Milton Friedman and Economic Debate in the United States, 1973–2006
Chapters 11 to 18

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[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.

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Conventions used in this book

The chapters in this book (those that cover blocks of years, i.e., Chapters 2−12) 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 ("Gordon (1986) remarked…") for publications that appeared during (or prior to) Friedman's lifetime, and in the present tense ("see Gordon (2015) remarks…") 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. 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, "*USA Today*, April 27, 1987"). 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.)

In order 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 (1987a, 1987b)…").

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 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 11: Debates on Fiscal Policy, Geopolitical Developments, and Aggregate Supply, 1980 to 1981

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I. EVENTS AND ACTIVITIES RELATED TO FISCAL POLICY, GEOPOLITICAL DEVELOPMENTS, AND AGGREGATE SUPPLY, 1980–1981

On January 10, 1978, the NBC network broadcast a news special, *Land of Hype and Glory*. The program dealt with the national publicity drives typically associated with the promotion of mass-market media products such as new films, books, or rock albums. Milton Friedman was interviewed on this program. In his remarks on air, Friedman defended the practice of having large-scale promotion campaigns. "Of course high-powered advertising or hype changes people's tastes," he observed. "But would you have the true tastes if there were absolutely no advertising?"[2]

In January 1980, two years after the broadcast of this program, Friedman was himself in the midst of a major promotional blitz. A publicity machine, centered on Friedman, swung into action with the aim of directing the tastes of the American viewing and buying public toward his own new products: his PBS television series *Free To Choose*, which began its broadcasts in the second week of January, and the accompanying Milton Friedman/Rose Friedman hardback book, which bore the same title and whose nationwide availability began early in the month.

In later months, Friedman's promotional tour would become international in character and would include a visit to London starting in late February, a trip to Canada in July, and a lengthy tour of Asia in September-October 1980. But he was naturally most concerned with drawing attention to *Free To Choose* in his home country—and was succeeding, with an unfriendly local columnist

[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 is grateful to Harold James for very useful discussions and information concerning the material covered in this chapter. Errors are the authors' responsibility alone. The author regrets to note that in the period since the research for this chapter began, six individuals—Kenneth Arrow, Robert Chitester, Martin Feldstein, Dale Jorgenson, Allan Meltzer, and Paul Volcker—whose interviews with the author are drawn upon below have passed away.
[2] Quoted in *Sarasota Herald-Tribune* (Florida), February 19, 1978, p. 21.

acknowledging that Friedman had become “ubiquitous in print and on the air” (*San Francisco Chronicle*, May 11, 1980). A couple of months earlier, the “Page Six” column of the *New York Post* had observed (March 4, 1980): “Friedman has turned himself into a one-man multi-media industry (with a little bit of help from Mrs. Friedman).”

Friedman believed that ongoing events in the United States, including high inflation and the tax revolt, were underscoring the relevance of the material covered in *Free To Choose* and were also making more people receptive to the message of the television program and book. He was quoted as saying in the leadup to *Free To Choose*’s television premiere: “I feel strongly that America is at a critical point in its history. More and more, people have become disillusioned about the possibility of solving our problems simply by throwing more tax money at them.” It was in the context of this disillusionment that he believed that his own “views are becoming more widely accepted” and his free-market prescriptions correspondingly likely to be given more weight as policy alternatives. “As a result, for the first time in many years, I believe we have a real hope of changing the course of events—of getting our country back on the track toward greater human and individual freedom.”[3]

The intensive schedule of interviews, public talks, signing appearances, and press conferences across the United States in which Friedman engaged during January 1980 therefore worked hand-in-hand with the fact that economic issues were extremely prominent in the national news (with an Associated Press report on the series stating that it would be “examining the nation’s economic crisis”).[4] Together, they led to the *Free To Choose* television series receiving considerable public attention when its PBS airings began in the second week of January 1980.

Issues covered in the series

Friedman’s first major on-screen appearance in the premiere episode, “The Power of the Market,” was in a sequence filmed in a crowded garment factory in the Chinatown area of New York City. Although Friedman’s address to camera referred to his surroundings as a factory, the publicity material for the program, as well as the *Wall Street Journal*’s October 1979 account of *Free To Choose*’s making, referred to it more bluntly as a sweatshop.[5] This opening would lead

[3] In *Boston Herald American*, January 4, 1980.

[4] See *State Times* (Baton Rouge, Louisiana), December 24, 1979.

[5] See *Wall Street Journal*, October 24, 1979, p. 32, and *Free To Choose*, PBS, Episode 1, “The Power of the Market,” January 12, 1980, p. 1 of transcript.

the series to be derided as a "salute to the sweatshop" (*The Sun* (Baltimore), April 4, 1983).[6]

It was Friedman's personal background that helped prompt his use of this setting. He wished to emphasize such workplaces as a means of getting started in employment. Friedman had repeatedly noted over the years that his mother had worked at a sweatshop upon migrating to the United States (for example, in *Chicago Sun-Times*, October 27, 1974, p. 85), and he did so again on the program. Indeed, both his parents had had such jobs, and he had told a hearing held on May 11, 1967, of an Illinois senate committee that they had each taken sweatshop jobs on arriving, separately, in New York City, in the 1890s (*Chicago Tribune*, May 12, 1967, Section I, p. 4).[7] In February 1978—during a press conference at which he displayed what one news report called his "habit of smiling slightly just before he delivers a particularly controversial statement"—Friedman had declared: "Sweatshops are marvelous. They are a very effective way for an immigrant to find his place in America. My parents worked in them. Most immigrants arrive with no skills and need to work at terms that will be worth someone's while to pay them, while they build skills that will support them." (*Rochester Democrat and Chronicle* (New York), February 24, 1978, p. 3A.) He made a similar point in episode 1 of *Free To Choose*.

The first episode of *Free To Choose* also featured location filming in Hong Kong. As had the sweatshop sequence, this coverage left Friedman open to the criticism that his examples showed the hardship associated with the marketplace (for example, *Los Angeles Times*, January 11, 1980*a*). But Hong Kong's market-oriented economy Friedman an opportunity to emphasize the price system as a means by which sellers received signals that served as the basis for their production decisions—as well as to highlight the historical rise in living standards that had been associated with market-based arrangements.[8]

In another sequence of the first episode of *Free To Choose*, Scotland provided a backdrop against which Friedman quoted Adam Smith. This content of the program notwithstanding, the series had a distinct contrast with the approach of John Kenneth Galbraith, who had described his *Age of Uncertainty* series in 1977 as covering "the history of economics" and said that, generally speaking, "subjects like unemployment, inflation, the welfare state aren't in the series" (*Daily News* (New York), May 15, 1977). Friedman took essentially the opposite tack in *Free To Choose*. Although he referred to Adam Smith and Keynes during the series, he expounded his

[6] In the same vein, on the day of the series' local premiere, one of Friedman's hometown newspapers, the *San Francisco Examiner*, published a negative preview titled "The Happy Sweatshop" (January 11, 1980*a*).
[7] See also Friedman's remarks as reported in *Chicago Sun-Times*, October 16, 1976.
[8] *Free To Choose*, PBS, Episode 1, "The Power of the Market," January 12, 1980, pp. 2–3 of transcript.

economic analysis and policy prescriptions primarily by focusing on concrete macroeconomic and regulatory matters.

The first episode of *Free To Choose* had highlighted the benefits that the market mechanism had provided in terms of the development of the U.S. economy, while warning that present-day trends toward greater public-sector intervention threatened to squander that heritage. The second episode, "The Tyranny of Control," developed this theme. Later episodes included those specifically dealing with topics Galbraith had eschewed: unemployment (considered mainly in an episode on the Great Depression), inflation, and welfare programs. Other episodes considered consumer protection, the labor market, and Friedman's school-vouchers proposal.

Coverage of the series

Friedman's advocacy of the legalization of narcotic drugs—although long on the record by the time of the making of *Free To Choose*—was not mentioned in the series' documentary portions (that is, the first, filmed component of each U.S. episode of the series—as distinct from the videotaped debates that took up the second half of almost all episodes). *Free To Choose*'s executive producer Robert Chitester recalled, with regard to this omission, that "it was conscious… We wanted to focus mainly on the economic-freedom [side] and touch on the fact that economic freedom was critical to the other types of freedom, and we did so." (Robert Chitester, interview, July 9, 2013.)[9]

This line of thinking partly arose from experimentation with wider-ranging coverage in both the planning and implementation of the 1977–1978 *Milton Friedman Speaks* lecture series that *Free To Choose* eventually superseded as the Friedman television project. For an April 1977 meeting in New York City with Chitester and other series planners, Friedman had laid out a list of possible topics to be covered in each lecture/episode of *Milton Friedman Speaks*.[10] In his memoirs with Rose Friedman, Friedman stressed the compatibility of the list with the eventual subjects of episodes of *Free To Choose*.[11] Narcotic drugs, it is true, had not appeared on this broad list of topics, so the lack of coverage of it in the *Free To Choose* documentaries was faithful to Friedman's original outline.

[9] See Chapter 16 below for an account of the more outspoken posture that Friedman, later in the decade, took with regard to drugs.
[10] The list was printed in Friedman and Friedman (1998, p. 603).
[11] Friedman and Friedman (1998, p. 473).

There were, however, a number of items in the 1977 list of topics that also did not make it into the *Free To Choose* series. One of these, “Capitalism and the Jews,” was one Friedman had put on the list with great reservations. It was included in part because he already had a so-far little-seen full lecture written on the subject. But its 1977 listing had a notation by Friedman to the effect that he was disinclined to include the lecture—which had not been particularly well-received when he had given it in the past—in the series. Another topic, “Moral Values and Free Enterprise,” made it into the unbroadcast *Milton Friedman Speaks* series (under the title “Is Capitalism Humane?”), but, consistent with Chitester’s conclusion that the coverage of the series should focus on more concrete economic topics, did not make it into the *Free To Choose* program. Discrimination, a topic covered in a *Capitalism and Freedom* chapter, was in Friedman’s 1977 list but not assigned an episode in either *Milton Friedman Speaks* or *Free To Choose*, and much of what Friedman had to say about it could be encompassed by two other topics in the list that went on to be assigned episodes in both series: education and the labor market. Tax and welfare were in Friedman’s 1977 list as items to be assigned separate episodes, and they were indeed given lectures of their own in *Milton Friedman Speaks*. But they were subsumed into the coverage of welfare and income redistribution in “From Cradle to Grave” and “Created Equal,” episodes 4 and 5 of *Free To Choose*.

The danger of being outpaced by events likely figured in the omission from *Free To Choose* of two topics that had been in both Friedman’s 1977 list and in the *Milton Friedman Speaks* series: energy and healthcare. At around the time of the April 1977 New York City planning meeting at which the series’ content was first mapped out, President Carter was launching his National Energy Plan. By the end of 1978, the president had signed into law energy legislation substantially different from the 1977 plan. This was followed in 1979 by National Energy Plan II in April and by a revised version of the plan in July. An episode critiquing existing U.S. energy policy was therefore in danger of being out of date by the time of its 1980 broadcast. In the event, energy and another topic in the 1977 list, pollution, received coverage in the course of the *Free To Choose* book but did not have assigned episodes in the television series. Likewise, health policy in the United States seemed to be in flux during the Carter years (although little material changed in practice) and was not covered specifically in the *Free To Choose* series of documentaries.[12]

[12] The Friedmans’ introduction to the U.K. paperback version of the *Free To Choose* book briefly laid out a distinction between national health insurance, “the United States mixed system,” and a “voluntary system,” although the authors did not elaborate on the third category (Friedman and Friedman, 1980c, p. 16). Details regarding Friedman’s preferred healthcare system would not be fleshed out by him until the 1990s.

Another proposed topic—"International Trade and Finance"—was a surprising omission from *Free To Choose*'s episode list, in light of the fact that it had been frequent *Newsweek* fodder (with a chapter of the 1972 *Economist's Protest* collection in 1972 chapter titled "International Economic Policy" bringing together various columns on the subject).[13] Friedman did devote a *Milton Friedman Speaks* episode to international economics (one that focused on free trade, though Friedman did also discuss the properties of floating exchange rates). But *Free To Choose* did not have a specific episode on the topic of international trade and finance. There was enough coverage of trade matters in the series, however, for part of one episode's text to be reworked into a magazine article titled "The Case for Free Trade" (*The Listener* (London), March 27, 1980).

"*More like a philosopher*"

The *San Francisco Chronicle*, when printing condensations of some of the texts of the *Milton Friedman Speaks* series, had referred to him as "the widely published scholar and philosopher" (January 23, 1979). In the same vein, at the time of *Free To Choose*'s early-1980 rollout, an interviewer of Friedman suggested that he was increasingly "sounding more like a philosopher than a Nobel laureate in economics" (*Valley Independent* (Mon Valley, Pennsylvania), February 2, 1980). Nonetheless, as indicated above, the format Friedman chose for *Free To Choose* series involved sticking to topics usually associated with economics. That was, after all, his forte—a fact Rose Friedman stressed during the airing of the series (*Daily Mail* (London), March 18, 1980): "What really annoys his critics is that his credentials as an economist are so good. No one can take them away from him." Friedman noted that the Nobel prize had also helped: "It has given me a wider audience and increased the degree of my credibility." (*Evening Gazette* (Worcester, Massachusetts), May 3, 1978.)

The concentration on the more strictly philosophical aspects of Friedman's libertarianism was confined to the episode on egalitarianism and the distribution of income, "Created Equal." This was, of course, a topic for which he felt economic analysis was appropriate—a position reflected in his observation in a 1953 *Journal of Political Economy* article that the "absence of a satisfactory theory of the personal distribution of income and of a theoretical bridge connecting the functional distribution of income with the personal distribution is a major gap in modern economic theory."[14]

[13] See Friedman (1972b, Chapter 5).
[14] Friedman (1953b, p. 277).

In the event, Friedman had left microeconomic research at around the time of the 1953 article's publication, and the arguments in the *Free To Choose* episode on inequality were not strongly linked to that earlier work. Instead—beyond articulating the unexceptionable position that centrally-planned societies offered less scope in practice for equality of opportunity and for rising up the income scale than did market economies—the episode made a string of points that Friedman had made at various times since the 1940s: that, as high tax rates could often be avoided, redistributive policies were partly stymied; that postwar welfare-state policies on the had altered the income distribution, but some of the redistribution had benefited the middle class rather than low-income citizens; that high taxation was a disincentive to effort and productive efficiency, and high incomes in a capitalist system were often needed as a reward for risk-taking or for those (notably in the sports and entertainment worlds) possessing unusual talents that the public benefited from witnessing. These myriad points, most of them dating back to Friedman's positions on the matter in the 1940s, were not mutually inconsistent. But they did not come through very cohesively in the episode, being presented in rapid succession alongside more familiar points from previous episodes regarding the price system, and interspersed with sound bites that concerned real-life case studies. Furthermore, the main part of the "Created Equal" episode offered criticisms of existing policies but not an alternative policy—a flaw that Friedman had associated with Galbraith's series (*The Observer* (London), February 17, 1980, p. 33).

By not providing own Friedman's own prescriptions, the main (documentary portion) of the "Created Equal" episode (broadcast in the United States in February 1980) left the impression that he favored a fully libertarian alternative. In fact, although Friedman did favor a flat tax on labor income above a certain amount, in the low-income range and in the case of the unemployed, he continued to favor a negative income tax. He acknowledged this in the debate portion of the episode. This was the session that opened with Peter Jay's withering criticism, quoted at the start of the previous chapter, of the presentation in the "Created Equal" documentary episode of *Free To Choose*. "I do recall that Peter really was hammering away," Robert Chitester recalled. "…I am obviously biased, because I don't think anyone ever has beaten Milton in any debate. And, believe me, I have witnessed hundreds of them. But I'd have to go back and look at it again [to see who won]. But my sense is this, that I think Peter raised some extremely important points." (Robert Chitester, interview, July 9, 2013.)

The debate portions of ***Free To Choose***

These debates featured as the second half of the first nine of the ten 1980 U.S. broadcasts of *Free To Choose.* In each of these debates, Friedman confronted a set of panelists on the topic of the episode, with the documentary portion of the episode serving as the jumping-off point for the debate.[15] The episodes were videotaped at the University of Chicago starting in early September 1979.[16] As discussed in the previous chapter, their recording took place at the University of Chicago. Specifically, it was at the Harper Library in the university's Graduate School of Business—a choice of venue that probably contributed, to a minor degree, to the myth that Friedman had been a professor at the business school, rather than in the economics department.

In setting up the debates, the program's makers were not able to get all the panel guests they wanted. For example, Ralph Nader turned down the invitation to appear in the debate for the episode, "Who Protects the Consumer?"[17] The participation of those who disagreed with Friedman was nevertheless vital and was a feature that distinguished *Free To Choose* from *The Age of Uncertainty*. As noted in Chapter 10, Friedman had himself turned down the opportunity to appear on the Galbraith series. His non-participation contributed to that series' imbalance. The closing discussion of that series instead featured Henry Kissinger, former U.K. prime minister Edward Heath, current U.K. Labour Cabinet minister Shirley Williams, U.K. trade union leader Jack Jones, and London School of Economics political scientist Rolf Dahrendorf.[18] This had certainly been an eminent panel, but it was one that very largely shared with Galbraith a rejection of Friedman's economics. It also consisted of individuals who—in marked contrast to Baumol and Blinder's (1979, p. 822) observation that the "typical mainstream economist's reaction to Galbraith is to ignore him"—likely subscribed to and, however inadvertently, encouraged the misconception that Galbraith was a major leader in economic research.

In contrast, *Free To Choose* secured participation by figures who were both high-profile and

[15] The debates had a different look from the main episodes of *Free To Choose*. The main episodes (the documentary portions, in which Friedman largely appeared on his own) were shot on 16-millimeter film (and so look washed-out and grainy when replayed in unremastered form). The debate segments that were appended to the first nine of the 1980 U.S. broadcasts of these episodes were recorded on Ampex two-inch videotape (Robert Chitester, personal communication, October 1, 2014). The tenth, concluding, episode's second half, which consisted of an interview with Friedman, was shot on film at indoor and outdoor campus locations at the University of Chicago.

[16] The remarks of Rose and Milton Friedman gave the taping sessions for *Free To Choose* debates as finishing on September 14, 1979, with the Friedmans' arrival in the cities for the preparation and recording of the session being on September 3 (Friedman and Friedman, 1998, pp. 494–495). It might have actually stretched beyond September 14, Friedman was still in the city of Chicago on September 18, when he gave a press conference on current events (*Chicago Tribune*, September 19, 1979, Section 1, page 2); his *Today* show interview with Phil Donahue, taped in Chicago, aired in late September, and the *Wall Street Journal*'s report of October 24, 1979, on the making of the show reported him as recently being in Chicago for the final week of taping of the debates. The interview with Friedman, conducted at the University of Chicago, for the tenth episode began with on-screen text explicitly dating the time of the interview as "Fall 1979."

[17] *Donahue*, NBC, September 6, 1979. Nader had, however, publicly debated Friedman as recently as May 1979. See Proprietary Association (1979).

[18] See Halliwell and Purser (1986, p. 11).

opposed to Friedman's positions. In the "Created Equal" episode, these were Peter Jay (who opposed Friedman's politics more than his economics) and political scientist Frances Fox Piven (opposed both to his economics and his politics).[19] In the case of the inflation episode, the panelists even included former Federal Reserve Board Chairman William McChesney Martin. "We did it that way to make it credible that we were not unwilling to face the arguments of those who disagree with us," Friedman explained (*Chicago Tribune*, July 20, 1980, p. 21).

The debate portions proved to be the liveliest part of the *Free To Choose* programs. A preview by a television critic noted that the debates were among the "rewards" of watching the program, noting that, in that night's episode, "Friedman engages in a provocative debate with a politically diverse group including socialist Michael Harrington" (*Los Angeles Times*, January 11, 1980*a*). The debate segments allowed Friedman to be more combative than he had been in the filmed portions of the episodes.[20] They also raised the analytical content of the program by featuring a back-and-forth exchange of views not present in the corresponding filmed portions of the episodes. Indeed, a *New York Times* reviewer of the series was expressed by the extent to which the debates forced Friedman to fight his corner: "He is rather rigorously challenged… so much so that it might be forgotten that this is supposed to be his showcase." (*New York Times*, July 14, 1981.) Another press commentator remarked of the debate segments: "No punches are pulled here and sometimes the give-and-take can be described as nothing less than verbal violence. In Part 5, 'Created Equal,' the exchanges are as shrill, strident, heated, raucous, and acerbic as any ever heard on a TV talk show." (*Kansas City Star* (Missouri), January 16, 1980.)

In particular, the debate segment of the "Created Equal" debate would become a YouTube favorite because of the extent to which Peter Jay and Frances Fox Piven put Friedman on the defensive.[21] Even Robert McKenzie, moderator of this and most of the other *Free To Choose* debates, joined in the attack. Like Friedman, McKenzie was an academic (in McKenzie's case, a

[19] The panel also included Thomas Sowell, Friedman's former student, who took Friedman's side in the debate.

[20] With regard to the U.K. counterparts to the debates, one viewer was impressed with Friedman's courtesy in the face of the aggressive critiques advanced by his fellow panelists (*Financial Times* (London), March 21, 1980).

[21] See the previous chapter. In the United Kingdom, these debates were not broadcast and were replaced in the BBC's abridged version of *Free To Choose* by panel discussions, taped by the BBC, among Friedman and local figures. These panels were chaired by Peter Jay. By the time of these broadcasts, word of Jay's exchanges with Friedman in the U.S. series had spread: "it is reported [that] they clashed quite sharply over certain areas," the *Times*' television commentator noted (*The Times* (London), February 23, 1980). For those hoping to get a feel for their U.S. sparring, the U.K. broadcasts would be an anti-climax, as, in these episodes, Peter Jay adhered to the role of moderator. He noted, "We did those debates together in the United States [for the 'Created Equal' episode; and for episode 3, 'Anatomy of Crisis,' on the Great Depression]. We did the British version of the same thing later—only then, I was chairing the discussion, rather than taking sides in it." (Peter Jay, interview, May 8, 2013.)

London School of Economics-affiliated political scientist) turned television presenter.[22] Although meant to chair the sessions neutrally, McKenzie was moved to remark to Friedman that in discussing U.K. postwar governments, "you're right squarely in my area of special interest… and you're wrong on this one, Milton," before a few minutes later assuring participants that "I'm back in my chairman's role."[23]

Peter Jay found himself in the debate just a few months after completing two years as U.K. ambassador to the United States. "I had left the embassy in the summer of '79." In order to continue working in the United States, he had applied for, and secured, a green card (for U.S. permanent residency), with Friedman writing a reference on his behalf. "I was, thanks to Milton and the green card, sitting around in Washington—basically, at the Brookings Institution. And why they invited me [to be a *Free To Choose* panelist], I don't think I've ever known." On receiving the invitation, Peter Jay accepted the opportunity with alacrity: "why would I argue?" (Peter Jay, interview, May 8, 2013.)

Notwithstanding the bumpy ride he encountered in such episodes as "Created Equal," Friedman was well served by his debating and economics skills in his confrontations with the panels, with the result that, on the whole, the debate portions of *Free To Choose* likely reinforced the message of the filmed main episodes.

[22] With regard to research publications, however, McKenzie went off the wagon before Friedman, largely stopping after the early 1960s and concentrating thereafter on teaching and U.K. radio and television interviewing and commentary. McKenzie died in October 1981 (*The Gazette* (Montreal, Quebec, Canada), October 14, 1981), while *Free To Choose* was still being rerun in U.S. markets.

[23] *Free To Choose* (U.S. television series), PBS, Episode 5, "Created Equal" (debate portion), February 9, 1980, pp. 9, 14 of transcript. Although not seen in the U.K. version of the programs, the London-based McKenzie also made his views known on the issues covered in the program by having a letter published about *Free To Choose* in the BBC's magazine *The Listener* (May 8, 1980). Still focusing on Friedman's remarks on the U.K. record in "Created Equal," McKenzie was highly critical of Friedman's characterization of U.K. public spending and asserted that Friedman was "dogmatic… he simply will not entertain any evidence which appears to conflict with his own *a priori* judgment." In support of this contention, McKenzie objected to Friedman not having engaged him in a post-broadcast dialogue that McKenzie had tried to kick off by furnishing Friedman with data that suggested that the size of the U.K. public sector was not out of the ordinary compared with the "European average." It is likely that Friedman's non-engagement on this matter did not, in fact, justify McKenzie's strongly adverse judgment about him. The fact is that, in discussing the role of the public sector in the United Kingdom, Friedman had a long record of focusing on U.K/U.S. comparisons. He had never developed a sustained, close interest in Continental European economic data, and it is not very surprising that he did not engage in an exchange that required becoming more immersed in them. And Friedman's position that the U.K. public sector's role in the economy was very substantial in absolute terms and in relation to many other countries was correct. Even a critic of Friedman's, the aforementioned former U.K. prime minister Edward Heath, observed: "Britain has a very large public sector—we can argue whether that is desirable or undesirable…" (*House of Commons Debates* (U.K.), January 19, 1983, p. 367, available at *https://hansard.parliament.uk/commons/1983-01-19/debates/233d5d62-fec7-4a3b-8bfe-de55a8d730b3/OppositionDay*.)

Broadcast and reaction

During the making of the program, the series' title was uncertain. The Friedmans in early 1978 had suggested *The Invisible Hand*, but the makers of the program had rejected that title in early 1978.[24] Friedman had written down a list of around sixty possible titles (*New York Times*, February 24, 1980*a*). At an early stage, the title *Free Man, Free Market* was one of the favorites (*Forbes*, January 8, 1979, p. 39). *Free To Choose* had been settled upon by the start of 1979. It had apparently been suggested originally by series producer Michael Latham as the title of the episode on consumer protection.[25] That said, Friedman had long had a liking, dating back to his 1940s writings, for the term "free to choose." After the making of the main part of the series was largely complete, Friedman, in what may have been subliminal advertising for his forthcoming series, used the term in the text of a *Newsweek* column (*Newsweek*, July 30, 1979).

The television series, and the book version (both the hardback and paperback) of *Free To Choose*, had the subtitle *A Personal Statement*. It would appear, however, that Friedman was not particularly enamored of the subtitle, and he rarely used it when referring to the title of the book or series. The bibliography and index of the Friedmans' later book *Two Lucky People* would give the title of the book just as *Free To Choose*, with no subtitle.[26]

Friedman's series, including the title *Free To Choose*, was announced by PBS in July 1979 as part of its schedule of programs for the 1979/1980 television season (*San Francisco Chronicle*, July 12, 1979). Although PBS constituted a nationwide collection of television stations, it was much more decentralized in its organization than the three major U.S. television networks, and so not all channels in PBS had undertaken to air Friedman's program. By late October 1979, however, 160 PBS stations had agreed to televise it (*Wall Street Journal*, October 24, 1979, p. 1). The number of stations that ultimately broadcast the series when the first episode went to air in January 1980 was 196.[27]

[24] Friedman and Friedman (1998, p. 496).

[25] Friedman and Friedman (1998, p. 496). Jeremy Siegel (interview, September 17, 2013) related, on the basis of his discussions with the Friedmans, that the title "*Free To Choose* was the title of the chapter about education. You know—choosing free education with the voucher system." But, as indicated above, in the end "it got switched [to being the main title]." Comparing *Free To Choose* with the original proposed title of *The Invisible Hand*, Siegel remarked: "I thought that [the title, *Free To Choose*] was very, very good. Not that *The Invisible Hand* is bad or anything. But it [the title, *Free To Choose*] just is so direct, and it just sort of summarized what it was [about]. So that's how that title got there."

[26] Friedman and Friedman (1998, pp. 642, 652).

[27] See Friedman and Friedman (1998, p. 498).

A further feature of PBS was that it did not share the networks' arrangement of having a uniform, nationwide primetime television schedule. Consequently, the broadcast of *Free To Choose* was scheduled inconsistently across the United States. Some point in the Friday-Sunday period was a prevalent choice, but even then the precise timeslot varied significantly. In Friedman's old home of Chicago, the series got a good primetime timeslot: Friday, 9 p.m. (*Chicago Tribune*, January 11, 1980). In his newer home of San Francisco—whose press club hosted an address Friedman gave on January 10, 1980, as part of the publicity for the program's imminent premiere—the timeslot was similar: Friday, 10 p.m. (*San Francisco Examiner*, January 11, 1980*b*). In Los Angeles, the series was assigned a 9.30 p.m. Friday slot (*Los Angeles Times*, January 11, 1980*b*). But the PBS station in California's capital city, Sacramento, gave the program a backwater slot of Saturday 1.30 p.m. (*Sacramento Bee*, January 6, 1980).

Numerous stations chose a Sunday broadcast date and so, as indicated earlier, January 13 (a Sunday) was a widespread premiere date. Subject to this choice, however, again the broadcast time varied considerably. In Scranton, Pennsylvania, the series was given pride of place with an 8 p.m. slot (*The Scrantonian* (Scranton, Pennsylvania), January 13, 1980). In contrast, in Albuquerque, New Mexico, it was allocated the noon slot (*Albuquerque Journal* (Albuquerque, New Mexico), January 13, 1980). New York City's Channel 13 scheduled the program in the evening, but still out of primetime, at 5 p.m. on Sundays—something that was considered a graveyard slot (*American Film*, April 1980). This was a time when television viewers were available but particularly unlikely to be tuning in to PBS.[28] The consequence, *Time* magazine noted (March 10, 1980), was that "in New York *Free To Choose* is getting barely 2 percent of the audience because its 5 p.m. Sunday time slot puts it up against prime sports viewing hours. Friedman in recent weeks has been no match for the *Wide World of Sports*." However, when interest in Friedman's views surged during the early Reagan era, Channel 13 rode the wave by re-running the program in mid-1981 in a more favorable, midweek primetime slot (*New York Times*, July 14, 1981).

New York City was by no means the only viewing region to rebroadcast the series. In fact, repeat showings became prevalent in television markets across the country during the early 1980s, and as early as July 1980 the Dallas PBS channel was starting an encore presentation: a

[28] Two of the major New York newspapers gave the new series little prominence in their television listings. The *New York Post* (February 12, 1980) gave the premiere episode's title but provided no indication that it was a new series, no synopsis, and no reference to Friedman. The listing in the New York *Daily News* (February 13, 1980) was even briefer.

rerun provided in the Sunday 5 p.m. timeslot (*Dallas Morning News*, July 6, 1980).[29] This was testament to the fact that the series had proved a success. That success in turn had the upshot that Friedman's status as a national celebrity had been enhanced and consecrated. The high profile of the TV program validated the subtitle of a *Wall Street Journal* piece that had appeared during the making of *Free To Choose*: "Nobel Economist Becomes TV Star" (*Wall Street Journal*, October 24, 1979), and in London, during the U.K. broadcast, *The Observer* referred to "Friedman's metamorphosis into TV star" (February 17, 1980, p. 33). Robert Lucas would reflect that, "in the sense of getting through to the public, he was incredible. Cab drivers would start telling you about his work. Everybody knew about what he was doing." (Robert Lucas, interview, March 12, 2013.)

Friedman knew that his series would be compared with *The Age of Uncertainty*: "The long and short of it, they'll call it," he remarked, referring to the difference in height between himself and Galbraith (*Valley Independent* (Mon Valley, Pennsylvania), February 2, 1980). Even ahead of *Free To Choose*'s appearance, the new economics textbook of Baumol and Blinder (1979) had taken an approach like this when contrasting the two, observing (pp. 818–819): "Milton Friedman is barely over five feet tall; John Kenneth Galbraith is about 6½ feet tall. There the similarity ends."[30]

Confident in the quality of *Free To Choose*, Friedman relished the comparison with the Galbraith program. On the eve of the series' premiere, Friedman declared that, unlike *Age of Uncertainty*, his own series amounted to "good television" (*San Francisco Examiner*, January 10, 1980) and was "the first economics program that is also good entertainment" (*San Francisco Chronicle*, January 10, 1980). "We benefited greatly from the Galbraith series," he elaborated after broadcast of his series was completed. "From a technical point of view, it got universally panned, even by those most sympathetic with the view it was expressing. Galbraith was reading

[29] Still earlier reruns appeared in the spring—for example, in the New Jersey area (*The Sunday Star-Ledger* (New Jersey), May 11, 1980).

[30] Friedman evidently took this reference to his height in Baumol and Blinder's textbook in good humor. He had the textbook prominently displayed on a bookshelf in his Hoover Institution office (*Cover Story: Inflation—It's Only Your Money*, PBS, July 1, 1980).

On other occasions, Friedman appeared more aggrieved by references to his height. Arthur Laffer (interview, June 10, 2013) recalled: "In fact, there were a couple of occasions where he took offense at it. [For example,] there was one debate that Galbraith had with him. Milton would go [first] to the podium. Now, I don't know how tall Milton really was… Milton would stand up on a soapbox, you know, a wooden box, to be at the podium. And [when it was his turn to speak] Galbraith got up and sauntered over the podium, and looked out, looked everywhere, and just booted this box out. And, of course, the whole place roars with laughter. And Milton was—you know, that really destroyed him for that debate." Laffer added: "I use the line today, I'm fairly short, I'm 5'6"—that one of the neatest things about being at the University of Chicago is whenever I followed Milton to the podium, I was able to actually raise the microphone."

from a Teleprompter, and unless you're a professional, it's bound to be dull." (*Chicago Tribune*, July 20, 1980, p. 21).[31] In terms not just of these aesthetic aspects, but also by the criteria of audience and impact, too, Friedman in 1980 surpassed Galbraith's 1977 television success.

In considering absolute levels of success and audience reach, however, one should not overstate the popularity of the *Free To Choose* television program. *Free To Choose* was a very successful television series by PBS standards, but it was not a high-rating program by the standards of the major networks. More people likely saw Friedman on his daytime appearances on *Donahue*, a program that regularly drew eight million viewers (*The Sun* (Baltimore), October 16, 1979, p. B5), than they did on his own daytime-and-evening program *Free To Choose* in 1980.[32] Being on PBS and not the three major networks of the time (CBS, ABC, and NBC), it was predestined to lose the ratings in its timeslot and to figure poorly in relation to major U.S. television network shows in other timeslots. Indeed, of the top thirty-two most-watched television programs in the September 1979–April 1980 U.S. television season, all were shows broadcast by the three major networks (Brooks and Marsh, 1999, p. 1253).

Some perspective on the fact that *Free To Choose* was a niche program is provided by a comparison with a network series of the same vintage, a science-fiction drama called *Galactica 1980*. Like *Free To Choose*, this was a ten-part, one-hour-episode program that commenced its U.S. broadcast in January 1980.[33] Furthermore, *Galactica 1980*'s timeslot of early Sunday night also basically mirrored that commonly allotted to *Free To Choose*. The premiere episode of *Galactica 1980* received an audience share rating of 29 (around fifteen million viewers).[34] On the standards by which the major U.S. television networks judged themselves at the time, this ratings performance was considered on the border between success and failure, and *Galactica 1980* ultimately was not renewed beyond its 10-episode first-season run. In contrast, in the case of PBS, a high ratings share during the 1980s was 5 or 6 (Starr, 2001, p. 282). The reality of

[31] The *Los Angeles Times*' television critic had had a similar perspective, arguing of *Free To Choose* (January 11, 1980*a*): "it is technically well-made and therefore probably will not lull viewers to sleep the way John Kenneth Galbraith's *Age of Uncertainty* series did." Halliwell and Purser (1986, p. 11) likewise judged that *Free To Choose* was "incomparably more effective" than *Age of Uncertainty*.

[32] Cumulatively, including reruns and aggregating tuning in to particular episodes, *Free To Choose*'s U.S. viewership may have exceeded by a modest margin, or greater, the viewership of any one of Friedman's *Donahue* appearances. Robert Chitester reported (personal communication, October 1, 2014): "*Free To Choose* was broadcast four times over three years by most public TV stations. We estimate total audience in U.S. was nine to twelve million."

[33] The span of broadcast of *Galactica 1980* was January 27, 1980, to May 4, 1980. Most of the episodes were broadcast starting in mid-March 1980 (McNeil, 1996, p. 311)—the time at which *Free To Choose*'s run on U.S. television was winding up.

[34] This implied that, in the week to January 30, 1980, *Galactica 1980* came in thirty-first of all U.S. television programs broadcast that week. See *The Charlotte Observer* (North Carolina), February 6, 1980.

public-television viewership meant that the aggregate viewing for any one of the episodes of Friedman's PBS program was less than a quarter of that received by ABC's *Galactica 1980*.

***Free To Choose** and Friedman's perspective on freedom and efficiency*

In an episode of the aforementioned *Galactica 1980* series, the Cylon Leader remarks to U.S. citizens on the imperative to undertake reform of their system in order to establish "a truly efficient society."[35] Many critics of *Free To Choose* implied that Friedman's prescriptions in the series had a similarly antiseptic ultimate objective. But Friedman's closing words in the final *Free To Choose* documentary episode—episode 10, "How To Stay Free" stated that he had a different aim: "a truly free society."[36]

This dovetailed with Friedman's prior expressions of what he conceived to be the aim of a free market as a policy choice. "The preservation of liberty, not the promotion of efficiency, is the primary justification for private property," he had remarked in a *Newsweek* column (March 13, 1978). Friedman had in fact long emphasized that, although he believed that a shift by the United States and other countries toward much greater reliance on the market would improve economic efficiency, such added efficiency was an ancillary benefit. In 1961 he had written that the "freedom of the individual" should be the "ultimate goal" when choosing between social arrangements (*Wall Street Journal*, May 18, 1961), and in a 1976 interview with Peter Jay he had observed: "My ultimate objective is individual freedom, and capitalism is certainly a means and not an end." (*The Jay Interview*, ITN, July 17, 1976.)[37]

He continued to believe that freedom and efficiency were both achieved by market arrangements (*Knoxville News-Sentinel* (Tennessee), September 16, 1977), as these promoted a "totally free and productive society."[38] But the priority of these goals in his mind was indicated in the filmed

[35] *Galactica 1980*, Episode 8, "The Night the Cylons Landed: Part 2," ABC, April 20, 1980.

[36] *Free To Choose* (U.S. television version), PBS, Episode 10, "How To Stay Free," March 14, 1980, p. 9 of transcript.

[37] Likewise, in his September 1979 debate, with Peter Jay and others, that was taped for the U.S. version of the "Created Equal" episode of *Free To Choose*, Friedman stated that insofar as he had a "god," it was freedom. (*Free To Choose* (U.S. television version), PBS, Episode 5, "Created Equal" (debate portion), February 15, 1980, p. 18 of transcript. See also Nelson (2020a, Chapter 9.)

Friedman remarked in 1977 that even freedom could be seen as a means to an end, with the end being individual development: "ultimately the real goal is for each of us to become a better person and to develop our own understanding of ourselves. From that point of view, you can say freedom… is the means to that end—and not itself an end. But when you come to look at social relations, it is in that context that freedom becomes the primary goal." (*New Guard*, April 1977, p. 10.)

[38] In a similar vein, in April 1981 Friedman remarked that market arrangements permitted "a widening of opportunities and an improvement in the standard of life" (*Evening Post* (Wellington, New Zealand), April 27, 1981).

portion of the first episode of *Free To Choose*, Friedman stated that a market-based system promoted economic success but, more importantly, it allowed a means by which countries and individuals could engage in voluntary cooperation. "That is why the operation of the free market is so essential. Not only to promote productive efficiency, but even more, to foster harmony and peace among the peoples of the world."[39]

In mid-1980, Milton and Rose Friedman noted that criticisms contained in letters they had received from the general public and in public reviews regarding the *Free To Choose* products (book and series) had included a recurrent accusation that the attitude they advanced was "inhumane." They rejected the criticism, on the grounds that it incorrectly presupposed that they were against the objectives of measures to aid the disadvantaged, rather than the specific measures adopted.[40] Likewise, one of the early published criticisms of Friedman's program stated that "Friedman hates government programs that help the less able" (*San Francisco Examiner*, January 11, 1980*a*), without indicating that Friedman had his own preferred, albeit scaled-back, version of support for low-income individuals.[41] Indeed, as his hardline libertarian critics emphasized, Friedman did not favor a fully *laissez-faire* solution. He had, as indicated above, affirmed his support for a negative income tax in the debate portion of the "Created Equal" episode of the U.S. version of *Free To Choose*. He also had done so in the main, filmed, portion, of the previous episode, "From Cradle to Grave."[42] In addition, the Friedmans outlined in some detail the idea of the negative income tax in the *Free To Choose* book.[43]

Wrapping up

"In some areas, we have more freedom than we've ever had before," Friedman acknowledged in a concluding interview for the wrap-up episode.[44] In this interview, he also stressed that the New

[39] *Free To Choose* (U.S. television version), PBS, Episode 1, "The Power of the Market," January 12, 1980, p. 4 of transcript.

[40] Friedman and Friedman (1980c, p. 15).

[41] The critic, Bill Mandel, also made statements intended as criticisms but that Friedman, and many other economists, could readily embrace as matter-of-fact descriptions of their own position: "he's against government-guaranteed equality of *result*" and "Friedman actually seems to believe the supply-and-demand stuff" (*San Francisco Examiner*, January 11, 1980*a*, emphasis in original).

[42] In this episode, Friedman remarked: "The best, or should I say the least bad, solution I have even been able to devise was something called the negative income tax… [g]uaranteeing at least a minimum income." (*Free To Choose* (U.S. television version), PBS, Episode 4, "From Cradle to Grave," February 2, 1980, p. 7 of transcript.) Friedman's advocacy of the negative income tax was also highlighted in published television listings of the episode, including the following: "Dr. Milton Friedman discusses his belief that the welfare system in the U.S. is dangerous and that the best answer to it is a negative income tax." (*Journal-Star* (Peoria, Illinois), February 2, 1980.)

[43] Friedman and Friedman (1980a, pp. 120–126).

[44] *Free To Choose* (U.S. television version), PBS, Episode 10, "How To Stay Free," March 14, 1980, p. 16 of transcript. For this episode, there was no debate hosted by Robert McKenzie. Instead, following a solo portion of

Deal watershed of greater federal government intervention in the U.S. economy was not something that he altogether opposed: many of Roosevelt's emergency measures had, he affirmed, been commendable and promoted economic recovery.[45] But Friedman indicated that he still wanted many of the permanent regulatory, taxation, and spending measures introduced by Roosevelt wound back: "we have to develop a series of policies which will enable us gradually to move from where we are to where we want to be. The first and most important step in my opinion, is to stop moving in the wrong direction."[46]

In this final episode, Friedman acknowledged that some of the measures he wanted to phase out were popular. But he elaborated in an interview that appeared before the episode aired that this popularity partly reflected, in his assessment, "an ignorance of the consequences of the measures that are taken—ignorance of a kind that economic science is capable of dispelling. So it seems to me that I have a special contribution to make in that sense." (*The Valley Independent* (Mon Valley, Pennsylvania), February 2, 1980.)

Challenges to the accuracy of the series

Some critics of the series, while taking issue with the interpretations and perspective Friedman provided in *Free To Choose*, also challenged him on grounds of factual accuracy. One of the most vehement criticisms of this kind appeared in a letter by Shaun Stewart to the London *Times* (March 28, 1980).[47] This letter contested Friedman's statement about the decontrol of prices that had taken place in western Germany in 1948. This identification of an alleged error by Friedman became a jumping-off point for the letter's criticism of his past work on monetary economics.

The letter's claim of error referred to Friedman's discussion of the relaxation of price controls in postwar Germany in 1948. In the episode on inflation, Friedman had said: "Early one Sunday morning, it was June 20, 1948, the German minister of economics, Ludwig Erhard, a professional economist… abolished almost all controls on prices and wages. Why did he do it on a Sunday morning? It wasn't—as you might suppose—because the stock markets were closed on that day, it was, as he loved to confess, because the offices of the American, the

Friedman's narration, he had a one-one-on discussion with Lawrence Spivak (who had previously interviewed Friedman on NBC's *Meet the Press* in mid-1970).

[45] For further discussion of Friedman's perspective on this matter, see Nelson (2020a, Chapter 2).

[46] *Free To Choose* (U.S. television version), PBS, Episode 10, "How To Stay Free," March 14, 1980, p. 14 of transcript.

[47] Stewart's letter was in reference to the U.K. version of the series, but it concerned the documentary portion of the inflation episode that also appeared in the U.S. version.

British, and the French occupation authorities were closed that day. He was sure that if he had done it when they were open, they would have countermanded the order."[48]

The Stewart letter on this statement made the accurate point that "the German Federal Republic did not exist in 1948." Relatedly, Erhard did not have the post of economics minister at the time of the 1948 decision.[49] That was, instead, a post that Erhard took after the allied-governed portion of Germany became the Federal Republic of Germany in 1949. Friedman's mislabeling of Erhard's 1948 post, however, was not terribly material: Erhard certainly was an official in the economic management of allied-occupied Germany during 1948, being director of the Economic Administration Office of the United Economic Area (covering the portion of occupied Germany governed by the U.S. and U.K. military authorities). Therefore, Erhard was, in effect, part of the pre-1949 government's economic management. Furthermore, Friedman's account, just quoted, clearly recognized that the Federal Republic of Germany did not exist at the time of the 1948 reform and that Erhard was answerable to the allied authorities in this pre-federal-republic stage of postwar western Germany's history.

