicated that his own amendment to the law was to acknowledge that spending in practice often exceeds revenue, but that the volume of revenue nevertheless acts as a significant restraint on spending.

Prior to these references being given by Friedman, it was sometimes implied that Friedman did not acknowledge his debt to Parkinson: in Sonenblum (1973, p. 101), for example, reference was made to "this idea of Milton Friedman—which he apparently took from Parkinson." But before this Sonenblum discussion appeared, Friedman had actually stated (Instructional Dynamics Economics Cassette Tape 109, October 18, 1972): "as Parkinson tells us, government's expenditures rises to meet its income." Indeed, Friedman acknowledged his debt to Parkinson very early, and he seems to have been converted to the starve-the-beast view of spending determination not long after Parkinson's book appeared. In *Business Week* (May 6, 1961, p. 113), Friedman judged that U.S. federal spending behavior had been "Parkinsonian to a *T*." He indicated on that occasion that he now supported tax cuts even when these were not accompanied by an explicit plan to cut spending commensurately.

The year 1961, therefore, seems to when Friedman commenced his advocacy of tax cuts as a means of putting downward pressure on government spending. Another early public

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[286] An example of the statements by Meiselman to which the article was referring was his Congressional testimony of October 13, 1969, in which he had referred to "the dilemmas posed by a Parkinson's Law of the Fisc that expenditures always rise to meet receipts." (In Joint Economic Committee, 1970b, p. 153.)
[287] Friedman (1974c, p. 76; p. 116 of 1991 reprint).
[288] Friedman (1978a, p. 12), reprinted in Friedman (1978a, p. 19). He had made similar remarks, including crediting Parkinson, in *New Guard*, April 1977 (p. 7).
[289] Friedman (1977p, p. 90).

endorsement he made of this idea was in December 1963: "I personally am now in favor of cutting taxes… [as] this is in the only way you will, in fact, get a cut in government spending." (*Christian Science Monitor* (Boston), December 5, 1963.) As already indicated, however, it was in 1967 that Friedman started becoming especially vocal in putting forward the revenues-drive-spending argument. Near, the end of that year, Friedman again drew upon it in a broadcast (December 24, 1967) of the revived *University of Chicago Round Table* radio program. After stating that postwar U.S. experience confirmed that tax revenue set a lower bound on federal spending, Friedman concluded that "if we want to keep down spending, we must keep down taxes."[290]

Starve-the-beast and beliefs in the size of the public sector

Those who shared Friedman's advocacy of restraint in government spending did not necessarily embrace his prescription regarding taxes. One notable dissenter with regard to Friedman's unqualified advocacy of tax cuts was Thomas Campbell. Friedman had been Campbell's faculty adviser when Campbell was an undergraduate student at the University of Chicago in the early 1970s. Campbell served in the U.S. Congress for most of the 1990s. As between deficit spending and tax-funded government spending, Campbell regarded the former as less undesirable. In addition, he eschewed reliance on tax cuts as a back-door way to restrain public spending. Campbell believed that the case for lower government spending should instead be made openly to the electorate—in a manner that allowed that case to be judged on its own terms. "If your real goal is to curb the size of government, you should try to do so and win on the merits. So, no, I did not believe in that. Running up the deficit and creating a fiscal crisis is not, to me, good public policy." (Thomas Campbell, interview, August 19, 2015.) On this view, tax cuts should not be called upon as a mechanism for holding down public spending even if, as a matter of positive economics, it was the case that revenues drove expenditures.

In the course of the aforementioned 1967 radio debate, Friedman acknowledged that one of his opponents in the debate—Robert Eisner—agreed with him on the positive-economics issue.[291] Indeed, just as Eisner had bridged the divide between Friedman and the Keynesians by incorporating the permanent-income theory into his analysis of the 1968 surtax, the very Keynesian Eisner found himself in agreement with Friedman on the position that tax receipts drove government spending. Eisner (1971, p. 77) stated, "in mathematical shorthand, $\delta G/\delta t$, the

[290] See the excerpt in *University of Chicago Magazine* (February 1968, p. 3).
[291] Again, see *University of Chicago Magazine* (February 1968, p. 3).

partial derivative of government expenditures with respect to tax rates, is positive. That is a fact that conservatives have known for a long time, but one which my liberal friends are sometimes reluctant to acknowledge. If the government is receiving more tax money, given the realities of the political world in which we live, it is likely to spend more."[292]

John Kenneth Galbraith was sympathetic to the position that tax revenue drove government spending. In contrast to Friedman, however, Galbraith regarded the regularity as a case *against* the cutting of taxes. He opposed both the Kennedy Administration's initial 1962 tax cut and the major 1964 tax cut of the Johnson Administration (*New York Times*, May 14, 1966). Robert Solow, a CEA member during the early Kennedy years, was aware of Galbraith's views on the matter during the internal governmental deliberations on the tax cut (Robert Solow, interview, December 31, 2013.) Indeed, although Bartlett (2007, p. 6) implies that it was only in the second half of the 1960s that Galbraith made known his disapproval of the tax cuts, along with the revenue-drives-spending basis for his opposition, Galbraith's position on this matter was publicly known during the first half of the 1960s, as both Galbraith's and James Tobin's subsequent commentaries on the matter (*in New York Times*, May 14 and 19, 1966, respectively) made clear.

Like Galbraith, Robert Eisner rejected the position that Friedman took that the realization that taxes drove public spending meant that taxes should be cut. Indeed, to those who favored greater government spending than Friedman did, the message of the argument seemed to be that raising tax revenues was an important requirement. That had indeed already been viewed as part of the realities of public finance by President Kennedy, according to the accounts of Walter Heller to which reference has already been made (those in Heller, 1966, p. 113, and *Sunday Times*, June 20, 1965). Heller recalled that Kennedy saw an elevated public-expenditure program as more politically feasible if it was proposed in the wake of a surge of tax revenue.

Friedman believed that this was also the political reality in the U.S. national discourse in the era after Kennedy's death. He attributed to President Lyndon Johnson the view that elevated tax levels needed to be maintained in order to promote high levels of public spending (Instructional Dynamics Economics Cassette Tape 11, January 1969). A particular development that Friedman would stress was the role played by the U.S. inflation over the 1960s and 1970s. As this took place in the context of unindexed income tax brackets, he saw it as an important source of growth in federal government spending—thanks to the fact inflation allowed tax revenue to rise

[292] The statement would have better relayed the point if "tax revenues" had been used instead of "tax rates."

in real terms.[293] He would repeatedly recall a remark that Senator Russell Long, of the U.S. Senate's finance committee, had made to him that the growth in the federal government's expenditure programs during the 1970s would not have been possible if the increase in the tax burden had had to be carried out via explicit legislation.[294] The developments of the 1970s would play a major role in making Friedman a very prominent advocate of tax cuts—as well as in leading him to support institutional limitations on tax revenue, of the kind implied by formal tax-limitation measures or by automatic indexation of tax brackets.

Friedman's affirmations of the starve-the-beast position, 1975 to 1978

The starve-the-beast posture remained a maverick position during the 1970s. A 1974 OECD publication, for example, presumed that the opposite view was correct when it observed that in recent years "the increase in proportion of GNP being absorbed by Government expenditures has led to high levels of personal taxation."[295] But as the federal government's spending and taxation shares had both risen since the 1960s, Friedman regarded his position as having been vindicated. Much fiscal policy analysis worked on the idea that government "expenditure is somehow independent of how much the tax system raises," Friedman observed at the American Enterprise Institute's panel discussion of indexation in mid-July 1974, and he believed that that position had been refuted by the fact that the shares had risen largely in unison.[296] Yes, he granted, the ratio of the federal budget deficit to national income had also risen. But, he noted, its rise had been in small in relation to the underlying tax and spending shares themselves.[297]

Friedman also reiterated the starve-the-beast view numerous times in his regular taped commentaries for subscribers. In this series, he remarked in 1974: "In the long run, Congress tends to spend whatever the tax system will raise, plus a good deal more."[298] In a 1975 entry, he affirmed: "I am in favor of cutting taxes under any circumstances for any reason or any excuse… because I believe that that is the only effective way really to cut government spending."[299] And, in late 1976, he again contemplated whether public spending was as high as it is primarily

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[293] For an early expression of this position, see *Newsweek*, July 15, 1968.
[294] See Friedman's remarks in Snowdon and Vane (1997, p. 210) and O'Driscoll and others (1997, p. 8), as well as his April 1996 remarks at Claremont McKenna College (broadcast on CSPAN, December 26, 1996). Friedman and Friedman (1985, p. 44) recounted the anecdote without identifying the senator by name.
[295] OECD (1974, p. 9). This quotation appeared in an OECD monograph on the negative income tax (a subject that the monograph considered without mentioning Friedman or any of his publications on the subject).
[296] In American Enterprise Institute (1974, p. 47).
[297] In American Enterprise Institute (1974, p. 48).
[298] Instructional Dynamics Economics Cassette Tape 160 (December 19, 1974).
[299] Instructional Dynamics Economics Cassette Tape 162 (January 1975, Part 2).

because of the tax revenue, and he stated: "My own conclusion… based… on a great deal of other material is that, on the whole, Parkinson is right when he says that spending rises to meet the income [that is, tax revenue, in the government's case] available."[300]

Friedman's reiterations of the starve-the-beast argument were so frequent in the six years through early 1978 that it is all the more surprising that multiple retrospectives have, as noted above, given him as articulating the position starting only in summer 1978 (or later). True, the audio commentaries just noted were not circulated widely. But the 1974 indexation debate was hardly low-profile: in addition to having audiotaped and printed versions, it was turned into a televised special. During 1972–1976, Friedman also articulated normative or positive versions of the starve-the-beast argument in his *Newsweek* column and in Congressional testimony in 1972–1976, as well as in further pieces that appeared in print during the same period.[301]

In early 1975, with tax cuts high on the national political agenda due to President Ford's proposed income tax rebate, Friedman criticized the specific proposal but added: "Our real concern should be, not the size of the deficit, but the size of government spending. If a tax cut, resulting in larger deficit, is a way to limit government spending, then I am for [it]." (*Journal of Commerce* (New York), March 4, 1975, p. 5.) In an interview two years later, he again confirmed that he was always in favor of cutting taxes in order to create pressure to restrain government spending (*New Guard*, April 1977, p. 7). And in his February 1978 speech on tax reform, Friedman remarked that in the wake of successive increases in taxes, "the result is higher and higher government spending."[302] In sum, there was a surfeit of public statements by Friedman before summer 1978 affirming the starve-the-beast argument.

And as the debate on the Kemp-Roth tax cuts proceeded in the U.S. press in summer 1978, two former heads of the Council of Economic Advisers indicated that they, too, rested their support for the Kemp-Roth bill on starve-the-beast arguments. As indicated earlier, by 1978 Herbert

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[300] Instructional Dynamics Economics Cassette Tape 198 (September 1976, Part 1). Other audio commentaries over this period in which Friedman suggested that tax cuts promoted government spending restraint or that that tax increases would promote more public expenditure, or in which he reiterated his support for tax cuts at any time included Instructional Dynamics Economics Cassette Tapes 155 (October 10, 1974), 161 (January 1, 1975), 176 (September 1975, Part 2), 178 (October 1975, Part 2), 197 (August 1976, Part 1), 202 (November 1976, Part 1), 203 (November 1976, Part 2), 207 (January 1977, Part 2), and 213 (May 1977, Part 1).

[301] See Friedman's *Newsweek* columns of November 6, 1972, and May 12, 1975, and his testimony of June 21, 1973, in Joint Economic Committee (1973a, p. 130), and of January 22, 1976, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1976, p. 2190). See also, in addition to the items already mentioned, Friedman (1974c, p. 85; p. 125 of 1991 reprint) and Friedman's 1972 discussion in Selden (1975, p. 195).

[302] *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978, p. 10 of transcript.

Stein had been conscious for a quarter-century of arguments that federal tax cuts raised tax revenue. In a *Wall Street Journal* article that appeared at the height of that newspaper's Laffer-curve debate, Stein reaffirmed his skepticism about the article and indicated that his own analysis of a tax cut's effect, for given government spending, was similar to Walter Heller's. Nevertheless, Stein parted company with Heller on the desirability of a tax cut, as Stein saw a "simple and strong" case for passing the Kemp-Roth bill as resting on the likelihood that the resulting reduction in revenue would discourage new public-expenditure programs.[303] Similarly, Alan Greenspan stated (*Omaha World-Herald* (Nebraska), July 27, 1978) that provided "there is a political limit to deficit spending," it was the case that the "basic purpose of any tax cut program in today's environment is to reduce the momentum of [government] expenditure growth by restraining the amount of revenue available."

As for the term "starve the beast" itself, it was one Friedman largely eschewed. Nevertheless, and contrary to what some subsequent accounts have suggested, it dated back at least to the period of the late-1970s tax revolt. The terminology therefore predated the Reagan era, even though Bartlett (2007, p. 6) could not locate an instance of usage of it in the public record before 1985 and Paul Krugman (*New York Times* online, September 14, 2003) categorically, but erroneously, stated that "starving the beast" was a "phrase coined by David Stockman, Ronald Reagan's budget director."[304] In fact, in April 1979, a local Californian newspaper ran an article concerned with "the tax-cutting aftermath of Proposition 13" that quoted a local council member, Jerry Wilhelm, saying: "We must force governing boards to examine how money is being spent. 'Starve the beast' us the therapeutic result of Prop. 13." (*Argus-Courier* (Petaluma, California), April 11, 1979.)

Finally, it should be underlined that, despite his many enthusiastic statements about limiting taxes as a means of setting off political mechanisms that restrained public spending, this was not Friedman's preferred approach to lowering government expenditure. He continued to favor measures (with a particular preference, from the 1970s onward, for the method of constitutional

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[303] *Wall Street Journal*, July 18, 1978, reprinted in Laffer and Seymour (1979; quotation from p. 53).

[304] Krugman made the same attribution of the phrase to David Stockman in *New York Times*, December 23, 2005—with Krugman claiming on that occasion that Stockman "memorably" deployed the phrase. D.H. Romer (1988, p. 63) earlier noted that the judgment that budget deficits engender spending restraint had been associated with Stockman by Senator Daniel Patrick Moynihan. Moynihan made this association in accounts reported in *New York Times*, July 11, 1985, p. A14, and *Washington Post*, January 14, 1993. In the first of these reports, Stockman also denied the attribution. Of the two news reports, the term "starve the beast" (or variants thereof) was only actually used in the latter, 1993 report, in which Moynihan used the phrase but made no claim that Stockman specifically deployed it.

amendment) that *directly* put limits on government spending—rather than relying on the indirect method of either tax cuts or tax-limitation amendments.

III. PERSONALITIES IN DEBATES ON REGULATION AND AGGREGATE SUPPLY, 1977–1978

JAMES DUESENBERRY

Friedman's proposals regarding the U.S. tax system, as expressed in his April 1976 *Newsweek* exposition of the case for applying a flat rate to American households, became a topic of discussion at an American Enterprise Institute conference, held in May 1976 in Washington, D.C.

Despite having an AEI affiliation over most of the 1970s, Friedman was rarely seen at AEI events during that decade, and he did not attend this particular conference, whose overall subject matter was income redistribution.[305] The conference's published proceedings, which appeared in 1977, showed the considerable attention that Friedman's column on tax reform had received at the conference—even though it had been an instance of popular writing and the AEI event had been a symposium showcasing applied research.

One of the notable commentaries on Friedman's piece recorded at the proceedings was that of James Duesenberry, professor of economics at Harvard University. Duesenberry indicated that he would likely differ from the column's degree of emphasis on efficiency as the criterion for designing tax reform. But he added: "I must confess that I have not read the article."[306]

Duesenberry's reaction captured three facets of his relationship with Friedman and with the macroeconomic debates in which the latter was engaged. First, Duesenberry had a posture that was largely one of opposition to Friedman. Second, unlike other high-ranking Friedman opponents, such as Paul Samuelson, Duesenberry was not heavily concerned with keeping tabs on Friedman's writings and with reacting to what he disagreed with in those writings. And third, by commenting on a Friedman piece that he had not read and doing so at a conference that

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[305] It should be distinguished from a conference (mentioned in Section I above) on income distribution on January 29, 1977—one held at the Hoover Institution and which Friedman, who was then nearing the end of his first month in California, did attend.
[306] In Campbell (1977, p. 173).

Friedman was not attending, Duesenberry underlined the peripheral nature of the role that he and other Harvard University economists had ended up playing in the Keynesian-monetarist debates of the 1960s and 1970s.

Harvard University and the Keynesian-monetarist debates

"Since a number of the leading North American Keynesians were located MIT and Harvard, the early debate between monetarists and Keynesian economists was often characterized in geographical terms: Chicago versus Cambridge (Mass.)" That was the retrospective judgment of Sheffrin, Wilton, and D.M. Prescott (1988, p. 168).

There was some substance in this characterization. This is evident in the fact that in 1977, as noted above, Rose Friedman juxtaposed Cambridge and (the University of) Chicago as rival bases of U.S. macroeconomic opinion. And, certainly, the presence at MIT of such prominent Keynesians—and longtime sparring partners of Friedman—as Paul Samuelson, Robert Solow, and Franco Modigliani amply justified regarding Cambridge, Massachusetts, as a key base of Keynesian economics during the 1960s and the 1970s. But the size and prominence of this MIT contingent raise the question of how valid the "Chicago versus Cambridge (Mass.)" label was as an encapsulation of the Keynesian-monetarist dispute, as opposed to "the University of Chicago versus MIT." Specifically: Did Harvard University pull its weight sufficiently to consider it—rather than MIT alone—as contributing over this period to the perception that Cambridge, Massachusetts, was a hub for U.S. Keynesianism?

In the consideration of this question, it will be seen below that Harvard University's Duesenberry did, in fact, play some role in the Keynesian-monetarist debates of the 1960s and 1970s, and that his activities in these debates included direct engagements with Friedman. Nonetheless, very little of his direct disputation with Friedman reached print, and he and the other Harvard University economists of his generation did not come to be seen as major figures in the Keynesian-monetarist debates.

During the 1940s and 1950s, Harvard University was a major center when it came to the advocacy of Keynesian economics and to criticism of the quantity theory of money, thanks to the presence of Alvin Hansen. In the 1960s and the 1970s, however, economists who taught at the university tended to have a low profile in the Keynesian-monetarist debates. Duesenberry, who had been teaching at the university since 1946 (Committee on Banking and Currency, U.S.

Senate, 1966, p. 2; *Record American* (Boston), January 1, 1969), formed the closest to an exception to this generalization—not really because of direct debates with Friedman, but because of his prominent status as a Keynesian.

To be sure, among Friedman's contemporaries, there was another Harvard University economics professor who had a far higher profile than Duesenberry as an adversary of Friedman's: John Kenneth Galbraith. But Galbraith largely absented himself from the formal economic-research debates. Furthermore, Galbraith's views, areas of greatest interest, and his choice of publication outlets all put him at a distance from the macroeconomic debate taking place in U.S. academia. Among more orthodox economists at Harvard University, therefore, the leading macroeconomist in this period, was, indeed, Duesenberry.[307] He was also the leading proponent of Keynesian economics at the institution after Hansen stepped back.[308]

Duesenberry's most fundamental research contribution was work on consumer behavior, and he would be credited in that connection with having introduced the concept of habit formation. His major consumption work (Duesenberry, 1949) was interpreted by Friedman as implying a "relative income hypothesis" to which Friedman's permanent income hypothesis was a challenge.[309] But, in time, Duesenberry's main contribution to the theory of consumption has come to be regarded as reasonably compatible with Friedman's.[310] The models used today in benchmark New Keynesian analysis tend to embed habit formation into the representative agent's utility function, thereby embedding Duesenberry's habit-based theory of consumption

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[307] During this period, Duesenberry also tried to keep Harvard University's economics department in the mainstream in the face of pressures to orient the department toward more radical or revolutionary economics. He made national headlines in 1973 when, as department chair, he was involved in discharging a couple of members of the department who had described themselves as developing a radical or Marxist critique of economics. Duesenberry was quoted as saying that the research output coming from this approach had not justified the department continuing the individuals in employment (*The Evening Star and Daily News* (Washington, D.C.), January 3, 1973).
[308] Franklin Fisher received his undergraduate degree from Harvard University in 1956, followed by a doctorate (see American Economic Association, 1970, p. 136). As an undergraduate, Fisher took courses from both Duesenberry and Alvin Hansen. He recalled Duesenberry as having given a "pretty good" course in macroeconomics. With regard to Hansen, Fisher observed: "I took one course from him [and] at that time I didn't think he was very good… He got mixed up a lot." Fisher further noted that "my recollection is that Galbraith was sort of certainly not central to the department." But he added that, on an occasion when he invited Alvin Hansen to an economics event organized by the students that Galbraith also attended, Fisher was struck by the extent to which Hansen regarded Galbraith as an heir: "Hansen basically said to Galbraith that he thought his most recent book was very good and we that didn't need any more of these [exercises in] mathematics. I was not impressed." (Franklin Fisher, interview, interview, January 16, 2015).
[309] See Friedman (1957a, p. 4).
[310] However, an article in the *New York Times* of June 4, 1995, was an example of a late discussion that, in arguing for the merits of Duesenberry's model of consumption, presented it as incompatible with the permanent income hypothesis (on which the piece offered a highly negative perspective).

into Friedman's permanent-income hypothesis (see, for example, Fuhrer, 2000; Christiano, Eichenbaum, and Evans, 2005).[311]

In U.S. economic policy, Duesenberry's credentials as a major Keynesian were cemented by his being a member (in 1966–1968) of the Council of Economic Advisers under President Johnson, and so he was one of those applying and expounding the "New Economics" in the national economic policy arena. At his confirmation hearing on January 20, 1966 (Committee on Banking and Currency, U.S. Senate, 1966, p. 3), Duesenberry observed: "I think that the basic ideas which go under the heading of the policy of new economics are very solidly based on the best kind of evidence that we can get—a great deal of it."

But as his participation in a 1969 conference (discussed below) attested, Duesenberry also worked in the field of monetary economics.[312] He taught in that area as the university's professor of money and banking (Duesenberry, 1983, p. 123) and, as noted in Nelson (2020b, Chapter 11), Friedman sent Duesenberry the 1961 draft of the Friedman-Schwartz *Monetary History* monograph. As was so often the case with Keynesians of his generation, Duesenberry found Friedman's work on consumption far more congenial than the Friedman research on money (Niels Thygesen, interview, February 10, 2015).[313]

The opposition to Friedman's monetarism coming from Duesenberry and other representatives of mainstream Keynesianism at Harvard University mainstream produced little of a legacy in terms of printed work. It became well established, nevertheless, thanks to oral traditions that proceeded in outlets that did not produce a written product. On the basis of his knowledge of the Harvard economics department of that era, Kenneth Arrow expressed confidence that "at Harvard they didn't take Friedman too seriously" (Kenneth Arrow, interview, December 7,

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[311] The precise way in which habit formation is specified tends to differ across models—in particular, on whether the habit formation is based on so-called internal and external habits.

[312] This fact was stressed in his January 1966 confirmation process. A letter by Senator Edward Kennedy endorsing Duesenberry's nomination to the CEA (see Committee on Banking and Currency, U.S. Senate, 1966, p. 1) described him as "the author of a number of excellent books and articles on consumer and business theories, economic growth and monetary policies." In the confirmation hearing, Senator William Proxmire stated to Duesenberry: "You are an expert on monetary policy, too." (January 20, 1966, hearing, in Committee on Banking and Currency, U.S. Senate, 1966, p. 3.)

[313] A similar evaluation of the respective merits of different elements of Friedman's macroeconomic work was prevalent among Duesenberry's colleagues at the university. (Deirdre McCloskey, interview, August 17, 2015.)

Although he used the "permanent income" terminology (see Duesenberry, 1963, p. 21), Duesenberry himself does not seem to have been altogether won over by the logic of the permanent income hypothesis. Notably, even in 1983, he viewed the 1968 tax surcharge as effective in restraining aggregate demand and as leading, in conjunction with monetary restraint, to the downturn of 1970 (see Duesenberry, 1983, p. 135).

2013). Such a characterization is consistent with the statement of Wassily Leontief, a longtime presence in Harvard University's economics scene, that "I oppose Milton Friedman 100 percent."[314] In the same vein, the *New York Times* (January 25, 1970, p. 22) characterized Friedman as someone regarded as a heretic at both Harvard University and MIT. A negative attitude toward Friedman was conveyed at Harvard University in internal forums, including economics classes. McCloskey (2000, p. 68) recalled that "the best way for a professor to raise a laugh at Harvard in the 1960s was to mention the name of Milton Friedman." In addition, the campus newspaper *Harvard Crimson* (May 5, 1964) observed that Friedman was treated as a "bogeyman" in the first-year undergraduate economics course at the university.

But, of the economists at two universities, only those at MIT really prominently communicated their opposition to Friedman in print or in other major outside forums. In between Hansen's retirement and Benjamin Friedman's emergence, during the 1970s, in the field of monetary economics, the eminence of Harvard University in monetary debates receded. Francis Bator, watching developments from the vantage point of the university's school of government, observed: "Alvin Hansen, early on, played a major role at Harvard. [There was] Hansen, Arthur Smithies, and Jim Duesenberry but… it sort of thinned out on the macro side." (Francis Bator, interview, January 6, 2015.)

The consequence of this lull was that published debates between Milton Friedman and Harvard University figures were almost negligible. "After Hansen, Jim Duesenberry played an important role but wasn't first order in this [the Keynesian-monetarist exchange], and so in a way, the Harvard department played a lesser role." (Francis Bator, interview, January 6, 2015.)[315]

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[314] Quoted in *Business Times* (Singapore), March 26, 1982. By the time he made this statement, Leontief was at New York University. However, he had worked at Harvard University from 1939 to 1975 (American Economic Association, 1981, p. 252).

Another longstanding Harvard University economist, Otto Eckstein, made a similarly negative judgment that "monetarist forecasting never was a serious undertaking" (Eckstein, 1976, p. 13). However, Eckstein qualified this statement with the acknowledgment that forecasts by Friedman and others using monetary aggregates had done better, during key points in the 1960s, than formal forecasts (of the kind with which Eckstein was associated) based on larger datasets and more elaborate econometric procedures.

[315] Another figure at Harvard University starting in 1962 (not at the department, but instead located at the business school) whose interests included monetary economics was Eli Shapiro. Shapiro had been an associate professor of economics at the University of Chicago's business school in 1946–1952, also serving as an editor of the school-managed Journal of Business, before being a professor of finance at MIT in 1952–1962 (see NBC, 1952, p. ii; American Economic Association, 1970, p. 396). Over and above his time at the University of Chicago, Shapiro had numerous occasions to interact with Friedman—he was a Columbia University graduate student in the late 1930s through 1945 (years during which Friedman was often also affiliated with the university), was deputy director of research for the Commission on Money and Credit, and participated along with Friedman in the conference recorded in Ketchum and Strunk (1965). However, Shapiro's specialities in economic research related to matters of corporate

This lesser role partly reflected the fact that, despite Duesenberry's undoubted contribution to macroeconomics, Harvard University in this era was not setting the pace in the monetary field or in economics more generally. Deirdre McCloskey attributed part of the reason for the low profile of Harvard University economists during the 1950s and 1960s, in relation to those at other institutions, to the fact that the Harvard University economists of the period tended to be technically outclassed. "I mean, Paul Samuelson ran rings around them. He ran rings around them [even] when he was there [as a graduate student]." McCloskey added: "The great success story of the Fifties in economics was MIT. And, if you can get beyond the ideology, the other success story, which became fairly obvious when I was there [1968–1980], was Chicago." (Deirdre McCloskey, interview, August 17, 2015.)

This status of Harvard University in economic research during the late 1950s and the 1960s, especially with regard to monetary economics, is brought out by the options facing David Laidler in 1960, when he chose graduate study at the University of Chicago. "I actually was offered money by quite a few places, and one of them was Harvard. If I had gone to Harvard, I would've taken price theory from Chamberlin, and macroeconomics from Leontief, and God knows what else. Chicago was offering me Milton for price theory, and Harry Johnson for [money/]macro, so I went to Chicago. I think Harvard was just tapped out, they needed renewing." Laidler affirmed that in Cambridge, Massachusetts, in the 1960s, when it came to research on macroeconomics and monetary issues, it was mainly the case that "the action was at MIT." (David Laidler, interview, November 6, 2014.)

Over the 1960s and the early 1970s, Harvard University did produce a number of Ph.D. graduates in economics—including Christopher Sims, Charles Goodhart, Donald McCloskey (later Deirdre McCloskey), John Eatwell, and Benjamin Friedman—who would become prominent critics of monetarism in the research and policy debates in the United States and the United Kingdom during the 1970s and 1980s.[316] But the university's researchers and teachers in

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and international finance that had little to do with the Keynesian-monetarist debates. With regard to domestic monetary economics, the coverage of Friedman in the Eli Shapiro, Ezra Solomon, and William L. White (1968) textbook was light but not markedly negative. And, with regard to international monetary economics, the position that Shapiro took during the 1960s was similar to Friedman's, in favoring liberalization of the existing system (see, for example, Eli Shapiro, 1966). Indeed, a favorable attitude toward Friedman's position on international monetary arrangements—as distinct from his views on domestic monetary policy—was something that Lawrence Officer felt was pervasive also at Harvard University's economics department during the 1960s (Lawrence Officer, interview, January 10, 2016).

[316] Another prominent critic of monetarism, Fischer Black, was also a graduate of Harvard University in the 1960s, albeit with a Ph.D. in mathematics rather than economics.

economics during the heyday of the Keynesian-monetarist debate did not include an individual who had a role matching that of Solow, Samuelson, or Modigliani at MIT (or of Tobin at Yale University): that is, a figure who prominently and repeatedly challenged Friedman's views on monetary economics or on aggregate-supply issues in research journals or in macroeconomics-focused conferences.

Accordingly, in contrast with Friedman's frequent entanglements with MIT economists, Robert Solow observed, "I don't think that Milton ever had any close association with anyone on the Harvard faculty." Solow went on: "He certainly knew Duesenberry, and Duesenberry knew him—they were not strangers—but they were not people who would normally converse. Milton was certainly always much more in contact with Paul [Samuelson] or with me than with anyone at Harvard… I don't remember him ever giving a talk at Harvard, but I'm sure he must have. But he really was not close to anyone like John [H.] Williams, or Alvin Hansen, or as I said, Duesenberry, at all." (Robert Solow, interview, July 7, 2014.)

Friedman on Harvard University's Keynesians

Friedman, for his part, rarely referred to currently-active Harvard University economists when discussing monetary topics or aggregate-supply issues. But he did direct attention to older-vintage writings originating from Harvard University's: During the 1960s and 1970s, Friedman had a penchant for quoting some of the more hardline anti-monetary-policy statements of the university's prominent macroeconomists of the 1940s, Alvin Hansen and John H. Williams.[317]

Friedman nevertheless had considerable exposure to Harvard University economists on the

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It should also be noted that three graduate students in economics at Harvard University during the 1960s, Robert Barro, Thomas Sargent, and David Sheppard, would in the 1970s become associated with the monetarist side of the Keynesian-monetarist debate (in Sheppard's case, in the U.K. version of the debate). Furthermore, Lawrence Officer, who over the 1960s was both a graduate student and an economics-department member at Harvard University, became sympathetic to Friedman's monetary economics after he left the university, as his own research findings made him more favorably disposed toward the monetarist position (Lawrence Officer, interview, January 10, 2016).

[317] See Nelson (2020a, Chapters 3 and 4). A remark that could be regarded as a sideswipe against Harvard University on Friedman's part was his endorsement (in *Newsweek*, September 13, 1982) of William Buckley's dictum that he would prefer to have his fate in the hands of the first 1,000 names listed in the Boston telephone directory than in the hands of the instructors at Harvard University. In this statement, however, Buckley apparently sought primarily to caution against reliance on elites rather than to cast aspersions on Harvard University (still less its economics department) specifically. Friedman's endorsement of the Buckley dictum could be interpreted in the same manner—although Friedman's own clashes with the university's economists on macroeconomic issues might have been an additional reason for his agreement with Buckley on the point.

conference circuit, and he in effect acknowledged the predominance of Keynesianism at Harvard University in a letter to George Shultz in July 1972. In that letter, Friedman expressed the assessment that only Dale Jorgenson (whose work on investment Friedman admired, and who in turn held the *Monetary History* in high regard) and Hendrik Houthakker (a former member of the Nixon Administration's Council of Economic Advisers) were the only people at the university who shared the general economic viewpoint that he and Shultz had.[318] Similarly, in a letter to Robert Barro of August 5, 1987, Friedman congratulated Barro on the latter's recent move to Harvard University's economics department but implied, in a jocular manner, that both he and Barro both had a longstanding adversarial relationship toward the macroeconomics prevalent at the university (Robert Barro, personal communication, March 12, 2019).

Duesenberry's research output and his interactions with Friedman

In a talk given in July 1981, Friedman came closer to identifying James Duesenberry specifically as one of his old sparring partners in the Keynesian-monetarist debate. "In preparing for this talk," Friedman observed, "I reread the proceedings of a conference held at Nantucket under the auspices of the Boston Federal Reserve Bank in June 1969. Out of that conference came a book entitled *Controlling Monetary Aggregates*. It was a depressing experience to reread those proceedings. The same people are now saying the same things they were saying then. The people who were then talking about the difficulty or impossibility of controlling the quantity of money through controlling the base are still saying much the same thing."[319] Though unnamed, the "people" to whom Friedman was referring were those contributors on the anti-monetarist side who were still active in the monetary field in 1981. These were: Paul Samuelson; James Tobin; Henry Wallich (in 1969 a researcher, in 1981 a policymaker); Sherman Meisel (in 1969 a policymaker; a 1981 a researcher)—and Duesenberry.

Duesenberry's article for the 1969 conference pointed to drawbacks of both interest-rate-oriented and reserves-oriented approaches to monetary policy, he singled out "a simplistic money supply policy" (p. 93) as particularly problematic.[320] He spoke more favorably of an "income-

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[318] Friedman letter to George Shultz, July 2, 1972, in the Friedman archives at the Hoover Institution. By this time, Gottfried Haberler had left Harvard University. As discussed below, Haberler differed from Friedman on matters concerning inflation. However, on other monetary issues, they had agreement—including, as Friedman noted (Instructional Dynamics Economics Cassette Tape 9, January 1969) on fixed versus floating exchange rates.
[319] Friedman (1982a, p. 103).
[320] With regard to the *feasibility* of monetary-aggregate control, Duesenberry was less negative. However, Duesenberry (1977, p. 54) later stated that he believed central banks could achieve "as much control" over the money stock as they wanted only "over any decade (although not in the very short run)." This contrasted with

expenditure-credit conditions' approach to monetary policy" (Duesenberry, 1969, p. 94) that involved targeting forecast real output growth, based on the estimated gap between output and potential output, and that also took into account balance-of-payments considerations (pp. 93–94).

Other than this piece, Duesenberry's antagonism toward monetarism was expressed only sporadically in print. A skeptical, but not highly critical, published comment on Friedman's demand-for-money work had appeared as Duesenberry (1959). But that very brief discussion comprised almost the only case in which Duesenberry provided a sustained published scrutiny of Friedman's monetary contributions.[321] Indeed, when Duesenberry and Friedman were both participants in the 1962 NBER conference in Pittsburgh on the state of monetary economics, Duesenberry's paper—which opened the conference and appeared right before the famous Friedman-Schwartz "Money and Business Cycles" paper in the published conference issue in 1963—did not mention Friedman at all, despite being concerned with money demand.[322]

It is the case, though, that during the 1960s Friedman and Duesenberry were pitted against each other in a number of forums for which no proceedings volumes were published.[323] These included American Bankers Association conferences and consultants' meetings at the Federal Reserve Board. At the American Bankers Association meeting of 1963 or 1964, Friedman and Duesenberry debated the Friedman-Meiselman study of monetary and fiscal policy effects. Paul Wonnacott, an attendee of the conference, recalled: "Duesenberry was getting very red—I mean, he was clearly very angry. And he said, 'If you really want to know, Milton, that study just gives me one fat pain in the neck.'" (Paul Wonnacott, interview, May 12, 2014.)

Duesenberry had, by this point, likely been simmering about the Friedman-Meiselman paper for

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Friedman's position that, in a fractional-reserves regime, monetary control was achievable over horizons of one to two quarters. Duesenberry's 1977 position therefore lined up with Friedman's (1982a) characterization of Duesenberry as one of those pointing toward the impossibility of money-supply control over the horizon Friedman believed achievable.

[321] A short introductory monograph on monetary matters that Duesenberry wrote (and that appeared multiple times in the 1960s and 1970s) mentioned Friedman (1960a) and Friedman and Schwartz (1963a) on the final page, with little comment, as further reading (Duesenberry, 1972, p. 136). The main text had made note of Friedman as "the best-known proponent of the quantity theory" (Duesenberry, 1972, p. 100) and acknowledged, but implied dissent from, the *Monetary History*'s position that monetary expansion could arrest depression (Duesenberry, 1972, p. 89).

[322] See Duesenberry (1963).

[323] Also in common with MIT Keynesians, Duesenberry is said to have reacted strongly and negatively to Gary Becker's work on the economics of the family. Indeed, Heckman (2014, p. 14) states that Friedman "frequently mentioned" a strongly negative presentation that Duesenberry made when he was the assigned discussant of Becker's work in this area at a 1959 event. However—as has been found in the case of Duesenberry's sparring with Friedman—Heckman notes that little of the Becker-Duesenberry contretemps actually made it into the published record.

a few years, as Friedman had presented an early version of it at Harvard University on February 17, 1960 (Tavlas, 2022, pp. 10–11).[324]

As already indicated, Duesenberry would later express his disagreement more openly in print, as in his contribution to the 1969 Federal Reserve Bank of Boston conference volume. That paper took issue with "the so-called monetarists" (Duesenberry, 1969, p. 87), although it did not refer to Friedman by name.[325]

Duesenberry on monetary policy

Although he disagreed with Friedman's conclusions about the importance of fiscal and monetary policy, Duesenberry was one of the Keynesians of his generation who (in contrast to the sometime practice of Okun and Tobin) in analytical work primarily treated the stock of money, rather than interest rates, as the variable set by the monetary authorities. Christopher Sims, a Duesenberry student during the 1960s, recalled that he took the same perspective in his own early work (such as Sims, 1972), "having been raised on actually Friedman, Modigliani, Duesenberry—all these guys treated *M* as a policy variable." (Christopher Sims, interview, March 15, 2013.) Consistent with this characterization, Duesenberry (1972, p. 4) stated: "The Federal Reserve System has the job of managing the money supply."

However, although he was not averse to viewing Federal Reserve policy in terms of its implications for monetary growth, it was far from the case that Duesenberry was sold on having U.S. monetary policy follow a monetary-growth rule, still less a *nonactivist* monetary-growth rule of the kind Friedman favored. Rather, when in 1969 he expressed the desirability of putting a "proposed course of action in quantitative terms," he indicated that this was "not necessarily [in] quantity of money terms" (Duesenberry, 1969, p. 94). Furthermore, as already indicated, he wanted the settings of monetary policy to be guided by the estimated size of the output gap and by a number of other policy objectives. It is clear, therefore, that although Duesenberry seemed willing to contemplate using the behavior of monetary growth as one criterion on which to judge

[324] Other than Duesenberry, another Harvard University macroeconomist with whom Friedman had numerous interactions was Francis Bator. Bator (who also had close past links with MIT, having studied and taught there) was also one of the few Cambridge, Massachusetts-based economists whom Friedman saw frequently after the Friedmans gave up their New England summer home in 1979–1980. In the 1980s, Bator and Friedman would continue to see each other regularly because they went to the same location, at the same time of year, for their annual skiing vacation in Alta, Utah. (Francis Bator, interview, January 6, 2015.)
[325] Duesenberry (1969, p. 87) did refer to Brunner and Meltzer, albeit without a bibliographical citation. Meltzer was Duesenberry's discussant at the conference (see Meltzer, 1969).

monetary policy stance, he explicitly favored an activist, countercyclical reaction function. And he continued to believe that monetary policy should be based on setting interest rates, even if, at a theoretical level, he continued to cast policy decisions in quantity-of-money terms.

This approach on Duesenberry's part was evident in formal policy advice he gave to the Federal Reserve during the 1970s. Although he was never a policymaker or any other kind of full-time Federal Reserve official, Duesenberry had, by the time of the 1969 Federal Reserve Bank of Boston conference, acquired a semi-permanent Federal Reserve affiliation. He had been made, beginning at the start of the year (*Record American* (Boston), January 1, 1969) a member of the board of directors of the Boston reserve bank. His membership of this board stretched through the first half of the 1970s, and one part of the job involved meeting with the Federal Reserve Bank of Boston's president, in the lead-up to the latter's attendance of the regular FOMC meetings in Washington. In these consultations, Duesenberry would discuss the appropriate setting of monetary policy. He continued to interact with successive bank presidents long beyond his period as a bank director: indeed, Duesenberry's was noted even in late 1996 as involved in discussions with the Federal Reserve Bank of Boston's president on current monetary policy (see Federal Open Market Committee, 1996, p. 10).[326]

A summary of Duesenberry's advice on monetary policy during the first half of 1975 was circulated to the whole FOMC (see Federal Reserve System Staff, 1975a, 1975b, 1975c). In these 1975 consultations, Duesenberry called for the Federal Reserve to produce higher rates of monetary growth.[327] Those prescriptions were in line with the fact, noted above, that Duesenberry was agreeable to thinking in terms of monetary policy in terms of monetary aggregates. His recommendations for monetary policy in early 1975 were, in addition, not altogether dissimilar to those Friedman was making at the time. As discussed in Chapter 5 above, Friedman was critical of the Arthur Burns-led FOMC for letting monetary growth fall to unduly low rates in the second half of 1974, and he had called for prompt actions to bring monetary growth up. Consequently, both Duesenberry and Friedman were recommending a major easing of monetary policy, registered in more rapid monetary growth.

A significant difference, however, between Duesenberry's 1975 recommendations and those

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[326] Even in a 2006 Federal Reserve Bank of Boston volume—recording a June 2004 conference at which Duesenberry was a discussant on the program—it was indicated that Duesenberry "currently serves on the Bank's academic advisory council" (Kopcke, Tootell, and Triest, 2006, p. 359).
[327] See the attributions to Duesenberry in Federal Reserve System Staff (1975a, p. 3; 1975b, p. 3; 1975c, p. 3). The quotations in the next paragraphs are from the first two of these sources.

Friedman was making was in the role assigned to interest rates in policymaking. Whereas Friedman favored dropping the federal funds rate instrument as part of a monetary targeting strategy, Duesenberry wanted targeting of the funds rate to be maintained. Indeed, in calling for a move to expansionary policy, he specified specific values of the funds rate that he wanted the policy of higher monetary growth to achieve. in his January 1975 consultation Duesenberry indicated that monetary growth should be raised from present levels but that this should be accomplished by changing policy settings promptly "to get the funds rate down to 6 percent and hold it there." This remained his advice in February, when the federal funds rate was still above 6 percent. On that occasion, Duesenberry was reported indicating that "open market rates should be [brought] down a bit more and kept there" and indicated that he would be comfortable if this policy resulted in the increase in the budget deficit being, in effect, accommodated by money creation.

The FOMC did, in fact, largely keep the federal funds rate at 6 percent or below until September 1977, late in Arthur Burns' tenure as Federal Reserve chair. By the time of the FOMC's late-1977 move to tighten its policy, the second monetary explosion of the 1970s had, as discussed in the previous chapter, been taking place over most of the preceding two-and-a-half years. Evidently, although it had likely appropriate in early 1975, the policy of moving rates into a lower range had been kept in force for too long and, ultimately, revived double-digit inflation. A policy—one more in keeping with Friedman's focus on monetary growth than with Duesenberry's recommendation of keeping the federal funds rate at 6 percent—that allowed short-term interest rates to start increasing prior to 1977 would evidently have been more consistent with avoiding an untoward revival of monetary growth and consolidating the disinflation that began in 1975.

It might be contended that, although Duesenberry's policy recommendation in the mid-1970s likely led to a less appropriate policy stance for that period than Friedman's would have, the overall *approach* to monetary policy that Duesenberry articulated had similarities to today's prevalent views about the setting of monetary policy. Indeed, although Friedman indicated that he found that the article made depressing reading, Duesenberry's 1969 piece on monetary policy targets and tactics contained points that have become prominent ones in the discussion of monetary policy conduct, including the concern that the stabilizing tendencies of monetary-growth rules would be compromised by "shifts in the money demand function" (1969, p. 83) and that the setting of rates should recognize that "the interest rate required to produce any target

GNP is constantly changing (p. 91).[328] In keeping with his 1975 policy recommendation, Duesenberry's preferred framework had elements that were, in some ways, modern: adjustable nominal interest-rate targets geared to final objectives, interest rates being kept down for stretches of time during the initial recovery from a perceived sharp downturn, and monetary growth being allowed to vary in response to money demand shocks.

Such an interpretation of Duesenberry's policy framework is, however, undercut by a major flaw in his analysis—his dismissal of the importance of a point Friedman had stressed: the distinction between real and nominal interest rates. "It is fashionable nowadays to emphasize the distinction between real and nominal interest rates. I doubt whether the concept of real interest rates has any real usefulness in short-run policymaking," Duesenberry (1969, p. 89) declared, while adding that "there is no well-defined empirical meaning to a real rate of interest." These were jarring statements—essentially implying that nominal, not real, interest rates, were what mattered for the behavior of private-sector spending—and they reflected a crucial omission in Duesenberry's analysis. They also revealed an issue on which Duesenberry's approach contrasted greatly with Friedman's way of seeing things. Overlooking, or abstracting from, the nominal-rate/real-rate distinction when specifying the IS-curve relationship was a shortcoming of traditional IS-LM analysis that Friedman highlighted. With regard to modeling the behavior of saving (and so of consumption) and investment, Friedman remarked in the early 1970s: "Omitting the real interest rate in that process is to leave out Hamlet. [A]nticipations of inflation… seem to me too important and too central to be pushed off stage…"[329]

By 1977−1978, the downplaying, of the sort in which Duesenberry engaged in 1969, of the nominal/real interest-rate distinction that Duesenberry endorsed in 1969 had become uncommon. Rather, in economic analysis, emphasis on the Fisher distinction was prevalent—thanks to the monetarism's influence, the rational expectations revolution, and the evolution of data in the United States and elsewhere. A prominent exception to this pattern, however, came in 1978 in a *Brookings Papers* article by Duesenberry's younger colleague at the Harvard University economics department, Benjamin Friedman.[330] Benjamin Friedman (1978a, pp. 600−601)

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[328] With regard to the first of these points, Duesenberry's concern that "there do appear to be significant shifts in velocity" produced by instability in money demand (1969, p. 92) appears to have been much less empirically important for the decade of the 1960s than he thought at the time.
[329] Friedman (1971g, p. 330). Also in Friedman (1971a, p. 40).
[330] Benjamin Friedman had become a member of the department of economics at Harvard University in 1972, his first position being that of assistant professor (American Economic Association, 1970, p. 152; B.M. Friedman, 1975, p. 277). By November 1976, he was an associate professor in the department (see Committee on Banking, Housing and Urban Affairs, U.S. Senate, 1976b, p. 81).

undertook policy analysis using an algebraic IS-LM setup of the kind Milton Friedman had criticized: one in which the nominal interest rate entered the IS equation in its own right (that is, without expected inflation being subtracted from it).[331]

Furthermore, in work published the same year, Benjamin Friedman (1978b) studied the relationship between short-term nominal market interest rates and inflation in his empirical work. He documented the point—one that has continued to be accepted as a key fact of the United States experience in the 1970s—that nominal rates in the 1970s had not kept up with inflation in practice in the 1970s in the United States. Specifically, he found that interest rates responded by only two-thirds of one percent to a one-percentage-point rise in expected inflation. In contrast to modern-day interpretations of this phenomenon in the 1970s data, Benjamin Friedman interpreted it primarily as a financial-market reaction, not a policy decision: "It is a truism that, for expectations of price inflation to affect interest rates, they must affect the behavior of borrowers or lenders or both."[332] That is, the incomplete adjustment of nominal interest rates to expected inflation was implied as being a pricing decision made by the private sector as a whole. Interpreted in that sort of framework, Benjamin Friedman's finding that interest rates responded only two-thirds of one percent to a rise in expected inflation might have been seen as having provided support to the doubts Duesenberry expressed about the practical importance of the concept of the real interest rate.[333]

There is, however, a different way of interpreting the non-adjustment of nominal interest rates: it reflected easy monetary policy. That is, it resulted from the holding-down of nominal interest rates by the Federal Reserve, in the face of upward pressure arising from the Fisher-effect mechanism. In terms of Benjamin Friedman's "truism," private-sector lenders were pricing the Fisher effect into nominal interest rates, but the central bank's creation of reserves (as well as its actions generating the creation of outside money) created new loanable funds that offset or even overwhelmed the Fisher-effect forces pushing up interest rates. This was Milton Friedman's

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[331] Congressional testimony that Benjamin Friedman gave in November 1976 was consistent with a position that aggregate demand depended negatively on the absolute level of nominal interest rates. In November 1976, the U.S. short-term real interest rate was around zero—both the federal funds rate and inflation being slightly below five percent. Starting from this position, Benjamin Friedman's advice was "to avoid significant increases in market interest rates... Substantial increases in market interest rates beyond current market levels would be damaging to the economy and would serve little purpose in terms of avoiding further increases in price inflation." (From B.M. Friedman's testimony of November 15, 1976, in Committee on Banking, Housing and Urban Affairs, U.S. Senate, 1976b, pp. 75, 76.)

[332] B.M. Friedman (1978b, p. 845).

[333] Even on this interpretation of Benjamin Friedman's results, however, a full vindication of Duesenberry's (1969) contention would have required a zero, not just an incomplete, response of interest rates to expected inflation.

interpretation of developments during the 1970s, and it also became the standard one.[334]

Milton Friedman was mainly referring to short-term interest rates in his accounts, while Benjamin Friedman (1978b) was studying longer-term rates. However, this difference still does not seem to validate Benjamin Friedman's implication that the onus was on market participants to make nominal rates adjust to inflation, and that the absence of such adjustment cast doubt on the presence of the Fisher effect. Instead, easy money likely kept longer-term nominal interest rates low in relation to inflation. Via the standard expectations theory of the term structure, central-bank efforts to hold down nominal interest rates at the short end of the term structure will be felt to some extent in the lack of full adjustment to inflation (or, equivalently, unusually low real interest rates) at the longer-term end too. And vitally, the conditions of excess demand prevailing over the 1970s are more consistent with easy money than tight money—with real interest rates pushing up demand, rather than with high nominal interest rates restraining it. In contrast to Duesenberry's dismissal of the real/nominal interest rate distinction, Milton Friedman's emphasis on the real rate as the driver of spending decisions appears to have been vindicated.

And Duesenberry would soon appear out of touch when he declared in his 1969 paper: "In practice, I know of no case when it can be said that an easy money policy, by itself, set off an expansion process which raised interest rates."[335] The United States' experience in the 1960s and 1970s provided many such cases. So, well before the 1960s, had the experience of many other countries.

Duesenberry and aggregate-supply relationships

Duesenberry would later categorize "inflation theory" as among his research interests.[336] His statements on this topic during the 1970s would, however, not be ones that stood up well in retrospect. Just as he was slow to grasp the importance, for the analysis of U.S. monetary developments, of the nominal-rate/real-rate distinction, Duesenberry articulated—in his writings and statements over the course of the 1970s—views about aggregate-supply relationships that the economics profession would, within a few years, deem obsolete.

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[334] See Chapters 2 and 5 above.
[335] Duesenberry (1969, p. 86).
[336] American Economic Association (1993, p. 135).

Duesenberry was at least not a proponent of hardline pure cost-push views of inflation. Nevertheless, the conception of aggregate-supply relationships that he expounded during the 1970s diverged greatly from the expectational-Phillips-curve/monetary-phenomenon view of inflation that Friedman advocated and that would ultimately prevail as the consensus view. Duesenberry started out the decade articulating permanent-tradeoff views of the Phillips curve. In a November 1970 speech in Boston, he discussed the relationship between inflation and unemployment: "Dr. Duesenberry appeared pessimistic on achieving full-employment and price stability at the same time."[337] Duesenberry, who gave 4 percent as the full-employment unemployment rate (itself a rate that would increasingly become regarded in official circles as too low an estimate), specifically gave 1.5 percent inflation as achievable with 5 to 5.5 percent unemployment and suggested that 4.5 percent unemployment rate implied a 3.5 percent inflation rate (*Boston Herald Traveler*, November 10, 1970).