The *Times* letter, however, went beyond the point about the status of the 1948 government of West Germany: it criticized the substance of Friedman's claim that Erhard engaged in insubordination. Stewart's letter implied that Erhard did not, contrary to the Friedman account, actually have a material role in the decision to decontrol prices at all. Stewart insisted that the decision was not a case of defying the allied authorities: "the western allies had decided on the reform," he claimed. On this account, there was no matter of insubordination arising from Erhard's action because there was no Erhard action, the decontrol decision having been made by Erhard's superiors in the government.

In this counter-claim, however, Stewart probably went too far. Friedman was far from alone in viewing Erhard as having preempted the allied authorities in initiating the decontrol process. It was a longstanding contention. A pamphlet about Erhard published in the 1950s had noted "this clear-cut act of insubordination" on his part (Joesten, 1957, p. 8). In the debate portion of the inflation episode, former Bundesbank governor Otmar Emminger endorsed Friedman's account: "May I, first of all, confirm two facts [on monetary reform and decontrol in Germany in 1948]

[48] *Free To Choose* (U.S. television version), PBS, Episode 9, "How To Cure Inflation," PBS, March 7, 1980, p. 5 of transcript.

[49] The specific point that Erhard's post was not, in 1948, minister of economics was something on which Friedman was taken to task shortly afterward in a letter, discussed presently, by J. Kipp Tenenbaum in the *New York Review of Books* (November 20, 1980), which specifically referred to the corresponding claim in the book version of *Free To Choose*.

which have been so vividly brought out in the film of Professor Friedman…"[50] Even a *New York Review of Books* letter (November 20, 1980) that strongly criticized Friedman's attribution to Erhard of another decision—currency reform—regarded it as factually correct that Erhard "exceeded his authority" by enacting price decontrol.[51]

The familiar narrative according to which Erhard indeed initiated decontrol has been supported by subsequent studies specifically concerned with Germany's economic reforms.[52] These accounts are not inconsistent with the notion that the allied government ultimately had to make the formal authorization of decontrol. They have implied, however, that this was a formality that accepted Erhard's move as a *fait accompli*. In fact, as Friedman's on-air remarks implied, the account that Erhard was acting beyond his rank in announcing decontrol was something he had likely heard over the years from Erhard himself. Erhard, who died in 1977, had been a co-attendee with Friedman of the Mont Pelerin Society in the postwar decades.[53]

Paul Samuelson, for his part, made a point related to Friedman's in his *Newsweek* column of March 9, 1981: "Ludwig Erhard, defying the occupation authorities in Germany, could by a stroke of pen suspend all pre-1948 rationing and turn the West German economy over to the

[50] *Free To Choose* (U.S. television version), PBS, Episode 9, "How To Cure Inflation" (debate portion), March 7, 1980, p. 7 of transcript.

[51] This letter focused on monetary reform and, as Stewart's letter had done in the case of decontrol, derided Erhard's role in the 1948 currency reform—which both the television and book versions of *Free To Choose* had similarly attributed to Erhard. Like the Stewart *Times* letter, this *New York Review of Books* letter instead contended that the decision in question was (alone) made by the allied military authorities. However, such a denial of a key role for Erhard in the currency accounts conflicted very sharply with many other accounts. Such accounts included a later one in an official history of the Deutschmark that stated that the currency reform "initiative was driven primarily" by German officials, with Erhard the "principal promoter" of the reform plan (Buchheim, 1999, p. 94). The appropriate conclusion appears to be that both the book and the series put Erhard too much at the center of currency reform (the installation of the modern mark) but that the backlash against this characterization unduly deprecated Erhard's role.

[52] For example, Kloten (1989, p. 48) referred to Erhard's 'insubordination against the British-American authority," and Buchheim (1999, p. 95) observed that "Erhard announced the wide-ranging relaxation of economic controls and the abolition of price controls" even though "the consent of the military governments was still outstanding" (that is, it was absent). The account of the incident in Mierzejewski (2004, pp. 69–70) indicated that Erhard was reproached for acting without authority and that, although the U.S. authorities were sympathetic with decontrol, the U.K. authorities would have vetoed the decontrol decision if it had not been announced by Erhard without prior consultation.

[53] See, for example, Hartwell (1995, pp. 113–116). The 1980 backlash against Friedman's remarks regarding Erhard made clear that there was some resentment at the extent to which Erhard became the central figure in accounts of the reform of western Germany's economy in the early postwar period. Charles Kindleberger brought this attitude out explicitly when he stated that an "error of historical understanding has to do with the Friedman view that the occupational forces had muddled monetary reform and were saved from ruining it by the wisdom and force of Ludwig Erhard. This is a thoroughly misguided view, based on strong priors, although it is part of legend." (Kindleberger, 1984, p. 23.) Even if one attributes currency reform *per se* wholly to the allied forces, the notion that they were "saved from ruining it" by Erhard actually survives, if (as seems appropriate) one contends that decontrol was Erhard's initiative and was a vital part of the process of harmonizing currency reform with nonfinancial economic recovery.

free-market system." Earlier, in his textbook account, Samuelson (1970, pp. 766−767) had noted that "the 1948 decision by Ludwig Erhard to end the rationing and price controls that were stifling the West German economy… improved the lot of just about every income class."[54] Not only did Samuelson's accounts conflict with the tenor of the Stewart letter to the London *Times*, which had suggested that decontrol (both in 1948 and later measures when Erhard was a minister) had been harmful for the German economy. They also underscored the fact that it was common ground among many economists that free-market reforms had improved economic welfare in western Germany.

Nobody would claim that Friedman was an expert on postwar Germany. Indeed, Continental Europe was generally a weak point in Friedman's knowledge, a fact prefigured by his receiving a C as an undergraduate in a course on European economic history (*Wall Street Journal*, June 1, 1993, page A13, and Nelson, 2020b, Chapter 14). And his statement regarding the specifics of Erhard's decontrol measure was likely not free of error: he had contended that wages were decontrolled by Erhard at the same time as the price decontrol, something that was apparently not the case—a point stressed by Weber (2019).[55] But others, including Emminger in the same program, had made the identical mistake. And, on other occasions, Friedman had been more careful to state that Erhard's decontrol had only pertained to prices.

As well as criticizing Friedman for stating that Erhard instituted wage decontrol, Weber (2019) challenged Friedman's remark that all price controls had been abolished by the 1948 reform. Friedman did, however, grant in various writings that Erhard's 1948 abolition of price controls was very widespread but was not a full-scale elimination.[56] Indeed, the book version of *Free To Choose* stated that the Erhard price decontrol pertained to "almost all" prices.[57] Furthermore, on the basis of a consideration of the discussions in the book version of *Free To Choose* and elsewhere, it is very evident that Friedman did not see his argument regarding the value of the

[54] Similarly, in Instructional Dynamics Economics Cassette Tape 187 (Paul Samuelson series) (September 1975, Part 2), Samuelson stated: "Erhard, as the chancellor [sic] under Occupied Germany, disregarding the American generals' instructions, swept away all price controls."

[55] Specifically, Weber (2019) took issue with a talk in China that Friedman gave in 1980, printed in Friedman (1990d, p. 41), that had similarities to the television discussion of the Erhard move. The same 1990 volume had, on its pages 108 and 123, remarks along the same lines that Friedman had given on his second visit to China, in 1988.

[56] To be specific, a correct statement would be that Erhard lifted controls on all industrial finished products—see Weber (2019, p. 6)—although Weber's contention that price controls remained prevalent through the early 1960s likely seriously overstates the extent to which meaningful price ceilings lingered in the German economy.

[57] Friedman and Friedman (1980a, p. 56). An expression of a similar sentiment came from Walter Heller, who in the *Wall Street Journal* of July 12, 1978 (p. 48 of 1979 reprint), noted: "As Chief of Internal Finance in our Military Government in Germany, 'I was there." Heller stated that "removal of rationing and wage and price controls" had been a source of the economic expansion of western Germany that followed.

Erhard reforms as resting crucially on literally *all* controls being abolished in 1948 and that, instead, what was important was what did, indeed, happen: a clearing-away of a large portion of the price controls that were in force.

A more clear-cut mistake was a numerical misstatement of Friedman's that he made in the episode on the Great Depression. As part of a recapitulation of the case Friedman and Schwartz's *Monetary History* had made that the authorities should not have let the Bank of United States fail, Friedman claimed that, even in the post-failure conditions, 92.5 cents of each dollar was repaid to former depositors.[58] This number was initially repeated in the 1980 book version of *Free To Choose.*[59] In 1986, as part of a brief but highly acrimonious exchange with Joseph L. Lucia over the *Monetary History*'s account of the Great Depression (an exchange discussed in Chapter 14), Friedman and Schwartz noted the fact that the 9.25 cents number was in error and that the *Monetary History*'s correct number of 83.5 cents should have been used.[60] In the 1990 paperback issue of *Free To Choose,* the Friedmans corrected the text to give the 83.5 cents figure.[61]

Notwithstanding the criticisms he received regarding specific aspects of the television program, Friedman was very pleased—both before and after transmission—with the way the *Free To Choose* series turned out. So pleased, in fact, that in later years he tended to play down somewhat the companion part of the *Free To Choose* project—the book by himself and Rose Friedman. But the book was a far more spectacular commercial success than the series, and it reached far more people.

***Free To Choose**: book promotion*

Friedman's publicity drive for *Free To Choose* during January 1980 included appearances specifically focused on the tie-in book coauthored with Rose Friedman. For signing appearances at bookstores, the Friedmans were accompanied by Peter Jovanovich of the book's publisher, Harcourt Brace Jovanovich (whose president was Peter Jovanovich's father, William Jovanovich). With regard to the Friedmans, Peter Jovanovich recalled (interview, March 24, 2015) that he first "met them on the day of publication, of all things. I had been running Macmillan's trade division, and then I joined, in 1980, to run HBJ's trade division." The

[58] *Free To Choose* (U.S. television version), PBS, Episode 3, "Anatomy of Crisis," January 26, 1980, p. 2 of transcript.
[59] Friedman and Friedman (1980a, p. 81).
[60] Friedman and Schwartz (1986b, p. 202).
[61] See Friedman and Friedman (1990b, p. 81).

publication date was January 4 or 5, 1980 (although the book had made it into some U.S. stores just before Christmas).[62] The Friedmans were in New York City for several days through January 8. While located there, they engaged in various book-promotion activities on the weekend of January 5–6 and the following days.[63] This included a press conference on January 7 at which Friedman both publicized the coming series and posed for a photograph in which he and Rose Friedman appeared with a copy of their book. In the photograph, Friedman held the book's back cover to the camera, as it was that cover that consisted of his wife's portrait (*The Plain Dealer* (Cleveland, Ohio), January 13, 1980).[64]

"And there were various events to be held in New York," Peter Jovanovich noted. "My father had a very good relationship with Milton and Rose, and he was CEO of the company. But the trade division was really what was publishing the book, so I ended up spending, actually, more time with Milton and Rose than he did. Sometimes it was the four of us who met together, or [otherwise] the three of us. So I squired him around to the press briefings and things like that, and so on and so forth. And they had a great time in Manhattan." (Peter Jovanovich, interview, March 24, 2015.)

Jovanovich recalled that their interaction made him "really get a sense of, actually, their lack of pretension. If you watch Milton Friedman on television, in his various interviews and so on—and I've seen lots of them—he obviously always thought he was right—and was sometimes rather imperious... But Milton and Rose, around me, acted like, how should I put it, regular people. And that was a little bit of a shock for me, because I spent my career in academic, and then in trade, publishing, and met a lot of famous people… I mean, it's hard to think of Milton and Rose and say, well, they were ordinary Americans. But, in some of the ways they behaved, they were just like ordinary Americans, and it was refreshing. I'd dealt with enough self-important, arrogant people," and, he found, the Friedmans were not like this. "When Milton was debating, then watch out. [But] if his coffee was late, he was a complete gentleman, as far as [saying] 'all right, OK, thank you.'… I've dealt with a lot of Nobel Prize winners. I would have never guessed that Milton Friedman would be one of the more modest ones, but he was… And so I just liked them. Usually, when you're shepherding around famous writers and academics,

[62] Leube (1987a, p. xvii) erroneously gave the release date as spring 1980.

[63] Prior to this, Milton Friedman had mentioned the book when appearing as an interview guest on the weekly program *Wall Street Week* (broadcast on the evening of January 4, 1980, and taped in the program's Maryland studio earlier that day). In its television listings, the national magazine *TV Guide* incorrectly listed the week's scheduled guest for the program as being veteran comedian Milton Berle (see *Honolulu Star-Bulletin*, January 9, 1980).

[64] Friedman also was a studio guest of ABC's *Good Morning America* on the following day, January 8 (*St. Petersburg Times* (Florida), January 8, 1980).

it's like dealing with movie stars. I'm the head of the department, and I've had authors scream at me because the right flowers weren't in their hotel room and all sorts of stuff—you know, like any other celebrities behave badly when they want to. But that was not Milton and Rose." (Peter Jovanovich, interview, March 24, 2015.)

Jovanovich recalled "one particular incident that occurred," when a New York-based periodical published a piece on the Friedmans containing "comments about how they weren't the most fashionably dressed human beings on the planet. And Rose said to me… 'You know, you have to understand. Milton was in the wilderness for most of his career. There were [for a long time] no consulting agreements. We weren't being asked places. We worked hard at what we did. What so many other economists get to see and do and so on—we weren't doing those things.' So she said, 'It's no surprise I don't have the most elegant outfit,' and so on. And I thought that was revealing. Rather than being hurt or snippy or whatever, she just, matter-of-factly, said [that real] success really came late—[though] intellectual success came earlier." (Peter Jovanovich, interview, March 24, 2015.)

In terms of his conversations with Milton Friedman during downtime intervals in their tour, Jovanovich recalled two elements: "One, he delighted in teaching, and two, he was very interested in these little things," such as the details regarding how the book publishing and marketing industry worked. In this vein, "the chit-chat with Milton was about microeconomics. It was not about macroeconomics—I mean, it was to some degree: I think he must have told me 12 times that inflation is, 100 percent, a monetary phenomenon. But most of it was this just him being curious about how things worked," as well as articulating economic principles (Peter Jovanovich, interview, March 24, 2015).

Free To Choose*: book conception*

A book tie-in to a Friedman television series was anticipated early in the planning stages, although Robert Chitester's initial suggestion was that it should consist of an expanded and updated version of *Capitalism and Freedom*.[65] The Friedmans wisely chose instead to produce a separate and free-standing book. Friedman eschewed the possibility of a lightweight, or "coffee-table" book, something often associated with tie-ins to documentary series. "I wasn't going to be something I'd be ashamed of." He instead saw the book as offering "a chance to get people to understand" his arguments in detail (*The Observer* (London), February 17, 1980, p. 33). "I'd say

[65] Friedman and Friedman (1998, p. 471).

one of the reasons why *Free To Choose* [the book] was so successful," Peter Jovanovich remarked, "… was that it was laying out very straightforward, easily understood principles. He had an ability to slip into a mode of erudition that was not that complex, so that, if you were not an economist, you could understand what was at stake… He was trying to explain, in basic terms, well, how does the world work?" (Peter Jovanovich, interview, March 24, 2015.)

In October 1979, as he was completing the series, Milton Friedman had just finished drafting the book with Rose Friedman and emphasized the book's centrality, stating that "I've spent my life as a scholar, not as a TV performer," and indicating that "I regard the whole TV program as an advertisement for the book." (*Wall Street Journal*, October 24, 1979, pp. 1, 32.) Though persuaded of the usefulness of a television program, he maintained: "People can be amused or entertained by a TV show, but their minds can be changed only by thinking about it carefully—by pondering it." (*Wall Street Journal*, October 24, 1979, p 32.)

But once he had had a chance to see the completed television series, Friedman's enthusiasm for the program was charged up, and his ranking of the two arms of the project was reversed: ten days ahead of the premiere of the program, he declared (*Wall Street Week*, Maryland Public Television, January 4, 1980, p. 13 of transcript): "the series is entertaining—the book is much duller." He nevertheless stressed that the television series' aim was modest: "The policy of the TV programs is not to persuade anybody [but to] make people think in a different way." (*Time* magazine, March 10, 1980.) The book provided a means of reinforcing this process.

The *Free To Choose* television series and book differed in their authorship: the text in the series' opening credits was, as already indicated, "*Free To Choose: A Personal Statement* By Milton Friedman" while the interior cover page of the book version read "Milton and Rose Friedman *Free To Choose: A Personal Statement*."[66] Discussing the making of the first book with Friedman for which she was receiving full co-credit, Rose Friedman observed: "It would be impossible to point out words or ideas which are exclusively mine or his. We did it together." The Friedmans explained their arrangement for the writing of the book as one in which one author composed a first draft of specific chapters, with the other author then working on the next draft of that chapter (*New York Times*, February 24, 1980*a*; *The Australian*, November 5, 1982).

Furthermore, the Friedmans' close friend Michael Walker has stressed that the

[66] Rose Friedman's middle initial of D (corresponding to her original surname, and now middle name, of Director) was reserved for the book's copyright notice.

dictation/interview format, used by the Friedmans for generating drafts of some of the popular writings Friedman had produced in the past, also likely played a key role in the writing of much of *Free To Choose*. Walker remarked that a "difference is certainly evident" between *Capitalism and Freedom* and *Free To Choose* on account of the manner in which the latter book was produced: "In fact, it was *not* written, it was *spoken*—spoken from questions that were put to him by Rose and tape-recorded," after which Rose Friedman took a first pass at reworking the audiotaped material into a prose piece. This mode of the book's creation, Walker believed, contributed to its accessibility to the reader (Michael Walker, interview, June 21, 2013.)

Friedman's satisfaction with the televised version may have played a role in his misremembering, in later years, the circumstances behind the writing of the *Free To Choose* book. It was certainly true that the TV series was well underway once the book was being drafted and that the catalyst for the *Free To Choose* project had been to produce a television series not a book. Thus, it was not correct to suggest that the television series was an adaptation of the book: Friedman himself upbraided his secretary Gloria Valentine when she once described the series in this way: "I was forced to think of *Free To Choose* as a TV series that was turned into a book because I got it wrong several times and Professor Friedman had to correct me." (Gloria Valentine, personal communication, February 26, 2009.)[67] Furthermore, it is the case, as Friedman remarked after the book's release (*New York Times*, February 24, 1980), that "[t]he book follows the series in content," as both products were divided into ten segments, and the episode titles of the series coincided with the chapter titles of the book.

In the later years of his life, however, Friedman went further, implying that the book was a straight adaptation of the broadcast series by stating that "the book was based on the TV program, because I insisted that I was not going to talk to a written script for the TV program, but I was just going to talk [impromptu]. Then, from the transcript of the TV program, we developed the book."[68]

This recollection was mistaken in important respects. It is clear that there was indeed no script for the television program and that Friedman's direct-to-camera monologues were not scripted.[69]

[67] The U.K. 1980 Pelican paperback edition of *Free To Choose*, no doubt without the Friedmans' permission, described the television series as "based on" the book of the same name (see the first interior page of Friedman and Friedman, 1980d).

[68] *Booknotes*, CSPAN, November 20, 1994, p. 7 of transcript.

[69] For example, in the first episode of the series as broadcast, in one monologue, Friedman fluffed a line and then backtracked. In another monologue, relating the "pencil" story, Friedman appeared to realize in the course of his narration that he had left material out and backtracked.

But it would be wrong to suggest that the book closely adhered to the transcripts of the series.[70] For example, Friedman's remark in episode 2 that markets operate on "self-interest or, if you prefer**,** greed" did not appear in the book version.[71] Likewise, some of his remarks concerning Keynes and pump-priming in the episode "Anatomy of Crisis" had no counterpart in the book.[72]

Not only did the *Free To Choose* book not use the text of the television series, it also relied fairly sparingly on text used in previous articles by Friedman. In contrast to *Capitalism and Freedom*, which had drawn liberally on preexisting Friedman publications and lectures, the *Free To Choose* book had only scattered repetition of text used in *Newsweek* columns or other published material.[73] An exception was the material on vouchers, portions of which overlapped considerably with a Friedman article in the *New York Times Magazine* (September 23, 1973).

The fact that much of the material in the book was not in the television program or in previous Milton Friedman products confirmed his observation around the time of the launch of the book and series that the book "expands more at length on the ideas that are presented in that series."[74] It would consequently not be correct to treat the book as subordinate to the television series in the sense of being a virtual transcription of the latter. Friedman's characterization at the time that the written version of *Free To Choose* as "a book that we've written to go along with this program" was more accurate than the later impression he gave that the book consisted of edited transcripts of the television series.[75] Rather, as indicated above, the book arose out of a Rose and

[70] Nevertheless, the *New York Times* (December 21, 2006) contained the basically incorrect (and certainly very incomplete) statement: "The book was drawn from transcripts of the television series."

[71] *Free To Choose* (U.S. television series), PBS, Episode 7, "Who Protects the Consumer?," February 23, 1980, p. 2 of transcript. This passage also was included in excerpts from the television series that appeared in *The Listener*, the U.K. broadcasting-focused magazine that presented coverage of programs recently transmitted on BBC radio and television. The magazine presented several articles in the spring of 1980, tied to the six-part version of *Free To Choose* shown nationally on BBC2. The edition of *The Listener* containing the quoted article was that for April 17, 1980. In the article, "if you prefer" was rendered as "if you prefer it."

[72] *Free To Choose* (U.S. television series), PBS, Episode 3, "Anatomy of Crisis," January 26, 1980, p. 6 of transcript. Reflecting his fondness for getting into print pieces of text he had written but not previously published, Friedman did later put some of his remarks on Keynes in that episode into written form as part of an article on Keynes in *The Economist* (London), June 4, 1983.

[73] In addition, the call for a unilateral abolition by the United States of its trade restrictions that appeared in *Free To Choose* (1980a, pp. 50–51) was a lightly rephrased version of text that in *Capitalism and Freedom* (Friedman, 1962a, p. 74). The *Free To Choose* book also had a passage that had appeared in a recent *Newsweek* column: this was probably a case of the column drawing on the *Free To Choose* manuscript, rather than the reverse. This passage contains a criticism of media coverage of economics: "Reporters and TV commentators seem especially resistant to the elementary principles they supposedly imbibed in freshman economics." (*Newsweek*, June 4, 1979; Friedman and Friedman, 1980a, p. 220.) The wording of the sentence appeared to be carefully constructed in order that Friedman did not claim personal knowledge of freshman teaching, his career experience in undergraduate teaching of any kind being notably limited.

[74] *Wall Street Week*, Maryland Public Television, January 4, 1980, p. 13 of transcript.

[75] *Free To Choose* (U.S. television series), PBS, Episode 7, "Who Protects the Consumer?," February 23, 1980, p. 14 of transcript.

Milton Friedman's collaborative process, starting after filming of *Free To Choose* began and spanning January-September 1979, as discussed in the previous chapter.

When commenting on a state of affairs in which her husband was often much more in the limelight than herself, Rose Friedman would explain: "I am just not a very competitive person." (*The Australian*, November 5, 1982.) In repeating this sentiment a year-and-a-half later, however, she added: "Being human, however, I do resent people telling me how much they liked my 'husband's book.' But only people who do not know us make this mistake. And most important, my husband always gives me equal billing." (*San Francisco Chronicle*, March 18, 1984, p. 9.)

The *Free To Choose* book was their most certified joint collaboration yet, with coauthor credit. Rose Friedman had, however, remarked when the whole *Free To Choose* project was only starting: "In almost thirty-nine years, neither one of us has said: 'That's your work.'" (*The Anchorage Times* (Alaska), June 10, 1977.) She had also explained how she perceived her overall position *vis a vis* all of Friedman's output: "My role is to provide him with the conditions that are best for his work. Then I'm [also] his sounding board, his critic, and his editor." (*The Argus* (Cape Town, South Africa), March 26, 1976.) She added several months later: "My husband has never written anything without talking to me, so I call our marriage a 'partnership.' I don't want to imply that I've done his work. I've never had ambitions of competing with my husband—and I'm smart enough to know that I couldn't." (*Sunday Sun-Times* (Chicago), October 31, 1976.) With regard to the *Free To Choose* book specifically, she would observe: "It would be impossible to point out words or ideas which are exclusively mine or his. We did it together." (*The Australian*, November 5, 1982.)

After the *Free To Choose* book's release, Rose Friedman alluded to the fact that the *New York Times* had not even reviewed their first published collaboration, *Capitalism and Freedom*. "Now, with this new one, they're writing news stories about it and reviewing it twice." (*The Observer* (London), February 17, 1980, p. 35.) The fact that *Free To Choose* received reviews in essentially every major media publication was a source of great satisfaction also for Milton Friedman, who saw it as a sign not only of the success of the *Free To Choose* enterprise but also of the fact of renewed interest in free-market ideas among the general public and opinion leaders.[76] In addition, a number of the reviews also appeared belatedly—for example, in March, when the book had been on release for two months. The appearance of these later reviews likely

[76] See Friedman (1982b, p. vii; 1991d), and *Booknotes*, CSPAN, November 20, 1994, p. 7 of transcript.

reflected a conclusion that the book could not be ignored, as it had become a major commercial success.

A bestseller emerges

Appearing in the *New York Times* the same day (February 24, 1980) as one of its reviews of the *Free To Choose* book was an interview with the authors. The printed location of the interview was revealing: it appeared in a regular *New York Times* column titled "Behind the Best Sellers." The book version of *Free To Choose* proved to be a blockbuster in book sales in a way the television version was not in audience ratings.

Peter Jovanovich recalled that "the book skyrocketed so fast" in sales. "Nobody saw this coming, even though there was the television program… So it was quite a phenomenon." The amount of copies printed in late 1979 for early-1980 publication had been high by the standards of Friedman's past books but modest in comparison with many other mass-market books: "our initial print run… was certainly inadequate." As a consequence of the unexpected demand for copies, "we were just printing like crazy. And I can't remember what our initial print run was, but it was certainly inadequate. So there were various periods in which… we were out of stock. And usually Rose, but Milton, also, called me to say, 'I was in Chicago, and they didn't have any books in the store.' And I would explain, 'We've got another 50,000 print run coming in this Friday, so it'll be on the trucks,' and so on. And I said, 'Demand is just far exceeding what the bookstores originally ordered.' The [pre-publication] orders were pretty modest, in fact." Jovanovich recalled that Milton Friedman had responded to this explanation, "Well, that's understandable"—a contrast with Jovanovich's past experiences with book writers, which had included "an occasion when I had to explain to an author that the book is out of stock, and still, I was thinking, "Could this person kill me with his phone?'" (Peter Jovanovich, interview, March 24, 2015.)

The initial print run was 35,000 (*New York Times,* February 24, 1980*a*). This scale of printing was considered to be a gamble on the part of the publishers, as Friedman's prior book sales did not really justify such a large run (see Nelson, 2020b, Chapter 11). However, sales since the turn-of-decade release of the book proved to be so brisk that, by early February, the print run had reached 100,000 (*San Diego Union* (California), February 8, 1980; *New York Times,* February 24, 1980*a*). By early March, about 107,000 copies had been sold in the United States (*Time* magazine, March 10, 1980). Eight months later, on November 9, the latest *New York Times* bestseller list had the book at number 12 in nonfiction—its fortieth week in the top-15 list (*New*

York Times, November 9, 1980). By early 1981, the hardcover edition had sold 400,000 copies (*Boston Globe*, April 1, 1981, p. 45).[77] The Friedmans' *Free To Choose* was the third-bestselling hardcover nonfiction book in the United States for calendar year 1980, and ninth-bestselling for the period August 1980-July 1981 (Lane, 1981, p. 426).[78]

A paperback edition was released in the United States in January 1981—although, oddly, the paperback did not include a new foreword or other material that could have acknowledged events that had occurred in the country since the hardback publication, most notably the election victory of Ronald Reagan.[79] The paperback edition itself also sold very well, and a front-page article on the *Wall Street Journal* (March 12, 1981) noted that the paperback edition was the number-three bestseller on college campuses—a development that the *Journal* took, along with Reagan's ascendancy, as a sign of "changing times."

The domestic success of the *Free To Choose* book was paralleled in international sales and attention. "In terms of impact around the world, people just have almost no conception of how much impact it had," Michael Walker observed (interview, June 21, 2013).

Economic rigor and the ***Free To Choose*** *book*

As a curtain-raiser for the economic policies of the 1980s, the *Free To Choose* book is of historical significance. Furthermore, it remains better-remembered than the television series, even in an age when the series is easily accessed electronically. But both at the time and in later years, the *Free To Choose* book left the economics profession far less impressed on the whole than the Friedmans' earlier book, *Capitalism and Freedom*.[80] Robert Lucas, for example, stated in a 1982 interview (Klamer, 1984, p. 52): "I like *Capitalism and Freedom* a lot. It's really written for economists in a way that *Free To Choose* isn't. *Free To Choose* gets careless about a

[77] See also Friedman (1982b, p. vii).

[78] Leube (1987b, p. xvii) erroneously stated that *Free To Choose* was the absolute top-selling nonfiction book in the United States in calendar 1980.

[79] The January 1981 publication date of the paperback was noted in the book itself as well as in the *New York Times* (January 11, 1981). It would have highly feasible for the paperback to have included material on Reagan's election, even though this only about two months before publication. This feasibility is brought out by the fact that, in the month of January 1981 in which the *Free To Choose* paperback appeared, "instant books" were appearing on the paperback market commemorating the life of John Lennon (after his assassination on December 8, 1980).

[80] As discussed in Nelson (2020a, Chapter 11), *Capitalism and Freedom* itself got a boost from the publicity generated by the *Free To Choose* products. A paperback reissue of the book was made by the University of Chicago Press in 1982—with the cover of this version alluding to *Free To Choose* by identifying *Capitalism and Freedom* as "[t]he classic statement of Milton Friedman's economic philosophy," and with Friedman providing a new preface (Friedman, 1982b).

lot of points that *Capitalism and Freedom* is very careful about." Over thirty years later, Lucas reaffirmed this judgment in an interview, conducted for this book (March 12, 2013). Friedman himself essentially granted much of this criticism as early as 1982 when he made it clear that he did not think *Free To Choose* was a better book than the "more fundamental" *Capitalism and Freedom*.[81] He had earlier acknowledged with regard to the content of the two books, "There is a lot of duplication." (*New York Post*, March 4, 1980.)

In a sense, the fact that the book was rated less highly as a contribution to economics than *Capitalism and Freedom* was a sign of success. Both books were aimed at the popular book market, but only *Free To Choose* made a big splash in that market.[82] The Friedmans' preface to *Free To Choose* made it clear that the book would contain more applications and less abstract discussion than *Capitalism and Freedom*, which they considered to be a parent of the new book.[83] *Free To Choose* did contain considerable analytical material, but it presented this material in a way that evidently worked, in the sense of being digestible by a mass audience.

Furthermore, what economists perceived as a lack of rigor in *Free To Choose* reflected to some extent a change in Friedman's perspective on the modeling of public-sector behavior. Both *Capitalism and Freedom* and the *Free To Choose* book had covered the subject of externalities, but by the time of *Free To Choose* Friedman was even less amenable to the notion that externalities justified government intervention than before.

The notion that government responses to externalities might make matters worse was a theme Friedman was accustomed to articulating by the early 1960s, but over the following two decades it had come to figure more prominently in his discussions. In a speech at the University of New Hampshire in May 1975, for example, Friedman remarked that when government funds were devoted to certain objectives, a recurring government outlay was committed even if the objectives were never achieved (*Nashua Telegraph* (New Hampshire), May 26, 1975). Not only did such programs not achieve their aims, he believed, but also the outcome of government programs often differed in an adverse way from what well-meaning proposers intend (*Arkansas Democrat* (Little Rock), February 22, 1974)—in part because he felt that more self-interested parties tended to have a dominant role in shaping the form of the program (*The Listener* (London), May 30, 1974, p. 689). This reasoning underlay Friedman's dictum that both

[81] Friedman (1982b, p. vii). See also Friedman (1991d) and the discussion in Nelson (2020b, Chapter 11).

[82] *Capitalism and Freedom*'s sales had, nonetheless, made it one of the University of Chicago Press' major commercial successes. The *New York Times* (February 24, 1980*a*) was one of many sources reporting that it had sold about 400,000 copies by the early 1980s.

[83] Friedman and Friedman (1980a, p. ix).

politicians and economists found that their best intentions often produced the worst harm (*Sacramento Bee* (California), February 20, 1977, p. A4). In February 1978, Friedman suggested that he had been struck by "the added experience we have had over the past decades of government failure. Indeed, as of today it is hard not to start out by saying that the right way to go is to assume first that there is government failure before you look at market failure."[84] In a talk given in Virginia on March 4, 1981, he stated that this government failure applied to attempts to deal with market failure: "government attempts to deal with such externalities have typically turned out to do more harm than good."[85]

The emphasis on government failure was buoyed by Friedman's interest in the public-choice literature—which the Friedmans' foreword to *Free To Choose* indicated was a body of research that in large part had emerged since the writing of the previous book and that had informed the arguments in the new book.[86]

Friedman's absorption in the public-choice literature, which sometimes seemed uncritical, is discussed elsewhere in this book and in companion volumes. It accounted for some of the difference between the perspectives of *Capitalism and Freedom* and *Free To Choose*. It had also been manifested in many other Friedman statements in the 1970s and 1980s. This included Friedman's statement: "For all I know, the most powerful person [in the United States] is somebody whose last name we don't know." (*Chicago Tribune,* July 17, 1977, p. A1.) In isolation, this statement might seem somewhat paranoid. But it was actually a corollary of a message of some of the public-choice literature, which had emphasized the role of the bureaucracy in the shaping of U.S. government policy.[87]

Despite this change in perspective, his outlook in *Free To Choose* remained similar to that in *Capitalism and Freedom*. And like that previous book, *Free To Choose* was primarily about market economics rather than monetary policy. Indeed, Ackerman (1982, p. 26) remarked: "The 'monetarism' for which Milton Friedman is well-known among economists is tacked on almost as an afterthought at the end of *Free To Choose*." Likewise, an economics journalist covering Friedman's visit to Australia in April 1981 estimated that "only about 10 percent of what he says has anything to do with 'monetarism.'" (*Sydney Morning Herald,* April 9, 1981.) These

[84] *Milton Friedman Speaks,* Episode 4, "The Role of Government in a Free Society," taped February 9, 1978, p. 22 of transcript.

[85] Friedman (1984e, p. 20). For the date of the lecture, see Valentine (1987, p. 544).

[86] Friedman and Friedman (1980a, pp. ix–x).

[87] For example, Gordon Tullock (1980, p. 178) wrote, with regard to the U.K. situation, that the "government finds itself so dominated by the civil servants."

assessments underestimated the degree to which the *Free To Choose* book covered monetarism, as the book's ten chapters included not only Chapter 9's "The Cure for Inflation," on the money/prices link, and Chapter 3's "The Anatomy of Crisis," which recapitulated the Friedman-Schwartz account of the Great Contraction and discussed the history of the Federal Reserve System. Monetary policy matters therefore took up about one-fifth of the *Free To Choose* book.

That percentage, although appreciable, confirmed that monetarism was largely a separate subject from advocacy of market economics.[88] Indeed, in late 1980, Friedman emphasized in his *Newsweek* column that "monetarist" was not synonymous with "pro-free market," even though he was associated with both labels (*Newsweek*, October 27, 1980). The fact that *Free To Choose* was mostly concerned with economic issues outside the area of monetary policy mirrored the contents over the years of Friedman's *Newsweek* column. The book also had occasion to cover matters at the intersection of microeconomics and macroeconomics, such as labor economics, with the authors reaffirming their opposition to the minimum wage.[89]

Although the abolition of the minimum wage was included, some of the other controversial Friedman positions on limiting government were also not covered. Martin Feldstein remarked of the book: "Well, it didn't have the same intellectual bite, and it—and it didn't sort of take on all of the sacred cows in Friedman style." (Martin Feldstein, interview, November 21, 2013.) Jeremy Siegel argued: "Really he wanted to expose the masses to his philosophy. So it's sort of 'Friedman-lite.' I mean, *Capitalism and Freedom* is much more radical in some sense, [for example in saying] 'Auction off the national parks.'… Obviously, in *Capitalism and Freedom*, it was j it was definitely pure Friedman, while this was 'Friedman-lite' for the masses. And extremely well done for the masses. So it was very, very different in purpose." (Jeremy Siegel, interview, September 17, 2013.)

Nevertheless, the position that came through from *Free To Choose* was sufficiently dramatic for most of the criticism to be to the effect that it had taken free-market positions too far. Some of this criticism came from economists—most eminently Kenneth Arrow, who wrote a long review in the *New Republic*. Arrow's statement that the Friedmans' "lack of interest in the distribution of income appears heartless" (*New Republic*, March 22, 1980, p. 27). With regard to the word "heartless," Arrow recalled, "I'm afraid I used that word; I should not have used it… This, I

[88] It was also a matter on which Rose Friedman was less steeped than she was with market economics. Milton Friedman took responsibility for drafting the first versions of the *Free To Choose* and *Tyranny of the Status Quo* chapters on money (*San Francisco Chronicle*, March 18, 1984, p. 9).
[89] Friedman and Friedman (1980a, pp. 237–238).

think, hurt him, and he wrote me a very angry letter." In the letter, in which Friedman pointed out instances in which the book had supported help to those earning low incomes, he likened his reaction to Arrow's remarks to being struck in the groin.[90] In the years that followed, "I made some attempts to meet him, and there were all these excuses [given why a meeting was not feasible]. I mean, we saw each other once or twice, but he did not seem to want to have meetings with me." (Kenneth Arrow, interview, December 7, 2013.) Consequently, although they were colleagues at Stanford University when Arrow rejoined the university in 1979 (so, in total, Friedman and Arrow were colleagues on and off from 1947 to 2006), their interactions were rare. By the second half of the 1980s, relations had recovered, and Friedman was a panelist at a seventy-fifth birthday event for Friedman in mid-1987.

Personal investment decisions in the Great Inflation

Following the success of *Free To Choose*, Friedman's celebrity was elevated further—though he put this into perspective by remarking that Galbraith and, in his time, Keynes, had achieved greater celebrity status than he had (Martin, 1983, p. 62).

The success of the project also transformed the Friedmans' financial position. Peter Jovanovich noted that "this was the first time, by the way, that they made any money, really. I know they did some consulting work and so on, but as far as money is concerned, this is the first time they really made any money, aside from his professorship and so on." (Peter Jovanovich, interview, March 24, 2015.) The fact that the Friedmans' financial position was greatly altered by *Free To Choose* was also stressed by Anna Schwartz in numerous conversations with the author over the years.

Capitalism and Freedom had done well enough to help finance the Friedmans' move in the 1960s to their larger (several-hundred acre) East Coast summer home—Capitaf—as well as major renovations to that home (*New York Times*, January 25, 1970, pp. 80, 84). But it was the Nobel award in 1976—whose $150,000-plus tax-free prize-money Friedman was photographed collecting as a check during the Stockholm festivities (*Independent/Press-Telegram* (Long Beach, California), December 14, 1976)—and then *Free To Choose* that put the Friedmans in a new financial league.[91] With regard to the television series, the Friedmans as of early 1980 were said to have earned about 100,000 pounds sterling (and to have 8 percent royalties of prospective

[90] Milton Friedman letter to Kenneth Arrow, March 19, 1980, Kenneth Arrow Papers, Duke University library.

[91] The precise dollar amount was closer to $160,000 (*Milwaukee Sentinel* (Wisconsin), October 30, 1976) and was given on one occasion as $166,000 (*Honolulu Advertiser*, September 26, 1977).

VHS and 16mm-film sales of *Free To Choose* visual products, although such sales ended up amounting to little in the 1980s) (*Daily Mail* (London), March 18, 1980).[92] And the book was a source of still more, and far greater, flows of income.

What Friedman could command as a speaking fee also changed dramatically. In July 1970, his speaking fee (at non-research events) was said to be $3,000 per talk (*The Oregonian* (Portland, Oregon), July 30, 1970). The price level ten years later had roughly doubled, but Friedman's speaking fee had quadrupled. By 1980, his fee was about $12,000 per lecture (*Fortune*, February 25, 1980, p. 110; *Chicago Tribune*, July 20, 1980, p. 21), and by late 1983, $15,000 (*San Francisco Chronicle*, October 6, 1983.)

What was Friedman's reaction to the turbulence of equity prices and interest rates in the 1970s and early 1980s in terms of management of his own (and, from 1976 onward, dramatically growing) portfolio? On this matter, Friedman was usually reluctant to be drawn in detail. Asked by a reporter, on the day of the October 1976 announcement, what he would do with his Nobel Prize award money, Friedman replied: "It's none of your business." (*The Detroit News*, October 15, 1976.) He expressed the same sentiment in friendlier terms a couple of weeks later during an appearance in Milwaukee: "Even if I knew, I wouldn't tell you." (*Milwaukee Sentinel* (Wisconsin), October 30, 1976.)[93] Nevertheless, a retort, often expressed sharply, that he would not comment on his personal finances remained something of a Friedman mantra. Asked in 1979 by a studio-audience member what he did with his money, Friedman replied, "That is my business and not yours." (*Donahue*, NBC, September 6, 1979). Similarly, when in 1981 an East Coast reporter who was writing a story on the wealth-management strategies of major economists contacted Friedman, Friedman returned the call "collect" (that is, reverse charge) from California and answered: "That's none of your business." (*Fort Lauderdale News and Sun-Sentinel* (Florida), August 23, 1981.) With regard to his earnings (as distinct from his portfolio decisions), Friedman had told a magazine at the time of *Free To Choose*'s rollout, "It's none of your damn business." (*Fortune,* February 25, 1980, p. 108). In 1986, he confirmed that "I have done very well [financially]," but added, once more: "I am not going to talk about my personal investment policy." (*American Banker,* April 30, 1986, p. 20.)

[92] The visual products, offered on the wholesale market rather than on a sell-through basis, were a set of videocassettes of the *Free To Choose* program (priced at $4,800), the corresponding 16-millimeter film version ($6,200), and the *Milton Friedman Speaks* series ($7,000 as a set, or $500 on an individual-episode basis.) (*Fortune*, February 25, 1980, p. 110.)

[93] On the return trip to Chicago from Milwaukee, Friedman did confirm that he had thought about how to spend the money. "I have, but I haven't decided yet. And I wouldn't tell you." (*Milwaukee Journal* (Wisconsin), November 1, 1976, Section 1, p. 12.)

This reticence reflected not only privacy considerations but also the sharp distinction made by Friedman between economic commentary and financial advice. Some financial columnists of the era, such as Sylvia Porter, covered both areas, and even Paul Samuelson occasionally gave financial advice in his *Newsweek* column.[94] Friedman generally refrained from doing so, and he likewise tended to eschew requests received from correspondents, interviewers, and investors to give financial advice: "You wouldn't ask a physicist to fix your furnace, would you?," he replied on a talk show when asked about where the market was going (*Dinah*!, March 23, 1977).[95]

In particular, Friedman was, as he noted in 1969, was "always very hesitant to predict stock prices" (Instructional Dynamics Economics Cassette Tape 39, first-half December 1969). Not being a business expert, he was hesitant to talk about prices of equity claims or the corporate profit behavior that ostensibly was the proximate driver of such prices. "I have never gone in for the business of trying to predict corporate profits, because I have not really studied that aspect of it carefully enough to have any considered and informed judgment," he explained (Instructional Dynamics Economics Cassette Tape 125, July 18, 1973).[96] Furthermore, his confidence in the connection between stock prices and those economic aggregates that he was more comfortable talking about—like money, real output, and inflation—largely dissipated in the 1970s. The aberrational behavior of equity prices during that decade greatly reinforced Friedman's already skeptical perspective on the reliability of the relations between equity-price behavior and the economy and hardened his reluctance to opine on the prospects for the stock market.

Friedman was willing, nevertheless, to discuss on occasion in general terms the principles of investing in the conditions of the Great Inflation and, in particular, to consider the suitability of different assets as protection against inflation. After inflation broke out again in 1973, he repeatedly expressed the view that long-term bond rates, although moving in the right direction, were too low in their absolute level (see, for example, Instructional Dynamics Economics Cassette Tape 130, September 26, 1973). As was indicated in Chapter 2 above, Friedman was impressed with the degree to which short-term nominal interest rates adjusted rapidly in response

[94] See, for example, his columns "Coping Sensibly" (March 6, 1978) and "No-Fuss Canny Investing" (February 4, 1980), reprinted in Samuelson (1983, pp. 198–203). Financial advice, as well as deliberations on the performance of mutual funds, formed the basis also for many instalments of Samuelson's audiocassette series.

[95] This position long predated the turbulence of the 1970s. In 1959, Friedman had remarked: "To expect an economist to be an expert on playing the stock market is like expecting a theoretical physicist to be able to fix his own furnace." (*Cleveland Daily News*, May 25, 1959, p. 2.) Friedman occasionally deviated from his practice of not engaging in dialogues on financial advice, particularly if giving the advice allowed him to talk about the costs of inflation. See, for example, *Newsweek*, October 8, 1973.

[96] Similarly, in *The Sunday Sun* (Baltimore), January 19, 1975, he remarked, "I won't guess about dividends," though he was prepared to make what was essentially a macroeconomic point—on the need to adjust estimates of corporate earnings for inflation.

to inflation developments during the late 1960s and the first half of the 1970s. In these circumstances, he preferred rolling over short-term securities to investing in long-term securities. But he stressed that he was recommending this as the best means of proxying the return on a physical asset by holding a nominal asset (*Chicago Sun-Times*, November 18, 1976). And real rates on short-term securities remained, for the most part, abnormally low on average—failing to have the trait, which some real assets possessed over the same period, of rising in real value.

In these circumstances—and with stocks having seemingly disqualified themselves as useful stand-ins for real assets—Friedman therefore pointed directly, as he had in the past, to the holding of physical goods as a means of protecting oneself against inflation. In the case of households, this meant purchases not only of real estate but also of other durable consumer goods, some of which would be classified as luxury goods. Friedman's real-assets recommendation therefore translated into his dictum: "The only effective hedge against taxes and inflation is high living." (*Chicago Tribune*, July 20, 1980, p. 22.) In an interview given in spring 1980, Friedman gave jewelry as an example (*Money* magazine, May 1980): "Let's suppose you invest $10,000 in a diamond ring and $10,000 in a government bond. From the diamond ring, you get income from the pleasure of wearing it, which you don't have to report on your tax return. From the bond, you get a $1,000 income a year—which you have to report and pay taxes on. So that diamond ring is a tax shelter."

As far as his own investments were concerned, Friedman indicated in the first half of the 1970s that (despite his growing reservations about equities as an investment) he was an owner of stock in U.S. firms (*Meet the Press*, NBC, June 28, 1970, p. 5 of transcript) and that he carried out the purchases himself (Instructional Dynamics Economics Cassette Tape 135, December 4, 1973). Rose Friedman later revealed, however, that later in the 1970s Friedman went from organizing his own share portfolio to delegating this task to an investment adviser after finding that he did not have time to adjust it promptly himself (*Daily Mail* (London), March 18, 1980). Then and for many years beyond, the details of Friedman's investments were mainly handled by the New York firm Oppenheimer and Company rather than himself, although he was consulted on the overall portfolio choice. The Friedmans' investment portfolio generally included both stocks and interest-bearing securities, and Milton Friedman himself disclosed in mid-1978 that he was investing in Japanese securities because "the Japanese yen is a very strong currency" and further U.S. dollar depreciation was "almost a sure thing." (*Wall Street Journal*, July 17, 1978, p. 27.)[97]

[97] In this interview, Friedman also stressed that he focused on long-term investments—an approach he continued in the 1980s, as discussed presently.

Paul Samuelson would disclose that at the end of the 1970s, the only financial-market instruments in which Friedman was investing were longer-term interest-bearing securities.[98] The switch to a bonds-only position was consistent with the scorn Friedman expressed at this time on the notion that stocks were a good hedge against inflation: "Stocks have proved a good hedge [until the mid-1960s, and] then proved a bad hedge." (*Donahue*, NBC, September 6, 1979.) As noted, he had also been critical of bonds as an investment over most of the 1970s. But with the coupon associated with newly issued ten-year U.S. Treasury securities passing 10 percent in October 1979, and Friedman initially reacting fairly favorably to the Federal Reserve announcements that month, he believed that successful disinflation made long-maturity Treasury securities a good investment: "if we really have come to a turning point, then long-term bonds will be a great buy. That's the gamble you're taking." (*Money* magazine, May 1980.)

In contrast, he believed, accurately, that stock prices, which had already stagnated in the 1970s, might be hit badly during a period of sustained monetary restriction. In *Newsweek* in 1978, he had stated that a monetary policy program to lower inflation would have short-run output costs but would ultimately usher in a noninflationary recovery "and even a stock market boom." (*Newsweek*, April 24, 1978.) By 1983, though Friedman was concerned (as it turned out, unduly) about the permanence of the decline in U.S. inflation with the stock market in better shape, Friedman had some good things to say about investing in stocks. In a rare discussion—in general terms—of his personal investment policy, Friedman observed, "On the whole, I believe in having a good part of your investments in stocks, in both good and bad times. We're living in a very uncertain world. Inflation is always a possibility—though it's by no means a certainty. My practice is to divide assets among those investments that will benefit from inflation and those that will be hurt by inflation, and to keep the portfolio balanced more or less right through the business cycle. I might do a little speculating along the way, but not much." (*Boardroom Reports*, May 1, 1983, p. 4.) He added in 1986: "I am an active investor in the sense that I have investments, but I am not an active speculator. I don't engage in short-term speculation… I have no complaints… I have made investments in a variety of things. Some of them have turned out very well, and some of them have not." (*American Banker*, April 30, 1986, p. 20.)

In addition to various adjustments to their financial portfolio, the late 1970s and early 1980s saw the Friedmans involved in property transactions associated with their physical relocations. Their main new residence was a condominium in the Royal Towers building in the Russian Hill area of San Francisco, which they purchased in late 1977 for about $250,000 or $265,000 (*Honolulu*

[98] See Samuelson's remarks in Mundell and Zak (2002, p. 37).

Advertiser, September 26, 1977; *Chicago Daily News*, October 3, 1977; *San Francisco Chronicle*, May 11, 1980).[99] When Charles Brunie, the head of Oppenheimer and Company, visited San Francisco, the Friedmans showed him their new home, with Milton Friedman telling Brunie that the investment income that the couple had accrued from their Oppenheimer and Company-managed portfolio had paid for the luxury apartment's purchase (Rudolf Hauser, personal communication, March 29, 2021).

The Friedmans also changed their summer residence. Rose Friedman described summer 1980 as the Friedmans' last time at the Capitaf rural home.[100] This was accurate: indeed, Milton and Rose Friedman signed their preface to the U.K. paperback edition of *Free To Choose* on June 16, 1980, from that location in Ely, Vermont.[101] But the Friedmans' last stay in Ely occurred against a background of their having already, by early 1980, bought a replacement summer home in Sea Ranch in northern California (*Fortune*, February 25, 1980, p. 110). Capitaf was already for sale in the spring of 1980 (*Money* magazine, May 1980), and the Friedmans' stay that year in their longtime summer home was both considerably abridged—lasting from mid-May to late June, whereas in 1979 it had spanned, with breaks, spring through early October—and broken up by many absences, with Milton Friedman making a flurry of speaking appearances in East Coast and Midwest cities during May-June 1980.[102] By July, Capitaf had been sold, leaving the Friedmans based in California on a full-time basis (*Chicago Tribune*, July 20, 1980, p. 21).

II. ISSUES RELATED TO FISCAL POLICY, GEOPOLITICAL DEVELOPMENTS, AND AGGREGATE SUPPLY, 1979–1981

THE REAGAN REVOLUTION

Meanwhile, over this same period, a former governor of California was far advanced in his new

[99] Strictly speaking, this was a cooperative apartment. See *San Francisco Chronicle*, June 4, 1979, and *Los Angeles Times*, November 2, 1980, Part IX, p. 1.
[100] Friedman and Friedman (1998, p. 560).
[101] Friedman and Friedman (1980c, p. 17).
[102] His engagements included being a joint dinner speaker with William F. Buckley, at a Heritage Foundation event in their honor in Washington, D.C., in the third week of May 1980 (*The Advocate* (Stamford, Connecticut), May 24, 1980), attendance of the Amway Corporation convention in Miami on May 29–30, participation in the June 3 sessions of the International Monetary Conference in New Orleans (see the next chapter), an address in St. Louis at the June 2–6 annual international convention of the General Federation of Women's Clubs (*Naples Daily News* (Florida), May 6, 1980), and a briefing on the economy to Oppenheimer and Company clients in New York City on June 18 (*Newsday* (Long Island, New York), June 19, 1980). The Friedmans' 1979 stay at Ely, although interrupted by a long spell in Chicago in September, had not concluded until about Saturday, October 6, when they began a cross-country drive back to California (*Richmond Times-Dispatch* (Virginia), October 12, 1979).

campaign for president.

In late 1973, appearing alongside Ronald Reagan at a press conference in Los Angeles, Friedman had remarked: “I’d be delighted to see Governor Reagan become president.” He judged, however, that this was not likely to happen. (*Independent/Press-Telegram* (Long Beach, California), November 1, 1973.) On that occasion, Friedman did not explain the basis for his judgment. But his reasoning could be gleaned from remarks that he had made during appearances alongside Paul Samuelson in the preceding days. In the course of these events, Friedman opined that, although the Democratic party of George McGovern had lost the 1972 presidential election, the Watergate scandal had “nullified” that election result put the Democrats of the McGovern stripe in the driver’s seat for the foreseeable future, and public spending would likely be a higher share of income in 1983 than in 1973. (*The Plain Dealer* (Cleveland, Ohio), October 27, 1973.) He perceived a national mentality in which the public sector was the “source from whence all goodies flow” and saw a routine acceptance of the practice of “spending someone else’s money according to the [directions of the] new reformers.” Friedman suggested that “we are in a period of decadence and our only hope arises out of the inefficiency of the government,” such inefficiency reducing the scope of the government to preempt private-sector decisions about the use of resources (*Granville Sentinel* (Ohio), October 31, 1973).

As has been indicated in previous chapters, however, Friedman would detect in 1975 and 1976 a renewed public feeling in favor of the government playing a reduced role in the economy. Although Ronald Reagan failed to secure the 1976 Republican presidential nomination, Friedman for a time entertained hopes that Jimmy Carter would end up following some of the policies that Reagan espoused. By April 1978, however, Friedman indicated that he had been largely disabused of this expectation. Reviewing President Carter’s record in the area of fiscal policy, Friedman observed: “He talked a great deal about economy in government—but has substantially increased government spending. He talked about keeping down the deficit, yet he has proposed a budget for the coming fiscal year that is higher than the last.” (*Kansas City Star* (Missouri), April 27, 1978.)

Friedman’s passing from the size of “the deficit” to that of the budget was deliberate. He did not approve of moves toward fiscal balance, such as those Carter would achieve in the second half of his term in office, that took place in the context of a rising overall government spending share of the economy. In mid-1980, Friedman mocked Carter’s claim to have presented a fiscally austere new fiscal program. A proposal Carter had outlined to have the budget balanced within a few years struck Friedman as an act of effrontery, as so much of the balancing process rested on the continuation of high inflation and the consequent increase in the ratio of tax revenues to U.S. output (*Newsweek*, June 23, 1980).

Friedman had remarked publicly in February 1980: "Yes, I'd endorse Reagan. I would like to see him elected. He's very receptive to ideas, [and] he would be the president most likely to implement by philosophy." (*The Observer* (London), February 17, 1980, p. 35.) He had noted a little earlier (*Valley Independent* (Mon Valley, Pennsylvania), February 2, 1980): "He's not the only one I would be happy with, but he's my favorite."

These statements—in which endorsing Reagan was treated as a hypothetical possibility—reflected the fact that Reagan had yet to win the Republican party's presidential nomination. There remained the possibility that George H.W. Bush would defeat Reagan in the overall primary contests, and Friedman's February 1980 statements were allowing for a scenario in which it would be Bush whom he would be supporting for president as the Republican candidate facing Carter in the fall. The February 1980 Friedman statements consequently understated the degree to which he had been a supporter of Reagan's presidential campaign in its informal period through fall 1979 as well as since it formally began in November 1979. Indeed, Friedman had already met Reagan in Los Angeles on January 21, 1980, to discuss economic policy (see Mallaby, 2016, p. 236).

After developing an edge over Bush in the late winter, Reagan prevailed in the Republican primary contests during the spring of 1980. Friedman welcomed not only this development but also the fact that, on the Democratic side, Carter eventually defeated challenger Teddy Kennedy, whom Friedman associated with "a continuation of New Deal policies."[103] Friedman was formally a member of Reagan's economic advisory group (see M. Anderson, 1990, p. 170), wrote a memorandum at the end of April on how a Reagan administration might approach monetary policy—with the memorandum addressed to George Shultz, who was coordinating economic-policy matters for the campaign (see Chapter 15 below)—and joined Shultz and others in a meeting that Reagan held in the Bay Area for an economic-policy discussion on May 9, after Reagan gave a speech in San Francisco (*San Francisco Chronicle*, May 10, 1980).

Yet, although Friedman was described by Shultz shortly before Election Day as Reagan's favorite economist (*New York Times*, October 31, 1980), his personal participation in the 1980

[103] Friedman (1990d, p. 48). Kennedy had advocated reintroducing compulsory across-the-board price controls, which would include a period of a wage/price freeze.

Reagan campaign was not, in fact, very great. The reason for this was straightforward: Friedman remained immersed in the promotion of *Free To Choose*. He was away, in China and East Asia, for much of the key campaign period. His Asian tour lasted a full six weeks (including three weeks in China). It occupied a good deal of September and continued well into October.

On Friedman's return to the United States, he made some public interventions in the days leading up to the November 4 election. In a speech in San Francisco on October 31, he remarked that Carter had "presided over a major expansion in the role of government," including an increase in the government spending share of income.[104] This contention would be borne out by the fact that in fiscal years 1980 and 1981, federal government outlays were 21.7 and 22.2 percent of real GDP—higher than any peacetime year up to that time.[105] Friedman's favorable comments regarding Reagan in the same speech paralleled those he provided in a near-contemporaneous *Newsweek* column (*Newsweek*, November 10, 1980). In the column, Friedman acknowledged that he had been absent from the country for a protracted period but confirmed that he had no doubt about the preferable election outcome. His column essentially endorsed Reagan, praising his "long-held philosophical commitment" to limited government, and suggesting that a Reagan victory "could open the road to a renaissance of freedom and prosperity."

The Chrysler rescue

This *Newsweek* column, although highly supportive of Reagan, expressed regret that recently the Republican candidate had expressed what Friedman considered "mistaken support" for a government guarantee, advanced by the Carter Administration with Congressional authorization, on a bank loan to the troubled Chrysler car corporation (*Newsweek*, November 10, 1980).

Friedman opposed the pro-guarantee stand that both Carter and Reagan took and that each leader ended up presiding over. "Chrysler ought to be allowed to go broke," Friedman had said to audience applause in a 1980 appearance on *Donahue* (NBC, April 16, 1980). In the previous year, as the prospect of government support for Chrysler came up, a Friedman *Newsweek* column had considered two scenarios resulting from bankruptcy being allowed to proceed. One was that in which the company continued in operation and the bankruptcy facilitated a restructuring of the company that restored its commercial viability. The other was one in which the company did fold. He regarded each of these scenarios as preferable to a government rescue or guarantee.

[104] Friedman (1980c, p. 244).
[105] See Council of Economic Advisers (2011, p. 284, Table B–79).

Friedman viewed a government loan guarantee as inconsistent with the notion of a profit-and-loss system, and as thereby moving away from the process of market-directed changes in the economy's pattern of enterprises—a process that he believed had helped underlie growth in the economy over the previous two hundred years (*Newsweek*, September 10, 1979).[106]

In the recession conditions of 1980, Friedman indicated that, of the two no-intervention scenarios he had sketched in his column's analysis, the one in which Chrysler closed down altogether was the more likely: "Letting Chrysler die a graceful death would have meant a stronger economy and a better and more productive automobile industry. Sure, a few people would have been hurt. But, as a whole, it would have been much better." (*Dallas Morning News*, March 26, 1980.)[107] Friedman had, however, indicated that a Chrysler closure would, in any event, likely still lead to its productive capital being combined with labor to produce output: "If its facilities are worth anything to produce automobiles, it will pay other automobile companies to buy and operate them, or new companies will be set up to acquire them." Alternatively, the productive capital might be redeployed in other areas of production. (*Newsweek*, September 10, 1979.) The upshot was that "the bankruptcy of a firm does not mean that machines are destroyed or factories fall to the ground."[108]

Friedman argued, in particular, that saving jobs was not a valid justification for government efforts to rescue Chrysler. Overall U.S. employment depended on the prevailing aggregate conditions, not the conditions facing an individual firm or industry—a reality that led Friedman to conclude with respect to the Chrysler guarantee: "In total, of course, no jobs are being preserved."[109]

As it happened, Friedman was optimistic about the longer-term outlook for U.S. car production: "We're singing requiems for the auto industry much too soon," he observed during a visit to Detroit in January 1981. "It's very far from being a dead industry." (*The Detroit News*, January

[106] Friedman had previously criticized loan guarantees proposed for Penn Central railroad and Lockheed company in the early 1970s. "Let them go bankrupt." (*Chicago Daily News*, May 21, 1971.)

[107] Speaking at the same March 1980 event, Paul Samuelson indicated that he, too, disagreed with the government support of Chrysler: "it was wrong." He added: "It is only a matter of time before Chrysler goes defunct, anyway." (*Dallas Morning News*, March 26, 1980.) In a further dialogue with Samuelson about five months later, Friedman, too, seemed gloomy about the company's long-term prospects: "the recent, ill-advised bailout of Chrysler" was an example showing how public-sector "rescue attempts are both costly and ultimately ineffective." On this occasion, Samuelson reaffirmed his own opposition to the loan guarantee: "[U.S.] autos have lost comparative advantage. The burden of proof should against governments' keeping uneconomic business activities alive." (*Newsweek*, September 8, 1980, p. 69.)

[108] Friedman (1981b, p. 13).

[109] Friedman (1981b, p. 13).

26, 1981.)[110] But he reiterated a few months later that the government should stay out of specific support for the industry. "It is very hard for me to see how you are going to justify imposing costs on workers who are receiving far less than Chrysler workers, in order to save the jobs of Chrysler workers." (*Evening Post* (Wellington, New Zealand), April 27, 1981.)

The Chrysler loan guarantee put U.S. taxpayer funds at risk, and as of October 1980, Friedman viewed it as likely that an actual government outlay would be needed to pay off the $1.2 billion loan. In the event, Chrysler paid its loan back early, and in the mid-1980s the Friedmans acknowledged that advocates of government intervention in U.S. industry "cite the government bailout of Chrysler Corporation as favorable exhibit Number 1."[111] They indicated nevertheless that the criticisms of the guarantee that Friedman had previously articulated stood, while noting also that Chrysler had received special treatment that was not given to the numerous U.S. corporations that, since 1980, had closed down or entered bankruptcy.[112]

"If Chrysler had gone bankrupt, it wouldn't have stopped operating," Friedman reiterated after the event. "In almost every case, when a company like that goes bankrupt, it continues to operate and produce goods. The company goes into receivership [and still] operates." Lamenting the fact that the Chrysler rescue had limited the losses that stockholders in the firm had racked up, Friedman added: "The worst thing about the Chrysler bailout is the number of future similar bailouts [that] it will lead to." (*California* magazine, October 1984, p. 76.)

The Reagan victory and transition

In the course of a talk given in Singapore in October 1980, Friedman had predicted a Republican victory in the presidential election: "I believe that Mr. Reagan will be elected."[113] In the same talk, he noted that both presidential candidates had stated during the campaign that the government's role in the economy should be reduced, but he implied that, of the two, it was only Reagan who was saying it "because he has long believed it."[114] Picking up a theme from the end

[110] On another occasion, Friedman contrasted favorably General Motors' adaptation to changing conditions with that of the other major U.S. car companies (*Los Angeles Times*, November 2, 1980, Part IX, pp. 1, 4). This attitude was of long standing on his part. In the wake of the 1973 OPEC price announcements and moves, Friedman had predicted that General Motors would adapt its models well to the 1973 oil price increase and indicated he had bought stock in the company on the basis of this expectation (Instructional Dynamics Economics Cassette Tape 135, December 4, 1973)

[111] Friedman and Friedman (1984, p. 119; 1985, p. 116).

[112] For other discussions of Friedman's analysis of the Chrysler guarantee, see Barro (1985a) and Rockoff (2021).

[113] Friedman (1981b, p. 6).

[114] Friedman (1981b, p. 5). In fact, with respect to government spending, Friedman's impression of Carter's position may have been based primarily on events in the first half of 1980, when Carter had been emphasizing fiscal

of *Free To Choose*, he saw the overlap in the message provided by the opposing candidates as reflecting a change in the tide of opinion. “In my opinion,” Friedman added, “this change in trend of opinion and of policy is a major factor that will dominate the developments in the world in the 1980s.”[115]

Reagan won a landslide victory on November 4, and the result also saw the Republican party win control of the U.S. Senate. Friedman expressed his delight. “I believe the fundamental trend is established by this election—that, in the United States, the direction is going towards reduced size of government.” The changes in the houses of Congress also reinforced his view that the election outcome signified “a change in public opinion” and not just “a tribute to Mr. Reagan or a repudiation of Mr. Carter.” (*Sunday Telegraph* (London), November 9, 1980, p. 40.)

Friedman and several other economic advisers met the president-elect in Los Angeles on November 16, and the advisers signed a memorandum that day titled “Economic Strategy for the Reagan Administration,” which contained a list of recommendations.[116] This was to a large extent an elaboration and recapitulation of Reagan’s previously stated economic goals and policies, as expounded in a speech the candidate had given to the International Business Council in Chicago on September 9 (see Reagan, 1980). Friedman himself publicly pointed to this speech as the guide to the next president’s economic policy (United Press International, November 21, 1980).

Following his participation in this presidential transition committee, Friedman did not take any truly permanent position in the Reagan Administration. Speaking in Dallas a couple of weeks after the election, Friedman confirmed he had no interest in joining the administration (United Press International, November 21, 1980). As discussed in the discussion titled “Arthur Laffer”

restriction as an anti-inflation measure. In the campaign proper (during which Friedman was, as noted, largely away from the country), President Carter took a more traditional stance. This pattern continued up to the election, with a reporter noting in late October: “One of the most interesting things about the last days of this campaign is the way in which Carter has found it expedient to display the liberal items in his vast catalogue of contradictions. He is no longer, as he was earlier this year, the austere budget cutter. He is the compassionate president, the protector of the great advances in the welfare tasks of government…” (*The Bulletin* (Australia), November 4, 1980, p. 94.)

[115] Friedman (1981b, p. 7).

[116] See *Wall Street Journal*, May 26, 2012, and Taylor (2012, pp. 77–78) for extensive excerpts. The full memorandum is reproduced in Shultz and Taylor (2020, Appendix C, pp. 81–93), with the memo title given in the text. (In Friedman and Friedman, 1998, p. 398, its title was instead given as “Economic Strategy for a Reagan Administration,” possibly reflecting a confusion with other Friedman memos in 1980–1981 that used the indefinite article.) The authors of the memorandum, who jointly formed Reagan’s Coordinating Committee on Economic Policy (Friedman and Friedman, 1998, p. 390; Shultz and Taylor, 2020, p. 93), had been in Los Angeles in the days up to their meeting with Reagan. Friedman had flown into the city over a week earlier (*Sunday Telegraph* (London), November 9, 1980, p. 1).

in Section III below, Friedman did serve from 1981 onward as a member of the President's Economic Policy Advisory Board. But that team met very intermittently, was expressly composed of economists or agency heads who were not currently permanent affiliates of the regular economic-policy agencies of the federal government (such as the Federal Reserve Board, the U.S. Treasury, and the Council of Economic Advisers), and did not play a central role in the administration's decision-making. In the Reagan era, Friedman was therefore, essentially, only an *ad hoc* adviser, much as he had been in President Nixon's early years.

And that was the way Friedman liked it. As discussed in Nelson (2020a, Chapter 3), Friedman subscribed to the maxim that he could make a greater difference to policy discussions by serving as an outside commentator rather than a policy official. Stanley Fischer, for one, saw merit in Friedman having this status. In the late 1970s, Fischer observed that "we are probably all better off for having Milton Friedman and Bob Lucas remain free to state their views without having to engage in any polite compromises."[117]

In his remarks in Dallas, Friedman remarked: "Don't kid people. There will be difficult times. Reagan will have to deal with the heritage of the bad economic policies of the Carter Administration. He can't turn the situation around on a dime." He nevertheless affirmed that he had "every confidence" that the Reagan years would ultimately see inflation defeated and stronger economic growth (*San Antonio Light* (Texas), November 22, 1980). As far as basic economic policy was concerned, he observed near the end of 1980: "I'm quite hopeful there will be a change." (*U.S. News and World Report*, December 15, 1980, p. 52.)[118]

On Inauguration Day, January 20, 1981, Friedman was among those providing commentary for National Public Radio's coverage of the events.[119] He had a further opportunity to discuss the new administration on a national program a couple of days later, when he appeared on ABC's morning television show *Good Morning America* (*New York Times*, January 20 and 22, 1981).[120]

[117] Fischer (1979, p. 174).

[118] Reprinted in Friedman (1983a, p. 197).

[119] It is tempting to see Reagan as having drawn inspiration from Friedman's *Newsweek* column of November 6, 1972, which stated, "Government is the problem, not the solution," when using a similar phrase in his 1981 inauguration speech. This had indeed been a longstanding Friedman formulation. It had also been, however, something of a boilerplate phrase in public discourse for years by 1981. For example, Vice President Nelson Rockefeller remarked in late 1975: "I think it is fair to say [that] there is a growing feeling… rather than looking to Washington for the solution of the problem, that the federal government is the problem." (Quoted in *The Chronicle-Telegram* (Elyria, Ohio), January 8, 1976.) Reagan himself had said in a 1977 interview: "Again, it's turning to the federal government for an answer when the federal government is the problem." (*Greenfield Recorder* (Massachusetts), July 13, 1977.)

[120] On the first of these programs, the other economist providing commentary was Walt Rostow, whose positions *vis a vis* those of Friedman are discussed in Chapter 15 below.

Full energy-price decontrol

One area in which President Reagan rapidly met Friedman's expectation was that of energy prices. Writing in the middle of the year, Friedman praised Reagan's "prompt oil decontrol" (*Newsweek*, June 29, 1981). By that point, oil and gasoline prices in the United States had been market-determined since Reagan removed all price controls through an executive order that came into effect on March 1. The president's move had ended the succession of price controls in the area of petroleum output that had been in force since August 1971.

The Reagan decontrol action was the first achievement of the administration referred to in the opening chapter, "The Reagan Record," of the *Tyranny of the Status Quo* book that the Friedmans wrote toward the end of the president's first term.[121] In counting this as a Reagan measure, the Friedmans' account fell into the category of what one writer claimed amounted to a propagation of "the myth that President Reagan decontrolled the price of oil in the United States." (*The Economist* (London), January 12, 1985.) The credit for this action, according to these alternative accounts, should be given to President Carter, not Reagan.

It is true, as discussed in the previous chapter, that the Carter Administration embraced an oil-price decontrol policy in 1979. It started implementing this policy in earnest on the basis of preannounced steps starting in January 1980.[122] Near election time, Council of Economic Advisers chair Charles Schultze was making a virtue of the market-oriented nature of the change, observing: "we're in the process of ending the idiocy of keeping domestic natural gas and oil prices below world market [values] and [thereby] subsidizing wasteful consumption."[123]

For his part, Friedman remained suspicious of the authenticity of Jimmy Carter's overall deregulatory inclinations. Evidence against such inclinations was provided by what Friedman called the "host of regulations which poured out in the last days of the Carter Administration" in a number of areas (*Human Events*, December 5, 1981, p. 1). Friedman gave Reagan credit for rescinding many of these new regulations during his first year in office.[124] Friedman was also critical of President Carter's post-election remarks regarding his defeat, as he felt that Carter had not absorbed the small-government message of the election result. According to Friedman's characterization, the defeated president regarded himself as having been unluckily caught in an

[121] Friedman and Friedman (1984, p. 2; 1985, p. 9).
[122] Less important aspects of price decontrol had been started in 1979.
[123] *Wall Street Week,* Maryland Public Television, October 17, 1980, p. 9 of transcript.
[124] See *Human Events,* December 5, 1981 (p. 1), as well as Friedman and Friedman (1984, p. 2; 1985, p. 9).

adverse political tide while following the right policies—when instead he should be questioning whether those policies had been right (*Sunday Telegraph* (London), November 9, 1980, p. 1). Friedman's own verdict on the Carter presidency would be encapsulated in the title given to a book made up largely of his *Newsweek* columns during the Carter years: *Bright Promises, Dismal Performance*.

With respect to energy price decontrol, the case for attributing this move almost wholly to Carter lies in the fact that his phased-decontrol plan, announced in 1979, would have seen the price of oil produced in the United States fully deregulated on October 1, 1981. Reagan's decontrol action made this occur instead seven months earlier—by replacing the remaining phases of price liberalization with instant deregulation. Therefore, oil prices were already set to be free by the early fall of 1981, and Reagan simply freed them earlier, at the close of the preceding winter.

It remains the case, however, that Reagan made a material contribution in expediting petroleum price deregulation. It is not literally true that, as claimed by Carter's domestic adviser Stuart Eizenstat (2018, p. 215), "Ronald Reagan simply completed the last step" of oil price deregulation. Rather, Reagan's early-1981 action preempted the six further monthly steps toward full deregulation envisioned under the Carter decontrol timetable. As Eizenstat in fact acknowledged at the time, compared with Carter, Reagan "took the additional step of accelerating the pace of full decontrol by several months and… decontrolling [retail] gasoline prices."[125]

It is also worth underlining that the Carter Administration's reluctance to bring oil prices to world levels in a prompt, as distinct from a gradual, manner was rationalized in part by a nonmonetary view of inflation to which administration economists adhered but that Friedman and, in turn, the Reagan Administration rejected. The largely nonmonetary perspective on the energy/inflation connection was still evident in the early 1980s in Paul Samuelson's commentaries. Anticipating that Reagan would swiftly decontrol energy prices, Samuelson had stated at the end of the Carter years that such decontrol would "certainly add" to U.S. inflation from 1981 to 1983 (*Newsweek*, January 5, 1981). To Friedman, such a prediction was problematic. He was very skeptical about whether an energy-price increase could add to inflation over a long period. As indicated in Chapter 3 above, Friedman granted that an oil-price

[125] From Eizenstat's testimony of July 15, 1981, in Committee on Science and Technology, U.S. House of Representatives (1981, p. 5). As Friedman and Friedman (1984, p. 2; 1985, p. 9) noted, Reagan's 1981 proclamation also removed the controls on quantities—that is, the regulations pertaining to oil allocation.

spike lowered the price level because it reduced potential output.[126] But he objected to the routine association made between energy-price increases and ongoing inflation behavior, believing that this reflected faulty cost-push ideas. By 1981 the economics profession—and, under Reagan and his subordinates as well as Paul Volcker, policymakers—had largely come round to Friedman's view of inflation.

Notwithstanding, however, his own continued dissent from Friedman on the energy/inflation connection, Paul Samuelson's position on decontrol evidenced a conversion to a position that Friedman and Reagan had long held. In mid-1973—before the first OPEC shock, but when energy-price pressures had already begun to emerge in the face of controlled domestic prices—Samuelson had the opposed "old-fashioned solution" of letting the energy price free. On that occasion, he had suggested that if the retail gasoline price was to be allowed to rise, the shift up should be achieved by, or the associated receipts absorbed by, tax increases (*Newsweek*, July 2, 1973). Subsequently, as has been indicated in previous chapters, he advocated gasoline rationing during the OPEC embargo period and, by the mid-1970s, he seemed satisfied with the operation of the two-tier oil price-control system. Samuelson was then a supporter of the 1977–1978 Carter proposal to harmonize the controlled prices with world prices using taxes alone. By mid-1979, however, he was referring to "the atrociousness of the way we are balancing out demand and supply" and seemed to have become more amenable than previously to a market-based solution (*Newsweek*, July 2, 1979).

Not all of the developments in the U.S. government's oil policy went Friedman's way. In particular, contrary to Friedman's wishes, the oil-price decontrol of 1980–1981 was accompanied by a tax on U.S. oil producers. Specifically, the windfall profit tax on oil, already advanced stage in Congress at the end of 1979, became law in 1980 and remained in place over most of the Reagan years. But, in contrast to the position shared by Samuelson and the Carter Administration until 1979 that old-oil producers should be denied altogether the proceeds associated with post-1972 world oil price increases, the new tax—being set at a 50 percent rate—allowed domestic producers of old and new oil alike to receive a sizable percentage of the profits associated with the first and second oil shocks.

In Shapiro and Watson's (1988, p. 124) chronology, U.S. petroleum-price decontrol in 1981 would be the third major oil price event of the postwar period (the first two being the 1970s

[126] That remained his view, evidenced in his observation that the "sharp jump in the price of oil in 1973–74 would have significantly reduced our productive capacity under the best of circumstances" though the U.S. energy policy response shaped, and in Friedman's view magnified, the decline (*Newsweek*, September 8, 1980, p. 68).

OPEC shocks). They found that the 1981 event was associated with a sizable decrease in oil prices. Indeed, the oil price reached its peak for the 1980s in 1981. Of course, the downturn in global oil prices likely reflected other factors as well as domestic price decontrol, including the world recession of the early 1980s. But Friedman's position that oil-price decontrol need not connote a surge, still less a lasting rise, in U.S. petroleum prices was borne out. His analyses of commodity-price developments through the mid-1980s are considered in Chapter 14 below.

Productivity and the role of government

Deregulation, of which oil-price decontrol was an example, was one of the four classes of policy measures that Friedman associated with the administration's economic policy, the other three being "lower marginal tax rates, lower government spending, … and a restrained and stable monetary policy." He viewed all of these measures as a means of enabling "the economy to become more productive." (*Human Events,* December 5, 1981, p. 1.)

Friedman's bleak assessment before the election was: "The United States at the moment is a sick country." (*The World This Weekend*, BBC Radio 4, October 19, 1980, p. 3 of transcript.) This was a remark partly made in reference to the ongoing occurrence of double-digit inflation. But it also reflected the turn that U.S. productivity had taken. As indicated in the previous chapter, by early 1979 the fact that the last five years had seen a pronounced productivity slowdown in the United States was becoming incorporated into discussion and formulation of stabilization policy. For this area of economic policy, the slowdown was a reality that had to be accepted and conditioned upon, leading to reduced estimates of the trend in potential output. This in turn could be traced. as Friedman put it in March 1980, to "declining growth in total productivity."[127] With regard to the development to which Friedman was referring, Norsworthy, Kunze, and Harper (1979, Table 1, p. 390) gave U.S. labor productivity growth as having averaged 3.3 percent in 1965–1973, 2 percent in 1965–1973, and 1 percent in 1973–1978. The United States' overall living standards were high, but the outlook for further improvement had dimmed—a combination reflected in Friedman's observation, "We are a wealthy, strong economy, but the tragedy is we are wasting it." (*Fort Worth Star-Telegram* (Texas), March 27, 1980, p. 4B.)

Of the four elements of Reagan's economic program that Friedman would cite, one—that regarding monetary policy—will be discussed in the next chapter. Administration intentions regarding monetary policy were a breed apart from the three areas of deregulation, tax policy,

[127] Friedman and Samuelson (1980, p. 34).

and government spending—not only because the Federal Reserve (rather than the U.S. president) decided monetary policy, but also on account of the importance of many nonmonetary factors in the determination of productivity and of longer-term real growth. In particular, the result Friedman saw as flowing from execution of monetary restraint—the elimination of inflation—did not, he believe, provide the answer to restoring U.S. productivity growth. This was not to say that he denied that that high and variable inflation lowered the level and the growth rate of productivity. On the contrary, he catalogued several reasons for believing that it did. For example, Friedman contended that inflation distorted households' decisions about the allocation of their savings, with an increased share assigned to activities that did not increase productivity (*Human Events*, December 5, 1981, p. 1). As for U.S. firms' behavior, he especially stressed the noise that inflation put into relative price signals and the obstacle that an erratic monetary environment long-range investment planning associated with an erratic monetary environment.[128] And in combination with an unindexed tax system, inflation had further scope to distort the allocation of resources, in part by leading to growth in the share of tax revenues to national income and so, he believed, also to growth in government spending as a share of income.

The last of these effects underlined, however, why Friedman did not believe inflation *per se* was central to the U.S. productivity slowdown. "I believe it is a mistake" to attribute the productivity slowdown largely to inflation, he observed in March 1980.[129] Ending inflation was an "essential precondition" to the restoration of economic health, he had stressed, but other measures had to be taken in conjunction with it in order to help restore "an environment favorable to innovation, investment, enterprise, and thrift." (*San Francisco Chronicle*, January 2, 1980, p. 23.)

Friedman's assessment was that the expansion of the public sector would have induced a productivity slowdown even if monetary policy had been restrained enough to avoid the inflation of the 1970s (*Newsweek*, August 23, 1982).[130] Thus, his conclusion was that inflation and slow growth, although they had occurred alongside each other in the United States and other countries in the 1970s, were largely separable problems.[131] To this end, in 1980 Friedman distinguished between "monetary policy proper" and "policy to raise output," with the shift in productivity behavior prompting discussion about the appropriate formulation of the latter.[132]

[128] On Friedman's discussion of distortions to relative prices, see Nelson (2020a, Chapter 7). On inflation as an impediment to investment, see, for example, *Newsweek*, March 5, 1979.
[129] Friedman and Samuelson (1980 p. 34).
[130] See also Chapter 6 above.
[131] See, for example, Friedman (1980b, paragraph 1, p. 55 [p. 49 of 1991 reprint]) and the discussion in *U.S. News and World Report*, December 15, 1980 (p. 51), as reprinted in Friedman (1983a, p. 194).
[132] Friedman (1980b, paragraph 9, p. 57 [p. 52 of 1991 reprint]).

Friedman summed up matters in April 1981. "Ending inflation, in my opinion, is a very desirable thing to do. In my opinion, it is likely to be a necessary precondition for resolving the other problems that countries have. But it is not a be-all and end-all of economic policy." (*Evening Post* (Wellington, New Zealand), April 27, 1981.)

Just as the realization of the country's post-1973 productivity slowdown was becoming widespread, the United States' productivity problems seemed to be acquiring an extra degree of severity. In the book version of *Free To Choose,* written during 1979, Milton and Rose Friedman would observe that U.S. productivity growth had more than halved from the 1960s to the 1970s, and that "by the end of the decade productivity was actually declining."[133] This pattern was confirmed by an absolute decline in productivity being recorded for calendar-year 1979. The end-of-decade fall was widely highlighted as a new wrinkle on the productivity problem by several other prominent figures, including John Kendrick (1980, p. 1), at a conference on the productivity slowdown held by the Federal Reserve Bank of Boston in 1980; by Arthur Burns in March 1980 Congressional testimony.[134] It was also referred to by President Carter himself (doing so, as it happened, in the March 1980 address that introduced credit controls—a measure that Friedman accurately predicted would worsen U.S. productivity).[135] Data revisions have made this deterioration less drastic than it appeared at the time. But the revised data confirm the fact of an absolute productivity decline in 1979—with a further decline in 1980.[136]

Although he conceded that actual declines in productivity would likely not continue, Friedman

[133] Friedman and Friedman (1980a, p. 191; see also p. 6).

[134] Friedman would refer to the previous year's decline in productivity in an October 1980 appearance (Friedman, 1980c, p. 247) and, after the 1980 election, in the aforementioned memorandum that he and other members of the Coordinating Committee on Economic Policy wrote to President-elect Reagan on November 16 (see Shultz and Taylor, 2020, p. 81).

[135] The reference in Burns' testimony (given on March 27, 1980) appeared in Joint Economic Committee, U.S. Congress (1980, p. 147). See Carter (1980a) for the speech announcing credit controls and the next chapter for a discussion of this program.

[136] In the 1981 *Economic Report of the President* (Council of Economic Advisers, 1981, Table B–38, p. 276), output per hour in the nonfarm business sector (which likely corresponded closely to the productivity concept used in Friedman and Friedman, 1980a, p. 191—the measure used there being described as "output per manhour of all persons employed in private business") peaked for the decade in 1977 at 119.1, declining to 118.9 in 1978 (a 0.2 percent decline) and 117.9 in 1979 (a further 0.8 percent decline). By the time of the 2011 *Economic Report of the President* (Council of Economic Advisers, 2011, Table B–49, p. 248), the decline in 1978 had been revised away, but productivity declines of 0.3 percent were registered for both 1979 and 1980. The revisions to the 1970s data are less substantial when the whole 1973–1979 period is considered, with productivity growth little changed overall. This is also true for output, which the 2011 *Economic Report* indicated in 1979 stood 19.1 percent above its 1973 level (using real GDP), compared with a corresponding figure of 18.2 percent in the 1981 report (using real GNP). See Council of Economic Advisers (1981, Table B–10, p. 245; 2011, Table B–2, p. 190).

highlighted “the change in the *trend*” as something that was of enduring concern.[137] He accompanied his concern about slower overall economic growth with the point that, with the growth in the public sector during the 1970s, private-sector income had borne the brunt of the slowdown. Friedman assessed that American households were worse off than six or seven years previously (*Donahue*, NBC, September 6, 1979), a claim he buttressed by stating that the ordinary family’s “command over goods and services” had diminished in recent years (*The Register* (Santa Ana, Orange County California), December 23, 1979, p. E10). Friedman’s Stanford University colleague, and frequent interlocutor, Michael Boskin, also stressed this point, stating that “real private economic growth has been nonexistent since 1973… real economic growth has slowed substantially and virtually of all it has gone into government spending.”[138]

More government spending on output meant less private spending for a given amount of output. Thus, Friedman, like Boskin, characterized the expansion of the government spending share as a drain on the U.S. economy.[139] But he did not see the impact of the expansion of government on private-sector resources as limited to this arithmetic influence, as he did not believe that output and its growth rate could be taken as given when government expanded. For Friedman, the sense in which the growth of the public sector had come at the expense of private sector income growth went beyond altering the shares of the national pie and concerned the size of the pie itself. “I would argue that what government has been doing over the last few years has, in effect, played a fairly significant role in the decline of our productivity performance,” Friedman had observed in mid-1979 (*Chicago Tribune*, July 15, 1979). Though Friedman viewed the consequences, in terms of slowed productivity, as having only manifested themselves in recent years, the causes of the slowdown went, in his diagnosis, back further in time, to the postwar increase in the role of government. Very soon after the 1979 *Economic Report of the President* made the growth slowdown official by marking down the post-1973 trend in potential real GNP, Friedman wrote that the shift of the productivity trend to a “snail’s pace” was due to the expansion of the public sector (*Newsweek*, March 5, 1979).

Friedman had been expressing concern for years of the possibility that a higher public sector could impair productivity.[140] He had remarked in *Capitalism and Freedom* that high taxation in the United Kingdom and the increase in welfare-state measures had likely held down postwar

[137] *Newsweek*, September 8, 1980, p. 69. Emphasis in the original.
[138] Boskin (1979, p. 39).
[139] See his testimony of May 17, 1979, in Committee on the Judiciary, U.S. House of Representatives (1980, p. 142).
[140] In addition to the examples given here, see Chapter 6 above as well as Nelson (2020a, Chapter 9).

U.K. economic growth.[141] With regard to the United States, Friedman had stated in a 1970 appearance on *Meet the Press*: “Taxes and spending coming down has a great deal to do with the longer-run health and vigor of the country.”[142] And in a talk five years later, he contended that the growth in government was a “major hindrance” to long-run growth in per capita income (*Dallas Morning News*, October 17, 1975).

At the close of the 1970s, with the slowdown in economic growth now a widely accepted fact, Friedman was outspoken in arguing that the slowdown both could and should be reversed—and that a reversal of the expansion of the public sector was the way of returning to higher productivity growth. “The slowing down of our productivity growth is fundamentally attributable to government policy… There I nothing, in my opinion, to prevent a return to very high rates of productivity growth, if we would eliminate the obstacle that the government has imposed on that development.”[143] As has been indicated already, he restated this position emphatically over 1980 and 1981.

Friedman acknowledged that “the period [from] 1950 to 1970 was a period of very rapid and dynamic growth” and that this period, too, had been accompanied by growth in the public sector.[144] But, with regard to this and earlier eras, his judgment on living standards was that “improvements have come in spite, not because, of governmental intrusion into the market place.”[145] The public sector in Western countries in 1950–1970 had, he granted, generally provided a more stable monetary policy framework than in the interwar period, and this had been conducive to technological innovations largely developed before World War II being felt in postwar economic growth.[146] But, conversely, he believed that the greater public sector role after 1945, including the step-up in government intervention in the decade starting in the mid-1960s, had stifled further innovation and together with other adverse effects of government intervention on potential output, had led, with a lag, to slower productivity growth. Hence, the growth slowdown, Friedman reaffirmed in mid-1980, “reflected primarily… an earlier explosion of government spending and government intervention into the economy.”[147]

[141] Friedman (1962a, p. 194).
[142] *Meet the Press*, NBC, June 28, 1970, p. 6 of transcript.
[143] Remarks by Friedman to *Quality* magazine, as printed in Committee on Ways and Means, U.S. House of Representatives (1980a, p. 100).
[144] From his remarks in Friedman, Porter, Gruen, and Stammer (1981, p. 23).
[145] Friedman (1976b, p. 6 [p. 10 of 1978 reprint]; 1977e, p. 8). For an earlier emphatic articulation by Friedman of this argument, see *Newsweek*, October 28, 1968.
[146] See his remarks in Friedman, Porter, Gruen, and Stammer (1981, pp. 23–24).
[147] Friedman (1980b, paragraph 3, p. 55; p. 50 of 1991 reprint).