Duesenberry had occasion to state his revised views of the unemployment/inflation relationship four years later, at a November 1974 conference whose proceedings were published in 1977.[338] Although he no longer expressed matters in terms of a straightforward downward-sloping Phillips curve, Duesenberry still seemed to believe that positive cost-push shocks continued to make price stability and full employment incompatible. He acknowledged that the central bank could indeed, through monetary control, secure a long-run inflation rate it desired (Duesenberry, 1977, p. 54), but "any reasonably acceptable level of employment" would require a notable degree of inflation: "There is a continuing conflict between employment objectives and price stability objectives." A solution would require addressing "all those special microeconomic problems that tend to generate inflation."[339]

Duesenberry and the Federal Reserve chairs, 1977–1978

It was from this perspective—according to which inflation largely reflected nonmonetary factors—that Duesenberry approached the setting of U.S. monetary policy in 1977 and 1978 under Federal Reserve chairs Burns and Miller. Duesenberry viewed the imperative as being to use monetary policy to reduce the unemployment rate, and he shared the administration's belief in these years that demand stimulation, not restraint, was needed. "I don't foresee that Burns

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[337] *Boston Herald Traveler*, November 10, 1970. The same news report used Duesenberry's position as chairman of the Federal Reserve Bank of Boston's board of directors to give misleadingl descriptions of him as "a top government economist" and even as "Fed Chairman."
[338] On the November 1974 date of the conference, see Krause and Salant (1977, p. vii).
[339] All the quotations are from Duesenberry (1977, p. 55).

will, in fact, drive interest rates up at a rapid rate. And in don't see the [Carter] Administration's policy as being so much more aggressive than seems reasonable to me or reasonable to Burns." (*Boston Globe*, November 25, 1977.) A month later, Burns' replacement by William Miller was announced. Duesenberry had the background of having known Miller from their period of overlap as members of the Federal Reserve Bank of Boston's board of directors.[340] At the time of Miller's nomination, Duesenberry saw easy monetary policy as being necessary—as well as likely to occur under the new chair. Miller, he predicted, would assign himself the task of "eliminating unemployment" (*New York Times*, January 3, 1978, p. 47).

In the event, as discussed in the previous chapter, unemployment did fall during Miller's tenure, but his period as Federal Reserve chair was characterized by successive tightening steps, and Miller would acknowledge that some of the generally higher U.S. unemployment rate seen in the 1970s reflected a rise in the natural rate of unemployment. The Federal Reserve under Miller's tenure would move away from views of the kind Duesenberry propounded, and toward acceptance of inflation as a monetary phenomenon and of the reality that the U.S. economy was in a situation of excess demand—not of major slack.

JAMES SCHLESINGER

Like James Duesenberry, James Schlesinger had a deep background in the Harvard economics world. In Schlesinger's case, however, this background consisted of having been a student, not a teacher. Schlesinger had acquired his bachelor's, master's, and Ph.D. degrees from the university in 1950, 1952, and 1956, respectively.[341]

During the 1950s, Schlesinger devoted considerable research effort to macroeconomic topics. One of the most prominent results of this was the publication, at roughly the same time as his receipt of a doctorate, of a *Quarterly Journal of Economics* article, "After Twenty Years: *The General Theory*."[342] The article's title and subject matter made it, in effect, a follow-up to a ten-year retrospective on Keynes (1936) penned by Gottfried Haberler, who was a member of Harvard University's economics department during Schlesinger's studies.[343] In common with both Haberler and Friedman, the writings on macroeconomics that Schlesinger produced in this

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[340] See the previous chapter and *Central Jersey Home News* (New Brunswick, New Jersey), December 30, 1977.
[341] See American Economic Association (1970, p. 386).
[342] See Schlesinger (1956).
[343] For Haberler's ten-year retrospective, see Haberler (1946, 1947). Haberler was at Harvard University from 1936 to 1971 (see American Economic Association, 1981, p. 178).

period took a position critical of Keynes' downgrading, in the *General Theory*, of the role that monetary policy could play in avoiding and ending depressions. Again like Haberler, but *unlike* Friedman, Schlesinger was a believer in cost-push forces as a major, and persistent, influence on U.S. wage and price pressures.[344] His 1956 doctoral dissertation was specifically concerned with this topic, and in a 1958 paper Schlesinger observed of developments in the United States since 1956: "The Consumer Price Index has climbed steadily upward… without a noticeable excess of demand."[345]

Through the very early 1960s, Schlesinger was evidently trying to stake out a particular position for himself in U.S. macroeconomic debate: the exponent of a middle ground between the hardline Keynesianism seen in the United States before the 1950s (which had featured of a full-fledged cost-push perspective on inflation, outside excess-demand conditions, and a downplaying of the role that monetary policy played in influencing total spending) and the position that Friedman had established by the mid-1950s (in which cost-push forces were rejected as a significant influence on inflation, and emphasis was placed on monetary policy as the key instrument available to the government for shaping aggregate demand).[346] But Schlesinger, despite a prominent academic location—he was at the University of Virginia from 1955 to 1963—had trouble distinguishing himself in the economics profession on these matters.[347] The "partial cost-push" view of inflation to which he subscribed was quite prevalent in U.S. Keynesianism in the late 1950s, as was some recognition of monetary policy's importance in the determination of aggregate demand.[348] Schlesinger consequently found it hard to make himself a central player in macroeconomic discourse in the late 1950s and early 1960s or to establish himself as Haberler's counterpart among the younger generation of economists.

Nor, despite clear attempts to do so, did James Schlesinger emerge as one of Friedman's major opposite numbers on matters concerning monetary policy, the business cycle, and inflation. His efforts in this direction including his declaring with regard to Friedman, in the course of an

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[344] For Haberler's statement of his position on inflation, see, for example, Haberler (1951).

[345] Schlesinger (1958, p. 312). Schlesinger's dissertation, completed in 1955−1956, was titled "Wage-Cost Price Relationships and Economic Progress" (see American Economic Association, 1970, p. 386, and *https://www.worldcat.org/title/wage-cost-price-relationships-and-economic-progress/oclc/76998246&referer=brief_results*).

[346] See, in addition to Schlesinger (1958), Schlesinger's (1960a) taking issue with hardline U.S. Keynesian Seymour Harris for his inconsistency and for his dismissal of a role for monetary policy in affecting inflation (p. 614), while also rejecting Friedman's advocacy of policy rules (p. 616).

[347] At the University of Virginia, Schlesinger was an assistant professor from 1955 to 1958, then an associate professor from 1958 to 1963 (see American Economic Association, 1970, p. 386).

[348] See Nelson (2020a, Chapter 10).

analysis of unions and inflation in Schlesinger (1958, p. 297), "we will concentrate on his views," and his subsequently publishing, soon after its appearance, a review of Friedman's 1960 book *A Program on Monetary Stability* (see Schlesinger, 1960b). But the journal in which Schlesinger's book review appeared (*Rivista di Diritto Finanziario e Scienza della Finance*) was an obscure one, even by the uncrowded standards of the early 1960s. Friedman was one of very few who ever cited Schlesinger's review when he fleetingly referred to it in 1961. And that 1960 article was virtually Schlesinger's final attempt to maintain a presence in the field of monetary economics.[349]

More than fifteen years later, in 1977 and 1978, the tables were turned. In terms of Schlesinger's career path, his period of closely monitoring Friedman's work had become a distant element in his rear-view mirror. He had now been himself nationally famous for several years, and he had moved to a situation of having little occasion to refer to Friedman at all. When Schlesinger (1977, p. xiii) remarked on "the stirring quotations from Milton's *Aeropagitca*," it was with reference to the poet Milton. In contrast, during these same two years of 1977−1978, Friedman was following Schlesinger's public remarks more closely than ever before.

Since the early 1960s, Schlesinger and Friedman had both moved from the research world to the public-policy world—completely so, in the case of Schlesinger. In 1977 and 1978, Schlesinger was a leading figure in U.S. national policy formation and commanded attention, and Friedman who would be in the position of repeatedly reacting to Schlesinger's activities and of discussing Schlesinger's statements, past and present.

This turnabout had its origins in Schlesinger's decision during the 1950s to split his research time between two distinct fields: macroeconomics and the economics of defense. It was in applying economic analysis to national-security matters that Schlesinger was much more successful in commanding attention than he would be in his endeavors in monetary economics. An early Schlesinger product the area of defense was his 1960 book, *The Political Economy of National Security*.[350] His decision to specialize on the economics/defense interface was reflected in his subsequent move to the Rand Corporation, in which Schlesinger served in senior positions from 1963 to 1969.[351] He left the Rand Corporation when the bridge he had built for himself

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[349] For Friedman's reference to the review and its "misconceptions," see Friedman (1961c, p. 447).
[350] See Schlesinger (1960c). The book was reviewed in the London *Financial Times* (October 24, 1960) by Friedman's former student, Samuel Brittan.
[351] See American Economic Association (1970, p. 386) and Europa Publications Limited (1986, p. 1430).

between economics and national security led to a role in government that concerned both areas. He joined the Nixon Administration as assistant director at the Office of Management and Budget from 1969 to 1971. There followed a further series of appointments under Nixon that were specifically in the security realm: head of the Atomic Energy Commission from 1971 to 1973, director of the CIA from February to May 1973, and then U.S. Secretary of Defense starting in mid-1973.[352] But Schlesinger's career in national policymaking seemed to have come to an unceremonious end when President Gerald Ford dismissed him as defense secretary in November 1975.

In late December 1976, however, President-elect Jimmy Carter announced that he planned for Schlesinger to head the making of energy policy in his administration. The New York *Daily News* applauded the announcement. In an editorial (December 24, 1976), the newspaper pointed to Schlesinger's experience in national-security and defense positions as "just what the energy post needs" and suggested that energy policy required the nation "to mount a massive effort, on the same scale and pushed with the same vigor as our post-Sputnik space program."

To Friedman, however, the notions that energy was a matter that demanded a government-orchestrated collective effort, and that concerns regarding high oil prices or limited energy supplies needed to be primarily viewed in national-security or national-planning terms, were misconceived. As he saw it, what was needed was the removal of federal government controls on the energy market, so that price signals could trigger appropriate responses of private-sector energy demand and supply. Neither high current energy prices nor concerns about future supply volumes were problems to be seen as a major government-planning tasks or ones for which skills in U.S. foreign or defense policy provided an ideal fit.

The "moral equivalent of war" and the Department of Energy

The Carter Administration, however, was going in a different direction from a market-based response—as was brought out in two televised addresses to the nation that President Carter gave on the topic of energy in February and April 1977.[353]

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[352] See Europa Publications Limited (1986, p. 1430). Friedman's former student Warren Nutter also followed an evolution from academic economics to defense roles in the Nixon Administration. Despite these parallels in their career trajectories, Nutter and Schlesinger did not get along at all (Jane Nutter, interview, April 21, 2014).

[353] The two talks (Carter, 1977b, 1977c) have sometimes been conflated. For example, Eizenstat (2018, pp. 165–166) very specifically identifies the second talk—on April 18, 1977—as delivered by Carter in a cardigan by the fireplace. This attire was actually used by Carter in his earlier (February 2, 1977) speech. The April 1977

In the February 1977 address, Carter indicated that he would be asking Congress to authorize the creation of "a new energy department," with its own representative in the president's cabinet. Carter cited the existing multiplicity of agencies having responsibility for energy policy and pointed to a single department as being able "to bring order out of chaos."[354] Friedman, who had wanted the main existing energy regulator/planner—the Federal Energy Agency—abolished, was appalled at the prospect of that agency, instead, being the chrysalis from which a new, full-scale government department emerged. In a *Newsweek* column, "A Department of Energy?," that appeared after Carter had formalized his request for the new body, Friedman declared that an energy department would "control the lifeblood of our economic system," and concluded: "Make no mistake about it. The establishment of a Department of Energy… would be a further major step toward converting our free-enterprise system into a corporate state." (*Newsweek*, May 23, 1977.)

There was, however, widespread Congressional support for a Department of Energy, and when this was created later in the year, James Schlesinger became Secretary of Energy. This would make him the only person who served in the cabinets of all the U.S. presidents of the 1970s.

Even prior to his return to cabinet, Schlesinger had been a formal member of the Carter administration, in his role as the assistant to the president on energy matters from January to August 1977 (Europa Publications, 1986, p. 1430). Carter had remarked in his February 2, 1977, speech that a comprehensive energy policy would be unveiled within a few months, and "Dr. James Schlesinger, who is assistant to me here in the White House, will be in charge of developing that new energy policy." The National Energy Plan—what Friedman would later label the "Schlesinger-Carter energy program" (*Newsweek*, October 17, 1977)—was accordingly unveiled on April 18, 1977. Over the following six months, Schlesinger, as the principal designer of the plan and head of energy policy in the administration, made a very large number of appearances at Congressional hearings and other public events in which he explained and promoted the plan.[355]

It was, however, Carter who first issued the plan publicly, in his April 1977 address to the nation. Friedman was incensed by a much-quoted passage of this speech. Applying to energy a phrase

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address was given by Carter from the Oval Office, in a business suit (see, for example, *The Decatur Daily* (Tennessee), April 19, 1977).

[354] Carter (1977b).

[355] For accounts of internal deliberations that have confirmed Schlesinger's status as the main orchestrator of the April 1977 National Energy Plan, see Biven (2002, p. 157) and Eizenstat (2018, pp. 150–164).

that was already long in use, the president likened the policy and community response envisioned in the plan to "the moral equivalent of war." Specifically, Carter stated: "This difficult effort will be the 'moral equivalent of war,' except that we will be uniting our efforts to build and not to destroy."[356]

Once stripped of its military component, the war analogy implied an imperative for an overtly collective response by the U.S. citizenry, with the aim of achieving a common purpose. In the same vein, James Schlesinger, in testifying for the plan, characterized the task as to "provide a challenge to the American people to meet a common goal."[357] Friedman regarded this whole approach as misconceived.[358] "There is fundamentally no moral problem involved whatsoever," Friedman contended (Instructional Dynamics Economics Cassette Tape 212, late April 1977). "… Moral values don't have anything to do with the fact that we are faced with scarcity, that we have the problem of how a limited supply of oil is allocated among various people." The parallel with war and the reference to morality were not appropriate to apply to this matter, which was instead best viewed as a "strictly technical economic problem" (*Newsweek*, June 13, 1977).

In light of Friedman's continuing diagnosis of the problem as economic, his advice and diagnoses during 1977 and 1978 paralleled those he had made since 1973: a decentralized market solution, guided by the price system. His associated policy prescription was, therefore, simple: "eliminate price controls on natural gas and on oil, let the market work."[359] The National Energy Plan eschewed this approach, as will now be discussed.

Natural gas crisis

"Jimmy Carter, before he was elected, said he was going to decontrol," Friedman observed in a 1979 interview that was concerned with new turmoil in supply conditions at U.S. gasoline stations (*Today* show, NBC, September 28, 1979). Carter's remarks before the 1976 election had, however, mainly limited advocacy of energy price decontrol to *natural gas* prices—indeed, Carter had explicitly endorsed continued regulation of domestic petroleum prices. In an

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[356] See Carter (1977c). Also quoted in *The Decatur Daily* (Tennessee), April 19, 1977.
[357] From Schlesinger's testimony of May 25, 1977, in Joint Economic Committee (1978b, p. 118). Schlesinger's testimony also repeated President Carter's "moral equivalent of war" formulation (p. 117). Perhaps fittingly in light of the parallels with war that it contained, this testimony was given on the day on which the film *Star Wars* premiered in Washington, D.C., and elsewhere in the United States.
[358] See also the discussion in Nelson (2020a, Chapter 9) as Friedman's warnings against trying to apply wartime notions of a common goal to economic policy in peacetime.
[359] *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978, p. 23 of transcript.

interview appearing in October 1976, Carter remarked: "There is no need to deregulate the price of old oil. The price of all domestic oil should be kept below that of OPEC oil. However, our natural gas supply is rapidly approaching critically low levels. Under the present regulated price structure, producers who attempt to exploit these deeper wells are forced to take a loss... We need to deregulate the price of gas for five years, with presently existing contracts remaining in force."[360]

In the event, a crunch on natural gas supply of the kind that Carter foreshadowed emerged promptly. It occurred just before he took office. The severe winter weather that the United States experienced in the early weeks of 1977 gave rise to what Friedman, in his postmortem on the episode, described as "widespread shortages of natural gas, leading to compulsory rationing and the closing of schools, factories and other establishments." (*Newsweek*, February 28, 1977.)

Friedman's column was titled "Gas Crisis: Weather Or Washington?"—and, as Carter had in his interview, Friedman focused on the hindrance to supply associated with natural-gas price ceilings. The winter natural gas emergency occurred against a background of a highly regulated market—one that Friedman in late 1977 characterized as featuring a "crazy-quilt" set of arrangements that "beggars description for its absurdity."[361] Although domestic production dominated U.S. gas supply—implying that the supply side of the market was dissimilar to the oil market—the natural gas market shared with oil the feature that it was subject to federal price control. In particular, natural gas produced in its state of use was priced freely, but the price of gas that was traded in interstate transactions was subject to federal price control. Friedman's column acknowledged that, irrespective of the presence of controls, the winter weather would have caused hardship and disruption. But the physical shortages of natural gas that various U.S. states experienced, he suggested, could be laid at the door of federal price controls, which largely prevented gas consumers from making purchases from out-of-state sources at a price higher than $1.42 per unit (*Newsweek*, February 28, 1977).

When Carter in 1976 endorsed deregulation of natural-gas prices, he in effect made this policy bipartisan, as President Ford had been seeking such price deregulation repeatedly since 1974.[362]

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[360] Carter (1976). See also the quotation from the 1976 Democratic party platform given in Chapter 6 above.

[361] Friedman (1977a, p. 24). Friedman had previously deployed the "crazy-quilt" label in reference to the natural-gas market's pricing system in *Newsweek*, February 28, 1977.

[362] See, for example, Ford (1976b). Paralleling their treatment of oil, the federal authorities drew an "old gas"/ "new gas" distinction. In contrast to oil, for which "new" product was subject to price controls only starting in late 1975, both old gas and new gas were subject to federal price controls by the time Ford took office, and Ford's deregulation proposals in this area (like Carter's in 1976) pertained solely to the new-gas price. Although he wanted all price

Indeed, Friedman had thought, incorrectly as it turned out, that a gas-price deregulation bill that Ford supported in early 1976 would get through Congress (Instructional Dynamics Economics Cassette Tape 185, February 1976, Part 1). And one of Carter's selling points for his own promises on energy was that he would be able to secure gas-price decontrol when Ford had not (Eizenstat, 2018, p. 144).

Once, however, the natural-gas issue moved to the forefront of U.S. policy debate in the early months of 1977, the matter of deregulation ceased to be one of bipartisan agreement, for Carter retreated from his support for deregulation. He was persuaded to do so by James Schlesinger—who had relayed, in a briefing, given on March 14, 1977, to cabinet and other high-ranking administration officials, his conclusion that "the free enterprise system would not alone solve our problem" on natural gas.[363] When, two months later, he testified publicly about the administration's National Energy Plan, Schlesinger conceded that "there is no question that the pattern of federal controls in the past has intensified the shortages and the distortions in the natural gas market."[364] However, he also affirmed that the National Energy Plan reflected the administration's judgment that it "reject[ed] the proposition that unconstrained prices in an alleged free market… is the appropriate answer to the current problem."[365]

Instead, the plan proposed to eliminate the intrastate/interstate gas-price distinction not by deregulation, but by the imposition by the federal government of a price ceiling ultimately

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controls on natural gas removed, Friedman largely accepted that the terms of the public debate centered on new gas prices, and he welcomed the (ultimately unsuccessful) efforts that Ford and members of Congress made during the mid-1970s at securing deregulation of new-gas prices (Instructional Dynamics Economics Cassette Tape 155, October 10, 1974; Instructional Dynamics Economics Cassette Tape 185, February 1976, Part 1).

[363] Eizenstat (2018, p. 152).

[364] Testimony of May 12, 1977, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1977b, p. 227).

[365] Testimony of May 12, 1977, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1977b, p. 70). As part of the argument against deregulation, Schlesinger raised the possibility that, if prices had not been controlled during the natural gas emergency earlier in the year, the price of natural gas might have spiked to around $5.50 (see Committee on Interstate and Foreign Commerce, U.S. House of Representatives, 1977b, pp. 71, 219, and his testimony of May 25, 1977, in Joint Economic Committee, U.S. Congress, 1978b, p. 9). This conjecture was likely based on a scenario in which interstate natural-gas prices were only decontrolled once the emergency began. Friedman's *Newsweek* column (February 28, 1977) on how market pricing would have better handled the winter weather was instead mostly based on comparing actual events with a situation in which all gas pricing had been decontrolled long enough for the free pricing system to have exerted an important influence on supply conditions. However, in Instructional Dynamics Economics Cassette Tape 207 (late January 1977), he contemplated a scenario in which prices were decontrolled during the emergency and suggested that the decontrolled price would not have had to rise too much above the existing (market-determined) interstate prices, as "many gas wells are located in places where there is no demand for them for intrastate use." He also pointed out that the authorities had temporarily relaxed the enforcement of price controls as one of their means of increasing gas supply during the recent emergency.

applicable to both intrastate and interstate transactions of $1.75 per unit (rising to about $2.15 in 1980).[366] When pressed on the fact that this represented a retreat from Carter's deregulation pledge, Schlesinger acknowledged that the plan indeed embedded a view that "there must be a cap" (that is, a mandatory price ceiling) but maintained that the plan could be seen "as a first step toward deregulation" because it raised the ceiling prices for natural gas and created conditions under which the gas market could operate on a national basis.[367]

But it certainly was not a free-market solution, even in the long term—Schlesinger described "market pricing of natural gas" as something that did not occur even at the end of the plan's implementation—but instead a proposal that involved a controlled-price national market.[368]

Even before the unveiling of the National Energy Plan in April confirmed the administration's rejection of gas price decontrol, signs that Carter would retreat from his pledge to deregulate the gas price had been discernible in his early weeks in his office. Friedman remarked in late January when Friedman anticipated that notwithstanding "all the fine talk about deregulation by both Mr. Ford and Mr. Carter" during 1976, it was unlikely that 1977 would see natural gas-price deregulation. Furthermore, by the time Friedman held a press conference on February 9 in the Los Angeles area, Carter had signed a bill to address the natural gas crisis via increased federal powers over the direction of the country's gas supplies (see Carter, 1977c). "Instead of energy legislation, the government should have abolished price ceilings," Friedman remarked (*The Herald* (Arlington Heights, Illinois), February 11, 1977). "There have been winters as cold as this before, but this is the first cold snap during which … the price[s] of gas and oil [are] determined, not by the free market, but by government fiat." He suggested that the right inference had not been drawn from the experience: "Amazingly, however, the result of all this is not, as one would expect, that people wake up and say, 'Boy, is this a stupid system.' Instead,

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[366] See, for example, Schlesinger's testimony of May 3, 1977, in Committee on Energy and National Resources, U.S. Senate (1977a, p. 46), of May 12, 1977, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1977b, 101, 218–219), and of May 25, 1977, in Joint Economic Committee, U.S. Congress (1978b, p. 8). As an interim measure, the plan would place a ceiling price on interstate natural gas of $1.42, below the $1.75 ceiling proposed on intrastate gas. See items numbers 42 and 43 in the official "Proposals in the National Energy Plan" (submitted by Schlesinger in his testimony of May 3, 1977, in Committee on Energy and National Resources, U.S. Senate, 1977, p. 9).

[367] The quotations are from Schlesinger's testimony of May 12, 1977, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1977b, p. 226). For Schlesinger's claims that the bill was market-friendly, in improving production incentives and ending the intrastate/interstate market distinction, see, for example, his remarks on pages 68–70 of the same hearings volume, as well as his statement that the bill would, partly through higher taxes, "have prices reflect replacement costs" (p. 217).

[368] From Schlesinger's testimony of May 12, 1977, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1977b, p. 70).

they say, 'The government should have still more power.'" (*Los Angeles Times*, February 10, 1977, Section 2, p. 12.)

In the event, the convoluted congressional processing of the natural-gas component of the National Energy Plan led to a separate, gas-focused bill that was more deregulatory in its long-run aspects than the administration had proposed. In February 1978, Friedman was pessimistic enough to declare: "I doubt very much that we will ever get complete decontrol of oil and gas prices."[369] But, by late that year, the gas bill had evolved into one that would deregulate newly discovered natural-gas prices in early 1985. This bill was passed, and President Carter signed into law as the Natural Gas Pricing Act of 1978.[370]

The date of deregulation was so far off—near the very end of President Carter's permissible time in office, if he served a full eight years—that there was considerable skepticism that the deregulation decision would stick.[371] Furthermore, the incentives the bill gave to supply in the interim were criticized, for example by Senator Russell Long, as inadequate (*Kansas City Star* (Missouri), September 7, 1978). And the new law extended the reach of price controls before removing them, as the administration's recommendation of extension of federal controls to intrastate prices was also accepted. This aspect of the legislation so incensed Kenneth Arrow—like Friedman, an advocate of energy price deregulation—that his own account of energy developments referred to the act's medium-term extension of price controls and not to its ultimate decontrol provisions.[372] Friedman was likewise unimpressed, as the natural-gas bill

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[369] *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, p. 32 of transcript.
[370] Carter's (1978b) remarks at the signing suggested that the bill reflected his 1977 plan and made no mention of the fact that in 1977 he had excluded price deregulation from his proposals. A more plain-speaking assessment was that made in retrospect by the president's domestic policy adviser: "he abandoned his campaign pledge to decontrol newly-discovered natural-gas prices, but finally [in 1978 he] embraced it" (Eizenstat, 1978, p. 211).
[371] The far-off character of the date of deregulation was noted at the bill's signing by Representative Harley Staggers, who joked that synthetic fuels may have rendered natural gas obsolete by the time deregulation became operative (see Carter, 1978c). The leadup to 1985 was also associated with calls to expedite and extend natural-gas price deregulation. For example, Charles J. DiBona (president and chief executive officer of the American Petroleum Institute) stated in an April 15, 1983, speech, "Energy: The Lessons Learned," to the Cleveland City Club Forum: "Specifically, I'd like to reverse our current policy [of] controlling through federal regulation the price of natural gas." (In part, this was clearly a reference to the control, not expiring in 1985, of the price of "old" gas.)
[372] See Arrow and Kalt (1979, p. 3). Arrow's emphasis on these intermediate-term features of the new natural-gas law was not altogether out of step with what Schlesinger himself emphasized about it in the leadup to its passage. When interviewer Jim Lehrer noted to Schlesinger, "When all this started many months ago, the original administration bill, of course, did not call for deregulation of natural gas [prices]," Schlesinger replied: "I think that this is good legislation... It has essentially the price trends that the president recommended eighteen [sic; sixteen to seventeen] months ago: [it has] a slightly broader definition, but it includes expansion of [price] controls to the intrastate market, as he requested. In 1985, controls are scheduled [in the bill] to come off. I think that our energy

crystalized, by the fact that "the so-called gas deregulation compromise" actually involved additional price regulations for the next six years. Their imposition, he suggested, "would certainly reduce the efficiency and output of that part of the gas industry now free from control, the intrastate market."[373] The 1985 decontrol of natural gas prices envisioned in the 1978 act did go ahead, however, and this outcome meant that the 1978 legislation did come to be seen as primarily ushering in deregulation.[374]

Energy plan or tax plan?

Once the natural gas emergency of early 1977 had receded, matters concerning oil pricing and U.S. oil dependence returned to the forefront of discussions of energy policy. It was a set of proposals concerning oil that formed the main elements of the National Energy Plan. The bad news for proponents, like Friedman, of a freely determined domestic oil price was that the plan eschewed this idea. The energy-industry newspaper *The Oil and Gas Journal*, (April 18, 1977, p. 1) accurately predicted, on the basis of a leaked draft of the plan: "President Carter's long-awaited energy message… proposes a permanent system of price controls on crude oil."

Friedman focused on oil policy in the *Newsweek* column he wrote after Carter's April 20 announcement of the plan. On this basis, he judged the plan "a monstrosity." Friedman also expressed disappointment with the plan's principal designer: "James Schlesinger is a highly trained and intelligent economist. He knows better." (*Newsweek*, May 2, 1977*a*, p. 21.)

The plan's orientation toward government targets regarding the private sector's usage of particular classes of energy—targets whose achievement would partly be achieved through edicts—was anathema to Friedman. The *status quo* for the United States prior to the announcement of Carter's plan, he remarked, was that "you have handcuffs on the production, distribution, and use of oil through government controls."[375] Instead of retreating from such a system in favor of a more decentralized and market-oriented solution, the plan sought to widen government direction. "The problem in energy is the extent to which the government has

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problems today are sufficiently serious that I am not going to spend a great deal of time on theological issues involving 1985." (*MacNeil/Lehrer News Hour*, PBS, August 31, 1978.)

[373] Letter (of August 22, 1978) to Senator Clifford P. Hansen (in *Congressional Record—Senate*, September 11, 1978).

[374] See "An Overview and History of Gas Deregulation" (*https://liheapch.acf.hhs.gov/dereg/gasoview.htm*). This source also notes that price deregulation in the case of "old" gas followed later, in legislation passed in 1989.

[375] *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, p. 32 of transcript.

interfered with the market," Friedman remarked in the wake of the president's April speech. "And Mr. Carter's 'solution' is to interfere still further." (*Democrat and Chronicle* (Rochester, New York), April 22, 1977.)

In testifying on the plan, James Schlesinger claimed that measures in the plan would "eliminate government regulation."[376] But this claim mainly rested on the fact that the plan envisioned conditions in which the entitlements program involving oil refiners might be phased out. Similarly, when Schlesinger stated that the plan relied on "a minimum of direct government regulation" and "attempted to avoid mandating major changes in the economy," he was apparently merely referring to the fact that compulsory measures pertaining to household behavior was largely absent from the plan.[377] The plan *did* envision increased mandatory measures being applied to firms: as John F. O'Leary, the head of the Federal Energy Administration, testified: "We have proposed measures that, if enacted, will require industry to do certain things."[378] Giving testimony to the Joint Economic Committee about a month after the plan was released, Otto Eckstein summarized his own assessment: "The president's program substantially increases the amount of regulation imposed on the economy."[379] Friedman concurred: as he put it bluntly the following September: "The Carter energy program, I think, will be another disaster."[380]

True to its self-description as a plan, the Carter program aimed to achieve numerical totals pertaining to the private sector's use of specific fuels by the mid-1980s. For example, in addition to seeking reduced oil consumption and importation, the plan contained a sub-program that Schlesinger called the "coal conversion program."[381] He characterized this measure as facilitating an "expansion of the use of coal" in the United States as part of "a switch from oil

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[376] From Schlesinger's testimony of August 8, 1977, in Committee on Finance, U.S. Senate (1977a, p. 7). The entitlements program was discussed in Chapter 6 above.
[377] The quotations are respectively from Schlesinger's testimony of May 25, 1977, in Joint Economic Committee (1978b, p. 116) and of August 8, 1977, in Committee on Finance, U.S. Senate (1977a, p. 55).
[378] From O'Leary's testimony of September 19, 1977, in Committee on Energy and Natural Resources, U.S. Senate (1977b, p. 107). Similarly, Schlesinger acknowledged: "For the industrial and utility sector, the [proposed] national energy act contains… regulatory measures." (From page 118 of his written testimony of May 25, 1977, in Joint Economic Committee, 1978b.)
[379] From Eckstein's opening statement for the hearing of May 20, 1977, in Joint Economic Committee (1978b, p. 5). This passage also appeared in the written version of his remarks (p. 13). Eckstein's stress on the plan's shortcomings in his testimony at this hearing contrasted with his initial reaction, which had been contrasted in the press with Friedman's by the headline: "Eckstein: It'll Work; Friedman: A Disaster" (*Democrat and Chronicle* (Rochester, New York), April 22, 1977).
[380] *Milton Friedman Speaks*, Episode 12, "Who Protects the Consumer?," taped September 12, 1977, p. 28 of transcript.
[381] See, for example, Schlesinger's testimony of May 25, 1977, in Joint Economic Committee (1978b, p. 131).

and gas to coal [reflecting its status] as our most abundant resource."[382] The specific target enumerated in the plan was: "Increase coal production by about two-thirds, to more than one billion tons annually"—and Secretary of the Treasury Blumenthal noted that the plan entailed that "we are really going to triple the use of coal for industrial uses."[383] Friedman lamented the fact that in the plan, public "utilities are going to be required at almost whatever cost to convert to coal, [while] factories are going to be prevented from using oil and, if not, they're going to be required to pay a very high tax."[384]

Indeed, various prominent features of the plan led Friedman to judge that it was really a tax program, rather than a program directed at the energy problem.[385] The criticism had substance in the fact that taxes figured very prominently in the program: the official list of its initiatives began with "1. Gas guzzler tax. 2. Standby gasoline tax."[386] Secretary of the Treasury Blumenthal himself said of the National Energy Plan: "It is, in many ways, a tax plan."[387] Although the administration advertised as a feature of the program a commitment that proceeds of energy taxes would be rebated to the public, Otto Eckstein judged that the proposed remittances would not achieve this aim. "In fact," Eckstein testified on May 20, 1977, "the money will not be [fully] recycled to the public. The federal government will keep quite a few billions for meeting, in fact, the general needs of the budget."[388]

Appearing as number 39 in the official list of the plan's proposals was "Crude oil equalization tax [COET]." Despite being catalogued below so many other items in the plan, the COET—also called the wellhead tax—was, in effect, as the Congressional Budget Office's Alice Rivlin would note, the "cornerstone" in the plan's aims for the pricing, consumption, and importation of oil.[389]

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[382] The two quotations are both from page 7 of Schlesinger's testimony of May 3, 1977, in Committee on Energy and Natural Resources, U.S. Senate (1977). A few weeks later, Secretary of the Treasury Blumenthal similarly stated that the conversion scheme reflected the fact that "there is a general view… that we have other sources of energy such as coal… and that we ought to emphasize those while we economize, that that is the right approach." (Testimony of May 25, 1977, in Joint Economic Committee, 1978b, p. 104.)
[383] The quotations are drawn from item number 101 in the official "Proposals in the National Energy Plan" (submitted by Schlesinger in his testimony of May 3, 1977, in Committee on Energy and National Resources, U.S. Senate, 1977, p. 10), and from Secretary Blumenthal's testimony of May 25, 1977, in Joint Economic Committee (1978b, p. 100).
[384] *Milton Friedman Speaks*, Episode 12, "Who Protects the Consumer?," taped September 12, 1977, p. 27 of transcript.
[385] *Milton Friedman Speaks*, Episode 2, "Myths That Conceal Reality," taped October 13, 1977, p. 31 of transcript.
[386] See Committee on Energy and National Resources, U.S. Senate (1977, p. 9).
[387] From Blumenthal's testimony of May 25, 1977, in Joint Economic Committee (1978b, p. 61).
[388] In Joint Economic Committee (1978b, p. 3).
[389] From Rivlin's testimony of September 16, 1977, in Committee on Energy and Natural Resources, U.S. Senate (1977b, p. 3).

The COET element of the package was a feature that added credence to Friedman's accusation that the plan was a tax-increasing package. Senator Joseph R. Biden, Jr. (D–DE) commissioned a Library of Congress analysis of the likely revenue that the COET would generate, and in mid-September 1977 he released the finding that the estimated revenue generated by the tax by 1985 was $19 to $30 billion—well above the administration's $12.2 billion figure.[390]

The COET was notable also for the fact that it reflected the administration's rejection of crude-oil price deregulation. Whereas the Nixon and Ford administrations had proposed moving from domestic oil price control to a program of decontrol accompanied by new energy taxes, the Carter Administration's National Energy Plan proposed the COET, a tax, as something to be employed in the oil market alongside permanent price control.

Perpetuating price controls

In one of his many Congressional appearances regarding the plan, Schlesinger claimed that it would institute "a rational pricing system," and in another he stated that "this program is basically the one that reflects… the use of the price mechanism."[391] Similarly, FEA Administrator O'Leary Administrator testified in June 1977 of the conditions that would flow from implementation of the plan: "The marketplace for crude oil will be returned to its pre-price-controls state…"[392] These statements gave the impression that the plan would institute decontrol of the price of crude oil produced in the United States. In fact, that move was notably absent from the National Energy Plan. Instead of decontrolling oil prices, the plan proposed to maintain, in perpetuity, price control on all the main categories of domestically produced crude oil that were currently subject to control and, within the confines of those controls, to make the path followed by the ceiling price somewhat lower—that is, more restricted—for most domestic oil. As Friedman put it in a September 1977 discussion of the plan, one of its properties was that "price controls are going to be kept."[393]

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[390] See Committee on Energy and Natural Resources, U.S. Senate (1977b, p. 82).
[391] The quotations are from testimony Schlesinger gave on May 3, 1977, and on June 1, 1977, in Committee on Energy and Natural Resources, U.S. Senate (1977a, p. 7) and Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1977b, p. 5), respectively.
[392] From O'Leary's testimony of June 23, 1977, in Committee on the Judiciary, U.S. Senate (1978, p. 26).
[393] *Milton Friedman Speaks*, Episode 12, "Who Protects the Consumer?," September 12, 1977, p. 27 of transcript. Similarly, Alice Rivlin of the Congressional Budget Office observed: "The administration's plan would retain controls on prices received by domestic oil producers." (Testimony of June 1, 1977, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives 1977a, p. 131.) See also the September 1977 Rivlin remark on price controls that is quoted below.

As far as Friedman was concerned, price controls were the "ultimate source of the whole energy problem," and Schlesinger's background in economics should have led him to an energy proposal that used the price mechanism and decontrolled energy prices.[394] Instead, the oil price controls originally imposed in 1971, and retained with modifications in the years since, would, under Schlesinger's proposals, be made permanent. What had been called old oil—that oil output generated by a U.S. well in place by 1973 that did not exceed the well's 1972 production levels—would still have its price held down to its middle-single-digit level—about $5.18 in 1977 (Sweeney, 1977, p. 185). And in the case of what had been customarily called new oil—oil generated at wells developed since 1972, or production at pre-1973 wells that exceeded 1972 levels—price control would, as had been the case since 1975 (see Chapter 6 above), also continue to be in force: the existing control system implied that the price of such new oil in 1977 was, at $11.28, about $2.25 below the world market price.[395]

Furthermore, under the plan, not only would these prices not be deregulated, but the previous legal provisions for some real increases in the controlled oil prices would be eliminated. Mid-1970s legislation had allowed the maximum prices for old oil and new oil to be raised by up to 10 percent per year. Under the National Energy Plan, this provision would be dropped, in favor of permanently restricting the increases after 1977 to the general inflation rate. As the Carter Administration in 1977 (erroneously, and in contrast to Friedman's projections) thought that the U.S. economy would experience inflation well below double digits in the coming years, this change was intended to mean, as Schlesinger confirmed in testimony, a lower ceiling over time on old-oil and new-oil prices than implied by the arrangements in place before the plan.[396] The upshot was, as Alice Rivlin of the Congressional Budget Office noted: "To oil that is already flowing, the producer's price would be frozen at the present level, except for adjustments for inflation…"[397]

Domestic oil arising from very new discoveries—defined as oil discovered after the National Energy Plan's public release in mid-April 1977—would, under the plan, be allowed to rise to the market price prevailing in 1977, and the producer would receive that price. But such product

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[394] The quotation is from *Milton Friedman Speaks*, Episode 12, "Who Protects the Consumer?," September 12, 1977, p. 26 of transcript.
[395] See the testimony of James Schlesinger of June 1, 1977, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1977b, p. 4).
[396] See Schlesinger's testimony of August 8, 1977, in Committee on Finance, U.S. Senate (1977a, p. 12).
[397] From Rivlin's testimony of September 16, 1977, in Committee on Energy and Natural Resources, U.S. Senate (1977b, p. 4).

would not be allowed to reach this price fully in 1980.[398] And, as discussed further below, this price would still a controlled price—one mimicking the 1977 world market price plus an adjustment for U.S. inflation over 1977−1980—rather than a true free-market price.

As part of what Friedman called the plan's preoccupation with "paying for energy through taxes" rather than market prices (*Newsweek*, May 2, 1977*a*, p. 20), there were also further proposed categories of oil. In giving an analysis of the plan his forecasting agency had produced, Otto Eckstein noted the complexity of the classification schemes: "The new energy program establishes six classes of oil, each with its own price control schedule: old old oil, old new oil, stripper well oil, new new oil, Alaskan oil, and foreign oil." [399] The situation was further complicated by the various names that administration officials gave when referring to "new new oil"—that is, the oil discovered in the United States from April 1977 onward that would have its price linked to the 1977 world price. In a single congressional hearing, Secretary of the Treasury Blumenthal variously called this category "truly new oil," "newly discovered domestic oil," and "the third tier."[400] Among other administration officials, William Nordhaus of the CEA opted for the term "'brand new' oil," while Schlesinger referred to the same concept as "frontier oil."[401]

Beyond the confusion over the names for different categories, there were concerns about a reliable means of distinguishing among oil types. Eckstein expressed doubt about there being "a practical means to identify and classify every barrel of oil and to track it through the productive and distributive process" and suggested that inaccuracies would emerge.[402] Another critic of the program observed (Sweeney, 1977, p. 188) that "the Carter Administration seems to be having great difficulty defining new-new oil… Even if a satisfactory definition can be found, enforcing compliance is extremely difficult." In the wake of the plan's proliferation of categories, Friedman noted caustically that it seemed to reflect a position that "if a little bureaucracy creates problems, more bureaucracy will solve them!" (*Newsweek*, May 2, 1977*a*, p. 20.)

The administration's basis for regarding itself as relying more on the price system—despite its

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[398] See, for example, Schlesinger's testimony of May 3, 1977, in Committee on Energy and National Resources, U.S. Senate (1977a, p. 52).
[399] From Eckstein's prepared testimony of May 20, 1977, in Joint Economic Committee (1978b, p. 13).
[400] See respectively, pages 81, 98, and 92 of Blumenthal's testimony of May 20, 1977, in Joint Economic Committee (1978b).
[401] See Nordhaus' testimony of June 29, 1977, in Committee on the Budget, U.S. House of Representatives (1977n, p. 3) and Schlesinger's testimony of June 1, 1977, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1977a, p. 4).
[402] From Eckstein's prepared testimony of May 20, 1977, in Joint Economic Committee (1978b, p. 13).

proposal to divide domestic oil product among a still greater number of categories than previously, regarded itself as relying more on the price system—rested on the inclusion in the plan of the COET or wellhead tax. Price control would apply to the prices producers charged, but the COET would then make the *post-tax price* equal across categories and have that price correspond to 1977 world market prices. Provided that the world price remained at its (real) 1977 level, a number of existing anomalies would be eliminated: the oil prices faced by all refiners would be uniform; prices of domestically produced oil (inclusive of COET) would be the same as the price of imported oil; and the price of gasoline in the retail market would tend no longer to be artificially low. The subsidy engendered by price control to the consumption and importation of oil would therefore be eliminated.

This streamlining would remove *some* of the distortions in the U.S. oil market, mainly those present on the demand side, and some of the existing administrative complications associated with price control. But it did *not* correspond to the elimination of price control. That the plan did not amount to decontrol—but, instead, to permanent price control—was, in effect, acknowledged at the official level by William Nordhaus when, in testifying in favor of the administration's energy program, he described it as moving from the previous "haphazard" set of pricing regulations to a more uniform arrangement: "there would be an enormous simplification of the existing price control system."[403]

The administration's assumption that there would be no second oil shock

In describing the COET and its implications for U.S. oil prices, Schlesinger remarked that the "tax is designed to raise the prices facing [every U.S.] refiner to a single uniform price equal, we trust, if world prices do not move significantly, to the world price."[404] The manner in which Schlesinger formulated this description reflected the reality that, in fact, the energy plan did *not* propose to bring the post-tax domestic oil price, unambiguously and automatically, to the world market level. Instead, it would adjust the controlled prices in a manner that lifted the post-tax price for all U.S.-produced oil to the world market price prevailing in 1977, after which the post-tax price would be kept at that level in real terms.[405]

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[403] From Nordhaus' testimony of June 29, 1977, in Committee on the Budget, U.S. House of Representatives (1977b, p. 3).
[404] From Schlesinger's testimony of May 3, 1977, in Committee on Energy and National Resources, U.S. Senate (1977a, p. 27).
[405] This was embedded in item number 40 in Schlesinger's list of the 113 proposals in the National Energy Plan, submitted as part of his testimony of May 3, 1977, in Committee on Energy and National Resources, U.S. Senate

This part of the administration's proposal was a mirror image of the fact that the design of the COET was predicated on there being no second OPEC shock. Schlesinger himself gave his grounds for believing that there would be no adverse oil shock in the coming years by suggesting that, as non-OPEC sources of oil were arriving "in the short run and up to about late 1979 or early 1980… the ability of the more hawkish members of OPEC to push up prices will be limited."[406]

This position was not out of line with Friedman's position that there would not be a second oil shock. Indeed, Friedman over this period continued his prediction that OPEC would break up or be forced to lower prices by end-decade. "Without our foolish and misdirected energy policies," Friedman remarked after the plan was released, "I believe that the OPEC cartel would by now have collapsed. And it still will[,] despite the best efforts of the U.S. government to prop it up." (*Newsweek*, May 2, 1977*a*, p. 20.) Similarly, during his June 1977 visit to Alaska, Friedman remarked that "the odds are that[,] sometime in the next five years, more or less, the OPEC cartel will break up."[407]

Both Schlesinger (believing that there would not be a second oil price hike in the next few years) and Friedman (believing that the first oil shock would be unwound in 1977–1982) were wrong. The second oil shock occurred in 1979. But, while Friedman was, in a sense, the more wrong of the two, as Schlesinger saw oil prices staying level while Friedman regarded them as likely to fall, Friedman's *policy prescription* was far more robust to the advent of a second oil shock than was Schlesinger's. Friedman's position that domestic price decontrol would eliminate the underpricing of oil in the United States was valid irrespective of what the future held in store for OPEC and its market power. In contrast, Schlesinger specifically predicated the COET proposal and the overall National Energy Plan on there being no further adverse oil shock occurring in coming years.

U.S. legislators were surprised at the lack of contingency for a future oil shock in the administration's 1977 energy proposals. Senator Biden indicated that he would be reluctant to vote for COET without knowing more about the form that energy policy would take if "the

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(1977a, p. 8): "40. Provide authority to establish a lid on both producer prices and the wellhead tax if world oil prices increase faster than inflation."

[406] From Schlesinger's testimony of May 25, 1977, in Joint Economic Committee (1978b, p. 122).

[407] Friedman (1977e, p. 8).

president decides to 'decouple' COET from keeping pace with world prices."[408] And Senator Teddy Kennedy indicated puzzlement at the administration's apparent willingness to allow such a decoupling, as it would allow domestic prices to become separated from the world market price once again after the administration had gone to the effort of introducing a tax designed to eliminate the existing separation.[409]

The administration's default position was that, if a new rise in the world oil price was large, the administration would *not* follow it by raising COET, and so the domestic post-tax price, commensurately.[410] Secretary of the Treasury Blumenthal actually made a virtue out of this property, testifying that "the [proposed] legislation permits us not to go ahead and keep increasing prices if .... we feel that we could insulate ourselves from whatever they [OPEC] might be doing."[411] Evidently, the notion—which Friedman had rejected repeatedly—that price controls provided a means by which the American economy could be insulated from OPEC oil-price moves was hard to shake out of U.S. public discourse, even when the administration was advocating the adjustment of home oil prices to the past OPEC shock.

Not only would price control in the face of a new oil shock not really insulate the country as a whole—it would, in effect, create new versions of the original price and supply distortions that the COET was designed to address. In particular, once a discrepancy between domestic and world prices reemerged, oil would resume being underpriced, and so overconsumed, in U.S. markets. Furthermore, in such an event, the entitlements system governing U.S. refineries—the system that both reflected and spread the underpricing of oil associated with price controls (see Chapter 6 above) would then need to be revived. Schlesinger had testified that under the plan "we can get rid of the existing entitlements system" that arose from the discrepancy between domestic and world market prices.[412] But both he and Blumenthal acknowledged that, in the event that a second oil shock occurred and COET (and corresponding price control) did not then

[408] See Biden's remarks (of September 1977) in Committee on Energy and Natural Resources, U.S. Senate (1977b, p. 83).
[409] See Kennedy's remarks of May 25, 1977, in Joint Economic Committee (1978b, p. 112). Kennedy—who, during the late 1970s, ran hot and cold on the general idea of deregulation as a national policy (see Boskin, 1987, p. 84)—subsequently seemed to support the administration's position that it would hold prices down if a second oil shock occurred. Kennedy stated the following July that "we must have some price controls if we are to prevent the OPEC cartel from inflicting damage [on]to our economy and to avoid windfall profits from being created by the escalation of international oil prices." (July 15, 1977, remarks, in Committee on the Judiciary 1977, U.S. Senate, 1978, p. 74.)
[410] See Schlesinger's testimony of May 25, 1977, in Joint Economic Committee (1978b, p. 120).
[411] From Blumenthal's testimony of May 20, 1977, in Joint Economic Committee (1978b, p. 92).
[412] From Schlesinger's testimony of May 3, 1977, in Committee on Energy and National Resources, U.S. Senate (1977a, p. 27).

move in step with the new world oil price, the entitlements program, after having been discontinued, would need to be brought back in.[413]

The National Energy Plan's lack of preparation for a second oil shock was also evident in its treatment of retail prices. One of the elements of the plan was: "Eliminate gasoline price controls, if feasible, with authority to reimpose controls if prices rise above a predetermined level."[414] As discussed in previous chapters, gasoline price controls, although present in the United States starting in 1971, were not a very heavily enforced aspect of petroleum price controls in the 1970s, and the underpricing of retail petroleum product really resulted from federal controls on *prices of crude oil*. Due to these crude-oil price controls, the gasoline price had far from adequately adjusted by 1977 to the first oil shock, and Friedman noted that the U.S. gasoline price in 1976 was actually 2 percent *lower* in real terms than its value two decades earlier (*Newsweek*, June 13, 1977).

If no second oil shock occurred, and the COET raised the post-tax oil price to the world level, then gasoline prices would very likely cease to be underpriced: Schlesinger estimated they would rise in such a situation by about 5 or 6 cents per gallon.[415] Together with price increases already allowed for in existing price-control legislation, this would, it was estimated, imply a total rise of 8 to 10 cents in retail gasoline prices by 1980 over the mid-1977 level—finally reflecting fully the 1973−1974 oil shock.[416] Formal removal of gasoline price controls would, against this background, be a logical corollary, but not the catalyst, of the gasoline price rise.

But the plan's ambivalence about removing gasoline prices reflected the uncertainty about how energy policy would handle a second oil shock—and, in particular, whether the U.S. policy response in that event would involve allowing (post-tax) prices of domestic oil to increase. As it

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[413] See Blumenthal's testimony of May 20, 1977, and Schlesinger's testimony of May 25, 1977, on pages 92 and 122, respectively, of Joint Economic Committee (1978b). The existence of this policy intention seemingly shaped a change in the CBO's summary of the plan. From saying, in June 1977, that the COET "would also lead to the elimination of the so-called 'entitlements program,'" Alice Rivlin of the CBO moved three months later to stating that institution of the COET would merely "probably lead" to the phasing-out of the entitlements program. (See, respectively, Rivlin's testimony of June 1, 1977, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives, 1977a, p. 131, and of September 16, 1977, in Committee on Energy and National Resources, 1977a, p. 4.)

[414] See item 56 in Schlesinger's list of the 113 proposals in the National Energy Plan, submitted as part of his testimony of May 3, 1977, in Committee on Energy and National Resources, U.S. Senate (1977a, p. 9).

[415] See Schlesinger's testimony of May 3, 1977, in Committee on Energy and National Resources, U.S. Senate (1977a, p. 40).

[416] See Alice Rivlin's testimony of June 1, 1977, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1977a, p. 131).

turned out, in the final year of the decade, 1979, the retail price went up much more by seven cents—by 23 cents, from 63 cents to 86 cents per gallon.[417] This was not because of COET (which had not been imposed) or price decontrol (which had not yet started). It was because, contrary to the Carter Administration's 1977 expectation, a second oil shock did occur within a couple of years. As had occurred in the case of the first oil shock, average petroleum and gasoline prices moved up, even in the face of domestic petroleum product being price-controlled—in large part because of the fact that the controls regime existed alongside a substantial, non-price-controlled, imported portion of petroleum consumed in the United States, with this component's behavior having a major bearing on average prices.

By the time of this new shock, the opportunity for the U.S. economy to adjust properly to the first oil shock through the option of prompt decontrol of gasoline and oil prices in 1977 had been lost—in favor of a protracted national debate about the alternatives to petroleum price decontrol that were embedded in the National Energy Plan. The *status quo*, described by Friedman (*Newsweek*, June 13, 1977) as one in which "government policies have been falsifying the true situation to consumers," could have been rapidly altered by instituting immediate price decontrol. But the Carter Administration had instead opted in 1977 to seek a multi-year, tax-based approach to the underpricing of energy.

Production incentives under the energy plan

One of the clearest deviations of the National Energy Plan from decontrol was in its proposed treatment of oil producers' revenues.

Although, under the plan, the price facing U.S. oil purchasers was envisaged as moving into line with world prices, the move higher in domestic prices would not be allowed to show up in increased after-tax revenue to those bringing the oil onto the market: the U.S. producers of oil from existing wells. Instead, the extra revenues associated with the higher purchase prices would go to the federal government, as COET receipts. There lay the most crucial flaw of the National Energy Plan, in Friedman's view: the neglect of "the supply side" of the oil market and its role played here by price.[418]

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[417] See *https://www.energy.gov/eere/vehicles/fact-915-march-7-2016-average-historical-annual-gasoline-pump-price-1929-2015*.
[418] Friedman (1977e, p. 8).