In Friedman's assessment, the expansion of the public sector had negatively affected both the level and growth rate of potential output, through a variety of channels. He cited the expansion of government as having adversely affected incentives to work, save, expand the capital stock, and innovate.[148] In various discussions he cited a number of specific channels through which this occurred. Higher marginal tax rates and increased unemployment benefits had reduced the amount of hours worked (see, for example, *Newsweek*, March 5, 1979, as well as Chapters 5 and 8 above). Expansion of regulations had stifled innovation and diverted private-sector resources toward dealing with regulatory requirements.[149] And higher corporate income taxes had discouraged investment, while the coexistence of tax shelters and growing tax rates had shifted investment into less productive activities.[150] Up to around 1981, Friedman was also still listing—though giving it much less prominence than he once did—a crowding-out of private spending by budget deficits as another channel through which firms' investment expenditures had been reduced. But by the start of the 1980s he was already downgrading the importance of this channel, setting the scene for his fuller embrace of the Ricardian equivalence proposition.

It was his tracing of the productivity slowdown to the effects of a more interventionist government sector that underlay Friedman's judgment that the slowdown was a genuine problem. To be sure, he did not adhere to the view that per-capita economic growth was always inherently desirable. He had observed in the mid-1970s that output growth was "not a desirable thing in and of itself; it is only good if people want it."[151] In 1979, Friedman affirmed to Phil Donahue: "I don't want to impose growth on anybody" (*Donahue*, NBC, September 6, 1979) and that it was appropriate for growth to cease if the community decided it was satisfied with the existing overall standard of living.

Friedman did not, however, see the slowdown in productivity in the 1970s (and still less the absolute declines) as having emerged in this benign manner. "Reduction in productivity of this nation and of its living standard simply is not normal, and it is not necessary," he observed (*Auburn Journal* (California), November 9, 1980). As was indicated in the previous chapter, Friedman did not see the post-1973 slowdown as reflecting a conscious private-sector decision at

[148] Friedman (1980b, paragraph 3, p. 55 [p. 50 of 1991 reprint]; 1983d, p. 11), and *U.S. News and World Report*, December 15, 1980, pp. 50, 51 (as reprinted in Friedman, 1983a, pp. 194, 195)

[149] Friedman and Friedman (1980a, p. 191). See also Friedman's application of this argument, in Instructional Dynamics Economics Cassette Tape 97 (May 3, 1972), to the Nixon wage-price controls.

[150] *Newsweek*, March 5, 1979, and September 8, 1980; *U.S. News and World Repor*t, December 15, 1980. As indicated, Friedman's view regarding this channel was paralleled how he envisioned some of the adverse repercussions of inflation.

[151] Vaizey (1975, p. 730). See also *The Listener* (London), May 30, 1974 (p. 690), and his remarks in Friedman and Kristol (1976, p. 31).

the individual level. Rather, the growth in the public sector, although supported by or acquiesced in by the private sector in its capacity as voters, had, in his view, contributed heavily to a slowdown in the growth of living standards that the public and private sector alike had not sought. This slowdown had not been envisioned by voters (or by their representatives) when they supported increases in the public sector's role in the economy. The community had gone along with the expansion of the public sector, but Friedman believed that it would not have done so if it had anticipated the implications for U.S. economic performance of that expansion.

How widely accepted is Friedman's diagnosis of the productivity slowdown? In a discussion that mainly focused on the productivity slowdown that occurred ahead of 1973, Stanley Fischer (1979, p. 170) suggested "the role of government interference in reducing trend productivity," while it had been emphasized by various commentators, had yet to be supported by formal analysis of growth. Paul Samuelson was also highly skeptical toward the notion that growth in regulations or in the size of the public sector importantly contributed to the U.S. economic slowdown, although he left open the possibility that it had done so, or soon would, in other Western economies (Samuelson, 1980, pp. 666–670; *Newsweek*, September 8, 1980, p. 68). As research accumulated, the economics profession was still far from a consensus on the causes of the post-1973 shift in productivity, but the notion that the growth of government contributed significantly acquired support on the basis of numerous studies, leading Robert Barro (*Wall Street Journal*, February 23, 2010) to refer to "the familiar pattern whereby countries with larger public sectors tend to grow slower over the longer term."

If the role of the state in the economy was substantially reduced, Friedman remarked in one of the debates on his television program, "I think the United States would prosper in a way that is hardly imaginable today."[152]

Fiscal policy: tax cuts and spending restriction

Friedman therefore primarily viewed the economic effects of the fiscal policy plans of the new Reagan Administration in the context of what he regarded as the likely improvements to aggregate supply that they were likely to generate. The administration laid out these plans in a 1981 document titled *Program for Economic Recovery* (see Boskin, 1987, pp. 51–52, 280). The document bore that title despite the fact that Reagan took office after the 1980 recession had

[152] *Free To Choose* (U.S. television version,), PBS, Episode 2, "The Power of the Market" (debate portion), January 19, 1980, p. 13 of transcript.

ended. The "recovery" envisioned was not a rebound from that recession but, instead, a lasting return to stronger and noninflationary economic growth.

The most publicized and most remembered aspect of the announced program—which corresponded to the item that was largely implemented—was a version of the Kemp-Roth tax cut proposal, which had been under consideration in Congressional forums since 1977. This tax-cut package (reducing labor income taxes by about 25 percent in total, compared with 30 percent in the original Kemp-Roth bill) received its final passage through Congress on August 4, 1981 (Carruth, 1993, p. 754). It was subsequently signed into law by Reagan, becoming the Economic Recovery and Tax Act of 1981 (Boskin, 1987, p. 57).

The act authorized income tax cuts to be phased in over 1981 to 1983. One aspect of the administration's original tax proposals that underwent change during the legislative process pertained to an aspect that the 1981 tax cut was perhaps most known for: a reduction in the top marginal income tax rate. Strictly speaking, as far as tax on hours of work was concerned, there was no reduction. As enacted, the 1981 tax cut left the top labor income marginal rate at 50 percent. "The '81 tax act did not cut the highest rate," Arthur Laffer (interview, June 10, 2013) noted, "…so the highest marginal earned income tax rate stayed at 50 percent." Indeed, it was the case that with regard to the personal-income aspect of the legislated tax cut, "most of the actual reductions [were] below the 25 percent marginal rate," as Michael Evans (1983, p. 97) noted. The 1981 act did, however, reduce the top income tax rate because it cut the top rate on households' investment income (often controversially labeled "unearned" income) to 70 percent to 50 percent.

Friedman was on record as favoring cutting the 70 percent top "unearned" rate and harmonizing it with the top rate on labor income (for example, *Newsweek*, September 8, 1980, p. 69). When advancing its 1981 plan, however, the administration had been reluctant to be the prime mover in cutting the top income tax rate. Is was, however, in favor of such a change , and its formal proposals did involve reduce the rate to 50 percent, albeit over three years.[153] As Friedman later noted (*California* magazine, October 1984, p. 75), it was actually the House of Representatives

[153] See M.K. Evans (1983, p. 97) and Boskin (1987, p. 58). (Friedman and Friedman [1984, pp. 33–34; 1985, p. 39] characterized the reduction from 70 percent to 50 percent as only emerging in the 1981 legislative negotiations. However, Reagan's original February 18 proposal clearly stated: "By 1984 the top marginal rate of 70 percent will be reduced to 50 percent for all income, regardless of source." See White House, 1981c, p. 4.). In his confirmation hearings, held on being nominated to be Reagan's Secretary of the Treasury, Donald Regan had criticized the 70 percent rate. Correspondingly, during the presidential transition period, Reagan's advisers were contemplating including the scrapping of the excess of the top investment-income rate over the top labor-income rate as part of the tax-cut plan (*Wall Street Journal*, January 8, 1981). As indicated, the official plan did do so, on a phased basis.

Democrats who introduced a counterproposal, ultimately incorporated into the tax-cut law, that did immediately make the top rate on all incomes 50 percent.[154] The Reagan Administration welcomed this addition to the plan.[155] In the tax cut as legislated, the reduction in the maximum rate on investment income became effective immediately (see Boskin, 1987, pp. 59, 204).

Friedman's attitude to the tax cuts was distinct from that articulated by two sets of economists supporting Reagan. On the one hand, supply-side economists viewed tax cuts hierarchical terms—as the most important part of the Reagan package and as warranting immediate implementation. On the other hand, Republican economists associated with previous administrations had, especially before Reagan's election victory, implied that tax cuts should be contingent on spending cuts and occur in the context of balancing the budget. Arthur Burns had been an exponent of this position (see Chapter 8 above).[156] So was George Shultz, who remarked in April 1980: "Yes, it's important to get taxes down. I subscribe to that. But you just can't go totally overboard… One needs to have a strategy for cutting spending." (*The Sun* (Baltimore), May 1, 1980, p. A8). Friedman maintained, similarly to Shultz, that "tax cut fever" should be distinguished from pressure to cut government spending (*Newsweek*, March 10, 1980) but, in common with the supply-siders, he favored tax cuts unconditionally, in large part because he saw measures that made tax revenue lower than otherwise as a means of putting downward pressure on government spending.

The Reagan Administration's official proposals seemed to offer a reconciliation of the competing positions of its economist supporters. Although it did not make the tax cut it advanced conditional on spending cuts being agreed to, the administration did not assume that the tax cut would fully pay for itself through a Laffer-curve mechanism.[157] It set out a plan to reduce

[154] See also Friedman and Friedman (1984, pp. 33–34; 1985, p. 39). The cut was not quite immediate, occurring on January 1, 1982. Laffer, Domitrovic, and Sinquefield (2022, p. 365) argue that a small part of this cut occurred on October 1, 1981, and Friedman and Friedman (1984, pp. 33–34; 1985, p. 39) implied that the whole top-rate tax cut came into force in 1981. But the cut in the top rate on investment income was, as noted, not really implemented until the start of 1982. The October 1 tax cut applied to paycheck withholding and so primarily pertained to federal tax on labor income (see *Boston Globe*, August 2, 1981).

[155] Arthur Laffer, in noting that "it wasn't even Reagan who dropped the highest rate from 70 to 50" percent, recalled: "Now, Reagan said he wouldn't have amendments [to his tax bill]. But we're all sitting in the Oval Office there, and there are about eight or nine of us, and Reagan said that this guy, a Congressman from Michigan, a Democrat, had proposed an amendment that eliminated the distinction between earned and unearned income. And [Reagan] asked us what we thought of it. And we all loved it. So it dropped the highest rate from 70 to 50. And you know, he said, 'Well, I like it too, and I think I'll go along with it.'" (Arthur Laffer, interview, June 10, 2013.) See also Laffer, Domitrovic, and Sinquefield (2022, pp. 364–365).

[156] Even after Reagan took office, Burns remarked (*Evansville Press* (Indiana), May 8, 1981): "I wouldn't reduce personal income taxes at all."

[157] See M.K. Evans (1983, pp. 95–100) and M. Anderson (1990, p. 152). In fact, the administration assumed that the tax cut would lower revenues substantially (see Boskin, 1987, p. 59), and Martin Feldstein commented at the

government outlays by about 3½ percent of total output by fiscal-year 1985, in a manner intended to achieve a balanced budget in mid-decade (Boskin, 1987, pp. 54‒55).

Although he was more sanguine about large budget deficits than many other economists were, Friedman was caught by surprise by the extent to which deficits would widen under Reagan. Indeed, in *Newsweek* (March 2, 1981, p. 34), Friedman suggested that under Reagan's economic program it should prove "relatively easy" to balance the federal budget by 1984.[158] In the event, however, the U.S. government's budget deficit expanded from 2.6 percent of GDP in fiscal-year 1981 to 4.8 percent in fiscal-year 1984 (Council of Economic Advisers, 2011, Table B‒79).

One reason why the balanced-budget prediction did not materialize reflected developments on revenue side. Friedman did not, as stressed in Chapter 8, expect tax cuts to raise revenue, on net. But he viewed the Reagan tax cuts as set to have the effect of limiting the projected increase in aggregate tax revenue, rather than actually reducing them (*Newsweek*, July 27, 1981). Indeed, during 1981 it was widely believed that, in conjunction with other factors making for higher taxes, the Reagan tax cuts would not even actually reduce effective marginal tax rates—it would merely stop them from rising. Representative of this view was a June 1981 media commentary stating of the Kemp-Roth legislation that "what once would have been deep tax cuts will now only offset 'bracket creep'" (*Charlotte Observer* (North Carolina), June 8, 1981). Multiple economic researchers studying the Reagan tax plan, including after it became law, expressed the same or a stronger judgment.[159] In the event, however, marginal rates on income *were* reduced by the Reagan tax cuts. Barro and Redlick's (2009, p. 42) estimate of the overall U.S. federal marginal tax rate registers a decline from 41.8 percent in 1981 to 39.3 percent in 1984.[160]

The ratio of federal tax receipts to GDP declined by a decided amount over the same period—from 19.6 percent in fiscal-year 1981, to 17.3 percent in fiscal-year 1984 (Council of Economic

time (*Financial Time*s (London), May 6, 1981) that "the president's actual program represents a total repudiation of the naïve theory that tax cuts are self-financing."

[158] Likewise, before the election, Alan Greenspan, who had been heavily involved as a Reagan adviser in the campaign, stated: "His program is, in fact, [not only] to cut taxes significantly—10 percent a year over the next three years, each year—but also to restrain the growth in federal spending. In other words, it is not correct to talk of his program as only the cut in taxes. It's a total package… Ronald Reagan actually wants to balance the budget." (*Wall Street Week*, Maryland Public Television, October 24, 1980.)

[159] See Tatom (1981, p. 29), Fand (1981, p. 30), and S. Meyer and Rossana (1981).

[160] The Barro-Redlick estimate was inclusive of Social Security taxes. These rose over the period, in part reflecting increases legislated in Carter's term. Friedman had expected that the tax cuts would "simply prevent an unlegislated tax increase as a result of inflation, or offset the legislated increase in social-security taxes" (*Newsweek*, April 19, 1982). He later revised this judgment to one applying only to 1981 and 1982 (Friedman and Friedman, 1984, pp. 30, 33; 1985, pp. 36, 39). This revised judgment is broadly consistent with the Barro-Redlick series, which does not go down below 1980 levels until 1983.

Advisers, 2011, Table B‒79, p. 284). Friedman had probably expected the tax-to-output ratio to fall much less than this—perhaps to about 19 percent (its value in fiscal 1980). Part of the reason why federal tax rates did fall, and tax revenue declined as a share of output, was that the more rapid than expected decline in U.S. inflation that occurred after 1980 restrained the amount of bracket creep. The 1981 tax act had introduced indexation into federal income tax brackets, but starting only in 1985.[161] The fact that inflation in 1982, 1983, and 1984 was lower than the administration (and Friedman) had anticipated in 1981 restrained the tendency for the ratio of revenues to output to rise. Correspondingly, the Economic Recovery and Tax Act correspondingly delivered a greater true cut in taxes than had been expected.

In any event, Friedman's support for the tax cuts was not conditional on their being consistent with budget balance. Even before Reagan had been sworn in, Friedman argued that it was "much more important" to lower the government outlays-to-income ratio than to reduce the deficit. (*U.S. News and World Report*, December 15, 1980, p. 51).[162]

It was in this area of restraint in federal outlays, however, that the Reagan Administration achieved little in aggregate, especially during its first term. Friedman had accurately described the situation prevailing at the close of the Carter Administration as follows: "There has been no period in peacetime history when government spending has been as high a fraction of national income as it is currently." (*U.S. News and World Report*, December 15, 1980, p. 50.)[163] But this situation did not change under Reagan. Outlays were, at 22.2 percent of GDP, the same share of income in fiscal 1984 as in fiscal 1981, after spiking upward in the interim years (Council of Economic Advisers, 2011, Table B‒79, p. 284). Friedman, though he had expected federal government expenditures to rise over the period (*Newsweek*, July 27, 1981), very likely had anticipated that they would fall in relation to total output.[164] Such an outcome would have been in line with the administration's own target, as stated in 1981, of bringing the share of outlays in gross national product from about 23 percent in 1981 down to 19.2 percent in fiscal-year 1985 (see Boskin, 1987, p. 55).

[161] The introduction of indexation into the income tax system lined up, of course, with a longstanding Friedman recommendation. In reaffirming this recommendation at the start of the decade, he had restated his (dubious) claim that unindexed brackets was "one of the major reasons we have inflation" (*Wall Street Week*, Maryland Public Television, January 4, 1980, p. 14 of transcript)—that is, he implied that the U.S. authorities had deliberately inflated with the aim of generating tax revenue. Similarly, after Congress enacted tax indexation, Friedman told Jerry Jordan that the U.S. economy would now have less inflation (Jerry Jordan, interview, June 5, 2013).

[162] Reprinted in Friedman (1983a, p. 195).

[163] Reprinted in Friedman (1983a, p. 194).

[164] Certainly, his own prescription was for a substantial but gradual reduction in the share. See *Newsweek*, October 1, 1979, *Newsweek*, September 8, 1980 (p. 69), and the discussions of Friedman's budget-limitation proposals in Chapter 10 above and in Friedman and Friedman (1984, pp. 57‒58; 1985, p. 60).

Even before Reagan took office, the extent to which he was actually committed to reducing government's spending share was being questioned: in his *Financial Times* column (November 7, 1980), Samuel Brittan had contrasted Reagan and Friedman unfavorably on this matter by suggesting that, in contending that the move to a smaller public sector would not involve any major government service being cut, Reagan was claiming something that Friedman did not.

Nevertheless, in its first year the Reagan Administration exuded considerable resolution in the area of government-spending control. In this period, a widespread impression was that the administration was serious about pressing ahead with securing the announced target reductions in the growth of federal outlays. This impression was held both by outside supporters of the administration like financial columnist Warren Brookes—who took the Reagan plan's proposed reductions in the ratio of federal spending to GNP through 1984 at face value (*Boston Herald American*, May 26, 1981)—and by critics such as James Tobin—who took the administration as engaging in a neutral or marginally tighter fiscal policy, thanks in part to the spending cuts accompanying the tax cuts (Tobin, 1981a, pp. 5–6).[165] In the fall of 1981, news coverage stressed the spending restraint of the administration, with the *Financial Times* (September 10, 1981, p. 46) reporting: "President Reagan's budget cuts, which he is expected to announce next week are believed to be in the range of $10 to 15 billion… in 1981–82, and up to $70 billion in the two succeeding years. Mr. Reagan's aim is… to wipe the deficit out altogether by 1983–84."

David Stockman, who served as Reagan's budget director throughout the administration's first term but who became disaffected with Reagan's approach to fiscal policy, would later cite the fact that in 1981 the administration had included in its projected spending cuts a category labeled "future savings to be identified" as evidence that the administration was never sincere in its plans to cut spending (Stockman, 1986, Chapters 4 and 6). The so-called undistributed cuts in the 1981 Reagan plan amounted to 1 percent of GNP (Boskin, 1987, p. 55). From Friedman's perspective, the failure to specify areas of spending cuts in advance did not amount to a devastating weakness of the plan. He had been in favor for several years of across-the-board cuts in government spending, with government departments and agencies presented with such a cut as a *fait accompli* and having to decide where to cut.[166] Friedman had implied that this approach was desirable, too, for the circumstances that Reagan faced, when he called for the

[165] Similarly, Martin Feldstein (in *Financial Time*s (London), May 6, 1981) stated: "In my judgment, the administration has proposed a deflationary package." His basis for this judgment was that the package included "major spending cuts" alongside tax changes that did not deliver a tax cut (net of bracket creep) until 1983.
[166] See, for example, Friedman (1977i, pp. 47–48) (also in Friedman [1978b, pp. 72–73, 1991a, pp. 153–154]) and *Milton Friedman Speaks*, Episode 6, "Money and Inflation," taped November 7, 1977, p. 34 of transcript.

incoming president to make "an immediate revision of the U.S. budget, with a lower ceiling on total expenditures." (*U.S. News and World Report*, December 15, 1980, p. 51.)[167]

Reagan did not engage in such across-the-board measures, but he did make spending control a high-profile issue in 1981. Against this background, in October 1981, Friedman strongly criticized federal spending. He expressed confidence that Reagan "will stick to his principles and take the long view" even though he gave him only "about a 50/50 chance" of success in reducing the role of government in the economy (*Spokane Chronicle* (Washington state), October 16, 1981). "President Reagan is willing to stick to his principles and take the long view away from socialism, and we need to support him," Friedman remarked sharply (*The Australian*, October 19, 1981).

On federal spending, Reagan in 1981 seemed, to outside appearances, to achieve success that paralleled his tax-cut triumph. As Friedman later noted, Reagan in the course of that year "got Congress to adopt considerable reductions in various governmental spending programs" during 1981 (*USA Today*, April 11, 1984). Indeed, about three-quarters of the specific savings that Reagan proposed were accepted by Congress (Boskin, 1987, p. 55). These achievements, however, were small in relation to the restraint in domestic federal spending needed to achieve the administration's stated plan to reduce federal spending to around 19 or 19.5 percent of total output by 1984. After his early successes, Reagan did not really "close the deal" on spending control by returning to the issue of spending restraint in a concerted, multi-year manner. At an early stage of the debate on the administration's expenditure proposals, Friedman argued in his *Newsweek* column (May 4, 1981) that the implementation of his negative income tax idea (NIT) would produce substantial savings and improvements in the delivery of federal welfare payments. But the administration did not propose the NIT and kept the multi-category welfare spending structure that Friedman criticized.

The president's spending-restriction plans also would become less ambitious over time. Of the savings that Reagan did propose, Friedman would note that Reagan's second-term proposed cuts favored a less wide-ranging area than those he had proposed in 1981 (*San Francisco Chronicle*, March 5, 1986). He would also observe that after initially proposing to abolish the Department of Education, Reagan soon withdrew the proposal in the face of opposition.[168] Indeed, opposition to Reagan's proposed restraint in nondefense federal spending became mobilized quite quickly,

[167] Reprinted in Friedman (1983a, p. 195).

[168] See Friedman (1984f, p. 33) and Friedman and Friedman (1984, pp. 144–145; 1985, pp. 139–140).

with House of Representatives Speaker Tip O'Neill claiming that Reagan's proposals would "tear asunder programs we've built over the years" (quoted in Cannon, 2000, p. 203), former president Carter speaking out against the proposals—arguing that they were inhumane (Brinkley, 1998, p. 60), and the House of Representatives' Committee on Ways and Means publishing a two-volume set titled *Impact of the Administration's Budget Cuts*, and giving the record of hearings convened in various cities in late 1981 and early 1982.[169] And, as the Friedmans later stressed, the measures being referred to typically did not entail absolute reductions in total real government spending but only proposals "to cut the increases in some categories of spending."[170]

This was, of course, true of the administration's spending proposals all along: even if fully enacted, they implied total federal government spending would rise: "President Reagan's program calls for a reduction… really in the projected *increase* in spending," Friedman noted (*Newsweek*, July 27, 1981). This distinction was substantive—"I don't believe when you're going up a slightly less steep hill that you're not going uphill," he observed soon after Reagan took office (*Arizona Republic* (Phoenix), February 24, 1981)—but made little imprint on political debate.

In the event, Friedman would express his disappointment regarding the progress made in restraining government spending during the early Reagan years. This he partly blamed on Congressional unwillingness to enact a number of those domestic spending cuts that Reagan did propose. He had initial hopes that Reagan's popularity would lead to successful passage of his spending-restraint proposals (*Arizona Republic* (Phoenix), February 24, 1981), but this only happened to a limited degree. It was "a joint failure by the president and by Congress," he suggested (*USA Today*, April 11, 1984). Friedman also faulted the U.S. general public for electing Reagan but then not robustly supporting him on the matter of restraint in government spending (*San Francisco Chronicle*, September 14, 1984).

The lack of ambition in government-spending restraint exhibited by the administration, as well as Reagan's limited political success in securing consent to those spending-restraint proposals he did advance after 1981, became apparent over time. By early 1982, Friedman's reading of the likely paths of outlays and revenues had changed sufficiently for him to declare, in a revised assessment, that it had been *never* realistic to expect U.S. budget balance in 1984.[171] He and Rose Friedman later added that, although both the 1981–1982 recession and the tax cut had

[169] See Committee on Ways and Means, U.S. House of Representatives (1982a, 1982b).
[170] Friedman and Friedman (1984, p. 29; 1985, p. 35).
[171] *American Attitudes*, BBC1, February 16, 1982, p. 5 of transcript.

contributed to the widening of the deficit, the promised 1984 balanced budget would have required a much more ambitious program of spending restraint than Reagan had ever, in fact, proposed—one that would have needed to be on a scale that wound back the rise in the government-spending share recorded in the late Carter years.[172]

The behavior of the broad measure of the overall public-sector spending produced by the Organisation for Economic Cooperation and Development would underline the limited extent to which the Reagan Administration's first-term measures had restricted overall government expenditure in the United States. This series implied that total outlays of the U.S. public sector were 36.9 percent of GDP in 1986: 2.8 percentage points higher than in 1981, and 5 percentage points higher than in 1979 (*OECD Economic Outlook*, June 1989, Table R–14, p. 185).

THE COLD WAR, DEFENSE SPENDING, AND ECONOMIC SANCTIONS

An assessment that Friedman gave at the tail-end of Reagan's tenure indicated that open-economy matters were the area in which he was least happy with the president's track record: "The Reagan Administration has been very bad on international trade and international economic policy." (*Forbes*, December 12, 1988, p. 164.)

In this assessment, Friedman's indictment of international economic policy stemmed mainly from developments during Reagan's final two years in office—especially those in 1987, when the U.S. Treasury authorized large-scale coordinated foreign exchange-market intervention. As discussed in Chapters 15 and 17 below, Friedman had been pleased with the pro-float policy that had mostly been in force in Reagan's tenure up until then—including throughout the president's first term—and was gratified that the administration largely reverted to this policy in 1988.

Trade policy, however, had been a very different matter. Here, Friedman's misgivings stretched over most of the eight years of the administration. The exception was in Reagan's early months, when Friedman still felt that the president would instill a firm free-trade tone. Friedman's preelection commentary had noted some of Reagan's statements on the matter during 1980 with disapproval, but he had discounted those remarks (the candidate's support of steel and automobile import restriction) as amounting to presidential-campaign aberrations (*Newsweek*, November 10, 1980).[173]

[172] See Friedman and Friedman (1984, p. 27, 30–31; 1985, pp. 33, 36–37) as well as the additional comment, added after the 1984 election, in Friedman and Friedman (1985, p. 16).
[173] See also Friedman (1980c, p. 247).

When, therefore, Reagan actually took office, Friedman had an optimistic attitude regarding the administration's likely trade policy. He was unhappy with the degree of protectionist measures already in force in the United States as of the start of 1981 but thought things would change for the better under the new president. In a meeting in Canberra, Australia, in April 1981, that Friedman had with Prime Minister Malcolm Fraser, Fraser countered Friedman's criticisms of Australian protectionist policies by giving examples of U.S.-government-imposed restrictions on the importation of Australian goods. Maurice Newman, who attended the meeting, recalled: "And Friedman said, 'Well, if I was talking to my president, I would tell him exactly the same thing.'" (Maurice Newman interview, September 18, 2013.) On Australian television in the same trip, Friedman likewise remarked: "I don't maintain a campaign for free trade in Australia and a campaign for protection in the United States. If you have read what I have written in the United States, you will find that I have written a series [in *Newsweek*] recently, attacking the automobile industries for their attempt to have restrictions on imports. I'm strongly opposed to them—and I believe that we will not have them, as a matter of fact. I believe there's been a great deal of talk—but the most recent announcement was that President Reagan had decided not to seek [from Japan] 'voluntary' restraints [on exports of automobiles to the United States]." (*Nationwide*, Australian Broadcasting Commission, April 9, 1981.)

The Reagan Administration indeed refrained from car-import restriction during its whole tenure. But Friedman would eventually be disabused of his wider hope that free trade would actually be a strong theme pursued by the administration. He would eventually have a catalogue of complaints about the Reagan Administration's record on trade, including with regard to the importation of motor vehicles other than cars.[174]

Even in Friedman's initial optimistic period concerning likely trade policy, he was very aware of the pressures on Reagan in the area of import restriction. Indeed, he saw these pressures at first hand during the period when the aforementioned proposals for restrictions on car importation were being mooted. Just before his April 1981 visit to Australia, Friedman was involved in a discussion of this matter at the White House—at the second of the occasional meetings of the Presidential Economic Policy Advisory Board (PEPAB).

This second PEPAB meeting was held at the White House on March 25, 1981. The PEPAB—a panel of individuals who, other than their PEPAB affiliation, were altogether outside the administration—included Friedman from its inception. The PEPAB meetings would typically

[174] Again, see Chapters 15 and 17 below.

involve Reagan himself being present for some of the time, and Vice President George H.W. Bush would attend longer stretches of the meetings. But the meetings would often center on discussions between the administration's economic officials and the PEPAB members. In the case of the March 1981 meeting, the official present was Secretary of Transportation Drew Lewis. In the meeting, the PEPAB members—including George Shultz, William Simon, Arthur Burns, and Friedman—were uniformly critical of the talk of restriction of car imports from Japan—the measure that Lewis was advocating (*Washington Post*, April 2, 1981). During a portion of the meeting that Reagan attended, what was said to be a "scathing" assessment made by Friedman, pouring scorn on the distinction between voluntary car-import quotas (of the kind Lewis was hoping the administration would broker with Japan) and legislated quotas, received an approving nod by the president—in what became interpreted as a foreshadowing of Reagan's ultimate rejection of the idea (*Walla Walla Union-Bulletin* (Washington state), April 3, 1981).

A reference that Friedman made in his remarks was revealing on what had been on Friedman's mind over the previous couple of years. He told Secretary Lewis of the need to acquire a historical perspective on the matter by reading the adverse comments made in Henry Kissinger's memoirs about the Nixon Administration's efforts to secure an agreement with Japan that would restrict its sales of textile products to the United States (*Washington Post*, April 2, 1981).

This account appeared in Henry Kissinger's (1979) recollections of his experiences during Nixon's first term. Although Friedman had highlighted its account of an economic-policy episode, Kissinger's book, *The White House Years*, had been decidedly non-economics-oriented—foreign policy, defense, and diplomacy being Kissinger's areas of expertise and responsibility and, therefore, forming the book's main subject matter. [175]

The fact that Friedman read Kissinger's memoirs indicated that during the late 1970s and early 1980s he was trying to make himself more informed on matters concerning foreign policy and geopolitics. This intention on Friedman's part lined up with observations that he was making publicly during this period about the high importance he now assigned to foreign policy.

Foreign policy dangers

In the course of 1980, Friedman was in the unaccustomed position of downplaying the

[175] The title referred to the fact that the period covered (1969 to mid-1973) was when his only post was as the president's adviser on national security and before he became U.S. Secretary of State.

importance of economics. "What happens to this country is fundamentally going to be determined more by foreign affairs than it will be by domestic affairs. We can mismanage our economy badly and recover, but if we slip in the foreign policy area, there is going to be no recovery," he remarked at the start of the year.[176] "I'm increasingly persuaded that economic policy is a backwater," Friedman was quoted as saying several weeks later. "We may continue to do the wrong things [in economics], and it won't really matter if we're taken over by the Russians."[177] In an appearance in Dallas in late March, he underlined what he saw as the more precarious nature of foreign policy than economic policy. In that area, the country could not retrace its steps as easily as it could in economic policy. So the wrong course in foreign policy could do something that mistakes in economic policy could not do: produce the nation's destruction (*Fort Worth Star-Telegram* (Texas), March 27, 1980, p. 4B).

In spring 1980 in Chicago, Friedman elaborated: "If this country is fundamentally threatened, in my opinion it is threatened much more by our weak position in foreign affairs than it is threatened by any mistakes we have made in economic management. We can recover from the mistakes in economic management. But if we [continue to] put ourselves in a position in foreign policy of the kind we've been putting ourselves in, we cannot recover—we're through." (*Donahue*, NBC, April 16, 1980.) He reiterated in June during one of his visits to Canada: "The economic problems are serious—but the foreign [policy] problem could be fatal." (*The Citizen* (Ottawa), June 9, 1980.)

Afghanistan, the Soviet Union, and economic sanctions

In his final *Newsweek* column of the 1970s—after a year in which the Middle East had featured heavily in the news in light of the Iranian revolution, the Camp David accord, the second oil shock, and most recently the taking of the U.S. embassy staff as hostages in Iran—Friedman emphasized the superpower aspects of the Middle East situation. He focused on the appeal that the Soviet Union likely saw in having command over the Persian Gulf's supply of oil—on which the Western economies heavily depended. "The danger is real… that the Soviet Union will succeed in its efforts to control that supply," Friedman remarked, adding that, if this eventuality occurred, it "would be a major threat to world peace and to the unity of the West, let alone to its prosperity." (*Newsweek*, December 31, 1979.)

[176] *Wall Street Week*, Maryland Public Television, January 4, 1980, p. 15 of transcript.
[177] *Insurance Marketing*, March 1980 (p. 5).

During the very week in which Friedman's column appeared, the Soviet Union invaded Afghanistan. Some retrospective accounts have suggested that the invasion was more an *ad hoc* defensive move by the USSR, rather than part of a concerted effort to expand Soviet influence (for example, Garthoff, 1994a, pp. 21, 1037). As a counterpoint, however, it has been noted that "whatever their initial motives, their [the Soviets'] Afghan adventure brought them closer to the Gulf" (*New York Times*, June 30, 1985). In that respect, the invasion at the end of 1979 brought the USSR a step closer to the realization of the scenario Friedman had laid out in his column.

The Soviet invasion of Afghanistan also gave credence to a wider perception: that the USSR was still seeking a world Communist revolution. The assessment in June 1981 of Eugene Rostow—the law professor/economist who, thirty years earlier, had been deeply involved in the same debates on the Korean War inflation in which Friedman engaged, but who was now eminent in foreign policy, was that "the Soviets are not seeking a few border changes: they are seeking a transformation of the system."[178] Regional conflict had been an active way in which the Soviet Union had sought to gain an advantage during the 1970s. Almost simultaneously with the invasion of Afghanistan invasion, *The Detroit News* (December 26, 1979) had editorialized: "Soviet adventurism in the Third World, although always a serious concern, is, in a sense, secondary. Europe is the main event." This assessment was widely shared. But, with Europe in a seeming stalemate owing to the Eastern Europe/Western Europe division, the Soviet Union's activities in the Middle East and the Third World over the 1970s could be interpreted as its prime means of advancing world regime change—as well as (as Friedman had emphasized) giving it scope to have greater control over the supply of basic materials to the Western economies.

Again, Garthoff (1994a, p. 52) offered a contrasting interpretation. He implied that it was not a reasonable reaction when "Western observers usually saw Soviet statements in support of national liberation and progressive revolutionary movements as reflecting an offensive thrust in Soviet foreign policy"—Garthoff's contention being that "Soviet leaders in fact usually made such statements in a defensive context." That may have been the usual context, but the statements coming out of the USSR's state-authorized literature also contained expressions of more activist intentions. For example, a book by USSR official K.N. Brutents, issued in 1977 and titled *National Liberation Revolutions Today,* had stated: "[Formerly] it was a case of defending the first socialist revolution against imperialism, whereas today it is a question of waging of the offensive against imperialism and world capitalism as a whole in order to do away

[178] Testimony of Eugene V. Rostow on June 22, 1981, in Committee on Foreign Relations, U.S. Senate (1981b, p. 41).

with them."[179] Soviet leader Leonid Brezhnev had himself stated in 1975 that "by an alliance of all anti-imperialist forces, new victories may be won in the historic struggle for the social and national liberation of peoples, and for peace and a bright Communist future for all mankind."[180]

Seeing the invasion was another sign that the USSR was on the move, Friedman shared the widespread alarm concerning the Soviet invasion of Afghanistan. The United States, he suggested, needed to "change course" in order "for us to respond effectively to Russian aggression."[181] After Carter had, in an end-of-year television interview, remarked that the invasion had reshaped his own assessment of the Soviet Union's intentions, Friedman gave the president a backhanded compliment, assessing that the Carter remark was "a courageous admission of almost unbelievable prior naïveté." (*Newsweek*, February 11, 1980.)[182]

The grain embargo

One of the steps that Carter took in reaction to the invasion generated Friedman' opposition. The president placed an embargo on U.S. grain exports to the Soviet Union. Friedman criticized the embargo in his New York City press conference on January 7, an appearance at San Francisco's press club on January 9, and in a *Newsweek* column (of January 21, 1980).

In these discussions, Friedman argued that the USSR would be able to get the grain it needed from the world market even if the United States was unavailable as a source. He stressed in his column that basic commodities like grain were highly fungible products. For this reason, he observed, OPEC in the past six-and-a-half years had been able to restrict world oil supply—but not the amount arriving in the United States. So OPEC had not been able to make its 1973–1974 embargo—an attempt to manipulate quantities of oil available to specific countries—stick, even though the price rise that the cartel initiated in the same period proved resilient. By the same

[179] Quoted in Weeks and Brodie (1983, p. 19).

[180] Quoted in Weeks (1987, p. 275). After the invasion of Afghanistan, Yuri Andropov, who would later succeed Brezhnev, remarked: "Ever more states are choosing the socialist path. This is the objective course of history." (*Pravda*, February 12, 1980, quoted in Weeks, 1987, p. 105.)

[181] *Newsweek*, February 11, 1980. As he would continue to do in the Reagan years (see Chapter 15), Friedman in this column pointed to retrenchment of domestic federal spending commitments as a necessary step in the process of positioning the country better for its defense challenges.

[182] The Carter interview in question was conducted with Frank Reynolds of the ABC network. The portion of the interview to which Friedman's column referred was the same as that quoted in *Detroit Free Press*, January 1, 1980, and at more length in Garthoff (1994a, pp. 1059–1060). Another economist's account, like Friedman's, suggested that, in this interview, "Jimmy Carter admitted that he had been badly fooled" by the USSR (M.K. Evans, 1983, p. 88). Carter, however, later remarked: "The Soviets under Brezhnev will seize on every opportunity to further the Communist cause. I was not misled about their ultimate intentions." (*Time* magazine, October 11, 1982, pp. 62–63.)

logic, the United States' effect on the USSR's access to grain would work via effects on the world price, not through the embargo *per se*. With the federal government now committed (under the embargo arrangements) to buy grain output intended for export to the Soviet Union, what the embargo would do was produce some net rise in the price that the USSR and every other country paid for grain. "It will have little or no influence on the Russians [that is, on the Soviet economy and on USSR government policy]," Friedman argued in his column—something that he suggested demonstrated the principle that "economic sanctions are not an effective weapon of political warfare."

Friedman indicated that he could see merit in the embargo provided that it was really an effective way of exerting pressure on the USSR, but it was not. It "will have next to no deterrent effect," he contended (*San Francisco Examiner*, January 10, 1980). "Cite me one example in history where economic sanctions have been successful against military force," he challenged San Francisco's journalists, while noting, "I wish it were otherwise, but the embargo simply is not realistic." (*San Francisco Chronicle*, January 10, 1980.) Furthermore, as he noted in New York City, the embargo had the disadvantage that "the American taxpayer will be giving a bigger subsidy to the American farmer" (*The Plain Dealer* (Cleveland, Ohio), January 8, 1980). The unpopularity of the embargo, among U.S. farmers and general population alike, was reflected in the fact that President Carter was booed when he referred to it in his August Democratic Party convention speech—and also in the pledge by candidate Reagan to repeal it.

President Reagan lifted the grain embargo in 1981. Indeed, commercial relations with the USSR largely proceeded normally, or even expanded, over Reagan's first term, even though the intensified Cold War atmosphere prevailing in the first half of the 1980s. His administration did not make economic sanctions a truly systematic part of the confrontation with the USSR (see Chapter 15).

Soviet-American relations and the Carter-Reagan defense buildup

Friedman advocated a political and economic system that was the diametric opposite of that the Soviet Union embraced. Furthermore, his own public posture toward the Soviets had never exhibited the more conciliatory attitude that U.S. public officials had shown in the 1970s and that was disappearing rapidly in 1979−1980. But notwithstanding Friedman's status as a resolute opponent of Communism, the Soviets displayed a degree of esteem for him.

Friedman's presence among the circle of economists associated with Ronald Reagan was noted in a commentary published three days after the 1980 presidential election in the Soviet Union's state-controlled press. The article in *Pravda* observed: "In Ronald Reagan's entourage are found such experienced public figures as former Treasury Secretary William Simon, economists Alan Greenspan and Milton Friedman, and other well-known people."[183] Ulam (1983, p. 288) suggested that this respectful reference to Friedman exemplified the Soviet authorities' regard for opponents who exhibited a consistent approach to policy.[184] If this assessment was accurate, the respect for consistency presumably offset the Soviets' opposition to much of Friedman's record, including his advocacy of free markets and his recent meeting (during the long China portion of his visit to Asia) with government officials of the People's Republic of China.[185]

The respect was mutual, in the limited sense that Friedman, despite his contempt for the USSR's economic and political system, regarded the Soviet Union's leadership as having shown resolution over the 1960s and 1970s in building up the country's military strength. In contrast, in his assessment, the period up to 1980 had been characterized by "military mistakes" on the U.S. side (*Newsweek*, February 11, 1980). "We are militarily weak and impotent," Friedman remarked, "and the Russians are taking advantage of that." (*San Francisco Examiner,* January 10, 1980.)

Well before 1980, Friedman subscribed to the view that a major defense buildup was in order. "Good government is one that would provide us with an adequate national defense, which the present government has certainly not been doing," Friedman said shortly after the invasion of

[183] Quoted in Ulam (1983, p. 288).

[184] Friedman's views on monetary policy were probably not among his most objectionable opinions for Soviet officials, in view of the fact that the Communist economic literature sometimes articulated a money-oriented framework in discussing aggregate price-level behavior. Indeed, Friedman liked to emphasize the extent to which Karl Marx and the Communist economic literature accepted quantity-theory propositions. See, for example, his commentary in *Newsweek*, October 27, 1980, and the discussion in Nelson (2020b, Chapter 11). But it would overstate things to suggest that the Soviet Union's articulation of monetary economics simply adhered to quantity-theory or monetarist lines. Rather, in practice, Soviet discussions of monetary matters were imbued with Communist ideology. For example, a government-approved set of lectures on monetary management, delivered in July 1962 (not long before Friedman's visit to the USSR), had stated dogmatically: "There is and can be no inflation in the USSR." (Moscow State University International Banking Summer School, 1962, p. 122.) And, speaking in February 1981, Leonid Brezhnev had made little allowance for Friedman's proposition of no long-run inflation/unemployment tradeoff, when he stated: "Measures taken by the bourgeois governments in order to fight inflation contribute to the stagnation of production and the growth of unemployment; attempts to stop the fall in production tend to intensify inflation." (Quoted in Ulam, 1983, p. 297.)

[185] By the time of Friedman's 1980 visit to China, the USA had established diplomatic relations with the country (at the start of 1979). In contrast, relations between China and the USSR continued to be poor, and the Soviet Union interpreted China's links with the USA in that light. Speaking for the Soviet government, military/political leader Dmitry Ustinov remarked that the USA was "making active use of China's present inflammatory policy of hostility to [Soviet] socialism" (*Pravda,* May 9, 1981, quoted in Garthoff, 1989, p. 288), while political leader Mikhail Gorbachev declared: "Militant imperialist circles are stepping up the arms race… Actively assisting the imperialists in this are their collaborators, the leaders of Peking, for whom the struggle against the socialist commonwealth [that is, the USSR and affiliated countries] has become one of their main policies." (In *Pravda,* May 27, 1981, as quoted in Weeks, 1987, p. 131.)

Afghanistan.[186] In April 1981, he observed: "over the past 20 years, the Soviet Union has been gaining enormously in military strength relative to the West, and I believe that we are entering a very dangerous decade, and so, regretfully, I do support… higher defense spending."[187]

When reviewing the historical record regarding U.S. defense spending, in their 1984 book *Tyranny of the Status Quo,* Milton and Rose Friedman noted a "decline in defense spending [that had] tapered off and after 1979 was replaced by a rise."[188] This description did not capture a point made during 1980 by former members of the Ford Administration, in response to suggestions that only under Carter had real U.S. defense spending started to grow again.[189] As these rebuttals stressed (and as the Friedmans' own chart of defense spending indicated), U.S. real defense expenditure started picking up in Ford's final year in office.[190] Perhaps in response to correspondents making this point to them, the Friedmans put in a makeshift correction in the 1985 revision of their book—changing the text to "after 1970 was replaced by a rise."[191] Although factually accurate, the revised wording was itself not very satisfactory. Many indictments of U.S. military policy—including some that Friedman had made in the past—had concentrated on the fact that the downward movement in real defense spending continued *well* after 1970, even as the Soviet Union's military efforts were expanding.

President Carter's tenure saw defense spending pick up in each of his years in office. Pressure to step up the rate of increase had already emerged before the invasion of Afghanistan: for example, Carter had pledged in 1978 to increase real defense spending by 3 percent per year in real terms on a multi-year basis. As Friedman later observed, defense spending then went up more steeply

[186] *Wall Street Week,* Maryland Public Television, January 4, 1980, p. 13 of transcript.
[187] Friedman (1981d, p. 16).
[188] Friedman and Friedman (1984, p. 25).
[189] See, for example, Gerald Ford's remarks in *Wall Street Journal,* June 23, 1980. See also the indignant statement by former national security adviser, Brent Scowcroft, that "the upward trend in real defense expenditures began… with President Ford." (*Wall Street Journal,* January 28, 1980.) Gray and Barlow (1985, p. 31), in making the same point, dated the rise in defense spending to fiscal-year 1976. In modern official data, calendar-year 1976 shows a further decline in real defense spending, and calendar-year 1977 shows the first rise in the decade (Council of Economic Advisers, 2011, Table B–6, p. 197). This pattern is in line with Scowcroft's suggestion, given in his 1980 piece, that the actual increase in U.S. defense outlays started only in fiscal-year 1977 (which was, essentially, a year for which budgetary moves reflected Ford-era decisions, although Ford left office during the year) but that an increase in real spending had been authorized also for the previous fiscal year.
[190] For the chart of U.S. real defense spending presented by the Friedmans, see Friedman and Friedman (1984, p. 71; 1985 p. 72). This chart largely agrees with that of Ramey and Shapiro (1998, p. 178) for the quarterly series, expressed in logarithms, over a longer period. (The Friedmans' chart showed a small mid-decade rise—one not seen in modern sources—before a resumption of the downward trend.) With regard to defense spending as a share of aggregate U.S. income, this did not pick up until later than 1977—specifically, in fiscal-year 1980. (See Council of Economic Advisers, 2011, Table B–79, p. 284. See also Friedman and Friedman, 1984, p. 72, and 1985, p. 73, as well as Ramey and Shapiro, 1998, p. 178.)
[191] Friedman and Friedman (1985, p. 31).

"in about '79 or '80, in the last year of the Carter Administration" (*Donahue*, NBC, April 25, 1984), and the Friedmans would attribute the upward contour in real defense spending to the administrations of both Reagan and Carter.[192] They specifically noted that Carter recommended decided increases in defense spending "during his last two years in office."[193] Subsequent economic-research contributions that have been concerned with defense spending as a fiscal policy variable have followed a similar line and so referred to the "Carter-Reagan" defense buildup (see especially Ramey and Shapiro, 1998, p. 176).

In his budget submissions shortly before leaving office, Carter had envisioned real growth in defense spending of about 5 percent per year over fiscal years 1982–1986. In March 1981, Reagan proposed a still-greater military buildup that would involve about 7 percent growth per year in real defense spending over the fiscal years 1982 to 1986 (Kanter, 1981, p. 27). The realized outcome for calendar years 1982–1986 was average growth in real defense spending of about 6.5 percent per year (Council of Economic Advisers, 2011, Table B–6, p. 197). In fiscal-year 1985, defense spending was about 6.6 percent of GNP, approximately equal to what the Reagan Administration had envisioned for that year in its March 1981 proposals (Boskin, 1987, p. 55).

Shortly after Reagan laid out his spending plan, Friedman was asked about defense appropriations. He remarked, "They will go up rapidly—but spending won't go up as rapidly as the appropriations. That's what everyone forgets." (*Boston Globe*, April 1, 1981, p. 45.) This proved to be an astute assessment. The Carter-Reagan defense buildup, although launched in fiscal-year 1980, was not manifested in earnest in actual defense spending until 1982 (see Ramey, 2011).

During the early stages of the appropriations process for the defense buildup, a news report had confidently stated that "the outcome of the defense buildup has got to be inflation." (*New York Post*, February 6, 1980.) In fact, inflation fell sharply when higher U.S. defense expenditures got under way on a large scale in 1982, and the inflation rate then reached a decade trough in 1985–1986 when the buildup was near its zenith.

Friedman's own accounts had long played down the link between U.S. military spending and

[192] Friedman and Friedman (1984, p. 70; 1985, pp. 72–73). Before the invasion of Afghanistan, the Carter Administration had already indicated that it would propose a 4.5 percent increase in real defense spending in its next budget (*The Detroit News*, December 9, 1979).
[193] Friedman and Friedman (1984, p. 70; 1985, p. 72).

inflation. Not only did higher defense spending not necessarily lead to higher monetary growth, but recent wars had not been closely tied to inflation. In World War II, there had been inflation (much of it unveiled after the war), but he believed it could have been largely avoided by a firmer monetary policy.[194] In the Korean War, the major burst of inflation had been associated with the early surge in consumer spending, rather than the military outlays associated with the war. And Friedman deprecated accounts that cast the inflation of the 1960s, still less that of the 1970s, as following inevitably from the United States' Vietnam War spending commitments.

Dissent and the freedom of trade in ideas

"We do not have freedom of trade in goods around the world, but we have freedom of trade in ideas," Friedman had remarked in April 1975.[195] He was speaking here primarily in reference to the Western nations. But in this period, and subsequently, the communications of ideas, including by covert means, were also reaching the Soviet Union and Eastern Europe. Correspondingly, Friedman explicitly included Eastern bloc nations among his examples when, just before the 1980 election, he observed: "Countries… have never been able to interfere effectively with the movement of ideas."[196]

In the past, the Soviet Union had—notwithstanding the controls on freedom of speech and communications channels in its own areas of influence—professed to welcome the international spread of ideas. For example, Soviet official V.M. Sikorskii had written in 1965 that "ideas, as is well known, travel without visas, and their diffusion cannot be stopped at the frontiers."[197]

The specific ideas that the Soviet Union likely applauded as of 1965 related to the alleged virtues of Communism as an economic system and the supposed success of the USSR economy. But by 1980, talk of such success had largely dissipated. As discussed in the previous chapter, there was now much clearer awareness in the United States about the extent to which the Western side had prevailed when it came to the competition on economic performance in the Cold War. The routine way in which the market system was now accepted as more productive and consumer-friendly than the USSR's was reflected in Paul Samuelson's remark in the very early days of

[194] This was his retrospective judgment on the matter. During the years of World War II, Friedman had held that aggregate demand policy could forestall a wartime inflation, but he had emphasized fiscal policy as the device that could achieve this. See Nelson (2020a, Chapter 3).
[195] Friedman (1975b, p. 9).
[196] Friedman (1980c, p. 244).
[197] Quoted in Kulski (1973, p. 71).

1980, at an NBER conference also attended by Friedman: "Markets are more effective for many facets of economic activity than people have been able to make fiats and commands be."[198]

The West's economic predominance was also widely known on the other side of the Iron Curtain—with the word getting out despite the higher living standards of market economies being ignored or played down in the Eastern bloc's official press. Relatedly, by the early 1980s the Communist system was facing overt and concerted defiance, with the USSR's Eastern European satellite of Poland seeing a conflict between the Polish government and the maverick labor union, Solidarity. Although it was not really an advocate of a change to a market system, Solidarity was calling for Poland to have liberties, such as freedom of association, available in the Western world. The Solidarity protest movement also highlighted an economic defect of the Communist system by pointing to the shortage of staple consumer goods.

In late 1980, Friedman pointed to the "changes in Poland" in the form of Solidarity's activities as an extreme case of the global movement against government control of the economy (*Sunday Telegraph* (London), November 9, 1980, p. 40). The course of events in Poland "manifested dramatically" this movement, he remarked (*Newsweek*, November 10, 1980).

Several months later, with the Polish authorities having seemingly concluded that concessions needed to be made to Solidarity, Friedman affirmed that the intellectual "tide that's now flowing is now reflected just as much in what is happening in Poland today, in what is happening in [economic changes in] China today, in the election of Margaret Thatcher in Britain, [and] the election of Ronald Reagan in the United States."[199] The behavior of the members of Solidarity in Poland, Friedman affirmed in May, was a case of a "revolt against their masters and against the growth of government" (*The Daily Oklahoman* (Oklahoma City), May 19, 1981).

A key difference from the U.K. and U.S. cases, of course, was that the developments in Poland did not entail a change in government. They consisted, instead, of a high-profile display of dissent toward a totalitarian system.

In time, the Solidarity movement in Poland would be seen as part of an epochal event of the 1980s: the collapse of the Soviet Union's control of Eastern Europe, exemplified by the fall of the Berlin Wall in November 1989 and the subsequent reunification of Germany, as well as the

[198] Samuelson (1980, p. 671). For more on this conference, see the discussion below titled "Martin Feldstein."
[199] Friedman (1981a, p. 5).

restoration of democracy and market economies in other countries, including Poland. At the end of 1981, however, a different outcome seemed to have been set in train. In mid-December, the Polish government engaged in a major crackdown, imposing martial law on the country.

Picking up ideas from Hoover Institution colleagues

The imposition of martial law underlined the Soviet Union's determination in the early 1980s to maintain the *status quo* in Eastern Europe. As already indicated, however, Friedman in this period believed that the USSR was concerned with going beyond maintenance of the status quo and expanding its empire. And, in this environment, he was concerned that the overall relationship between the superpowers might yet lead to the outbreak of World War III. In a talk given in China in the fall of 1980 on "The Western World in the Eighties," Friedman opened by remarking: "The future not only of the West but of the world as a whole depends to a large extent on developments in the international sphere—in particular, the relations between the United States and the Soviet Union. The danger of a major conflict between the two superpowers is clearly present."[200] Similarly, in August 1981, Friedman laid out two alternative scenarios that the U.S. economy might adhere to over the next few years—one in which the authorities persevered with a disinflationary policy was seen through, and one in which they abandoned it—but added the qualification that, in concentrating on these two scenarios, he was assuming that "we can avoid a third world war." (*The Vancouver Sun* (British Columbia, Canada), August 24, 1981.)

For Friedman, a key difference between the early 1980s and prior postwar episodes of international tension was that he was now located in an organization—the Hoover Institution—in which experts on foreign policy and defense were office neighbors or near-neighbors. This difference in circumstances raises the question of to what extent Friedman became steeped in these colleagues' views through interaction with them or through their publications (including numerous pamphlets and books issued by the Hoover Institution).

In some respects, such exposure was limited. Typically, Friedman worked at home for most days of each week. And when he did come onto the Hoover Institution premises, he naturally interacted most often with fellow economists or with guests visiting his office. Indeed, when it came to one of the most famous defense experts at the Hoover Institution, Edward Teller,

[200] Friedman (1990d, p. 43).

Friedman would speak warmly about Teller publicly but saw little of him in practice.[201]

Nevertheless, interaction between analysts in different disciplines was encouraged by the Hoover Institution's practice of having, every weekday afternoon, a coffee-and-cookies event, to which all its affiliates were invited.[202] Friedman attended these gatherings often. It was, therefore, through these and other forums that he involved himself in numerous, if sporadic, discussions with those colleagues who specialized in defense and geopolitics. These colleagues tended, as he did, to take a posture strongly suspicious of the Soviet Union and its intentions and to be in favor of increased U.S. defense efforts. But the precise way in which Friedman approached East/West relations in the 1980s likely partly reflected the cumulative effect of his interaction with Hoover Institution experts and his exposure to the perspective they took on particular issues.

A possible specific instance of the consequent change in Friedman's outlook toward defense matters is the topic of the likely character of a nuclear war between the superpowers. In the 1970s and 1980s, defense hardliners in the United States tended to expound the position that the Soviet Union did not subscribe to the view that "mutual assured destruction" was the inevitable result of nuclear war—and that the USSR instead followed a nuclear-war-waging strategy, aimed at victory. These hardliners also suggested that the United States should itself adopt a war-winning posture with regard to strategic nuclear conflict. Their viewpoint was ultimately influential on military policy, being reflected in President Carter's decision in the summer of 1980 to change U.S. strategic doctrine toward a war-winning strategy and the Reagan Administration's maintenance of this line.[203] Before then, the viewpoint had also appeared extensively in the research literature on the subject, including that produced at or published by the Hoover Institution. An example was Hoeber and Douglass' (1980) study "Soviet Approaches to Global Nuclear Conflict," which appeared in a Hoover Institution Press volume

[201] In Friedman (1977a, p. 15), he remarked that Teller should have won a Nobel prize in physics, and the Friedmans cited him as an authority in their *Free To Choose* book (Friedman and Friedman, 1980a, p. 191). On the limits to the Friedman/Teller interaction in their years at the Hoover Institution, see Nelson (2020b, Chapter 14).

[202] John H. Moore recalled: "There was really a very collegial operation at Hoover in those days, focusing in part—and I'd say even in large part—on a kind of a tradition there of having coffee about 4 o'clock in the afternoon every day. And there'd be coffee and cookies in the commons room. And people from the academic staff or the policy staff would just come on down and sit around and have coffee and talk—coffee and cookies and so forth. And then, of course, people were [also] invited to come to presentations on fields outside their own. So there was really a very collegial atmosphere." (John H. Moore, interview, April 29, 2014.)

[203] See Pipes (1981, p. 135). Richard Pipes of Harvard University was the most well-known public exponent of this view, though he also acknowledged that he was propounding in the public square an interpretation of Soviet doctrine that had been made numerous times by Western analyses (see Pipes, 1981, pp. 135, 136, 168). Friedman, along with Arthur Burns, Paul Samuelson, and numerous others, had been a joint signatory with Pipes in an open letter published in the advertising space of the *New York Times* (June 7, 1967) supporting Israel in the Six-Day War.

that also included a chapter by the Friedmans (an excerpt from the *Free To Choose* book).[204]

Friedman's own commentary on these matters remained very limited, but on the basis of the available evidence it is possible to detect a change in Friedman's own thinking on them—and one that was in a direction toward that taken by defense hardliners affiliated with the Hoover Institution. In his *Playboy* interview given in late 1972, Friedman had suggested that a war between the United States and the USSR would be nuclear and would be "extremely short," presumably culminating in world destruction, and involving little mobilization of U.S. resources.[205] In the early and mid-1980s, he affirmed that a world war would be "catastrophic" and had to be prevented.[206] But a change in what he now said was that Friedman now seemed to think that a war might well be prolonged in nature and involve large-scale mobilization of resources. He suggested that the extent to which American economic resources could be rapidly reallocated to military use would affect the chances of U.S. victory (see Chapter 15). And the Friedmans suggested that even a war involving the use of nuclear weapons might last weeks.[207]

Also in common with what was being articulated on the hardline side of the national-security debate, Friedman now seemed to think that an outcome of a new world war might be something other than outright global destruction. In particular, he expressed fear that the USSR might well emerge as the victor in the war. For example, in mid-1980 Friedman remarked that what most worried him "is that we all may come to be satellites of the Soviet Union. We've been very fortunate to enjoy freedom, but we will not [continue to] enjoy it unless we take the measures necessary to protect it." (*Chicago Tribune*, July 20, 1980, p. 22.) It may be that, at this stage, he viewed as plausible the prospect, often sketched by defense hardliners at the time, of a USSR war victory, achieved through a first strike on the United States' land-based missiles (or a warning to make such a strike) which would, in the scenario sketched, give rise to a U.S. capitulation rather than to an escalation in hostilities.[208]

Many national-security observers viewed this scenario as farfetched. But it had been invoked not only by hardliners at academic institutions and public-policy institutes but also, to some extent, by policy officials. In particular, the possibility of a Soviet Union-instigated first nuclear

[204] See Friedman and Friedman (1980b).
[205] *Playboy*, February 1973 (p. 62), reprinted in Friedman, 1975a, p. 24; 1983a, p. 40). See also Friedman's remark (*Newsweek*, June 26, 1967) that World War III, should it occur, "is likely to be brief."
[206] See Friedman (1990d, p. 43) for the quotation and Friedman and Friedman (1984, p. 73, 1985, p. 74) on the need for war prevention.
[207] Friedman and Friedman (1984, p. 73; 1985, p. 74).
[208] In this connection, see Friedman and Friedman's (1984, p. 73; 1985, p. 74) statement that "national surrender" on the part of the United States must be avoided through defense preparation as an outcome of nuclear war.

strike was cited as one of the reasons for the United States' procurement of mobile land-based strategic nuclear weapons (such as the MX missile). Such weapons were on course to be central in the next generation of the United States' nuclear-missile forces, even before the Carter-Reagan defense buildup proper was underway.

Continuing opposition to the draft

Although he saw eye-to-eye with defense hardliners on the need for a defense buildup, and viewed a large-scale war as something for which the United States needed to be prepared, Friedman parted company with many of them on an issue that had seemingly been resolved in his favor after the United States withdrew from combat in Vietnam: conscription.
At the start of the 1980s, there was considerable support among hardliners for restoration of compulsory U.S. military service. For example, a one-time senior U.S. soldier argued (Walt, 1979 p. 18) that "[w]e must reinstitute a draft, though not the same unfair system we had before. A draft should be absolutely fair so that every American male, upon reaching 18 or high school graduation, can serve two years in the cause of defending his country."

Friedman continued to be outspoken against what he described as the "plague" of conscription—using his *Newsweek* columns to criticize the calls during 1979 for its restoration, as well as the action taken in that direction by the Carter Administration's reintroduction, in 1980, of draft registration (*Newsweek,* April 16, 1979, May 14, 1979, February 11, 1980, and September 29, 1980).[209] As discussed in Chapter 10, Friedman challenged the position taken by many defense hardliners on the issue when he confronted them at a conference that Hoover Institution held on conscription in December 1979 (published as M. Anderson, 1982). He did so again in 1981, when he criticized a pro-conscription editorial in the *Wall Street Journal*—a newspaper with whose positions on defense matters he usually found himself in agreement but that had turned strongly pro-draft over the course of the 1970s and continued to press for conscription in the 1980s. Friedman felt that the newspaper was overlooking the degree to which the 1970 Gates Commission report had assembled comprehensive evidence in favor of an all-voluntary force (*Wall Street Journal*, June 11, 1981).

The criticisms of the post-1973 system voiced by defense hardliners in the *Wall Street Journal* and other outlets did not lead to a return of the draft. As noted above, the Carter Administration

[209] The "plague" description appeared in his column of May 14, 1979.

did reactivate draft registration, which had been discontinued in 1975.[210] But it went no further than that. And, likewise, the Reagan Administration continued the all-voluntary forces model that had been in force since 1973.

III. PERSONALITIES IN DEBATE ON FISCAL POLICY, GEOPOLITICAL DEVELOPMENTS, AND AGGREGATE SUPPLY, 1979–1981

ARTHUR LAFFER

Arthur Laffer and Friedman were economic advisers to Ronald Reagan both in the 1980 election campaign and during the Reagan presidency. In the latter period, the occasions when Friedman saw Reagan were typically also in Laffer's presence—in the context of the occasional PEPAB gatherings. Laffer and Friedman were mainstays of PEPAB from the time its first meeting was held at the White House, on February 1, 1981.

The joint presence of Friedman and Laffer at the PEPAB gatherings lined up with the popular perception of the two economists. In the early 1980s, the two of them were linked in public discourse—both being seen as providing the inspiration for the Reagan-era economic policies.

Friedman and Laffer were also, despite notable areas of agreement, emblematic of a tension that existed in the economic advice being given to the Reagan Administration. On fiscal policy and regulatory policy, the two of them had considerable overlap in policy recommendations—so much so that Laffer remarked, "On public policy, I can't remember ever, ever, ever being pitted against Friedman" (Arthur Laffer, interview, June 10, 2013)—despite some differences in view about the structure of the economy. But on monetary policy—which, despite the independence of the Federal Reserve, figured heavily in the Reagan Administration's deliberations and in public scrutiny of the administration in its early years—they had starker disagreements (which in part stemmed from a longstanding bone of contention between them: the connection between inflation and exchange rates).

The divergence between Friedman and Laffer had its counterpart in the makeup of the full-time economic personnel of the administration. These included people drawn from both the supply-side and monetarist camps. In the U.S. Treasury, for example, Secretary Don Regan's Under Secretaries included both supply-sider Norman Ture and monetarist Beryl Sprinkel. Regan

[210] See U.S. Secretary of Defense Harold Brown's testimony of January 31, 1980, in Committee on Armed Services, (1980, p. 34).

himself was quoted early in the life of the administration that, on account of his having subordinates of such different economic persuasions, "At times, I feel like a referee." (*Financial Times* (London), February 18, 1981.)

The references in the media to Friedman and Laffer as influences on the administration amounted, however, to only the latest instance of their names appearing together in economics discussions. Laffer had been at the University of Chicago for stretches of the 1960s and 1970s—being at the business school from 1967 to 1976 (albeit with lengthy breaks). He therefore was at the university over much of Friedman's final decade or so at the economics department.

In his role as an academic economist, Laffer produced a considerable number of research publications through 1975. But much of the profile that Laffer established starting in the early 1970s arose from his activities in economics that were outside his formal research output. In these activities, he paralleled aspects both of Friedman's early career path—by serving in the economic staff of the federal government in his late twenties—and of Friedman's later career path—as a frequent participant in public-policy discussion, making contributions to economic debates in the media and in policy circles. It is in these capacities that Laffer attained national and world fame. The controversies in which Laffer participated—and sometimes helped initiate—in the course of his activities also brought out the fact that, although Friedman and Laffer were both critics of Keynesian economics, there was a mixture of tension and solidarity between them on economic matters.

Early interactions

Laffer considers what amounted to "my real start with Milton—and that was before I had met him" as being his exposure to Friedman's work while majoring in economics at Yale University, from which Laffer received his undergraduate degree in 1963. "I read a lot of Milton Friedman," with the result that "Friedman was my hero as an undergraduate at Yale." Laffer elaborated: "And my inclinations back then were more towards the free-market, pro-growth—you can call it 'conservative'—side. And I adopted a number of Milton Friedman's positions back then, as a Yale undergraduate, in seminars, debates, etc." Laffer suspects that he received a low grade for an exam answer when he penned "a macroeconomic Friedman response" in the comprehensive exam (Arthur Laffer, interview, June 10, 2013).[211]

[211] At Yale University, Laffer noted, while "he was read," Friedman was "held up as an example of what's wrong." (Arthur Laffer, interview, June 10, 2013.)

Laffer then studied economics at Stanford University.[212] He noted, with regard to Friedman, "I think he came out to Stanford a couple of lectures when I was a graduate student. And also, I met him during the interview process"—the process that led to Laffer being hired by the University of Chicago (Arthur Laffer, interview, June 10, 2013). After joining the business school in 1967, Laffer became a regular attendee of the Friedman-run Workshop on Money and Banking. Indeed, then-graduate student Fred Levin, who overlapped with Laffer at the workshop from 1968 to 1970, recalls him as having been the only participant who regularly held his own in debate with Friedman. "I remember Friedman being very intimidating there, as he was in the lectures [too]. He was very quick and extremely smart—and, you know, pretty controlling. But I remember that the only person I've ever seen argue with him successfully was Art Laffer, who was just as quick as Friedman. And he would stand up to Friedman." (Fred Levin, interview, March 10, 2014.)[213]

Arthur Laffer observed of the money workshop: "The students were permitted to be unruly—which was not true in the trade workshop. [In the money workshop,] they were permitted to yell and scream at a presenter—which made it a very volatile environment… [Students could be] very aggressive in those [sessions], always trying to impress Milton." (Arthur Laffer, interview, June 10, 2013.) Marc Miles—a graduate student at the university starting in 1971 who would have Laffer as a dissertation adviser following Laffer's return to campus in 1972 (see Miles, 1984, p. ix), recalled: "Laffer used to joke that when Friedman opened the workshop with 'Mr. *X* [the invited speaker] is going to show us today why 1 plus 1 is 3,' that was the cue for the participants to take out their knives and go after the speaker." (Marc Miles, personal communication, March 10, 2014.)

Laffer noted that at the workshop sessions, "I was attacking [monetarism]. With a couple of other guys as well. [There was] Fischer Black, myself, Dick Zecher, and a bunch of others." Laffer noted that, in his own output of work, there was "a lot of my life there which was not favorable to monetarism. That doesn't mean I didn't think the world of Milton—didn't think that he had done a great job. But, you know, this is like anything else in economics: evolving." With regard to Friedman's reaction to criticisms of monetarism, Laffer noted that "he was

[212] Early printings of Krugman (1994) (corrected in later printings as well as the 1995 paperback version) stated that "Arthur Laffer has a Ph.D. from the University of Chicago" (p. 86). Likewise, Jack Kemp (in his prepared statement in Joint Economic Committee, 2000, p. 128) referred to "Robert Mundell and a student of his when he was at the University of Chicago, Arthur Laffer." In fact, Laffer was not a teacher at the University of Chicago, not a student.

[213] Over the same period, however, see Robert Gordon was also present at the workshop sessions, challenging monetarism from a fiscal-policy-focused Keynesian perspective. See Nelson (2020b, Chapter 14).

always perfectly professional in every response." He added: "Milton was professional. He did not have to be professional with me—he did not. He could have just whacked me. He never once chose to do that." Indeed, Laffer judged that "Milton was extremely personable," and, in particular, "When you got to know him, he was really charming." He found Friedman "so much fun to discuss, and debate, and to argue with. And *he* loved it… He was willing to debate any economic issue, any issue in any field… I mean, there was no topic that Milton Friedman didn't find fun [to argue about]." Laffer suggested that as a senior colleague, "I couldn't ask for a finer person," and summed up these and later interactions as: "Friedman was so kind to me personally. Didn't mean he pulled his punches professionally. But kind to me personally." (Arthur Laffer, interview, June 10, 2013.)

Although Krugman (1995, p. 11) would later claim of Laffer, "he has never tried to break into the world of conventional academic research," Laffer was, as already indicated, actually a prominent journal contributor until 1975. Indeed, by the early 1970s, as Victor Canto noted of Laffer, "he had published like five or six articles already" in journals, as well as articles in books (Victor Canto, interview, September 12, 2015). It was in 1970, Laffer recalled, "when I got my first big paper published, which was 'Trade Credit and the Money Market,' in the *Journal of Political Economy*. Which was a big deal for me." He noted: "I think it was scheduled to be the lead article, and then Milton had an article coming out [in the same issue], so I fell to second place." (Arthur Laffer, interview, June 10, 2013.)[214]

The "1065 and all that" controversy

It would, however, take a temporary move by Laffer away from the University of Chicago, to serve as an economist at the Office of Management and Budget in 1970–1972, for him to attract much wider attention—and for the differences in the views between himself and Friedman to become very widely known.

David Ranson recalled of Laffer's OMB position: "he was recommended by Friedman, I understand, to [George] Shultz as a bright young assistant professor who could be brought to Washington and take a senior position. And Shultz did, in fact, hire Laffer in 1970 to be the economist—and they didn't call it 'chief economist,' but that's really what it was—of the Office of Management in the budget, which was a new agency at that time. Nixon had appointed Shultz

[214] See Laffer (1970) and Friedman (1970c). Laffer had presented his paper at Friedman's workshop on November 28, 1969 (University of Chicago Library records).

to head up this new agency, which was a much larger manifestation of the [old] budget bureau. And so [as possible staff] Laffer started calling a couple of Ph.D. students that he was sitting on the committee of. And I don't know how many there were, maybe only two, I was the first one he called… I took the job. Even though I was at Boston Consulting Group, and enjoying and doing well there, the OMB opportunity was really something I couldn't turn down." (David Ranson, interview, April 30, 2014.)[215]

It was at the CBO that, in early 1971, the work subsequently written up in Laffer and Ranson (1971a, 1971b) was produced. It consisted of econometric modeling of nominal GNP behavior—loosely descended from the St. Louis equation linking nominal-income increases to increases in money and in government purchases. Using as an input a hypothesized rate of monetary growth of 6 percent, the Laffer-Ranson equation produced a forecast for nominal GNP of $1,065 billion, implying about 9 percent growth in aggregate U.S. nominal income for 1971 on a calendar-average basis.[216] This was seen as a highly controversial forecast at the time, as the outside consensus projection implied nominal income growth of only about 6.7 percent (*Financial Times* (London), February 16, 1971).[217]

The Laffer-Ranson equation involved departures from the St. Louis framework that, as discussed below, put Laffer at odds with a key monetarist proposition. But the forecast number itself now looks unexceptionable. From today's perspective, a 9 percent prediction of nominal income growth on the basis of 6 percent M1 growth does not seem a controversial prediction for 1971 or, for that matter, for any year from 1955 to 1981. When M1 velocity was subjected to univariate time series analysis, a recurrent finding concerning its pre-1982 postwar velocity was that it had a roughly 3 to 3.5 percent annual trend growth rate—see Rasche (1987, pp. 13, 16).[218] Formally,

[215] Laffer received the offer after Robert Lucas, then at Carnegie Mellon University, had turned down the position. That position was, in essence, a continuation of the job that John P. Gould had held when Shultz was Secretary of Labor in 1969–1970. See McCallum (1999a, pp. 286–287).

[216] The 1971 *Economic Report of the President* gave a preliminary calendar-year-1970 number for nominal GNP of $976.8 billion (Council of Economic Advisers, 1971, Table C1, p. 197), so the $1,065 billion forecast advanced by Laffer and Ranson (1971a) and embraced in the 1971 *Economic Report* amounted to a 9.03 percent implied annual average nominal income growth for 1971.

[217] Accounts that discuss the controversy over this forecast—variously called the "1065 and All That" controversy (a term used, for example, by Laffer and Ranson, 1971b, on account of the forecast number, and also in homage to the satirical book on history teaching, *1066 and All That*: see Sellar and Yeatman, 1930, as well as Friedman's reference to the book, in the context of the forecasting controversy, in Instructional Dynamics Economics Cassette Tape 66, January 27, 1971) or the debate over "Laffer's Money Machine" (on account of the instant money-income relationship in the Laffer-Ranson equation)—have included Miles (1984, pp. 86–88) and Matusow (1998), as well as the books by Domitrovic (2009, 2020). None of these accounts consider Friedman's reaction in detail, however.

[218] A qualification is that this 3 percent velocity trend was maintained in the second half of 1970s only for the modern definition of M1. The pre-1980 definition of M1 had this trend quality until the mid-1970s, while the post-

(log) M1 velocity turned out to be well represented as a random walk with a drift parameter implying 3 percent annual average velocity growth. The proliferation of time-series studies in macroeconomics after the early 1970s, as well as the onset of monetary targeting, made this pattern well known. In retrospect, it is apparent that the roughly 3 percent velocity trend was likely driving the Laffer-Ranson prediction to a considerable extent. But because Laffer and Ranson were using a multivariate regression and not employing modern time-series methods, this fact was not clear at the time.

Instead, the forecast was seen as outlandish. It obtained national prominence—and generated a wide-ranging outside reaction of incredulity—after the Nixon Administration used the $1,065 billion nominal income prediction as its baseline GNP forecast in the 1971 *Economic Report of the President*. The Laffer-Ranson model was not cited by officialdom as the source of the $1,065 forecast, but the model was indicated by the administration as supporting that specific number.[219] The Laffer-Ranson prediction consequently "got a huge amount of press at the time," Laffer recalled (interview, June 10, 2013). After its issuance in early February, the $1,065 billion projection was being described as a "bitterly controversial forecast" (*The Sunday Sun* (Baltimore), February 7, 1971) and had created a storm.[220] "Everybody was very critical, as I remember it," David Ranson observed (interview, April 30, 2014). "I can't remember any support we got almost from anywhere."

Paul Samuelson was particularly outspoken in his criticism, including in a *Newsweek* column titled "Economic Snake Oil" (March 8, 1971), in material he added to a book chapter concerned with monetarism and its variants (Samuelson, 1971, pp. 13–16), and in an Instructional Dynamics cassette commentary titled "Why They Are Laughing At Arthur Laffer."[221] He drove

1980 definition of M1, which was included newer forms of checkable deposits, had this property over the whole period from the mid-1950s until the late 1970s (and until 1981). See Simpson (1980).

[219] David Ranson recalled of the model that "it turned out that that was relied upon by Shultz to a much greater degree than anyone could possibly have expected. The number, 1,065, though, didn't come out of the model. It was kind of reverse engineering. The question addressed to the model was: Can we get a nominal GNP target of 1,065—which it had already been decided would be a satisfactory result in the economy. And the model was used, I think really wrongly, to derive money supply assumptions that would be compatible with that." (David Ranson, interview, April 30, 2014.)

[220] Although the *Economic Report of the President* was dated February 1971, the administration's $1,065 billion forecast was unveiled in late January (*Minneapolis Tribune*, January 27, 1971; Instructional Dynamics Economics Cassette Tape 66, January 27, 1971).

[221] Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 70 (February 19, 1971). M. Anderson (1990, p. 147) and Domitrovic (2009, p. 11) claimed that Samuelson gave a talk with this title at the University of Chicago, with Domitrovic specifying the location of the talk as the economics department. Neither author provided a source. In fact, these accounts confused the Samuelson audiotaped commentary of February 1971, which had the title referring to Laffer, and the April 1971 Samuelson talk at the University of Chicago (which, as discussed presently, was held under the auspices of the business school, not the economics department).

his criticisms home in a talk on the current economy at the University of Chicago business school's twentieth annual business economists' conference. Samuelson's address, given during the lunchtime closing of the conference, was delivered on April 27, 1971.[222] Friedman was in the audience.[223] The Samuelson speech was reported in the press, in large part because of his criticism of the $1,065 billion forecast and of the Laffer-Ranson model (*Chicago Tribune*, April 28, 1971; *Chicago Daily News*, April 28, 1971; *Chicago Sun-Times*, April 28, 1971).[224]

Jack Gould of the business school recalled: "It was a thing called the business economists' conference. It was a conference for people who were business accountants, bank economists, and corporate economists, and others who were working in that area. I came back from Washington at the end of 1970… Sid Davidson was dean, and he asked me to run that conference. I don't know who was doing it before, maybe Sid himself, but somebody was, and he asked me to do it [now]. And I was interested in expanding the reach of that, to just attract more people. And I contacted Samuelson myself. I didn't know him very well, but I just picked up the phone and said to him, you know, 'We have this conference, and we'd love to have you as the keynote speaker,' and it turns out he said yes, which was great. I didn't fully expect that (*laughs*)—but he did. Now, he had been critical about the [administration] forecast, claiming it was too aggressive, prior to this conference. He was [already] on the public record on that. It wasn't, like, the turning point [as he had voiced the same criticism before]. So he was the speaker at the conference. And he obviously mentioned this forecast, and he talked about Art Laffer, also, at that point, in kind of a negative way." (John P. Gould, interview, March 20, 2015.)

[222] Domitrovic (2020, p. 131) states that the occasion was "probably" a business-school event—for which he does not give a specific date but which he contends occurred "[s]everal weeks" after Samuelson's audio commentary regarding Laffer. Domitrovic also conjectures that the talk was given at the business school's annual forecasting event, its economic-outlook luncheon. In fact, no speculation is needed on whether Samuelson spoke at a business-school event (he did—as was recorded in the next-day press coverage). The talk's precise date, as noted, was April 27 (and so was, in fact, given nearly ten weeks after his audio commentary). It formed no part of the business school's economic-outlook luncheon (an event held in the fall—and later, in both midyear and the fall) but, instead, was part of the agenda for the school's annual business-economists' conference (a series held each spring at that time, though later discontinued). As noted, Samuelson was the 1971 conference's lunchtime speaker. John P. Gould (interview, March 20, 2015) emphasized of the event, "That was not the forecast luncheon"—which by longstanding arrangement was held late in the year to discuss the economy's course in the following year—but, instead, was part of "a completely separate conference" series from the outlook-conference events.
[223] See Friedman's letter to James Meigs, April 16, 1971 (Milton Friedman Papers, Hoover Institution). In addition, during his visit, the economics department hosted a dinner for Samuelson at Arnold Harberger's house (John P. Gould, interview, March 20, 2015).
[224] On the previous day (April 26), Herbert Stein of the CEA had spoken at the conference, including with regard to the $1,065 billion number, which he described as the administration's target (*Chicago Tribune*, April 27, 1971).

Being still based at the CBO, Laffer was not present at this University of Chicago event.[225] But he was very aware of Samuelson's targeting him for criticism over the previous three months.[226] "It was horrifying. I mean, you know, it's like being a baby mouse, and the giant bull elephant comes and stomps, and stomps, and stomps, and stomps… Jane Samuelson, his oldest child, was [earlier] my student at Stanford, and she was my favorite, and I was her favorite. You know, I would talk to her dad when we would have dinner together at the student dining hall, my wife and I and Jane, and her classmates, a couple of her classmates. So it [the situation of being attacked in 1971 by Samuelson] was bizarre." He added, however, that Samuelson "sure as hell made me famous very quickly… I mean, he's the one who gave the speech in Chicago." (Arthur Laffer, interview, June 10, 2013.)[227]

Friedman's own reaction to the controversy was more circumspect. He had, as discussed below, strong disagreement with one key aspect of the Laffer-Ranson specification. However, as already noted, the numerical prediction of nominal GNP of the Laffer-Ranson model was not greatly out of line with the money/income relationships Friedman had studied, and Friedman indicated that the forecast might well be reached (Instructional Dynamics Economics Cassette Tape 67, February 10, 1971, and Tape 72, April 21, 1971). Alluding to Samuelson's prominence in the public discussion, Friedman also indicated that the reaction by the economics profession against the forecast had been excessively negative (Instructional Dynamics Economics Cassette Tape 72, April 21, 1971). But he added some months later (Instructional Dynamics Economics Cassette ape 79, August 11, 1971) that "Arthur Laffer has a deal of responsibility for this [backlash] himself. He's a brash young man who has done good work—and will do good work [again] in the future. But he didn't show up at his best in this episode."[228]

In this commentary and elsewhere, Friedman also was at pains to emphasize that the Laffer-Ranson specification should not be considered representative of monetarist views. The last point reflected, in part, the fact that, in the course of the backlash against the Laffer-Ranson model,

[225] Charles Nelson (personal communication, April 20, 2014), who attended the event, recalls that the dean of the business school (Sidney Davidson) was dismayed at the fact that Samuelson used the occasion to engage in such sharp criticism of an absent member of the school.

[226] As stressed in Nelson (2020b, Chapter 15), the Instructional Dynamics cassettes were not widely listened to, despite being publicly available. Laffer had heard the audio commentary, however (see Waitley and Tucker, 1989, p. 168). Indeed, it was Friedman who sent him the Samuelson cassette in question (see Domitrovic, 2020, p. 131).

[227] Notwithstanding his trenchant public criticism of Laffer over the Laffer-Ranson forecast, Samuelson evidently saw merit in some of Laffer's research, as he included Fama and Laffer (1971) in a book of readings on finance that Samuelson coedited (see Bicksler and Samuelson, 1974).

[228] Friedman expressed similar sentiments to Albert R. Cox on July 30, 1971 (Milton Friedman Papers, Hoover Institution) while indicating that he believed that Laffer and others had been too hasty and unqualified in propounding the Laffer-Ranson equation.

that model was often being interpreted as the monetarist forecast of GNP for the year: “To Chicago, I was an enormous source of embarrassment,” Laffer recalled (*Wall Street Journal*, October 8, 1981, p. 1). But it also reflected the fact that Laffer had in part advanced the Laffer-Ranson model as a *critique* of monetarist propositions concerning the lags between money and income. Laffer and Ranson had used nonseasonally adjusted data in their estimation of the nominal-money-/nominal-income relationship and, on the basis of that data, had reported an *instantaneous* relationship—not even a distributed lag starting at zero—between the series, rather than the lag from money to income that Friedman had long stressed. This finding was a major source of controversy, with the London *Financial Times* (February 16, 1971) describing the no-lag finding as “a new monetary theory proposed by a 30-year-old economist [Laffer]” and as having “caused something of a sensation.” It was indeed this issue, rather than the authors’ numerical finding, that was the major one on which Friedman parted company with Laffer. Friedman remarked publicly that the finding of no lag was “contradicted by the mass of evidence in the past.” And, privately, he was said to have tried to lower OMB director George Shultz’s enthusiasm for the Laffer-Ranson specification (*Washington Post*, February 14, 1971).[229]

[229] As discussed in Nelson (2020b, Chapter 15), the issue of whether monetary growth preceded nominal and real income growth has frequently been part of debates about whether the significance of monetary growth in predicting GNP reflects effects of monetary policy on income. Even a lag of income behind money was capable of being interpreted as consistent with no effects of monetary policy, as was stressed in Tobin’s (1970) challenge to Friedman’s work on lags and in Fand’s (1970) discussion of early critiques of the St. Louis equation. But the presence or absence of a lag was often linked to discussions of causation, and the entanglement of these two issues has also affected discussions of the Laffer-Ranson equation.

In particular, it has been suggested in retrospectives that the Laffer-Ranson equation was not really being offered as an indication of how monetary policy might be set to generate a particular GNP level, but instead as a demonstration of the endogeneity of the money stock—that money responded passively to GNP movements, irrespective of what the monetary authorities did—and as a numerical representation of the correlation between money and income, conditional on other variables (see, for example, Laffer and Canto, 1981, p. 87, and Miles, 1984, p. 89).

At the time, however, Laffer and others did subscribe to the standard view that monetary policy actions could affect M1. Some of Laffer’s work stressed the slippages between influencing the monetary base and influencing deposit-inclusive aggregates—“I thought that there were a lot of reasons why M1, M2…were *not* in the control of the government” (Arthur Laffer, interview, June 10, 2013)—while his later work also cast doubt on the usefulness of M1 and M2 as measures of the economically-relevant money series. But at the time (and, for that matter, later) Laffer also attached importance to the monetary base as a variable that reflected the actions of the central bank, and so it remained the case that the M1 series used by Laffer and Ranson (1971a, 1971b) could be interpreted as a series on which monetary policy (as summarized by the base or other variables directly susceptible to policy influence) had a substantial effect.

David Ranson also indicates that at the time of the Laffer-Ranson work at the CBO, his own inclination was to interpret M1 as a monetary-policy variable and their equation as demonstrating the dependence of macroeconomic series on monetary and fiscal policy variables (*inter alia*). “Laffer really wasn’t a monetarist—which set off quite a fault line [between him and George Shultz]. But Laffer saw the possibility of actually just doing the empirical work—and then the interpretation was up to the user. Laffer would never have interpreted the results as showing that if you create some money, it’ll produce GDP [or GNP]. I know for a fact that Laffer never believed that. I tended to believe that myself, because I’d been indoctrinated back in Chicago [as a graduate student], and that was the implication of Friedman’s thinking about money supply. And Laffer quickly converted me to thinking that money is endogenous.” (David Ranson, interview, April 30, 2014.) It should be stressed, however, that, then and

Confronted with reports of Friedman's criticism, Shultz remarked publicly: "That is Friedman's conclusion [o]n the basis of his work, but that does not prevent a young man from coming along and looking at things in a different way… [M]aybe he [Laffer] has made an advance, and maybe he has not. Time and criticism will tell."[230]

Laffer—who returned to the University of Chicago campus to present the Laffer-Ranson paper at Friedman's workshop on March 16, 1971—stuck to his view that the money-income relationship was instantaneous.[231] But the verdict of the economics profession has been overwhelmingly on Friedman's side concerning the existence of lags. The Federal Reserve Board was unimpressed by the Laffer-Ranson claim of no lag, and Chairman Burns authorized the release in April 1971 of an analysis by Federal Reserve Board staff economist James Pierce that was highly critical of the Laffer-Ranson specification (see Joint Economic Committee, U.S. Congress, 1971b, pp. 300–312, and *The Sunday Sun* (Baltimore), April 25, 1971).[232]

Friedman himself pointed out that the lag between monetary growth and nominal income growth showed up in four-quarter growth and annual data. This result seemed inconsistent with Laffer's position that the lag in the seasonally-adjusted quarterly data was an artefact of the *X*–11

later, Laffer was not a subscriber to the view that monetary policy had no effect on the economy or that monetary policy had no effect on M1, and also that he was himself quite amenable to using the monetary base as an indicator of monetary policy actions and believed that central bank actions affecting the base had repercussions for other monetary and financial series. These positions implied that he was not really expounding as nonmonetarist a position on causation as those made by hardline Keynesians like Kaldor (1970), for example. In the write-up of the Laffer-Ranson model for publication, Laffer and Ranson (1971a, p. 247; 1971b, p. 82) had passages distancing themselves from an inference about direction of influence in their equations, but they did not take a hardline anti-monetary-policy position.

[230] Testimony of February 17, 1971, in Committee on Ways and Means, U.S. House of Representatives (1971, p. 63).

[231] See Laffer and Canto (1981, pp 86–88). Laffer also affirmed his findings on the lag in the interview for this book, stating that "one of the real problems we have with money data and GDP and all that is you got a ratio-to-moving average type of seasonal adjustments. And it introduces, if you have a blip, a one-month blip, it's spread out [by seasonal adjustment]. So you really have a hard time discovering leads and lags with seasonally adjusted data. … I found that there were no lags and leads, there was a huge instantaneous correlation between money and GDP with no lags and leads." (Arthur Laffer, interview, June 10, 2013.)