Raising prices through taxes instead of decontrol would "do nothing to stimulate supply," he remarked. "…This is a one-armed approach, with all the emphasis on [reducing petroleum] consumption and very little, except in words, on production." (*Stanford Daily* (California), April 28, 1977.) In particular, Friedman regarded the plan as not including an incentive to increase domestic oil production. By contrast, price decontrol would provide such an incentive.[419]

The notion that the Carter-Schlesinger plan addressed the problem of high world oil prices by creating incentives to reduce demand but not incentives to boost supply was also stressed by *The Oil and Gas Journal*. "President Offers Only Half an Energy Policy," the newspaper editorialized (April 25, 1977), remarking of Carter that "he offers an energy program that tackles only one side of the problem—demand… without meaningful incentives for development of more supply." In a later editorial, it illustrated the problem by invoking the president's former occupation as a peanut farmer: "Suppose July Carter the farmer were asked to design a scheme to stimulate peanut production. Would he slap a lid on prices [of] all peanuts except those producers in fields plowed for the first time after April 20, 1977…? That makes no more sense for peanuts than it does for petroleum."

By the time Secretary of the Treasury Blumenthal appeared before Congress' Joint Economic Committee on May 20, 1977, to advocate the energy plan, these editorials had appeared, and Friedman had made his similar criticisms in *Newsweek*, while other figures like Ronald Reagan and William Simon had criticized the plan publicly on parallel grounds.[420] Without naming any specific critic, Blumenthal remarked: "Those who say this program does not emphasize production are wrong in my judgment."[421] By way of refutation, Blumenthal provided a table in which the revenue accruing to a U.S. oil company from an incremental increase in oil output compared favorably with the situation facing producers in other countries (Joint Economic Committee, 1978b, pp. 97–98). However, the high margin arising from oil production that Blumenthal cited was for the extremely special case of new-new oil. Producers of this type of oil

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[419] See, for example, *Newsweek*, May 2, 1977*a*, and Friedman's follow-up columns over the rest of 1977, as well as his remarks in *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978, p. 23 of transcript.
[420] Reagan had remarked (on *Meet the Press*, NBC, May 1, 1977; quoted in Pater, 1978, p. 92): "I have felt for a long time that the government is not the answer to the energy problem. Government *is* the problem… The great problems in the energy field came about with government's involvement in the marketplace, regulation, price-fixing and so forth, and I think that today the answer lies in the marketplace." For William Simon's reaction, see *Washington Star* (Washington, D.C.), May 9, 1977. Simon later elaborated on his criticisms of the Carter energy program in *Wall Street Journal*, June 10, 1977, and in Simon (1978, pp. 91–92).
[421] From Blumenthal's testimony of May 20, 1977, in Joint Economic Committee, 1978b, p. 81):

would indeed be able to keep a very large share of their receipts, and their receipts would reflect the world price (albeit doing so only starting in 1980). The fact was that, in contrast, oil production arising from sources on-stream in 1977 would have the receipts associated with moving domestic oil prices to the world price essentially fully taxed away via the COET.

Blumenthal remarked, accurately enough: "I think frequently when people have criticized us for not enough incentives having been built in for additional production, what they mean is that we should have just decontrolled everything."[422] But, insofar as quantity supplied did depend on prices in the oil market, it *was* the case that a more comprehensive liberalization of the price system, accompanied by more widespread retention of profits by U.S. producers than the National Energy Plan, was required if U.S. oil production was to be stimulated. Friedman stressed the notion that higher prices, accruing to producers, would both lead new wells to spring up—decontrol "would give people an incentive to produce new sources of energy and of crude oil" (*Stanford Daily* (California), April 28, 1977)—and stimulate higher production from existing wells—"the incentive to produce is clearly greater if the seller gets the higher price" (*Newsweek*, May 2, 1977*a*, p. 21).

From this perspective, Schlesinger and the other makers of the energy plan had cornered themselves by offering a system in which only a sliver of domestic oil production—that associated with "new new oil"—would involve producers receiving market prices. Even Schlesinger acknowledged that that part of the market was so small that it was likely "there will be no substantial flow of oil" arising from that market segment.[423] And when Blumenthal was asked by a senator whether it was the case that most of the known unproduced U.S. oil output lay in fields whose output the Carter Administration proposed to keep at previously controlled prices, Blumenthal answered that he did not know.[424] In fact, his questioner (Senator Jacob Javits, R–NY) was correct. Correspondingly, a Congressional Budget Office study noted that the current prospect of increased oil production lay mainly in "increased development of known but costly fields and advanced recovery from older fields."[425]

Notwithstanding this acknowledgment, the CBO study came out in favor of the notion that

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[422] From Blumenthal's testimony of May 20, 1977, in Joint Economic Committee (1978b, p. 98).
[423] From Schlesinger's testimony of June 1, 1977, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1977a, p. 4).
[424] See the exchange of May 20, 1977, in Joint Economic Committee (1978b, p. 94).
[425] Congressional Budget Office (1977, p. 25).

decontrol of prices would not stimulate oil production.[426] In the Carter Administration during 1977—though not after its about-turn in energy policy in 1979—this would be the position taken, too, in defense of both the formulation of the COET and the overall energy plan. For example, in September 1977, FEA Administrator O'Leary would flout the notion of an upward-sloping supply curve by asserting in congressional testimony: "If producers were allowed higher prices for crude that they are already producing from existing wells… the result would be no increased production from those properties."[427] As part of its explanation for its goal of denying (to existing oil producers) sales revenues arising from higher prices, administration officials gave the impression that oil provided to the market by existing fields came from an existing stockpile, rather than out of a production process. For example, Blumenthal described oil output generated from existing oil fields as "oil the producer has in his inventory," and Schlesinger rationalized the COET by stating that the administration sought to preclude "substantial inventory profits on all of the existing inventories."[428]

Friedman was not only affronted by officials' disregard of the notion that oil supply was price-elastic. He was also repelled by the logic of Schlesinger's position (which echoed that of longstanding proponents of oil-price control) that it would be untoward to allow existing producers' revenues to be higher when oil prices were higher. Schlesinger suggested in testimony that giving market-determined marginal revenues to anything other than totally new sources of oil would be "providing inventory profits unrelated to economic contributions."[429] A similar sentiment appeared in the administration's original document concerning the energy program, and, in analyzing that passage of the document, Friedman seized on what he termed its "subversive" character.[430] By the same logic, Friedman suggested, homeowners should have their capital gains taxed away when they sold their houses.[431]

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[426] Again, see Congressional Budget Office (1977, p. 25).
[427] From O'Leary's testimony of September 16, 1977, in Committee on Energy and Natural Resources, U.S. Senate (1977b, p. 77).
[428] The Blumenthal statement was made in his testimony of August 9, 1977, in Committee on Finance, U.S. Senate (1977a, p. 74). The Schlesinger quotation is from his testimony of August 8, 1977, in Committee on Finance, U.S. Senate (1977a, p. 8).
[429] From Schlesinger's written statement for the hearing of May 25, 1977, in Joint Economic Committee (1978b, p. 117).
[430] See *Newsweek*, March 13, 1978, *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978 (p. 19 of transcript), and Friedman (1983d, p. 150). The passage of the plan being criticized was Executive Office of the President (1977, p. 50). Similar passages appeared in Executive Office of the President (1977, pp. XI, 30).
[431] Again, see *Newsweek*, March 13, 1978, *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978 (p. 19 of transcript), and Friedman (1983d, p. 150).

This objection to the philosophical doctrine embedded in the plan related to Friedman's supply-based economic argument against the COET. "Taxing away the fruit of past investment is hardly the way to encourage new investment," he suggested, adding that it was "simply stupid of us" to institute policies that made routine the outright seizure of revenues associated with higher prices (*Newsweek*, June 13, 1977).

Having sales revenues taxed away, replacing market mechanisms with taxes as a means of adjusting prices, and the range of quantitative regulations in the plan all amounted to a move "to socialize energy," Friedman suggested when reflecting at the end of 1977 on the administration's plan (*Newsweek*, December 19, 1977). This judgment paralleled one he had made in Alaska in the previous June, when he had remarked that the various prerogatives given to the government on the production, distribution, and importation of oil meant that "the energy policy that Mr. Carter has proposed is a policy[,] fundamentally[,] of nationalizing the energy industry."[432]

"*War profiteers*" *and economists' views on decontrol*

In the same Alaska remarks, Friedman suggested that, to his knowledge, there was essentially no disagreement among economists that price decontrol would be the best response in current circumstances.[433] This was an exaggeration—even if attention was limited to economists outside the government (and so leaving aside the leading economists in the administration like Schlesinger, Blumenthal, and Nordhaus who were making the case for the National Energy Plan's alternative to decontrol). For example, we have already seen (Chapters 3 and 6) that, before 1977, Paul Samuelson spoke favorably about the system of controls on U.S. oil prices. Samuelson also reacted favorably to the Carter energy plan.[434] However, although Friedman was incorrect in suggesting near-unanimity among economists against oil price decontrol, it was indeed the case that there was a large body of opinion among economists favoring decontrol. This was despite testimony by administration officials suggesting that decontrol advocacy was limited to very few people.[435]

An occasion arose in late 1977 to make the public more aware of the existence of a critical mass of economists in favor of decontrol. The immediate catalyst occurred when President Carter

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[432] Friedman (1977e, p. 8).
[433] Friedman (1977e, p. 8).
[434] See, for example, *Newsweek*, May 2, 1977*b*. Samuelson also declared that decontrol had "no chance of acceptance" politically (p. 20). This remark likely partly underlay Friedman's misconception that Samuelson saw decontrol as the best policy but as having to be ruled out (only) because it was not politically acceptable.
[435] See, for example, the quotation from William Nordhaus in the first section of this chapter.

remarked at a news conference on October 13 that "there is potential war profiteering" in the behavior of oil companies and stated: "This could develop, with the passing months, as the biggest ripoff in history."[436] Carter's remarks were followed within a couple of weeks by Friedman and a number of other economists putting their names to a statement by "Economists for Responsible Energy Policy" that was organized by the National Taxpayers' Union and placed as a full-page advertisement in the *Washington Post* (October 27, 1977), the *Wall Street Journal* (November 3, 1977), and some other newspapers. The economists stated: "If the market is allowed to set the price, this will bring forth additional energy supplies which will lead to lower prices in the future." They contrasted this arrangement with the Carter plan, which mostly lacked this production-stimulating aspect.

As with so many committees' statements in which Friedman participated, the finished product read much like a solo-authored Friedman article: Friedman acknowledged that the published version reflected his overhaul of an early draft (*Chicago Daily News*, December 1, 1977).[437] And it covered much the same ground as his *Newsweek* critiques of Carter's energy policy. In particular, the statement maintained: "The so-called 'energy program' would be the largest peacetime tax increase in history." The statement also criticized Carter for retaining the Nixon and Ford policy of energy price controls and suggested that "the President's intemperate language reveals the essential weakness of his case."

It is notable that among Friedman's cosignatories was Robert Lucas.[438] This example establishes that the conclusion made by a study of Lucas' public-policy activities that "Lucas's participation in the public debate was 'unintended': the public always *solicited* Lucas to express his opinion and advice, not the opposite" (Goutsmedt, Guizzo, and Sergi, 2018, p. 1, emphasis in original)—

[436] Carter (1977e).

[437] Friedman's influence on the published statement broadly conformed to his observation, made earlier in the year, with regard to jointly-written public statements: "it's the old story: the person who writes the first draft generally tends to control the direction of the later ones." (Instructional Dynamics Economics Cassette Tape 212. April 1977, Part 2.)

[438] The list of signatories included several economists affiliated with, or visiting, the Hoover Institution at the time, including Friedman, Martin Anderson, Karl Brunner, Allan Meltzer, and Robert Hall (who had recently published an analysis quantifying the distortion to demand and supply produced by the U.S. energy price controls: see Hall and Pindyck, 1977); a number of University of Chicago economists other than Lucas, including George Stigler, Yale Brozen, Gale Johnson, and Sam Peltzman; various academics who had been workshop or dissertation students of Friedman's including William Poole, Richard Selden, Thomas Sowell, and Warren Nutter; and public-choice specialists like Alman Alchian, William Allen, Gordon Tullock, and James Buchanan. The signatories also included the virulently anti-Friedman libertarian Murray Rothbard.

is factually incorrect.[439] The conclusion is directly refuted by the existence of the 1977 open letter that Lucas co-signed with Friedman. Therefore, Lucas did, in fact, offer unsolicited policy advice to the public. As will be seen, during the 1970s, he did so on more than one occasion with Friedman.

In fact, Friedman's public-policy activism was something that Lucas watched closely. It has already been discussed in the previous volume that Lucas' time as a graduate student in the early 1960s left him struck by Friedman's bringing of current public-policy issues within the scrutiny of economic analysis.[440] This left a lasting impression on Lucas, who would remark of Friedman's writings, "I'm sure I've read everything he's ever written."[441] Lucas' career trajectory also predisposed him toward being steeped in the public-policy side of academic macroeconomics. Friedman once described his first full-time academic appointment—at the University of Wisconsin in 1940–1941—as having been complicated by the fact that the university's "business school… was trying to take over the economics department."[442] This description contrasts with the situation that Lucas had in his own first full-time academic job—his position, held for more than a decade, at the Carnegie Institute of Technology (or Carnegie Mellon University, as it was renamed during Lucas' employment). At that institution, no takeover by the business school of the economics department was either necessary or feasible: the university's main economics department has always been located in its business school. Lucas was consequently a member of a business school throughout the period from 1963 to 1974— a situation that tended to bring him into forums discussing current public policy.

Beyond these university forums, Lucas made a major public-policy intervention in late 1971 by co-signing with Friedman, Allan Meltzer, Karl Brunner, and others a public statement opposing the prospect of a resumption of fixed exchange rates, after the previous August's liberalization

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[439] The study purports to provide an exhaustive account of Lucas' activities in public policy in 1968–1987, based on Lucas' personal papers. However, as is often the case with studies relying heavily on unpublished archival material, this investigation actually neglects altogether items in the *public* record (in this case, the open letters Lucas coauthored with Friedman) and so is not actually exhaustive. This public record also refutes the authors' claim (p. 3) that, with regard to ascertaining Lucas' public-policy activity, "[a]rchives are essential because… it would otherwise be impossible to detect it from other sources." Lucas' activity in public policy discernible from material in print in the 1970s, and to suggest otherwise implies insufficient familiarity with the published record.

[440] See Nelson (2020b, Chapter 11).

[441] In Snowdon, Vane, and Wynarczyk (1994, p. 221). As of June 2019, the sparse bookshelf of Lucas' office at the University of Chicago contained numerous Friedman-authored books, including *Capitalism and Freedom*, the 1975 version of *An Economist's Protest*, and *Tyranny of the Status Quo*. (Robert Lucas, personal communication, June 3, 2019.)

[442] Academy of Achievement interview, January 31, 1991.

moves (*Democrat and Chronicle* (Rochester, New York), December 9, 1971).

Lucas' public-policy-related activities in the 1970s, including several contributions to the series ostentatiously called the Carnegie-Rochester Conference on Public Policy, flowed naturally from this background. So too, did his policy-oriented research paper "Rules, Discretion, and the Role of the Economic Advisor," prepared for an October 1978 NBER conference, in which Lucas discussed the Laffer curve and tax-based incomes policy, as well as the budget-limitation movement, discussed in Section II above, in which Friedman was involved.[443]

Consumption incentives, relative prices, and the proposed rebate

The 1977 open letter on energy that Friedman, Lucas, and others co-signed also had criticism of the treatment of the demand side of the market proposed in the Carter plan. The specific criticism offered centered on the fact that, although the plan did propose to bring the petroleum prices facing U.S. purchases to current world levels, this was to be accomplished by taxes rather than decontrol. This, the letter contended, made it likely that, if the oil price came down in the future (as Friedman believed it would, even if conditions for higher U.S. supply were not improved), "the cost to the consumer" would not fall, as the COET would not be lowered appropriately (*Washington Post*, October 27, 1977). That this concern had some substance was later brought out by experience in Australia. When world oil prices did decline by a large amount in 1985−1986, the Australian government allowed only about a quarter of the decline to be felt in domestic gasoline prices, with the rest of the decline offset by an increase in excise tax (*The Age* (Melbourne), February 25, 1986).

For the immediate future, however, 1977's National Energy Plan would seek to align the prices facing consumers better with the world price. James Schlesinger had stressed the economic basis for this move when he testified that, under the plan, conditions would emerge in which "consumers recognize the true replacement cost of oil and respond by conserving."[444] For his part, Friedman had acknowledged merit in this aspect of the plan when he noted that the administration's plan "goes partway in the right direction by letting some prices rise" (*Newsweek*, May 2, 1977*a*, p. 20). Although he disliked this being accomplished via new taxes,

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[443] See Lucas 1980, pp. 204−205). An early discussion of Lucas' later (early-1980s) public-policy interventions with regard to national debate in the fiscal area was that of Blinder (1987, pp. 89, 220−221). These interventions are discussed in Chapter 11 below.

[444] From Schlesinger's written submission for his testimony of May 25, 1977, in Joint Economic Committee (1978b, p. 119).

he granted that these “taxes have the desirable objective of restricting consumption” of energy (*Stanford Daily* (California), April 28, 1977) and that the price signal was the same to consumers whether effected through taxes or decontrol (*Newsweek*, May 2, 1977*a*, pp. 20–21). Friedman summarized matters in June 1977 by noting that, insofar as the plan was aimed at “discouraging consumption” of energy through higher prices, this was “all to the good.”[445]

That higher domestic energy prices should be accompanied by tax rebates to U.S. households was something the Carter plan had in common with the Ford Administration’s unsuccessful proposals to decontrol oil prices in 1975. But it was a proposal to which Friedman was unsympathetic, and he opposed the plan’s rebate component (*Newsweek*, June 13, 1977). An objection to the rebates that he raised one occasion may seem jarring: “I have no incentive to conserve is what I spend with the left hand is going to be given back to me with the right.”[446] This objection was commonly made in congressional hearings but, in defending the COET, the Congressional Budget Office’s Alice Rivlin had a straightforward answer: “it isn’t likely that [the consumer] will spend all of his rebate on petroleum products ... [W]hat influences the relative amount of petroleum products ...that he purchases ... is presumably the price of petroleum relative to other things.”[447] In other words, the objection articulated by Friedman described what households *could* do with the rebate, not what they likely *would* do.

One economist’s retrospective on the rebate proposal, in noting that the objection just described was a widespread reaction to the proposed rebate, suggested that it reflected a “fundamental misunderstanding” of the influence of price changes on incentives.[448] Indeed, there remains considerable belief among economists in what Knittel (2014, p. 116) calls the “merits of taxing a

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[445] Friedman (1977e, p. 8). There remained some prominent figures who—in contrast to the position taken by Friedman, Paul Samuelson, and the Carter Administration—believed that energy should remain underpriced in the United States. In this connection, Senator Henry M Jackson claimed that energy demand was price-inelastic—in a May 1, 1977 appearance on CBS’ news discussion program *Face the Nation* on May 1, 1977, he declared “Tax will not deter consumption” (see Pater, 1978, p. 87)—while economics columnist Eliot Janeway suggested that raising energy prices at home would have the effect of ratifying the oil shock: “President Carter’s crazy-quilt energy proposal, aimed at cutting fuel consumption by raising the cost, has legitimized the OPEC stickup.” Friedman, of course, did not disagree that Carter’s energy plan was a patchwork and that, in its totality, it was liable to work to bolster OPEC. But the recognition of the need for the prices facing consumers to rise to world levels was an aspect of the plan that he was positive about.

[446] *Milton Friedman Speaks*, Episode 9, “The Energy Crisis: A Humane Solution,” taped February 9, 1978, p. 25 of transcript.

[447] From Rivlin’s testimony of September 16, 1977, in Committee on Energy and National Resources, U.S. Senate (1977a, p. 16).

[448] See *New York Times*, February 16, 2006. This article claimed that the rebate program in question was offered in conjunction with a gasoline-tax proposal Carter advanced in 1979. Most likely, however, the article’s author meant instead to refer to the 1977–1978 proposal of the COET and an accompanying rebate.

product to change marginal incentives while returning the revenues from the tax to consumers." Friedman was certainly aware of the price responsiveness of energy demand. Was his opposition to a rebate/energy-taxes combination therefore an instance of what Paul Samuelson (in Silk, 1976, p. 52) characterized as Friedman's willingness to tailor his responses in his popular discussions in a non-scientific manner—so that, in certain cases, Samuelson alleged, Friedman "gives you a hokum answer," rather than one based on economic analysis?

The answer is a qualified no. Friedman's objection to the rebate quoted above should be taken alongside other points he made concerning the price and income repercussions of oil-price increases. Friedman did have grounds in his own economic framework for believing that accompanying the lifting of consumer petroleum prices with a community-wide tax rebate was economically unjustified. His opposition to the rebate should be understood in that light.

To advocates of the Carter plan, a price-rise-for-rebate swap made sense because, absent the rebate, a major rise in gasoline prices would raise the overall price level and lower real aggregate demand. For example, Secretary Blumenthal asserted that there was "no reason to assume that other prices will fall just because petroleum prices rise," and, along the same lines, Alice Rivlin suggested that "the rebate will preserve [consumers'] real purchasing power." To Friedman, however, a gasoline price rise *per se* would *not* permanently raise the consumer price level. Monetary policy would largely set the level of nominal spending and, subject to the lid on nominal spending placed by monetary policy, a gasoline price rise would eventually work itself out as solely a relative-price change.[449] From that perspective, the rebate was unnecessary as a demand-preserving measure, and its proponents underestimated the role monetary policy played in determining both spending and inflation.

Indeed, as previous chapters have discussed, Friedman had a dim view in general of tax rebates as a policy measure. This was so despite the fact that his name is associated with two measures that are widely associated with the notion of checks being paid out to the population: "helicopter money" and the negative income tax. However, once Friedman's analyses of each of these two items are considered carefully, the reason for his opposition to general rebates becomes clearer.

Helicopter money has become in recent decades part of a monetary policy prescription or as a

[449] In particular, it would do so holding constant the response of potential output. Friedman did believe that the oil shock had raised the price level by lowering potential output—but that this lowering had already occurred by 1977. See Chapter 3 above.

means of describing how to provide monetary stimulus to the economy under certain conditions (see, especially, Bernanke, 2002b). Friedman's own analysis did not, however, use helicopter money in this way. His paper "The Optimum Quantity of Money" in 1969 invoked the helicopter-money thought experiment.[450] But Friedman did not advance helicopter money as a policy prescription. Rather, he used that concept—money injections associated with uniform increases in money balances across the community—to consider a monetary action in isolation, without the need to consider effects on the commercial banking system or on the distribution of money holdings across individuals.[451]

And when something akin to helicopter money was presented as a practical prescription for fiscal policy, Friedman opposed it. In a foreshadowing of his reaction to the 1977 plan, in early 1975 he objected to the idea of issuing tax rebates as energy prices rose and referred to it derisively as a case in which the government would "rain checks" on the population (*Newsweek*, January 27, 1975, p. 25). One of his criticisms was that rebates would disconnect income tax paid from work hours. As Friedman described his view when momentum for rebates as an anti-recession move grew: "I oppose the tax rebates because they are like dropping money from a helicopter and are no reward" for hours worked (*Journal of Commerce* (New York), March 4, 1975, p. 5).[452]

For similar reasons, Friedman drew a sharp distinction between his negative-income-tax proposal and a rebate issued to the whole population. He particularly disliked the idea of rebating the proceeds of a specific energy tax, as in the Carter plan. That only encouraged the idea that the

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[450] See Friedman (1969a, pp. 4–5, 9, 11–13, 15–16). Various sources have been suggested as possible inspirations for Friedman's helicopter-money scenario. James Lothian (at an American Economic Association session on January 8, 2011) speculated that Friedman may have developed his helicopter imagery on the basis of reading Irving Fisher's (1912, p. 243; 1920, p. 45) image of Santa Claus doubling individuals' average amount of money holdings. Other antecedents to the helicopter imagery have been offered by Hendry's (1985, p. 81), who pointed to Alfred Marshall's (1926, p. 45) discussion (in 1897 parliamentary testimony) of what might happen if the U.K. government distributed new pound notes *en masse* by mail, and by Clayton, Gilbert, and Sedgwick (1971, p. 6) and Gilbert (1982, p. 52), who noted that David Hume (1752b, p. 307) contemplated a scenario in which overnight "every man in Great Britain should have five pounds slipped into his pocket."

[451] Relatedly, at around the time Friedman (1969a) was being finalized, Friedman noted that one might be able to abstract from an "initial effect on the credit market" in monetary analysis if one considered "a monetary system in which the quantity of money was being increased or decreased, let us say, by government passing out handouts or imposing taxes on people and taking back the money." (Instructional Dynamics Economics Cassette Tape 10, January 1969.) See also Friedman and Schwartz (1982, pp. 483–485) and Friedman (1992a, p. 32).

[452] He voiced the same criticism of the Carter Administration's early plan, discussed in the previous chapter, to issue $50 tax rebates as an economic-stimulus measure (Instructional Dynamics Economics Cassette Tape 206, January 1977, Part 1). And, notably, Friedman also voiced sympathy with the derision that this plan was receiving as being tantamount to having checks thrown onto the population from planes (Instructional Dynamics Economics Cassette Tape 209, March 1977, Part 1 and 210, March 1977, Part 2) and himself characterized it as "a proposal to send out a rain of $50 checks to all and sundry" (Friedman, 1978b, p. 8).

rebate was being issued as specific compensation for higher energy costs and that it should be spent on energy products. Friedman reaffirmed that, instead of rebating a specific tax's proceeds, the government should instead target support to low-income individuals by bringing the negative income tax into the U.S. tax system (*Newsweek*, June 13, 1977).

Scrutinizing Schlesinger

As for the question Friedman had posed when the National Energy Plan was first announced—Why had Schlesinger advanced it instead of embracing price decontrol?—Friedman sought the answer, as he now seemed to do on so many matters, in public-choice theory.

Friedman believed that Schlesinger was reluctant to lose another major government post. Yet, as Friedman saw it, energy price deregulation would have this effect, as it would decentralize decisions concerning the distribution of energy. "To propose the right answer—eliminating government controls and letting the free market work—would render unnecessary the agency that he is destined to head!" (*Newsweek*, May 2, 1977, p. 21.)

Friedman later pursued this theme using a well-tried technique in analyzing policymakers who were former academics: contrast their current position on an issues with that expressed in their writings as a researcher.[453] Friedman was taken with an article titled "Systems Analysis and the Political Process" had published in the University of Chicago's *Journal of Law and Economics*—an article Schlesinger had written, Friedman remarked, "when he was still simply an economist."[454] Schlesinger's article had been in the field of public choice, and Friedman, repeating his criticism of the emphasis on taxes in the energy program, suggested that Schlesinger had anticipated his own career path when he had written in the 1968 article that politics was designed "to extract resources from the general taxpayer with minimum offense and to distribute the proceeds among innumerable claimants in such a way as to maximize support at the polls."[455] Schlesinger's immersion in the political world, Friedman suggested, had made him less well disposed to the market solution that he would likely have embraced as an academic economist.

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[453] This technique mirrored, for example, the approach Warren Nutter had taken in his critique of Henry Kissinger's approach to foreign policy (see Nutter, 1975).
[454] *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 9, 1978, p. 25 of transcript.
[455] See Schlesinger (1968, p. 285). Friedman's column also quoted other portions of the 1968 paper. Later, Kemp's (1979, p. 141) critique of the Carter Administration's energy policy also seized on quotations from Schlesinger (1968).

Friedman's own reliance on the theory of public choice may, however, have led him to an overly cynical interpretation, as it did with his analysis of stabilization policy. Schlesinger's academic background also included, as we have seen, strong advocacy of cost-push views of price-setting. Even before entering public service, therefore, he was more inclined than Friedman was to see a role for the state in overriding the pricing decisions of the private sector.

Legislative developments in 1978

Friedman granted, of course, that allowing the domestic oil price to be market-determined meant, under conditions prevailing in 1977, that it would be permitted to move to the price set globally by a monopolistic group, namely, OPEC (*Newsweek*, June 13, 1977). But he also believed that—contrary to what was suggested by the more hardline cost-push or administered-price perspectives on price-setting decisions—the price mechanism operated powerfully on consumers and producers alike, even when monopoly conditions prevailed on the supply side. A monopolist was sensitive to demand and supply conditions, and price signals might ultimately give rise to extra supplies that undermined the monopoly position. Friedman contended that both the strength of OPEC as of the late 1970s and the severity of the United States' energy problems since the 1973−1974 oil shock resulted from the fact that, as he put it in his February 1978 talk on the energy crisis, the price system had not been allowed to operate in the energy market in the United States.[456] If the U.S. energy market were allowed to work freely and all prices in the market decontrolled, Friedman had told an audience a few days earlier, "you will find the so-called energy problem will disappear faster than you can say 'James Schlesinger.'"[457]

By the time Friedman made these remarks, a distinct change had occurred in the fortunes of the Carter Administration's National Energy Plan. The plan had been passed with very little alteration by the U.S. House of Representatives on August 5, 1977.[458] In particular, the COET had "sailed through the House without serious challenge," the *Washington Post* observed (September 16, 1977, p. 3 of reprint). The House's passage of the administration's energy plan had come on the heels of President Carter signing into law the legislation Congress had passed

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[456] See *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978 (p. 4 of transcript) and Friedman (1983d, p. 143).
[457] *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978, p. 23 of transcript. Friedman was here offering a variation on the longstanding saying—which has been much less prevalent in the twenty-first century than it was in the twentieth—that something that happened very rapidly could be said to have occurred before (or more quickly than) "you can say 'Jack Robinson.'"
[458] See, for example, *San Antonio Express* (Texas), August 22, 1977, and the statement of William H. Dempsey in Committee on Finance, U.S. Senate (1977b, p. 1222).

creating the Department of Energy.[459] Schlesinger was therefore understandably buoyed when, in an appearance before the Senate's finance committee on August 8, he observed that essentially all of the 113 proposals embedded in the National Energy Plan had been passed by the House of Representatives.[460] A couple of weeks later, Schlesinger, newly installed as Secretary of Energy, predicted that the Senate would pass the plan by the end of the year (*San Antonio Express* (Texas), August 22, 1977).

Friedman therefore seemed to be poised to be on the losing side of the debate on the plan when he remarked on Boston radio around this time: "The Carter energy program is a monstrosity—a potentially devastating blow to the economy—and a tremendous new invasion by government bureaucracy in the marketplace." (*Boston Herald American*, August 26, 1977.)

Within weeks, however, the climate was changing. Resistance in the Senate to the COET proposal hardened, reflecting the opposition on multiple fronts: advocates of continuing low consumer prices for energy, supporters of decontrol, and those who felt that oil producers should be allowed to receive proceeds from the COET if, as a *quid pro quo*, they stepped up their investment spending (*Washington Post*, September 16, 1977). By mid-October, Friedman, who was enjoying the spectacle of Schlesinger "trying to sell a disastrous so-called energy program" (*Newsweek*, October 17, 1977), was predicting that the National Energy Plan would not secure passage through Congress.[461] And in late November, the National Taxpayers' Union, having paid for the publication of the "Economists for Responsible Energy Policy" letter by Friedman and others, indicated that it was unlikely to organize further letters, as it, too, had concluded that the legislative prospects for the Carter energy proposals were poor (*Chicago Daily News*, December 1, 1977).

At the end of August 1978, after over a year in office as energy secretary, Schlesinger was still advocating the plan, but he now seemed resigned to the fact that Congress would not approve the COET (*The MacNeil/Lehrer Report*, PBS, August 31, 1978). COET, indeed, never became law. When President Carter signed a series of energy bills into law the following November, he put a good face on what had been passed, suggesting that it reflected the "principles" embedded in his original energy plan (see Carter, 1978b). He made a more candid assessment the following May, by which time fuel-supply problems had again become acute at service stations in the United

[459] This occurred on August 4, 1977 (Carruth, 1993, p. 730).
[460] See his testimony of August 8, 1977, in Committee on Finance, U.S. Senate (1977a, p. 7).
[461] *Milton Friedman Speaks*, Episode 2, "Myths That Conceal Reality," taped October 13, 1977, p. 31 of transcript.

States: "I sent up proposals in 1977 as part of the National Energy Plan to increase production, reduce consumption and cut back on our reliance on imported oil—COET, independent users' tax, etc.—[and] none of these proposals were passed into law."[462]

Economics versus engineering and the prospect of decontrol

About six weeks after the National Energy Plan was unveiled, Friedman had remarked of the president: "His energy program, as I mentioned earlier, is not a program which is designed to give the market greater play. [Instead,] it's a program for running things through government. He is, fundamentally, as so many people have pointed out, an engineer. That's his background, that's his training, that's his disposition."[463]

Indeed, notwithstanding the heavy input that an economist (namely, James Schlesinger) had had into the formulation of the energy plan, Friedman judged that it highlighted "what you might call the difference between the economic way of thinking and the engineering way of thinking," as the plan confirmed the continuing prevalence in U.S. energy policy of an engineering-oriented mindset.[464] The engineering mindset, as he saw it, treated scarcity of resources as a justification for government planning of the country's energy arrangements—whereas economic thinking instead saw the price system as conducive to handling a situation of scarcity.

The engineering aspect of the plan was evident in Schlesinger's frequent citation of projections of declines in world oil production. For example, Schlesinger stated in May 1977 that world oil production, at that point amounting to about 60 million barrels per day, "will probably never exceed 75 to 80 million barrels a day."[465] This prediction was not contradicted by the data for

[462] Carter (1979a, p. 184).
[463] *The Open Mind*, PBS, May 31, 1977, p. 14 of transcript.
[464] See *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978 (p. 4 of transcript), and Friedman (1983d, p. 143).
[465] From Schlesinger's testimony of May 25, 1977, in Joint Economic Committee (1978b, p. 115). Similarly, about fifteen months later, Schlesinger reaffirmed that, "inevitably, we are going to lose the capacity, worldwide, to increase the production of oil… In the future, we will be able to increase production by only one or two percent a year… [O]ur projection of oil supply by 1985 is now smaller than it was a year ago." (*MacNeil/Lehrer News Hour*, PBS, August 31, 1978.) The year 1985—which the Carter Administration and other government sources tended to invoke as a crunch year for U.S. energy supply if the country's energy arrangements did not become more centrally organized (see, for example, Eizenstat, 2018, pp. 146–147)—was a date for which Friedman offered his own half-jocular forecast. In February 1978, upon noting that many observers were predicting an oil emergency in 1985, Friedman suggested that 1985 would feature an oil glut. (*Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, p. 15 of transcript.) This particular Friedman prediction concerning the fortunes of the world oil market was one that would be borne out.

many years.[466] But technological advances and oil discoveries eventually led it to be sharply refuted. Indeed, during 2019, world oil production actually exceeded the 100 million barrels-per-day mark.[467] Another widespread prediction in this period was that the United States' oil production and its contribution to global oil production was bound to continue to decline. At the official level, Schlesinger stated: "We face the fact that domestically we are running out."[468] Correspondingly, a Stanford Research Institute study produced in March 1977 saw a future in which "the United States becomes a much less significant factor in world supplies" (Henry, 1977, p. 56) and projected the U.S. contribution to world petroleum output would fall from 23.7 percent in 1970 to 10.9 percent in 2000 (p. 68). In the event, in 2019 the U.S. share of world oil production was back up to 20 percent.[469]

Although he ventured to state that the energy crisis had, at heart, "nothing to do with the world running out of oil," Friedman did not offer a detailed challenge to these projections.[470] Nor did he object to such projections being made. He did, however, stress their fragility—and that the allocation of energy in the United States should not be guided by such predictions, but instead by a decentralized and freely operating price system. The latter system, he emphasized, was resilient to long-range predictions regarding energy supplies being incorrect or subject to major revisions, whereas energy policies based on centralized plans lacked this robustness property.[471]

Friedman indicated in early 1978 that, notwithstanding what he saw as the clear merits of a market-based solution to the energy problem, he was "very pessimistic" about there being

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[466] See the charts available at *https://www.iea.org/data-and-statistics/charts/world-oil-production-by-region-1971-2019* and *http://stats.areppim.com/stats/stats_oilprod_1960x09.htm*.
[467] See "Energy Slideshow," Federal Reserve Bank of Dallas, April 5, 2021, at *https://www.dallasfed.org/research/energy*, as well as "Global Liquid Fuels," U.S. Energy Information Administration, April 6, 2021, at *https://www.eia.gov/outlooks/steo/report/global_oil.php*. In contrast, Schlesinger had projected world oil production peaking in the 1990s, followed by "a long side… dwindling away through the twenty-first century." (From his testimony of May 3, 1977, in Committee on Energy and Natural Resources, U.S. Senate, 1977a, p. 6. Similarly, in testimony given on June 1, 1977, Schlesinger testified that world oil production would peak in ten to fifteen years' time: see Committee on Interstate and Foreign Commerce, U.S. House of Representatives, 1977a, p. 4.)
[468] From his testimony of June 1, 1977, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1977a, p. 4).
[469] See the U.S. Energy Information Administration's table, "The 10 Largest Oil Producers and Share of Total World Oil Production in 2020," April 1, 2021, at *https://www.eia.gov/tools/faqs/faq.php?id=709&t=6*.
[470] The quotation is from *Milton Friedman Speaks*, Episode 12, "Who Protects the Consumer?," taped September 12, 1977, p. 26 of transcript.
[471] See *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978 (pp. 3–4 of transcript) and Friedman (1983d, pp. 142–143).

"ultimate effective decontrol" of U.S. petroleum prices.[472] Developments over the remainder of 1978 provided little basis for believing that this pessimism was unfounded. True, the non-passage of much of the Carter-Schlesinger plan had prevented oil price control from officially being made permanent. But the flipside of this development was the preservation of the *status quo*: existing price controls on U.S. oil product remained in place. Five years after the 1973–1974 oil shock, the United States still had energy arrangements that priced domestic oil below world levels. And it was against this backdrop that, at the dawn of 1979, a new oil shock was materializing.

[472] *Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, p. 33 of transcript.

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 10: The Tidal Year—1979

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July 30, 2023

I. EVENTS AND ACTIVITIES IN 1979

Milton Friedman surely distinctly remembered retiring. In September 1979, it was approaching three years since he and Rose Friedman had left the city of Chicago for northern California. At the time of that move, he had certainly viewed it as amounting to retirement. To be sure, Friedman envisioned continuing some writing and speaking activities in his new location at the Hoover Institution, and, consistent with this, Gloria Valentine herself had relocated to the Bay Area at the same time as the Friedmans' move, in order to continue as Friedman's secretary. But as Valentine recalled: "He did say he was retired when he first came to Hoover."[2] In line with this sensibility, during a talk in February 1977—about six weeks after his move to California, and several months before his University of Chicago affiliation formally shifted to emeritus status—Friedman had described himself as being "on the verge of retirement."[3]

That Friedman initially had this attitude likely reflected the perspective he took toward the momentous decision to leave the University of Chicago. In formal terms, he was simply assuming, starting in the fall semester of 1977, a new full-time job that itself entailed a fresh academic affiliation. But the indications are that Friedman saw this switch as portending a material step-down in his own level of public activity. Fifteen years earlier, Friedrich Hayek, too, had acquired a new university position on departing from the University of Chicago—and this fact would not prevent Friedman from characterizing Hayek as having "essentially retired" upon leaving the University of Chicago.[4] Friedman evidently envisioned his own move to California along the same lines. So, with teaching now behind him, Friedman in 1977 clearly

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[1] Email: Edward.Nelson@frb.gov. The views expressed in this paper are those of the author alone and do not necessarily reflect the views of the Board of Governors of the Federal Reserve System or its staff. The author regrets to note that, since the research underlying this chapter was begun, seven of the individuals whose interviews with the author are quoted below—Robert Chitester, Lyle Gramley, Sir Antony Jay, Henry Manne, Charles Schultze, Peter Sinclair, and Paul Volcker—have passed away.
[2] Gloria Valentine, personal communication, March 22, 2009.
[3] See Friedman (1978b, p. 6), which relayed remarks Friedman made at Pepperdine University on February 9, 1977.
[4] CSPAN, November 20, 1994, p. 3 of transcript.

regarded himself as in the process of transitioning to what in a few years would be a *bona fide* retirement.

But instead, nearly three years later, and with the end of the decade a few months away, here was Friedman, immersed in activities that hardly corresponded to those associated with retirement. On this September 1979 occasion, he was before five television cameras and confronted by a panel of interlocutors.[5] One of these panelists—former London *Times* economics editor Peter Jay—opened his remarks by observing: "I'm a great admirer of Professor Friedman: I've studied him, I've listened to him, I've debated with him; and always before, I've found him at least clear, even when he's been wrong. Today, I found him grossly confused."[6] A heated exchange, taking place before a small audience and the video cameras, ensued. And this debate was being conducted at the institution that Friedman had formally left: the University of Chicago.[7]

The occasion was the taping of one of the debate portions for the U.S. version of the program *Free To Choose*. The on-screen title of this series was *Free To Choose—A Personal Statement By Milton Friedman*. Friedman's early years of nominal retirement had seen him become the star of his own television show.

Although the broadcast of the program would not occur until 1980, the episodes proper (both the Friedman-hosted documentary segments and the debates that followed them in the U.S. version of the series) were made in 1979. Likewise, the book that Milton and Rose Friedman wrote to tie in with the series was completed in 1979.

The "turning of the tide" during 1979

The Friedmans titled the final chapter of their book "The Tide Is Turning." This was a reference to the momentum that was building in public discourse, in the United States and elsewhere, in favor of free-market economics, along with the parallel convergence toward the Friedman position on the problem of how to cure inflation—a problem that was itself the subject of a chapter in the book.

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[5] On the five-camera setup for the taping of these exchanges, see *Chicago Sun-Times*, July 9, 1980.
[6] *Free To Choose*, PBS, Episode 5, "Created Equal," February 15, 1980, studio debate portion, p. 6 of transcript.
[7] Specifically, the taping occurred in the business school's Harper Library. See *Chicago Sun-Times*, July 9, 1980, and Friedman and Friedman (1998, pp. 492, 499). See also the next chapter.

Friedman had thought he perceived the trends of mainstream thought in the United States moving strongly his way at previous points in postwar history, both with regard to public opinion—most notably at the time of the writing of *Capitalism and Freedom*—and with regard to policymaking—most particularly during the first two years of the Nixon Administration. These previous occasions had been followed by snap-backs and by a renewed embrace of the economic approaches that he had critiqued. But the change in opinion in 1979 that the Friedmans were highlighting proved to be far less ephemeral than the earlier shifts. The year 1979 did indeed prove to be a tidal year regarding Friedman's wider influence. As well as being, as discussed below, a time in which the material change in opinion Friedman had generated in macroeconomic consensus became consolidated, 1979 saw major steps on the national and international stage that increased the influence of his views on economic policy. The key events were the election of the Thatcher Government in the United Kingdom in May, and, at home, the introduction during the year of strategic and tactical changes to U.S. monetary policy in directions Friedman had advocated, and Ronald Reagan once again becoming, in November 1979, an official aspirant for the U.S. presidency. Together, these developments set the stage for the situation of the early 1980s—a period when Friedman's economics was especially influential on policy formulation—as well as for the era beyond that, in which he continued to leave a major imprint on many nations' economic strategies.

It was also the case that 1979 was a tidal year for Friedman outside the area of actual policymaking. His own professional activity shifted sharply and in a manner that would permanently increase his public profile.

Not a research year

Although the influence that he had established up to 1979 in large part reflected his research output, Friedman's own shift in activity during that year did not reflect new production on the research front. On the contrary, more than any previous year, 1979 was marked by Friedman's putting distance between himself and the research world.

One of the signs of this was the near-total lack of participation on Friedman's part during the year in macroeconomic research conferences. Certainly, even in the 1960s, there were important

conferences at which Friedman's work was heavily discussed but that he did not himself attend.[8] He was, nevertheless, highly prevalent on the conference circuit in that decade. By 1979, however, Friedman's attendance of conferences concerned with his own research area had become rare. For example, in the third week of January 1979, the American Bankers Association (ABA) held a mass-attendee "Conference on Reserve Requirements and the Role of the Federal Reserve System."[9] The lunch speaker at the conference was Governor Charles Partee, who, being a Federal Reserve Board member and both a maker and key spokesperson regarding current monetary policy, was the sort of figure whom Friedman had been pitted against at multiple events in the 1960s. But Friedman was not at the ABA conference—an absence made more poignant by the fact that those who did attend included, from the succeeding generation of monetarists, Jerry Jordan, and from the generation of monetarists preceding Friedman, Clark Warburton. This proved to be one of Warburton's very last participations in monetary discussions before his death in September 1979.[10]

The month of September 1979 also saw a symposium on current monetary policy issues held at New York University's Salomon Brothers Center for the Study of Financial Institutions. Again, Federal Reserve policymaker Charles Partee was a key speaker at this event. Back in 1968, Friedman had been a major speaker at a Salomon Brothers forum at New York University on monetary and fiscal policy. But he was absent from the September 1979 event—at which Partee's monetarist sparring partner was, instead, Allan Meltzer.[11]

Virtually Friedman's only participation in a monetary policy conference during the year—and then not as a paper presenter or as a contributor in any other way to the resulting printed

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[8] Examples included the American Bankers Association conference in September 1965 on monetary policy, the 1966 UCLA conference on monetary indicators, and the Federal Reserve Bank of Boston's first conference (in 1969) on controlling monetary aggregates.

[9] The conference was held on January 18–19, 1979. See American Bankers Association (1979).

[10] Warburton died on September 18, 1979, at age 83, having been hospitalized upon having a heart attack on August 25 (*Washington Star* (Washington, D.C.), September 21, 1979). Prior to this, two papers paying tribute to his career had recently been written, and both were published in the year of Warburton's death: Bordo and Schwartz (1979) and Cargill (1979). Cargill presented his paper at a Federal Reserve Bank of San Francisco seminar in early 1977, with Friedman as his discussant. Cargill observed: "Milton Friedman had great respect for Clark Warburton… And I think Friedman was a deep thinker, and he saw, you know, all of this as part of a continuum in terms of trying to understand how monetary policy works." With regard to discussions in which he had himself engaged with Warburton, Cargill remarked: "I think Clark Warburton was disappointed, [as] I think he felt that history had left him behind. [But] he never once to me said anything negative about Friedman, Schwartz, or the work they were doing. Nothing other than, 'Well, you know, I kind of did the same [thing],' or 'I said the same things twenty years [earlier].' That's it… No evidence of animosity or sour grapes." (Thomas Cargill, interview, April 17, 2015.)

[11] The event took place on September 27, 1979. See Hamburger (1980).

volume—was at a Federal Reserve Bank of San Francisco proceeding in October 1979.[12] His attendance of this conference sent a signal—for which the subsequent quarter-century would offer only a handful of exceptions—that he was henceforth only willing to attend a U.S. monetary policy conference if it was held practically next door to his home or office. The convenience of proximity also was evident in Friedman's participation at a conference, held on December 13−14, 1979, concerned with matters entirely outside the monetary sphere. This was an event at the Hoover Institution devoted to the subjects of conscription and of proposals to restore registration for the military draft in the United States. Friedman not only attended this workshop—which was more public-policy than research-oriented in content and, despite heavyweight participants like himself and Albert Rees, was not limited to economics)—he was also a speaker on the program. Thanks to this and his other interventions during the event, he featured heavily in a weighty (424 pages) record of the conference that appeared in 1982.[13]

During 1979, Friedman did give a research talk in the United Kingdom on monetary matters. The occasion was on April 9, when, at the Royal Society in London, he discussed historical interrelations between the U.S. and U.K. economies.[14] But even this talk—the first annual Harry Johnson Memorial Lecture—occurred during a trip to the United Kingdom whose itinerary reflected Friedman's change in focus. The Johnson Memorial Lecture was a rare academic engagement in a multi-week visit to London spent primarily on other pursuits—with Friedman continuing to work intensively on the *Free To Choose* series, while also finding time to give a talk to a financial institution and to have meetings with U.K. politicians.

By the late 1970s, he had abandoned efforts to keep a presence in the U.K. conference scene. Friedman had not, even in his research heyday, ever tried to emulate the Harry Johnson practice of being a fixture of U.K. and U.S. academic conferences alike.[15] But he had once made some effort to maintain a profile in dialogues that were specifically among U.K. researchers and were about research rather than current policy. Reflecting this practice, he had, in 1968, attended a U.K. conference that formed the basis for an early issue in an early issue of the *Journal of Money, Credit and Banking*. And later, during his September 1970 U.K. visit, he participated in

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[12] The acknowledgments of Paul Evans' paper (1980, p. 94) thanked Friedman for feedback—apparently relayed at this October 1979 conference. See also Friedman's (1984a, p. 58) citation of the associated published conference volume.
[13] See Martin Anderson (1982).
[14] See Friedman (1980b, p. 511).
[15] Through this practice, as Peter Sinclair put it, Johnson "was the David Frost" of economics, with each regularly hosting "shows on both sides of the Atlantic" in the early 1970s in their respective spheres of economic-research conferences and television (Peter Sinclair, interview, November 13, 2014).

both a Sheffield University conference on monetary economics and the Econometric Society meetings that were being held at Cambridge University. In contrast, from May to September 1979, the United Kingdom was the venue of a string of conferences covering topics and literatures in which Friedman's name featured heavily: a conference on May 9–10 at the City University (London) on monetary targets; a Conference on Practical Monetarism organized by the Money Study Group and held at Oxford University (September 26–28, 1979); and a Social Science Research Conference on Rational Expectations in Brighton (also held in September 1979).[16] Friedman did not attend any of these events.[17]

The year 1979 was also not one that could be judged to have seen major activity in terms of the appearance of new Friedman publications. During the year, he continued to produce a stream of *Newsweek* columns as well as various op-eds in other outlets, mainly in the *San Francisco Chronicle*. But 1979 saw no new article on his part in any research journals: there would be no item by him of this kind that could be cited in future years as "Friedman (1979)."[18] His failure to publish a journal article in 1979 made 1978–1979 the first back-to-back years of no Friedman research journal publications since 1933–1934, the period before he began publishing. And on the rare prior occasions when he had not published a research journal article in any individual year, he had published an article in a public-policy journal or a book chapter during the year. But, other than a debate with Ralph Nader that was put out in a little-seen volume recording a corporate body's conference proceeding, 1979 saw the appearance of no such articles.[19]

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[16] The dates of the conferences appear, respectively, in the dust jacket text of Griffiths and Wood (1981), in the covering material of Jonson (1979), and in Attfield, Demery, and Duck (1981, p. 349).

[17] Friedman continued to be a regular attendee of the free-market Mont Pelerin conferences held around the world. He was not involved in travel connected with these events during 1979. But Mont Pelerin conferences—which were largely outside the world of economic research—would occasion continued international travel on his part during the 1980s.

[18] A couple of "phantom" citations of an alleged Friedman 1979 research article should be noted. As discussed in Chapter 4, the journal contribution by Judge Milton Friedman (1979) has sometimes been incorrectly included in Friedman bibliographies. And although F.L. Rivera-Batiz and L. Rivera-Batiz (1985, pp. 332–333) cited a Friedman article as published in January 1979, the item in question, although based on remarks given in April 1979, actually saw print in a journal issue dated January 1980 (see Friedman, 1980b).

[19] The item referred to is Proprietary Association (1979).

Friedman's (1968a) article "The Role of Monetary Policy" was reprinted in a 1979 readings collection (Korliras and Thorn, 1979, pp. 91–102), and it was cited in that reprinted form by Thomas Wilson (1984, pp. 26, 39) and by Gavrilenkov (1995)—who actually referenced it as "Friedman, Milton (1979)" (p. 188). This form of citation was not appropriate, however, as the 1979 version was a pure reprint, containing no material not in the 1968 original. By 1979, "The Role of Monetary Policy" had actually been reprinted many times—including in the late 1960s in Joint Economic Committee (1968, pp. 206–215) and in Friedman's (1969c) book *The Optimum Quantity of Money and Other Essays* and in early-1970s readings collections like Smith and Teigen (1970, pp. 476–488), Shapiro (1970, pp. 408–424), and Gibson and Kaufman (1971, pp. 169–179).

Another 1979 reprint of a Friedman work was to be found in an unlikely source. Speaking to a hearing of the House banking committee that he chaired, Representative Henry Reuss complained that the Federal Reserve Bank of

And until the *Free To Choose* book—which would come to be treated as basically a 1980 publication—crept into some U.S. bookstores in the very last days of 1979, the year saw the appearance of no new book of which Friedman was an author.[20] One long-promised book, in particular, did not see release. During his April 1979 Harry Johnson Lecture, Friedman referred to the "study that Anna Schwartz and I have been doing for the National Bureau of Economic Research" and indicated that he would draw results from that study.[21] But, as he implied when he went on to note that this Friedman-Schwartz study was a sequel to books that had appeared in 1963 and 1970, the new volume—*Monetary Trends*—had been under preparation for many years. And there was more work to go. The decade of the 1970s would elapse without *Monetary Trends* appearing, and further rewriting of the *Trends* manuscript lay ahead in 1980 and 1981.