The date of Laffer's workshop presentation is given in University of Chicago Library records. Robert Gordon likewise recalled that the Laffer-Ranson version of the "St. Louis equation was presented by Laffer in one of Friedman's money workshops," with Gordon among those present who were critical of the model (Robert Gordon, personal communication, March 5, 2014). Laffer also attended or heard about a presentation that David Booth (after whom the university's business school was later named) had given to the workshop one week earlier. Booth "went in the money workshop to show that money supply did not predict stock prices. And he started the seminar… [saying] 'Let the data speak for themselves.'" Laffer observed that Friedman was combative at the event: "just remember, David Booth then was not David Booth now. He was a young kid, and naïve, insecure, all those things… [In response to Booth,] Milton quoted someone—I forget, some great economist—saying, 'He who lets the data speak for themselves, listens to a fool.' And it was off to the races." (Arthur Laffer, interview, August 11, 2014.)

[232] Prior to this, it had been reported: "The economists at the Federal Reserve [Board] in Washington dismiss the Laffer theory [about lags] as absolute rubbish…" (*Financial Times* (London), February 16, 1971).

seasonal adjustment procedure that was often used in the preparation of official U.S. data.[233] Furthermore, Hamburger (1971) established that the lack of a lag from money to income that Laffer and Ranson had found resulted not from their use of non-seasonally adjusted data but from their inclusion of the Korean War in their sample period. Furthermore, in economic research since the early 1970s, the lag from monetary policy variables to real and nominal income has been heavily reaffirmed, both in VAR studies such as Christiano, Eichenbaum, and Evans (2005) and in correlation studies such as that of Belongia and Ireland (2016).

With respect to the Laffer-Ranson prediction regarding 1971's level of U.S. nominal income, the verdict of history is more complex. In the summer of 1971, incoming data led the government to abandon the $1065 billion forecast, on the grounds that the outcome would be lower than this (*Dallas Morning News*, July 9, 1971; *Newsweek*, August 2, 1971). Subsequently, of course, the Nixon Administration made an explicit *U*-turn with the New Economic Policy of August 1971. This necessarily meant the projections and strategy outlined by the administration earlier in the year were superseded; what Hester (1982, p. 233) called the "moment of notoriety" of the Laffer-Ranson model had passed. Indeed, even though Meltzer's (2009a, 2009b) historical account would cover the Nixon Administration's economic policy in the early 1970s in detail, the $1,065-billion projection receives only one mention—and its origin at the OMB only a brief footnote—and the administration's later dropping of the forecast was not referred to at all.[234]

Nevertheless, numerous postmortems have appeared over the years on the $1,065 billion projection. By the mid-1970s, revisions to GNP data had put the actual forecast close to the Laffer-Ranson forecast, and on this basis one supporter of supply-side economics has claimed that Laffer was "vindicated by history" (Domitrovic, 2009, p. 11). Samuelson (1984, p. 6), however, pointed out correctly that the appropriate criterion for judging the accuracy of the Laffer-Ranson prediction was whether the *growth-rate* prediction was accurate, and Samuelson maintained that it was not, even with the data revisions taken into account.[235]

[233] See Friedman's remarks in October 1972 in the course of the floor discussion in Selden (1975, pp. 305–307).

[234] The coverage can be found in Meltzer (2009a, pp. 629–630). (The titles of the books notwithstanding, Meltzer 2009a, actually covers domestic economic policy through mid-1971, not through the end of 1969.) In part, the sparseness of this coverage reflected the fact that, though he "went through that whole period" in the research for his book, Meltzer had the view that the Laffer-Ranson equation had received too much attention to begin with and so did not want to dwell on the equation in his own account (Allan Meltzer, interview, April 8, 2015).

[235] Krugman (1995, pp. 86–87) made the same point.

Notwithstanding his trenchant public criticism directed toward Laffer with regard to the Laffer-Ranson income forecast, Samuelson evidently saw merit in some of Laffer's research, as he included Fama and Laffer (1971) (an article that had appeared in the same issue of the *Journal of Business* as Laffer and Ranson, 1971a) in a book of readings on finance that Samuelson coedited (see Bicksler and Samuelson, 1974).

In his own retrospective on the projection, in a discussion in 1972, Friedman suggested that the Laffer-Ranson model's implied growth rate projection of 9 percent was indeed well above the actual 1971 growth rate of 7.5 percent (Council of Economic Advisers, 1972, Table B1, p. 195). But Friedman defended this error as being of the same order of magnitude as past CEA errors (Instructional Dynamics Economics Cassette Tape 99, May 17, 1972). Because the CEA had so badly underpredicted the strength of nominal spending in the late 1960s, this was perhaps only a backhanded compliment to Laffer on Friedman's part. But further revisions to GDP data have considerably reduced the size of the Laffer-Ranson forecasting error, with the nominal income (GDP) growth in 1971 being given in the 2011 Economic Report of the President as 8.5 percent (Council of Economic Advisers, 2011, Table B–1, p. 189). Nominal income growth then picked up further in 1972, so 9 percent nominal income growth was ultimately reached and surpassed. This raises the possibility that the key error in Laffer and Ranson's prediction may well have been their failure to allow for lags.

International economics

The "1065 and all that" debate now had receded, but Friedman and Laffer would find themselves on opposite sides of public debate on several occasions in the following few years, despite their shared free-market orientation. The disagreement now centered on international matters.[236]

Laffer was, over this period, strongly emphasizing what he believed was the centrality of the law of one price in aggregate U.S. price-level analysis.[237] In the period after he returned to the University of Chicago from Washington, D.C., Laffer wrote *Wall Street Journal* op-ed contributions in which he indicated that devaluations of exchange rates led to no improvement in competitiveness or the trade balance (*Wall Street Journal,* February 5, 1973) and, indeed, that the surge in U.S. inflation in 1973 could be attributed almost exclusively to the willingness of the authorities to permit a fall in the exchange rate (*Wall Street Journal*, January 14, 1974).

Reflecting Laffer's high profile on the issue, his was the first name that Obstfeld (1993, p. 231)

[236] Dialogue on domestic matters between them also continued, including at the money workshop in 1972–1976. Laffer and Richard Zecher (1975) also produced an article (which had evolved from a presentation in the money workshop on October 28, 1969) that complemented the Laffer-Ranson questioning of monetarists' evidence on lags by providing a critical analysis of Irving Fisher's empirical work on lags. "And Milton was upset at that article as well." (Arthur Laffer, interview, June 10, 2013.)

[237] This tendency is brought out in the monograph of Laffer and Miles (1982). Although this book covered events up to the late 1970s (because of revisions to the text made primarily by Marc Miles). This textbook was adapted and expanded on the basis of Laffer's lecture notes, for classes given at the University of Chicago in his spells there through 1970 and from 1972 (Arthur Laffer, interview, June 10, 2013).

gave when naming economists who believed—contrary to Friedman's exchange-rate article—that exchange-rate depreciation improved the trade and current account balances. Relatedly, Krugman (1995, pp. 95–97) would point to Laffer's 1973 *Wall Street Journal* pieces in underlining the fact that Laffer took the law of one price as almost the exclusive lens through which to look at the U.S. inflation of the 1970s.[238]

Krugman's critique of Laffer's perspective on inflation was presaged by Friedman's own public reactions to Laffer's *Wall Street Journal* contributions. Friedman penned a letter to the *Journal* (February 5, 1974) to respond to Laffer's January 10 op-ed and responded also in his cassette commentaries (such as Instructional Dynamics Economics Cassette Tape 139, February 4, 1974, and Tape 147, May 30, 1974). Noting in his letter that "Mr. Laffer tells us that relative price changes cannot be achieved via exchange rate changes," Friedman made clear that he continued to believe that they could. He elaborated on this point in his audiotaped responses, which emphasized that the existence of a considerable nontradables sector in the United States (and the sensitivity of that index to aggregate demand) meant that the overall price level could be insulated from a devaluation.[239]

This was a position that Friedman would reaffirm in the 1980s when McCloskey and Zecher (1984) contended that the *Monetary History* should have relied more on the law of one price in its analysis of U.S. inflation behavior in the interwar period.[240]

Laffer's perspective on the exchange rate-prices link made him much less favorably disposed toward floating exchange rates than Friedman, and this would be a recurrent source of conflict between the two, beginning in the early 1970s.[241] Such conflict had preceded the *Wall Street Journal* pieces. When, in late 1972, Laffer had suggested to *Newsweek* that Friedman was a major influence on Secretary of the Treasury Shultz's steps toward floating exchange rates,

[238] Krugman (1995, p. 95) refers to a "series" of Laffer *Wall Street Journal* pieces on the matter during this period. There were actually basically only two, although many other *Wall Street Journal* contributions then referenced Laffer's work. Laffer also produced a number of—fairly low-profile—research articles giving empirical evidence on the matter, including a piece originally given at a conference held by the U.S. Treasury in April 1974 (Laffer, 1977). This item would be cited by Obstfeld (1993, p. 231) (and by Laffer himself in Laffer and Miles, 1982, p. 346) as the representative research by Laffer on the matter.

[239] In principle, one could argue for the importance of monetary growth for understanding inflation while seeing the law of one price (or a depreciation-to-inflation channel) as the mechanism through which the money/prices relation is manifested. But Laffer did not propose this interpretation of his results, which he instead saw as a challenge to monetarism: "it is virtually inconceivable that excessive money growth is to blame for the almost unprecedented rates of inflation recently experienced" (*Wall Street Journal*, January 10, 1974).

[240] See Friedman (1984i).

[241] He explicitly critiqued Friedman's case for floating exchange rates in Laffer (1973) and Laffer and Miles (1982).

Friedman was irritated rather than flattered, feeling that his role had been given an inflated picture in Laffer's remarks (see Chapter 3 above). In view of the common University of Chicago affiliation and Laffer's recent return to the university, the Laffer account likely struck many readers as one authorized by Friedman, or at least informed by interaction with him—an impression that Friedman was anxious to dispel. "Art Laffer unfortunately has a propensity to shoot his mouth off when he shouldn't. And he should not have said what he said there," Friedman remarked (Instructional Dynamics Economics Cassette Tape 108, October 5, 1972).

The Gold Standard, fixed exchange rates, and monetarism

Although supply-side economists were most associated with tax prescriptions, a matter of common ground among many of them, especially those prominent in the *Wall Street Journal*, was that they favored fixed exchange rates. Partly for that reason, they were hostile to Friedman's views on monetary policy. In 1981, former *Journal* writer Jude Wanniski brought the clash into the open, including in a public letter in which he wrote (*Washington Post*, July 27, 1981): "Milton Friedman and his monetarist students... may cripple Ronald Reagan with their high interest rates[,] as they have Margaret Thatcher. The president may give Professor Friedman's ideas a few more months, for old times' sake, but monetarism will be buried too."[242]

This degree of criticism was too much for Arthur Laffer, who said: "I wish that my friends wouldn't imply that I would join in an attack on the man that I consider the best economist in the country." (*Business Week*, August 24, 1981.)[243]

Laffer's refusal to endorse the severe criticisms of Friedman being made by some supply-siders made things easier for the new stage of their relationship—the membership of the President's Economic Policy Advisory Board. "We communicated a great deal during these times, and always sat together at the PEPAB meetings, or at least almost always. And he made me take my jacket off and keep it off even with the president came in—everybody else [had jackets on]—just to show that we were deferential to intelligence, and not authority. He was quite a character." Laffer attached much importance to the downtime in the meetings, because of the more informal

[242] Wanniski had made similar criticisms of Friedman in prominent op-eds in the *Wall Street Journal*, November 11, 1980*a*, and the *New York Times*, July 26, 1981.

[243] This description of Friedman was carefully worded. Laffer actually considered Robert Mundell "probably the single best economist in the last century." But Mundell was Canadian and, although affiliated with Columbia University from the 1970s onward and teaching there, was known for spending much of each year in Continental Europe. Laffer has added that Mundell "doesn't have Milton's personality. And Milton's damn close to being as good as Bob in economics." (Arthur Laffer, interview, June 10, 2013.)

interaction it allowed with Friedman and others. "We got our value in the breaks—talking with each other. That's when things were really cool." (Arthur Laffer, interview, June 10, 2013.)

As for the Friedman-Laffer relationship in the Reagan years, both Friedman and Laffer would emphasize the solidarity between them at PEPAB meetings, particularly on the need to resist tax increases.[244] But monetary policy, including the open-economy dimension of it in the form of exchange-rate policy, remained a source of tension. Laffer would stress with regard to their respective positions that "there was no disagreement on the general framework of rules versus discretion." (Arthur Laffer, interview, June 10, 2013.) But their differences on the appropriate monetary policy rule were profound. In late 1981 Laffer was quoted saying (*Wall Street Journal*, October 8, 1981, p. 1): "Gold has always been the most important issue in my mind." He hoped that the United States would resume a peg of the gold price. After Reagan's election, Laffer had remarked: "We've gone off the gold standard three times in our history"—evidently including the end of Bretton Woods in his count—"and each time the inflationary effects have been disastrous." (*Palm Beach Post* (Florida), December 4, 1980.) Indeed, in the spring of 1981, he had predicted that Reagan would seek to go beyond the 1981 package and seek more tax cuts later—something that did not seem likely at the time, but which indeed eventuated—and that the United States would return to the gold standard under his presidency—something that certainly did not happen. (*Milwaukee Sentinel* (Wisconsin), May 11, 1981.) Laffer and Friedman therefore brought different advice to the PEBAB meeting table: Friedman urging monetary aggregate control and a continued float, and Laffer advocating a restoration of a gold price peg.

Even under the current, Volcker-managed, U.S. monetary policy arrangements, Friedman and Laffer disagreed on what was needed to achieve disinflation. As discussed in the next chapter, Friedman expressed the view in late 1980 and early 1981 that the U.S. needed to go through another period of recession in order to bring inflation down. Such a position rested, of course, on a view of the inflation/unemployment linkage in terms of short-run nonvertical, long-run vertical Phillips-curve. Laffer, who had little room for short-run price stickiness in his analysis, poured cold water on the need for a period of weak real performance: "It makes no sense at all to squeeze inflation out of the economy. You grow out of inflation." (*Milwaukee Sentinel,* May 1, 1981.)[245]

[244] Friedman and Friedman (1998, p. 392); Arthur Laffer, interview, June 10, 2013.
[245] Similarly, a few weeks later, Laffer remarked that "the only way to stop inflation is to grow faster." (*Houston Post*, May 22, 1981.)

Jerry Jordan, who sat in (as a CEA member) on the approximately four PEBAB meetings that took place from mid-1981 to mid-1982, referred to "shouting across the table… Laffer versus Milton" as something that "would occasionally happen, but you also had in the room Arthur Burns, Alan Greenspan, Charlie Walker, Bill Simon. Now those are a lot of people that could state very strong positions. Herb Stein was in there [too]." (Jerry Jordan, interview, June 5, 2013.) In the PEPAB years, Herbert Stein would be a unifying figure for Laffer and Friedman, on account of his opposite view from theirs on the importance of keeping the Reagan tax cuts. Of the other PEPAB members, Arthur Burns and William Simon would leave the panel to take full-time foreign policy posts in the administration, as would another early PEPAB member, George Shultz.

Friedman, as earlier noted, had no interest in a full-time government position. Indeed, he had earlier indicated that he was "not willing to be in any formal advisory position with any official status for a government, including my own" (*The Australian*, October 27, 1977). His PEPAB membership represented something of a break with that posture—although not a very dramatic one, because PEPAB was little more than a talking shop.[246] Friedman was proud enough of his PEPAB affiliation to devote considerable space to PEPAB in his 1998 memoirs and to describe himself, in another 1990s retrospective, as having been "known as a close adviser to Reagan" (Snowdon and Vane, 1997, p. 197).

Despite the sporadic nature of his contact with Reagan, there was substance to Friedman's description of himself as a close adviser, particularly during the president's first term. On the basis of Friedman's and Reagan's interaction at the PEPAB meetings, Jerry Jordan assessed the relationship between the two in these terms: "Very close. They had a close relationship going back to when Reagan was governor... And so great admiration, and mutual, both directions. Milton, of course, commanded a great respect and following across the Atlantic and the Pacific by then. And Reagan was certainly a big fan." (Jerry Jordan, interview, June 5, 2013.)

Paul Volcker observed of Friedman and Reagan: "They certainly [were in accord] in the free-enterprise stuff and all the rest. I don't know how close it was, but they certainly had a relationship, and I think he was certainly influential in reinforcing the instincts that Reagan already would have had about open markets and all that stuff." (Paul Volcker, interview, October 16, 2013.)

[246] For further discussion of this point, see Chapter 14.

The differing advice that Laffer and Friedman gave to Reagan on monetary matters encouraged a sense of rivalry between the two, evidenced in a Laffer remark reportedly made in 1983 when asked whether Friedman was influential on Reagan. Laffer's answer to the question was paraphrased as: "Hell, no! The president thinks Milton is a funny little man, but he can't understand a word he says." (*American Banker*, November 1, 1983, p. 1.) Against this, however, one must also consider Laffer's observation on another occasion concerning Reagan: "I don't think he listens to anyone very much. He's pretty stubborn." (*Palm Beach Post* (Florida), December 18, 1982.)

Supply-side economics

The respective views of supply-siders and Friedman on one particular issue—taxes and revenues—have already been discussed (Chapter 9 above). But what was Friedman's view of supply-side economics more generally?

As a prominent economist on the public square, Friedman was frequently asked his opinion about supply-side economics. Two typical answers he gave, both in April 1981, were ones that many economists echoed. One was: "I think the only thing new about supply-side economics is its name."[247] The second was: "Supply-side economics, properly done, is simply good economics."[248]

The reason why supply-side economics was not considered a new body of thought by Friedman and others was that it emphasized the features it emphasized—notably the impact of taxes on relative prices on private-sector behavior—were already embedded in economic theory, especially in growth economics and microeconomics. Some of the prominent supply-siders did not greatly disagree with this judgment regarding originality. Indeed, Laffer's former student and coauthor Marc Miles, although once inclined to prefer to call supply-side economics "incentive economics" (Miles, 1984, p. ix), has indicated that he now prefers simply to call it classical economics (Marc Miles, interview, February 20, 2014). And Canto and Laffer (1990, p.

[247] Friedman (1981d, p. 23). Later (*Newsweek*, July 27, 1981), Friedman put it in the following terms: " 'New' (i.e., old) theories of monetarism and supply-side economics."

[248] From his remarks in Friedman, Porter, Gruen, and Stammer (1981, p. 27). See also Friedman (1982a, p. 54). Many similar assessments can be found in the research literature. For example, Brunner and Meltzer (1993, p. 319) stated that their lectures did "not consider supply-side economics as a distinct hypothesis. The reason is that, when stripped of some exaggerated claims, supply-side economics is basic to economics." They added of supply-siders like Laffer (p. 320): "They have not provided a distinct hypothesis, and [they] are not a separate 'school.'" Likewise, Tatom (1981, p. 16) observed that the supply-side approach had been at the core of economics since Adam Smith. See also Keheler (1982).

xxv) would state that supply-side economics was “little more than a new label” for classical or neoclassical economic analysis.

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It was not only the case that supply-side economics was not really a new body of thought—it was also true that most of its proponents in the public square were not basing their positions on a major set of new theoretical or empirical contributions to the research literature. They were certainly succeeding in driving national policy debate, and Friedman reaffirmed in 1980 that the “so-called tax revolt” was a real phenomenon in U.S. public discourse.[249] One of the key supply-siders, Arthur Laffer, had indeed figured prominently in that revolt, ultimately exceeding Friedman’s own profile in it—and had research publications to his credit. But the tax revolt and the public-policy supply-side movement had not stemmed primarily from a body of research output, even though some prominent supply-siders, Laffer among them, had economic-research backgrounds.

The fact that the supply-side drive that was being covered in the media in the early 1980s and that was influential on the Reagan Administration did not originate from a prior body of research activity on the part of its proponents contrasted with the case of monetarism. Like supply-side economics, the monetarist movement had some advocates who came from the media, the financial markets, or business, rather than from research institutions. Nevertheless, the public debate on the monetarist side of the Keynesianism-monetarism debate (and, for that matter, the monetarism-vs.-supply-side-economics debate that was emerging in 1980–1981) prominently featured figures such as Friedman, Karl Brunner, and Allan Meltzer, who had also been major contributors to the monetarist research literature. On this dimension, supply-side economics really was different from both Keynesianism and monetarism. In this connection, Robert Lucas (1980, p. 204), writing in 1978, emphasized that the Laffer curve, although rationalizable in terms of a “theory of sorts,” owed its prominence in recent policy discussions to developments and debates separate from those in the economic-research literature. Lucas made a related, but broader-ranging, remark in 1986: “Supply-side economics never really existed as a body of academic work or research. To me, that was a theory exhorted on the editorial pages of the *Wall Street Journal*.” (*Newsday* (Long Island, New York), October 19, 1986.)

[249] Friedman and Samuelson (1980, p. 13). And when, in April 1981, it was suggested to him that the Carter Administration’s resistance to the movement associated with Proposition 13 might be grounds for doubting the merits of the movement, Friedman replied: “Where is the Carter Administration now?” (*Nationwide*, ABC, April 9, 1981.)

It was nevertheless true that there *was* a line of professional economic research on supply-side economics during the 1970s that continued into the early 1980s—and it was one that often focused on taxes and incentives. In this vein, Woodford (1999a, p. 28) would distinguish "the resurgence of emphasis upon the supply side among academic macroeconomists" from the public debate on supply-side economics that occurred during the same period.[250] And when, in 1990, Lucas wrote an article that provided analytical support for major reductions in tax rates on capital and capital gains, he did so by citing studies of taxation that had been contributed to the research literature by economists who had not been part of the public supply-side movement associated with Arthur Laffer. These researchers, Lucas indicated, comprised the "supply-side economists" whom he had found persuasive.[251]

Among those whom Lucas included in this list of bibliographical references was Martin Feldstein. Feldstein, who had communicated his research in public-policy forums in the 1970s as well as academic venues, had been described in one newspaper analysis as "cut out of the same cloth" as Milton Friedman (*Tucson Citizen* (Arizona), November 29, 1977). There was considerable truth in this assessment, as will be shown below. This was so despite the fact that many other economic researchers in the 1970s overlapped more closely with Friedman than Feldstein did in their specific research interests. Feldstein did not really emulate Friedman in the choice of topics on which to focus. Unlike Lucas—who had built on Friedman's work on

[250] A book of readings by two supply-siders (Bartlett and Roth, 1983) made some effort to encompass work by the supply-siders prominent in public debate and that done in the research literature, as it reprinted articles by both sets of authors. So the readings included not only articles by Michael Boskin and Jerry Hausman, but also pieces by the more public-policy-oriented figures like Laffer, Norman Ture, and Paul Craig Roberts. The authors' inclusion of Boskin's (1981) article, however, only underscored how little ground the latter group of supply-siders made in the area of research-journal-oriented work, as Boskin's paper, "Some Issues in 'Supply-Side' Economics," cited Martin Feldstein, Friedman, Robert Hall, Lucas, and George Stiglitz, but not Laffer.

[251] The opposite contention is conveyed in the following statement by Stephen Moore (2010): "The triumph of supply-side policies seems hard to refute in retrospect. As Robert Lucas, the Nobel Prize-winning macroeconomist, initially skeptical of supply-side economics, wrote in 1990, 'The supply-side economists have delivered the largest genuine free lunch I have seen in 25 years in the business, and I believe we would be better off if we listened to their advice.'" Without so indicating (for example. by ellipsis points), Moore's quotation of Lucas compresses the actual passage from Lucas (1990, p. 314), which was: "The supply-side economists, if that is the right term for those whose research I have been discussing, have delivered the largest genuinely free lunch I have seen in 25 years in this business, and I believe we would have a better society if we followed their advice I have seen in 25 years in this business, and I believe we would have a better society if we followed their advice." The omitted passage and Lucas' reference list indicated that the supply-side economists to whom Lucas was referring were contributors to the research literature and did not include Laffer or other public supply-siders. That Lucas came round to greater agreement with the supply-side movement on its policy recommendations evidently did not entail a revised view of the economic contributions of Laffer *et al.*, but, instead, absorption of more mainstream research on tax policies as well as his own research. In addition, the initial skepticism Lucas had about supply-side economics to which Moore refers amounted to questioning whether tax cuts would raise revenue. Lucas did not criticize a renewed emphasis on economic policy on incentives, which he characterized as a breath of fresh air (*New York Times*, April 13, 1981). And well before then, Lucas and Rapping (1969) had emphasized the response of labor supply to real wage signals.

monetary policy and the Phillips curve—and even Laffer—who was known to use Friedman's analyses of exchange rates and monetary policy as jumping-off points in expounding his own perspective on these matters—Feldstein was most often engaged in research on subjects that did not have a major intersection with the main ones on which Friedman had written. The Feldstein article that Lucas (1990) had cited, "The Welfare Cost of Capital Income Taxation" (Feldstein, 1978), was a case in point. Although the topics of the capital market and of taxation had occupied Friedman in the 1940s, and he had continued to cover them in his teaching and policy discussions, his research in the decades since had largely been concerned with other matters.

A research conference that Feldstein organized at the very start of the 1980s was sufficiently broad-ranging—being concerned with the postwar development of the American economy, with an emphasis on the question "can the poor economic performance of the 1970s be reversed?" (Feldstein, 1980b, p. 7)—for both Friedman's and Feldstein's interests to be covered, and Friedman accepted Feldstein's invitation to participate. This National Bureau of Economic Research conference (which also marked the organization's sixtieth anniversary) was held in Key Biscayne, Florida, on January 3–4, 1980.[252] Friedman, who used his main remarks at the conference to highlight the distortions to the financial system created by the U.S. government's deployment over the years of foreign exchange controls and ceilings on deposit interest rates, attended the day-and-a-half event right before he and Rose Friedman flew to the northeast United States to promote *Free To Choose*.[253]

Feldstein's research agenda

By the time of this conference, Feldstein had a longstanding familiarity with Friedman's work. Although he had gone outside the United States—to Oxford University—for his graduate studies in the mid-1960s, Feldstein had occasion to study Friedman's writings closely while working on his Ph.D. "In Oxford, I read various things by Friedman, and the thing that, looking back, struck me as probably most important to me was his consumption-function book… [and] seeing an example of good econometric work like Friedman's [*Theory of the*] *Consumption Function*. And of course, I'd heard about his basic results when I was an undergraduate at Harvard—but

[252] See Feldstein (1980a, p. vii), National Bureau of Economic Research (1980, p. 12), and Robert Gordon's photographs of the event (such as *https://gordon.economics.northwestern.edu/antiques-1978-94/feldstein-nber-conference-american-economy-in-transition-key-biscayne-fl-january-1980-david-packard-and-milton-friedman/*). In National Bureau of Economic Research (1980, p. 12), the conference title was given as "Postwar Changes in the American Economy," with the session at which Friedman was a speaker listed seventh among the nine agenda items. The published volume was, however, titled *The American Economy in Transition*, with Friedman's contribution appearing in the first chapter.

[253] See Friedman (1980d) for his main contribution to the conference.

actually reading the book and seeing how he used econometrics was really eye-opening." The subject matter of the book also served as a catalyst for Feldstein. "First, I was super impressed. Here's a very careful study showing the drivers of saving. And then I realized, 'Gee, what was missing in all this?,' because by this point I was beginning to think about, and teach in Oxford regarding, public finance, I realized that what was missing in this was Social Security… But it led me to say, well, what if we modify the Friedman *et al.* consumption analysis to include Social Security, or what I called 'Social Security Wealth,' as another driver of the savings decision? So it was an attempt to extend and further test Friedman that led me to the Social Security Wealth idea." (Martin Feldstein, interview, November 21, 2013.)

The research using this idea (Feldstein, 1974a) concerned the impact that the Social Security system had on households' incentives to save out of their disposable income. It was one of a variety of studies Feldstein made during the 1970s on how tax-and-transfer arrangements affected private-sector incentives. Over the course of the decade, Feldstein was a leading figure in developing an analysis of fiscal policy actions that emphasized on supply-side effects, whose importance had previously been downplayed by the Keynesian revolution's emphasis on the repercussions for aggregate demand of these actions. The advent of high inflation made the supply-side focus become more prevalent: "it was the increase in the rate of inflation that I think shook everybody up and caused people to realize that traditional views were no longer viable." (Martin Feldstein, interview, November 21, 2013.)

Feldstein, like Friedman, stressed that inflation had raised real tax rates. Friedman wrote of "the unlegislated rise in taxes produced by inflation."[254] And Feldstein (1981a, p. 164) specifically noted: "Investors in stocks and bonds now pay tax rates of nearly 100 percent—and in many cases more than 100 percent—on their real returns. This change has taken place without public debate and even without legislative action."

Saving and investment

Some of Feldstein's work on the private sector's behavior in response to the incentives and disincentives associated with fiscal initiatives pertained to the supply of labor—most notably his research, discussed in Chapter 5 above, on the effect of unemployment insurance. His primary concern, however, was on the effect on capital formation. Feldstein used the term "capital formation" to cover saving decisions and not just investment—and occasionally used "capital

[254] Friedman (1978c, p. 7), also in Friedman (1978b, p. 14).

formation" or "capital accumulation" as simply meaning saving, while taking for granted that tax measures that depressed U.S. saving would depress business investment spending as well, by depriving firms of resources with which to finance their expansion of plant and equipment.[255] Friedman, too, had long taken it as axiomatic that measures that altered aggregate saving in the United States would move investment spending in the same direction and that a major increase in the latter series would not occur without an increase in saving (see Nelson, 2020a, Chapter 5).

As the 1980s began, both Feldstein and Friedman therefore saw the road to higher U.S. business investment spending as lying largely in developments that encouraged higher U.S. saving rates.[256] In Friedman's case, he believed that, quite apart from enhancements to aggregate saving, disinflation and tax changes would channel existing saving into business investment—his belief being that much saving by Americans as of 1980 was taking place in outlets that could not be intermediated to businesses to finance their operations (*Newsweek*, September 8, 1980, p. 69). In the event, however, although—as discussed below—investment did rise in the 1980s, U.S. national saving apparently hardly did so at all. During the 1980s, the U.S. saving rate fell or was flat in the 1980s, according to standard metrics, despite cuts in personal taxes.[257]

The fact that investment went up in the 1980s without a corresponding rise in saving reflected the advent of a sustained phenomenon that Friedman and Feldstein had not expected: an opening up and distinct widening of the U.S. current account deficit. Both of them had been accustomed to the postwar pattern of low U.S. current-account deficits and close alignment of national investment and saving rates. Indeed, in Feldstein's case he produced a classic paper at the start of the decade (Feldstein and Horioka, 1980) that argued that not only for the United States, but also for most other countries, it was to be expected that each increment of new investment would largely come from increases on the margin in home saving. But although Friedman, in common with Feldstein, did not anticipate the emergence in the early 1980s of major U.S. current account deficits, he welcomed it. In particular, Friedman viewed the associated major net capital inflow as a benefit to U.S. capital formation and employment (see Chapter 15 below).

Consequently, much of Feldstein's analysis in the 1970s had focused on the disincentives to private saving activity as a reason for low U.S. investment. As it transpired, lower inflation and tax cuts in the 1980s had not led to higher saving, but investment had increased anyway.

[255] See, for example, Feldstein (1977a, p. 188 of 1978 reprint),

[256] This view would be challenged by Summers (1981).

[257] Modigliani (1988, p. 418) noted that a move up in the U.S. investment share while the U.S. saving rate did not rise was a development in the 1980s that represented a break with the typical historical pattern.

Friedman and others would point to respects in which saving in the 1980s was likely being understated (see Chapter 17). Nevertheless, the fact that saving had not obviously risen in the 1980s seemingly undercut somewhat Feldstein's arguments regarding the effect of taxes on capital formation. But Feldstein could point to the fact that in addition to stressing the adverse effects that the tax system had on saving, he had also emphasized that taxes were also hurting investment directly as well. For example, in August 1977 he had stressed both sides of the market for capital in remarking: "During the last decade, effective tax rates have increased dramatically on capital gains, on interest income, and on the direct returns to investment in plant and equipment."[258]

In pointing toward the adverse effect of taxes on investment spending, Feldstein had support from Paul Volcker. Volcker was often a skeptic regarding tax cuts, and in the years leading up to the Reagan tax cut had publicly opposed a general reduction in taxes. But he had testified in 1979 that "taxes that are placed on investment seem to me pretty heavy."[259]

The Reagan tax cuts of the early 1980s included measures directed specifically at encouraging business investment spending. It was estimated that the 1981 tax legislation, in conjunction with 1982 modifications, would reduce the effective marginal tax rate facing firms with regard to their plant and equipment operations would decline from 32.8 percent in 1973–1981 to 15.8 percent. Pointing to these estimates, Kendrick (1984, p. 35) stated: "This should increase the investment share of GNP." Milton Friedman's own brief retrospectives on the economic effects of the Reagan tax cuts implied that they did, indeed, boost business investment (*Wall Street Journal,* December 14, 1988). In modern data, the U.S. gross private domestic investment share does show a rise—but not an impressive one: 18.84 percent of GDP in 1984–1988, versus 18.74 percent in 1979–1983.[260] Proponents of the Reagan tax cut, including Paul Craig Roberts, would stress, however, that once the decline in the relative price of investment goods was taken into account, the rise in the investment share was sharper, with Roberts claiming (*Business Week*, August 21, 1989): "In real terms, gross investment as a share of GNP reached a postwar high in

[258] Remarks of August 30, 1977, in Feldstein (1981a, p. 164).

[259] Testimony of November 13, 1979, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1980a, p. 35). This remark came at a time when Volcker was voicing opposition to proposals for general tax cuts. On CBS' *Face the Nation*, he had remarked: "There are good, basic reasons for having a tax cut. But the time [for it] is not right now—when we have the extent of inflationary pressures that we have." (*American Banker*, September 27, 1979.)

[260] Calculated using "Shares of gross domestic product: Gross private domestic investment, Percent, Annual, Not Seasonally Adjusted," available in the Federal Reserve Bank of St. Louis' FRED portal (*https://fred.stlouisfed.org/series/A006RE1A156NBEA*).

the 1980s."[261] In the area of investment, therefore, the importance of the arguments Feldstein had advanced in the 1970s seemed to have been largely borne out by developments during the 1980s.

As has been indicated, Feldstein was mainly concerned in his research with the interaction of inflation and U.S. tax policy, together with the repercussions for private-sector decisions, rather than the reasons why high inflation had come about. He would, however, make clear in his public commentary that he viewed the 1970s inflation as having reflected faulty advice to policymakers given by economists. In the 1980s, Feldstein was quoted as observing: "It was not events but ideas that propelled us towards the increasing inflation rate." He elaborated: "The upward drift in inflation was the result of a fundamental set of beliefs about the economy and macroeconomic policy that were shared by economists and policy officials." (*Financial Times* (London), July 24, 1986.) This way of putting the matter left open what were the specific fallacious theories that underlay policymakers' actions in the 1970s. But Feldstein was clear in indicating that the move away from these theories had two key effects: it drove the profession toward a modeling of inflation much closer to those laid out by Friedman, and there was a parallel change at the policy level—reflected in both the Volcker Federal Reserve and the Reagan Administration—overturning previous perspectives in officialdom regarding inflation in favor of one oriented on a monetary explanation.

Feldstein further assessed, accurately, that a restrictive monetary policy could, even against a background of tax cuts and higher defense spending, succeed in bringing inflation to much lower levels by mid-1982, by instilling "a period of sustained slack." Once this disinflation was secured, he predicted correctly, it could be followed by a noninflationary economic expansion (*Financial Times* (London), December 31, 1980).

Teaching at Harvard University

The different outlook that Feldstein brought toward macroeconomic issues from that associated with Keynesianism left its mark on his teaching at Harvard University. At the end of the 1960s, Harvard University's economics department made a set of new appointments in order to revitalize the department (Poterba, 2003, pp. 297–298).[262] Feldstein joined the department in

[261] Real investment ratios also exhibited historically high values when purchases of consumer durables were included in the investment definition (see Barro, 1989, p. 50). But these purchases did not comprise the type of capital formation—business investment spending—that the administration sought to boost via the investment-oriented portion of the 1981 tax cuts.

[262] This revitalization occurred after Harvard University's economics department had, by 1968–1969, "become a somewhat troubled institution," in the words of Allison (1984, p. 543). Baumol and Blinder (1985, p. 234)

1968 (American Economic Association, 1981, p. 149), and among those joining around 1969 in senior positions were Kenneth Arrow, Zwi Griliches, and Dale Jorgenson.[263]

Feldstein, who had, as indicated, been much influenced by Friedman's work on consumption and was also following Friedman's monetary research, taught the university's first-year graduate macroeconomics course in the early 1970s. "Indeed, my co-teacher was Janet Yellen, who had just arrived from Yale, bringing with her beautiful notes on Jim Tobin's graduate macro course. So we were [teaching] a strange hybrid of sort of traditional Keynesian-type macroeconomics and these new ideas that were coming along that were really quite different." (Martin Feldstein, interview, November 21, 2013.)[264]

Lawrence Summers, who followed his undergraduate years at MIT in the early 1970s with graduate studies at Harvard University, has indicated that his views on Friedman changed markedly during his years at the latter institution (*New York Times*, November 19, 2006). Asked to describe the main factors leading to this revised assessment, Summers gave pride of place to "the experience of getting Marty Feldstein's perspective on macroeconomics. Learning about the accelerationist Phillips curve—the natural rate hypothesis, which I had been taught as an undergraduate at MIT was a fantasy—[and] seeing that become the conventional wisdom by the time I left graduate school was probably the single most important thing. And Marty Feldstein's influence, in emphasizing incentive effects, in emphasizing a range of perverse consequences of government programs and the like, was probably the other thing." (Lawrence Summers, interview, November 22, 2013.)

Heading the NBER

The exposure that Summers had to Feldstein would not have happened if the University of

identified Cambridge, Massachusetts, as one of the centers of activity in "the height of the radical student movement of the late 1960s," and in that period the turmoil spilled over into the economics department of Harvard University, with dissension among department members that partly stemmed from the same issues that were driving the student protests of the time (Murray Milgate, interview, January 22, 2015). Of the era that preceded his joining the department in 1969, Dale Jorgenson remarked (interview, September 12, 2014): "Well, you know, I can't really say anything about that that would sound great if you played it back (*laughs*). I mean, it's just something that's awfully hard for me to say [something about]."

[263] The arrivals of Griliches (from the University of Chicago) and Jorgenson (from the University of California, Berkeley) received coverage in the *Sunday Herald Traveler* (Boston), May 11, 1969. In Feldstein (1977a, p. 178 of 1978 reprint), Feldstein was said to have been "Professor of Economics at Harvard [University] since 1960." This backdating may have been on the basis of Feldstein having been employed by the economics department in a teaching position when he was an undergraduate student.

[264] Janet Yellen, upon receiving a Ph.D. from Yale University in 1971, served as an assistant professor at Harvard University from 1971 to 1976 (American Economic Association, 1981, pp. 449–450).

Chicago's economics department had been successful in an attempt in the mid-1970s to recruit Feldstein from Harvard University. Friedman had been involved in that recruiting attempt.[265] Despite its failure, Friedman and Feldstein would soon have a shared affiliation, as, in 1977, Feldstein became president of the National Bureau of Economic Research—the organization with which Friedman was still a research associate.

As of 1977, Feldstein recalled, "the institution had really fallen on pretty bad times… [and] there was a general [reaction of:] 'Marty, why are you, you know, getting involved with that rather sick organization?'" The NBER had been involved in organizing influential conferences in the 1970s, including those covering the work on human capital and the economics of the family primarily associated with Gary Becker. But its function in these instances had been one of assembling researchers, many of whose work had not been produced under NBER auspices. Its period of generating in-house research product remotely comparable in impact to the Friedman-Schwartz *Monetary History* was gone.

Recognizing this reality, Feldstein did not try to revive the Burns-era tradition of commissioned NBER research output. Instead, he shook the organization up by making it a more truly national entity but serving the role of a network. Previously, the NBER had been a small outfit that assigned specific research projects among its staff and affiliates. When Friedman wrote an article for the Bureau's *Annual Report* in 1964 titled "The Monetary Studies of the National Bureau," he was discussing work he had authored (including the joint work with Anna Schwartz) or been closely involved in, in an oversight capacity (Phillip Cagan's work on the money supply process).[266] In contrast, in the new NBER, the Bureau became a vehicle for convening conferences, and issuing working papers of, economists at U.S. universities undertaking self-directed research.

The *Washington Post* (January 4, 1981, p. A14) was therefore both behind the times and off base when a critical news article in its pages stated: "For most of its life, the 60-year-old National Bureau of Economic Research in Boston [sic] was a respectable Establishment economic think tank. But in the last few years its new president, Martin S. Feldstein, has turned it into a forum for airing controversial ideas, such as the concept that Social Security withholding siphons

[265] Later, the elements of similarity in the names and initials of Friedman and Feldstein would on occasion lead to the two being confused for one another (for example, *Baltimore Sun*, March 9, 1984, described Friedman as the current CEA chair). Their affiliations would also be confused over the years, and one printed list of macroeconomic experts included the following: "Otto Eckstein, Harvard; Paul Samuelson, M.I.T.; Milton Friedman, Harvard; Martin Feldstein, Harvard" (Committee on the Judiciary, U.S. House of Representatives, 1980, p. 243).
[266] See Friedman (1964a).

money out of private savings and productive investment." The implication was that Feldstein was transforming the NBER into an organization driven by a specific policy agenda along Reagan-Republican or free-market-focused lines. But Feldstein did not actually exercise much direct control over the research produced by the NBER, let alone the policy conclusions springing from that research. And his changes to the NBER had the effect of making him less able to exercise such personal control, even if he had wanted to do so.

Indeed, the *Washington Post*'s casual error of mistaking Boston for Cambridge, Massachusetts was revealing about the misguided nature of the article's interpretation of the Feldstein NBER. Feldstein's changes brought many Cambridge-based economists affiliated with Harvard University and MIT into the organization—hardly a move likely to turn the NBER into a Republican "think tank." And many of the economic researchers on whom Feldstein would confer NBER membership in the late 1970s and the 1980s would work for future Democratic administrations (or receive appointments from those administrations to government agencies, such as the Federal Reserve Board).

One effect of the change in NBER organization in the direction of decentralization was that the one-time perception that Milton Friedman was a leading researcher at the NBER—the face of its work on monetary matters—was dissipated. Instances like that in 1969, when Paul Samuelson (1969, p. 9) referred to "Professor Friedman at the National Bureau," were now much less likely in the Feldstein regime. Likewise, whereas, in 1970, James Tobin naturally treated the organization's work on money as enmeshed with Friedman, referring to "the work of Friedman and his associates at the National Bureau of Economic Research" (Tobin, 1970, p. 302), in the 1980s, in contrast, the NBER was much more of a profession-wide organization, no longer associated with a specific approach to monetary economics. Indeed, during the Feldstein era Tobin himself served, from 1981 to 1986, as Yale University's representative on the NBER Board of Directors (National Bureau of Economic Research, 1986, p. 14).

A Christmas tribute

Although he was no longer very active in NBER circles by the end of 1980, Friedman continued to be a prominent voice of economics made in the profession and in the media. This prompted Feldstein to put pen to paper on the subject—via a poem. Feldstein read his composition, which was titled " 'Twas a Night in the Sixties," at the Harvard University economics department student-faculty 1980 Christmas party. The poem—discussed in Nelson (2020b, Introduction)—went through a catalogue of issues—including money, the Phillips curve, and the Fisher effect—

on which Friedman's position had been a maverick one in the early 1960s but had become prevalent among economists by 1980. It concluded with a reference to *Free To Choose*.

Recalling the poem, Feldstein said: "It was a spur-of-the-moment thing. I literally wrote it out the first time over dinner before going to the student-faculty Christmas party. And it was certainly well received as entertainment." Feldstein's poem was published as a novelty item in the back pages of the December 1981 issue of the *Journal of Political Economy* (Feldstein, 1981b).

The admiration was reciprocated. Anna Schwartz remarked of Friedman's own high regard for Feldstein (interview, April 22, 2003): "I'm sure that Friedman was asked whether Marty Feldstein was the right choice to be the head of the Bureau, and I'm sure he said: 'Yes, he's a good man.'" Furthermore, by the start of 1981 Friedman was on record listing Feldstein, who had already won the John Bates Clark medal (in 1977), among those he expected would be future winners of the Nobel economics award.[267]

The year 1981 would, however, see developments that would put Friedman and Feldstein on opposite ends on a public-policy matter.

Research funding

1981 saw Friedman and Feldstein take different sides in an acrimonious dispute over a Reagan Administration proposal: government funding for research. During the Reagan Administration's search for domestic federal spending reductions, it laid out proposed cuts to National Science Foundation (NSF) research grants. In these proposals, the social-science portion of the NSF budget allocations, including that allotted to economics, would receive a disproportionately large reduction of 75 percent (*New York Times*, April 13, 1981).