The closer that book got to finalization, the more complete became the transition in Friedman's research status—from a producer of new studies and participant in research gatherings, to someone talked about by active researchers but himself mostly following other pursuits. That transition had proceeded over the 1970s, and it was at an advance stage in the closing days of 1978 when, at that year's American Economic Association meetings, Bennett McCallum observed that eleven years earlier, "Milton Friedman's suggestion that unemployment could be kept low only by accelerating inflation seemed radical; now even many activists doubt that it can be kept low by *any* monetary policy stance."[22]

As will be seen below, even during 1979 Friedman felt strongly about this issue that he used the occasion of discussions of inflation/unemployment relationship to stress that, unlike some of the research "activists" (as McCallum called them), he did continue to believe that monetary policy was highly nonneutral in the short run and that a successful U.S. disinflation would require a

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New York's (1979) publication *Federal Reserve Readings on Inflation* had drawn exclusively from Federal Reserve System publications (remarks of May 15, 1980, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1980b, p. 15). Reuss' criticism was not really well taken, however. The compilation did give voice to figures who were essentially outside the Federal Reserve System. Specifically, it reprinted (see Federal Reserve Bank of New York, 1979, pp. 89–106) the debate in the *Federal Reserve Bank of San Francisco Economic Review* of Friedman and Modigliani (1977), including the unfavorable remarks Friedman made in that debate on the Federal Reserve's record.

[20] The year did see the appearance for the first time of an Italian translation of the Friedman-Schwartz *Monetary History* and a German translation of *An Economist's Protest*. See Valentine (1987, p. 528).

A couple of potential misattributions of 1979 books by Friedman should also be mentioned. First, Milton R. Friedman issued *Friedman on Leases: 1979 Cumulative Supplement* (M.R. Friedman, 1979). Second, author Milton M. Friedman produced *An Adult Guide to Beginning Piano and Basic Musicianship* (M.M. Friedman, 1979). An additional factor liable to confuse the latter book with the output of economist Milton Friedman was the book was released by Prentice Hall, a past publisher of his own work.

[21] Friedman (1980b, p. 499).

[22] McCallum (1979b, p. 244).

protracted period of above-normal unemployment. But, in other respects, the manner in which McCallum placed Friedman's work in context amounted to something Friedman could agree with it. McCallum's summary conveyed the fact that Friedman's position on monetary matters, although once controversial and heavily disputed, had over the prior decade increasingly become the consensus view, as well as a jumping-off point for new research. This pattern was evident in the year in which McCallum's article appeared in print, 1979: Notwithstanding the dearth of new Friedman research publications, his impact continued to be felt heavily in the U.S. economics profession, including in the research world.

One of the manifestations of Friedman's longer-term influence came in the messages sent in 1979 by figures who were veterans, on the Keynesian side, of the older debates with him. For example, Paul Samuelson declared in August 1979 that there was only one type of Keynesian left—"quasi-Keynesians" (*Financial Times* (London), August 6, 1979). Earlier that same summer, another longtime Keynesian bastion, Harvard University, acknowledged Friedman's contributions by having him on the program of a commencement ceremony, at which Friedman was furnished with an honorary doctorate (*Boston Globe*, June 8, 1979). And, in a review of the state of macroeconomics as of the late 1970s, Michael DePrano—himself one of the Keynesian researchers who had had a printed exchange with Friedman during the debates of the 1960s (see DePrano and Mayer, 1965)—assigned Friedman a prominent place. In reviewing the previous decade's developments, DePrano made a point of highlighting the comparative lack of success and acceptance of Friedman's early-1970s research output and the fact that it had taken subsequent research by others to formalize some of Friedman's major contributions. But DePrano also acknowledged that Friedman's 1960s contributions had provided for the foundations for much of the agenda of macroeconomics in the 1970s.[23]

Developments in the policy world

This consolidation of Friedman's influence on the economics profession coincided with a time in which his influence on U.S. economic policy was nearing its maximum. Just as Friedman's influence on research in 1979 occurred despite the fact that he was not generating much new research output, his step-up in influence on policy in 1979 owed little to his direct participation in policy forums during that year.

[23] See DePrano (1979, pp. 21–23).

Some such participation did occur in the course of the year. Friedman's continuing activism in the area of constitutional limitations did produce the occasion for what turned out to be his final in-person testimony at a Congressional hearing.[24] But he had done more of this kind of activity in past years: in particular, 1959 and 1969 had seen him go to Washington twice to make two separate appearances at Congressional hearings. In contrast, although he did give one piece of spoken testimony on fiscal policy, he did not take the opportunity to testify in one of the myriad Congressional hearings held during 1979 on monetary policy and energy policy. And although, as discussed at the end of this chapter, Friedman was an insider in the Reagan presidential campaign, his participation as an adviser to Reagan in 1979 (and continuing into 1980) was far less extensive than of numerous others. He would not be as central to the Reagan campaign as he had been in the 1964 Barry Goldwater campaign or even the 1968 Richard Nixon campaign, and his input was limited in part by the fact that Reagan had a crowded field of economic advisers (consisting, other than Friedman, of veterans of the 1969–1977 Republican national administrations, confidantes Reagan had acquired over the years in California, and members of the supply-side movement). As indicated earlier, Friedman's 1979 influence on the shaping of national economic policy instead rested mainly in the influence of his prior body of published writings.

A busy celebrity

What mainly occupied Friedman in 1979 mainly occupied with making the next step in his evolution as a celebrity. Michael Canes, who, as a graduate student, had known Friedman at both the University of Chicago and Stanford University in the 1960s, and had stayed in touch with him, particularly on matters concerning energy policy, noticed Friedman's shift in priorities when, in summer 1979, he visited the Friedmans' East Coast residence of "Capitaf." "I took my family up to Vermont on vacation, and he had a home up there—a summer home in Vermont... And I thought, 'Well, this will be kind of nice. He's away, you know, out in the woods somewhere, and have a chance to chat for a bit, get to know him just a little better. And for my wife and the children, it could be kind of fun for them to meet this guy.'… And so we got in touch with him, and he invited me, and so we go up there. And, yeah, there he was—he and Rose, up there this place—idyllic-type place, and all of that. But it wasn't like he was isolated *at all*. When I got there, within 10 minutes or 15 minutes, a German camera crew was coming in there. And they were filming him for some show or special or whatever it was they were doing.

[24] See Section III below.

And he had other things going, too. So, while, you know, he was very pleasant and welcoming and all that stuff, he was in and out within 10 minutes. And Rose spent some time with us, and she was very gracious—you know, a very lovely person. But it didn't work out the way I thought it would, at all. He just was a very, very busy guy." (Michael Canes, interview, November 7, 2013.)

The main item among the "other things going" that this "very, very busy guy" had during 1979 was the making of the series *Free To Choose*—a process that overlapped, in its later stages, with the Friedmans' hectic writing of the book version of *Free To Choose*. It was in October 1969 that Senator William Proxmire had remarked to Friedman: "Some people say you have brought Adam Smith into the 20th century."[25] But it was a decade later that free-market activism actually occupied most of Friedman's time. The making of the *Free To Choose* series, which had begun in 1978 and had involved much location shooting, likewise accounted for much of his travel during 1979. It was work on the program that occasioned his long London in the spring, his visit to the University of Chicago in September (for the videorecording of the program's debates), and the Friedmans' trips to numerous other places in order to film sequences for the series.

By the end of the year, with production of the series complete in time for its scheduled early-1980 broadcast, Friedman was set to break into the television market as a presenter. And, in having a television program of his own, Friedman owed a definite debt to John Kenneth Galbraith.

Galbraith provides a precedent for ***Free To Choose***

In 1975−1977, Galbraith had set a template to which Friedman would subsequently adhere: writing and hosting a U.K./U.S. coproduction of a multi-part, one-hour series specifically on economics. Galbraith's series was, therefore, an important catalyst for *Free To Choose*. The success of the Galbraith television program both underlined the feasibility of a Friedman television project and created momentum for providing a long-form, televised economic discussion that provided a counterpoint to Galbraith's position.

Galbraith's series was titled *The Age of Uncertainty*. As early as August 1974, the *New York Times* referred to "the thirteen programs [actually, of course, thirteen *episodes* of a single

[25] From Proxmire's remarks in Joint Economic Committee (1970a, p. 810).

television program] he [Galbraith] is preparing for the British Broadcasting Corporation." The article—for which Galbraith supplied an observation that "Milton Friedman… is in the same category as the great witch doctors of the past"—related that Galbraith was already in an advanced stage of writing scripts for the various episodes (*New York Times*, August 4, 1974). As the series' preparation proceeded, Galbraith's television project received further prominent press coverage (see, for example, *Washington Star* (Washington, D.C.), April 23, 1975).

The production of the Galbraith series included location photography in Washington, D.C., in September 1975—when Galbraith gave some narration-to-camera from the Board Room, the traditional venue for in-person FOMC meetings (*Washington Post*, September 27, 1975). At that time, the series was already commissioned for broadcast in the United Kingdom by the British Broadcasting Commission (BBC), which produced the program and financed it in conjunction with the Canadian Broadcasting Corporation and Los Angeles' public-television channel, KCET (*South China Morning Post* (Hong Kong), April 25, 1975). The U.K. broadcast duly took place, with the program—which, as an in-house BBC production, benefited from the production values the BBC gave to documentaries—appearing on a weekly basis starting in January 1977.[26] Galbraith's series aired, it is true, on the channel BBC2, which, though it had a national audience reach, traditionally did not have the mass viewership in the United Kingdom of its sister channel BBC1. But by the standards of BBC2, it was a high-profile program—and it was well received.

In the United States, Galbraith's program was not on the schedule of the three major television networks and so had a lower profile than those networks' nighttime series. But *The Age of Uncertainty* received what was, in effect, a national U.S. broadcast, by being shown in primetime by many of the affiliate channels of the free-to-air, mostly-commercial-free Public Broadcasting System (PBS). The series also received extensive publicity when its U.S. broadcasts occurred. In the New York City area, the series' broadcast premiered on May 19, 1977 (*Sunday News* (New York), May 15, 1977). On the day of the premiere, New York City's mass-circulation *Daily News* newspaper ran a preview of what it called Galbraith's "splendid series," which it declared was "both historically sound and at the same time personal" (*Daily News* (New York), May 19, 1977).

[26] The *Washington Post* account (September 27, 1975) of the making of Galbraith's series stated that it would start its U.K. broadcast in 1976, and indeed Halliwell and Purser's (1986, p. 11) entry for the series registered its broadcast in the United Kingdom as having taken place in 1976. In fact, however, the series was always envisioned for 1977 broadcast (*South China Morning Post* (Hong Kong), April 25, 1975), and the 13-part weekly series began its U.K. airing in January 1977 (*Daily Mirror* (London), January 10, 1977).

The *Age of Uncertainty* series actually received widespread praise in the United States. Galbraith, after years of success in the market for popular and business books on economics, had now made a splash on television in both the United Kingdom and the United States with his series. "Here's television's first show on economics, and it's an economic success," Galbraith was quoted remarking after the U.K. airing of the program (*San Francisco Examiner*, May 15, 1977).[27] The U.S. press article so quoting Galbraith noted: "*The Age of Uncertainty* is the latest 'volume' in that superlative series of the BBC's 'television books' which began with Kenneth Clark's *Civilisation* and continued with Alistair Cooke's *America* and Jacob Bronowski's *The Ascent of Man*, and these have been shown and reshown on TV networks around the world." (*San Francisco Examiner*, May 15, 1977.) The article added that, in the area of actual books, Galbraith had produced "consistent bestsellers" since the 1950s.

Friedman was in his Vermont second home when this recitation of Galbraith's success appeared in the San Francisco press, so he was spared from reading it. But he had been aware of Galbraith's series for a long time. He had turned down the opportunity to confront Galbraith on a panel in an episode of the program, on the grounds that he did not want to be perceived as "a tail to his [Galbraith's] kite" (*San Francisco Examiner*, January 5, 1980). He had, however, agreed to make a preemptive strike on the program by giving an address in London (on August 31, 1976) at the start of the 1976/1977 U.K. television season in which Galbraith's program was airing.[28]

This talk, to the Institute of Economic Affairs, was necessarily concerned with Friedman's reactions to the critique of orthodox economics contained in Galbraith's past writings—so it conveyed Friedman's impressions from "reading Galbaith," rather than with the yet-to-be-broadcast television series.[29] The occasion received a decent amount of media coverage in the United Kingdom when it led off a Friedman pamphlet-cum-book, *From Galbraith to Economic Freedom*, that the Institute of Economic Affairs published at the end of January 1977 (during the fourth week of the U.K. broadcast of *The Age of Uncertainty*).[30] Friedman's talk was also broadcast on U.K. national radio as a 45-minute evening program, *Friedman on Galbraith*, on

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[27] Implicitly, Galbraith was here distinguishing an economics-focused television program from series, like *The Money Programme* in the United Kingdom and *Wall Street Week* in the United States, that already existed when his program aired. Notwithstanding their heavy preoccupation with economics, these programs were primarily concerned with business and financial-market developments.

[28] Earlier on the same U.K. trip, during his talk at the Mont Pelerin Society meetings in Aberdeen, Scotland, Friedman had mentioned the fact that Galbraith was making a television series (Friedman, 1976f, p. 6; 1977h, p. 8).

[29] See Friedman (1977i, p. 13; 1978b, p. 53) and *The Listener* (London), April 21, 1977, p. 500.

[30] Press coverage included that in the *Evening Standard* (London), January 31, 1977.

March 8, 1977, as Galbraith's 13-part series neared its conclusion.[31] The pamphlet also appeared in 1978 in the United States as part of Friedman's book, *Tax Limitation, Inflation and the Role of Government*, discussed in the previous chapter.[32]

But the impact of this Friedman critique of Galbraith (which consisted, in considerable part, of his recapitulation of past analyses of Galbraith's economics by other economists) was small, both in the United Kingdom and the United States, when compared with that of the *Age of Uncertainty* series. And it did not become even the standard reference on critiques of Galbraith. Notably, the textbook discussion of Galbraithian economics in Baumol and Blinder (1979, pp. 822–823) did not mention Friedman's talk.[33]

Galbraith made *The Age of Uncertainty* a multimedia success for himself, as he produced a tie-in book that bore the same title of the series (see Galbraith, 1977) and whose release date (of March 29) was some weeks ahead of the U.S. broadcast of the series.[34] This publication added to Galbraith's track record of major sellers in the book market. In 1977, therefore, Galbraith had done once again what Friedman had (with the partial exception of *Capitalism and Freedom*) been unable to do—produce a major popular book. And he had stolen a march on Friedman in generating his own widely-watched television series.

But when it came to having a television series on economics and accompanying bestselling tie-in book, Friedman would end up having the last laugh. *The Age of Uncertainty* paved the way for Friedman's own series by setting a precedent for a television program that gave an academic economist's perspective on economics. At the time of the launch of his own program, Friedman acknowledged that "the Galbraith series undoubtedly stimulated people to believe that it was desirable to have a series with a different point of view and that it was possible to do something on television." (*The Plain Dealer* (Cleveland, Ohio), January 13, 1980.)

In 1977, the Friedmans had indicated that the Bronowski series *The Ascent of Man* (shown on U.K. television in summer 1973 and then having a U.S. broadcast in 1975) was their main initial template for what became *Free To Choose*, while also mentioning *Civilisation* (broadcast in the

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[31] The radio version of the talk was also largely reproduced in *The Listener* (London), April 21, 1977.
[32] See Friedman (1978b, Chapter 4).
[33] Baumol and Blinder (1979) focused instead on Solow's (1967) review of Galbraith (1967). (Friedman's talk itself referred to this "extremely critical review" by Solow: see Friedman, 1977i, p. 27; 1978b, p. 60.) This remained true of discussions of Galbraith in later editions of the Baumol-Blinder textbook, such as Baumol and Blinder (1982).
[34] For the book's release date, see *Cedar Rapids Gazette* (Iowa), May 1, 1977.

United Kingdom in 1969).[35] Although the physical sciences (the subject matter of *Ascent of Man*) and art (the concern of *Civilisation*) were not strong personal interests of Friedman's, he had shown some attention to the historical developments of each of these fields, and he admired both series for their high production values and educational content. Even in his 1977 discussion, however, Friedman granted that "Galbraith's series... *The Age of Uncertainty*" was a further example of the "kind of thing" that had preceded his own planned series—while also implying that he hoped that his series would be regarded as highly dissimilar to Galbraith's.[36]

The fact that, in contrast to much popular perception, Galbraith did *not* represent the consensus of economists—and was not a producer of mainstream, peer-reviewed economic research—would also prove helpful to Friedman. The unrepresentative economic perspective of Galbraith's program, and in particular his critical view of market mechanisms, created pressure on the BBC and on PBS to agree to support a program presenting an alternative position.[37] And in 1977, as he became more familiar with the television world, Friedman was stepping forward to fill this gap.

Milton Friedman Speaks *and moving to the* ***Free To Choose*** *series*

It was in early 1977 that Friedman became involved in a prospective television program. On January 14, 1977, he met Robert Chitester in San Francisco.[38] "I was then a manager of a public television station [WQLN in Erie, Pennyslvania]," Chitester recalled. Through that position, he was familiar with the Galbraith series, which had just started its U.K. broadcast and scheduled for a PBS run in several months. "There was a need for a contrary expression [to *The Age of Uncertainty*]." (Robert Chitester, interview, July 9, 2013.)

Friedman recalled of the discussions with Chitester, "I agreed, subject to his ability to raise the money," adding: "I really didn't think it was going to happen" (*Wall Street Journal*, October 24, 1979, p. 32). Nevertheless, to help make it happen, the Friedmans cut back on other commitments. As part of this process, in mid-1977, they, in effect, terminated their long-running

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[35] See Vahimagi (1994, pp. 170, 210) for the broadcast dates of these series.
[36] Instructional Dynamics Economics Cassette Tape 215 (December 1977, released January 1978).
[37] Friedman acknowledged this consideration as a factor in Friedman and Friedman (1998, p. 474).
[38] The date was given in Friedman and Friedman (1998, p. 471), as well as the back cover of the *Free To Choose* DVD box set.

audio commentaries.[39]

The Friedmans and Chitester worked out a format for a television program. But Chitester had reservations on the proposal that the three of them had converged on, even as he pressed ahead with arrangements to make the series. "Milton had it in his head that all he would have to do would be to do a series of lectures, and then we could throw a few pictures on the screen and that that would be a good TV series. Well, I didn't try to directly persuade him otherwise… but Milton came round to the recognition that that wasn't going to work. But, in the meantime, we went ahead, and we arranged for [Friedman to give] those fifteen lectures all over the United States. Well, about the third or fourth lecture in, Milton admitted to me, 'Well, Bob, I was wrong, and you were right, and now I'm stuck having to do the rest of these lectures.'" (Robert Chitester, interview, July 9, 2013.)[40] Indeed, in December 1977, by which time the broadcast series was firmly decided as being the documentary-plus-debate mix that would eventually characterize the broadcast programs, Friedman had given only seven of the lectures. He remained committed to giving, and did give, the remaining eight lectures in the first half of the coming year of 1978.

This series of fifteen lectures—under the overall title *Milton Friedman Speaks*—involved, as discussed in the previous chapter, travel by Friedman across various parts of the mainland United States. These series of talks—taped (or, in the case of one episode, filmed) from September 15, 1977, to May 19, 1978—consisted mainly of public talks or Friedman's contributions as a guest speaker at non-academic conferences. Friedman's initial confidence that these lectures would eventually air as a multi-part television series was exhibited during the first lecture (taped at the University of Chicago). In talking to the audience about the lecture in progress, he referred to it as "this TV show."[41]

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[39] See the Friedmans' discussion in Instructional Dynamics Economics Cassette Tape 215 (December 1977, released January 1978). This farewell tape appeared more than six months after the Friedmans made their last regular recording in the series.

[40] As late as February 1978, it was reported that the *Milton Friedman Speaks* lectures then being made would form the basis for a fifteen-part 1979 television series (*Rochester Democrat and Chronicle*, February 24, 1978, p. 3A). However, the decision that the lectures would not be the main basis for the series, and that the lectures would be separately available in the videotape market, had already been settled by late 1977 (Instructional Dynamics Economics Cassette Tape 215, December 1977; released January 1978). The *Wall Street Journal* (July 17, 1978, p. 27) reported that the current plan (which would likewise be abandoned) was to intersperse the broadcast series with excerpts from *Milton Friedman Speaks* (which, the article also confirmed, would itself be marketed separately as a standalone videotape series available for order).

[41] *Milton Friedman Speaks*, Episode 1, "What Is America?," taped October 3, 1977, p. 10 of transcript.

"I think that the lecture series is a very impressive piece of work," Chitester observed in retrospect. Nevertheless, the lecture series was produced during a period when "I was struggling with him [Friedman] to come around to what television was." (Robert Chitester, interview, July 9, 2013.)

As already implied, the plan for Friedman's program to consist of a series of across-the-country lectures would be abandoned on the grounds that it constituted too stilted a format. As Friedman subsequently conceded, "there was no way to create an attractive TV show by taking extracts from lectures" (*Wall Street Journal*, October 24, 1979, p. 32). But, as indicated above, by the time decision was made not to follow this format in the actual television series, Friedman had started taping the lectures and was booked to give many more.

A newspaper article on Friedman that appeared at the time of the *Free To Choose* series in 1980 implied that the lectures recorded in 1977 and 1978 for *Milton Friedman Speaks* eventually "formed the basis" for the screened series (*The Observer* (London), February 17, 1980, p. 33). This, however, was not really the case. The lectures amounted to the abandoned initial template for the series. The actual series was a separate endeavor and seen as such by Chitester and the Friedmans by late 1977. The text of several of the lectures did find their way into print, usually in condensed versions, in the form of Friedman-authored newspaper articles and book chapters over the late 1970s and the 1980s. But the recorded *Milton Friedman Speaks* lectures did not find their way to any appreciable extent in the broadcast Friedman sequences in the *Free To Choose* series. For example, Friedman's exposition regarding how the process of production and delivery of a pencil exemplified the working of the market system appeared in both *Milton Friedman Speaks* and the first episode of *Free To Choose*. But the latter series used new footage of Friedman narrating the story from a desk, rather than replaying the much longer version of the story he had given in 1977 in his University of Chicago *Milton Friedman Speaks* lecture.[42] Nor did the lectures receive their own television broadcast. The sequence of lectures did, however, receive a limited commercial videotape release in 1979−1980—largely on the wholesale market

[42] See *Free To Choose*, PBS, episode 1, "The Power of the Market," January 12, 1980, pp. 3−4 of transcript. Friedman had told the "pencil" story in a talk at Lehigh University at Bethlehem, Pennsylvania, in February 1970 (*The Morning Call* (Allentown, Pennsylvania), February 6, 1970) and in *Milton Friedman Speaks*, Episode 1, "What Is America?," taped October 3, 1977, pp. 10−15 of transcript, as well as Friedman (1977o, p. 10), in each case crediting it to Leonard Read. He also credited the story to Read without actually recounting it, in Friedman (1976f, p. 16; see also Friedman 1977h, p. 11). See also his remarks in CSPAN, November 20, 1994, p. 9 of transcript.

rather than as sell-through videocassettes.[43] A generation later, within a couple of years of Friedman's death, *Milton Friedman Speaks* would finally be issued to the U.S. retail market, in the form of a DVD box set.

The upshot of the decision to abandon *Milton Friedman Speaks* as the basis for a television program was that Friedman moved closer to the Galbraith template of an economist-narrated documentary series. It was this more ambitious conception of Friedman's television series that would constitute *Free To Choose*. The production of this series would occupy Friedman for much of the final two years of the 1970s.

The power of television

Free To Choose signified a recognition on Friedman's part of what he called "the power of television" as a means of reaching the public.[44] He had generally been only an unenthusiastic consumer of television, having expressed reservations about the broadcast media for years. Even in the *Milton Friedman Speaks* lectures, Friedman had remarked acidly that TV and radio provided "what some people regard as entertainment."[45]

Earlier (*Newsweek*, December 1, 1969), Friedman had referred to the "deadening uniformity" and "limited choice" associated with U.S. network television, which he viewed as operating in an environment in which only mass-audience products emerged and niche-audience programs would not be commissioned. When, in May 1978, Friedman referred to the "wasteland… in television" (even while he was giving a talk then planned to be part of a television series), this was one of a number of occasions on which he appealed to the "wasteland" description that one-time Federal Communications Commissioner Newton Minnow had made in the early 1960s when referring to the content of U.S. broadcast television.[46]

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[43] Ordering information for that video series would be provided on the dust jacket of the U.S. hardback version of the *Free To Choose* book. Despite its appearance in such a prominent location, his information was apparently intended for promoters of the book rather than for the everyday reader.
[44] The quotation is from Friedman (1982c, p. vii).
[45] *Milton Friedman Speaks*, Episode 14, "Equality and Freedom in the Free Enterprise System," taped May 1, 1978, p. 20 of transcript.
[46] The Friedman quotation is from *Milton Friedman Speaks*, Episode 14, "Equality and Freedom in the Free Enterprise System," taped May 1, 1978, p. 33 of transcript. For earlier, related statements, see Friedman (1970c, p. 720), Friedman's reference to "a wasteland of television, in the phrase that Minnow used some years ago" (*Milton Friedman Speaks*, Episode 4, "The Role of Government in a Free Society," taped February 9, 1978, p. 26 of transcript), and Friedman's (1962c, p. 284) *Price Theory* text. Minnow made his statement in May 1961, so the invocation of it in the problem-set part of *Price Theory* was testament to the fact that the problem questions in that text pertained primarily to the 1960/1961 version of the class. Minnow was one of only two people referred to in

Friedman attributed this situation to the regulatory framework of U.S. television and contrasted that with the book industry, in which "Westerns, mysteries, and popular romances" were mass sellers, but in which more limited-readership publications were also viable.[47] Friedman lamented governmental and network opposition to the emergence of pay TV—an alternative that he saw as one means of securing more diverse program content (*Newsweek*, December 1, 1969). Along these lines, Friedman observed in May 1976: "The FCC [Federal Communications Commission] has been a major obstacle to the advance of competition—it has slowed the introduction of pay TV, of cable TV, and of other new methods of providing programs. The FCC has been dominated by the networks."[48] During the 1970s, Friedman also looked forward to the spread of videocassettes as an alternative to television.[49]

But Friedman bowed to the reality that television had become and would remain a means of mass communications. "Television is hard to turn down, given the chance to address such an enormous audience," he observed (*The Observer* (London), February 17, 1980, p. 33). Even before Galbraith's program provided an example of a show specialized on economics, Friedman had expressed an interest in raising the standard of economic coverage in television.[50]

The economic deterioration of the 1970s had brought economic discussion more to the fore in television news coverage. As indicated in Chapter 8, the year 1977 when Friedman started his television project was a year during which he already anticipated the return to double-digit inflation of 1979 and 1980. Once this indeed occurred, it would have the effect of making a television series dealing with the economy even more topical when the program went to air in early 1980.

Friedman's television experience did not, of course, begin with *Free To Choose*. Nelson (2020a,

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the original version of *Price Theory* (in Minnow's case, the reference consisting of being quoted but not named) who were still alive sixty years after the book's publication (the other individual being Arnold Harberger, mentioned on page 266 of the text). Minnow died on May 6, 2023.

[47] Friedman (1970f, p. 73).

[48] From Friedman's remarks of May 25, 1976, in Friedman and Nader (1976, p. 33). See also his remarks in *Speaking Freely* (WNBC, May 4, 1969, p. 35), Friedman (1970f, pp. 72–76), Friedman (1977i, p. 39) (reprinted in Friedman, 1991a, p. 146), *Reason* magazine (August 1977, p. 29), *Milton Friedman Speaks*, Episode 4, "The Role of Government in a Free Society," taped February 9, 1978 (p. 13 of transcript), and *Milton Friedman Speaks*, Episode 14, "Equality and Freedom in the Free Enterprise System," taped May 1, 1978 (pp. 32–33 of transcript) for expressions of similar sentiments. Friedman had earlier called for the FCC's abolition during a 1971 appearance in Detroit (*Detroit Free Press*, March 9, 1971).

[49] See *Playboy* (February 1973, p. 58) (reprinted in Friedman [1975a, p. 17; 1983b, p. 26]), and *Milton Friedman Speaks*, Episode 14, "Equality and Freedom in the Free Enterprise System," taped May 1, 1978, p. 34 of transcript.

[50] *CBS Morning News*, CBS, February 17, 1976, p. 21 of transcript.

2020b) and previous chapters of this book have had occasion to refer to numerous television appearances that Friedman made dating back to the 1950s—including a regular spell as a commentator on *CBS Morning News* in the mid-1970s (see Chapter 6 above). Indeed, at the very start of the first *Free To Choose* broadcast, Robert McKenzie, moderator of the debate portions of the program's U.S. episodes, described it as "Milton Friedman's first major television series." In making this qualified statement, McKenzie may have been alluding to the occasional commentaries for *CBS Morning News*. But he was also likely mainly referring to the many occasions in the past when Friedman had appeared at length as a guest on others' television series. These included longtime local Chicago television broadcasters like Elliot Janeway and Irv "Kup" Kupcinet.

The most prominent example of a talk show featuring Friedman—because of the frequency of his appearances, because it was a national show, and because he had been, on a number of occasions, the sole guest, appearing for the full length of the broadcast episode—was, however, likely *Donahue*. Phil Donahue had been a pioneer of the format of the daytime, studio-audience participation public-affairs-oriented talk-show format later associated with Oprah Winfrey. Years later, Donahue would recall (*Piers Morgan Tonight*, CNN, January 7, 2012): "We seldom repeated a guest… Not at all—[or] very few." But Friedman was an exception to this generalization.[51] Donahue's move to the city of Chicago in 1974 led to multiple Friedman appearances on the nationally-syndicated program. Friedman appeared on *Donahue* more than once a year in the period from 1974 to 1976.[52]

In September 1979 and in the early months of 1980—a period during which taping of the debate portions of, and then the publicity tour for, the *Free To Choose* PBS episodes brought him back

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[51] In the 2012 interview, Donahue cited Ralph Nader as one of the exceptions. In addition, many show business personalities made repeat appearances on *Donahue*, which was not invariably a current-events program and often covered current movie or musical releases.

[52] Friedman appeared in *Donahue* on May 1, 1974, again in March 1975, and in consecutive episodes in September-October 1975, followed by another appearance in November 1976. He may also have appeared in other episodes over this period. In the recollection of Gloria Valentine (personal communication, January 15, 2009), "Professor Friedman appeared on too many *Donahue* shows while we were in Chicago[,] and I never had any written confirmation of his appearances because Phil Donahue would call and ask him to appear. It was all too convenient because the show was indeed broadcast from Chicago." She added with regard to Friedman's *Donahue* association, "My reaction is so unfavorable because we would be deluged with a mountain of correspondence… [T]he deluge would continue for a long time because the show would be aired in different parts of the country at different times… During the airing of a *Donahue* show, the address of the guest would appear repeatedly on the screen. Apparently a very large number of the viewers felt they should write to the guest because of the address appearing. Actually we received some letters that said, 'I saw your address and thought I would write to you. Sincerely yours, (name).'"

to the city of Chicago—Donahue had Friedman back as a guest. Friedman made a total of three appearances on Donahue's programs over these months.[53]

A remark that Donahue made on the first of these appearances shed light on how Friedman had reached national celebrity: "One of the wonderful things about you is that, when you speak, I almost always understand you." By 1980, Friedman's success in communicating his economic views to popular audiences had encompassed both popular written outlets like *Newsweek* and television, and, in the realm of television, it had traversed both specialist financial-news programs like *Wall Street Week* and general-audience programs like *Donahue*.[54]

In his September 1979 *Donahue* appearance, Friedman—in reply to a challenge from Donahue that the market system operated on "greed"—gave his own perspective on the market system.[55] "Well, first of all," Friedman replied, "tell me, is there some society you know that *doesn't* run on greed? You think Russia doesn't run on greed? You think *China* doesn't run on greed?... The world runs on individuals pursuing their separate interests… In the only cases in which the masses have escaped the kind of grinding poverty you're talking about, the only cases in recorded history are where they've had capitalism and largely free trade. If you want to know where the masses are worst off, it's exactly in the kinds of societies that depart from that. So that the record of history is absolutely crystal clear that there is no alternative way so far discovered of improving the lot of the ordinary man that can hold a candle to the productive activities that are unleashed by a free enterprise system." He added: "You know, I think you're taking a lot of things for granted. Just tell me where you're going to find these angels who are going to

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[53] Two of them (in early September 1979, and in April 1980) were on Donahue's daytime talk show, and a third (late September 1979) was on NBC's *Today* early-morning show—to which Donahue at the time was a contributor, providing interviews from Chicago.

[54] Larry King (in the early 1980s still primarily a radio show host) also had Friedman as a guest on multiple occasions, naming him in L. King and Yoffe (1982, p. 47) as one of the celebrities he had interviewed. One occasion on which King interviewed was on May 30, 1980, when Friedman was at the Amway Corporation Leadership Convention of May 30, 1980, for King's radio program. (Gloria Valentine, personal communication, November 24, 2008.) Friedman later appeared on King's CNN television program a few weeks after the 1987 stock market crash (*New York Times*, November 26, 1987).

[55] Although *Donahue* episodes were essentially ephemeral, not being rebroadcast after the television season in which they originally appeared, the 1979 *Donahue* episode featuring this Friedman statement was kept in the public eye because it was issued (along with a separate *Donahue* instalment, in which Ayn Rand was the guest) in a commercially-available videotape—released in 1994, long after the broadcast year. The 1979 Friedman *Donahue* episode (along with excerpts from it) was later uploaded to YouTube and other internet outlets. Some of these outlets erroneously gave the date of the episode as 1980. The correct date for this episode, used in Nelson (2009a, 2009b) and here, was obtained by the author via consultation of newspaper television program listings on microfilmed and internet-scanned back issues of newspapers published in the Chicago and New York City areas.

organize society for us. I don't even trust you to do that—let alone myself."[56]

Friedman had made similar statements before.[57] But this delivery of it on television, and its forceful, uninterrupted and self-contained character, meant that the vignette became (posthumously, via the internet) one of Friedman's most remembered sound bites—far more well-known than any single moment in the *Free To Choose* television program.

Notwithstanding his frequent appearances on TV as an interview subject or guest panelist, Friedman knew little before *Free To Choose* got off the ground about the production side of television. His limited experience with the medium as of early 1977 was evident in the fact that, at this time, Friedman was still using the term "TV producers" to mean manufacturers of television sets (Instructional Dynamics Economics Cassette Tape 210, March 1977, Part 2).

Over the following three years, however, Friedman became steeped in the making of television programs. Chitester recalled that, when they arrived at the *Free To Choose* format, which would involve considerable direct-to-camera monologues by the host, Friedman observed, "I could never do many, many takes, or anything like that." In the event, as Chitester noted, "He did exactly that [many takes]. But it took a while, and he had to grow into it." Friedman "learned on the job" when it came to television, Chitester remarked, adding: "He just immersed himself in [the questions], 'How do you do this, or do that? What is the craft of television production?'" (Robert Chitester, interview, July 9, 2013.)

Public television

Neither Galbraith nor Friedman broke through, via their programs, into the big time of U.S. commercial network television. Rather, each of their programs was shown on the Public Broadcasting System (PBS), the American public-television network.

Public television, in the U.S. context, does not mean state-owned television. The "public" part instead refers to the tax deductibility of private-sector donations to PBS, to the provision of some

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[56] *Donahue*, NBC, September 6, 1979.
[57] For example, in his 1973 *Playboy* interview, Friedman asked (February 1973, p. 66 [Friedman, 1975a, p. 31; 1983b, p. 50]): "What kind of society isn't structured on greed?" And in a 1976 interview with Peter Jay on U.K. commercial television, Friedman had stated in response to a Jay remark: "Stop at that point. Can you name me a system which is not based on greed?" (*The Jay Interview*, ITN, July 17, 1976.)

federal government financing of PBS, and to the general absence on the network programs of within-program commercial interruptions.

Although a recent study of aspects of the creation of the *Free To Choose* program asserts that the fact that *Free To Choose* was a PBS broadcast amounted to "privatizing the production of public television" (Jack, 2018, p. 514), the implied claim that *Free To Choose* set a precedent on this score is highly questionable. PBS had, in fact, routinely purchased programs made by commercial enterprises in the past. And, with regard to payment for the broadcasts made by PBS, it was noted well before *Free To Choose* became a reality that "a key slice of public television's support [is] the sponsorship of high-quality, primetime programs by major U.S. corporations" (*Dayton Daily News* (Ohio), February 24, 1977).

In the case of *Free To Choose*, Robert Chitester and his production colleagues made the program by raising private-sector financing from various corporations and foundations, making the series using their own production companies, and selling to PBS the initial U.S. broadcast rights to the series. The total amount raised was $2.4 million (*The Plain Dealer* (Cleveland, Ohio), January 13, 1980). The fact of the corporate sponsorship and the identities of some of the sponsors were given in press coverage of *Free To Choose* in 1979 and 1980, and the Friedmans provided a long list of the sponsors (corporations and foundations) in *Two Lucky People* in 1998.[58] In contrast, Jack (2018) presents the sponsorship details as a revelation obtainable only by a perusal of unpublished items in the Friedman archives—and does not even cite *Two Lucky People* (which had a whole chapter, as well as an appendix, concerned with *Free To Choose*) or even the *Free To Choose* book, let alone any other Friedman book or article.[59] In addition, Jack's (2018, p.

[58] See Friedman and Friedman (1998, p. 603). The list was out of alphabetical order—and was perhaps given in order of size of donation. It was not a complete list of donors. For example, it did not include the Smith Richardson Foundation—which had donated $16,200 to the project (*The Greensboro Record* (North Carolina), March 12, 1979, p. B2).

The *New York Post* (March 4, 1980) had one of the reports of donor numbers for *Free To Choose*. It indicated that the Sarah (Mellon) Scaife Foundation had given $500,000, Getty Oil $330,000, and Readers Digest Association $300,000.

[59] That is, Jack (2018) treats access to the Friedman archives as a *substitute* for an examination of Friedman's published writings and of the economic-research literature on Friedman. These aspects of the citation and analytical practices of the Jack (2018) study actually preclude it from being able to address properly the premise of the article's title—that the series advanced "libertarian ideology." For, by not considering the specialized economic literature contained in the research and textbooks authored by economists, Jack is not in a position to distinguish ideological aspects of *Free To Choose* from those respects in which Friedman was articulating the consensus of economists or was voicing positions that hardline libertarians would oppose. Two other notable shortcomings of the Jack (2018) study are: (*a*) it mentions (p. 527) the Federal Reserve Bank of Dallas' 2003 conference on *Free To Choose* without citing the resulting proceedings volume (see Wynne, Rosenblum, and Formaini, 2004); and (*b*) it gives the wrong year (1990, instead of the correct year of 1991) for the CNBC repeat/update of the series (p. 527).

517) claim that Galbraith's PBS series included, and Friedman's PBS series did not, "coverage of a broad range of economic ideas" is at variance with the fact that Friedman debated critics of his views in the second half of the first nine episodes of the programs broadcast on PBS.[60] Indeed, as was indicated at the start of this chapter, he came under withering criticism in these debates.

Friedman's evolving attitude toward his public role

Although PBS was not the same as "big three" network television, the fact was nevertheless that Friedman now had his own television program. His effort to reach this point reflected, in part, a change in his own perception of his public function.

Friedman had, of course, long had another life as a *Newsweek* columnist: "The way to get the Nobel prize is obviously to write a column for *Newsweek*," Friedman had quipped on the day his Nobel was announced (*Chicago Daily News*, October 14, 1976, p. 17; also in *The Oregonian* (Portland, Oregon), October 15, 1976) and, striking a more serious note a little later, he cited his *Newsweek* column as what "has probably kept me in the public eye, more than anything else" (*Chicago Tribune*, November 28, 1976). But, notwithstanding this and his many other engagements, from the 1940s onward, in media discussions of economic policy, Friedman until the late 1970s had characterized his role in advocating policy change in quite circumscribed terms. His extensive public policy work had remained, until that time, essentially moonlighting from his main role as a researcher and teacher. He was quoted saying two weeks after receiving the Nobel that he was "just a college professor" (*Milwaukee Sentinel*, October 30, 1976).[61]

Furthermore, when it came to his public pronouncements, Friedman's position was: "It's not my job to persuade people about things." He described his function as, instead, one of keeping ideas alive so that they remained policy options for the future (*Chicago Daily News*, October 14, 1976,

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[60] Episode ten was a wrap-up episode, in which Friedman again faced skeptical questioning.

[61] Anna Schwartz's (1979a, p. 67) profile of Friedman also described him as (having been) a "college professor." Friedman used this specific term on a number of occasions in characterizing his position (for examples, see Nelson, 2020a, Chapter 4, as well as Instructional Dynamics Economics Cassette Tape 10, January 1969). It was, however, a little unusual as a label for University of Chicago economics professors of Friedman's era, as "college" at that university referred specifically to the undergraduate population, for whom Friedman taught classes only infrequently. William Dewald, at the university in the early 1960s, recalled the situation as follows: "I taught undergraduate as well as graduate level courses at Chicago, as did some others, too. Senior faculty didn't teach undergraduate classes, although many gave individual lectures in undergraduate classes." (William Dewald, personal communication, November 7, 2013.)

p. 6).[62]

Even before the 1970s, however, Friedman's activities were not really as circumscribed as this description suggested. He certainly tried to persuade economists, policymakers, and business people in his personal interactions with them.[63] In addition, his *Newsweek* column aside, Friedman's pre-1970 public activities included such high-profile ventures as participation in the Goldwater and Nixon presidential campaigns and his activism in various forums on abolishing conscription. Then 1970−1971 had seen considerable public activism on Friedman's part as he urged, unsuccessfully, that aggregate demand restraint be continued and then opposed wage and price controls once Nixon introduced them. Indeed, with regard to Friedman's outlook toward monetary matters, Patinkin (1984, p. 99) would liken Friedman to Keynes on the grounds that both economists regarded the galvanization of public opinion in support of their policy positions as a key part of their task.[64] Those who worked with Friedman shared this assessment. Anna Schwartz (1998b, p. 5) noted that in the areas of both monetary economics and market economics, Friedman based his case on reasoned argument but nonetheless was "basically an evangelical, with [a] crusading zeal to make readers and listeners understand his vision." Rose Friedman, long involved in the public-policy aspects of her spouse's activities, stated (*Daily Mail* (London), March 18, 1980): "His life is committed to telling people: 'We are going down the wrong tracks, and that is why countries like America and Britain are having so many problems.'"

Nonetheless, for a long time, Friedman largely resisted the characterization of himself as someone whose task was to persuade people on matters of economic policy. The advent of *Free To Choose* confirmed a change in his position. In the first episode of the series—for which the

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[62] Friedman articulated this picture of his role on several occasions. See Nelson (2020a, Chapter 9). This particular 1976 quotation was repeated by the *San Francisco Chronicle* (January 20, 1979, p. 9) when advertising a forthcoming series of Friedman op-eds for the newspaper.

[63] In *The Times* (London), September 28, 1982, Rose Friedman suggested that their joint memoirs would talk about Friedman's long-running attempts to persuade fellow economists of his views, and Friedman (1986b, p. 3) would later recall his interaction with Federal Reserve Board members as consisting of trying to persuade them of the merits of a constant-monetary-growth rule. (As well as his one-on-one interactions with Board governors and chairs, Friedman probably had in mind his appearances at the outside consultants' meetings with Board governors from 1965 to 1978, discussed in the previous volume and previous chapters of this book.) In addition, in a talk on May 7, 1970, when defending the Nixon Administration's and the Federal Reserve's disinflationary strategy against calls for a macroeconomic U-turn, Friedman (1970g, pp. 64–65) told an audience of executives that he was trying "to persuade people like yourself that many of the policies which you propose, and that you bring pressure for, are most unwise…"

[64] Patinkin, who lived outside the United States, probably had in mind Friedman's popular writings, many of them pre-*Free To Choose*.

broadcast date, in many U.S. markets, was January 12, 1980, although in some cities it debuted a day or two earlier—Friedman acknowledged that he, too, now perceived his role as one of pushing along public opinion: "my whole function and purpose is to try to persuade the people, to make a different thinking politically profitable."[65]

Friedman "has not shrunk from advocating and defending novel and unorthodox ideas in public forums," Anna Schwartz noted in a mid-1979 profile of Friedman in *Challenge* magazine.[66] "This unusual degree of engagement," she went on, "…has made him a celebrity."[67] Indeed, by this time—even before the release to the public of the *Free To Choose* book and television program—Friedman had, in the estimation of many, already passed Galbraith in national fame, with another magazine's interview with him naming him "America's best-known economist" (*Expo*, Summer 1979, p. 16).

Schwartz's profile of her longtime writing partner also referred explicitly to what had cast a pall over Friedman's fame in the second half of the 1970s: "organized demonstrations have been conducted against him" over Chile. Schwartz saw this outcome as stemming from a prevalent practice of incorrectly attributing to Friedman a "support for the military regime in Chile," but she acknowledged that the critics' attacks on Friedman on this issue had built up an image of him as a "hardhearted reactionary" in some circles.[68] During 1979 and into the new decade, Friedman would continue to have some of his public engagements interrupted by protests related to Chile.

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[65] *Free To Choose*, PBS, Episode 1, "The Power of the Market," debate portion, p. 10 of transcript. The preface to the *Free To Choose* book went some way to reconciling Friedman's varying attitudes about persuading people, telling the readership that the "only person who can truly persuade you is yourself," yet expressing the hope that the book would shape the reader's views (Friedman and Friedman, 1980, p. xvi). (Friedman also quoted this passage in Friedman, 1982c, p. viii.)

[66] Note that Schwartz, correctly, did not put a "for" after "advocating"—clearly showing conscious or unconscious awareness that placement of the word "for" in between "advocating" and the item being advocated was neither necessary nor appropriate (see, for example, Merriam-Webster, 2002, p. 31). Likewise, the joint writings of Friedman and Schwartz make it clear that they were aware that the correct formulation was "advocacy of" (when referring to making the case in favor of a concept or policy), not "advocacy for"—see, for example, Friedman and Schwartz (1963a, pp. 227, 383). The same awareness has not been present in a couple of the recent writings on Friedman cited above. Jack (2018) repeatedly incorrectly refers to "advocating for" and "advocacy for" free-market economics. By this curious rule, Friedman's (1943, p. 61) statement, "The spendings tax has been advocated for many years" would instead be rendered as: "The spendings tax has been advocated for for many years." (Jack's only instance of correctly omitting the "for" is actually when quoting Friedman-composed materials—see Jack, 2018, p. 519.) In a related vein, Appelbaum (2020) similarly incorrectly used "advocacy for" or "advocate(s) for" on a total of five occasions and the correct construction only once. This improper usage was especially jarring in light of the fact Appelbaum's piece was a preface inserted into a reissue of a Friedman book that used the terms correctly.

[67] Schwartz (1979a, p. 67).

[68] Schwartz (1979a, p. 67).

Meanwhile, in Chile itself, in 1979 saw a momentous, and ultimately disastrous, move in economic policy, when, in March, its exchange rate was fixed at a high level against the U.S. dollar—just ahead of the early-1980s period of sharp dollar appreciation and world commodity-price weakness (see Dornbusch, 1986, p. 2). The economic crash in 1982 that flowed from this decision would set off an intensification of the criticism in the international media Friedman over Chile's economic policy—and, for some time, would end Friedman's ability to point toward sizable growth in real income as being associated with the economic policies instituted by the junta.

The Chile issue flared up at the mid-December 1979 Hoover Institution conscription conference at which Friedman was one of the presenters, when a pro-draft audience member implied that Friedman's visit to Chile meant that what he said on other matters, including conscription, should be dismissed. The session chair, Albert Rees, ruled the intervention out of order without seeking a response on Friedman's part.[69] Earlier, during one of the *Free To Choose* panel debates videotaped in Chicago in September, one of Friedman's critics, Frances Fox Piven, raised the matter.[70] In response, the moderator, Robert McKenzie, indicated that he did not want the panel discussion to dwell on Chile.[71] Friedman nevertheless had already briefly responded, staying that "Chile is not a politically free system, and I do not condone the political system. But the people there are freer than the people in communist societies, because government plays a smaller role because [of] the free enterprise that has been emerging." He reaffirmed that he would like to see Chile "get rid of the junta."[72]

The development of the series

The debate portion, although an agreed part of the broadcasts for the United States, was a late step in the production of the *Free To Choose* series. Indeed, the panelists at those September 1979 debates had as the jumping-off point of their discussions the filmed documentary portion of the episode, hosted and narrated by Friedman. The part of the series that was being developed during 1977 and 1978, and then made mainly from summer 1978 to summer 1979, was the documentary portion.

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[69] See Anderson (1982, p. 198).
[70] *Free To Choose*, PBS, Episode 5, "Created Equal," February 15, 1980, studio debate portion,, p. 8 of transcript.
[71] *Free To Choose*, PBS, Episode 5, "Created Equal," February 15, 1980, studio debate portion, p. 9 of transcript.
[72] *Free To Choose*, PBS, Episode 5, "Created Equal," February 15, 1980, studio debate portion, pp. 8–9 of transcript.

It was in these filmed episodes that U.K. writer Antony (Tony) Jay had a key production role. His involvement began once Friedman and Chitester arranged for Jay's U.K. company, Video Arts, to make the series. Tony Jay recalled: "He [Friedman] tried to find people in America to produce it, and he found anybody who wanted to produce it also wanted to tell him that he was wrong and that he didn't understand. So he approached his friend Ralph Harris at the Institute of Economic Affairs and said, 'Well, is there anybody in England who could set this up?' Knowing that I was a film producer, Ralph got in touch with me and said would I be interested." (Sir Antony Jay, interview, May 29, 2013.)

Although he had longstanding sympathy for free-market ideas, Jay had had problems gaining a good feel for the economic analysis associated with those ideas. In this connection, his confidence in Friedman's public communications skills was bolstered when Jay read Friedman's *Playboy* interview.[73] "I'd given up on the idea of ever understanding economics by reading, or trying to read, the standard economic books, and then suddenly I read this interview in *Playboy*. And I thought, 'Well, if that's economics, I can understand it,' because [in the interview] Milton is so lucid, and so down to earth, and practical and factual." (Sir Antony Jay, interview, May 29, 2013.)

Tony Jay viewed it as imperative that the monologue portions of the series not seem like lectures to the viewer: "when you're doing a television documentary like that, you're really only talking to one person. Whereas, in a lecture, you're talking to a roomful—a lecture theater-full." Friedman expressed some worry about losing control of the series and initially expressed considerable irritation with Jay's suggestions in correspondence and by telephone.[74] Jay noted that, nevertheless, Friedman came to appreciate the merits of Jay's posture "very quickly… Milton very quickly got onto the idea—that it's a personal talk, a talk to a single person, and it's not the same as a public lecture." (Sir Antony Jay, interview, May 29, 2013.)

Compression and simplification of the program's scripts also ensued from the recommendations of Jay and Chitester. Tony Jay recalled with regard to Friedman's scripts that "the original drafts

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[73] See also Friedman and Friedman (1998, p. 411). On the basis of that account, it would seem that the Friedmans had the impression that Tony Jay had read the interview well before the conversations about the *Free To Choose* project. However, Jay's recollection (interview, May 29, 2013) was that, when the Institute of Economic Affairs approached him about possible involvement in the *Free To Choose* project, "I think they sent me the *Playboy* interview." Jay did note that, through his longtime interest in current affairs, he had, by this time, considerable familiarity with 'Friedman's political-economic approach, which I absorbed quite a lot in years previously." (Sir Antony Jay, interview, May 29, 2013.)

[74] See Friedman and Friedman (1998, pp. 479–480).

were terribly detailed, and went on and on supporting things with arguments—which would bore the audience. And we said [to Friedman], 'No, this is television. If people really want to know the details of the argument, they can get your books and look into it further. But [on television] all you've got to do is to put forward your view—not endlessly support it with argument and refutation.' And I think Milton, being very quick-minded, saw this and went along with it." (Sir Antony Jay, interview, May 29, 2013.)

Friedman accordingly went for script simplification. However, he *did* use scripts. Although a *Wall Street Journal* report on the 18-month making of the programs (October 24, 1979, p. 32) implied that the material he delivered to the film camera in the episodes was spontaneous, being unprepared except for their general gist, this was not to the case.[75] Friedman did prepare scripts of what he planned to say and, as already indicated, he made multiple takes of his deliveries to camera. His observation to the *Wall Street Journal* reporter that "I'd be dead as a doornail if I had to read scripts" (*Wall Street Journal*, October 24, 1979, p. 32) and his later remark that "I insisted we have no script" (*Chicago Tribune*, July 20, 1980, p. F21) did not actually imply that his addresses to camera consisted of full-scale ad-libbing.[76] Instead, they were references to the fact that the words he used in the program were his own—and to his practice of not memorizing or reciting the scripts he had prepared. In the course of his multiple takes, Friedman did make changes to his to-camera narration, with these alterations partly reflecting his consultations with the production crew on making improvements in his exposition.

A combination of international filming, direct-to-camera monologues by Friedman on location, and narration by Friedman of other location footage, became the format of the main portion (the filmed documentaries) of the *Free To Choose* episodes.

For the shooting of these programs, Tony Jay (who would eventually take a "Creative Consultant" credit for the series) recalled that his Video Arts company secured for *Free To Choose* "a terribly good director called Michael Latham, who got on terribly well with Milton… And, actually, Milton was terribly impressed with Mike Latham and his understanding of communication." The *Wall Street Journal* on-set report noted that Latham shared with the director on-set consultations with Friedman regarding upcoming scenes to be shot. However,

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[75] The videotaped second halves of each episode—the free-for-all panel debates—were indeed unscripted.
[76] Friedman had also affirmed that the television program was unscripted in *New York Times*, February 24, 1980.

Latham's formal role was of producer of (the filmed portions of) *Free To Choose*.[77] The contemporaneous *Wall Street Journal* report on the making of the series observed that Rose Friedman, who had no on-screen role in the program but was one of its associate producers and was constantly present during filming, played an important role in honing the manner and substance of Friedman's deliveries in the series. Tony Jay recalled that Latham encouraged this process during filming: "he was very good, also, in bringing Rose into it, because she was really quite important, and Milton paid a lot of attention to her judgment." (Sir Antony Jay, interview, May 29, 2013.)

When it came to Friedman traits that were on display as they made the program, Tony Jay recalled being especially "impressed by two things, really. One was his intellectual clarity, and the other was his wonderful good humor. And when we were filming *Free To Choose*, even if the assistant cameraman disagreed with him [on economics], he'd have a long conversation with him to try and show him he was right, but, always, he was in terribly good humor. He was never vindictive or confrontational. He was persuasive." (Sir Antony Jay, interview, May 29, 2013.)

The 1979 portion of the making of the series coincided with the Friedmans' frantic drafting of the book version of *Free To Choose*. "We started writing on January 1, 1979, we delivered the manuscript in September, and our book was in the bookstores by Christmas," Friedman observed. We literally worked on it morning, noon, and night. I tell you: I'll never do anything on such a tight deadline again." (*New York Times*, February 24, 1980.)

The broadcast of the television program and the widespread distribution of the book would both begin in the first half of January 1980 and would be accompanied by a series of publicity appearances by Friedman. The broadcast episodes of *Free To Choose* and the accompanying book, and the reception that each received, will be analyzed in detail in the course of the next chapter's coverage of developments in 1980.

Teaching judges

Although the teaching of university students was now behind him, for Friedman in 1979 the teaching of economics courses was not quite over. 1979 saw the culmination of Friedman's involvement as one of the lectures in a special short course in economics principles. This was

[77] In the BBC documentary tradition (from which Latham came), the producer had an activist function in filmed-on-location documentary programs, of the kind often reserved solely for the director in other areas of television and film (see Halliwell and Purser, 1986, p. 656).

teaching in a loose sense: the course of lectures would be delivered to U.S. trial judges. Consequently, considerable audience participation was par for the course—making the occasion more of a seminar-conference program.

The annual series of economists' lectures provided to U.S. judges was organized by Henry Manne of the University of Miami. The series began in 1976/1977 academic year, and Friedman was a lecturer in the initial three years for which the lecture program—called the Economic Institute for Federal Judges—was offered.[78]

Manne quipped that "the first program we had in economics for federal judges was the greatest economics faculty ever assembled in the history of the world." Paul Samuelson and Almen Alchian were among Friedman's fellow teachers. "It was a fantastic group," recalled Manne, "and they were all magnificent teachers, and all of them sensed, I think, the importance of what we were trying to do." (Henry Manne, interview, April 30, 2014.)

Manne added that "Paul Samuelson was the only one who was leery about it." Samuelson was concerned that the course would talk up free markets instead of being an exposition of straight economic analysis. But, as the course proceeded, Samuelson was won over, having become satisfied that its intent was to provide a serious exposition of technical economics. Indeed, Samuelson continued to participate as a teacher of the course for several years after Friedman ended his own involvement in it. "Milton and Paul and Armen—all of them really just came alive with that kind of audience," Manne observed (Henry Manne, interview, April 30, 2014).[79]

In a foreword to a recent reissue of *Capitalism and Freedom*, Appelbaum (2020, p. xvi) devotes a paragraph to Manne's course and concludes: "Manne had succeeded in reeducating fully 40 percent of all sitting federal judges." The *Oxford English Dictionary* notes of the word "reeducation": "From the mid 20th cent.[,] often associated with programs for the internment of political dissidents in some authoritarian societies, esp. communist China and Vietnam." It follows that, unless one believes it appropriate to liken judges' signing up to a short economics

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[78] In December 1976, the first time the course was offered, it was a five-day program. Friedman participated on this occasion and then made return appearances on April 28, 1978, and April 13, 1979 (Gloria Valentine, personal communication, February 22, 2015). By the time of the 1979 course, the overall program consisted of two weeks of lectures. (See *Fortune*, May 21, 1979, for a news article giving some examples of the course's content in 1979, when other teachers included Martin Feldstein.)

[79] Later, the teachers included Benjamin Klein, Friedman's former student who had become prominent in the law-and-economics field (*Fortune*, May 21, 1979, p. 59).

course to the internment by authoritarian regimes of political prisoners, Appelbaum's use of the term "reeducating" is over the top.

Furthermore, his implication that judges were converted to free-market economics by the course is vulnerable on two other counts: it presumes that the judges were highly pliable, rather than being independent thinkers; and, as Samuelson was a teacher of the course—a fact Appelbaum does not mention—and Samuelson in the 1970s was a defender of both the 1971 wage-price freeze and the decade's maintenance of energy-price controls, the economists with whom Manne launched the course were hardly uniform exponents of a free-market line.

Ash, Chen, and Naidu (2022) argue on the basis of statistical evidence that, in the event, the course did likely have an influence on some of the subsequent judgments made by attendees and on the language they used. The subject matter of the judgments in these cases appears, however, to have been largely in areas like antitrust that were not Friedman's specialty and that were not a major concern of the lectures he gave to the course.[80]

For his own part, Friedman, in his contributions to the course, covered a mixture of his positive-economic analysis and his normative economics. The content of his contributions also consisted of a combination of macroeconomics and microeconomics. The range of material that Friedman covered was reflected in the fact that his final contribution to the course, for a session given on April 13, 1979, titled: "Capitalism, Freedom, and Inflation." (Gloria Valentine, personal communication, February 22, 2015.)

Three years after he had dropped out as one of the teachers of the course, Friedman continued to underline the importance of the course—although he stressed that not only lawyers, but also managers of corporations, needed to become more literate regarding the principles of economics.[81]

Healthcare

In the mid-1970s, Friedman had suggested that it was inevitable that, at some point in the next

[80] Ash, Chen, and Naidu (2022) go further and suggest that Friedman's lectures were notably uninfluential. Their basis for doing so, however, is to take as established fact the unproven—(and undocumented other than via an implausible anecdote)—proposition that Friedman's lectures focused on drug legalization.
[81] Friedman (1982c, p. 58).

decade, the U.S. government would introduce national health insurance (Instructional Dynamics Economics Cassette Tape 152, August 21, 1974). This prediction was not realized. But in the summer of 1979—a period corresponding to the midpoint of the decade to which Friedman had made the prediction—it looked like that development might well eventuate, as the political momentum in favor of national health insurance was stronger than it had ever previously been in the United States.

The buildup of support for a move to universal health insurance, and not any enthusiasm on his part for the measure, had been the basis for Friedman's prediction. Friedman himself was strongly opposed to a federal government universal health insurance scheme. In 1977, he remarked: "There are some strictly public health activities in which the government has been involved for quite some time. I am talking about programs such as the control of epidemics, assuring the quality of water, and measures of this kind that have been in the public domain for over 100 years. So there is something to be said for governmental expenditures in these areas of health care. Also, as long as there are governmental programs to assist the indigent, these inevitably would include assistance to pay healthcare costs. But, beyond these two elements, I believe there is no case whatsoever for governmental financing or involvement in healthcare." (*Federation of Hospitals Review*, April 1977, p. 23.)

In that same year, Friedman indicated that the Carter Administration was likely to try to institute a federal-government-operated, single-payer system. Friedman remarked in May 1977 regarding the then-new president: "He is not a 'conservative' in any way whatsoever, so far as I can see, in the sense of being in favor of a small government. He has come out openly in favor of vast expansions in government power. He has come out in favor of a national health insurance program which would, in my opinion, be a medical as well as a social and financial disaster in the United States." (*The Open Mind*, PBS, May 31, 1977, p. 14 of transcript.)

When President Carter advanced a National Health Plan over two years later, on June 12, 1979, the sensitivity of the issue of medical insurance had been heightened in U.S. political debate by Carter's political prospects in 1980: he was on course to be challenged by Senator Teddy Kennedy in the Democratic primaries. Senator Kennedy had already come out in favor of government-provided universal health insurance, and, indeed, had announced a specific proposal to that effect on May 14, 1979.[82]

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[82] On the dates of the announcements of each plan, see Hales (1980, p. 347).

In contrast to the Kennedy proposal, the Carter plan did not seek to have government-provided insurance cover all the U.S. population—and so did not actually amount to a national health insurance or single-payer scheme of the kind seen in several other advanced economies. The Carter plan nevertheless sought to move to an outcome in which everyone in the United States possessed health insurance. Carter proposed to secure such universality by mandating that each U.S. firm purchase insurance for their employees—buying the insurance either from private-sector providers or from a new government health-insurance agency (to be called HealthCare). Unlike Kennedy's plan, in which insurance would cover wide-ranging medical expenses, the health insurance that would be made mandatory under the Carter plan pertained only to catastrophic medical contingencies. The Carter proposal was formally submitted to Congress on September 25, 1979, as draft legislation labeled The National Health Care Act. The Kennedy proposal had already been submitted, under the name of The Health Care for All Americans Act, on September 6.[83] Neither of the rival bills became law.

In late 1979, when outlining his disagreements with the Kennedy single-payer proposal in particular, Friedman criticized the record of universal health insurance in other countries.[84] He also professed to see the global trend as being one of moving away from government-provided health insurance. "One of the interesting cases is Australia," Friedman remarked in this connection. "Australia, a few years ago, introduced national health. They have now backed away and terminated it." (*The Register* (Santa Ana, Orange County, California), December 23, 1979, p. E10.)

It turned out, however, that the case of Australia would provide only a limited, and ultimately only an ephemeral, example of a retreat by a country from national health-insurance arrangements. Coming into office in late 1975, Prime Minister Malcolm Fraser had pledged that he would maintain the national health-insurance scheme introduced by the previous Labor government. In the event, over 1976–1979, Fraser did, as Friedman implied in his interview remarks, dismantle aspects of universal health insurance—via steps that involved permitting taxpayers to opt out of the insurance scheme (and the associated income-tax surcharge) and restoring more user-pays elements to health provision. Nevertheless, even in these years, the policy of Fraser's government was that every Australian had to have health insurance of some

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[83] On these details of the bills and the dates of their submissions to Congress, see Committee on Ways and Means, U.S. House of Representatives, and Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, pp. 1–2, 10–11).
[84] See also his earlier remarks in *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978.

kind. It was only in 1981 that something truly close to the abolition of universal medical insurance was instituted by the Fraser Government—via a policy change under which the government continued to cover the medical costs of the elderly and those on low incomes but made no requirement that Australians purchase any insurance—and made no promises to pay the healthcare costs of the population as a whole (*The Age* (Melbourne), April 30, 1981). This more limited insurance regime failed to "take," however. The Labor government that succeeded Fraser's instituted a new universal medical insurance scheme in February 1984, and that scheme (called Medicare) remains in place today.

Friedman's own posture regarding health insurance would also undergo change to a less *laissez-faire* approach. As of the 1970s, his position was that there should be no requirement for U.S. citizens to purchase medical insurance. In addition, although, as indicated above, he was in favor of U.S. government support for low-income citizens' health costs, he believed that there should not be funds earmarked for this purpose. Rather, the support provided by the government should take the generic form of outlays directed to low-income households through a negative-income-tax system (Instructional Dynamics Economics Cassette Tape 102, June 28, 1972; *The Jay Interview*, ITN, July 17, 1976). In contrast, by the early 1990s, Friedman was in favor of a legal requirement that all households purchase catastrophic health insurance (*Wall Street Journal*, November 12, 1991). The 1979 Carter plan, as noted above, had envisioned such mandatory purchases being undertaken by firms on employees' behalf—rather than by American households themselves, as in Friedman's latter-day plan (which is discussed in more detail in Chapter 18 below). Friedman had, nevertheless, made a notable change in his position—a change that aligned him, to a considerable degree, with what the never-implemented National Health Plan of the Carter Administration had advocated.

Productivity

One of Friedman's concerns about the prospect of national health insurance was the likelihood that it would harm U.S. national productivity. Although he conceded that this particular measure was likely only to result in a small reduction in U.S. productivity (*Federation of Hospitals Review*, April 1977, p. 23), such a move would have constituted another of the items diverting the direction of resources from the private sector to the public sector that Friedman believed were in large part responsible for the weakening of U.S. productivity performance in the 1970s.

In early 1979, the scale of the deterioration in the trend of U.S. productivity became recognized to a far greater extent than it had been before. To some degree, the weakening since the 1960s had already been acknowledged, as estimates of potential output growth as high as 4 percent had become uncommon. But estimates above 3 percent remained prevalent until 1979. The productivity softness of the first half of the 1970s had largely been interpreted as a demand rather than a supply phenomenon. Even the 1977-vintage revisions of aggregate supply made by the Council of Economic Advisers and numerous other analysts did not perceive a change in the slope of the path of potential output after 1973. Typically, these estimates merely suggested that potential output was growing more slowly than in the 1960s—or that there may have been a one-time shift down in the level of potential output in the United States and other countries around late 1973.[85] A development that apparently affirmed the validity of this approach in the case of the United States was that, in the initial years of the recovery that began in 1977, the rebound in U.S. aggregate output following its 1975 trough resembled the strong recoveries seen in earlier postwar expansions (see McNees, 1978). In retrospect, however, this development seems to have been a source of false comfort regarding the aggregate-supply picture. The observed expansion of output likely reflected the flexible short-run response of real economic activity to aggregate demand stimulation, rather than true resilience of productivity growth (even though the strength in real GDP was associated, to some degree, with rises in measured productivity).

Data revisions after the 1970s would tend to raise estimates of real output in certain years, but they would not change the basic picture of a disappointing performance with regard to U.S. economic growth over the decade. In 1970, Paul Samuelson had suggested that a 50 percent rise in aggregate U.S. real output could be expected to occur over the decade (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 61, October 19, 1970). In contrast, the actual increase from 1969:Q4 to 1979:Q4, according to modern data on U.S. real GDP, was slightly under 38 percent.

By 1979, the evidence already available was confirming the reality of a step-down in the growth rates of productivity and output—and not just a one-time downward shift in their levels. Signs of a lasting post-1973 slowdown in productivity became more evident during 1978. Although Blinder (1979, p. 64) would emphasize that in the 1975:Q4−1977:Q4 period, "productivity growth was almost exactly equal to its 1967−1973 average," data for the year 1977, considered

[85] See, for example, Artus (1977) and Wonnacott and Wonnacott (1979, p. 334). At this stage, the CEA accepted that U.S. productivity growth had moved down around 1967 but recognized no step-down during the 1970s. See Orphanides (2003, p. 656).

in isolation, would point toward likely weakness in productivity growth.[86] Although he recognized this fact, CEA Chairman Charles Schultze initially remained optimistic, stating in a memorandum in mid-1978 with regard to weak productivity growth in the year to 1978:Q2: "We have not been able to explain it, but [we] see no reason to assume that this very bad experience will persist."[87] Subsequently, however, data released in January 1979 registered productivity growth of 0.4 percent for 1978, the lowest reading since 1974 (*San Jose Mercury* (California), January 27, 1979; Carruth, 1993, p. 733). Although, as noted in the previous chapter, this weak 1978 productivity number would be revised up to a much stronger value, the revisions largely reshuffled the poor productivity data points to the years adjacent to 1978, rather than eliminating the evidence of sustained weakness. Even in retrospect, therefore, it is clear that the Carter Administration was on solid ground when, in January 1979, it substantially revised down its estimate of the post-1973 growth in underlying productivity. This reassessment put post-1973 potential output growth at 3 percent, down from the previous estimate of 3.5 percent (*Citibank Monthly Economic Letter,* February 1979, p. 1).

It is against this background of a basic reassessment of productivity trend that Friedman declared with regard to the rise in output since 1975: "this has been a kind of joyless expansion" (*Chicago Tribune,* July 15, 1979). By this, he meant that the recovery, although associated with increases in employment, had seen little growth in per capita output or living standards. "I doubt that there has been any expansion in the postwar period which has been as weak in terms of productivity," Friedman observed (*Chicago Tribune,* July 15, 1979). He would be comfortable about an outcome of little or no growth per capita, he told Phil Donahue a couple of months later, if that outcome emerged from a concerted decision by the private sector, on the average, that it was satisfied with its existing level of income and was, consequently, now unwilling to devote resources, in aggregate, in pursuit of improvements in U.S. productivity. But this scenario did not, in fact, describe the process by which low productivity growth had come about in the United States. Rather, the country still had the characteristic that "we have a desperate *desire* to grow," and such growth was, in Friedman's view, being impeded by increasing government intervention in the economy (*Donahue*, NBC, September 6, 1979).

The longer-term policy agenda, as far as Friedman was concerned, should be to bring the country's productivity growth back up, by increasing the role assigned in the U.S. economy to market mechanisms. For the moment, however, he accepted as a fact the decline in the post-

[86] See DeMilner (1978, p. 528), Miller (1978c), and Section I of the previous chapter.
[87] Quoted in Biven (2002, p. 201).

1973 productivity trend embedded in the official 1979 revision of potential output. This revision had a bearing on his own prescription for monetary policy, as Friedman's monetary-growth recommendation required an estimate of potential-output growth. Although he had been outspoken before 1979 on the likelihood that the natural rate of unemployment had risen in the United States, Friedman had, until this point, not really concentrated attention on the possibility that the growth rate of potential output had declined. Indeed, in 1978, in giving 4 percent as his recommended long-run growth rate for the M2 aggregate, Friedman was using a number that apparently stemmed from a stationary-velocity assumption alongside an assumed potential output growth rate of about 4 percent—higher than the 3.5 percent espoused by the CEA at the time (*Newsweek*, July 24, 1978).[88]

After 1978, however, Friedman more fully absorbed the post-1973 decline in potential growth. This change in his thinking was evident in the contrast between his 1976 remark with regard to the prospect of 4 percent output growth that year—"It wouldn't be a vigorous expansion" (Instructional Dynamics Economics Cassette Tape 188, March 1976, Part 2)—and his 1986 reference to "a vigorous 3.8 percent" growth (*Wall Street Journal,* September 18, 1986). Indeed, Friedman evidently accepted the new official estimates of potential growth quite rapidly. Soon after those new estimates were issued, Friedman was giving potential growth as 3 percent or 3 to 3.5 percent (*San Jose Mercury* (California), February 11, 1979) and, by midyear, he had incorporated the administration's 3 percent potential-growth number into his monetary policy recommendations: The Federal Reserve, he said in June, should get M2 growth down by 1 percentage point per year until it reached 3 percent, then keep it there. "I guarantee that will bring inflation to zero," Friedman said, adding: "If that's wrong, I've wasted a lot of years of my life." (*American Banker*, June 12, 1979, p. 3.)[89]

The Soviet Union's economy reconsidered

Although the productivity slowdown of the 1970s was only really being recognized in the closing years of the decade, it was not, of course, new for U.S. economic-policy discourse to lay great stress on the need for improvements in national productivity and to secure a higher long-

[88] He had earlier, however, indicated his agreement with the lowering of estimates of potential output growth to 3.5 percent (Instructional Dynamics Economics Cassette Tape 208, February 1977, Part 1).
[89] On *Wall Street Week* (Maryland Public Television, January 4, 1980, p. 12 of transcript), Friedman reverted to giving "about 4 percent" for his preferred ultimate M2 growth rate and so presumable his estimate of long-run potential growth. But this was probably a slip: On other occasions in the first half of the 1980s (for example, Friedman, 1984a, p. 58), he was explicitly giving an assumed value for potential real output growth of 3 percent.

term economic growth rate. Calls to bring up the United States' rate of economic growth had been very prominent, for example, in debates on economic policy in the late 1950s.[90] But one notable difference between that era and the discussions of U.S. productivity during 1979 was the weight placed on the economic performance of the Soviet Union. In contrast to the late 1950s, during 1979 the postwar economic-growth record of the USSR was no longer being cited as a motivation for the United States to up its game or as an economic threat to the nation.

The different outlook toward the USSR by 1979 was reflected in remarks made in the middle of the year by former Under Secretary of State George W. Ball: "The Soviet Union today is faced with terrible problems that make even the problems that preoccupy us look rather simple. They have an economy which simply is not working, and in the nature of things I do not think can be made to work."[91]

Observations such as Ball's reflected the fact that, in the course of the 1970s, there had been a coming of age among watchers of the USSR. The view, so prevalent in the 1950s, that the Soviet Union was a serious economic competitor *vis a vis* the USA had been decisively banished. The chronically poor state of the Soviet economy was recognized not only by longtime analysts of the Soviet economy who were outside government in 1979 but also by current U.S. government officials. For example, in testimony to the U.S. Senate's Committee on Armed Services during January 1979, General David Jones, Chairman of the Joint Chiefs of Staff, made what was by then an uncontroversial statement of fact: "The Soviets have some very severe problems. They are clearly not an economic superpower."[92]

The position expounded in the 1950s and 1960s by Friedman in articles and talks, and by his student Warren Nutter in much research—that the Soviet command economy was fundamentally weak—was, therefore, very commonly held in the United States and in the wider Western world by the end of the 1970s. Reflecting this change, the Friedmans, when writing the book version of *Free To Choose*, had no reason to refer to Soviet economic performance as other than an example of the negative results that could be expected to result from large-scale preemption by the government of market mechanisms.

Nutter himself did not live to see the full extent to which his and Friedman's position on the

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[90] See the discussion in Chapter 7 above.
[91] From Ball's testimony of July 19, 1979, in Committee on Foreign Relations, U.S. Senate (1979, p. 395).
[92] From Jones' testimony of January 25, 1979, in Committee on Armed Services, U.S. Senate (1979, p. 35)

USSR economy had been borne out in U.S. discussions by the end of the 1970s. He died of cancer on January 15, 1979, ten days before the testimony by General Jones quoted above (*New York Times*, January 16, 1979).

Return to double-digit inflation

One of the prominent fears expressed in U.S. public debate in the late 1950s—of being superseded economically by the Soviet Union—had therefore proved groundless, and twenty years later it had been completely discarded. But one could not say the same thing about another fear prevalent in that late-Fifties era: of an impending eruption of inflation. The fear was indeed overplayed in 1958–1959, and, to Friedman's dismay, prompted what was likely an unsound monetary policy tightening in that period, as the Federal Reserve based its actions on a misguided assessment of the amount of inflationary momentum present in the U.S. economy. But, by the late 1970s, the United States had experienced the reality for several years of high inflation—and at rates generally far in excess of those that had been merely feared in the late 1950s.

Furthermore, over 1979, the inflation problem got considerably worse. Federal Reserve chairman Miller had remarked on October 3, 1978: "I don't accept the idea that we have an underlying inflation rate of seven to eight percent. That is our current inflation rate, but I believe that the trend and the actions that have already been taken will begin to bring that down." After stressing recent movements in the exchange rate and in domestic costs as forces responsible for the current rate of inflation, Miller pointed to "the prospects for reducing inflation by 1 to 2 percent next year [that is, 1979]."[93] In contrast, Friedman's prediction about 1979's inflation rate, made in a television appearance the following month, went in the opposite direction: "inflation will get over 10 percent next year."[94] Incoming inflation data soon vindicated this prediction and confounded Miller's.

The twelve-month percentage change in the consumer price index had already reached 9 percent in December 1978. As reports at the time stressed, 9 percent rate amounted to the second-worst rate with which the United States had closed any year since 1947—being behind only the December 1974 reading (*Financial Times* (London), January 25, 1979) The Associated Press report on the day of the CPI announcement tried to put things in a somewhat positive light: "the

[93] In American Productivity Center (1978, p. 33).
[94] *Meet the Press*, NBC, November 12, 1978, p. 9 of transcript.

inflation picture improved in the last two months of the year," it observed, while adding: "The administration is predicting a 7.4 percent rate for 1979." (*Kansas City Star* (Missouri), January 24, 1979.) But inflation proceeded to get much worse in 1979. On a twelve-month basis, CPI inflation rose above 10 percent in March 1979. It had teetered on the brink of 10 percent in the February figures, which had had a twelve-month increase of 9.9 percent. "Look in the sky, it's 15% inflation!," one headline had read upon the release of the February CPI data (*Daily News* (New York), March 24, 1979). That 15 percent figure had been arrived at by a procedure Friedman usually deplored—of taking a one-month increase in the CPI and annualizing it—in this case, to reach 15.4 percent.[95] But the procedure did legitimately communicate the fact that the late-1978 slowing in monthly rates of price increase had been ephemeral. Buoyed by such high rates of monthly increase, the twelve-month CPI inflation rate stayed above 10 percent after March 1979, and it closed the year at 13.3 percent, a new post-1946 high for the end-of-year inflation rate (*The Oshkosh Northwestern* (Wisconsin), January 25, 1980; *The Sun* (Baltimore), January 26, 1980).

Inflation had already reached nearly 12 percent when Charles H. Brunie, a friend of Friedman's who employed him as a consultant to his asset-management firm Oppenheimer and Company, was moved to join a debate being played out in the pages of the *Wall Street Journal*. Brunie wrote that "it was particularly disturbing to read [econometric modeler] Michael Evans' comment (Letters, Aug. 30 [1979]) that no one correctly forecast inflation for 1978 and 1979. He's wrong, because Milton Friedman did." As well as recalling forecasts that Friedman had made in 1976 and 1977 of double-digit inflation at decade's end, Brunie quoted Friedman saying in mid-1978 that "it would be a miracle if inflation peaked below 10 percent, and 10−12 percent or 10−13 percent would be more likely." (*Wall Street Journal*, October 1, 1979.)

Recognition of the modern inflation epoch

The revival of U.S. inflation, after its mid-decade decline, to a new postwar peak in 1979 meant that the label Walter Heller had attached in 1976 to the experience so far—"The Great Inflation

[95] Expression of monthly or quarterly rates of change in annualized form was a much more prevalent practice in U.S. economic discussion than it was, at this point, elsewhere. Friedman had expressed reservations about what he called "the newspaper discussion of prices" in which "everybody rapidly multiplies [a newly released monthly increase] by twelve" (Friedman, 1971h, p. 9), though, of course, he occasionally engaged in similar practices himself. Paul Samuelson, in the same vein, had referred to the "foolish people who multiply a one-month figure by twelve" (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 136, September 6, 1973; see also Tape 119, January 12, 1973, in the same series).

of the 1970s"—proved to be a more than apt description of the whole decade's experience. Friedman had foreshadowed this eventuality in February 1974 when he remarked in a talk at Harding College, Arkansas, that a "Great Inflation," not a Great Depression, was what the United States would likely face in the years ahead (*Arkansas Democrat* (Little Rock), February 22, 1974).

Walter Heller would reuse the term "The Great Inflation of the 1970s" in an early 1979 piece he wrote with George Perry for a bank newsletter in Heller's home city of Minneapolis (*Minneapolis Tribune*, January 19, 1979). Others, too, were likewise thinking of the 1970s period as reflective of a new epoch. But whereas Heller, Perry, and other Keynesians were inclined to think of the new situation as mainly reflecting a greater tendency for inflation to have momentum of its own separate from the settings of aggregate-demand policy. Friedman, in contrast, was inclined to view the United States' recurrent high inflation in terms of a change in the nation's monetary policy to a different regime during the 1960s and 1970s.

One of the ways in which Friedman looked at the modern policy regime was via the perspective of time-series analysis. Here, he drew particularly on work published in the middle of the decade by his former student Benjamin Klein, who had regarded the United States as having in effect adopted in recent decades "a new monetary standard" (Klein, 1975a). In documenting this point, Klein (1975b) had focused on the univariate time-series properties of U.S. inflation in different historical periods—including a post-Korean War sample that included the years of rising inflation after 1964 and that culminated in the major breakout year of 1973.

The rise in inflation in the 1964–1973 decade, together with the inflation observations recorded for the years of the 1970s beyond Klein's sample, forced a rethinking of the basic time-series properties of U.S. prices and inflation. For one thing, a fact already discernible on the basis of the data for the 1950s and 1960s was now irrefutably clear: the price level was clearly nonstationary. The gentle price rise of the price level in the decade following the Korean War, together with the more rapid pace of increase since the 1960s, already constituted sizable evidence against the presumption that the price level was stationary. Remnants of the notion that there was some mean-reverting tendency in the price level (or, more plausibly, a reversion to a mild linear trend), were, however, [present in some of Friedman's commentary and research in the late 1950s and even into the late 1960s (see Nelson, 2020a, Chapter 10). But, by the mid-1970s, he had been disabused of that notion. In June 1976, for example, Friedman observed that, in contrast to past eras, "there's no relationship between the price level and the interest rate," and

in particular, "the price level in the United States is something like 60 percent or 70 percent above where it was in 1969," and short-term nominal interest rates were not.[96] By 1979, the manifest nonstationarity of the price level had seeped so much into U.S. discussion that it was mentioned in fictional television programs. When a character in the network drama series *Eight Is Enough*, Nancy Bradford, was employed in the financial markets, one of her lines of dialogue in an episode broadcast in November 1979: "Inflation has caused the average price level to more than double in the last ten years."[97]

In the period with which Klein was concerned, and even more so in other research completed by 1979, the key question about univariate U.S. inflation dynamics had, instead, become whether even the inflation rate itself was stationary. On the basis of estimation using a sample ending in July 1971, C. Nelson and Schwert (1977, p. 481) had conjectured that it was not, stating had argued that "the autocorrelation structure of the CPI monthly inflation series… suggests that the series may reasonably be represented as a first-order moving average process in its first differences, implying nonstationary behavior in the rate of inflation." As high-inflation observations accumulated in the United States over the 1970s, the case for representing inflation as an I(1) (nonstationary in levels) time series, such as a random walk or some other form of ARIMA (autoregressive integrated moving average) process, built up. Klein (1975b, pp. 134−135), using annual data, found that the random walk was a good approximation of annual inflation dynamics in the 1956–1973 period. The actual first-order correlation that he reported for inflation (0.75) indicated, however, that, as yet, the basis for treating inflation as stationary remained quite sound. That basis eroded as data for 1973−1979 were recorded. McCallum (1994, p. 235), for example, showed that the sum of coefficients in an autoregressive representation for post-Korean War quarterly inflation was only once above 0.75 in successive sample periods ending each year from 1966 to 1972, but that the sum was consistently above 0.85 on samples ending in any year from 1973 to 1980.

By the end of the 1970s, advances in time series analysis allowed much more formal means of testing whether a series was nonstationary (specifically, an I(1) series that was stationary in its first differences) was available in the work of Fuller (1976) and Dickey and Fuller (1979). It would turn out that results obtained using these formal tests also implied that a random walk, or

[96] Friedman (1976i, p. 9).
[97] *Eight Is Enough*, ABC, November 21, 1979. This line of dialogue reflected a minor, and accurate, element of forecasting on the part of the program's scriptwriters. The statement that the price level had more than doubled in the past ten years was not quite true in the spring-summer 1979 period during which the program was likely filmed. But it had become true by the time of broadcast.

perhaps an augmentation of this difference-stationary representation to allow for multiple lags in the autoregressive specification of inflation, was a good approximation of the U.S. inflation process.[98]

In *Monetary Trends*, published in 1982, Friedman and Schwartz remarked of the modern U.S. regime: "There is no widespread anticipation that the price level will revert to any 'normal' level; it (or perhaps its derivative) is now nearly of the nature of a random walk, perhaps with drift."[99] The hypothesis embedded in the latter part of this statement—that postwar U.S. inflation might be a random walk and, even more so, the additional stipulation that it was a random walk *with drift* (the drift, in this instance, being positive—that is, an upward trend)—was not really very well established at the time, in 1979−1980, when Friedman and Schwartz wrote the quoted words.[100] They were making their description of the time-series properties of U.S. inflation largely on the basis of Klein's 1975 work, possibly combined with perhaps Friedman's empirical generalization that U.S. inflation's successive peaks and troughs had been progressively higher in each inflation "cycle" since 1960 (see, for example, Instructional Dynamics Economics Cassette Tape 204, December 1976, Part 1). But they were not making their statement on the basis of econometric analysis of the late-1970s U.S. inflation data, and they certainly had arrived at it on the basis of the formal statistical tests emerging in the recent literature.

Notwithstanding the tentative foundations for Friedman and Schwartz's statement, it would prove prescient, once the data for the late 1970s and for 1980 reinforced and confirmed the evidence of a random-walk inflation series. And this was even more so once econometric research such as Shapiro and Watson (1988, pp. 122−123) and King, Plosser, Stock, and Watson (1991, p. 825), making use of the Dickey-Fuller statistical tests, had suggested that U.S. inflation might well be difference-stationary (and possibly, on samples ending around 1980, also had drift—a positive mean on the dimension of its first difference) rather than itself being covariance stationary.

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[98] The use of the Dickey-Fuller tests in macroeconomics had been pioneered by C. Nelson and Plosser (1982). The research for this 1982 study started in 1979, when the authors received a National Science Foundation grant for the underlying project (see C. Nelson and Plosser, 1982, p. 139). A working-paper version of the paper was issued in August 1980 (see Darby, 1983, p. 476). Nelson and Plosser's sample period ended in 1970 and, unlike later authors, they did not consider the post-World War II period in isolation or test for the stationarity of inflation (as distinct from the log price level).

[99] Friedman and Schwartz (1982, p. 570).

[100] Friedman and Schwartz were, however, not correct in implying that it was only "possibly" the case that the random-walk behavior of the (log) price level had a drift component.

Warning of recession

For Friedman, the move of inflation into double digits, 1979 provided further confirmation that U.S. monetary policy in prior years had been too loose. The *current* stance of monetary policy, however, was another matter. In his first *Newsweek* column for monetary policy in early 1979, titled "The Fed: At It Again" (February 19, 1979), Friedman's criticism of the Federal Reserve centered on the indictment that its stance was now, in fact, too tight.

The Federal Reserve tightening that had occurred during 1978 had led, by the end of the year, to a major shift down in the annualized rates of growth of M2 in the low single digits. At one time, Milton Friedman would have been angry or mystified in the face of a severe lurch of this kind. By early 1979, however, he was accustomed to such swings, even in an era in which the Federal Reserve paid increased attention to monetary growth.

And so it was with a tone of world-weariness that Friedman prefaced his column's analysis. Friedman began by noting that his monetary policy commentaries in *Newsweek* had had "a cyclical pattern." They had predominantly consisted of taking the Federal Reserve to task for policies that produced excessive monetary growth. But, he added, in the remainder of the time, the Federal Reserve did exercise restraint, only to go too far in that direction and raise the risk of an unnecessarily severe recession. On these latter occasions, "I find myself criticizing the Fed for increasing the quantity of money too slowly." The time had arrived, Friedman explained, for him to shift to that line of criticism once more.

What had prompted this commentary was a decline in M2 growth from its double-digit average rate before 1978 to what Friedman reported as a 3.3 percent annualized rate since September 1978.[101] The latter rate, Friedman said, "is fine as an ultimate target."[102] But the Federal Reserve's tightening had, in the last few months of 1978, moved M2 growth so quickly to this

[101] On quarterly and somewhat revised data, the weakness in M2 would mainly be recorded somewhat later—occurring in the first quarter of 1979. In particular, the tabulations in Simpson (1980, p. 113) showed that weakness in monetary growth was less pronounced but visible for the modern definition of M2 than for the old definition of M2 (which was still the official series in early 1979). The modern M2 series has annualized growth of 6 percent in 1979:Q1—above the annualized rate that Friedman reported for old M2 for late 1978/early 1979, but lower than in any quarter since 1974.
[102] *Newsweek*, February 19, 1979. Friedman's endorsement of 3 percent (rather than 4 percent) as the appropriate long-run rate of M2 growth was, apparently, an early sign of his acceptance of the changes made in early 1979 to the official estimate of potential output growth. These revisions, made by the Carter Administration, were discussed above.

low rate that Friedman felt that a U.S. recession was now a serious danger. "They haven't been doing it gradually; they've been hitting us over the head with a blunt hammer," he observed in an interview, given in early February at his office at the Hoover Institution, that appeared at the same time as the column (*San Jose Mercury News* (California), February 13, 1979).
"*Goodbye M1*"?

Although Friedman had based his discussion entirely on M2 behavior, the more widely-watched M1 aggregate seemed to corroborate his diagnosis of a shift toward stringent monetary conditions. Indeed, M1, as defined as the time, registered an absolute decline in the early months of 1979 (*Financial Times* (London), August 6, 1979; Simpson, 1980, p. 112). Friedman, however, did not cite M1 behavior in support of the judgment concerning monetary conditions that he expressed in the February 1979 *Newsweek* column. Instead, he discounted M1's signal completely.

Friedman's basis for doing so was closely related to a factor that would be highly relevant for the analysis of M1 for the decades after the 1970s. In late 1978, federal banking regulators considerably relaxed the conditions under which commercial banks could allow no-penalty transfers of their customers' deposit balances from interest-earning time deposits (included in M2 alone) when a top-up was needed of the same customers' noninterest-bearing demand accounts, which were included in M1 (see Cagan, 1979, p. 123). For Friedman, the change meant that the demand-deposit/time-deposit distinction was more tenuous than it had been at any point since the 1930s. The exclusion of time deposits from the definition of money, and the consequent focus on M1 rather than M2 in monetary analysis, constituted a practice that he and Schwartz had already largely eschewed in their coauthored work. But in 1978–1979 Friedman further argued that the late-1978 institutional change should make U.S. monetary analysts, who tended to favor M1, focus squarely on M2 (*Newsweek*, October 30, 1978, see also *Newsweek*, February 19, 1979). M1 would be a "nearly useless aggregate" from now on, Friedman declared, and his column discussing the regulatory change included a subtitle "GOODBYE M1" (*Newsweek*, October 30, 1978).

This drastic conclusion would be tempered, however, over the course of 1979. Legal challenges, launched by thrift institutions, to the 1978 regulatory changes put a brake, for the moment, on commercial banks' scope to routinize the conversion of demand deposits into time deposits (*Kansas City Star* (Missouri), April 28, 1979; Cagan, 1979, p. 123).

Furthermore, the reliability of M1 would be bolstered somewhat by the Federal Reserve's broadening of the definition of M1 in 1979−1980, discussed in a later chapter. This redefinition removed one major source of outflows from M1 in the late 1970s: the shift out of commercial bank demand deposits into the essentially checkable accounts—Negotiable Order of Withdrawal (NOW) accounts—that had increasingly been offered by thrift institutions during the 1970s. With NOWs included in the revised definition, the decline in M1 registered in early 1979 was not a feature of the revised series, and M1 behavior became closer to that of M2 for that period.[103]

The fact that the change in M1 definition brought M1 and M2 into better alignment also suggests that, in 1978 and 1979, Friedman may have overestimated the role of banks' automatic transfer services as a factor distorting incoming M1 data in that period. Because the M1 redefinition eliminated some of the more anomalous swings recorded in incoming M1 data over 1978 and 1979, it is likely that the main culprit driving these swings did not consist of the automatic-transfer services—which shifted funds across bank-deposit classes but kept those funds in the commercial banking system—but the growing popularity of NOW accounts, which were outside the old definitions of both M1 and M2 and amounted to nonbank depository institutions' liabilities. But the day would come when automatic transfers were a very important factor indeed making for distortions in the M1 series, even in its modern definition. Automatic transfers continued to be practiced in some form by commercial banks from 1979 onward, but in that year, and into the 1980s, they largely consisted of services provided to large-balance customers and were predominantly transfers from retail-deposit accounts to wholesale-deposit accounts.[104] The effects of these practices on the behavior of M1 and M2, though not negligible, were limited in this era.

In the 1990s, however, automatic transfers, known as "sweeps," became prevalent on a large scale as a means by which commercial banks handled their retail customers' accounts, including non-business customers, and would be used by banks to re-weight their deposit liabilities away from demand deposits (which remained, at that time, subject to reserve requirements) and toward time deposits (which were free of reserve requirements starting in 1991). "Sweeps," or automatic transfers, would indeed come to dominate and distort M1 behavior in the 1990s and

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[103] See Simpson (1980, pp. 112−113).

[104] For their high-balance customers, commercial banks around 1979 were showing a tendency to make automatic transfers of demand deposits to interest-earning accounts, in this case shifting the funds to wholesale instruments like bank-issued repurchase agreements and Eurodollar accounts. See Whitesell and Collins (1996, p. 2).

beyond (R.G. Anderson, 1995; Cynamon, Dutkowsky, and Jones, 2006). These would eventually make M2 a clearly preferable measure of money to M1. This outcome was just as Friedman had anticipated in 1978–1979, but much further in the future than he had thought.

In any event, it was on M2 that Friedman focused squarely in making his judgment that the FOMC had tightened too much. In his *Newsweek* column, he left open the possibility that recession could be avoided: he hoped that the period of M2 growth would not last "too long" and that the Federal Reserve "promptly end its overrestrictive policy" (*Newsweek*, February 19, 1979). In his press interview given around the same time, however, Friedman was more categorical about what would flow from the policy actions already taken. "We're going to have a recession [in] the middle of this year," he remarked (*San Jose Mercury News* (California), February 13, 1979).

The U.S. Department of Commerce indicated its own vote of confidence in M2 when, effective starting March 1979, it replaced real M1 with real M2 in the Index of Leading Indicators that it reported each month (Laurent, 1993, p. 11). The overall readings of the index during this period supported Friedman's warning of a recession. Arthur Burns was not impressed by the signal, telling a reporter that the index of leading indicators was "a very poor index right now" as it was "heavily influenced by the money supply statistics, which should never have been in it." (*Newsweek*, May 14, 1979.)[105]

Intramural disputes among monetarists

More surprisingly, Friedman's assessment of early-1979 monetary conditions was disputed by fellow monetarists. The picture of restriction that M1 and M2 alike provided in this period did not prevent a public flare-up among monetarists about judging policy stance. The reason for the different judgments was found in the behavior of the monetary base. The base continued to grow strongly into the new year, and the Shadow Open Market Committee (SOMC), whose members included Karl Brunner, Allan Meltzer, and Anna Schwartz, saw base growth as providing, at this time, the best indication of monetary growth among the various aggregates. Indeed, in late November 1978, Brunner had argued that it might be preferable to conduct aggregate monetary

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[105] As mentioned in Chapter 5, money was added to the index in 1975. The 1979 replacement of M1 in the M2 in the index may have been partially influenced by Friedman's warnings that automatic transfers would critically distort the behavior of M1 starting in late 1978. Real M2 remained one of the twelve series in the index of leading indicators (which, in more recent years, has been collected by the Conference Board) until 2011.

analysis from this point on in terms of the base, especially until the definitions of broader monetary series were improved to handle recent financial innovations.[106]

In light of the behavior of the base up to January 1979, Brunner rejected Friedman's suggestion that monetary policy had lurched into tightness. Striking public dissent from Friedman's analysis came from Anna Schwartz, who sided with her SOMC colleagues on the dispute and was quoted in the business press saying: "We think Milton overreacted." (*Business Week*, April 2, 1979, p. 17.)

Friedman stuck to his position as generally weak data on monetary growth continued to come in during the first half of 1979. Monetary growth "has slowed to such an extent that even Milton Friedman has called for some monetary relaxation," it was observed in spring (*Investor's Chronicle* (London), May 4, 1979), and in June 1979 Friedman told a meeting in Washington, D.C., of the National Federation of Independent Business: "We are, at the moment, poised on the edge of a recession." (*American Banker*, June 12, 1979, p. 3.)

As Friedman's position hardened, the disagreement with other monetarists receded somewhat. Monetary base growth came closer into line with M2 growth, and Karl Brunner would view the six months to April 1979 as one characterized by restrained base growth (see Brunner, 1980b, p. 18). By mid-1979, both Brunner and Allan Meltzer took the position that monetary policy had indeed become appreciably more restrictive, even though they did not share Friedman's assessment of the sheer magnitude of the tightening in the past nine months (*Wall Street Journal*, June 19, 1979).

G. William Miller and the recession warnings

Federal Reserve Chairman Miller was long familiar with Friedman's warnings of recession by this point. In February, after Friedman's *Newsweek* column appeared, Senator Orrin Hatch (R–UT) had used it as the basis for written questions to Miller. Senator Hatch stated:

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[106] See Brunner (1979, p. 38). Brunner here (as well as elsewhere: see Nelson, 2019, for a discussion) argued that the base, as a series that and excluded commercial bank deposit liabilities and (at this time) had no interest-bearing components, was insulated from financial innovations that bore on bank deposit liabilities, such as switches between demand deposits and time deposits and changes in arrangements concerning the payment of interest on various classes of bank deposits. This was an argument for which Friedman had shown some sympathy: see Chapter 8 above.

> Many economic observers, although pleased that the Federal Reserve has decided to restrain the growth of the money supply, are concerned that the Fed is reducing the rate of money growth too rapidly. Milton Friedman, one of the few economists to foresee our present accelerating inflation and predict last year's high inflation rate, has become critical of the Fed for reducing money supply growth too drastically. He argues that the Fed is putting us back on the roller coaster of inflation, recession, and more inflation.[107]

Miller made no reference to Friedman in his reply and cast doubt on the significance of the money data (including M2) before concluding: "The Federal Reserve believes that the deceleration implied by its current longer-run growth ranges for the monetary aggregates is consistent with the avoidance of recession."[108]

As he was making this observation, Miller was entering the most controversial period of his time as chair of the Federal Reserve. This was the brief, but much-remembered, time in spring 1979 when Miller led the Federal Open Market Committee majority vote against proceeding with rapid increases in the federal funds rate.[109] In doing so, Miller resisted pressure coming from the Carter administration to expedite tightening, presided during couple of dissenting votes by his deputy on the FOMC, Paul Volcker, and greatly fostered criticism of his leadership and judgment in financial and media commentary.

As it happened, however, Miller's posture during this episode was broadly consistent with Friedman's judgment that U.S. monetary policy had already tightened too rapidly and that steps to retrace tightening (which, in terms of interest-rate policy, might mean a pause on rate increases) were warranted. Indeed, *Business Week* implied that Miller might have been paying more attention to Friedman's warnings than Miller's public statements were acknowledging. In trying to bolster monetary growth in the spring of 1979 in order to avoid recession, the magazine commented, "he is behaving like a monetarist economist who sleeps with *A Monetary History of the United States* under his pillow." (*Business Week*, May 7, 1979, p. 24.) This characterization, of course, overdid Miller's alignment with the monetarist position. But Miller did seem to share with Friedman the contention that holding back on further tightening moves might be more consistent with the stated policy aim of an economic slowdown, but not a recession, during 1979.

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[107] In Committee on the Budget, U.S. Senate (1979, pp. 204–205).
[108] In Committee on the Budget, U.S. Senate (1979, p. 205).
[109] See the discussion titled "Paul Volcker" in Section III below.

Such a slowdown, in turn, was viewed as a desirable development. This was especially the case in light of the Carter Administration's 1979 revisions, already discussed, to potential output. These revisions heightened the likelihood that aggregate output was poised to overshoot its full-employment value and reinforced the need to moderate economic expansion for the time being. Chairman Miller frankly described the current policy strategy when, during an appearance in St. Louis in mid-June 1979, he observed that monetary restraint was being applied "to reduce the real rate of growth of the economy" for disinflationary purposes (quoted in *St. Louis Globe-Democrat*, June 15, 1979).

As it happened, the U.S. economy did avoid a recession in 1979. Explaining why this had happened, Friedman would cite the fact that monetary growth picked up after the first quarter of 1979, allowing an outcome in which "instead of a continued recession that everyone had been forecasting, the economy turned around" (*Business Week*, May 12, 1980, p. 16).[110] This would, however, prove to be only a narrow, and short-lived, escape: the U.S. economy entered recession very soon after calendar 1979, reaching its cyclical peak in January 1980. By then, Friedman's fears of recession had become widely shared. The position that the U.S. economy would avoid a recession altogether had been held at the official level—by the Federal Reserve Board and the Carter Administration—in the first half of 1979. But that expectation had then been widely revised inside and outside officialdom, in light of two dramatic further events, discussed in the next two sections: the tightening of monetary policy under Miller's successor Paul Volcker; and the second oil shock.

II. ISSUES AND ACTIVITIES, 1979

THE SECOND OIL SHOCK

Friedman would have occasion to refer to "the fall of the shah in 1978" (*Newsweek*, September 15, 1980) when discussing the role that events in Iran had played in shaping the course of oil prices in 1979.[111] His dating was not precisely correct. Although the revolution in Iran was very much underway during late 1978, the shah did not leave Iran—in effect, conceding his loss of office—until January 16, 1979, with Ayatollah Khomeini then returning to the country in

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[110] Friedman's reference to a "continued" recession here apparently was a reference to the weak real output growth in the first half of 1979. Initial estimates of second-quarter 1979 economic growth were negative (*Financial Times* (London), August 6, 1979). And even the modern, revised real GDP data record growth in the first half of 1979 as very low: increases of 0.7 percent and 0.4 percent at annual rates in 1979:Q1 and 1979:Q2, respectively.
[111] He would reuse this phrase in *Newsweek*, March 21, 1983.

February 1979, in order to take power (Cook, 1979, p. A45). Nonetheless, Friedman's basic position—that the Iranian revolution would give rise to oil price "levels that only a few years earlier would have been regarded as impossible" (*Newsweek*, March 21, 1983)—was sound. By the end of 1979, OPEC had ratified what was, in essence, a doubling of the world oil price—a doubling that was applied to the already high level established by events earlier in the decade.

The extent to which developments in Iran came as a surprise to the outside world was underlined by the fact that it was not until January 3, 1979, that the U.K. government officially canceled a visit to Iran by the Queen that had been scheduled to take place during the year (Cook, 1979, p. A45). Likewise, John F. O'Leary, the Deputy Secretary of the Department of Energy, observed "I did not predict Iran," when he was asked, in May 1979, to explain why the U.S. government had, until recently, been predicating its policies on an assumption of unchanged or declining real oil prices.[112] Friedman himself would give the fact that "I did not foresee the revolution in Iran" (*Newsweek*, March 21, 1983) as the reason why his longstanding prediction of a major decline in oil prices was not finally realized at the end of the 1970s.

Instead, oil prices exhibited a strong pickup over the course of 1979. The first couple of months of the year saw what O'Leary referred to as the "Iranian outage"—major interruptions of production of oil and large-scale suspension of exports of oil in Iran during the revolution period and its immediate aftermath.[113] Before the revolution, Iran produced about 6.5 million barrels of oil per day and exported about 5.5 million of it, and, of this, about 900,000 barrels went to the United States (*Rockford Register Star* (Illinois), February 15, 1979*a*, p. A10). The period of revolution was accompanied by a large decline in production and, in particular, a dearth of oil for export, implying what Friedman called a "near-cessation in the flow of oil from Iran" (*Newsweek*, March 21, 1983).

Reflecting the revolution and actions by OPEC as a whole, the world oil price registered a major increase. OPEC had already announced, in mid-December 1978, a planned rise in the oil price of 14.5 percent to be implemented over the course of 1979 (Federal Reserve Board, 1979, p. 202). This decision showed up in an oil-price increase early in the year—to $13.34 per barrel at

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[112] From O'Leary's testimony of May 16, 1979, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 191). On his reference to pre-1979 declines in real oil prices, see page 190 of O'Leary's testimony, in which he had cited a 20 percent decline in the world market's real price of oil in 1977–1978. Friedman would likewise cite the real price decline in the world oil price of "about 25 percent" from 1974 to 1978 (*Newsweek*, September 15, 1980).

[113] From O'Leary's testimony of May 16, 1979, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 190).

the end of March 1979. By mid-year, however, the increase in oil prices had become sharper than envisioned by OPEC the previous December, with the world price standing at $14.54 (International Monetary Fund, 1981b, p. 145). And OPEC had a meeting scheduled for late June for a further reevaluation of its pricing policy.

And, because, even under the regime of controls on prices of American oil production, U.S. petroleum prices were sensitive on the margin to changes in the world oil price, gasoline prices shot up. They stood at about 65 cents per gallon in mid-February (*Rockford Register-Star* (Illinois), February 15, 1979*a*).

The rises in prices of petroleum products in the United States were judged in some quarters to be out of proportion to the actual reduction in supply implied by the Iranian outage and other international developments (see, for example, *San Jose Mercury* (California), August 13, 1979). Friedman attributed the extra-large price response to "a major increase in demand" by users in a panicky reaction to the prospect of a longer-lasting disruption to supply (*Newsweek*, September 15, 1980).