"I spent my life in the university, I'm not against universities, God knows," Friedman would observe in 1984 (*Donahue*, NBC, April 25, 1984). But his policy proposals would, in their implications for financing universities, not infrequently be at variance with those advocated by his colleagues in academia. That proved to be true on the issue of public-sector funding of research done at universities. Friedman had downplayed the importance of government financing of research for years, stressing in *Capitalism and Freedom* that major inventions had

[267] See Chapter 6 above.

arisen from privately-financed research.[268] He had parted company in early 1974 with Morris Adelman on whether the energy crisis brought out the need for increased government finance of research and development in the energy area.[269] His view that it did not lined up with the emphasis he had put in a 1970 appearance in which he suggested that research suited the market system: "I regard as one of the great achievements of market arrangements, of voluntary arrangements, the great developments in science and knowledge. Ask: How did we get to learn as much as we do about physics and biology and chemistry? Was there some central, governmental board that was assigning tasks to people? No… [I]t came about through the voluntary cooperation of scientists who had similar objectives, who worked with one another as they wished, nobody was forced to engage in it. And, in the process, they produced tremendous change." (*NET Journal Presents Conservative Viewpoint*, WTTW Channel 11 Chicago, May 4, 1970.)

The consensus of economists on the matter had been crystalized in the conclusion stated in Samuelson's textbook (1970, p. 800) that government support of research, through grants and tax concessions directed toward such activity, was necessary because of an externality that implied that the private sector tended to underinvest in research. Friedman's contrarian perspective was one he voiced at the January 1980 NBER conference that Feldstein organized. The market had a longer time horizon than the government, he suggested, so no "government role was appropriate for research projects [having] long lead times," and there should be no tax credits to businesses on this account.[270]

This remark was made mainly in reference to firms' in-house research for product development, rather than the exchange and dissemination of ideas associated with research done at universities. But the dispute over the Reagan Administration's planned NSF cuts allowed Friedman to highlight the fact that he also opposed government support of universities' research. In their *Free To Choose* book, the Friedmans had already stated: "We believe that the National Science Foundation, the National Foundation for the Humanities, and tax subsidies to higher education are all undesirable and should be terminated."[271] Friedman's profile on the subject was raised by

[268] See Friedman (1962a, pp. 3–4). Friedman made similar comments in his interview with Phil Donahue on the NBC *Today* show (September 28, 1979).
[269] *Long-Term Energy Crisis Solutions*, January 24, 1974.
[270] From the summary of the conference discussion in Feldstein (1980a, p. 612).
[271] Friedman and Friedman (1980a, p. 68). Friedman would also call for the abolition of the NSF in Friedman and Samuelson (1980, p. 32). He had previously challenged public funding for research, including social science research, in *Milton Friedman Speaks*, Episode 4, "The Role of Government in a Free Society," February 9, 1978, p. 19 of transcript, and *Milton Friedman Speaks*, Episode 15, "The Future of Our Free Society," February 21, 1978, p.

an interview that he had with *Science* magazine (October 3, 1980). In the interview, Milton Friedman that funding of university research "extract[s] money from the low-income taxpayer."

These statements positioned Friedman in a different corner from that occupied by other leading economists when the Reagan Administration unveiled its NSF proposals. Feldstein was reported to be "outraged" by the proposed cuts to economic research (*Wall Street Journal*, March 27, 1981). He felt limited, however, in his scope to engage on the matter publicly. "I remember being asked by people in the economics profession if I would speak out against the idea of cutting it. And I was in an awkward position because, by then, I was running the NBER, and so I'd be talking about what was a major source of funding for the NBER. So, on the one hand, I was in favor of shrinking the size of government and then [on the other, would seem to be] trying to defend my little piece of the budget." He was also constrained by the NBER's tradition of not making policy recommendations: "certainly we couldn't" take an official position on the matter of the administration's proposed cuts (Martin Feldstein, interview, November 21, 2013).

Instead, Feldstein sent a memorandum to NBER researchers—by now a very considerable number of academic economists in the country, thanks to his reforms to the institution—indicating that the NBER could not take an official position on the cuts, but strongly encouraging NBER members to speak out in their individual capacities against the suggested NSF cuts.
By this point, Robert Lucas was already one academic economist stirred to react publicly against the administration proposals. Lucas stated that the cuts were "a big mistake" (*Business Week*, April 6, 1981). Lucas elaborated on his concerns in a *New York Times* op-ed (April 13, 1981). He noted that his own research had, since 1964, been facilitated by NSF grants.[272] Lucas added a

6 of transcript. In *The Energy Crisis*, January 24, 1974, Friedman had also indicated that he did not support the favorable tax treatment given by the federal government to research-supporting private foundations.

[272] In a letter to Robert Gordon during this period (June 1, 1981, and available in the Milton Friedman Papers, Hoover Institution), Friedman contended (in a rare, if indirect, acknowledgment of Lucas' 1972 paper in the *Journal of Economic Theory*) that much of Lucas' work had been theoretical and so needed little funding (with Friedman incorrectly asserting that the underlying research projects had not drawn on NSF grants). Friedman reasoned that research that obtained mathematical results analytically did not rely much on research grants, as opposed to research that involved data collection and econometric estimation, as the latter set of work, being numerical, might involve more computer time and field work. Quite apart, however, from the fact that analytical calculations in mathematics can involve computer time, Friedman's observation reflected his lack of knowledge of Lucas' decade-plus away from the University of Chicago (1963 to 1974). During that period, Lucas' research output had partly arisen from a process of taking advanced-mathematics classes at Carnegie Mellon University and having some of his teaching load relieved. Research grants provide one means of reducing the teaching load of the recipient, by "buying out" teaching time.

During Lucas' years away, the Department of Economics at the University of Chicago had also benefited from NSF grants. For example, the acknowledgments section of the Friedman-supervised Ph.D. dissertation by Warren Coats stated (Coats, 1972, p. ii): "I was provided with grants for computer time from an NSF fund at the University of Chicago."

point that reflected his background as an ex-business school teacher: in his view, social-science research had boosted U.S. firms' productivity, in part through economic research percolating into the syllabus material used in MBA courses.

Friedman's own reaction to the administration's proposals, as elaborated in a *Newsweek* column (May 18, 1981) was that the proposals' disproportionate targeting of cuts at economic research in the proposals was indeed unwarranted, but his solution was simply that the cuts be made proportionate, not by moderating the reduction in grants to social science, but by cutting non-social science funds further. Furthermore, he added, in his ideal world the NSF would be abolished outright.[273] He argued (as he had in the Friedmans' *Science* interview) that grants might orient research toward areas different from those researchers might otherwise prefer doing.

Friedman predicted in his column that his fellow economists would not take kindly to the position he was taking, and they did not. Lucas was one of those who challenged Friedman on the matter. Lucas particularly took exception to a remark of Friedman's he had seen in which Friedman suggested that a truly motivated researcher would not need NSF grants to proceed with their research. "I can remember (*laughter*) [saying to him]: 'I thought you knew the supply curve sloped up.' When he said that a scholar is going to be driven by his own dedication and is not going to worry about funding—come on! …[W]hen he said it wouldn't make any difference, I think he was going a little too far." (Robert Lucas, interview, March 12, 2013.)

In the event, allotment of the NSF budget to social-science research did see a cut in fiscal-year 1982—but one far less sizable than what the administration had proposed (see Larsen, 1992, pp. 170−171).

Friedman leaves the NBER

It can be questioned whether by 1981 Friedman was in a strong position to make judgments about research-funding matters, as he had curtailed his own scientific activities. During 1980 and 1981, however, he and Schwartz were involved in a major research undertaking: finishing their *Monetary Trends* book for the NBER. The book went to press in 1981.[274]

[273] Little wonder, then, that in the acknowledgments section of the poem that he published that paid tribute to Friedman, Feldstein noted wryly that the NSF had not been a source of "financial support for this project" (Feldstein, 1981b, p. 1266).
[274] See Friedman and Schwartz (1991, p. 39).

Feldstein had not been following the work on the monetary-trends project closely: "I really didn't have much knowledge of what they were doing on that project at that time." (Martin Feldstein, interview, November 21, 2013.) But he judged that the completion, after more than three decades, of the Friedman-Schwartz monetary project was a logical point at which Friedman's affiliation with the NBER should end.[275]

In 1981, therefore, Feldstein let Friedman know that the latter would no longer be an NBER research associate. "I think Marty Feldstein didn't handle Friedman's separation from the Bureau in a very diplomatic way," Anna Schwartz recalled (interview, April 22, 2003). "I think Friedman really resented the way Feldstein said to him: 'OK, the book is out [to press]: your association with the Bureau now has come to an end. If he had said, you know, 'We would welcome any kind of further research you would be interested in doing,' it might have turned out differently."

A surface gloss on Friedman's departure from the NBER was provided by it being presented officially as his retirement. Friedman was also given a Research Associate Emeritus title by the NBER. But he rarely used it, and he would give his years at the NBER as having ended in 1981.[276]

[275] The writing on the wall regarding this move had been provided in the fact that the periodical *NBER Reporter* had not been including Friedman for some time in its listings of the organization's research associates.

[276] See Europa Publications (1986, p. 522).

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 12: Debates on Monetary Policy and Macroeconomic Stabilization, 1980 to 1981

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I. EVENTS AND ACTIVITIES RELATED TO DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1980–1981

In his acceptance speech on receiving the Democratic party nomination for president in August 1980, President Jimmy Carter referred to his already-nominated opponent, Ronald Reagan, as being immersed in "a world of tinsel and make-believe."[2] This reference seized on the preexisting perception that Reagan's command of issues was shallow—and that he had used his Hollywood experience to base his political career on learning scripts and reading them out. In 1980, this perception was pervasive, with one observer noting that "the characteristic tone of U.S. reporting about Reagan is one of condescension. It is implied, sometimes written, that Reagan does not understand what he is saying, that[,] if he does[,] he does not believe what he says, and that even if he understands what he says and believes it to be true, he won't really do anything about it." (*The Bulletin* (Australia), July 29, 1980, p. 75.)

In contrast, Reagan possessed credibility with Milton Friedman—both during his unsuccessful 1976 contest for the Republican nomination and over the 1980s as a candidate and then U.S. president. Two factors likely contributed importantly to Friedman's favorable judgment on Reagan.

One was Reagan's consistency over time. Friedman remarked in Los Angeles in November 1981 that, instead of adapting to changes in majority political opinion over the decades, Reagan had maintained his own policy approach for thirty years.[3] Along the same lines, in a speech given in Sydney in the previous April, Friedman had remarked: "Ronald Reagan is no ordinary

[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. Errors are the authors' responsibility alone. The author regrets to note that, since the research for this chapter began, six individuals—Stephen Axilrod, Joseph Burns, Lyle Gramley, Charles Schultze, George Shultz, and Paul Volcker—whose interviews with the author are quoted below have passed away.
[2] See Carter (1980b).
[3] Oppenheimer and Company (1981b, p. 11).

politician. He has been flying in the same direction all by himself for many years now… As a result[,] you can have rather more confidence than usual in his ability to stick by his policies and carry them out."[4]

A second likely key factor was Reagan's focus on economics. This factor, closer to home for Friedman, was connected to the first. Friedman's conviction that Reagan was his own person may have been in good part due to the fact that Reagan possessed both a Hollywood career background and an interest in domestic economic policy. For, despite Carter's attempt to characterize Reagan as a standard Hollywood product, Reagan had marked himself out from other well-known actors in his policy stance. It was unusual in the first place for leading Hollywood actors to be on the Republican side. But Reagan's considerable focus on free-market advocacy—something that made Reagan the leading example of what Friedman regarded as a new type of Republican, the free-enterprise Republican (*Arizona Republic* (Phoenix), February 24, 1981)—amounted to another distinguishing facet of his public-policy position. A focus on economic liberalism was rare among his generation of Hollywood actors. It would remain uncommon among successor generations, with the most notable later case of a well-known actor propounding free-market views being Arnold Schwarzenegger—who would stun reporters by appearing at a dinner event held in Friedman's honor in San Francisco in October 1983.[5]

In Reagan's own generation, the Republican side in Hollywood tended, instead of economics, to concentrate on social issues, law and order, and national defense. During the 1950s, in particular, this side was heavily focused on carrying over anti-Communism into domestic political debate. Although, of course, a fundamental part of the capitalism/Communism division pertained to the appropriate way to organize the national economy, this 1950s U.S. debate was basically one concerned with political discourse and civil liberties, rather than being a dialogue focused on economics. The notorious 1950s McCarthyite period was, in particular, dominated by the issue of whether the holding of Communist beliefs or affiliations by American citizens should, *ipso facto*, be grounds for excluding these individuals from various lines of work. This was a public policy matter on which Reagan himself took a stand, both as a political activist and as an official representative of actors—and he was, with qualifications, essentially on the side of those seeking to restrict the job opportunities available to those identifying themselves with Communism (see Weisberg, 2016, pp. 26–27). But it was a matter on which Friedman was critical of the stand taken on the anti-Communist side, both at the time and subsequently (see

[4] Friedman (1981e, p. 12).

[5] See the reports in the editions of the *San Francisco Chronicle* and the *San Francisco Examiner* of October 5, 1983.

Nelson, 2023). Among his later criticisms were his negative remarks in *Capitalism and Freedom* regarding Hollywood blacklisting practices.[6]

Friedman's negative perspective on the McCarthyite movement was underscored again in the 1980s when Friedman satirically likened feelings about U.S. monetarists to 1950s attitudes toward U.S. Communists. There was, he suggested, an easy way of bringing out the limits to the Federal Reserve's acceptance of monetarism at the time of its adoption in 1979 of nonborrowed reserve targeting in 1979. "If at the outset of the experiment, each member of the Board of Governors had been asked, 'Are you now, or have you ever been, a monetarist?,' not a single one would have answered 'yes.'"[7]

Another bitter area of U.S. debate in which Friedman had parted company to some extent with prominent Hollywood Republicans—although, in this case, less so with Reagan himself—pertained to the outlook on the student-protest movement associated with the Vietnam War. Friedman was himself highly critical of this movement because of its disruptive effects on university teaching and administration, and he was also opposed many of the political and economic prescriptions of the movement. Nevertheless, he was careful to note that the upsurge in campus activism could be traced to "what happened in the '60s, which was a reaction to the draft, which I opposed [too]." (*Oakland Tribune* (California), April 18, 1977, p. 12.) And, of course, Friedman grounded his opposition to conscription in free-market economics and not just in civil-liberties arguments.

Even before Schwarzenegger emerged on the scene, Reagan was not entirely alone among prominent Hollywood actors in his free-market-economics focus. Another example—albeit someone who was nearly twenty years younger than Reagan—was John Gavin, a Hollywood actor from 1956 onward.[8] By early 1971, Gavin had been selected to succeed George Lazenby in the part of James Bond, only to be sidelined shortly thereafter when Sean Connery was restored to the role.[9] Thereafter, Gavin remained in acting for nearly a decade, but his interest in

[6] Friedman (1962a, p. 19).

[7] Friedman (1984c, p. 397). Friedman made these remarks in December 1983. He had already published a similar observation in print in Friedman (1983e, p. 7).

[8] See Europa Publications (1986, p. 546).

[9] See [*Weekly*] *Variety* (Los Angeles), March 3, 1971, as well as Rubin (1981, p. 100). Gavin was to have played Bond in the film adaptation of *Diamonds Are Forever*. In the early planning stages of this film, it was intended that the storyline would feature the twin brother of Goldfinger as the villain. In contrast to the storyline of *Goldfinger*, the new villain would be concerned with the smuggling of diamonds rather than gold (Rubin, 1981, pp. 101–102). This change in the villain's interest (from gold in the original *Goldfinger* film and book) to diamonds would have been in keeping with the *Diamonds Are Forever* title. But it also likely reflected the fact that the aims that Goldfinger sought in the original *Goldfinger* (both book and film) had basically amounted to the end of the

economic matters was reflected in, and likely intensified by, a side job as a spokesman for Bank of America during the turbulent macroeconomic years from 1973 to 1980. During the Reagan presidency, Gavin switched to a diplomatic career, serving as U.S. ambassador to Mexico from 1981 to 1986.[10] After this, he returned to the United States. Gavin, a one-time Stanford University undergraduate, would attend a Hoover Institution event, held on July 16, 1987, to mark Friedman's seventy-fifth birthday (*The Progress* (Clearfield, Pennsylvania), July 18, 1987).

Another free-market-oriented actor—and closer to a contemporary of Reagan—was Robert Montgomery. Montgomery was, in fact, about seven years older than Reagan—being born in May 1904, the month after Arthur Burns. Montgomery died in October 1981, so he saw little of the Reagan presidency. But he had, in common with Burns, served as an adviser to President Eisenhower, and in this capacity he—also like Burns—had had his own office in the White House during Eisenhower's first term. Furthermore, early in the Eisenhower years, Montgomery had published an article in a December 1953 issue of the magazine *The Freeman*—a fortnightly publication that the Friedmans would later name as one of the few major free-market periodicals of the era.[11] That same *Freeman* issue contained an article by Milton Friedman: a popular exposition of his case for floating rates (December 14, 1953*a*). The issue of international monetary arrangements was another one on which Friedman was during the 1950s, and remained over the 1980s, at odds with a great many on the Republican and free-market sides—including *The Freeman*'s editor, Henry Hazlitt, who was an ardent supporter of the gold standard or of rigidly fixed exchange rates. This was an area of disagreement that Friedman acknowledged in the article, when he observed that support for intervention in foreign exchange markets could be

international gold-price peg (an aim Friedman too supported, but by means of market pressures and policy change—not the criminal sabotage of the Bretton Woods system of the kinds Goldfinger pursued in the novel and film) had had largely been achieved by 1971. This course was further sealed by the closing of the gold window in August 1971 when as Friedman put it in January 1980 (Friedman, 1980d, p. 81), the previously "reduced role of gold [in the U.S. monetary system]… was finally consummated." By this time, *Diamonds Are Forever* was being made, with the planned story link to *Goldfinger* having been dropped. As it happened, the film appeared in the same year in which Friedman contributed a foreword to a published study of the diamond industry. See Friedman (1971b).

[10] During his confirmation hearing of April 22, 1981, Gavin implied that the economic issue he planned to focus on as ambassador was the development of a U.S.-Mexico free trade area (Committee on Foreign Relations, U.S. Senate, 1981a, p. 34). In the event, after his first year in office, it was the Latin American debt crisis that was the predominant economic issue facing Gavin.

[11] Friedman and Friedman (1988, p. 464). The Friedmans recalled the magazine as being published by the Foundation for Economic Education (FEE). However, the 1953 issue in which Friedman's article appeared during the period before it was a FEE publication (or—to describe the initial arrangements more precisely—before the FEE's president, Leonard Read, took a separate position as publisher of *The Freeman*: see *Greensboro Record* (North Carolina), June 21, 1954). Indeed, as of 1953, Friedman was estranged from the FEE (see Friedman and Friedman, 1998). He was still smarting from the experience of the production of Friedman and Stigler (1946)—a pamphlet that the FEE had published, but only after first threatening to omit, and then deciding to include an editorial disclaimer regarding, a favorable remark the authors made about income redistribution as a government policy aim.

found both on “the extreme left and the extreme right of the political spectrum.”[12]

Robert Montgomery’s own *Freeman* article (December 14, 1953*b*) was briefer, and much lighter in substance, than Friedman’s piece. But it concerned a topic that Friedman himself discussed repeatedly in his own writings during the 1950s and 1960s: the drift in U.S. political usage toward the deployment of the term “liberal” to apply to those inclined toward, rather than against, economic interventionism. “The professed liberals of today do not really march under the banners of genuine liberalism or progressivism but, on the contrary, represent the worst type of reaction,” Montgomery wrote. “For the reactionary is the person who insists that the key to progress lies in more and more government.” On Friedman’s part, this was a point that he was still pressing home more than a quarter-century after the *Freeman* issue that had featured his and Montgomery’s contributions. As the 1980s were being ushed in, Friedman remarked (in *The Register* (Santa Ana, Orange County, California), December 23, 1979, p. E11): “Let’s call a spade and spade. I’m a liberal in the true and classical sense of the term—of somebody who believes in freedom. The kind of people who now call themselves are collectivists—they believe in government control, they believe in reducing individual freedom and having bureaucrats tell me what to do instead of letting me do it myself. We’re not going to call them ‘liberals.’”

Robert Montgomery’s daughter, Elizabeth Montgomery, followed in her father’s footsteps both in joining the acting profession and in being politically active. But she differed sharply from Robert Montgomery in her political leanings. So much so, in fact, that, in 1988, Elizabeth Montgomery narrated a documentary that essentially accused of the 1980 Reagan presidential campaign team of conspiring with the government of Iran to postpone the release of the U.S.-embassy hostages until after the November election.[13] As this documentary also attested, Elizabeth Montgomery’s political interests, like those of many actors, were centered on foreign policy rather than economics. It is nevertheless the case that the biggest commercial success of her career—the fantasy-comedy series *Bewitched*—provides a notable spotlight on economic developments. This is because its period of broadcast coincided with important turning points in the course of U.S. inflation.

The *Bewitched* series was an instant success, finishing second-highest among all programs in national ratings for the 1964/1965 U.S. television season (Brooks and Marsh, 1999, p. 1248). Its

[12] *The Freeman*, December 14, 1953*a*, p. 203 (Friedman, 1968a, p. 217). See also Friedman (1953a, p. 203). Even in the issue of *The Freeman* in which Montgomery’s and Friedman’s articles appeared, Hazlitt’s policy differences with Friedman were evident, as the magazine’s two lead editorials strongly backed Senator Joseph McCarthy’s anti-Communist campaign (*The Freeman*, December 14, 1953*c*, 1953*d*).
[13] *Coverup: Behind the Iran-Contra Affair*, 1988.

debut month was September 1964, just ahead of the formal start of that season. In this month, the United States' 12-month CPI inflation was 1.2 percent. In April 1965, near the end of that first season, the rate moved above 1.2 percent. It would not return to 1.2 percent for over two decades. In the meantime, the *Bewitched* series' final season of first-run broadcast was 1971/1972, the final primetime network episode—a rerun—being in July 1972. In that month, the twelve-month CPI inflation rate was 3.0 percent. Although well above 1964/1965's inflation rates, this was close to the lowest rate in the whole of the 1970s—occurring just ahead the decade's all-time low of 2.9 percent in August 1972. These comparatively low summer-1972 inflation rates reflected the maximum effect on the measured inflation rate of the Nixon price controls introduced in August 1971 and—very likely—also the peak of the cumulative response to the tight monetary policy initiated by Federal Reserve Chairman Martin in 1968 and discontinued by the Arthur Burns Federal Reserve over the course of 1970.

During the interval between the premiere of *Bewitched* and the series' final regular network broadcast, U.S. inflation had, in February 1970, reached a post-Korean War then-peak of 6.4 percent. The impact on the national mood of this inflation rate, in combination with the restrictive monetary policy stance that Arthur Burns inherited when he took over the Federal Reserve during this period, was reflected in dialogue in the April 9, 1970, episode of *Bewitched*. In a jarring interruption to the fantasy air of the series, one character (Larry Tate) lamented the country's current constellation of "inflation and tight money."

After the *Bewitched* series ended, Elizabeth Montgomery focused her acting career on television movies. Among these, the first broadcast in the 1980s was a telemovie titled *Belle Starr*, broadcast on CBS on April 1, 1980.[14] It was the country's twenty-third most-watched program for the week (*San Francisco Examiner*, April 9, 1980)—attaining a considerably higher viewing total than had ever been secured by *Free To Choose*, which had recently finished broadcasting on U.S. public television. The telemovie's transmission occurred against a historically notable economic background. For the weeks spanning the completion of *Free To Choose*'s U.S. screenings and the broadcast of *Belle Starr* coincided with a major milestone in the trajectory of the U.S. inflation rate. On March 25, 1980, a Bureau of Labor Statistics release indicated that, in the year to February 1980, the rate of increase in the total consumer price index had been 14.1 percent.[15] It would transpire that in March 1980 the twelve-month CPI inflation rate was even

[14] Friedman had made an appearance on CBS just a few days earlier, offering commentary in the news special *Carter and the Economy: Other Views* (March 26, 1980).

[15] *Washington Star* (Washington, D.C.), March 25, 1980. In modern data on the CPI as given in the Federal Reserve Bank of St. Louis' FRED portal, the effect of subsequent rounding of the index (associated with changes in the year assigned an index value of 100) has been to give this rate as 14.2 percent.

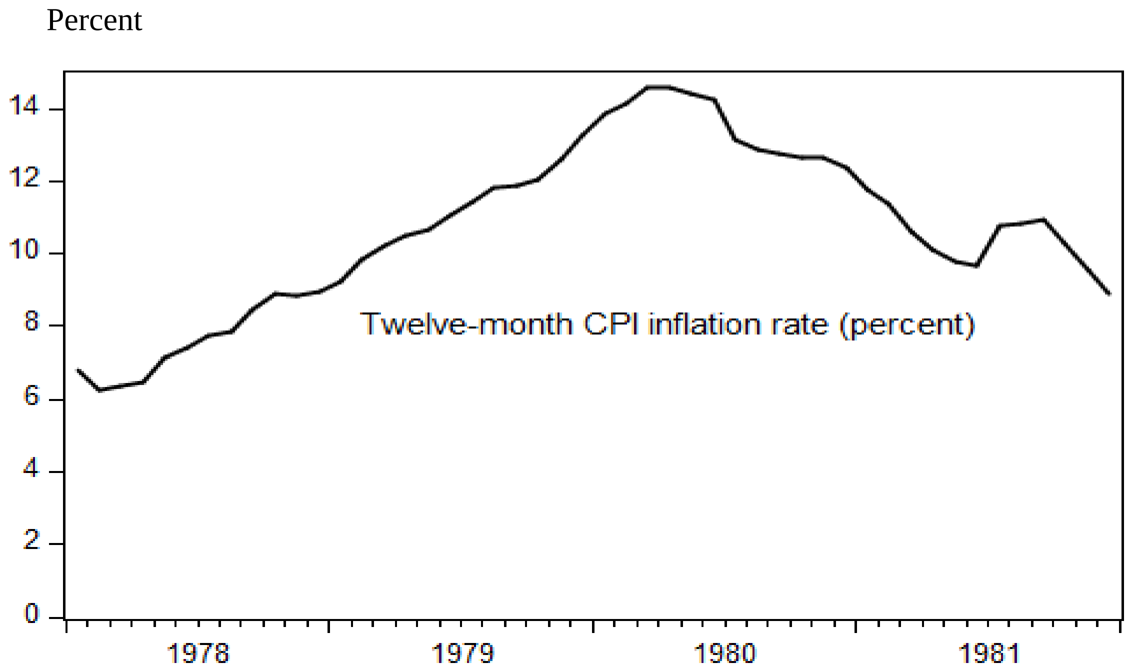

Figure 1. Twelve-month percentage increase in the consumer price index, January 1978–December 1981.
Source: Federal Reserve Bank of St. Louis' FRED portal.

higher: 14.7 percent. This was an inflation rate almost maintained in April.[16] These March and April 1980 readings on inflation—comprising "the nearly 15 percent peak in early 1980," as Friedman later remarked (*Newsweek*, June 15, 1981)—would be the top rates recorded not only during the U.S. Great Inflation (see Figure 1) but also over the whole postwar period.

In terms of annual-average data, CPI inflation was 11.3 percent in 1979 and 13.5 percent in 1980. As 1974's inflation rate had been 11 percent, the consecutive years' rates turned out to be two highest of any in the postwar period.[17] The December-to-December twelve-month CPI inflation rates were 13.3 percent in 1979 and 12.5 percent in 1980. Again, these were the

[16] On the March 1980 14.7 percent rate, see *Miami Herald*, April 23, 1980, and Bureau of Labor Statistics (1980). Retrospectives on the period sometimes instead give the March 1980 CPI 12-month inflation rate as being either 14.6 percent (cnn.com, January 12, 2022) or 14.8 percent (Bolhuis, Cramer, and Summers, 2022). These differences from the originally reported number of 14.7 percent largely reflect rounding of the CPI series that has occurred as the base period for the index has been moved to later years. Modern seasonally adjusted CPI data now give the twelve-month inflation rate as being 14.6 percent in both March and April 1980. Rounding has had a different—upward—effect on the non-seasonally adjusted CPI 12-month inflation rate, which now shows a March 1980 peak of very close to 14¾ percent: see *https://fred.stlouisfed.org/series/CPIAUCNS#0*. Expressing that rate in single-decimal-point form gives the 14.8 percent rate that is sometimes said to be 1980's inflation peak.

[17] See, for example, Dornbusch and Fischer (1987, internal front cover) and Council of Economic Advisers (2011, p. 263, Table B–63).

postwar period's highest rates, as they both exceeded 1974's inflation rate of 12.3 percent.[18]

The inflation peak in early 1980

Friedman would later say that inflation's peak was actually "more than 15 percent during the 1970s" (*Wall Street Journal*, April 15, 1988). As has been seen, the twelve-month rate did not quite reach this value, either during the 1970s or when it actually peaked during the first four months of 1980. The citation of a 15-plus percent rate rested on using the annualization or compounding of certain quarters' or months' CPI inflation rates. This was a practice about which Friedman had considerable reservations. But it is not surprising that Friedman in 1988 remembered inflation passing 15 percent, because references to such rates were pervasive in the media reporting of the inflation situation in early 1980. In particular, the first CPI data release for the decade, that pertaining to January 1980, provoked such headlines as "Inflation Running Wild—Annual Rate Soars to 18.2%" (*New York Post*, February 22, 1980)—the "annual" rate in this case being an annualization of January's monthly rise in the CPI of 1.4 percent.

At the time, Friedman cautioned against taking the annualized rates at face value: "The basic inflation rate is far too high, but it is not as high as the nearly 20 percent [annualized headline] rate of recent months." Monthly rates of price increase were bound to be lower in the rest of 1980, he noted, "simply because recent very high rates reflect transitory shocks." (*Newsweek*, March 24, 1980.) In a contribution to a CBS news special on inflation, Friedman likewise observed that a step-down in inflation from its "extraordinarily high" rate of recent months was locked in because of the boost to recent rates arising from "accidents, aberrations. The fundamental underlying inflation rate is lower." (*Carter and the Economy: Other Views*, CBS, March 26, 1980.)

One factor that Friedman would stress as having exaggerated the headline inflation rate in 1979 and 1980 was the fact that the CPI treated mortgage rates in a manner that likely gave them an outsized role in the calculation of housing costs.[19] That mortgage interest rates did matter for the cost of living was not in question. Nor was the fact that these rates had risen significantly: reflecting the recent runup in longer-term market interest rates, the mortgage rate prevailing in the United States in early 1980 was around 12.75 percent (*San Jose Mercury* (California), January 28, 1980). But, as was also the case in a number of other countries' CPIs at the time, the

[18] See Figure 1 as well as Council of Economic Advisers (2011, p. 263, Table B–63).

[19] See Friedman's remarks in *Newsweek*, July 25, 1983, and in Friedman and Friedman (1984, p. 89; 1985, p. 90).

mortgage rate was given what was widely considered a disproportionate weight in the computation of housing expenses (*Kansas City Times* (Missouri), January 22, 1980; Gordon, 1993, pp. 261–262). A further measurement problem lay in the fact that the United States' housing market, unlike that in the United Kingdom and some other countries, underwent only a limited move to adjustable-rate—and, in particular, short-term-rate-linked—mortgages during the Great Inflation period. Yet the official CPI measurement process essentially treated the interest rate on new mortgages as applying to existing mortgages—thereby overstating the effect of increased mortgage rates in 1979–1980 on the cost of living (Blinder, 1982, p. 272; Gordon, 1993, p. 261). A subsequent reform to the price index in 1983 removed the disproportionate weighting to mortgage rates.[20] By then, however, the very sharp movements in mortgage rates seen during the 1979–1982 monetary policy regime had had major repercussions for the CPI's behavior.

A second item contributing to elevated inflation rates in 1979–1980 was, Friedman acknowledged, the second oil shock. Correspondingly, in June 1980 he observed that inflation across countries had been "made worse by the energy crisis."[21] But he remained at pains to stress that OPEC had produced a sudden move in specific prices that had been superimposed on what would have already been a high inflation rate. "Inflation is caused in Washington by the government and nowhere else," Friedman remarked in November 1980. "The talk that inflation is caused by the oil problem is [offering] an excuse and not a reason." (*San Diego Union* (California), November 22, 1980). He affirmed the following April: "Inflation everywhere is made at home and not abroad. It has not been produced by OPEC and the Arab sheiks."[22]

In particular, Friedman insisted that double-digit inflation would have materialized even absent the second oil shock. On this, he had a new ally in Arthur Burns, who of late had shown some signs of second thoughts about the importance he had ascribed, during his 1970–1978 Federal Reserve tenure, to nonmonetary sources of inflation. Testifying to Congress' Joint Economic Committee in late March 1980, Burns contended that "perhaps 2 percent of our inflation rate, possibly a little more, could be attributed to the [rising] oil price," and so he implied that inflation would still be in double digits in the absence of the oil event.[23] Burns seemed amenable to viewing, as Friedman did, recent inflation as reflecting excessive demand, which had been

[20] See Gordon (1993, p. 261) and Friedman and Friedman (1984, p. 89; 1985, p. 90).

[21] Friedman (1980b, p. 56; p. 51 of 1991 reprint). In the U.S. case, some of the 1980 rise in retail gasoline prices reflected an adjustment not only to the second oil shock but to the *original* oil shock, as part of the domestic petroleum price increases in that year reflected the relaxation of domestic price controls in force over the 1970s.

[22] Friedman (1981e, p. 6).

[23] From Burns' testimony of March 27, 1980, in Joint Economic Committee, U.S. Congress (1980a, p. 139).

manifested in rapid rates of growth of aggregate nominal spending in the late 1970s.

It was likely that there was less harmony between Friedman and Burns on the precise source of this excess demand. Friedman believed that it, and so ultimately 1980's double-digit inflation, had its origins in the behavior of monetary policy in the later part of Burns' tenure as Federal Reserve chair. That this was not just an *ex post* judgment on Friedman's part was reflected in the fact that, near the end of the Burns era at the Federal Reserve, the *Chicago Tribune* had assigned to a report on Friedman's the headline "10% Inflation in 1980?" (November 14, 1977).

The conclusion that the United States' second bout of double-digit inflation had deeper origins than the second oil shock is supported by the behavior in 1976–1977 of M2 growth—the basis for Friedman's predictions of an inflation eruption. It is also implied by a comparison of four-quarter rates of increase in the CPI and the output deflator in 1979–1981. The latter index is widely acknowledged as being much less sensitive to oil-price swings than is the CPI (see, for example, Blanchard, 1997, pp. 31–32). Starting in 1983, Friedman's accounts of early-1980s U.S. inflation developments would center on the behavior of the deflator rather than the CPI.[24]

Making a definitive statement about how the deflator inflation rate behaved in the Great Inflation period is complicated by the open-ended nature of the revision process to price indexes in the national accounts. Even though the official revisions to aggregate national income (nominal GNP—or nominal GDP, which would become the preferred series in the United States) is a process that winds down over time, the historical behavior of the associated price index typically continues to change when a new base year is chosen. The combination of data revisions and the changing reference year implies that what the official U.S. data say about the course of the output-deflator inflation rate during the Great Inflation period is something that has changed over time. For example, on annual-average data, Dornbusch and Fischer (1987, internal back page) gave U.S. GNP deflator inflation as 8.8 percent in 1979, 1980 9.1 percent, and 1981 9.6 percent.[25] In contrast, data as of 2011 gave average-year GDP deflator inflation as being, on average, a bit lower: 8.3 percent in 1979, 9.1 percent in 1980, and 9.4 percent in 1981. But, across various vintages of data, two aspects of the behavior of the deflator have remained quite resilient: (*i*) The peak of the GDP deflator inflation rate was closer to the CPI inflation peak in

[24] See *Newsweek*, July 25, 1983, and Friedman (1984c, pp. 399–400; 1985b, p. 56). In Friedman (1983e, pp. 11–12), he used both series but focused on the deflator.

[25] Gordon (1993, p, 263) suggested that, as of the time of his writing, "the annual increase in the fixed-weight GNP deflator never exceeded 9.3 percent during the Volcker era." As indicated in the next, previous and later vintages of both the GDP and GNP deflator suggest that the peak rate was somewhat higher than this.

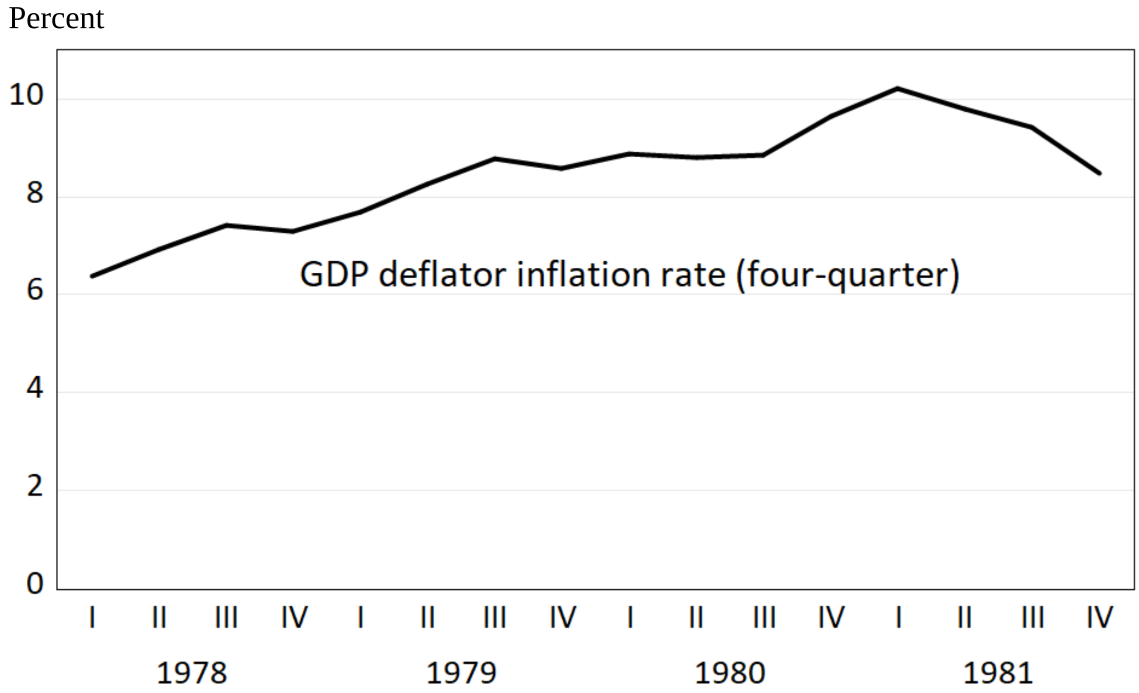

Figure 2. Four-quarter percentage increase in the GDP deflator, United States, 1978:Q1–1981:Q4.
Source: Federal Reserve Bank of St. Louis' FRED portal.

the 1974–1975 inflation peak than in the second of the twin peaks in 1979–1981, when it was well below the CPI inflation peak.[26] (*ii*) Nevertheless, the deflator inflation rate, like the rate of CPI increase, *did* move into double digits during *both* peaks, not just the first. In Figure 2, for example, the modern data on the four-quarter GDP deflator inflation rate show a peak at 10.2 percent in 1981:Q1.

President Carter and inflation, 1980–1981

The publication, noted above, of in mid-February 1980 of 1.4 percent increase in the monthly January CPI data was one of a number of events that elevated the alarm about inflation in U.S.

[26] As of the mid-1970s, the official U.S. data suggested that four-quarter GNP deflator inflation rates were notably below the CPI inflation rate—but matched or exceeded the double-digit CPI inflation readings recorded during 1974. See H.T. Shapiro (1977, Figure 2, p. 273). In annual-average data, these high mid-decade inflation rates show up largely in the year after their appearance in the quarterly data. The 1979 *Economic Report of the President* (Council of Economic Advisers, 1979, Table B–3, p. 187) gave GNP deflator inflation as having been 9.7 percent in 1974 and 9.6 percent in 1975, while the 1983 report gave the respective rates as 8.8 percent and 9.3 percent (Council of Economic Advisers, 1983, Table B–5, p. 169). With regard to the United States' GDP deflator inflation rates, the International Monetary Fund (1986, p. 9) gave the values as 10.1 percent for 1974 and 10.9 percent for 1975, while the Council of Economic Advisers (2011, Table B–3 p. 193) implied rates of 9.1 percent in 1974 and 9.5 percent in 1975.

public discussion. Toward the end of the year, Paul Volcker would recall the January-February 1980 period as seeing a "very serious deterioration in public confidence in the government's ability and resolve to rein in inflation."[27]

The January CPI data was reported as leading to "new demands from Congress that Carter strengthen his faltering anti-inflation program" as well as to commentary by Carter's primary challenger, Teddy Kennedy, who proclaimed that Carter was "playing a losing hand in fighting the battle against inflation" and who reaffirmed his own advocacy of mandatory wage and price controls (*Daily News* (New York), February 23, 1980*a*, p. 3). The *Daily News* editorialized (February 23, 1980*b*): "Foreign crises and alarms have occupied the nation's attention for the last few weeks. But the January cost-of-living figures are enough to remind Americans that inflation is still Public Enemy No. 1."

It was in this atmosphere that, as Gordon (1993, p. 261) put it, "a panicked Carter Administration" put together a new anti-inflation package, which the president announced on March 14 (see Carter, 1980a). Most substantively—in terms of the eventual response of the economy—the new program contained credit controls. These controls, discussed in detail in Section II below, indeed produced a substantial and rapid reduction in aggregate demand, and so on that score they were genuinely anti-inflationary. But the controls proved to be highly disruptive to the Federal Reserve's already-announced anti-inflation program of gradual monetary restriction.

In the March anti-inflation program, President Carter also announced measures to tighten fiscal policy, including by reducing planned outlays. These left Friedman unimpressed because they in effect only implied a "slightly smaller *increase*" in government spending than what the president had previously envisioned (*Newsweek*, March 24, 1980, emphasis in original; see also Friedman's remarks in *San Antonio Light* (Texas), March 30, 1980).

Another prominent element of the March 1980 program underlined the degree to which the administration's overall view of the inflation process was still at variance with the perspective on inflation that Friedman propounded. Although, in its approach to the double-digit price rises of 1980, the Carter economic team acknowledged excess demand as one source, it continued to stress cost-push elements as prominent drivers of inflation. Correspondingly, in the

[27] From Volcker's written submission in his testimony of November 19, 1980, in Committee on Banking, Housing and Urban Affairs, U.S. House of Representatives (1981a, p. 33).

announcement of his March 1980 package, Carter (1980a, p. 6) indicated: "Our third area of action is the voluntary wage and price standards." These were a continuation of the guidelines that had been part of Carter's anti-inflation programs since 1978. Now, they were to be reemphasized: "I'm sharply expanding the price and wage monitoring activities of the Council on Wage and Price Stability," Carter remarked. "Its current staff of 80 people will be more than tripled."[28]

Friedman, of course, was already on record as seeing Carter's efforts to use incomes policy against inflation as wholly misconceived. "Carter has no economic strategy for battling inflation, so I can't comment on it," he had remarked (*Chicago Tribune,* October 8, 1979). In later years, Friedman's grasp of the English language would be questioned when his criticisms of incomes policies like Carter's were quoted. Specifically, Friedman would be taken to task for saying that "voluntary wage and price controls, if enforced" would be harmful (*The Plain Dealer* (Cleveland, Ohio), March 27, 1983, p. 24). What such criticisms of Friedman's statement overlooked was that the "voluntary" description was the administration's—and when he used the word, Friedman viewed it as being in quotation marks. "We already have wage and price controls, [and] the Carter Administration's controls are not 'voluntary'—I don't care what they are called," he observed (*Dallas Morning News,* March 26, 1980).[29] The fact that the administration was urging compliance with the controls, was indicating that companies that did comply would have improved chances of receiving government contracts, and was contemplating penalties in the event of non-compliance implied that they were not truly voluntary, Friedman believed. Consistent with Friedman's characterization, when the Mobil oil company indicated it would not adhere to the administration's price guidelines, President Carter noted, "Sanctions against Mobil are being considered," implied that a recent contract would not have been awarded to Mobil had it occurred after the company's flouting of the guidelines, and stated that the government was engaged with Mobil executives "to try to force them, through persuasion" to rescind recent price increases (Carter, 1980c, p. 7).

What the March package instead should have done, Friedman argued, was to "end the mislabeled 'voluntary' price and wage controls" (*Newsweek,* March 24, 1980). In response to the package as announced, he suggested, there should have been resignations on the part of members of Carter's economic team. "It would have been a lot more healthy for the government had there

[28] Carter (1980a, p. 7).