There were, nevertheless, some hopes that the Iranian revolution would prove to have only very transitory effects on the supply and price of oil alike. For example, Philip Caldwell, president of Ford Motor Company, implied in remarks in mid-February that the current disruption to oil supply was temporary in nature (*Rockford Register Star* (Illinois), February 15, 1979*b*).[114]

But, just as, in 1974, the passing of the oil embargo—the previous episode of a major physical cutoff for the United States of oil imports—did not see the 1973−1974 oil price hike wound back, the return of more-normal oil-industry conditions in Iran after February 1979 did not see the 1979 price pressure reversed. Some indication that this would be the case occurred in February 1979 when, in testimony given by U.S. government officials, it was suggested that, when Iran oil imports resumed, they would be at a lower level, as a deliberate policy choice of

[114] Philip Caldwell, the Ford Motor Company executive, should not be confused with Philip E. Coldwell, who was a member of the Federal Reserve Board from 1974 to 1980 after having served in various positions (including president, 1968−1974) at the Federal Reserve Bank of Dallas. Both these individuals would eventually be at odds with Friedman in print: Friedman would criticize the views on trade expressed by Ford's Philip Caldwell (*National Review*, April 6, 1984, p. 44), while Philip E. Coldwell (who had by then left the Federal Reserve, after having attended well over 200 meetings of the Federal Open Market Committee from March 1957 to February 1980) would criticize Friedman's interpretation of and recommendations concerning U.S. monetary policy—while also daring Friedman to take a seat on the Federal Reserve Board instead of criticizing the performance of those who had previously accepted such appointments (see *Wall Street Journal*, February 9, 1981).

the new government (*Rockford Register Star* (Illinois), February 15, 1979*a*). Consistent with this prediction, as of late May 1979 Iranian oil exports, which had resumed, were 1.6 million barrels per day below their pre-revolution volumes.[115] Furthermore, there was prominent speculation that Iran might well hold back on restoring former levels of exports and that its ultimate setting of oil production would be made in coordination with the outcome of OPEC's end-June 1979 meeting on oil prices.[116]

President Carter embraces domestic price decontrol

The new developments in the market for world oil occurred in a situation in which U.S. domestic oil and retail gasoline prices had still not fully adjusted to the original oil shock in 1973−1974. Friedman had declared in August 1978 that the United States was in need of "a truly fundamental energy reform, which eliminated all controls on prices as well as our present subsidies [to] oil imports, which abolished the Department of Energy, enabling the market to go to work to produce, price, and distribute the energy we demand." In conjunction with reforms in other aspects of economic policy, this move, Friedman suggested, would "demonstrate to the world that we were awakening from our drugged state."[117] Nevertheless, as indicated in the previous chapter, he remained doubtful that U.S. oil prices would ever, in fact, be decontrolled.

A different outlook for energy policy was expressed in the television drama series *Dallas*. In the episode broadcast on U.S. network television on February 23, 1979, oil magnate (and lead character) J.R. Ewing stated: "I'm telling you—it is just a matter of time before the government lifts those price controls." Just under six weeks later, this prediction, articulated in the fictional realm, was borne out in the real world when, in a televised national address, President Carter announced that he would be decontrolling oil prices (see Carter, 1979b).

The Carter Administration's longstanding aim had been that the prices facing U.S. purchasers of crude oil and retail gasoline should fully reflect the increase in oil prices that had occurred since 1973. It was not, however, until the announcement in Carter's April 1979 address that U.S.

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[115] See the submission of Erskine N. White, Jr., on behalf of the National Association of Manufacturers, for the hearing of May 31, 1979, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 562).
[116] See the remarks of Representative Clarence Brown during the hearing of May 31, 1979, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 498).
[117] Letter of August 22, 1978, to Senator Clifford P. Hansen, published in *Congressional Record—Senate*, September 11, 1978, p. 28645.

government policy in implementing this aim was to allow prices to be market-determined. Previously, the administration had merely been seeking that the prices faced by petroleum users be made equal to market values. In particular, through its Crude Oil Equalization Tax, or COET, proposal of 1977−1978 (discussed in the previous chapter), the administration had sought to raise the post-tax U.S. oil price to world levels—but to do so, not through actual decontrol of the pre-tax price, but by maintaining price controls, while using taxes to raise the price actually paid by purchasers. Although, under legislation passed in 1975, the United States' existing oil price controls were set to expire in 1981, the administration had, in the COET plan, proposed to supersede this legislation and to make oil price controls permanent. Then, as it became clearer during 1978 that the COET would fail to get U.S. Senate approval, the administration explored alternatives to decontrol and continued to do so into the first three months of 1979.[118] For example, the administration explored alternative means of raising petroleum prices, such as through a steep increase in the sales tax applying to gasoline (*Great Falls Tribune* (Montana), February 24, 1980). Such deliberations were testament to the administration's desire to make home petroleum prices more comparable to world levels through instituting taxes rather than through abolition of price control.[119]

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[118] A contrary position has been taken by Eizenstat (2018, p. 212), who claims that it was at the Bonn economic summit of July 1978 that the Carter Administration committed itself to decontrol by agreeing to language in the summit communique "committing the U.S. government to lift controls on oil prices by the end of 1980" (p. 212). (Meltzer, 2009b, p. 945, similarly claimed, without documentation: "At the Bonn summit in July 1978, President Carter had agreed to decontrol domestic oil prices…") In fact, the summit communique did not make this commitment, and Carter's decontrol announcement in 1979 resulted from a distinct decision process. What Carter affirmed in 1978 was that, even if the COET plan failed, he would maintain the administration's efforts to seek a rise in U.S. oil prices to world levels. In particular, the July 1978 summit led to a communique statement: "the U.S. remains determined that the prices paid for oil in the U.S. shall be raised to the world level by 1980." (Quoted in Hakes, 2021, p. 240.) Note the word "remains"—confirming that this was *not* an announcement of a new policy of the U.S. government with regard to domestic pricing. Indeed, as Secretary of Energy James Schlesinger testified on April 24, 1979: "There has been a commitment by the administration for two years, and more, to move to world oil prices." Note also the communique's reference to prices being "raised" but not necessarily decontrolled. Schlesinger observed that, when the administration's existing policy was reaffirmed in the "Bonn commitment to raise [U.S.] energy prices to world levels," it was still "hoped that that could be done through the crude oil equalization tax." (The Schlesinger quotations are from Committee on Interstate and Foreign Commerce, U.S. House of Representatives, 1979, p. 27.) See also the editorial in the *Washington Post* (July 23, 1978), which similarly made the points that President Carter had long been committed to bringing U.S. oil prices up to the world level but still had not committed himself to doing so via decontrol instead of by some form of taxes (including tariffs, which he was able to institute under existing law).

[119] In a memorandum to Carter coauthored by Secretary of the Treasury Blumenthal on June 7, 1978, in which it was recommended that Carter make the pledge at Bonn to have oil prices raised even if the COET plan was not passed, the authors stated that it was "not essential to specify at the summit exactly what actions you would take" in place of COET and mentioned "fees and/or quotas" as an alternative option to decontrol that would bring home oil prices up (see Galpern, 2012, p. 489). And after the summit, a memo to the president of July 7, 1978, by Blumenthal, Charles Schultze, and some of Carter's foreign-relations personnel made it clear that a variety of options existed as means by which the Bonn summit pledge could be fulfilled, with decontrol being only one of these options (see Galpern, 2012, p. 494).

The administration's resistance to the decontrol option ended, however, with the April 1979 Carter speech, in which he noted that "the law gives me the authority at the end of next month to carry out this decontrol process," and he indicated that he would invoke that authority.

As the reference in the *Dallas* series indicated, there was much sympathy in the oil-oriented state of Texas for decontrol. Against this background, the financial columnist for the *Dallas Morning News* greeted Carter's announcement with a mixture of praise and sarcasm. Altwegg remarked that following the speech, "everyone was talking about what the president has had to say. After all, it preempted the first half of *Hawaii Five-O*, so a lot of people watched it." Altwegg welcomed the fact that "Mr. Carter has come round to the belief that price controls on crude oil should be phased out, or 'deregulated.'" This announcement, Altwegg observed, amounted to an endorsement of "what so many people have been saying for so long," but he implied that the Carter move was a case of the president changing his mind on the virtues of deregulated markets (*Dallas Morning News*, April 7, 1979).

This characterization was not altogether accurate, as Carter had, before April 1979, already been an enthusiast for deregulation in other sectors of the U.S. economy. It was nevertheless the case, however, that there was a qualitative difference between Carter's new acceptance of oil-price decontrol and most of his earlier endorsements of deregulation (such as in the area of airlines). Those earlier cases had usually been ones in which the immediate effect of deregulation would likely be lower prices faced by consumers. This was not the case for oil: although some, including Friedman, argued that oil-price decontrol would *ultimately* lower energy prices, it was widely accepted that the inevitable short-run effect of decontrol would be higher petroleum prices in the United States.

Consequently, whereas, according to the cost-push logic that the president typically applied to inflation analysis, other deregulation moves could be motivated as anti-inflation moves because they lowered prices in the deregulated sector, this could not be applied to the case of oil decontrol. For example, Carter would go on to state, on June 21, 1979, when advocating a trucking deregulation bill: "The best anti-inflation medicine, in my opinion, is real competition under the American free enterprise system."[120] This reasoning clearly did not apply in the short run to oil-price decontrol, even if one accepted (as Carter did, and Friedman did not) the cost-push reasoning that centered inflation-control policy on government attempts to influence

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[120] Carter (1979c).

individual categories of goods prices. Consequently, unlike the other major Carter deregulation initiatives, Carter's acceptance of oil-price control was, as discussed further below, was centered on the microeconomic arguments for deregulation and on the virtues of providing price incentives to producers.

In response to Carter's decontrol plans, Friedman made it clear that he was far from satisfied. Nearly six months after the Carter address, Friedman remarked: "Jimmy Carter has not been in favor of decontrol… [A]ll he has come out for is gradual decontrol." (*Today*, NBC, September 28, 1979.) The reason why Friedman at this late point to refer to Carter's move as an announced, rather than an actual, policy was because Carter's proposal was not only phased but very spread-out and not even starting in 1979. The main phased decontrol of old-oil prices was due to start on January 1, 1980, and to occur in a staggered fashion through the end of September 1981.[121]

This timetable was chosen despite the fact that Carter had the power to initiate general oil price decontrol at any date from June 1, 1979, onward. Indeed, he could make full decontrol instantaneous if he wished: "President Carter has the authority to decontrol the prices of crude oil and gasoline immediately," Friedman observed (*Newsweek*, July 30, 1979). Friedman had earlier noted that, in contrast to areas such as disinflation—with regard to which he did favor a gradual policy—he opposed gradualism in area of natural gas and oil price deregulation, as the price mechanism could not completely clear markets and send the proper signals to market participants unless there was a situation of no price control (*Reason* magazine, August 1977, p. 29).[122] In the aftermath of Carter's decontrol announcement, Friedman declared that the president should decontrol petroleum prices fully and instantly—and have the law changed to remove remaining controls on natural-gas prices as well (*Newsweek,* June 11, 1979).

Supply incentives recognized

One of the major reasons the Carter administration gave for the phased character of decontrol—that the president's anti-inflation program required a slower increase in energy prices than otherwise—would have left Friedman unimpressed, owing to its emphasis on specific price rises

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[121] See Carter (1979b) as well as James Schlesinger's testimony of April 24, 1979, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 20) and Charles Schultze's testimony of May 16, 1979, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 163).

[122] In the late spring of 1979, in the wake of the decontrol announcement, Friedman remarked: "Obviously, I welcome the decontrols, but all the rest of it is a disaster, including the gradual [implementation]… To really work, the program should call for immediate, complete decontrols." (*Expo*, Summer 1979, p. 18.)

as the source of inflation.[123] Friedman would, however, have reason to take satisfaction in the rationale that leading figures in the administration gave during 1979 when making the case for the overall policy of decontrol. Not only did they emphasize, as they had when selling the 1977 energy plan, the need to discourage energy demand by making the price faced by U.S. oil users equal to the world price and to remove the subsidy to oil importation implied by the existing system of price controls and entitlements. They also—this time, *in contrast to* 1977—stressed that decontrol would boost U.S. oil production. This shift on the part of U.S. officialdom amounted to a major change in philosophy—one in the direction of Friedman's thoughts on the matter.

For years, Friedman had cited the discouragement of U.S. oil production implied by the country's two-tier price system, under which strict price controls applied to one broad category of oil output—old oil—a situation magnified since 1975 by the higher, but binding, ceiling applying to another important category, new oil. He reaffirmed this point in his *Newsweek* column over 1979, including in his July 30 column lamenting the existing practice of "prohibiting the owners of domestic wells from receiving more than $5.94 for some categories of oil" (that is, old oil). This column also drew attention to the fact that the new-oil price ceiling of $13.24 was now also being rendered much lower in relation to global free-market prices by the recent world oil price increase.

The supply-depressing effects of price control had also repeatedly been pressed by members of the oil industry. This continued to be the case in 1979. For example, in a hearing on May 16 on the Carter decontrol proposal, the president of the Independent Petroleum Association of America, Jack M. Allen, testified that producers "are plugging oil wells today, and that is a tragedy that we should not be having," while adding that this precipitate closure of wells resulted "because the lifting costs exceed the $6 per barrel that is permitted for old oil."[124]

In 1979, there remained some skeptics among legislators regarding the notion that price controls had important supply-restraining effect. For example, the same day on which Allen testified saw

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[123] James Schlesinger's testimony of April 24, 1979, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 19) stated that "to minimize the inflationary impact that could result at the time of decontrol, the president has adopted a more gradual path of phased decontrol." Similarly, Carter, in his telegram to senior U.S. diplomats on April 5, 1979, had justified the highly graduated nature of decontrol on the grounds that it would "minimize the burden on our anti-inflation program." (See Galpern, 2012, pp. 623−624.)
[124] From Allen's testimony in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 218).

Representative Al Gore (D-Tennessee) asserted: "We don't get any more old oil by decontrolling the price of oil already found."[125] But, predominantly, what was striking about the 1979 discussions was the extent to which, among elected representatives and the executive branch, there was now acceptance that price controls were a significant impediments to U.S. oil supply and to old-oil production in particular.

This shift was most evident in the statements of leading figures in the Carter Administration. As discussed in the previous chapter, in 1977 the administration had deprecated arguments that decontrol would boost oil supply. In contrast, in his energy address of April 5, 1979, President Carter stated that "federal government price controls now hold down our own production."[126]

These sentiments were echoed by Carter's subordinates in their own public statements. For example, on April 24, 1979, the Secretary of Energy, James Schlesinger, testified that the existing oil-price control system was in effect an "import subsidy program" that "reduced domestic production of oil." He also noted that "we have put in place a massive regulatory bureaucracy" in order for the federal government to administer the existing oil system.[127] This message was not altogether different from, although implying less personal culpability than, that Friedman articulated in his *Newsweek* column some weeks later (in his column of June 4, 1979) when he lamented "how useless, indeed harmful, are the activities of James Schlesinger and his 20,000 employees."

The head of the Council of Economic Advisers, Charles Schultze, also affirmed the importance of decontrol as an impetus to higher production when he stated that "even in old-oil reservoirs… additional oil can be produced. So higher prices would provide incentives… to bring into production this additional oil from older fields."[128] On the same day as Schultze's testimony, a submission by Secretary of the Treasury Michael Blumenthal observed that the decontrol program "eliminates the disincentive to produce from old oil fields, since the profit earned from increased production in old oil properties will be the same as from investments in new oil properties."[129]

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[125] From Gore's remarks of May 16, 1979, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 270).
[126] Carter (1979b).
[127] In Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 18).
[128] From Schultze's testimony of May 17, 1979, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 340).
[129] From the prepared portion of Blumenthal's testimony of May 17, 1979, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 379).

These sentiments echoed Friedman's many remarks on the subject, including his 1975 reference to the "disincentive to produce old oil" (Instructional Dynamics Economics Cassette Tape 168, May 1975, Part 2).

A further poignant statement by Blumenthal was reported in early July: "The more I'm in the government, the more market-oriented I become. No bureaucrats… at the Department of Energy, trying to figure out how much gasoline each gas station in the country should get, can set out a way to distribute gas in this country." (*Time* magazine, July 9, 1979.)

Gasoline lines and allocations

This Blumenthal statement was made with specific reference not to the price controls but, instead, to government directives concerning the distribution of gasoline to service stations. These federal controls over fuel quantities in the United States—the allocations system—had been in force since 1973 and they underlay Friedman's statement during the Ford years, "I'm astounded with the willingness of the public at large to let the people who cannot deliver the mail deliver gasoline." (*Corpus Christi Times* (Texas), February 14, 1975.) It was, however, just as President Carter was going in a deregulatory direction by foreshadowing the end of oil price controls that the effects of the allocations system—a distinct aspect of federal regulation of petroleum—really came to the fore.

In conjunction with the still-in-force price controls, the allocations system led to major gasoline shortages and lines at service stations over much of 1979. Carter had launched his 1977 energy plan while noting that it was being launched alongside placid conditions at the retail level: "The 1973 gas lines are gone."[130] By the time the 1979 energy plan was being debated, however, the situation was very different, with the deputy secretary of the Department of Energy, John J. O'Leary, noting in mid-May 1979 that there were "long lines in gas stations in certain parts of the country, and weekend closings in most of the country."[131]

Over the summer of 1979 and into the early fall, Friedman repeatedly discussed the return of gasoline lines. Although his *Newsweek* column of June 4 cited immediate elimination of the country's petroleum price controls as the way to end the gasoline shortage, his July 30 column added "eliminating government allocation of gasoline" as a second necessary condition.

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[130] Carter (1977c), also quoted in *The Decatur Daily* (Tennessee), April 19, 1977.

[131] From O'Leary's testimony of May 16, 1979, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 173).

Friedman had long noted that, notwithstanding various claims that the U.S. government-controlled prices but had not engaged in rationing, there was actually a form of rationing in place as part of the federal government's network of controls, even though this rationing system was not usually visible to purchasers at the retail level. As he observed soon after the first oil shock: "if you have a look at what is going on now, in a sense, you already have rationing—except it's being called 'allocations.'" (Instructional Dynamics Economics Cassette Tape 136, December 13, 1973.)

The U.S. government had, through the allocations system, set up arrangements designed to distribute domestically-produced and imported petroleum product across service stations. The distribution rules had been guided by how gasoline had been demanded in the 1972–1973 timeframe.[132] By 1979, these distribution rules were sufficiently out of kilter with the pattern of gasoline demand across the United States that major gasoline shortages emerged in many areas of the country. Up to this point, following the end, in early 1974, of the Arab oil producers' embargo, the official price controls on petroleum products had tended not to be associated with major shortages at service stations—as the importation of oil imparted some upward slope into both the supply and price of petroleum available to U.S. purchasers. But in 1979, the phenomenon of major queues at, and exhaustion of supplies at, of gasoline stations returned—a joint product of the allocations system and the price controls.[133] Although the Iranian outage in early 1979 played an important role in initiating the new wave of gasoline lines, these lines continued after Iranian imports resumed and were particularly acute during the summer.

As the shortages continued past summer 1979, Friedman remarked on television that "there's no doubt that long gas lines are produced by Washington and nobody else." He cited the controls on the prices of crude oil and retail gasoline, as well as the official controls on allocation of gasoline from station to station. "People in Washington are simply not in a position to do a sensible job of shifting gasoline around," Friedman noted (*Today*, NBC, September 28, 1979). But the experience of 1979 saw the administration give little ground on quantitative controls: in April, even while testifying in favor of post-1979 price decontrol, James Schlesinger, remarked:

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[132] See the testimony of May 16, 1979, of Charles J. DiBona, president of the American Petroleum Institute, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 129).

[133] The role of the allocations rules in generating the gasoline lines of summer 1979 was acknowledged in the account of the Carter years by the president's former domestic adviser: see Eizenstat (2018, pp. 234–235, 671, 682). Eizenstat's stress on the interaction of these allocation controls with *retail gasoline price* controls in generating queues at service stations during this period may, however, obscure the fact that, even if no gasoline price controls had been present, the federal government's separate (and, in practice, much more rigidly enforced) crude-oil price controls were another factor that, in conjunction with the allocations system, made for shortages in this period.

"I think we would have to retain the authority to allocate in the event… of a crisis."[134] Furthermore, Carter's adviser on inflation, Alfred Kahn, suggested in late June that the United States should consider direct rationing of consumers' access to gasoline (*Detroit Free Press*, June 29, 1979*a*).

The exasperation on the part of Friedman and other economists at the failure of the Carter Administration to expedite deregulation of the energy market—and to go in a different direction in important respects—was reflected in a joint statement, published as an advertisement in the *Washington Post* (October 11, 1979) and, as with the 1977 statement, arranged by the National Taxpayers Union. Friedman's name led a long list of signatories, which also included several who had been prominent allies of his in monetary-economics debates in the 1960s and 1970s (including Anna Schwartz, David Meiselman, Robert Lucas, and Allan Meltzer), some figures (including Arthur Laffer and Alan Greenspan) who had been other major free-market advocates in the 1970s, and many former Friedman students (including Douglas Adie, William R. Allen, Warren Coats, Sam Peltzman, William Poole, Thomas Sowell, and Richard Timberlake).

The statement took Carter to task for blaming OPEC for the gasoline lines, while also challenging the stress being put on developments in Iran: "A disruption in world oil production, such as that caused by the revolution in Iran would not lead to gas lines here, as it did not in other countries, if the market were permitted to allocate supplies." The blame for the lines at service stations instead lay with "the government price controls and allocation[s] system." The statement called for Carter to abolish these regulations immediately criticized his move (made in mid-July) to add a new quantitative control in the form of an oil-import quota.[135]

The degree of frustration that had been generated over the summer of 1979 at the government control system was evident in the fact that the joint statement also called for the abolition of the Department of Energy. This was not a new prescription on the part of Friedman, but it was notable that so many others signed up in endorsing it, including signatories to the statement, like Martin Bailey and Robert Gordon, who had looked more favorably on government intervention in the economy than he had.

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[134] From Schlesinger's testimony of April 24, 1979, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 91).
[135] Carter had announced this move in his July speech, referred to further below, on new energy-policy measures (Carter, 1979d).

The prospects for the oil industry

An op-ed by Friedman that appeared in the *San Francisco Chronicle* shortly before the Carter decontrol announcement noted that, in light of postwar developments, advocacy on economic grounds of the nationalization of private-sector industries had all but dried up: "Nobody makes that argument anymore."[136] Friedman feared, nevertheless, that the oil industry might be an exception to this trend. Indeed, near the end of the year, he speculated in his *Newsweek* column that a second Carter term might see the big oil companies nationalized (*Newsweek*, December 10, 1979). In the same vein, in California (one of the states struck the most by the summer gasoline lines), the newspaper *San Jose Mercury*, in an editorial some months earlier (August 13, 1979) on "The Unspeakable Word" (that is, nationalization), had suggested that the U.S. oil companies had fallen so much in national standing that, barring a change in image, "they can expect to hear the unspeakable work spoken often, and loudly, in the future."

Carter's move toward decontrol ostensibly moved the energy sector closer to a free-market system because it sought to terminate the federal government's oil-price ceilings. But, as Friedman had stressed in the 1940s in his critique of Oskar Lange's "market socialism," even a system in which prices cleared markets need not correspond to a true market system, if the government preempted the flows of incomes implied by freely operating prices and made other interferences in the production process.[137]

Likewise, Friedman likely drew little comfort from the fact that, in Carter's decontrol announcement in April, the president had cited the fact that in the federal government's efforts "to control energy price, production, and distribution, the federal bureaucracy and red tape have become so complicated, it is almost unbelievable." For Friedman had long suspected that streamlining the operations of government was an interest of Carter's mainly "so that the power of Washington can be exercised more efficiently."[138] Correspondingly, to Friedman, Carter's 1979 energy proposals (officially called "The National Energy Plan II"), when viewed as a whole, represented a move toward greater public-sector control of the oil industry—and did not, on net, take the government away from directing the nation's energy production and

[136] *San Francisco Chronicle*, April 3, 1979. The op-ed was a condensation of a *Milton Friedman Speaks* talk that Friedman had given in 1977.
[137] See Nelson (2020a, Chapter 4).
[138] Instructional Dynamics Economics Cassette Tape 202 (November 1976, Part 1). Another motivation, already noted, that Carter had for lowering or streamlining government regulation—the control or lowering of inflation—obviously also did not impress Friedman favorably.

distribution.[139] On the contrary, he argued in a summing-up in October. "The Carter program would be another disastrous step [toward] converting America from a free society to corporate state."[140]

Other than the continuation of allocation powers, noted above, and what Friedman perceived as the threat embedded in the plan of the return of price controls in the future (*Newsweek*, July 30, 1979), there were a couple of features of National Energy Plan II, especially in its modified July 1979 version, that underlay Friedman's warnings about it. First, the plan involved extensive plans for the federal government to identify potential alternatives to oil and promising alternative methods and make large outlays on these ventures. Writing in *Newsweek*, Friedman made the point that the proposed government activity in energy development was likely to involve the country spending much more per unit on synthetic fuels than the consumer would pay even for the higher (*Newsweek*, July 30, 1979). He also continued to have a negative reaction to the plan's notion, previously made during the Nixon and Ford years, that the federal government should spearhead research into alternatives to oil. In opposing the synthetic-fuel programs, Friedman was able to gain agreement from the other economists in the joint public statement mentioned earlier: "Where is the evidence that our energy future would be more safely entrusted in government than to private companies[,] operating under the spur of profit and competition?" (*Washington Post*, October 11, 1979.)

The public statement did not, however, refer to another prominent item in the new energy plan: the proposed windfall profits tax. As proposed by the administration, this tax would apply to those profits, during and after the price-decontrol process, to sellers of old oil, that arose from the difference between the market price and the previously-applying old-oil price ceiling. The tax would also apply to profits that most sellers of oil accrued from market prices in excess of $16. In effect, the proposed windfall profits tax was targeted at oil sales receipts that U.S. oil producers received as a result of the first oil shock and the emerging new oil shock.

Friedman considered the windfall profits tax to be akin to the existing price-control system on the grounds that both mechanisms denied producers from "reaping the benefits" of having set up oil production sites before the oil shocks. Both mechanisms amounted to a "socialist solution" in

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[139] On the "National Energy Plan II" label, see U.S. Department of Energy (1979).

[140] Davidson and Friedman (1979, p. 410). By the time of this statement, the Carter energy program included both the version of National Energy Plan II announced in April-May 1979 and the augmentations to the plan Carter had made in mid-July. These augmentations sought to increase further the role assigned to the federal government in the long-term planning and production of national energy resources.

which firms, in an unfavorable outcome, had to absorb losses arising from risky ventures but, in a favorable eventuality, had to hand much of the associated profits over to government: a "heads-you win, tails-I-lose" situation (*Newsweek*, July 30, 1979)[141] This argument mirrored what Friedman had contended since 1973: in order for there to be incentives to add to supply, the revenue from price rises should accrue to U.S. producers.

The argument also mirrored points Friedman made of the April 1977 Carter plan. However, the 1979 windfall-tax proposal differed from the 1977 plan, as the Carter administration had, as indicated above, become more sympathetic to supply-incentive arguments. In 1977, it had essentially proposed that all of the receipts arising from the first oil shock should be denied to oil produced from old wells (and a considerable portion denied to newer wells, too). These receipts would be diverted entirely to tax revenues. In 1979, the administration was arguing, instead, that oil companies indeed have these receipts as income but then pay tax on the receipts at an extra-normal rate.[142] And the rate that the administration proposed for this purpose was notably below 100 percent—which had been the implicit tax rate embedded in the administration's 1977 COET plan. In 1979, the labor organization AFL/CIO called for an 85 percent windfall tax rate, and the House of Representatives' Committee on Ways and Means recommended a rate of 70 percent.[143] Neither of these rates was far below 100 percent. But the Carter Administration's own proposal in 1979 advanced a much lower rate: 50 percent.[144] In addition, the notion that a windfall profits tax should serve as a *quid pro quo* in exchange for oil price decontrol had been proposed previously, by both the Nixon Administration (in early 1974) and the Ford Administration (in early 1975) as well as by a group of Republican senators in 1977.[145]

Among commentators on policy developments, Friedman was not completely on his own in

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[141] See also his remarks in *Expo* (Summer 1979, pp. 18, 20).

[142] For example, Secretary of the Treasury Blumenthal testified on May 17, 1979, that the "reason that we are not [in the proposed changes] taxing away all of the increased revenue and, indeed, are leaving a very important part of the increased revenues with the owners and producers of old oil… [is so] substantial [production] incentives can be provided." See Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 393).

[143] See respectively *San Jose Mercury* (California), August 13, 1979, and *Detroit Free Press*, June 29, 1979*b*.

[144] See, for example, James Schlesinger's testimony of April 24, 1979, in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979, p. 20), as well as the discussion in U.S. Department of Energy (1979, p. 80).

[145] On the 1974 and 1975 proposals, see Committee on Ways and Means, U.S. House of Representatives (1974, 1975) and the discussion in Chapter 6 above. On the inclusion of a windfall profits tax in the energy plan advanced by Republican senators in 1977, see Committee on the Judiciary, U.S. Senate (1978, p. 226). In mid-1979, soon after the second oil shock was formalized by an OPEC decision, George Shultz reaffirmed his support for this kind of tax: "When I was Secretary of the Treasury, I did propose a windfall profits tax... [to be] phased out at the end of five years. I [still] think there is something to be said for an approach like that, when you have this tremendous, sudden rise." (Testimony of July 30, 1979, in Committee on the Judiciary, 1979, p. 576.)

wholeheartedly opposing a windfall profits tax on oil profits and in seeing a special tax as a disincentive to the risk-taking.[146] But among economists, he did not have a clear critical mass of support. Kenneth Arrow did voice doubt about the feasibility of designing an appropriate windfall profits tax on oil revenues, although he was not categorically opposed to the idea of instituting such a tax.[147] Support for a windfall profits tax as part of the U.S. policy response to the oil shocks of the 1970s was, in fact, quite commonplace among economists. Even Arthur Laffer had, in 1977, had been favorably disposed toward giving the U.S. president the authority to activate a windfall profits tax, provided that price decontrol was put into effect.[148] The fact that the Friedman-led joint statement by economists in October 1979—which essentially repeated numerous other criticisms Friedman made of the Carter administration's energy policies—did not take a stand on the National Energy Plan II's proposal for a windfall profits tax attested to the fact that Friedman's outright opposition to the tax was not uniformly shared even by economists who basically favored a free-market approach to energy policy.

Friedman, therefore, looked poised to be on the losing side of the windfall-tax debate. He recognized this in mid-1979 when, after declaring that "Mr. Carter's call for the tax is pure demagoguery," he granted that the tax might well be enacted into law as "Congress is a bunch of demagogues too" (*Expo*, Summer 1979, p. 18). The House of Representatives passed, on June 28, 1979, its own version of the windfall profits tax with a rate of 60 percent (lower than that recommended at the committee stage, but higher than the Carter Administration's desired 50 percent rate).[149] But, as of early December 1979, this bill had not yet been passed by the Senate. The possibility therefore remained that the windfall profits tax would not become law in 1980, even though oil price decontrol would definitely proceed.[150] Against this background, Friedman devoted a December *Newsweek* column largely to reiterating his stand against the idea of a windfall profits tax.[151] He restated his position that the owner of an asset should not be subject to extra-large tax on the receipts arising from the appreciation of that asset, even if others judged the appreciation to be the result of "fortuitous changes in market conditions" (*Newsweek*,

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[146] See, for example, *Kansas City Star* (Missouri), April 11, 1979, and Kemp (1979, p. 116).
[147] See Arrow and Kalt (1979, p. 35).
[148] See Laffer's testimony of May 20, 1977, in Joint Economic Committee (1978b, p. 15).
[149] See Committee on Finance, U.S. Senate (1979, p. 103) and *Detroit Free Press*, June 29, 1979*b*.
[150] Carter had written to senior U.S. diplomats abroad in April: "My decision to decontrol oil prices is not contingent on the passage of these taxes." (Telegram of April 5, 1979, quoted in Galpern, 2012, p. 624.) Similarly, Secretary of the Treasury Blumenthal stated, in testimony of July 10, 1979, that the administration's policy was to press ahead with decontrol, "whatever else is done" in the area of taxes on energy. (In Committee on Finance, U.S. Senate, 1979, p. 54.)
[151] Friedman had been critical of an excess profits tax (a variant of the windfall profits tax) as far back as 1953. See Friedman (1953b, p. 8).

December 10, 1979). On December 17, 1979, however, the Senate passed its own version of the windfall profits tax, thus making it very likely that a version of the tax would become law in 1980 (*Bangor Daily News* (Maine), December 19, 1979).

The second oil shock becomes official

The second oil shock became official on June 28, 1979, when the outcome of the midyear OPEC meeting in Geneva was a new world oil price floor of $18 (*Detroit Free Press*, June 29, 1979*c*). A statement by President Carter observed that this move implied a cumulative 60 percent increase in the world oil price since December 1978 (*Detroit Free Press*, June 29, 1979*d*). As it transpired, the price increases still had much further to go, and the world price of crude petroleum went from $18 at the end of the third quarter of 1979 to $26 per barrel six months later (International Monetary Fund, 1981b, p. 145).[152] Likewise, although U.S. gasoline prices were assessed by Secretary of the Treasury Blumenthal as likely to be pushed up by twelve cents a gallon by OPEC's June decision, the increase in gasoline prices in 1979 proved to be far in excess of this: the rise in the annual national average of the gasoline price was 23 cents from 1978 to 1979—from 63 cents to 85 cents. And in November 1979, U.S. gasoline prices averaged about $1.04 per gallon (*Rutland Daily Herald* (Vermont), December 20, 1979).[153]

Soon after the OPEC June oil-price decision, President Carter stated that it would add 2 to 2½ percentage points to the inflation rate (*Dallas Morning News*, July 2, 1979, p. 1A).[154] About ten weeks later, he remarked that he viewed the main effects of higher energy prices as having already been felt and that the inflation rate should "go down [in] the rest of the year" (*Omaha World Herald* (Nebraska), September 13, 1979). In contrast, as discussed in Section I above, U.S. inflation continued to increase over the rest of 1979 and into 1980. But, with global oil prices having also risen further over this period, Carter blamed the additional upturn in inflation on the oil shock. For example, in his televised debate with Ronald Reagan in October 1980, Carter stated that "in the first quarter [of 1980], we did have a very severe inflation pressure,

[152] The Texas crude oil price shows a somewhat more rapid rise—from $14.85 in December 1979 to $32.50 in December 1979 (*https://fred.stlouisfed.org/series/WTISPLC*).
[153] Blumenthal gave his estimate in testimony on July 10, 1979 (Committee on Finance, U.S. Senate, 1979, p. 49). For the annual averages of the retail price of U.S. gasoline, see "Fact #915: March 7, 2016: Average Historical Annual Gasoline Pump Price, 1929–2015," *https://www.energy.gov/eere/vehicles/fact-915-march-7-2016-average-historical-annual-gasoline-pump-price-1929-2015*.
[154] In his testimony of July 10, 1979, Secretary Blumenthal indicated that this estimate included effects of oil price increases since December 1978 and prior to the June OPEC decision, and he suggested that the contribution to higher inflation would be spread about equally over 1979 and 1980. See Committee on Finance, U.S. Senate (1979, p. 49).

brought about by the OPEC price increase."[155]

Much analysis at the time shared Carter's emphasis on the second oil shock as the driver of the United States' inflation problems. For example, in mid-1979, a *Minneapolis Star* commentary asserted that the "escalating price of gasoline… is contributing mightily to the rate of inflation," while the *Miami Herald* responded to the outcome of the Geneva OPEC meeting with the headline "OPEC Sends U.S. Some More Inflation."[156] Friedman, however, eschewed such diagnoses. "OPEC does not cause inflation—no, sir," he testified in a hearing in May 1979.[157] "Inflation is not produced by high oil prices," he reiterated in an interview that appeared as it was becoming clear that a lasting oil-price increase was in train, while lamenting the "gross confusion" between relative prices and absolute prices involved in blaming inflation on energy-price behavior (*Expo*, Summer 1979, p. 18).[158]

Friedman would subsequently grant that "the most recent oil shock" was a source of a temporary boost to inflation during the period in which the new energy prices took effect, irrespective of what a monetary policy stance a country had adopted in the leadup to the shock.[159] In the case of the United States, this boost was, as he saw it, what pushed the 12-month rate above fourteen percent in early 1980 when his own prediction, on the basis of prior monetary developments, had been for a peak in inflation in 1979−1980 of twelve or thirteen percent.

OPEC's consecration in June 1979 of the second oil shock did bring greater concurrence in U.S. commentary on the likelihood of a 1979 recession—something Friedman was already expecting on the basis of monetary patterns. President Carter remarked: "I think the OPEC decisions will make a recession much more likely than it was before." (*Dallas Morning News*, July 2, 1979.) And, testifying in mid-July, Federal Reserve chair Miller argued that the monetary policy tightening had been calibrated in a manner designed to produce slower economic growth but not a recession, but, on account of "the oil shock, we now expect there to be a recession this year."[160] And, writing in the *Financial Times* in early August, Paul Samuelson, noting that the Chase

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[155] "October 28, 1980 Debate Transcript: The Carter-Reagan Presidential Debate," p. 3.
[156] *Minneapolis Star*, June 29, 1979, p. 6A; *Miami Herald*, June 30, 1979.
[157] From Friedman's testimony of May 17, 1979, in Committee on the Judiciary, U.S. House of Representatives (1980, p. 154).
[158] The interview was conducted ahead of the summer of 1979, while Friedman was physically located at the Hoover Institution (*Expo*, Summer 1979, p. 18).
[159] Friedman (1982a, p. 102).
[160] From Miller's testimony of July 17, 1979, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1979b, p. 3).

Econometrics firm saw negative economic growth firm in each of the four quarters from 1979:Q2 to 1980:Q1, characterized the environment as one of near-unanimity about 1979 being a recession year, and he implied that Friedman should be among those congratulated for successfully predicting this eventuality (*Financial Times* (London), August 6, 1979).

Writing in September, Friedman, too, thought the prediction had been realized and opened a *Newsweek* column: "Recession? Looks like we're in one."[161] As we have seen, however, the U.S. economy did not enter recession in 1979—with Friedman seeing a lessening of monetary policy tightness in the spring-summer 1979 period as the reason. By the time recession did actually begin in 1980, not only had the second oil shock developed to a much fuller extent, but monetary restriction had again become a major factor making for recession.

Carter's shake-up

President Carter made a televised address to the nation on July 15, 1979. Although originally planned to be centered on his announcement of further changes in his energy policy in light of the summer's energy-supply disruptions, the portion of the speech only came after Carter had spoken at length about his concern regarding the country's overall mood and condition. This broad-ranging aspect of the speech would come to be characterized in many retrospectives as a criticism by the president of the attitude of the U.S. citizenry and would be used against Carter. In the immediate aftermath of the speech, however, Friedman contrasted this aspect of the speech favorably with its policy component. "As a philosophical and theological speech, it was excellent," Friedman remarked on the speech, "[but] as a proposal for our energy crisis, it was terrible." (*Chicago Tribune*, July 16, 1979, p. 12.)[162] His *Newsweek* column (July 30, 1979) following the address criticized Carter's failure to cite price controls as the reason for the gasoline shortages.[163] As already indicated, the speech instead put great emphasis on long-term

[161] *Newsweek*, October 1, 1979. As already noted, the second-quarter U.S. GNP data had initially pointed in this direction by registering negative real growth. It was on the basis of this single-quarter decline (later revised to a slightly positive number) that a *Financial Times* news item (July 21, 1979) was headed: "U.S. GNP Fall Confirms Recession." Similarly, Paul Samuelson, after tentatively stating that "we appear to be in a recession," went on in the same column to refer to "the present recession" (*Newsweek*, September 24, 1979).

[162] Friedman's praise for the philosophical aspects of Carter's speech was consistent with remarks he had made over the years that were not too far removed from some of the sentiments Carter had expressed. For example, in testimony given to a Congressional hearing on October 6, 1969, Friedman had referred to "the widespread feeling of malaise in this country… reinforced by dissatisfaction over Vietnam." (In Joint Economic Committee, 1970a, p. 810.) That said, in 1969 and 1979 alike, Friedman's identification of the underlying reasons (other than the Vietnam War) for this national mood, together with his prescribed remedy, differed from Carter's in 1979.

[163] Even Friedman's old sparring partner Robert Eisner was quoted criticizing Carter for not mentioning the price mechanism in the speech (*Chicago Tribune*, July 16, 1979, p. 12). Carter's speech had gone in the opposite

government development of synthetic fuels—a proposal that led Friedman to remark: "The idea of creating a system that involves [the equivalent of] wartime economic planning would be disastrous if it ever was carried out." (*Chicago Tribune*, July 16, 1979, p. 12.)

The two weeks after Carter's speech were followed by numerous personnel changes. Among the announcements made in this connection was that James Schlesinger would depart the post of Secretary of Energy and Michael Blumenthal would go as Secretary of the Treasury. Carter nominated G. William Miller as Blumenthal's successor and, on July 26, put forward Paul Volcker to succeed Miller as Federal Reserve chair. Following their confirmations, Miller and Volcker were both sworn into their new positions on August 6, 1979 (see Carter, 1979e).[164]

In a *Newsweek* column to mark Volcker's ascendancy, Friedman reviewed the record of monetary growth and inflation during the 1960s and 1970s. Inflation was likely to reach a peak before long, Friedman predicted, in light of the decline in monetary growth (measured by the M2 series) since 1977. But the United States' economic record since the mid-1960s suggested that this peak would not be followed by a restoration of price stability. "The problem is not, as President Carter [in his July 15 speech] asserts, a lack of confidence," Friedman wrote. "The problem is rather that the public is very confident that the government will produce inflation and will mismanage the economy. We do not need more confidence in bad policies. We need better policies." (*Newsweek*, August 20, 1979.)

INTRODUCTION OF THE NEW OPERATING PROCEDURES

The changes in Federal Reserve policy associated with the ascension of Paul Volcker to the position of Federal Reserve chair in 1979 have given rise to a voluminous research literature that

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direction, stating (Carter, 1979d): "I ask Congress to give me authority for mandatory conservation and for standby gasoline rationing." Eizenstat (2018, p. 689) indicates that Carter in July 1979 instantly rejected Eizenstat suggestion of moving to immediate decontrol at the retail level, that is, to abolish the allocations system and the controls (essentially controls on the markup over the wholesale price of gasoline) on the retail price of gasoline. Although Carter did not mention the allocations system in his speech, Secretary of the Energy Schlesinger had testified several days earlier that "the president has decided he does not want to scrap… the allocation[s] system." (Testimony of July 10, 1979, in Committee on Finance, U.S. Senate, 1979, p. 139.)

[164] Volcker had been vice chairman of the Federal Open Market Committee under Miller. This position was due to Volcker's status as president of the Federal Reserve Bank of New York. To become Federal Reserve chairman and chairman of the FOMC, he had to receive presidential nomination and Senate confirmation (and, in the process, also become a member of the Federal Reserve Board). Consequently, when Friedman wrote to Volcker to mark "your 'promotion'" (see Silber, 2012, p. 148), the word "promotion" was in quotation marks to reflect the fact that the move from the vice-chair to chair position involved these nomination-and-confirmation processes in the executive and legislative branches of the U.S. government, rather than comprising simply a promotion in rank within the Federal Reserve System.

now spans more than four decades. Those changes can, essentially, be divided into two groups: tactical changes—the Federal Reserve's adjustments, as embedded in the "New Operating Procedures" issued on October 6, 1979, to its market operations and monetary-control techniques—and strategic changes—alterations to the doctrine guiding U.S. monetary policy (in particular, the Federal Reserve's conception of the nature and extent of its influence on inflation), together with the related shift in policymakers' reaction function.

Both the changes at the tactical level and those at the strategic level owed much to the longer-term influence that Friedman had on thinking about monetary policy in the United States. As it happened, the tactical changes proved largely ephemeral—with the Federal Reserve's 1979 move to a reserves-based operating procedure being, in effect, terminated in 1982 in favor of a return to operations largely focused on the management of the federal funds rate. The strategic changes proved far longer-lasting—the year 1979 saw a distinct and permanent change in the Federal Open Market Committee's reaction function (see, for example, Clarida, Galí, and Gertler, 2000) and in its conception of the scope for monetary policy to set the longer-term inflation rate (see, for example, Romer and Romer, 2002b, and Nelson, 2005).[165]

From a longer-term perspective, therefore, the strategic changes were clearly more profound than the tactical changes. It follows that, of the two sets of changes, the strategic changes amounted to a much better example of Friedman's lasting influence on the course of monetary policy. Nevertheless, the tactical change—the FOMC's adoption in 1979 of what were called the New Operating Procedures—seemed to be the more dramatic event at the time—and it certainly was the one to which Friedman devoted more attention in 1979. That change in monetary-control techniques is therefore considered here first—in the remainder of this section. Strategic changes are then considered in the discussion titled "Paul Volcker" in Section III. It should also be noted that, in the analysis of the New Operating Procedures presented here, the focus will be on the buildup of outside pressure to move to new operating techniques and on Friedman's initial reactions to the Federal Reserve's 1979 changes. No attempt is made to give a detailed account

[165] Meltzer (2009b, pp. 1033–1034) also stressed that strategic changes in policymakers' thinking were the most important outcome of the October 1979 developments even though they were not the main focus of media coverage at the time. Meltzer, however, expressed the strategic change as an increased weight on inflation, in relation to real variables, in the Federal Reserve's objective function. This way of portraying the change was not ideal. It failed to capture either (*i*) the fact that the Federal Reserve saw inflation as highly costly (in its economic effects) both before and after 1979, or (*ii*) the notion that the Federal Reserve moved to viewing inflation as a monetary phenomenon in this period. In contrast to Meltzer's characterization of the 1979 change in strategy, that shift is seen best as a change in the Federal Reserve's model of the economy—and *not* as a change in the respective weights placed on its nominal and real economic objectives.

of the developments within the Federal Reserve that led to the choice of the new procedures.[166]

The pressure for reform of operating procedures

As has been noted in the previous volume and in earlier chapters, Friedman granted that, during the 1970s, the Federal Reserve had paid more attention to monetary aggregates and that, indeed, a specific target rate of growth for the aggregates had typically been a factor entering into the Federal Open Market Committee (FOMC) meeting-to-meeting policy decisions (and had also weighed on the monetary policy actions, such as with respect to reserve requirements and the discount rate, taken by the Federal Reserve Board). As Friedman saw it, a major (though not exclusive) reason why the FOMC had not done better in achieving steady monetary growth lay in its continued use of the federal funds rate as an instrument.

For the majority of the 1970s, the FOMC had had an adjustable target for the funds rate—with the adjustments ostensibly guided by the need to achieve the intermediate, monetary-growth targets. But, as Friedman often noted—especially in his commentaries from 1975 onward, when the monetary-growth targets became more official—the FOMC had a fine record at achieving its funds-rate target but nowhere the same degree of precision or success with regard to delivering monetary growth on target. This point would, in retrospect, be underlined by Rudebusch's (1995, p. 258) chart of the federal funds rate and the target rate from 1974 to 1979, showing a very close match between the target and actual series. In addition, the deviations between the two series did not have the persistent, one-sided character that had frequently characterized the Federal Reserve's pursuit of monetary targets during the 1970s.

Friedman saw the means to secure much better control of growth in series like M1 and M2 as lying in a move by the Federal Reserve to direct management of its balance sheet, in place of the use of a federal funds rate target. Reflecting this conviction, in a letter to Volcker on July 31, 1979—written after Volcker had been nominated to be chair and at a point when his accession to the position the following week was virtually assured—Friedman stated that the Federal Reserve System needed to make "major changes in its method of operation" in the direction of arrangements better suited to deliver a gradual step-down in the rate of monetary growth.[167]

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[166] Friedman's reactions to unfolding developments during the 1980–1982 portion of the New Operating Procedures period are considered in later chapters. On the internal Federal Reserve deliberations that led to the 1979 change in procedures, see especially Lindsey, Orphanides, and Rasche (2005) and Meltzer (2009b, pp. 1016–1033).
[167] Quoted in Silber (2012, p. 149). Silber notes (p. 148) that the handwritten letter was dated "July 31, 1912 (Friedman's birthday)." It was likely that this was nothing other than a slip in writing the date of July 31, 1979

As Volcker took office, the outside pressure on the Federal Reserve to reform its operating procedures was also reaching a crescendo: Friedman's call in his *Newsweek* column (August 20, 1979) for the Federal Reserve to abandon its "love affair with interest rates" was echoed by his friend, UCLA's William R. Allen, who declared in a radio commentary in the fall of 1979 that the Federal Reserve's existing operating procedures amounted to an "American tragedy" and that the U.S. monetary authorities had been "mesmerized" by interest rates.[168]

Why was the choice of short-run policy instrument crucial for successful monetary targeting, according to this interpretation? One reason was that the federal funds rate target was adjusted by only small increments. The Federal Reserve, as we have seen, raised the funds rate by an amount in excess of the rise in inflation in 1978, and monetary growth fell. But in the year starting in mid-1978, its increases in the funds rate, although sufficient to brake monetary growth during part of that period, were, on the whole, less than the rise in inflation. As this phenomenon unfolded, the Citibank *Monthly Economic Letter*—a voice of monetarism in financial commentary during the 1970s and, late in the decade, often authored by Friedman's former student James Lothian—observed (January 1979, p. 5) that "the inflation-induced rise of interest rates seems to have had little impact on spending and output, and there is no reason to believe it should have."

In addition, the preoccupation with small adjustments to the federal funds rate reduced the Federal Reserve's ability to generate a smooth profile in the time series of the quantity of bank reserves. As Anna Schwartz wrote to legislators in April 1979: "The attempt of the Federal Open Market Committee to peg the federal funds rate results in a procyclical behavior of the monetary aggregates. During cyclical expansions in business, the FOMC accelerates the flow of reserves to banks in order to slow the rise in the federal funds rate; during cyclical contractions, it decelerates the flow of reserves in order to slow the decline in the federal funds rate."[169]

These criticisms—according to which the monetary growth rate implied by a chosen federal funds rate varied both with the business cycle and with inflation—were expressions of a critique

(which, obviously, was Friedman's birthday, too, but a date he was far less accustomed to writing down). A more intentional reference by Friedman to his birthday occurred later in the year when his *Newsweek* column of December 10, 1979, outlining the possible state of economic policy in 1984, set the scene with the opening line "Washington, D.C., July 31, 1984." This piece of whimsy on Friedman's part was omitted by editor William R. Allen when the column was reprinted, otherwise intact, in Friedman (1983a, pp. 319–322).

[168] Allen's broadcast was dated October 1979 (but evidently drafted before the changes in operating procedures announced on October 6. See the transcript of the broadcast in Allen (1981, pp. 242–243).

[169] Schwartz (1979b, p. 309).

of Federal Reserve operating procedures that Friedman himself had articulated many times.[170] He summed things up in June 1976 as follows: "Whenever market interest rates have been tending to rise for whatever reason, the Fed has tended to produce a more rapid rate of monetary growth than it intended. Whenever market interest rates have been tending to fall, the Fed has tended to produce too slow a rate."[171] The upshot was that, as the *Wall Street Journal* piece (January 23, 1979) observed, the Federal Reserve still gave more attention to nominal interest rates than Friedman wanted, notwithstanding its use of monetary targets.

Part of this attention was manifested in the FOMC's reluctance to make rapid adjustments to the federal funds rate. The inadequate adjustment of the federal funds rate in the face of economic conditions reflected not only the fact that adjustments were typically small but also the fact that they were gradual.[172] Two economists at the Federal Reserve Bank of San Francisco—John Judd and Friedman's former student John Scadding—discussed these issues in a paper planned for circulation to the FOMC in October 1979.[173] Judd and Scadding (1979, p. 23) observed that "the FOMC … [has] moved the [funds] rate only slowly, or by small amounts, when confronted with less than complete evidence that policy should be changed." They added (p. 27) that, in particular, "the FOMC generally has moved its funds-rate operating instrument very cautiously in attempting to achieve [monetary] targets, … [so] funds-rate changes have generally not been large enough nor timely enough to stabilize money growth." In this way, Judd and Scadding concluded that the tactical choice of an interest-rate instrument had had a detrimental effect on the Committee's ability to meet its stated monetary targets.

Volcker acts

Judd and Scadding's paper was external evidence of the fact that there was agitation within the Federal Reserve System in August-September 1979, above and beyond the pressure coming from the traditionally monetarist Federal Reserve Bank of St. Louis, for the Federal Reserve to move away from interest-rate-centered operating procedures. The unsettled state of financial markets in early October 1979, manifested *inter alia* in further dollar depreciation and rising gold prices, amplified the pressure for a change in Federal Reserve policy and helped prompt major actions

[170] For documentation of this point, see, for example, Nelson (2020a, Chapter 9) and Chapter 5 above.
[171] Friedman (1976i, p. 9).
[172] See also the discussion in Nelson (2019) of Allan Meltzer's public activities during 1979 calling for a change in operating procedures and emphasizing the over-gradual nature of the FOMC's interest-rate adjustment.
[173] The plan to distribute the paper was dropped when the change in Federal Reserve operating procedures was announced, and it was instead published in the Federal Reserve Bank of San Francisco's publicly-issued research journal. See Judd and Scadding (1979, p. 23).

and public statements on the part of Volcker and his colleagues.

On October 6, 1979, Chairman Volcker announced a series of new measures, including a change in operating procedures. A Federal Reserve statement issued in conjunction with the announcement indicated that monetary policy "will focus on supplies of bank reserves in trying to control the growth of the money supply instead of, as now, focusing on the federal funds interest rate." (Quoted in *Financial Times* (London), October 8, 1979*a*.)[174]

Friedman's reaction

The centerpiece of the policy change was the shift to a reserves aggregate—nonborrowed reserves—as the Federal Reserve's principal policy instrument. This item, which is discussed at length presently, was, however, announced by Volcker on October 6 as one part of a *package* of measures.

One other element in the package tended to attract the headlines in the immediate wake of the announcement (see, for example, *Dallas Morning News*, October 7, 1979)—a 100-basis-point increase in the discount rate (to 12 percent). Reflecting the change in atmosphere that occurred during the fall 1979 period, this increase reflected a unanimous Federal Reserve Board vote, in contrast to the 50-basis-point increase in September, for which Volcker had only received 4-to-3 voting support from the Board.[175] In Friedman's view, the discount-rate increase received exaggerated attention in the news coverage of the October 6 announcements (*Newsweek*, October

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[174] See also that day's Federal Reserve press release, printed in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1979, pp. 44–49) (and also excerpted in *Financial Times* (London), October 8, 1979*b*). This statement indicated (p. 44) that the FOMC's "change in the method used to conduct monetary policy… involves placing greater emphasis in day-to-day operations on the supply of bank reserves and less emphasis on confining short-term fluctuations in the federal funds rate." This passage of the statement was excerpted, with a couple of non-material errors in the reproduction of the quotation, in Friedman (1983a, p. 8; 1985a, p. 54) and was correctly quoted in Friedman (1984a, p. 28).

[175] The contrast between the September and October votes, which Volcker mentioned in his October 6 public remarks, was highlighted in the Associated Press' report on the Federal Reserve's measures. See *Miami Herald*, October 8, 1979. A reported reaction among commercial bankers at the time was that the discount-rate increase was mainly "symbolic" (*New York Times*, October 8, 1979*a*, p. D6). Its perception as a symbolic act may have been due both to the fact that the discount rate had diminished in importance in U.S. monetary policy since the 1960s and to the need to revise the damage to assessments of the Federal Reserve's resolve that had been created by the previous, close-run discount-rate vote. James Meigs, whose reaction to the Federal Reserve's October 6 announcement is discussed further below, conjectured that the Federal Reserve was including a major discount-rate change as part of its package because this element would elevate the importance of the new package in the perception of other countries (in which the discount rate was often the central policy rate). See Meigs' remarks in *San Jose Mercury* (California), October 14, 1979, p. 1E.

22, 1979). And even a Board spokesman stressed to the media on the weekend of October 6–7 that the procedural changes unveiled in the just-announced package had "far more significant" announcements than the discount-rate rise.[176]

A further measure announced on October 6—and another one that Friedman felt got too much attention (*Newsweek*, October 22, 1979)—was the imposition of a marginal reserve requirement of 8 percent on the issuance of commercial banks' managed liabilities—among them such wholesale instruments as marketable certificates of deposit, repurchase agreements, and bank borrowings in the Eurodollar and federal funds markets (*New York Times*, October 8, 1979*b*, p. D8; *Financial Times* (London), October 8, 1979*a*).[177]

Meltzer (2009b, p. 1026) implied that this increase in marginal reserve requirements was a highly restrictive action—a surprising judgment, as that measure was directed at bank liabilities that were *outside* the traditional M1 and M2 definitions and so did not directly bear on the aggregates that Meltzer and other monetarists most emphasized.[178] Karl Brunner's reaction at the time was different from Meltzer's retrospective one—and it matched more closely the expected monetarist reaction. Brunner contended that the marginal reserve requirements suggested a concern with credit control rather than monetary-aggregate control.[179] Volcker's own explanation for the reserve-requirement move focused on credit: banks' wholesale deposit issuance, he suggested, "had financed much of the recent buildup in bank credit.[180] Volcker had earlier indicated that this was the case when he stated that the requirement would be in place "so long as credit expansion is excessive" (*Omaha World Herald* (Nebraska), October 10, 1979, p. 4). Friedman disapproved of marginal reserve requirements and made clear he did not think they

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[176] See the Associated Press' report in the *Omaha World-Herald* of October 8, 1979—a report that, the Board spokesman's message notwithstanding, made a headline out of the "lending rate hike."
[177] This marginal reserve requirement had some similarities to one in force in the United Kingdom at the time that was known informally as "the corset." The requirement's presence in the October 6 package, and its prominence in the public announcement of the package, were likely what mainly prompted the *Financial Times* (October 8, 1979*a*) to misconstrue the package in these terms: "In essence, Mr. Paul Volcker, the new chairman of the Federal Reserve Board, has switched from a traditional reliance on the level of interest rates to control credit demand to a U.S. version of Britain's banking 'corset.'"
[178] Some wholesale items were included in the revised definition of M2 in 1979−1980, only to be taken out in subsequent redefinitions. (In particular, overnight repurchase agreements and Eurodollars issued by banks were initially included in the new M2 series—see Hafer, 1980, p. 27. Hetzel's, 2008, p. 156, statement that managed liabilities were "not part of… M2" was not fully true of the modern M2 series until 1996.) Wholesale deposits did feature prominently in the redefinition of M3 in 1979−1980, but they were not yet in the M3 series (which was one of the officially-targeted monetary aggregates) as of October 1979.
[179] See the passage in the written submission accompanying Brunner's testimony of December 4, 1979, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1980a, pp. 110−111).
[180] From his October 17, 1979, testimony, in Volcker (1979a, p. 889).

were necessary for what the Federal Reserve should be concerned with: money stock control.[181]

The inclusion of a quantitative measure in the package directed essentially at credit growth rather than the monetary aggregates likely informed Friedman's judgment that the October 6 announcement was "ambiguous" as an expression of the Federal Reserve's determination to improve monetary control (*The Guardian* (London), October 22, 1979). While stopping short of saying that the new reserve requirement would have no effect, Friedman made it clear that he believed that it amounted to an attempt by the Federal Reserve to introduce new quantitative controls on commercial bank expansion, in place of older controls that the banks had been able to bypass (*Newsweek*, October 22, 1979). He summed things up by remarking of the discount-rate increase and reserve-requirement move: "If they were the only two parts [of the package], the whole thing could be dismissed as purely cosmetic." (*Richmond Times-Dispatch* (Virginia), October 12, 1979.)

Accordingly, for Friedman, the major change in the October 6 measures was the shift to the new operating procedures—comprising a focus on bank reserves and a countenancing of larger short-term fluctuations in the federal funds rate, and intended to deliver greater control of monetary growth. "It wasn't spelled out in detail—it was left very vague and very ambiguous," Friedman remarked, "but, if it means that the Fed is going to shift to controlling the money supply through the monetary base, it's of potentially enormous significance." (*Richmond Times-Dispatch* (Virginia), October 12, 1979.)

In the Cambridge/Boston area of Massachusetts that had for so long been a center of opposition to monetarism, a newspaper editorialized on the Federal Reserve's announcement. The editorial noted that, although high nominal interest rates had accompanied inflation for years, and there had been previous instances in which the Federal Reserve attempted to use what seemed like high interest rates as a policy against inflation, the present episode involved quantitative control that was designed to help ensure that the slower monetary growth emerged—"a strategy that monetarists like Milton Friedman have been urging for years." (*Boston Herald American*, October 11, 1979.)

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[181] See his remarks in *Newsweek*, April 14, 1980. The occasion for these 1980 remarks was the Federal Reserve's later increase in the marginal reserve requirement to 10 percent. This was part of the 1980 credit-controls package (Meltzer 2009b, p. 1050). At the time of the October 1979 announcement, Friedman's former student James Meigs had likewise said that the marginal reserve requirement was "sort of a side issue, a form of selective control" designed to influence bank loans rather than deliver *bona fide* monetary control (*South China Morning Post* (Hong Kong), October 14, 1979).

In line with this sentiment, when taking stock a couple of weekends after Volcker had made the move, the *Los Angeles Times* correctly observed that the truly distinctive part of the package was the move to a reserves-based operating procedure and "letting interest rates jump around with far greater volatility than before." It noted further: "Pressure for such a move has been building for more than a decade, from a growing cadre of economists known as monetarists and led by Nobel laureate Milton Friedman." (*Los Angeles Times*, October 21, 1979, Section 1, page 14.)

The *Los Angeles Times* analysis noted also that Friedman was skeptical with regard to whether a material—and durable—change in operations was going into effect. This skepticism was already evident in public statements he had made before the *Los Angeles Times* article appeared. Speaking in San Francisco shortly after the procedures were inaugurated, Friedman called them a "long overdue change," but he added his caveat that "I remain skeptical until I see proof" that a change of substance in policy procedures had taken place (*San Francisco Chronicle*, October 18, 1979). In the 1980s, Friedman would frequently point to his *Newsweek* column in reaction to the October announcement (*Newsweek*, October 22, 1979) as evidence of his wariness at the time of the announcement of the New Operating Procedures.[182] He cited that column as evidence, in particular, that he had was not very impressed by the announced change (see Idea Channel, 1987). The column did express the hope that the Federal Reserve had indeed turned over a new leaf, but it alluded to the previous occasions (most notably when the FOMC apparently adopted a reserves instrument in 1972) when Friedman had taken at face value an announced change in Federal Reserve procedures, only to find that the change in operating procedures had been illusory and that, in practice, the FOMC was still focusing on the federal funds rate.

Friedman reinforced this point in remarks he made to a reporter: "The Federal Reserve has been very stubborn. On several previous occasions, they have made noises of a similar kind. Each time, they reverted to their former method of operation. The question now is whether they really will carry through on this." (*Los Angeles Times*, October 21, 1979, Part 1, p. 14.)

The new operating procedures and monetarist prescriptions

Friedman's former student, James Meigs, had a longstanding interest in the issues involved in the unfolding 1979 developments. Nearly twenty years earlier, he had produced the Friedman-supervised dissertation work on the money-supply process and on the Federal Reserve's attitude

[182] See, for example, Friedman (1984g, p. 397).

to bank reserves—work published as Meigs (1962). In 1979, Meigs, now an academic based in California, welcomed as the "really important announcement" in the Federal Reserve's October 6 package the move to the reserves-based procedure: "if they are serious about that, it would be a revolution in their procedures—and extremely helpful." (*San Jose Mercury* (California), October 14, 1979, p. 1E.)[183]

Monetarists had reason for taking some satisfaction not only in the move itself, but also in how policymakers articulated the rationale for it—and in particular, in the way in which officials put the decision in the context of money-supply theory. Although the *New York Times* (October 8, 1979*a*, p. D6) noted in the wake of Volcker's announcement that "many economists" saw money-supply determination in monetary-base/money-multiplier terms, it was nevertheless true that, as part of the Federal Reserve fightback against monetarism over much of the 1960s and early 1970s, monetarists had from time to time found themselves accused publicly by central bank officials of having an unsophisticated, textbook-like view of money creation that did not describe reality. Meigs himself had documented, and rebutted, criticisms of this type in his 1972 book *Money Matters*.[184] Within a couple of weeks of the October 6 announcement, Volcker himself publicly endorsed the essence of the money-multiplier approach when he remarked in testimony that "the provision of reserves to the banking system… ultimately governs the supply of deposits and money."[185]

When it came, however, to the *details* of how to proceed in light of the acceptance of the importance of bank reserves for monetary control, differences between monetarists and the Federal Reserve remained. In particular, Friedman would always reject the notion that the Federal Reserve's New Operating Procedures coincided with the particular procedures he had advocated.

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[183] Within the Federal Reserve System, the Federal Reserve Bank of St. Louis had also had, as already indicated, a long tradition of emphasizing bank reserves and putting forward reserves as the appropriate monetary policy instrument. After the October 6 announcement, the bank's president, Lawrence K. Roos, remarked that it marked the adoption of "what we've been advocating for years… This is a major change of emphasis... [toward] what the St. Louis Fed has [been] preaching." (*St. Louis Post-Dispatch*, October 8, 1979.) Increased attention to reserves-based operating procedures had been a feature of internal analysis within the Federal Reserve System during Miller's tenure (see Meltzer, 2009b, pp. 982, 991–992), and Miller had joked during a visit to the city of St. Louis: "Now, the St. Louis Fed runs the whole thing… We in Washington are trying to do something about it, [by] trying to get control [back] from St. Louis." (*St. Louis Globe-Democrat*, June 15, 1979.)

[184] See Meigs (1972, pp. 158–164).

[185] From Volcker's (1979a, p. 889) testimony to the Joint Economic Committee on October 17, 1979. Similarly, a little over a week earlier, Volcker (1979b, p. 3; p. 35 of the printed version) had stated: "More emphasis will be placed on limiting the provision of reserves to the banking system—which ultimately limits the *supply* of deposits and money—to keep monetary growth within our established targets for this year." (Emphasis in original.)

And, indeed, there were substantial differences between how the Federal Reserve conducted monetary policy starting in October 1979 and the techniques that Friedman wanted them to adopt. One of these differences—the fact that the reserves instrument the Federal Reserve chose to use as its operating target was nonborrowed reserves—was, in itself, a crucial difference. Certainly, nonborrowed reserves differed from what Friedman advocated. Friedman had wanted monetary base or total reserves to be used as the policy instrument (with its values, in turn, varied with the aim of achieving a monetary-aggregate target, such as for M1 or M2).[186] The attraction of base control lay principally in the fact that, as it covered both types of major Federal Reserve liabilities—currency and bank reserves—it had the closest relationship to total Federal Reserve assets. Consequently, base-control procedures involved the simplest instructions, in the formulation of open-market policy, about actions to be taken with regard to the volume of Federal Reserve assets. But Friedman had long granted that, if there were unusual conversions of deposits into currency, it was not appropriate for the Federal Reserve to keep the base constant or growing constantly. Indeed, the Federal Reserve's failure to allow the base to rise to accommodate currency demand had been one of the points on which he and Schwartz had, in their *Monetary History*, criticized its conduct during the early 1930s. Correspondingly, even in the 1980s, when he advocated direct base control most strongly, Friedman indicated (albeit sometimes only in asides) approval of the notion of permitting exemptions to a strict setting of the base.[187]

What he wanted more fundamentally was Federal Reserve control of total reserves. As the 1979 announcement involved a move to control of nonborrowed reserves, not of total reserves, it seemingly fell short of what Friedman wanted. But the Federal Reserve's manipulation of nonborrowed reserves might, under certain conditions, succeed in delivering control of total reserves: because the difference between the two was discount window borrowing, variations in such borrowing could be monitored and nonborrowed reserves accordingly managed with an eye

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[186] At points especially from the early 1980s onward, as discussed in late chapters, Friedman saw special virtue in the monetary aggregate targeted simply being the base. On this shift in emphasis on his part, see later chapters.

[187] Although Friedman saw merit in a practice in which the Federal Reserve sought to offset, by movements in the monetary base, the implications for the money stock of variations in the currency/deposit ratio, he did not view fluctuations in that ratio as having been a major source of short-run and medium-run shifts in the growth rates of U.S. monetary aggregates, except during financial panics. (See, for example, Friedman and Schwartz, 1963b, p. 46.) That position allowed him to move between recommendations of monetary base control and of control of total reserves in his prescriptions for central banking. Likewise, another monetarist, Allan Meltzer, was prone to treat base control and reserves control almost interchangeably, writing in the immediate aftermath of the Federal Reserve's policy change: "All that the Fed has to do is control the size of its own balance sheet from quarter to quarter and from year to year. That is what control of bank reserves and the monetary base means." (*New York Times*, October 14, 1979. See also Meltzer, 2009b, p. 1027.)

to their implications for total reserves. Indeed, in an early research paper on the New Operating Procedures, Gilbert and Trebing (1981) characterized Federal Reserve policy under that regime as, in essence, entailing a targeting of total reserves—their grounds being that nonborrowed-reserves operating targets were chosen in conjunction with an assumption about the volume of bank borrowings from the Federal Reserve and so implied policymaker aims with regard to the total volume of reserves.[188]

Friedman himself recognized discounting as a fact of central banking, short of major institutional reform like a move to 100 percent reserve requirements. He was also willing to accept that distress lending through the discount window that raised total reserves in the short run was compatible with tight control over total reserves over monthly periods or longer.[189] It was probably with such an arrangement in mind that, in a public statement made in early 1978, Friedman himself actually advocated the adoption of a nonborrowed reserves instrument as an option: in recommending "abandonment of the futile attempt to control interest rates," he proposed that instead "the key operating instrument [be either] the volume of the monetary base or of unborrowed reserves."[190]

The fact that the Federal Reserve chose nonborrowed reserves, rather than total reserves, as its operating instrument in October 1979 was, therefore, not in itself a matter to which Friedman very strongly objected—at first. Correspondingly, even in light of the decision by the FOMC to make nonborrowed reserves—and not a broader balance-sheet item—the targeted series, he described the 1979 change in operating procedures as a "highly desirable" move (*Wall Street Journal,* February 1, 1982). Having made its October 1979 change in operating procedures,

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[188] Volcker buttressed the notion that the Federal Reserve was concerned with total reserves when he indicated in his Congressional testimony of October 17, 1979, that a reason for the Board's increase in the discount rate as part of the October 6 package was in order that restraint on nonborrowed reserves show up in the behavior of total reserves and "not be offset by excessive borrowing [by commercial banks] from the Federal Reserve Banks" (Volcker, 1979a, p. 889; also in Volcker [1979b, p. 3; p. 35 of the printed version]). On October 15, Volcker had likewise testified: "we shall be focusing on how much reserves we should be providing to the banking system… The more they borrow, the less we will want to provide through [open-market purchases]" (in Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1979a, p. 7). Volcker, therefore, set out a total-reserves-targeting interpretation of the new techniques publicly in October 1979. It is not the case that he—as was implied by Meltzer (2009b, p. 1027)—waited until February 1980 to do so.
[189] See Friedman (1970i, p. 16c; 1980a, paragraph 17, p. 59 [p. 56 of 1991 reprint]). In addition, in principle, total reserves could be insulated thoroughly (and instantaneously—rather than over time, as Volcker had implied) from the effects of discount window lending by offsetting open market operations. Anna Schwartz (in *The Banker* (London), February 1985) emphasized this point. However, even if such continuously offsetting operations failed to take place, it would be expected that very short-term borrowing activities by commercial banks would recede as a source of variation in total reserves behavior over longer horizons.
[190] From his letter (of January 25, 1978) to Representative Parren J. Mitchell. See Friedman (1978d, pp. 156, 157).

what the Federal Reserve should do, Friedman remarked in early 1980, was stick to it (*Wall Street Week*, Maryland Public Television, January 4, 1980, p. 11 of transcript).

However, other aspects of the Federal Reserve's operating arrangements had the effect, in Friedman's view, to frustrate the close control of reserves and of monetary aggregates that nonborrowed reserves control had the potential to deliver. These aspects included the continuation of a subsidized, rather than a penalty, discount rate, and to be linked automatically to a rate like the Treasury bill rate instead of being subject to discrete, administered changes.[191] But the central criticism Friedman made of the new arrangements was in the seemingly esoteric area, discussed in more detail in later chapters, of reserve accounting.

In essence, the matter at issue here related to the fact that, throughout 1979 to 1982 and until 1984, the Federal Reserve maintained its practice, introduced in 1968, of setting commercial banks' required reserves as functions of commercial banks' deposit liabilities two weeks earlier. For Friedman and other monetarists, this lagged reserve accounting (LRA) scheme was a major impediment to control of aggregate reserves and money. Although, as indicated above, Volcker cast the practice of determining nonborrowed-reserves determination as tantamount to the setting of the level of aggregate reserves, this perceived equivalence was, in an important respect, only an aspiration—one that, though achievable over longer periods, was difficult to turn into a reality in any specific week or month.

The ongoing institutional fact was that reserve requirements were set as a function of commercial banks' deposit levels prevailing two weeks earlier—not on current deposits. Required reserves were, therefore, predetermined. Consequently, in the absence (in this era) of a very substantial stock of excess reserves outstanding in the banking system, the predetermined nature of the level of required reserves under LRA obliged the Federal Reserve to create a certain volume of total reserves in any given period. Its prerogatives at this short horizon were limited to choosing the mix between reserves supplied via the discount window, and nonborrowed reserves, supplied through one form or another of open market operations. Furthermore, the lagged reserve requirement arrangement built in a tendency for the Federal Reserve to validate, rather than counteract, deviations of monetary growth from target through its bank-reserve creation today.

These properties led Friedman to judge that LRA had been "an extremely serious hindrance" to

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[191] See, for example, *Newsweek*, July 28, 1980, *Wall Street Journal*, January 30, 1981, and Friedman (1982a, p. 117).

short-run monetary-aggregate control in 1979 to 1982.[192] He would zero in on LRA in numerous critiques that he made of the new procedures during the period from 1980 through mid-1982.

The smokescreen interpretation

Within days of the October 6 launch of the New Operating Procedures, Karl Brunner voiced strong skepticism about the sincerity of the announced change: "The Federal Reserve bureaucracy has grown up with interest rates, and they're not about to let the market set them." (*Wall Street Journal*, October 12, 1979.) The FOMC's means of carrying out its new procedures did not altogether dispel this impression, as, in making a policy decision pertaining to the coming intermeeting period, it continued to specify a tolerance range for the federal funds rate (see, for example, Gilbert and Trebing, 1981, p. 8). Friedman objected to the presence of this tolerance range for the federal funds rate and would urge that it be dropped.[193]

In the assessment of Fred Levin, a Federal Reserve Board staff member at the time, this range meant that there was in a sense in which in 1979–1982 the FOMC and the implementers of the FOMC's directives "never gave up control on a day-to-day, week-to-week, month-to-month, of the funds rate." Rather, they routinely assessed what a particular money-stock and nonborrowed-reserves constellation implied for the federal funds rate, and "when that relationship gave results that they didn't like, they fudged it a little," and "they would jiggle with the formula, so as not to come up with a ridiculous target for the Fed funds rate. So it wasn't really controlling nonborrowed reserves or controlling the money stock on a short-term basis." (Fred Levin, interview, March 10, 2014.)

Almost from the very beginning of the New Operating Procedures period, some outside observers went further—and contended that the Federal Reserve never really made a change toward a reserves-based procedure and continued covert management of the federal funds rate.[194] This "smokescreen" interpretation became prevalent to an even greater extent in retrospectives on the regime. But even a participant in economic policy in 1979, Charles Schultze of the

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[192] See Friedman (1983a, p. 9). See also Friedman (1984a, p. 28; 1985a, p. 54).
[193] See *Newsweek*, July 28, 1980, and Friedman (1982a, p. 104).
[194] An early remark to this effect was Goldfeld's (1982, p. 148) observation that the October 1979 changes arose "perhaps because of a desire to divert responsibility for high interest rates to impersonal 'market forces.'" It also deserves mention that Judd and Scadding (1979, p. 31) had stated that targeting reserves would reduce the perceived responsibility of the Federal Reserve for the level of interest rates. They had, however, indicated that their view that this would be an improvement in public understanding of the fact that many forces other than monetary policy tended to exert pressures on interest rates.

Council of Economic Advisers, was inclined to interpret Volcker's action along these lines: "I thought it was political genius, in the sense of: [Under an explicit funds-rate regime,] you could never have gotten rates up to what he had to get them to." Schultze added, however, that subsequently he had "said this to Volcker a couple of times, and he strongly says no," rejecting Schultze's rationalization for the 1979 operational shift (Charles Schultze, interview, July 9, 2013.)[195] Volcker himself observed that "obviously, that explanation has always annoyed me. I mean, it's true that, when we went to a tough money supply, a disciplined thing, interest rates were going to go up, and we were always aware of that [with regard to] short-term rates." Subject to that caveat, he was emphatic that these rates were, indeed, market-determined under the new procedures (Paul Volcker, interview, October 16, 2013).

In opposition to the smokescreen interpretation, there is, in addition to Volcker's denial, the fact that the federal funds rate during the 1979−1982 period had the features not only of being frequently high but also of fluctuating greatly (with three separate crossings through the 15 percent barrier, according to the monthly-average data, as well as a period of single-digit values during 1980). The fact that the FOMC stated a federal funds rate range in 1979−1982 did not itself imply that policymakers were, in effect, still administering the rate. For the stated range was a wide one, and Judd and Scadding (1979, p. 24) contrasted the "wide-band federal-funds rate constraint" adopted in the October 1979 arrangements with the close control and cautious adjustment of the federal funds rate that the FOMC had exercised until October 1979.

For his part, although, on occasion, Friedman seemed well disposed toward seeing the New Operating Procedures as a smokescreen behind which the FOMC continued to manage rates, that was not his baseline interpretation. On the contrary, during 1980 and 1981, and through mid-1982, Friedman, as discussed in later chapters, often interpreted U.S. short-term interest rates as market-determined rates and not as the result of explicit choices on the part of the FOMC. And in 1984 he buttressed the notion that 1979 had seen a change in operating regime, when he remarked: "A 'monetary policy experiment' *was* conducted from October 1979 to the summer of 1982."[196]

The monetarist critique and the viability of interest-rate practices

An acceptance of the fact that, in October 1979, the FOMC had changed its operating procedures

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[195] On Schultze's dialogue with Volcker on this matter, see also Taylor (1995, p. 780).
[196] Friedman (1984g, p. 397, emphasis in original).

did not imply an acceptance of the fact that the change was permanent. And, indeed, Friedman's own perspective on the change was, as noted above, permeated with doubt about its likely permanence. So, too, was the reaction of James Pierce, a former member of the economist staff at the Federal Reserve Board. Although he did not deny outright that a move of substance had occurred in the FOMC's policy procedures, Pierce, speaking in the days after the October 6 shift, did question whether it amounted to a lasting or "real move." Rather, Pierce suggested, "the Fed wants to increase interest rates and doesn't want to be blamed for it" and would, he predicted, revert to the system of targeting the federal funds rate once interest rates peaked (*San Jose Mercury* (California), October 14, 1979, p. 1E).

Pierce's prediction that a peak in interest rates would be followed by a prompt end to the New Operating Procedures was not really borne out. Instead, as already noted, the federal funds rate (measured using its monthly average) moved above 15 percent on three separate occasions during the 1979–1982 period. It is, nevertheless, true that the New Operating Procedures did not last, that the FOMC resumed management of the federal funds rate in the second half of 1982, and that the level of short-term interest rates was thereafter generally in a lower range than had been the case for most of the New Operating Procedures period.

The Federal Reserve itself left the door open for a return to targeting the federal funds rate in its announcement of October 6 when it indicated that its management of nonborrowed reserves was an arrangement instituted "for the time being" (*Financial Times* (London), October 8, 1979*a*). The termination in 1982 of the New Operating Procedures no doubt largely reflected dissatisfaction with the interest-rate volatility associated with those procedures and the traditional central-bank preference, noted by Friedman and by Karl Brunner in the 1979 quotations given above, for managing short-term interest rates. But, as will now be discussed, there was, beyond this, likely a deeper macroeconomic reason for the ephemeral character of the Federal Reserve's adoption of the New Operating Procedures: in the 1970s, there were greater analytical and historical foundations for interest-rate-oriented policies than monetarists tended to concede.

Among those prepared to accept an influence of monetary policy on aggregate demand and prices, there existed, as of 1979, a long historical tradition accepting the interest-rate management as a means of steering economic conditions. Although the modern research literature in favor of interest-rate rules—such as the contributions of McCallum (1981) and Woodford (2003)—had yet to appear, there was available a literature of a past vintage that

advocated interest-rate management for economic-stabilization purposes—most notably Wicksell (1935, 1936) and Keynes (1930).

What was also present in the postwar decades through 1979 was a more informal acceptance in practical circles of the idea that interest-rate management could work as a means of stabilizing or destabilizing the economy. Even hardline Keynesian Seymour Harris had conceded this point, albeit in a backhanded manner, when he testified in 1958: "as far as I know the economic history of this country, it has never been true when we introduced a high-rate money policy that this has not been followed by a recession or a depression."[197]

Harris, as it happened, had been a member of a camp that had more confidence that a high interest-rate policy could dampen output rather than that it could subdue inflation. This perspective was, as discussed in the next section, becoming increasingly rare by late 1979—although it was certainly still present in some of the reactions by economists, other commentators, and members of Congress in that period.[198] Predominantly, the need to raise interest rates to fight inflation was understood by the time Paul Volcker became Federal Reserve chair. For example, during the early months of Volcker's tenure, when the FOMC was still targeting the federal funds rate, John H. Perkins, president of the Continental Illinois Bank and also of the American Bankers Association, observed: "Our essential problem is inflation. If we restrain the supply of money to deal with inflation, we get high interest rates. The alternatives—such as starting the printing presses, mandatory controls, and other approaches—do not solve the problem." (*Chicago Sun-Times*, September 7, 1979.)

Of course, an important element of an appropriately disinflationary interest-rate reaction was likely to be the requirement that real, and not just nominal, interest rates rose in the tightening process. But, by 1979 at least, this aspect of the problem had also become well understood at the policymaker level. Specifically, internal FOMC discussions in the 1978–1979 period did generally show an understanding that real short-term interest rates needed to rise.[199]

In this connection, the critique that Friedman and other monetarists made of the Federal

[197] From Harris' testimony of April 24, 1958, in Committee on Finance, U.S. Senate (1958, p. 2014).
[198] On the reaction of members of Congress on this matter, see Nelson (2005), as well as the ambiguous statement to Volcker at a hearing of October 15, 1979, by Senator Robert Morgan (D–NC): "Mr. Volcker, I'm not sure that I understand where we're going with your high interest rates, but I would agree that you probably had little choice." (In Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1979a, p. 17.)
[199] See Nelson and Schwartz (2008, p. 845) for a consideration of the FOMC deliberations in this era on this point.

Reserve's pre-1979 policy can itself be criticized for eliding the distinction between the *need for lower monetary growth* and the need for *new operating procedures*. As we have seen, they regarded the latter as helping accomplish the former, because they felt that short-term interest rates were being adjusted too sluggishly to deliver successful monetary control and noninflationary monetary growth. This begged the question, however: Could not the Federal Reserve have simply kept the federal funds rate as an instrument and resolved to adjust it more promptly in response to economic conditions? After all, episodes that Friedman had referred to generally favorably—as cases in which monetary policy provided an acceptable average economic performance—included the 1920s and 1950s decades in the United States and the experience after 1973 in Japan. These instances had not been associated with an abandonment of an interest-rate instrument. Instead, the central bank had adjusted the interest rate in such a way as to secure noninflationary monetary growth rates.

Friedman emphasized in his August 20 *Newsweek* column, after Volcker's ascension, the need for a reform of Federal Reserve operating procedures—specifically, one that went in the direction of dropping the federal funds rate as the Federal Reserve's policy instrument. He recognized that the Federal Reserve had not targeted the federal funds rate for its own sake, but with other economic objectives in mind. As already indicated, Friedman acknowledged, in particular, that federal funds rate decisions during the 1970s had been guided by monetary-aggregate growth targets.[200] He also granted that achievement of these targets was in principle compatible with the central bank's employment of an interest-rate instrument—so that the choice between short-term rates and reserves as a policy instrument was, in principle, a choice concerning tactics, not strategy.[201] Friedman even, on occasion, referred to the scope available to improve monetary-growth control in the context of the existing operating procedures. In so doing, he was implicitly indicating that the FOMC could formulate a more stabilizing interest-rate reaction function than the one they were so far employing.[202] Friedman and other

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[200] See the discussions in previous chapters, as well as specific Friedman items such as Instructional Dynamics Economics Cassette Tape 52 (June 10, 1970), Friedman (1970i, 1971c), and *Newsweek*, December 8, 1975.
[201] See Friedman (1971c, p. 26; 1980a, paragraphs 10 and 14, pp. 57, 58 [pp. 53 and 54–55 of 1991 reprint]) and Instructional Dynamics Economics Cassette Tape 179 (October 1975, Part 2), as well as the discussion in Nelson and Schwartz (2008, p. 849). Prior to the late 1970s, the recognition at the U.S. policymaker level that the real interest rate likely had likely been rendered unduly low by inflation was marred by policymakers' view that the appropriate relationship between nominal interest rates and inflation could be restored by the deployment of nonmonetary instruments against inflation. See previous chapters as well as Nelson (2020b, Chapter 15) and DiCecio and Nelson (2013, pp. 406–407).
[202] For example, in mid-July 1976, in Instructional Dynamics Economics Cassette Tape 195 (July 1976, Part 1), Friedman suggested that improved monetary control was possible even under "their present and unnecessarily imperfect techniques."

monetarists felt, nevertheless, that the interest-rate instrument choice was very prone to have the effect of limiting the Federal Reserve's achievement of its monetary targets, so that the choice of tactics had contributed to the failure by the authorities to achieve their monetary strategy.

Meltzer (2009b, p. 1017) contended that monetarists grasped the point that a successful interest-rate policy—one that was stabilizing with regard to economic outcomes—was feasible, but that "they did not emphasize the last point and insisted on the importance of controlling money directly." This characterization applies well to Friedman. In retrospect, it appears appropriate to regard Friedman as having been too skeptical of the possibility of delivering stabilizing interest-rate rules in practice, even though he did not claim that this was inherently unachievable. He would then be caught off-guard by the extent to which the Federal Reserve, after 1982, was able to reconcile what he regarded as outmoded central bank practices, like interest-rate management, with the stabilization of real activity and inflation.

III. PERSONALITIES, 1979

PAUL VOLCKER

Even in 1979, however, Friedman was able to keep strategy and tactics sufficiently separate in his thinking about U.S. monetary policy for him to see that, in a realm basically distinct from the issue of appropriate operating procedures, there were important aspects of policy strategy—in the analysis of the reasons for, costs of, and solution for inflation, and of the relationship of inflation both with interest rates and with real variables—that needed to change from past practices, in order for successful disinflation to occur. Paul Volcker's becoming Federal Reserve chair provided an opportunity for this to happen. In important respects, by 1979, Volcker's thinking on these issues overlapped with Friedman's own.

Interactions between Volcker and Friedman

When, in the spring of 1974, Volcker's departure after more than five years at the U.S. Treasury was announced, the Associated Press noted: "Although Volcker is not widely known in the United States, he is well known in international monetary circles…" (*Washington Star-News* (Washington, D.C.), April 9, 1974.) The specialized nature of Volcker's eminence was also alluded to more than five years later when, at Volcker's swearing-in as Federal Reserve chair, President Carter observed, "Internationally, Paul Volcker is well known," while adding that "Mr.

Volcker is a man who has devoted his entire professional career to dealing successfully with the complexities of monetary theory and practice and international finance."[203]

By mid-1978, as Friedman recalled it, he had known Volcker "for many years."[204] It was in the course of the Volcker activities that Carter referred to in the field of international finance—during Volcker's tenure in the Nixon Administration as Under Secretary of Monetary Affairs from 1969 to 1974 (see, for example, Europa Publications Limited, 1986, p. 1663), in practice a position largely concerned with the international monetary system—that Friedman first met Volcker. Having left that position, Volcker was back in public office before long—this time mainly concerned with *domestic* monetary and financial issues—as president of the Federal Reserve Bank of New York starting in 1975. With regard to Friedman, Volcker recalled that during his bank presidency, "I met with him a few times when I was in New York [and Friedman was visiting the city]. He was always mad at the Federal Reserve." (Paul Volcker, interview, October 16, 2013.)

Beyond their discussions of current policy developments, Friedman and Volcker had an exchange in mid-1978, when Friedman wrote to the bank president to ask permission to film, for the *Free To Choose* series, a sequence in the Federal Reserve Bank of New York's gold vault.[205] Volcker recalled that, in Friedman's proposal, "he had a video [that is, a television crew], and he wanted to take a picture of the gold vault." Volcker indicated that "I wouldn't let him. And he got very annoyed with me (*laughter*), because I didn't give him the great privilege of [agreeing to the *Free To Choose* camera crew] photographing him in our gold vault so he could talk about the Gold Standard." As for the reasons for his refusal, Volcker observed: "Oh, I just thought, you know, we can't—we're not going to make a special deal for Milton Friedman, of all people. He could go down and *visit* the gold vault. He just couldn't take a crew down there and photograph it and use it as a prop while he was criticizing Federal Reserve policy. (*laughter*)." (Paul Volcker, interview, October 16, 2013.)[206]

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[203] In Carter (1979e).
[204] Friedman and Friedman (1998, p. 486).
[205] See Friedman and Friedman (1998, pp. 485–487).
[206] The Federal Reserve Bank of New York's disclination to allow filming in the gold vault paralleled the situation faced in the mid-1960s by the makers of the James Bond film *Goldfinger*, who were denied permission to film inside Fort Knox's gold vaults or take photographs of the interior for research purposes (*Evening Capital* (Annapolis, Maryland), December 19, 1964). Another connection between Friedman and *Goldfinger* arose through the television series *Yes Minister* and *Yes, Prime Minister*, screened in the United Kingdom during the 1980s. Actor Richard Vernon, who guest-starred a few times in the two series, played a character who was ultimately (in the 1987 *Yes, Prime Minister* episode "A Conflict of Interest") appointed governor of the Bank of England. In *Goldfinger*, Vernon had played a senior Bank of England official. The coauthor of all the episodes of both series was the *Free*

When reminded that he ultimately *did* grant permission for Friedman and his crew to film some footage on the bank premises, Volcker replied. "I think maybe. I don't think of the gold vault, [though]." (Paul Volcker, interview, October 16, 2013.) In the event, Friedman's account in *Two Lucky People* stated unambiguously that the location filming included "a scene filmed in the gold vaults of the Federal Reserve Bank of New York" and, in quoting a thank-you letter he wrote to Volcker in 1978, implied that he and his team may have filmed in both the gold vault and the meeting room of the bank's board of directors.[207]

It appears that both Volcker's and Friedman's recollections were each correct to some extent. The *Free To Choose* episode on the Great Depression had footage of Friedman in the bank's board room and also of Friedman standing *just outside* the bank's gold vault, but not within the gold vault itself—a situation that necessitated him telling viewers: "Inside is the largest hoard of gold in the world."[208] So Friedman did not actually get to stand before the cameras amid the gold holdings and so use them directly as an on-screen backdrop. Those holdings, as it happened, became even more valuable in between the time of Friedman's filming the sequence in 1978 and the broadcast of it in 1980: the annual-average price of gold was $193 per ounce in 1978, rose to $306 in 1979, and reached a twentieth-century peak of $615 in 1980.[209]

As Federal Reserve chair, Volcker would be regarded by contemporaneous and retrospective commentaries alike as putting Friedman's ideas into practice, particularly during his early years in office. But it should be stressed that Volcker was by no means someone who professed to know the chapter and verse of Friedman's writings. And although, as discussed below, he did absorb many key points Friedman had advanced, and his policies reflected this absorption, he had many disagreements with Friedman on many specifics of monetary economics. In addition to having much divergence with Friedman on normative matters, Volcker also did not have a very favorable view of Friedman's empirical work on money demand. Volcker recalled that, when he was a staff economist at the Federal Reserve Bank of New York in the 1950s, he was

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To Choose series' creative supervisor, Antony Jay, who recalled that the fact that Vernon played a senior Bank of England official in both *Goldfinger* and *Yes, Prime Minister* was not intentional, adding: "Of course, he was the ideal kind of character [actor] for that." (Sir Antony Jay, interview, May 29, 2013.) In one of Vernon's appearances in *Yes Minister* ("The Quality of Life," broadcast March 30, 1981), his dialogue included one of a couple of mentions that Friedman received in the series.

[207] Friedman and Friedman (1998, p. 486).

[208] *Free To Choose*, PBS, Episode 3, "The Anatomy of Crisis," January 26, 1980, pp. 1 and 4 of transcript (quotation from p. 4). As for the use of the board room: By 1978, meetings of the Federal Reserve Bank of New York's board were not central in monetary policy. But they were had played a more important role in the interwar period with which the "Anatomy of Crisis" episode was concerned.

[209] See *https://nma.org/wp-content/uploads/2016/09/historic_gold_prices_1833_pres.pdf*.

asked to analyze a study of velocity that Friedman had written. Volcker's memory of this exposure to Friedman's analysis was not a favorable one: "I remember I first, you know, had an intellectual 'meeting' [with Friedman], if that's the right word" when carrying out that analysis. "And my memory is I was given a copy of his great opus [written] with his partner. And they [the bank hierarchy] said, 'What do you think of this?' And I looked at it, and I said I didn't think much of it. But what I remember about it… is that he concluded that money was a luxury good, in effect—and that the velocity of money circulation would decline, you know, as a matter of long-term trend… But no sooner did he say that than the velocity then began increasing." (Paul Volcker, interview, October 16, 2013.)

Indeed, this negative impression of Friedman's predictive power was something that Volcker would affirm at a Federal Open Market Committee meeting during his period as chair. In the course of a meeting session held on July 9, 1986, after a fellow Board governor had been talking about the early postwar behavior of U.S. monetary velocity, Volcker remarked: "That's when Milton Friedman wrote his great tome saying there was an inexorable secular decline in velocity." Volcker then added caustically: "At which point, it rose from 2 to 4."[210]

Volcker left his Federal Reserve staff position to work at the Chase Manhattan Bank as an economist in the period 1957−1962 (Europa Publications Limited, 1986, p. 1663). It may be that his recollection of studying the Friedman piece on velocity actually pertained to an experience while in the latter position, rather than at the Federal Reserve. In any event, it is likely that the Friedman study that Volcker scrutinized was a version of Friedman's 1959 demand-for-money study or perhaps a draft section of the Friedman-Schwartz *Monetary History*.[211] Indeed, Volcker's description of the Friedman manuscript had similarities to arguments laid out in Chapter 12 of the *Monetary History*, "The Postwar Rise in Velocity," in which the authors examined the velocity rise of the 1940s and 1950s and suggested that it was unlikely to continue.

There may, however, be a semantic issue involved in Volcker's conviction that Friedman was proved wrong by events. M1 velocity indeed continued to rise in the 1960s and 1970s, and an M1-type definition of money was the money-supply concept most emphasized by the Federal Reserve through about the mid-1980s. Friedman and Schwartz, however, were concerned with an M2 concept. And M2 velocity did not rise during the 1960s and over most of the 1970s. It was, instead, largely trendless in the 1960s and, as discussed below, into the 1970s. Indeed, as

[210] Federal Open Market Committee (1986, p. 46).
[211] The former item was Friedman (1959).

discussed in Chapter 3 above, Friedman actually took pride in the Friedman-Schwartz prediction that velocity's rise (evident in the first decade after World War II, regardless of the definition of money) would cease in the case of M2 (see Chapter 2 above). That said, Friedman's solo work and Friedman and Schwartz's joint analyses in the 1950s and 1960s were indeed predicated on a belief, which they had repudiated by the end of the 1970s, that the income elasticity of M2 was well above unity.[212] Thus, as Volcker implied, their analysis in the 1950s and 1960s suggested that money was a luxury good and pointed toward a mild declining trend in M2 velocity for the 1960s and 1970s. And this decline, indeed, did not materialize.[213] It was only really in the early 1970s that Friedman embraced a model of money demand that did not imply a trend in M2 velocity arising from growing real income.

The trendless character of M2 velocity was well enough established that Robert Gordon saw fit to refer to it in a January 1980 retrospective on macroeconomic developments in the 1960s and 1970s.[214] With regard to the definition still in use in 1979, however, an important qualification was in order: that series' velocity had risen about 2.1 percent per year in the second half of the 1970s (that is, the period 1975:Q1−1979:Q4) (see Simpson, 1980, p. 105). Friedman himself recognized this fact in his *Newsweek* column (August 20, 1979) heralding Volcker's tenure, when he plotted the trend lines of M2 growth and inflation since 1960 and noted "the steeper trend in inflation than in money." Some of this discrepancy was due to the productivity-growth decline. But part of it was due to the increase in M2 velocity from 1975 to 1979—something that Friedman indicated was "no mystery," as it reflected Americans' rational decision to move out of monetary assets in the face of ongoing inflation. As discussed in Chapter 8, much of the flight out of M2 to which Friedman referred was into assets then outside M2—such as deposits at thrift institutions and shares in money market funds—whose interest rates were more closely linked to market rates. When these assets were included in the redefined M2 series in early 1980, the new M2 had a velocity that was largely trendless since the mid-1950s and, in particular, exhibited a much smaller increase since 1975 (see Simpson, 1980, p. 105).

From Miller to Volcker

For velocity, therefore, whether the inflationary environment had imparted an upward trend

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[212] See Nelson (2020b, Chapter 12) and Chapter 2 above.

[213] Friedman (1960a, p. 91) laid out his money growth recommendation on the assumption of a 1 percent decline in velocity.

[214] Gordon (1980, p. 125). Gordon gave the "remarkable constancy" of M2 velocity as lasting until 1977 (p. 125) while also stressing that he was referring to the pre-1980 definition of M2 (p. 101).

depended on the specific time series used. This was not the case, however, for another variable: U.S. market interest rates. Whichever interest-rate series one considered, an upward trend in nominal rates was unmistakable by the end of the 1970s.

By late 1979, the upward trend in interest rates on longer-maturity U.S. government bonds was reaching an especially notable watershed. The longer-term Treasury securities market had been placid in 1976 and 1977 and into early 1978, even as Friedman had increasingly warned about coming double-digit inflation. Indeed, in December 1977, the average value of the ten-year secondary Treasury security rate was, at 7.69 percent, actually below the average value prevailing in December 1975. As late as September 5, 1979, Miller's successor Paul Volcker was able to testify that longer-term interest rates had not risen much during the Federal Reserve's tightening.[215]

Bearing Volcker's testimony out, the average ten-year rate had, in August 1979, averaged 9.03 percent—virtually the same level as that prevailing in December 1978. But, from late summer 1979 onward, the situation changed at a rapid pace. Much of the rise in longer-term rates that then occurred—shown in Figure 1—amounted to a rise in expected long-term inflation—which had "picked up markedly" by the end of 1979 in the judgment of Levin and Taylor (2013, p. 238) on the basis of a decomposition of long-term rates (see also Ang, Bekaert, and Wei, 2008) and an examination of other measures of longer-term inflation expectations.

This was a distinctly different situation from the earlier period of double-digit inflation in 1974–1975. To be sure, bond rates had risen considerably in the mid-1970s, but they had nevertheless implied an expectation in the bond market that conditions close to price stability would be resumed within a few years. With the late-1979 data in hand, Charles Nelson's judgment was that, over the second half of the 1970s, "the yield curve… sort of broke loose… The level [of the curve] broke loose. Whereas, before, the long end of the yield curves seemed very well anchored." (Charles Nelson, interview, September 9, 2013.)

The breaking loose was a development acknowledged by the U.S. Treasury when it marketed a new long-term bond in the fall of 1979. In his September 5 testimony, Volcker, while referring to the constancy of long-term rates, had also contrasted that "sitting here in 1979," it was a considerable contrast in the atmosphere with twenty years earlier: "1959, when one could assume

[215] See Volcker (1979d, p. 740).

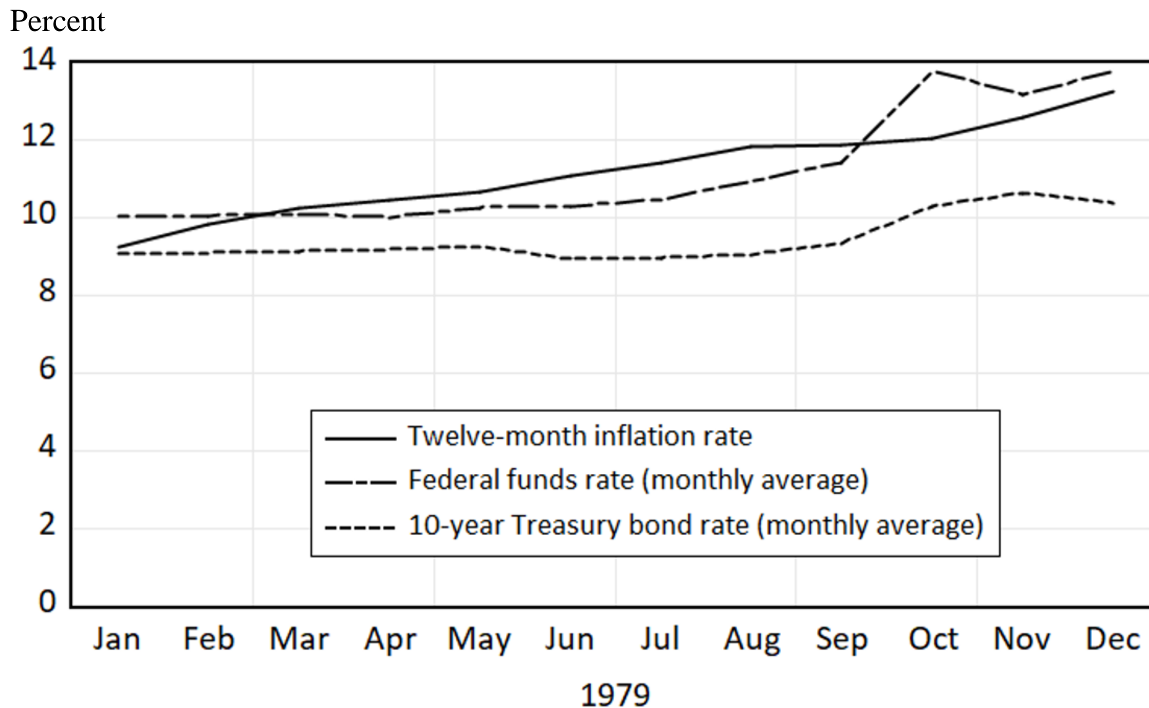

Figure 1. CPI inflation, federal funds rate, and U.S. Treasury ten-year bond rate, 1979.
Source: Federal Reserve Bank of St. Louis' FRED portal.

a fairly decent record in price stability, [and] one could assume that people did not expect big changes in prices and a big generalized inflation."[216] In fact, as already noted, the year 1959 had been characterized by fears of upcoming notable inflation.[217] But whereas in 1959, sizable inflation had amounted to a highly exaggerated fear, in 1979 it was a reality.

Furthermore, even the degree of inflation feared in 1959 was small in scale compared with both actual and expected inflation in 1979. This contrast was reflected in the U.S. Treasury's behavior with regard to bond issuance in the two periods. In October 1959, it acknowledged inflation fears by issuing a high-coupon longer-term bond. These securities became known as "Magic Fives," on account of their roughly five-year maturity and their 5 percent stated interest rate (*Capital Times* (Madison, Wisconsin), October 27, 1959), the latter being the highest coupon rate on a Treasury bond since 1921. In contrast, on October 11, 1979, the Treasury issued a long-term bond bearing a coupon rate above 10 percent for the first time (*Kansas City Times*

[216] From Volcker's testimony of September 5, 1979, in Committee on the Budget, U.S. House of Representatives (1979, p. 298).
[217] See Romer and Romer (2002a) and Nelson (2020a, Chapter 10).

(Missouri), October 12, 1979*a*). The secondary market's ten-year Treasury rate also crossed 10 percent on a monthly-average basis in October 1979 and would stay in double digits for several years.

The rise in the ten-year rate reflected, of course, not only the increase in inflation expectations but also the monetary policy tightening under Volcker. Likewise, the rise to 9 percent under Miller had partly reflected the considerable monetary policy tightening under Volcker's predecessor. Volcker, indeed, expressed the element of continuity with Miller when he testified on October 17, 1979 (Volcker, 1979a, p. 889): "In one sense, the Federal Reserve actions announced on October 6 were part of a continuing effort to maintain control over money and credit expansion. Our basic targets were not changed." Volcker was following the same monetary targets for 1979 that Miller had pursued, and the federal funds rate had already been rising under Miller.

Indeed, as stressed in Chapter 8 above, the growth rate of M2 had come back into single digits during Miller's tenure, in the wake of the second monetary explosion of the 1970s. This was a fact in which Miller took pride. As he observed in the mid-July hearing held shortly before his departure from the Federal Reserve was announced: "In March 1979, one year after I joined the Fed, the growth rate of the monetary aggregates was slower." Miller added immediately with regard to that remark: "Even the [Federal Reserve Board] staff is nodding"—a less-than-subtle reference to Miller's continuing problems in developing a solid relationship with his staff economists, some of whom were in the audience at the hearing.[218] Lyle Gramley, who had moved from the Board to membership of the CEA by the time of Miller's tenure as chair, kept in touch with former Board colleagues and recalled, with regard to their impressions of Miller, "Yeah, I heard stories. (*laughter*)… [They were] not very favorable." (Lyle Gramley, interview, June 24, 2013.)

Miller, having already had his public reputation damaged by Federal Reserve Board dissension over discount-rate decisions in 1978 (see Chapter 8), encountered further public criticism during 1979 as inflation proceeded. Miller was rebuked heavily in the media on the basis of economic developments and on his perceived shaky command of issues related to monetary policy, with such commentaries appearing not only in the specialized financial press but also in other outlets, with conservative commentator William Safire proclaiming in his *New York Times* and

[218] From Miller's testimony of July 17, 1979, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1979b, p. 73).

syndicated column that Miller "has been… a disaster." (*Kansas City Star* (Missouri), June 5, 1979.) For his part, Friedman referred to Miller directly very little in his own public comments during 1979.

Although critical of the uneven way in which monetary policy had been tightened under Miller, Friedman had reason to be pleased with various other aspects of the Miller regime. A number of statements that Miller made during 1979 were along the same lines as Friedman had given over the years. For example, in Congressional testimony given in February, Miller made the case for continued aggregate restraint as follows: "I think we have to have the persistence, the discipline, the willingness to pursue austerity for a period, in order to make up for our past excesses. There can't be a free lunch in this process; we just have to pay the piper for what has happened over 12 years."[219] And, in making policy prescriptions outside the field of monetary policy, Miller seemed largely on the same page as Friedman: Miller urged restraint in government expenditure, and in mid-July 1979 told his Congressional interlocutors that the Carter Administration should "decontrol oil prices immediately."[220]

By this late stage of his tenure, Chairman Miller had gone through a notable period in which he was subject to criticism by other economic arms of the federal government. In early 1979, when—in contrast to its opposition to interest-rate increases the FOMC took under Burns—the Carter Administration urged, in a semi-public manner, that Miller pick up the pace of interest-rate increases: the London *Financial Times*, for example, referred to "recent public disagreement over interest rates between Treasury and Federal Reserve officials" (April 23, 1979), and Paul Samuelson also mentioned the dispute in his *Newsweek* column (April 30, 1979).[221]

Charles Schultze recalled that, in that period, "Mike Blumenthal, the Secretary of the Treasury, and I were two of the people giving him flak." Blumenthal presented Miller with a memorandum giving his and Schultze's arguments for a tighter monetary policy (*Washington Post*, May 6, 1979). Schultze observed that his own estimation at the time was that "the economy was actually getting to the point where it could be overheated—which had not been true until then. And, taken together with oil-price problems, we thought that was dangerous, and

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[219] From Miller's testimony of February 22, 1979, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1979a, p. 79).
[220] From Miller's testimony of July 17, 1979, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1979b, p. 89).
[221] Earlier, the emerging dispute had been discussed by political commentators Rowland Evans and Robert Novak (*Omaha World-Herald* (Nebraska), April 10, 1979).

that the Fed [should tighten more]." In contrast, Schultze noted, "Miller probably had a more pessimistic GDP economic forecast, etc. But, in any event, we were pretty sure he was already too easy. And we kind of leaked this out [to the media]. And you didn't have to go very far to figure who and where the leaks were coming from. So both of us got a memo, [with a note by President Carter] in the margin saying, 'You just leave Miller alone.' So I remember that. So, when you're talking about too much flak on Miller, I know where some of it came from." (Charles Schultze, interview, July 9, 2013.)[222]

It was this brief episode in which the Carter Administration urged that Miller tighten policy at a more rapid pace, that likely helped cement the misconception (already discussed in Chapter 8) that Miller eased monetary policy or reduced the federal funds rate during his tenure. Miller certainly contributed to the myth that he cut interest rates as chairman by publicly foreshadowing imminent interest-rate increases that did not, in fact, come about.[223] In July 1978, for example, he had remarked: "I said yesterday that I believe the interest rates will peak between now and the end of the year, and that, next year, we could see a reduction in interest-rate pressures. I continue to believe that…"[224] In contrast, the trajectory of the federal funds rate under Miller was decidedly upward-sloped. In December 1978, the federal funds rate reached a monthly average above 10 percent for the first time since 1974. And, in July 1979—Miller's last full month heading the Federal Reserve—the average federal funds rate was, at about 10.5 percent, over 250 basis points above its level of a year earlier.

Volcker, vice chairman of the FOMC during Miller's tenure, attended almost all of the FOMC meetings at which Miller presided and mostly voted with Miller at the meetings, even though he favored a more rapid increase in the federal funds rate than that Miller put forward. The gap between their views became more a matter of public record, however, when Volcker actually dissented from the majority FOMC vote on two occasions. At the FOMC meeting held on March 20, 1979, Volcker and three other Committee members voted for a "somewhat more restrictive policy posture" than that implied by the slight increase in the federal funds rate for which the majority had voted (Federal Open Market Committee, 1979a, p. 416).[225] Volcker also

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[222] See also *Washington Post*, May 6, 1979.

[223] For further discussion of Miller's position on making projections of interest rates, see Nelson (2005, 2021).

[224] From Miller's testimony of July 28, 1978, testimony, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1978d, p. 64). By the middle of the following year, Miller had sworn off predicting interest rates, and when asked to do so in a session with journalists, he joked: "When[ever] I do, reporters all leave the room to call the brokers, and that breaks up the press conference." (*St. Louis Globe-Democrat*, June 15, 1979.)

[225] It was unusual in FOMC meetings in the 1970s for the vice chair to enter a dissenting vote, but not unprecedented when Volcker did so. Notable earlier occasions were dissents by then-Federal Reserve Bank of President Hayes in

dissented at the next meeting four weeks later (Federal Open Market Committee, 1979b, p. 491). His dissents occurred in the time during which Carter Administration figures were also urging a sharper increase in rates. This same period corresponded, ironically, to one of the rare times when Friedman would likely have favored Miller's monetary policy position over Volcker's, as Friedman believed that a too-rapid tightening (as measured by monetary growth) had recently occurred and that monetary growth should be raised above its recent rates (see Section I above).

By the time Miller decided to leave the Federal Reserve the following July, he and Volcker were again allies in FOMC votes, and Miller favored Volcker as his successor. When Miller departed the Federal Reserve, it was not, as some revisionist accounts implied, an ignominious easing-out but, instead, a move to one of the most prestigious positions in the executive branch. In becoming Secretary of the Treasury, Miller received what was widely perceived at the time as, in effect, a promotion. Indeed, Federal Reserve Board staff in the summer of 1979 were blindsided by Miller's departure, as Robert Solow recalled. From what was said to him by "friends or former students on the [Board] staff, in conversation," Robert Solow noted that "they thought that it was shabby, and contemptuous of the Fed, to take that job [as chair], and then just leave it right away—even to be Secretary of the Treasury." The fact that Miller "became a member of the Federal Reserve Board, and left it almost immediately when he got a better offer, as he thought a better offer to be Secretary of the Treasury [to be]—I think [in doing that] he did a lot of damage to the morale of the Fed. And one of Paul Volcker's achievements, while maybe not as important as some of the others, was to restore morale to the staff of the Federal Reserve." (Robert Solow, interview, July 7, 2014.)

The growing acceptance among commentators in the late 1970s that inflation was a monetary phenomenon meant that Miller often received the blame for the double-digit inflation that came about during his tenure. Retrospective accounts have typically assigned even more responsibility to Miller for high inflation. As emphasized in Chapter 8 and in the discussion above, that judgment seems misplaced, as the breakout of inflation that occurred during Miller's incumbency probably reflected, in large measure, monetary policy actions taken in 1976 and 1977, before he became chairman. For his part, Friedman acknowledged that monetary growth had been brought down in 1978 and that the early 1980s disinflation could therefore be traced in part to the monetary tightening that commenced under Miller. And in a discussion that Friedman

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the FOMC meetings of January and February 1972. See Federal Open Market Committee (1972b, p. 8; 1972c, pp. 8–9). Hayes' dissent was mentioned by Friedman in Instructional Dynamics Economics Cassette Tape 99 (May 17, 1972).

had with his friend in Michael Mork in April 2006, the month after Miller's death, Miller's record as Federal Reserve Chairman came up. A chart of M2 growth that Mork had prepared reminded Friedman that Miller had overseen a major reduction in monetary growth. Friedman remarked that a reevaluation of Miller's record was required that gave Miller due recognition for his part in the early-1980s disinflation. (Michael Mork, personal communication, May 8, 2013, and October 30, 2013.)

Miller's successor Paul Volcker likewise regarded Miller's tenure as having positive attributes. "You know, from my perspective—I was in New York then—he *was* too easy. (*laughter*)… [But] he wasn't terrible. You know, he was an intelligent man—and did a number of things: [among them,] he shepherded [to Congressional approval] the Monetary Control Act [of 1980] for a while, while he was there. And, administratively, in the Federal Reserve, I think he did a pretty good job… [So] the [overall] feeling that he was a weak chairman was a little bit unfair." (Paul Volcker, interview, October 16, 2013.)

But while Miller's tightening of monetary policy in the second half of 1978 had provided some foundations, it was when Volcker became chair that a major change in the perspective on, and direction of, U.S. economic-stabilization policy was really in the offing. This helped put into practice policies that reflected a sentiment that Volcker had made in early 1977 (Volcker, 1977, p. 25): "In a sense, the long run of which the monetarists speak has caught up with us."

Resolve against inflation

Friedman had observed in late 1973 that, at some point in the decade ahead, the American public would get so fed up with inflation that it would countenance a long recession to restore price stability (Instructional Dynamics Economics Cassette Tape 136, December 13, 1973). At a dinner during mid-1979 with William F. Buckley and John Kenneth Galbraith, held at Galbraith's summer home (which, like Friedman's was in Vermont), Friedman suggested that the United States might have, indeed, reached a point at which a disinflationary policy might prove sustainable. Friedman told Buckley and Galbraith that his grounds for believing this was that inflation was hurting everyone and that the U.S. citizenry had come to resent it more than it did unemployment, especially in view of widespread recognition that the natural rate of unemployment had risen and that the hardship associated with being unemployed was typically less than it had been in the 1930s. (*Daily News* (New York), November 7, 1982.)

But if a multi-year disinflationary policy was now more feasible, what form should it take? In a *Newsweek* column during this period (June 18, 1979), Friedman suggested that President Carter should ask Congress to pass a law requiring that the Federal Reserve strictly follow a multi-year program of reductions in monetary growth. That, of course, did not happen. But Volcker did, in effect, unilaterally introduce a multi-year, monetary-policy-focused attack on inflation. The multi-year aspect of it was largely unwritten.[226] But Volcker laid out his intention of moving to price stability by continuation of disinflationary monetary policy settings: "I think we're going to have to exercise our efforts to restrain growth in money and credit indefinitely."[227]

In the process of U.S. economic policy reaching this point, there were relevant factors going beyond the fact that public opinion that had, indeed, changed in a crucial manner. In particular, and very crucially, it was also the case that the Federal Reserve's thinking about the inflation process had undergone an overhaul. Volcker would come to recall the October 1979 changes in the following way: "We said, in effect, that the United States was experiencing high inflation that needed to be dealt with and that inflation is a monetary phenomenon."[228]

Resolution of the cost-push dispute

The corollary of the Federal Reserve's acceptance of inflation as a monetary phenomenon was a repudiation, on its part, of the cost-push views of inflation that until 1979 been prevalent in the Federal Reserve leadership. As discussed below, this shift at the Federal Reserve mirrored a similar change underway in the economics profession at the time. Nevertheless, the shift took place at the policymaker level of the U.S. central bank even as cost-push views remained pervasive in the nation's executive branch. For, as the Federal Reserve tightened policy during 1978 and 1979, the Carter Administration articulated the view that inflation could be fought by nonmonetary devices as well (Romer and Romer 2002b; Nelson, 2005). So an irony is that, even though Friedman in his June 1979 *Newsweek* column had suggested that the way to secure a monetary policy-centered disinflation program was to strip the Federal Reserve of its monetary policy independence, it was the independence of the Federal Reserve that allowed it to launch

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[226] To some extent, the requirement that the Federal Reserve gradually move to price stability was already embedded in laws discussed in Chapter 8: the Federal Reserve Reform Act of 1977 and the 1978 Humphrey-Hawkins legislation.
[227] From Volcker's testimony of October 15, 1979, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1979a, p. 26).
[228] Volcker (1994, p. 146).

under Volcker a disinflationary program that was based on a perspective on inflation's causes that ran against the grain of that to which the Carter Administration subscribed.

In 1979, the administration continued to articulate a cost-push perspective both through its policy proposals and its public analyses of inflation's causes. With regard to the former, having already introduced wage guidelines in 1978, President Carter attempted to put his incomes policy on a more formal footing by detailing, in January 1979, a tax credit that would be given to firms that kept nominal wage increases below 7 percent a year (*Manchester Union Leader* (New Hampshire), January 30, 1979). The scheme—the "real wage insurance" scheme that was discussed in Chapter 8 and was based on "tax-based incomes policy" ideas—was not adopted into law. The fact that it was advanced is, nevertheless, notable as the proposal exemplified the administration's continuing adherence to wage-push perspectives on inflation that was rapidly becoming rare in academic circles, in financial markets, and in the Federal Reserve.

In testifying in favor of the real wage insurance scheme, then-Secretary Michael Blumenthal did recognize demand restraint as *one* of the necessary conditions for inflation control, while also being emphatic that such restraint was insufficient: "We are dealing with a specific problem of wage-price momentum for which all the old ideas have proved inadequate."[229] He added: "We need this new tool, and we need it as soon as possible."[230] In the same vein, testifying the following day Charles Schultze stated: "Under present circumstances, clear standards for private wage and price behavior are needed as a complement to fiscal and monetary restraint."[231] These statements bore out the observation made at the start of the year by *New York* magazine that the monetary perspective on the current inflation's causes, "mainly developed by Nobel laureate Milton Friedman, is taken very seriously within financial circles here and abroad but these days it mostly seems to get just lip service in Washington." (*New York*, January 8, 1979.)

As if to confirm this assessment, at the end of January 1979 Secretary of the Treasury Blumenthal told a Congressional hearing: "There are many causes of inflation. I wish there were

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[229] From Blumenthal's testimony of January 29, 1979, in Committee on Ways and Means, U.S. House of Representatives (1979, p. 13). Also quoted in *Kansas City Star* (Missouri), January 29, 1979.
[230] From Blumenthal's testimony of January 29, 1979, in Committee on Ways and Means, U.S. House of Representatives (1979, p. 13).
[231] From Schultze's testimony of January 30, 1979, in Committee on Ways and Means, U.S. House of Representatives (1979, p. 88).

a single one. We would then be able to design a cure. [But] there are many complex causes... We, therefore, must devise multiple cures and approaches to deal with these multiple causes."[232]

But, after January 1979, the monetary view of inflation gained rapid and considerable ground in the United States. Vital impetus for this development actually came indirectly from the administration itself, through the historical revisions that it issued at the start of the year of estimates of potential output. As discussed in the first section of the present chapter, these revisions, in effect, lowered the contour of U.S. potential output over the 1970s. One, more mundane, effect of this changed picture was that it likely made policymakers more amenable to a tighter monetary policy. The much-narrowed historical and current output-gap estimates made U.S. economic overheating a far more likely prospect for 1979 than it had seemed previously—with even Blumenthal and Schultze, as discussed above, taking this view by March 1979 and being worried about an imminent overshooting of potential output by actual U.S. output.[233] Even among cost-push adherents like Blumenthal and Schultze, therefore, the role that demand restriction had to play as a necessary condition for generating disinflation was increased.

More profoundly, the changed official estimates helped instill what Romer and Romer (2002b, p. 33) called "the modern consensus" on the links between monetary policy, economic slack, and inflation. The 1979 estimates of potential output now offered much less support to the notion that the 1970s had been a period of simultaneous deep negative output gaps and high inflation, and they greatly buttressed the characterization Friedman and other monetarists had given of the decade being one of excess-demand driven inflation. The output-gap revisions, as they were digested, made the response of inflation to the output gap now more detectable than previously—largely rendering obsolete Schultze's observation in January that "inflation is so resistant to the traditional measures of [demand] restraint."[234] With the new perspective on the 1970s in hand, it was no longer true, as Arthur Okun would claim (in a posthumously published discussion) with

[232] Testimony of January 31, 1979, in Joint Economic Committee, U.S. Congress (1979, p. 4).
[233] Despite the disagreement between the administration and Miller in spring 1979 on the pace of tightening, with the administration favoring a more rapid imposition of monetary restriction, Miller was concerned to tighten monetary policy during 1979 in a manner that prevented output from surpassing the level of potential output. So was Volcker. For example, Volcker warned of an overheating danger in his September 5, 1979 testimony (Volcker, 1979d, p. 739): "A moderation in the growth of aggregate demand was welcome this year—even essential—if the economy was to avoid the kind of pressures on capacity that could only aggravate inflationary forces." As it happened, according to modern estimates, output *did* overshoot its potential level during the year and so likely produced the aggravation of inflation that Volcker regarded as a risk. Indeed, retrospective estimates would suggest that a positive output gap emerged in 1977−1978, and this continued into 1979. See Orphanides (2003, p. 655).
[234] From Schultze's testimony of January 30, 1979, in Committee on Ways and Means, U.S. House of Representatives (1979, p. 88).

regard to the late 1970s that "the monetarist model has not dealt successfully with the phenomenon of chronic inflation," that, in particular, monetarists had been refuted about the connection between slack and inflation, and that Friedman's position that an oil price shock had no lasting inflationary effect had been discredited.[235]

The importance of the output-gap revisions in changing views about the character of inflation is underlined by the fact that in December 1978, Alan Greenspan, a longtime holdout against cost-push interpretations of the 1970s inflation, seemed to be pivoting toward those interpretations. Greenspan remarked: "Our problem is one for which we have no firm theoretical understanding. For the last six or seven years, we have been endeavoring to establish a theoretical framework for a condition of chronic inflation in a period of less than full utilization of resources. It is not an explanation to say that there… is simply demand-pull. Our problem is not something we fully understand."[236] The revised output-gap estimates in 1979 and their widespread acceptance made such a pivot unnecessary, as inflation in the 1970s was now highly amenable to being understood using a short-run nonvertical, long-run vertical Phillips-curve equation in which cost-push shocks were only a source of temporary movements in inflation around its mean.

Such a view of inflation, furthermore, suggested that a policy of aggregate-demand management that allowed the output gap to become negative for a time was necessary to reduce inflation—and that no other policy was likely to do so. Perhaps reflecting the spread of this mindset, as early as February, Citibank's economic analysts were suggesting that the U.S. authorities *wanted* to push output below its potential level, in order to get inflation down (*Citibank Monthly Economic Letter*, February 1979, p. 2). Whether this was true as early as this point of the year, when Miller still headed the Federal Reserve, is open to question, in view of Miller's longstanding sympathy with cost-push accounts of the 1970s inflation. But even Miller seemed to be moderating his stand during 1979 on the matter of what nonmonetary weapons against inflation could do: in midyear, he remarked that the administration's wage-price standards were

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[235] Okun (1981, pp. 1, 2, 3–4; quotation from p. 1).

[236] From Greenspan's remarks of December 5, 1978, in Daly and others (1979, p. 24). Greenspan's surrounding remarks indicated that Greenspan was not countenancing moving to a *pure* cost-push position, in which inflation was insensitive to slack. He was, however, now entertaining the possibility that cost-push shocks might be a source of prolonged, one-sided pressure on inflation for a given setting of monetary policy. Embrace of such a position would have moved him away from a monetary view of inflation to a "partial cost-push" view of inflation. The latter view was exemplified by Paul Samuelson's stance (given in 1969) that "we are not masters of our own house domestically" (that is, control of aggregate demand could not deliver price stability alongside full employment) because "we have wage-price spirals which are out of our control" (quoted in Hinshaw, 1971, p. 43). See also Romer and Romer (2004, p. 158), who provided an early discussion of this lapse in Greenspan's position and who stressed that his policy prescriptions did not change markedly in light of it.

only "a minor part of the anti-inflation program," in contrast to the "big guns of monetary and fiscal policy" (*St. Louis Globe-Democrat*, June 15, 1979). It was, however, under Paul Volcker that it became much more the case that the doctrine underlying Federal Reserve strategy was one in which the U.S. monetary authorities would accept Friedman's position that inflation was a monetary phenomenon and that, accordingly, monetary policy was the only effective device that could be used against inflation.

For Friedman, this development did not come a moment too soon. The need for effective monetary control was urgent, Friedman observed in October 1979, because "sooner or later, the adverse effect of inflation will destroy the country." (*San Francisco Chronicle*, October 18, 1979.)

A convergence of views

The articulation by Volcker of a monetary view of inflation was part of an overall sketch that he provided during the late 1970s on his views on aggregate economic relations. During his tenure as Federal Reserve Bank of New York president, Volcker had made a number of statements on monetary policy, inflation, and disinflation that acknowledged the validity of key positions that, at the time, were closely associated with the monetarist position and diverged from much of the Keynesian and administration economic thinking of the time (see, for example, Romer and Romer, 2004, p. 156, and Meltzer, 2009b, pp. 946, 1010).[237] The convergence of his views with those of Milton Friedman, in particular, continued to be evident in statements that Volcker made on matters of economic substance in his early period as Federal Reserve chairman.

On money and aggregate demand, Volcker testified on October 15, 1979: "M1, M2… I think those are the variables that are important for economic activity."[238]

With regard to money and inflation, Volcker remarked at the same Congressional hearing that "in very simple terms, of course, what the Federal Reserve does is control or influence the supply of money and credit," that the decision had been made to respond to "the situation by moderating the growth of money and credit rather than simply following a passive course of creating more

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[237] Meltzer's documentation on this matter, while valuable, unnecessarily quotes (in the pages indicated) manuscript or proof copies of Volcker talks without indicating that the items in question (respectively, Volcker, 1976b, 1978b) were published in widely available journals containing the same passages and so were not really, as Meltzer implied, archival items.

[238] In Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1979a, p. 8).

money," and that this would "begin to lay the foundation for a sustained effort on the inflation front."[239] This perspective was one Volcker reaffirmed repeatedly during his first term as chairman. For example, two years into this term, Volcker told a Congressional questioner: "I certainly agree that there is a broad relationship between growth in the money supply and inflation. That is the basis of our policy…"[240]

With regard to the inflation-unemployment relationship, Volcker had in the 1960s been a subscriber to a long-run Phillips-curve tradeoff (see Nelson, 2020b, Chapter 13). His thinking on this matter had changed greatly by the time he became chair in 1979. This was brought out in his public statements, including a speech a few days after his October 6 announcement. Here, partly echoing the 1977 speech in which he credited monetarists on the matter, Volcker observed: "After years of inflation, the long run has caught up with us. We can no longer blithely assume we can buy prosperity with a little more inflation."[241] In the same passage, he indicated that "so-called tradeoffs between inflation and employment" had disappeared once expectations of inflation adjusted to new conditions. And, a few days later, Volcker indicated that it was price stability that would "improve the prospects of economic performance in the longer run."[242]

On interest rates, Chairman Volcker continued to press a point that he had made as a reserve bank president—and that Friedman had made repeatedly for decades. During his presidency, Volcker (1978b, p. 332) had stated that monetary expansion might ultimately mean "not lower, but *higher*, rates" of interest. He reaffirmed in October 1979 testimony that efforts to achieve stabilization or reduction in nominal interest rates via more rapid monetary growth would ultimately be a "futile effort"—capable of being effective only in the short run.[243] Applying his views on the shorter-run and longer-run influences of monetary policy on interest rates to a context of disinflation, he observed in his October 1979 conference speech: "When the money supply is brought clearly under control and expectations of inflation dissipate, interest rates will tend to decline."[244] Volcker, both as Vice Chairman of the FOMC in 1975−1979 and as

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[239] From Volcker's testimony of October 15, 1979, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1979a). For the first two quotations, see page 4 of this hearings volume, and page 5 for the third quotation.
[240] From Volcker's testimony of July 16, 1981, in Joint Economic Committee, U.S. Congress (1982, p. 83).
[241] Volcker (1979b, pp. 9–10; p. 41 of printed version).
[242] The quotations are respectively from Volcker, 1979b, p. 9; p. 41 of the printed version) and his testimony of October 15, 1979, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1979a, p. 5).
[243] From Volcker's October 15, 1979, testimony in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1979a, p. 4).
[244] Volcker (1979b, pp. 6–7; pp. 38−39 of the printed version). For further discussion of Volcker's statements on the Fisher effect as chairman, and the different effects of monetary policy on interest rates at different horizons, see Nelson (2021).

Chairman from 1979 onward, particularly stressed the importance of inflation expectations as an influence on longer-term interest rates.[245]

On exchange-rate management versus floating rates, Volcker had a background of being in the Nixon Administration in a period, 1971 to 1974 that, in Friedman's evaluation, was one of a generally excellent record in the area of international economic policy. However, as indicated in Chapter 4 above, Friedman largely credited George Shultz with arguing the case for floating rates against skeptics who, he believed, included Volcker. Friedman would, nonetheless, have reason to be pleased with Volcker's answer at a press conference in October 1979: "In some sense, the less intervention, the better; if you don't need to intervene, so be it."[246] In a broader sense, of course, the Volcker response to a situation in fall 1979 in which U.S. exchange-rate weakness figured prominently was to focus on domestic monetary policy and continued free floating. This was much closer to Friedman's preferred approach than the intervention-centered Carter Administration/Federal Reserve response to dollar weakness in the fall of 1978.

With regard to the positions on economics that Volcker took during the 1970s, one of the more nuanced cases—closely linked, of course, to the contest discussed above between cost-push and monetary views of inflation—was the matter of incomes policy. Over the decade from February 1971 through January 1981. Volcker was in national office during roughly half of the time. In the course of most of those years of service, the incumbent administration (Nixon, and later Carter) had an official policy either of mandatory controls (Nixon, starting in August 1971) or an informal, and increasingly codified, wage-oriented incomes policy (under Carter). Against this background, Volcker's ability to speak out against incomes policy, without appearing to undercut national economic policy, was circumscribed. His likely underlying feelings had, however, been allowed to surface publicly when, about seven months before the Nixon Administration moved to controls, Volcker criticized other country's incomes policy experiences (*Morning News* (Wilmington, Delaware), January 15, 1971).

In his time as a reserve-bank president, Volcker exhibited some sympathy with cost-push views

[245] Public statements that Volcker made as Vice Chairman in this connection included Volcker (1976c, p. 155) and his testimony of July 30, 1979, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1979b, p. 5). There were, in fact, many occasions in the 1970s and 1980s when Volcker publicly stressed the influence of inflation expectations on longer-term interest rates (see also Nelson, 2021). Contrary to the implication of Goodfriend and King (2005, p. 981), his belief in this influence was not a new piece of information disclosed for the first time publicly by the release, in 2004, of the 1979 and 1980 FOMC meeting transcripts.
[246] Volcker (1979c, p. 7).

of inflation, as indicated in the previous chapter. But as outlined above, revisions of estimates of potential output in early 1979 gave renewed credibility to demand-based accounts of the 1970s inflation.[247] And in his confirmation hearings that July, Volcker discouraged the practice of "decomposing the price indices all the time, and saying inflation is just due to this factor or that factor."[248]

In 1979 and 1980, Volcker was publicly supportive of the Carter Administration's incomes policy efforts. And he was, and remained, one of many who saw factors in the wage-setting mechanism, such as backward-looking indexation arrangements, as likely to prolong inflation and make more difficult the disinflation process.[249] Nevertheless, Volcker also took positions on the wage-price relationship that were notably close to those Friedman had articulated: monetary policy could bring down inflation even if nominal wage growth was not initially brought down; and, relatedly, nominal wages, being largely set in advance, tended to be based on expectations of past price conditions and tended to lag developments in prices.[250]

After the Carter Administration was out of office, Volcker could be less uninhibited in his criticism of nonmonetary approaches to inflation control. In a hearing held on July 16, 1981, after a member of Congress had suggested to him that an incomes policy would be appropriate, Volcker responded, "I think the problem is that those approaches have been tried and have been particularly unsuccessful, to put it mildly… I do not see the usefulness of that approach."[251]

Friedman's views on trial

Volcker had written to Friedman in August 1979 that, although he expected Friedman to continue to be critical of U.S. monetary policy, he welcomed his future commentary: "you know

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[247] See also Romer and Romer (2004, p. 157) on the "much more realistic views" Volcker articulated in 1979.
[248] From Volcker's testimony of July 30, 1979, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1979b, pp. 38–39).
[249] On this point, see Volcker's testimony of October 15, 1979, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1979a, p. 20). In common with many, Volcker believed that indexation would likely exacerbate the real costs of disinflation, by being linked to inherited rates of inflation, as opposed to the kind of indexation schemes Friedman envisioned when he advocated indexation.
[250] See Volcker (1979b, p. 10; p. 42 of printed version). On Friedman's exposition of these points, see Chapter 3 above as well as Nelson (2020a, Chapter 8; 2020b, Chapter 13). In contrast, in a written submission to Congress for his testimony of January 29, 1979, Secretary of the Treasury Blumenthal had stated: "To bring down price inflation, we must bring down wage inflation…" (In Committee on Ways and Means, U.S. House of Representatives, 1979, p. 16.)
[251] See also Meltzer (2009b, pp. 1058, 1097) for documentation of other occasions on which Volcker criticized incomes policy and wage-push theories of inflation.

I will not be unhappy to have you preaching the doctrines of monetary rectitude as we move ahead" (in Silber, 2012, p. 149). In the event, Friedman became much more negative in the 1980s about Volcker's record than Volcker likely could have expected. Even so, Volcker's feeling about Friedman's commentaries in retrospect had elements of his initial positive attitude: "he was a pain, frankly, because he wouldn't—he'd never want to give the Federal Reserve credit for anything, anytime," but, in the course of these public remarks, Friedman did stress the importance of monetary restraint. "So, in a sense, obviously, Friedman was helpful in that." (Paul Volcker, interview, October 16, 2013.)

Notwithstanding his status as an outside critic, Friedman found that he had a heavy stake in the course of the 1979 policy experiment. The *Financial Times* immediately judged that the October 1979 change amounted "to a considerable revolution in U.S. monetary policy" (*Financial Times* (London), October 8, 1979*c*). In the revolution in tactics and strategy that it adopted in 1979, the Federal Reserve was perceived, and with good reason, as adopting a template largely laid out by Friedman.

As Volcker put it, "you know, we adopted what could be termed certainly a Friedmanite policy—emphasized the money supply, and all the rest. Partly because Friedman had made that such a bellwether that we kind of capitalized on that, psychologically. Because it was a way to explain the importance of getting after inflation and the fact that, you know, you had to deal with [the] money equation." (Paul Volcker, interview, October 16, 2013.)

The 1979 shift thus confirmed Friedman's success in getting his views accepted and vindicated the statement in the Hoover Institution's annual report in 1978 that Friedman "is the nation's leading monetary economist."[252] But it also meant that Friedman would be criticized if the change was not perceived as a success. The headline—"Testing Monetarism Theory"—of a syndicated newspaper article (*South China Morning Post* (Hong Kong), October 14, 1979) that appeared shortly after the October 1979 measures were announced made it clear that the monetarists, not just the Federal Reserve, would be judged on the basis of how the new regime turned out.

Volcker's move did not exactly have enthusiastic public support, but it had a certain amount of general acceptance. "Not so long ago the current level of interest rates would have been regarded as unthinkable," one newspaper editorialized. "The fact that the public is not storming

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[252] Hoover Institution (1978, p. 12).

the temples of economic power is evidence that the nation is ready for economic discipline to slow a rising cost of living…" (*Kansas City Times* (Missouri), October 12, 1979*b*.)

Friedman also stressed in mid-1979 his belief that, by now, there was considerable professional support for his position on how to deal with inflation: "many leading economists do stress these fundamentals."[253] Even among U.S. economists, however, there remained notable opposition to the monetary view of inflation and to Volcker's application of it. For example, James Tobin reacted to the October 1979 announcement by suggesting that the reduction in inflation that could be expected from years of monetary restraint would be small (*New York Times*, November 11, 1979).[254] Another U.S. academic (albeit one who, by 1979, was outside the mainstream of economics) who took to the public press to challenge the monetarist approach was Leonard Rapping. A one-time supporter in his research of Friedman's views on inflation, Rapping had changed his tune: "Regrettably, our central bank has only the equivalent of nuclear weapons in its arsenal for controlling inflation. Inflation cannot be responsibly controlled through either monetary or fiscal demand management." (*New York Times*, October 28, 1979.) A major test of whether monetary tightening would resolve the United States' inflation problem was clearly in store for the country in the 1980s.

JERRY BROWN

In introducing Volcker at a Congressional hearing in October 1979, Senator William Proxmire contended that "we must recognize that the Federal Reserve alone cannot eliminate inflation… Fiscal policy must also continue to be restrictive at least until inflation is brought under control, and that means that the Congress must spend less and reduce the deficit."[255]

Friedman believed that the Federal Reserve could eliminate inflation on its own. He did not accept the close linkage that Proxmire's remarks implied of the case for fiscal tightness with the

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[253] From his remarks (given in a letter) in Friedman and Goldberger (1979, p. 36).
[254] Paul Samuelson, although not exactly contradicting Tobin's position, had foreshadowed accurately early in Volcker's tenure what likely might ensue from a "Federal Reserve-Carter policy of austerity." Samuelson contrasted the situation in the United Kingdom facing the newly-elected government of Margaret Thatcher, who had time to have a costly recession and still go to the polls in conditions of lower inflation and economic recovery, with the situation in the United States, for which "the benefits of a U.S. recession that reduces two-digit inflation to high one-digit rates would be more likely to come *after* a one-term president [Carter] is writing his memoirs in Georgia" (*Newsweek*, September 24, 1979). The main element of error in Samuelson's prediction was that U.S. inflation had, by mid-1982, fallen much more than he had implied was likely.
[255] From Proxmire's remarks of October 15, 1979, in Committee on Banking, Housing, and Urban Affairs (1979a, p. 2).

need to address the country's inflation problem. Nevertheless, Friedman agreed with the basic fiscal policy prescription of federal spending restraint that Proxmire propounded. As discussed in Chapters 5 and 9 above, Friedman saw merit in federal spending restriction both as a short-run measure—on the grounds that it would, in the event of a restriction of aggregate demand, tend to place more of the burden of adjustment on the public sector—and as a long-run measure—something that could improve U.S. productivity performance by reallocating resources to the private sector. It was primarily with this long-run motivation in mind that Friedman pressed ahead, over the course of 1979, with his push for a constitutional amendment restricting the U.S. federal budget.

On January 30, 1979, Friedman appeared at a press conference in Washington, D.C., to launch the National Tax Limitation Committee's draft constitutional amendment on balancing the budget. The draft amendment, fit onto a single page of the committee's press release (see Committee on the Judiciary, U.S. House of Representatives, 1980, p. 125) was the work of the committee's drafting panel, on which Friedman had served during the previous few months.[256]

The *Washington Post* report (January 31, 1979) on the press conference noted an oddity: although advanced by a tax-limitation committee, the "proposed amendment would not limit taxes." A second oddity was one Friedman reveled in: although widely reported and promoted as a balanced-budget amendment, it was, in fact, a spending-limitation amendment—one requiring, except for cases of emergencies, the planned growth in nominal federal spending to be subject to an upper limit equal to the recent growth in nominal national income. Indeed, its formal name was the Federal Spending Limit Amendment proposal.[257] Furthermore, when nominal income growth had embedded in it a rate of inflation above three percent, the limit on federal spending was specified as being at a lower ceiling still—a reflection of Friedman's belief, already noted, that federal spending restriction should bear a disproportionate amount of the overall restriction of aggregate demand during a process of disinflation.[258]

At the press conference, Friedman argued that the proposed amendment was "a thoughtful, reasonable alternative" to rival proposed amendments which would actually directly require a

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[256] The draft amendment was also reproduced in Friedman and Friedman (1980, pp. 313–314).
[257] See, for example, the prepared statement of William Craig Stubblebine for the hearing of November 1, 1979, in Committee on the Judiciary, U.S. House of Representatives (1980, pp. 362–363).
[258] As Friedman had foreshadowed during the drafting process (see Nelson, 2020a, Chapter 3), the proposed amendment provided for the suspension of the amendment's budgetary restrictions in the event of war (see the text of the amendment in the reference cited above, as well as the discussion in *Newsweek*, February 12, 1979).

balanced budget. He suggested that "a simple budget-balancing amendment… turns out to be impossible to implement," and that the amendment he was advancing would "let the budget balance itself" over time while also achieving a 5-percentage-point reduction in the share of federal outlays in national income, with the aim of bringing it down to about 20 percent by the end of the 1980s (*Washington Post*, January 31, 1979).

The press conference received national publicity, and Friedman's association with the proposal led syndicated newspaper columnist to call it the Friedman amendment (*New York Times*, February 5, 1979). By the time of the press conference, however, this proposal had been overshadowed by the fact that Jerry Brown, who in 1975 had succeeded Ronald Reagan as governor of California, had interposed himself into the debate on constitutional budgetary limitation. In his second-term inauguration speech on January 8, 1979, Brown had himself called for a balanced-budget amendment (*The Courier-Journal* (Louisville, Kentucky), January 10, 1979).

Lewis Uhler of the National Tax Limitation Committee had professed to be delighted with Brown's intervention: "Brown has done great things for us. He has made us the moderates on this issue." (*Los Angeles Times*, February 18, 1979, Part I, p. 25.) One political commentator observed, however, that although "Brown's amazing ability to command media attention" had been good for drawing attention to the amendment movement, and that Brown had established himself as the "most vociferous champion" of a constitutional requirement for a balanced budget, his emergence as a key advocate of federal budget limitation might ultimately have a negative effect on the cause, in light of his reputation for flip-flops on economic policy and the suspicion that he was seizing on the issue to boost his planned campaign against Jimmy Carter for the 1980 Democratic presidential nomination (*Daily News* (New York), February 28, 1979).

Friedman's perspective on Brown's entry to the debate was very much along the same lines as this commentary. After Brown came out in favor of an amendment, Friedman did claim that he was "delighted" by the fact that governors in the United States were keeping up the momentum for a federal constitutional amendment (*Los Angeles Times*, February 18, 1979, Part I, p. 25). But the fact was that Friedman was not happy at the prospect of Brown being the national face of budget limitation. This was despite some pleasant impressions Friedman had had of Brown, particularly during the latter's first three years in office. Friedman had noted with approval that Brown had continued to some extent the small-government attitude that had characterized Reagan's tenure as governor (*Newsweek*, March 1, 1976). Along these lines, in August 1975

Brown had remarked in a television interview: "I don't think government ought to be the answer to everything. If it is, we're not going to have a free country anymore."[259] About eighteen months later, in a meeting that in effect marked Friedman's move to the West Coast, Brown and Friedman had a five-hour discussion in the village of Bodega Bay, north of San Francisco, and found much agreement on issues related to limiting the role of the public sector in the economy (*The York Dispatch* (Pennsylvania), February 11, 1977). After this meeting, Friedman indicated that he saw some ability in Brown, crediting him with being "a maverick, more or less made in his own image" and willing to break with his own party on certain issues. On the whole, however, Friedman saw little cohesion in Brown's approach to economic policy (*Reason* magazine, August 1977, p. 29).

Brown and Friedman were subsequently, however, on opposite sides in the Proposition 13 debate in 1978. Even though Brown made efforts after the referendum result to build bridges with some of Proposition 13's proponents, Friedman remained unpersuaded of Brown's credentials on economic matters.

In March 1979, Friedman addressed Brown's support of a balanced-budget amendment directly in a *Newsweek* column bearing the blunt title (all in capital letters—reflecting the magazine's brief employment of that format for its article headings): "JERRY BROWN'S KISS OF DEATH."[260] The title was the most negative thing said about the governor in the column: Jerry Brown was not Friedman's least favorite politician by any means. But the column did lament the likelihood that Brown's prominent support for a budget-balancing amendment would make it a "football of partisan politics," instead of, as Friedman would prefer, one associated with both sides of the U.S. Congress—the body in which Friedman hoped progress toward an amendment would be centered. He also noted that Brown was perceived as using the issue as a "launching pad" for a 1980 primary challenge to Carter and that this would discourage those Democrats who wanted the president to have a second term from endorsing an amendment (*Newsweek*, March 26, 1979).[261]

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[259] This interview was excerpted in *Call to Convention: Governor Brown and the Tax Revolt*, PBS, April 19, 1979.
[260] This was Friedman's second successive column on the matter. He had devoted his March 5 column to the National Tax Limitation Committee proposed spending amendment that he had co-drafted and, in particular, to its potential macroeconomic benefits.
[261] A few weeks after his column appeared, Friedman also discussed Brown's activity in the area when interviewed for a television news special, made by San Francisco's local PBS channel WQED and widely shown nationally, titled *Call to Convention: Governor Brown and the Tax Revolt* (see *San Antonio Light* (Texas), April 15, 1979, and *https://americanarchive.org/catalog/cpb-aacip_55-v40js9hr9n*).

Friedman then went back to his running theme of the primacy of restraining spending rather than some other index of the budget. He had already remarked on television that "Howard Jarvis' activities since [Proposition 13] have been a mixed blessing" because they diverted attention toward tax limitation rather than spending limitation" (*Meet the Press*, NBC, November 12, 1978, p. 1 of transcript). On a related tack, Friedman's *Newsweek* piece proceeded to take Brown to task for emphasizing budgetary balance as a virtue in itself, when, as Friedman saw it, one should "seek a balanced budget not for its own sake but in order to halt inflation and reverse the steady increase in the fraction of our income being spent by Washington on our behalf" (*Newsweek*, March 26, 1979).

Brown was not the only politician failing to grasp this point, Friedman believed. The same was true of many national legislators. At the January press conference, Friedman had optimistically suggested that it was "enormously likely" that Congress would act in the area of constitutional limitation on the budget because "they're under the gun, they're aware of the vast public sentiment for something like this" (*New York Times*, January 31, 1979). In a sense, this expectation was borne out: over a dozen proposed amendments had been introduced in the Senate alone by May 1979 (*Congressional Digest*, May 1979, p. 136). The National Tax Limitation Committee had actually hired two Washington-based lobbyists to advance its own proposed draft of the amendment. But the variety of alternative schemes only underlined a key concern of Friedman's—that "the right kind of constitutional amendment," that is, one like his that sought to hold down spending, be the one adopted (*Los Angeles Times*, February 18, 1979, Part I, p. 25). He opposed the notion that "it's OK to balance the budget by increasing taxes" (*Call to Convention: Governor Brown and the Tax Revolt*, PBS, April 19, 1979).

In any event, regardless of the specific variant being contemplated, any proposed constitutional amendment pertaining to the federal budget faced enormous odds against being adopted. Friedman's column on Brown's entry into the debate was, nevertheless, bullish both with regard to a federal limitation on the budget being added to the constitution and to the form of the amendment being along the lines that Friedman favored. In this connection, Friedman was receptive to Brown's proposed route of calling a constitutional convention to consider alternative proposed amendments.[262] But even if such a gathering did not go ahead, Friedman suggested, the

[262] In addition to expressing this sentiment in his *Newsweek* column of March 26, 1979, Friedman remarked in an interview for public television that was taped at his Hoover Institution office: "I don't have any objection to the convening of a constitutional convention. I believe that the fears that have been expressed about it are grossly exaggerated. All a constitutional convention can do is propose—nothing is adopted [into the constitution] unless it's passed by three-quarters of the states. You're not going to get crazy amendments proposed. If you did, you're not

option of holding it—what Friedman, in a rare embrace of terminology associated with game theory, called "the credible threat of a constitutional convention"—could expedite Congressional action on an amendment (*Newsweek*, March 26, 1979).[263]

Testifying to Congress

Friedman's emphasis on federal spending as the key variable, and not the numerical difference between expenditures and tax revenue, permeated testimony he gave to the House of Representatives' Committee on the Judiciary on May 17, 1979. This was Friedman's first appearance at a Congressional hearing since his move to California in 1977. It would also prove to be his last-ever in-person delivery of testimony to Congress. Friedman's opening statement repeated a point he had made for years on public spending control: "We need to cure the defect in our political structure, to have an effective way in which the public can express its judgment on the part of its income that it wants the government to spend on its behalf." An amendment, together with the public consensus that would underlie its passage, was, Friedman suggested, "by far the best way to achieve this objective."[264]

A poignant exchange in the question-and-answer part of Friedman's testimony brought out poignantly how some of his longtime views on fiscal policy still survived in his thinking in 1979. His remarks were a response to a question that cited the "absolutely overwhelming case" that Charles Schultze had articulated in his testimony at an earlier proceeding of the hearings against raising taxes or reducing federal spending in an effort to balance the budget when a recession occurred.[265]

Although Friedman, elsewhere in his testimony, made it clear that he did not think that fiscal policy was the powerful influence on aggregate demand that Schultze believed it to be, his answer to this particular question was conciliatory. "The answer to that is that I am not in favor

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going to get them adopted." Friedman also conjectured, as he had in *Newsweek* column, that his own preferred form of budget limitation would be what a convention would favor. He stated in the interview: "I would predict that, if a constitutional convention were convened to consider a balanced budget amendment, in the course of those deliberations it would tend to switch toward limiting government spending, either in addition to, or instead of, mandating a balanced budget." (*Call to Convention: Governor Brown and the Tax Revolt*, PBS, April 19, 1979.)

[263] Brown himself had said of the prospect of a convention: "so this is a good prod to get Congress to move" (*Call to Convention: Governor Brown and the Tax Revolt*, PBS, April 19, 1979).

[264] From Friedman's testimony of May 17, 1979, in Committee on the Judiciary, U.S. House of Representatives (1980, p. 123).

[265] From the question of Representative John F. Seiberling in the session of May 17, 1979, in Committee on the Judiciary, U.S. House of Representatives (1980, p. 134). Schultze had testified to the committee on March 28.

of a balanced budget amendment. The amendment I am defending is not a balanced budget amendment. It is a spending-limit amendment, and it has none of the defects that Mr. Schultze was pointing to about the balanced budget amendment. On the contrary, this [the National Tax Limitation Committee] amendment has built into it an automatic anticyclical component."[266]

Friedman's reference to the "automatic anticyclical component"—the operation of fiscal policy's automatic stabilizers—brought out why the drafting group for the amendment was able to get agreement on it not only from members of the group—like Milton Friedman, Rose Friedman, and James Buchanan—who tended not to put great weight on fiscal policy as a stabilization-policy tool—but also among those members—like Robert Gordon and Paul McCracken—who were much more sympathetic to Keynesian views on the power of fiscal policy.[267] But it was also the case that, even in his monetarist years, Friedman was not averse to the operation of automatic stabilizers and to the notion that they might help, on the margin, in easing business-cycle fluctuations.[268] Indeed, even in his March 26 column on Brown (which Friedman included as an enclosure in the written portion of his May Congressional testimony), he had cited the merits of aiming for a balanced full-employment budget, only to indicate that there were problems making that a concrete policy prescription. This practical concern was certainly justified by the very recent, and substantial, official revisions of U.S. potential output.

After the spotlight that Jerry Brown's activities early in the year and the Congressional hearings in May had put on the issue of constitutional restriction on the federal budget, the topic faded as a matter of national attention, especially as the concerns related to energy prices and gasoline lines came to the fore during the summer of 1979. On September 21, during his time in the city of Chicago to record the *Free To Choose* studio debates, Friedman attempted to breathe life back into public discussion of a spending-limitation amendment by holding, with Lewis Uhler, a press conference.

This event, held at the city's Marriott hotel, was essentially a relaunch of the campaign for the National Tax Limitation Committee draft amendment they had issued the previous January. At

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[266] From Friedman's testimony of May 17, 1979, in Committee on the Judiciary, U.S. House of Representatives (1980, p. 134).
[267] See Committee on the Judiciary, U.S. House of Representatives (1980, p. 126) for the list of members of the drafting committee.
[268] Samuelson's (1973a, p. 848) listing of "Anticyclical fiscal and monetary policies" as among the things that Friedman was against did not, therefore, provide an altogether accurate characterization of Friedman's views on the dimension of fiscal policy.

the Chicago event, Friedman suggested: "The growth of the budget now is determined by each special interest group seeking a program. This amendment would make one special interest group a check on another—instead of all of them ganging up on the taxpayer." (*Chicago Sun-Times*, September 19, 1979.)

By this point, not only had national interest in formal budget limitation dwindled, but so also had public interest in Jerry Brown's likely presidential campaign, which Brown had still not yet formally launched. Brown had been unable to displace Edward Kennedy in the public mind as the likely principal Democratic challenger to President Carter. Indeed, Paul Samuelson had predicted, during Carter's summer troubles, that the president might cede the Democratic nomination to Kennedy by simply deciding not to seek reelection (*Financial Times* (London), August 6, 1979). In the fall of 1979, by which time it was clear that Carter was not withdrawing, Samuelson used his *Newsweek* column to give Kennedy tips for a successful candidacy against the president (*Newsweek*, October 15, 1979). And when Friedman a little earlier mentioned in his own column that Carter was "fighting on two fronts for reelection" (*Newsweek*, October 1, 1979), he was likely referring (on the Democratic side of these fronts) predominantly to Kennedy, rather than Brown, as the president's challenger. Brown went ahead with his campaign but, in the event, continued to fail to gain traction. The perception of the Democratic contest as really being one solely between Carter and Kennedy was a constant during the 1980 primary season and was already firmly fixed even by fall 1979.

Looking ahead to 1980

Brown therefore went unmentioned when, near the end of his extended spell in the city of Chicago, Friedman appeared with Phil Donahue again, for an interview on the *Today* show (NBC, September 28, 1979), and was asked his views about several of the individuals likely to seek the presidential nomination in 1980. "I assume would not support the candidacy of Edward Kennedy," Donahue said to Friedman. "I would not," Friedman confirmed. Nor, Friedman added, would he support the president's reelection: "Jimmy Carter I would find it impossible to support. I think he's been a disaster."

Turning to Republican candidates, Donahue asked about John Connally—the Secretary of the Treasury whose tenure had seen the introduction of price controls. Friedman had had warm words to make about Connally when he had left his Treasury position in mid-1972. On that occasion, Friedman had stressed the fact that Connally had, on net, presided over a liberalization

of international monetary arrangements (Instructional Dynamics Economics Cassette Tape 100, May 31, 1972). But Connally had blotted his copybook with Friedman anew when, as presidential adviser, he had played a key role in recommending to President Nixon the new price freeze of 1973 Instructional Dynamics Economics Cassette Tape 122, June 6, 1973). And, even in the international area, Friedman could not overlook the fact that, after the initial August 1971 move toward exchange-rate flexibility, Connally had made, via the Smithsonian agreement, "an attempt to reestablish the system of fixed exchange rates" (Instructional Dynamics Economics Cassette Tape 146, May 20, 1974). "I would not support John Connally," was Friedman's simple answer to Donahue in 1979.[269]

The next name that Donahue offered was Ronald Reagan. "Yes, I would support Ronald Reagan," Friedman confirmed. Donahue asked, "Have you told him?" "Well, I'm not sure whether that's a subject matter for public discussion," Friedman replied. This was, of course, a *de facto* affirmative answer. Indeed, Michael Parkin, who visited the Hoover Institution for the 1979/1980 academic year, got the impression on the basis of his frequent lunches with Friedman during that year that "Milton's goal in life was to get Reagan nominated" (Michael Parkin, interview, May 29, 2013).

Reagan would officially launch his candidacy for the nomination on November 13, 1979.[270] The team that would supply Reagan with memoranda on economic matters during the Republican presidential primary campaign and later national election campaign had, in its membership, not only Friedman and Martin Anderson but also individuals, such as Arthur Burns and Alan Greenspan, who, in contrast to Friedman and Anderson, would probably have favored Gerald Ford over Reagan as a 1980 candidate, had Ford decided to be a formal contender for the Republican party nomination. "Those of us who were in the Ford Administration and considered Ford a wonderful person deserving of support stayed with him [in 1976]," Alan Greenspan recalled. "But none of us considered Reagan an enemy. He was an opponent during the 1976 primary campaign, but not an adversary. And almost all of us, when Ford left office, joined the Reagan camp." (Alan Greenspan, interview, August 19, 2013.)

Friedman's endorsement on national television of a Reagan candidacy was followed a few

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[269] Friedman had also described (in so doing, exhibiting his too-ready tendency to make analogies in domestic political and social debate with Nazism or totalitarianism) Connally's idea of compulsory government service for 18-year-olds as "a Hitler-type youth program" (*Manchester Union Leader* (New Hampshire), February 16, 1979).
[270] See, for example, Merz (1980) and Cannon (2003, p. 544). (In Friedman and Friedman, 1998, p. 389, Milton Friedman erroneously suggested that the Reagan campaign only formally commenced in 1980.)

months later by Reagan's public endorsement of Friedman's major new multi-media project. With the *Free To Choose* television series due to start broadcasting early in the New Year, the Friedmans' companion book to the series started to appear in U.S. bookstores in late 1980. This $9.99 hardcover book had a dust jacket whose inside front text began with Reagan's words: "A superb book. The Friedmans eloquently diagnose the problems facing America and make imaginative proposals for change. It is 'must reading' for everyone—from the president to the private citizen—who is concerned with the future of America." When Reagan wrote the endorsement, "the president" to whom he was referring as needing to read *Free To Choose* was Jimmy Carter. But by January 1981, when the book, which had been a bestseller in 1980, became available as a mass-market paperback, Carter was in his last days in office, and Reagan himself was president-elect.

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