[29] Likewise, Friedman and Friedman (1980, p. 68) earlier referred to "President Carter's so-called voluntary wage and price controls."

been some resignations of some economic advisers."[30]

In making this recommendation, Friedman was not suggesting that Carter or his economic officials were personally responsible for inflation. Anna Schwartz had once pointed to Emperor Diocletian as an example of a head of state who had stepped down in light of inflation getting out of control (see Schwartz, 1973, p. 246). But Diocletian had been the main monetary authority of the Roman empire as well as its head of state. In contrast, the U.S. executive branch was not the country's principal monetary authority—the Federal Reserve was. Friedman certainly associated with the U.S. president a vital role in creating an atmosphere in which a noninflationary monetary policy could be followed by the Federal Reserve—a matter discussed further below. But he still associated most of the decision-making prerogatives regarding monetary policy on a month-to-month and year-to-year basis with the Federal Reserve itself. Indeed, as already noted, Friedman's analysis, particularly its emphasis on lags, implied that the monetary policy actions that had produced the 1979–1980 inflation had been made when the Federal Reserve was run by someone out of office by 1980: Arthur Burns.

In these circumstances, Friedman hoped for the resignation by Carter's economic officials in response to the March 1980 anti-inflation program not as a means of suggesting that the president was directly to blame for the high inflation rate, but, instead, as a protest against the measures that Carter was advancing as putative anti-inflation devices. In particular, a similarity that Carter in 1980 and Diocletian in the fifth century did possess was that they, as well as many other leaders in the 1475 years in between, took a cost-push orientation to inflation and embraced incomes policy as the remedy. Friedman's suggestion that Carter's economic personnel should resign presupposed that they, like him, saw incomes policy as a false cure and had opposed the president's intensified use of incomes policy in the March 1980 package. The package, Friedman believed, was a reflection of "the disparity that exists between advice given and advice taken" (*San Antonio Light* (Texas), March 30, 1980).

Schultze, Kahn, and Miller

Here, however, Friedman was likely laboring under a misapprehension. As had so often been the case in his analysis of actual policymaking over the 1970s, Friedman in 1980 seemed to be

[30] *San Antonio Light* (Texas), March 30, 1980. Because, in the U.S. executive system, policy is decided on by the president and not by the cabinet, Friedman might not here have been excluding from the category of "advisers" those administration officials, including the Secretaries and Under Secretaries of the Treasury, whose appointment required Senate approval.

presuming that economic officialdom accepted that inflation was a monetary phenomenon and that the cost-push diagnosis of inflation was accordingly misconceived. In fact, however, this presumption, although it was essentially accurate by 1980 as a characterization of the Federal Reserve (at least at the top leadership level), was not valid when applied to key economic personnel in the Carter Administration, even in 1980. In particular, as of 1980, cost-push views of inflation continued to be espoused by CEA chair Charles Schultze, inflation "czar" Alfred Kahn, and Secretary of the Treasury G. William Miller.

Schultze described U.S. inflation in wage-push terms: "When we came in [in 1977], kind of the underlying rate of inflation was something like 6 percent a year measured by business cost growth, and it's now 9," he observed in October 1980.[31] "What happened," he testified the previous month, "… is that various events—principally… two major surges in [the] oil price… have pushed up the rate of inflation, but—once up there—it doesn't come down or comes down slowly."[32] This diagnosis made Schultze amenable to incomes policy. His retrospective judgment on the Carter wage/price guidelines, however, was "they didn't work… I don't think it [incomes policy] really had a lot of impact, [although] it may have had some… Because of that, I think, nobody has really pushed that since." (Charles Schultze, interview, July 9, 2013.)

Alfred Kahn was head of the Council on Wage and Price Stability—and so was at the forefront of the administration's wage/price guideline activity since 1978. Friedman viewed Kahn as exhibiting "obfuscation" since taking on this position, in his pointing to nonmonetary approaches to diagnosing and resolving inflation (*Newsweek*, March 24, 1980). But Kahn evidently viewed himself as addressing important wage-push forces. In April 1979, he had stated that there was "no alternative" to wage/price guidelines and proclaimed that there was "genuine reason to expect or hope that after a few months we will get a tapering off" in inflation (*Manchester Union Leader* (New Hampshire), April 27, 1979, p. 24). A year later, Kahn was still stressing guidelines' role in preventing a wage-price spiral, asserting: "An acceleration of wage increases can only mean an acceleration of inflation" (*Daily News* (New York), April 10, 1980, page 54)—through a policy (guidelines) that was allowed for more flexibility than formal wage-price controls. He viewed himself as holding the line against such formal controls: if the price guideline were violated, he suggested, then "there would be a total loss of credibility on the wage [guideline] side and a total insistence on mandatory wage and price controls." (*Los Angeles*

[31] *Wall Street Week*, Maryland Public Television, October 17, 1980, p. 9 of transcript.
[32] From Schultze's testimony of September 8, 1980, in Committee on the Budget, U.S. House of Representatives (1980, p. 5).

Times, April 22, 1980, Part IV, p. 1.)[33]

The Secretary of the Treasury, G. William Miller, was likewise on record as subscribing to cost-push explanations regarding inflation.[34] He was accordingly enthusiastic about the prospect of setting up a permanent incomes-policy arrangement in the United States. In early 1979, as Federal Reserve chair, Miller had offered words of support for on the administration's attempts (which had subsequently languished) to introduce a tax-based form of incomes policy: "real wage insurance appears to be an appropriate instrument to consider in attempting to damp a self-perpetuating wage-price spiral."[35] Miller had been a member of Carter's cabinet since August 1979.[36] He consequently became even more directly involved in the promotion of incomes policy. Indeed, shortly before the 1980 presidential election, Miller declared the Carter guidelines program successful in holding the year's inflation down by 1 to 1.5 percentage points, stated that "in December we'll decide what kind of income[s] policy we want for next year," and suggested that a second-term Carter Administration might try to return to the tax-based incomes-policy idea (*Chicago Tribune*, October 14, 1980).

Friedman's negative reaction to the March 1980 Carter plan was accompanied by his concerns about how it would be interpreted by the general public. Its announcement occurred, he noted, in the context of a "reduction in inflation rates which is coming anyway" (*Carter and the Economy: Other Views*, CBS, March 26, 1980). That step-down would reflect both the passing of the transitory shocks of early 1980, as well as the delayed effect of the monetary policy firming that had taken place over 1978 and 1979—the decline that monetarists "have been predicting for some time" (*Dallas Morning News*, March 26, 1980).[37] This gave rise to what Friedman regarded as "perhaps the greatest danger [stemming] from his [the president's] cosmetic proposals" (*Newsweek*, March 24, 1980). "My main concern is that when inflation starts

[33] Unlike Friedman, who believed that most forms of incomes policies encouraged policymakers to stimulate aggregate demand excessively, Kahn associated mandatory but not voluntary controls with the tendency for incomes policy to be treated as a substitute (*Los Angeles Times*, March 23, 1980).

[34] See the previous two chapters for discussion.

[35] Letter by G. William Miller of January 17, 1979, in Committee on Ways and Means, U.S. House of Representatives (1979, p. 402). The main reservation Miller had about real wage insurance was the cost in lost tax revenue. He was unambiguous in suggesting that adherence by the private sector to the Carter Administration's wage/price standards (its stated limits on private-sector wage and price increases) "will benefit us all." (January 25, 1979, testimony in Committee on the Budget, U.S. House of Representatives, 1979a, p. 36.)

[36] As discussed in the previous chapter, this move from Federal Reserve Chair to Secretary of the Treasury was seen at the time as a move to an even more prestigious job—in effect, a promotion. Some later accounts incorrectly portrayed Miller as having been pushed out of the Federal Reserve job or as stepping down (*a la* Diocletian) in the face of being blamed for high inflation.

[37] Friedman had stated in the middle of the fall of 1979: "I think in the next four or five months, inflation will go down." (*San Francisco Examiner*, October 18, 1979.)

dropping, later this year… Mr. Carter's inadequate plan will get the credit." (*Dallas Morning News*, March 26, 1980.) Not only would the public's understanding of the reason why inflation had fallen be hindered, but also, Friedman feared, a declaration of victory might trigger an end to monetary restraint.

The link between the chances off preservation of monetary restraint and the mood set by President Carter is discussed later. But the Friedman criticism of much of the president's March 1980 program boiled down to a difference between the Friedman and Carter diagnosis of inflation—a difference that spanned the life of the administration. A year before the 1980 package, a newspaper columnist hostile to the president had juxtaposed a quotation from Carter, "It's a myth that government itself can stop inflation," against one From Friedman that "government, and government alone, is the source of inflation." "When it comes to economics," the columnist asked, "how many Americans truly prefer the analysis of Jimmy Carter to that of Milton Friedman?" (*Manchester Union Leader* (New Hampshire), March 1, 1979.)

Federal Reserve policy

"No country has ever succeeded in cutting inflation except by slowing down the rate of monetary growth," Friedman remarked a couple of weeks after President Carter launched his new anti-inflation program (*Arkansas Gazette* (Little Rock), March 28, 1980). By this point, in March 1980, the Federal Reserve was over five months into its usage of the New Operating Procedures (NOP) and its more intense efforts to control monetary growth with the aim of achieving disinflation. These shifts had signified a considerable acceptance of Friedman's views on inflation—and they contrasted with the ongoing dissent at the White House from the notion that inflation could only be cured by monetary means. "The Fed has [so far] been neglecting the quantity of money," Friedman had observed when the Federal Reserve announced its new arrangements in the system, while adding that financial markets, such as those in the foreign exchange market, "have for years learned that the thing you want to look at is what happens to the quantity of money." (*San Francisco Examiner*, October 18, 1979.)

The early 1980s would prove to be the high-water mark in the agreement on the part of financial markets, the Federal Reserve, and the U.S. economics profession with Friedman's emphasis on monetary growth in the evaluation of monetary policy. That focus would lose support as the 1980s and 1990s progressed.

In contrast, Friedman's position that monetary policy had primacy in the behavior of inflation—

already widely supported among U.S. economists by 1980, despite the continuing resistance to that position at the White House—would *continue* to prevail and would evolve into a consensus position. This represented a major change from what Eliot Janeway, an economics columnist and a longtime sparring partner of Friedman's, described as the former situation in which monetary policy was considered a yawn-worthy conversation-stopper in Washington, D.C., circles (*San Francisco Sunday Examiner and Chronicle*, December 4, 1977).

"The Fed has the authority and capability to slow the quantity of money," Friedman remarked in November 1980. "The solution [to inflation] is [in] the Fed's behavior." (*San Diego Union* (California), November 22, 1980.) By this point—late 1980—Friedman had become very unhappy with the Federal Reserve's record under the NOP and was several months into what proved to be a basically permanent disaffection with the Paul Volcker-led Federal Reserve. The basis for this disaffection will be detailed in subsequent sections of this chapter. But, in the main, they centered on the major swings in monetary growth that occurred in practice under the Federal Reserve's new procedures—swings that Friedman considered to be both harmful and avoidable.

One of the reasons Friedman considered the volatility of monetary growth to be harmful—especially in view of the FOMC's 1966–1977 record—was that the upward lurches that were part of the pattern were liable to be interpreted as an abandonment of monetary restraint. That prior record underlay Friedman's caution at the time of the announcement of the NOP, when he remarked that the "real question is whether this is a turning point or isn't" and noted that he himself had "been so unsuccessful in the past in making predictions about what the Fed is going to do" (*Richmond Times-Dispatch* (Virginia), October 12, 1979).

In the very early months of the NOP, however, the picture was clearer: monetary growth (both M1 and M2) decelerated considerably in the fourth quarter of 1979 (Simpson, 1980, Tables A1 and A2, pp. 112–113). This was true both of the "old" definitions of money that the Federal Reserve Board was reporting and that the FOMC was targeting at the time, and of "new" definitions of money that the Federal Reserve Board introduced in February 1980 (with historical data back to 1959 provided for the new M1 and M2 series) and that became the new series targeted by the FOMC.[38] The definitional changes were partly made in response to the recommendations of the Bach Committee on which Friedman had served, and they were

[38] The newly defined aggregates were announced in early February 1980 (*Financial Times* (London), February 8, 1980) and became the official money series in March 1980 (see Whitesell and Collins, 1996, Table 1, p. 4).

designed to make the definitions of money better aligned with the modern financial system. Nonbank sources of demand deposit-type assets—such as NOW accounts issued by thrifts—were added to M1, and less-liquid thrift accounts as well as money mutual fund accounts were added to M2.[39]

On January 3, 1980, in concluding his main contribution to the NBER conference in Florida, Friedman indicated that he regarded disinflation as more likely than he had six months earlier while still expressing concern about the fragility of this change in direction.[40] Shortly afterward, speaking in New York City on January 7, Friedman, while still cautious, cited as a sign pointing to a genuine policy change the fact that monetary growth had slowed down since October 1979 (Oppenheimer and Company, 1980, p. 3).

This praise turned to criticism as early as Friedman's *Newsweek* column of March 24, 1980, when he described the record so far as "uneven." The developments in monetary aggregates during the credit-controls period proved to be a clincher in leading Friedman to a negative verdict on Volcker and the NOP. Even beyond this period, the course of monetary growth varied so much within the year during both 1980 and 1981 that developments in those years are better considered episodically—as they will be in Sections II and III, which consider in detail the behavior of the monetary aggregates and the Federal Reserve over the period. Ahead of that discussion, Table 1, drawn from Dornbusch and Fischer (1987, p. 396), gives, for 1980 and 1981, the target ranges specified for the FOMC with regard to M1 and M2 and the outcomes (actual four-quarter growth rates) for those series.[41]

[39] Other changes made included the exclusion of foreign-held deposits from M1 (Bernanke and Mishkin, 1992, p. 193). In the summer of 1981, nonbank-issued travelers' checks were added to the M1 definition: see Rasche and Johannes (1987, p. 24). (Bank-issued travelers' checks were already included in demand deposits: see R.G. Anderson and Kavajecz, 1994, p. 9.)

[40] Friedman (1980d, pp. 85–86).

[41] For both 1980 and 1981, Friedman's (1984b, p. 56) numbers on the M1 target ranges and outcomes were the same as those given in Table 1 (as were those Friedman used in *Newsweek*, May 2, 1983). In contrast, there has been a variety of other figures given for the M1 targets and outcomes in these years, especially in the case of 1981. In particular, for 1981, Broaddus and Goodfriend (1984, p. 7), Bernanke and Mishkin (1992, p. 190) and Meltzer (2009b, p. 891), while giving the same target range as that given in Table 1, each reported a much lower outcome regarding M1 growth in 1981 (2.3 percent reported by Bernanke and Mishkin, with 2.4 percent given in the other two sources), while Argy, Brennan, and Stevens (1990, p. 53) gave the same 5.1 percent M1 1981 outcome as that listed in Table 1, but they reported the target range as having been 6 to 8.5 percent. The reason for all these discrepancies is that the FOMC started out 1981 by specifying different targets for shift-adjusted M1 (a series intended to make an allowance for the introduction of nationwide negotiable orders of withdrawal, or NOW, accounts) and non-shift-adjusted M1. (In both cases, the M1 aggregate in question was the modern M1 series, also called M1B.) The 3.5 to 6 percent target range initially was intended to pertain to the shift-adjusted series, with the 6 to 8.5 percent range referring to the non-shift-adjusted series (see Thornton, 1982, p. 6). In the event, the M1 series *lacking* shift adjustment came to be regarded as the more reliable of the two M1 series, so the FOMC's targets for shift-adjusted M1 came to be compared with outcomes regarding shift-adjusted M1—as they were in Dornbusch

Table 1. Four-quarter growth rates of monetary aggregates, actual and targets, 1980 and 1981

Year	M1 target range	M1 actual	M2 target range	M2 actual
1980	4.0 to 6.5	7.2	6.0 to 9.0	9.4
1981	3.5 to 6.0	5.1	6.0 to 9.0	9.2

Source: Dornbusch and Fischer (1987, p. 396).

Note: Bernanke and Mishkin (1992, p. 190) reported somewhat higher M2 growth outcomes (9.6 percent in 1980, 9.4 percent in 1981) than did Dornbusch and Fischer (1987), whereas Argy, Brennan, and Stevens (1990, p. 54) reported a lower outcome (9.0 percent) for 1980 and a higher outcome (9.4 percent) in the case of 1981.

Two points emerge from the table that will feature prominently in subsequent discussions. The first is that M1 growth was, on average, close to the rate implied by the FOMC's targets over 1980–1981 but did not meet them in 1980, which saw a target overshoot. Second, little of the retrenchment in M1 growth seen in 1981 was evident in M2 growth, which had come off its late-1970s double-digit rates but in each year was well above the 7.5 percent midpoint of the FOMC's target range. In effect, the Federal Reserve's restraint of the early 1980s maintained most of the reduction in M2 growth that had earlier occurred in the face of upward pressure(notably that associated with a rising own rate on M2 deposits), but it did not produce lower M2 growth. As discussed later, in terms of their within-year movements M1 and M2 continued to display a notable degree of comovement. Nevertheless, the basic conflict between M1 and M2 when it came to average behavior would muddy the evaluations—including some of those made by Friedman—of the monetary policy stance prevailing in these years.

President Carter and the Federal Reserve

Their differences in pattern notwithstanding, it was the case that the annual growth rates of both M1 and M2 in 1980 and 1981 were lower than the rates seen in the second monetary explosion of the 1970s—that of 1976–1977—that had preceded the second major peak in inflation. A continuous concern Friedman held over the course of 1980 and 1981 was that monetary policy

and Fischer (1987, p. 396) and Friedman (1984b). The shift-adjusted M1 series was specifically criticized as having been less reliable than the non-shift-adjusted series by Alan Reynolds in Federal Reserve Bank of Atlanta (1982, p. 142) and by Friedman in his PEPAB memorandum of January 14, 1983 (Friedman, 1983f, p. 6). The judgment that the shift-adjusted series was less reliable than the unadjusted series largely prevailed (see Cagan, 1984, p. 41). (For a defense of the shift-adjusted series, see Lindsey [1983, pp. 13, 33; 1986, pp. 183, 186, 187].)

would change decisively in an expansionary direction—in such a way that a new monetary explosion occurred.

During 1980, Friedman interpreted Carter's anti-inflation program and the administration's related initiatives in that light. He feared that Carter's continued stress on nonmonetary solutions to inflation "may induce us to avoid facing our real problem"—the imperative need for a sustained restriction of aggregate demand (*Carter and the Economy: Other Views*, CBS, March 26, 1980). The scenario Friedman particularly feared was that incomes policy might get the credit for the fall of inflation from its peak, he suggested, and "we may be lulled into postponing still longer an effective attack" on inflation via multi-year monetary restraint (*Newsweek*, March 24, 1980). "My main concern is that when the inflation rate goes down, and it will, it will seduce the American people to believe that they don't have to do the hard stuff to bring it under control," Friedman observed (*San Antonio Light* (Texas), March 30, 1980).

As a factor lowering the likelihood of this scenario, however, Friedman suggested that a recession might already be in motion as a result of the Federal Reserve's restraint. In early March 1980, Friedman hypothesized that a recession could start that month or in April. If so, he suggested, it would not be confirmed until the June-July period, by which time "by then even President Carter won't be able to go into an election-year speed-up" and "he'll make a virtue of necessity, call inflation our number-one problem, and stick with the recession to fight inflation" (*The Indianapolis Star*, March 7, 1980). In the event, the U.S. economy's cyclical peak would be dated to January 1980, and Carter's acknowledgment the recession would come earlier than Friedman anticipated—in mid-April (*Omaha World-Herald* (Nebraska), April 18, 1980).

In some respects, the embrace of austerity on the part of the administration that Friedman envisioned was realized. For example, Alfred Kahn stated: "We are determined not to make the mistake of shifting course [too] easily. For the months ahead, as far as I can see, the course is restraint."[42] As already noted, the administration revised its fiscal plans in the early months of 1980, and in the wake of this and other measures Paul Samuelson, who had already assessed that by the end of the 1970s there was "almost as much political mileage to be gained from fighting inflation as from fighting a recession" (*Financial Times* (London), December 31, 1979) remarked (in *Newsweek*, May 19, 1980) that "two-digit inflation was so intolerable that it was considered good election-year politics for a Democrat to fight it with recession."

[42] From Kahn's appearance on CBS' *Face the Nation* program, as reported in *Manchester Union Leader* (New Hampshire), October 15, 1979.

In other respects, however, the administration was at cross-purposes with the Federal Reserve on the need for sustained restriction. His support of restraint notwithstanding, Alfred Kahn maintained until shortly before the president acknowledged a recession that the need for output to contract might be circumvented by adherence to the administration's wage-price guidelines. And President Carter's own emphasis on restraint also receded as the year progressed.[43] In the October 1980 presidential debate, he implied that the brief recession that had occurred was all that had been needed: "The recession that resulted this time was the briefest since the Second World War. In addition, we've brought down inflation. Earlier this year, in the first quarter, we did have a very severe inflation pressure brought about by the OPEC price increase. It averaged about 18 percent in the first quarter of this year. In the second quarter, we had dropped it down to about 13 percent. The most recent figures, the last three months, on the third quarter of this year, the inflation rate is 7 percent—[though that's] still too high…"[44]

The Carter Administration also had a mixed record in its final eighteen months in office with regard to the support provided to the Federal Reserve. This issue was raised in a newspaper editorial that appeared a few weeks after the October 1979 launch of Volcker's new policy arrangements. Having noted, "What may prove to be a historic turning point in government policy has been reached," with the Federal Reserve in effect drawing on the advice given publicly by "Milton Friedman and other 'monetarist economists," the editorial concluded: "President Carter should stick with Volcker, follow his advice and back him up." (*The State-Journal* (Springfield, Illinois), October 26, 1979.)

In judging whether this advice was followed, it deserves emphasis that it would not be correct to suggest that Carter gave the monetary policy tightening his unqualified backing. Meltzer (2009b, p. 1035) stated that President Carter did not criticize Federal Reserve policy publicly at all during the 1980 election campaign. But this claim is shown to be an overstatement: Meltzer's later discussion (p. 1065) itself acknowledged that Carter criticized the Federal Reserve on at least one occasion during the campaign—in October 1980, after the Federal Reserve Board had increased the discount rate (see also *Wall Street Journal*, October 15, 1980, and Silber, 2012, pp. 190–191, 267).[45]

[43] See also the previous chapter.

[44] "October 28, 1980 Debate Transcript: The Carter-Reagan Presidential Debate," p. 3.

[45] The discount rate was raised on September 25, 1980—a 100-basis-point increase to 11 percent (Federal Reserve Board, 1981, p. 82)—and Meltzer, without providing sources, gave Carter's public interventions as occurring in September as well as October. The president's most publicized remark on the matter, however, was on October 2: see Carter (1980d). In response, at a Reagan campaign event, former president Gerald Ford criticized Carter's

Indeed, Carter's unease regarding demand restraint as a tool against inflation had been manifested publicly almost a full year earlier, very soon after the Federal Reserve launched its October 1979 measures. On October 11, 1979, Carter told a labor-union audience, "Austerity is unavoidable and inevitable—we can no longer postpone it" (*Daily News* (New York), October 12, 1979)—yet also stated that interest rates were too high and that "I will not fight inflation with your jobs" (*Manchester Union Leader* (New Hampshire), October 12, 1979; *Daily News* (New York), October 12, 1979, p. 2). Likewise, although Meltzer (2009b, p. 1025) highlighted the fact that, during the early fall of 1979, Carter did "not threaten to oppose" the major Federal Reserve policy change that was subsequently announced on October 6, 1979, it is the case that the administration economic team both had misgivings about that change ahead of its introduction and regretted its introduction after the fact—features of the administration's reaction that Meltzer partially acknowledged but did not stress.

Publicly, it is true, Secretary of the Treasury Miller stated, soon after the New Operating Procedures were introduced, that "the monetary actions of the Federal Reserve, recently taken, are both desirable and appropriate."[46] But Lyle Gramley, who had the vantage point of the White House as a member of Carter's Council of Economic Advisers, recalled that "when he [Volcker] introduced his change in *modus operandi* in October, the Carter Administration was strongly opposed to that" (interview, June 24, 2013)—a recollection consistent with the statement by Carter's domestic adviser Stuart Eizenstat, who expressed regret that, when Volcker was nominated, "it was not clear that he had a monetarist background," and stated that when the new Federal Reserve chair foreshadowed his change in policy procedures to the administration, "We were all concerned that the policy would lead to too much uncertainty in interest rates. That view was communicated to the Fed, to no avail…" (*New York Times*, June 19, 1983.) Furthermore, in October 1980, Secretary of the Treasury Miller publicly voiced misgivings about the Federal Reserve's monetary policy that were similar to those President Carter articulated earlier in the month.[47]

All this said, however, it is also the case that the details of the record of monetary policy during 1980 do not support various suggestions—including some made by Friedman—that the Federal

comments. See *Washington Post*, October 4, 1980 (p. C6), and *The Plain Dealer* (Cleveland, Ohio), October 4, 1980.

[46] From G. William Miller's testimony of October 17, 1979, in Joint Economic Committee, U.S. Congress (1980b, pp. 5–6).

[47] Miller was quoted as having remarked during this period, "It's in everyone's interest to avoid a runup in rates that will abort the recovery," while also stating that the Federal Reserve's monetary policy responsibility was "broader than mechanistic adherence to regulating the money supply." (*Wall Street Journal*, November 11, 1980*b*, pp. 1, 19. Earlier quoted in *El Paso Herald-Post* (Texas), October 21, 1980.)

Reserve succumbed to political or public pressure during 1980 and so made a policy *U*-turn. In particular, as discussed in Section II below, it is not valid to conclude that the Federal Reserve abandoned its disinflationary policy for a time ahead of the 1980 presidential election.

Recession and protracted disinflation

Friedman was emphatic that a recession could not be avoided as part of the disinflation process. His *Newsweek* column of November 12, 1979, appearing early in the period of the new Federal Reserve procedures, included the observation: "I know of no example of a country that has cured substantial inflation without going through a transitional period of slow growth and unemployment." This passage would be largely repeated in the Friedmans' *Free To Choose* book, which appeared a few months later.[48] It would also be quoted in successive editions of Dornbusch and Fischer's textbook (for example, 1984, p. 416; 1987, p. 499) and be described as a "well-known observation" of his in Kashyap, Lamont, and Stein (1994, p. 569). In all cases, his quotation was used because it encapsulated Friedman's belief in the short-run real effects of monetary policy.

That belief was also evident in another late-1979 statement, when Friedman insisted to a reporter that—contrary to what some administration economists were implying—the real costs of disinflation could not be sidestepped (*The Register* (Santa Ana, Orange County, California), December 23, 1979, p. E11): "No, there is no way to avert the recession part. Once you have expanded too rapidly, once you had too much of an inflationary episode, there is no way of getting out of it without going through a recession." "There's no easy fix to it [inflation]," he elaborated the following March (*San Antonio Light* (Texas), March 30, 1980). "It'll require two to three to four years of subnormal economic growth and involve an increment of high unemployment, also." Various predictions Friedman made during the 1980s would go astray, but this one was accurate: real GDP growth averaged 4.4 percent per year during the 1977−1979 years of accelerating inflation, 0.1 percent during the 1980−1982 years of Volcker restriction, and 5¼ percent during the 1983–1985 period when inflation was back in low single digits (Council of Economic Advisers, 2011, Appendix B, p. 192).

The 1980s experience in the United States became a new empirical example of Friedman's propositions about the monetary policy-demand-inflation connections. These propositions

[48] See Friedman and Friedman (1980a, pp. 276–277), in which "unemployment" was expanded to "higher than usual unemployment."

brought together his work on the money/inflation link and his analysis of the Phillips curve. They stated that ending inflation (*i*) could not occur without monetary restriction, (*ii*) required only monetary restriction, and (*iii*) would not occur immediately nor without an interim period of a weaker real economy. Even before the Volcker disinflation provided further validation of them, these propositions had acquired wide acceptance in the economic community—a state of affairs reflected in William R. Allen's observation in mid-1981 that Friedman is "the chief architect of modern monetary analysis and its use in assessing monetary policy."[49]

At the start of the 1980s, Friedman had had an opportunity at the Florida NBER conference to see how his views on inflation and its connection to real variables had become the consensus of macroeconomic opinion. He was pleased with what he called Robert Gordon's "perceptive" paper overviewing macroeconomic developments,[50] Gordon (1980) indicted the economics profession for "[i]gnoring the lone voices of Milton Friedman and Edmund Phelps to whom few listened in 1966" (p. 130) and noted that, following his graduation from MIT in the late 1960s, Gordon had "almost immediately found [that] graduate school education incapable of explaining the evolution of the economy" (p. 134).

Gordon's observations were made all the more poignant by the fact that not only was Friedman among the conference participants, but so too was Paul Samuelson. Samuelson himself observed the following spring (*Newsweek*, April 28, 1980) that "the price level is the hardest variable for econometricians to forecast and they have chronically been too optimistic in their inflation estimates" in recent years—although he did not mention that, unlike econometricians, Friedman had been able to forecast the double-digit inflation of 1979−1980.

A number of the same econometricians to whom Samuelson referred had produced simulation results that, although not denying outright that monetary restriction could end inflation, implied that very large losses in employment and output would be associated with only incremental improvement in inflation. In Chapter 5 above, Okun (1978) was noted as a celebrated example of such studies, and other chapters have pointed to the extent to which leading Keynesians such as James Tobin and Walter Heller stressed that those studies implied low dividends, in terms of inflation reduction, would flow from demand restraint.[51] Confronted with estimates attributed to

[49] Allen (1981, p. 55).

[50] Friedman (1980d, p. 82).

[51] Early in the period covered in this chapter, James Tobin testified to a Congressional committee that "an extra point of unemployment appears to bring down the inflation rate down by a quarter or at most a half a point in a year." Even in giving these estimates, Tobin likely felt that he was being charitable toward demand-based approaches to disinflation, as he also suggested that reducing the growth rate of nominal spending was "not a

Okun as well as a Data Resources Incorporated estimate that getting inflation even below 8 percent would require a long recession with an average 9 percent unemployment rate, Friedman remarked in mid-1980: “Those studies have no validity at all.” (*The Spectator* (Hamilton, Ontario, Canada), July 15, 1980.)

This was a blunt assessment—especially so in view of the fact that one of the authors of the studies, Arthur Okun, had died suddenly only a few months earlier (on March 23). But the substance of the assessment was largely borne out by later developments. The early-1980s disinflation supported the view that prior studies had overstated the size and duration of the negative output gaps required to bring U.S. inflation to low single digits. They did so in part because the information set on which they were relying would be superseded by still further reassessments, taking place over the 1980s, of the extent to which the U.S. economy had been overheated during the late 1970s. Even the 1979 official revisions to potential output had, it turned out, not gone far enough: Orphanides (2003, p. 655) would show that the CEA’s early-1979 assessment was that recent quarters’ output gaps were around minus 2 percent—a contrast with later estimates that, by that point, output had already overshot potential again by about 2 percent. The notion that output gap mismeasurement had been a major factor behind excessive policy ease during the 1970s was consequently reinforced.[52] The flipside of this was that, although the policy tightening that began in the late 1970s eventually produced a notably negative output gap, in the initial stages the demand restriction merely made the positive output gap *less positive*, thereby pushing output down closer to its potential level.

Ronald Reagan and disinflation

Despite his strong difference with them with regard to quantitative estimates, Friedman did not disagree with his critics on the point that disinflation would be a lengthy and costly process: “Even the best policies will take time and involve a difficult transition.” (*Newsweek*, March 24, 1980.) But he did not see the real costs as truly avoidable: one could “accept a temporary economic slowdown now as part of a program for ending inflation,” he noted in June 1980, or instead “experience a more severe slowdown somewhat later as a result of continued or accelerated inflation.”[53]

sufficient condition” for removing inflation, and that, with regard to a determined and prolonged restriction of aggregate demand, “no one can be sure whether or by how much the inflation rate will be reduced after several years of this cure.” (All quotations are from Tobin’s testimony of March 4, 1980, in Committee on the Budget, U.S. Senate, 1980, p. 120.)

[52] Taylor (1979, p. 562) had discussed this interpretation, crediting it to William Fellner.

[53] Friedman (1980b, p. 56; p. 51 of 1991 reprint).

With the Federal Reserve, if not the Carter Administration, now accepting inflation's monetary character, Friedman told a monetary conference in New Orleans at the start of June 1980 that the "primary obstacle to ending inflation" was not "a lack of knowledge [of] how to end inflation"—rather it was the potential "lack of political will to take the necessary measures."[54] Paul Volcker was present at that conference, and Friedman would go through a roller-coaster of shifting opinions about whether Volcker was, in fact, persevering with the "necessary measures." Speaking in October 1980, Friedman—despite his great dissatisfaction with how the Federal Reserve had actually conducted policy over the past year—expressed hope that the early-1980 data had corresponded to what "may be the final spurt" in the upward trajectory of U.S. inflation, "with a downward rather than an upward trend" to characterize the rest of the century.[55] In another talk during the same period, Friedman referred to the "reaction against the recession and unemployment" in U.S. public debate and implied that provided that reaction was resisted in favor of monetary restraint, this restraint and other policy moves should permit "a strong rebirth of economic growth and a decline in the rate of inflation to a low level over the rest of the eighties."[56] By the end of the year, however, he was questioning whether the recent monetary-growth record (in particular, the rebound in monetary aggregates since around May 1980) justified such optimism—or even the conclusion that the Federal Reserve was really fighting inflation (*Newsweek*, December 29, 1980).

Friedman had not, however, abandoned hope that the current episode might still turn out, on balance, to be one of sustained monetary restriction. He saw the incoming Reagan Administration as offering an opportunity for securing the needed disinflation. Friedman later expressed satisfaction with the passage in Reagan's speech of September 9, 1980, by which time he had received the presidential nomination, in which the Republican candidate stated: "We must establish a stable, sound, and predictable monetary policy."[57] Reagan could, Friedman believed, reinforce the Federal Reserve's inclination to exercise monetary restraint by setting a supportive tone. And, in Friedman's view, Reagan could go beyond this—as Friedman was not opposed in principle to the executive branch putting public and private pressure on Federal Reserve policy, as long as that pressure was in the direction of promoting a noninflationary policy.

The change in administration certainly ended any basic disagreement between Volcker and the executive branch regarding the monetary nature of inflation. Reagan had told a media

[54] Friedman (1980e, p. 2).
[55] Friedman (1990d, p. 36). Also quoted in *Pittsburgh Press*, November 9, 1980.
[56] Friedman (1990d, p. 49).
[57] Reagan (1980a). Also quoted in Friedman and Friedman (1984, p. 1; 1985, p. 11).

convention in spring 1980: “Inflation is caused by government. The price of goods can go up only if the government creates too many dollars.” (*The Sun* (Baltimore), May 1, 1980, p. A8.)

Nevertheless, for different reasons, both Carter and Reagan had reached Election Day 1980 giving voters the impression that the brief 1980 recession had comprised the entirety of the period of negative real economic growth that would need to be associated with the Federal Reserve’s monetary restraint. To be sure, Reagan himself appreciated the basic problem of ending restraint too early, remarking in 1980 (Hayward, 2009, pp. 194–195): “We know that we have to have a consistent monetary policy that doesn’t do what we've done over the last few decades, of the roller-coaster effect—of when unemployment gets out of hand and it looks like hard times, they flood the market with paper money. And then when that brings on inflation, then, all of a sudden, you pull in and tighten it down and you go the other way.” This was a realistic description of past patterns. But it was a hardly palatable prospect for the new president and many of his subordinates that going “the other way” in the direction of weaker real economic activity was the way in which to start his presidency.

In fact, Reagan took office amid talk of sustained economic recovery. Public discourse only half-acknowledged that the U.S. economy was actually back in an expansion phase as of early 1981: the aftereffects of the 1980 recession were still present, interest rates were back to historical highs, and indeed some commentators blamed Friedman for advocating the changes in monetary policy that had produced a poor condition of the economy.[58] As indicated in the previous chapter, Reagan himself encouraged the conclusion that his administration started from a state of recession, and that his term would be associated with continuous economic growth from its first year in office, by calling his economic program a recovery plan.

This message was reinforced by the administration’s forecasts. Instead of starting his administration with an admonition that both real and nominal aggregate demand would be restricted in the immediate future, Reagan provided economic projections at the start of 1981 in which aggregate real output growth was positive throughout the forecast horizon and in which nominal income growth did not exhibit the slowdown implied by the FOMC’s monetary targets (*Washington Post*, March 28, 1981; Tobin, 1981a, p. 9; M.K. Evans, 1983, pp. 5, 28–29).

This was not altogether a new experience for Friedman. Richard Nixon had been elected in 1968

[58] For example: “Milton Friedman’s theory of monetarism too over the U.S. Federal Reserve… The results were the same: rising unemployment, a wave of business bankruptcies, and no noticeable brake on inflation.” (*The Gazette* (Montreal, Quebec, Canada), January 8, 1981.)

saying, to Friedman's disapproval, that his disinflation program would not produce a recession. In the event, Nixon explicitly endorsed a program, to be implemented by the administration and the Federal Reserve, of restriction of aggregate demand, and a recession did occur. This experience likely informed how Friedman interpreted the Reagan Administration's early denials that a recession would be necessary. Friedman had himself remarked that he would compromise if he was in public office (*Seattle Times*, October 22, 1976), and it is in that light that he evidently interpreted the recession-free projections issued by the Reagan Administration. He had no doubt that renewed recession would be necessary to bring inflation down, and what mattered, especially in view of the Nixon precedent, was Reagan's resolve once that recession occurred. "The main problem democratic societies face in trying to conquer inflation is that they do not have the will to stick to it."[59]

Another economic adviser to Reagan during the 1980 campaign, George Shultz, had faulted Carter for his public criticism of the Federal Reserve's tightening moves (*Washington Post*, October 4, 1980, p. C6). Shultz's retrospective judgment was that Reagan's conduct as president in his first two years in office provided a major contrast with the Carter example. The Reagan posture, Shultz believed, reflected "steadfastness" and the fact that the economic perspective that Reagan took "wasn't just a political view." Shultz observed further: "He held—I've talked to Paul Volcker about this—he held a political umbrella over Paul Volcker. And his political people would run in and say, 'Mr. President, this [restrictive monetary policy] is going to cause unemployment, and we'll lose seats in the midterm elections.' And he basically—Reagan basically said, 'If not us, who? If not now, when? You've got to do this if you're going to have a decent economy.' So I thought in many ways that was his finest hour domestically—because he took a short-term hit, knowingly, for a long-term gain." (George Shultz, interview, May 22, 2013.)

Reagan also provided some explicit backing for monetary restraint—such as when he stated in a speech in Chicago in September 1981: "The Federal Reserve is following a conservative and careful approach to the money supply, which will ensure that once recovery begins, it won't kick off another round of inflation." (*Financial Times* (London), September 4, 1981.)[60] Consistent with this public stand, when testifying late in the following month, Paul Volcker remarked of the administration's posture on monetary policy: "They have been supportive of the general thrust of

[59] Friedman (1981e, p. 13).

[60] Bernanke (2010, p. 10) also offered as an example a later (February 1982) Reagan public endorsement: "This administration will always support the political independence of the Federal Reserve Board."

policy, if I may put it that way."[61]

In light of the record, the review of the course of monetary policy in the 1980s by Mussa (1994) suggested that "the most important contribution of President Reagan to the fight against inflation" consisted of self-restraint: not pressuring the Federal Reserve to ease policy during 1981–1982.[62]

Friedman, too, would come to be a major proponent of the notion that Reagan's conduct in 1981 and into 1982 played a crucial role in allowing a disinflationary policy of monetary restriction to proceed. This was, however, mainly an interpretation that he expounded in retrospective accounts, starting in the late 1980s, rather than at the time.[63]

In contrast, during the early 1980s, especially in 1981, Friedman's judgments concerning monetary policy were tempered by the conflicting evidence coming from M1 and M2 about whether the Federal Reserve stance was tight. However, the cloudiness of the picture had lifted by early November 1981: in light of sustained slow M1 growth and the emergence of a new recession, Friedman had come round to the view that monetary policy was tight and had been so for much of the year.[64]

Having reached the judgments that monetary policy was restrictive and U.S. aggregate demand had responded to the monetary policy tightening, Friedman expressed the wish that the recession would prove to be unlike those in the past, including the recession in the early Nixon years, whose disinflationary benefits were dissipated by not being followed by "overly stimulative monetary policy to cope with the recession," and that policymakers not ease even if the recession worsened through mid-1982 (Oppenheimer and Company, 1981b, p. 11). He worried that the Federal Reserve and Reagan would be pressured to reverse course "by stepping on the gas, pouring more money into the [system]" (*Detroit Free Press*, November 6, 1981). This pressure should be resisted, he argued, even if the recession worsened through mid-1982.[65]

[61] Testimony of October 29, 1981, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1981c, p. 552).

[62] Mussa (1994, p. 135). Bernanke (2010, p. 10) stated along similar lines: "President Reagan's support for Volcker's politically unpopular disinflationary policies and for the principle of Federal Reserve independence proved crucial to the ultimate victory over inflation."

[63] See Chapters 14 and 16 below.

[64] For details, see the discussion titled "Henry Kaufman" at the end of this chapter.

[65] Oppenheimer and Company (1981b, p. 11). This echoed many remarks Friedman had made in the early Nixon years—such as in 1970, when he expressed the hope that "the administration has the courage and the foresight to stick to the policy it's been following" (Friedman, 1970d, p. 73).

The Federal Reserve's future

Although his November 1981 remarks did call for the Federal Reserve to resist pressure to change course, Friedman was very far from a friendly commentator with regard to the Volcker Federal Reserve. His open-minded, and sometimes favorable, initial perspective on the post-October 1979 regime had evaporated rapidly as swings in monetary growth started showing up in 1980. By the time Reagan was elected president in November, Friedman was very much focused on seeing that more impetus was provided, including in the form of Congressional and administration pressure, for the Federal Reserve to overhaul its monetary-control arrangements.

"Reagan had a high respect for Milton," George Shultz recalled (interview, May 22, 2013), and this fact worried Arthur Burns. Burns and Friedman had been back on friendly terms for some time, and Burns' wife Helen indicated that the poker games that Friedman and Arthur Burns first played at Rutgers University in the early 1930s continued fifty years later (*The Times-Picayune* (New Orleans), June 22, 1980, p. 4). Furthermore, Burns and Friedman gave a surface impression of agreement on policy matters by being co-signatories with George Shultz and others in their November 1980 memorandum to president-elect Reagan. But, even before their crucial rift in the early 1970s, they had not seen eye-to-eye on economic matters, as Burns had noted in his remark in early 1969 that he and Friedman had very heated but friendly discussions as summer neighbors (*Time* magazine, February 7, 1969). Burns' views on the role of monetary policy in inflation control had then changed in 1970 and, most likely, never fully changed back. Furthermore, he had never subscribed to Friedman's advocacy of assigning a strict policy rule to the Federal Reserve, and this attitude had been reinforced by his years as head of that body.

Paul Volcker recalled that, as of late 1980, "Arthur Burns was really upset with Milton Friedman and other conservative Republicans that he thought were out to get after the Federal Reserve when Reagan became president. He was very upset, because he thought Friedman was kind of the intellectual leader of that—what he saw as a building of a coup against the Federal Reserve. I never saw him so upset as when he (*laughter*) was at some meetings with them—I guess in the interregnum between Carter and Reagan." (Paul Volcker, interview, October 16, 2013.)[66]

Arthur Burns would have clout with Reagan himself and would serve in the administration.[67] But

[66] In Kohn and others (2019, p. 55), Volcker named Friedman and William Simon as the Republican economists who Burns feared wanted to have the Reagan Administration "taking over the Federal Reserve."

[67] With the exception of President Johnson—whose economic policy he heavily criticized from outside government—and President Carter—who removed him as Federal Reserve chair—Burns developed good relations with all presidents from 1953 through 1987. In the cases of most of the Republican presidents, Burns' relationship

he evidently feared that those more critical of existing central-bank arrangements might prove more influential than he was.

Burns emphasized his concerns when discussing Friedman and the incoming Reagan Administration in a meeting with Volcker on November 19, 1980. In particular, at this meeting, Burns stressed Friedman's contention that the Federal Reserve should be replaced by a computer (see Silber, 2012, p. 194).[68]

This particular Friedman maxim was of long standing. Near the end of 1976, in his remarks in Sweden on receiving his Nobel, Friedman was quoted saying (*Chicago Sun-Times*, December 11, 1976, p. 58): "my monetary studies have led me to the conclusion that central banks should be replaced by computers."[69]

The vision of turning central bank policy into a computerized process was one to which Friedman returned many times. At the aforementioned New Orleans conference in early June 1980, it had been suggested at Friedman's lunch table that his proposals for a monetary rule implied a state of affairs in which monetary policy could largely be carried out by a computer rather than policymakers. "Indeed," Friedman replied (*New York Times*, June 8, 1980, p. F6). Over the rest of the 1980s, the desirability of replacing the Federal Reserve became a repeated Friedman refrain in interviews and op-eds, and this continued even into the 2000s.[70]

was well developed before they took office. Of course, this sequence of Burns' relationships with presidents started with Dwight Eisenhower in 1953 when Burns headed the CEA. Joseph Burns recalled (interview, September 12, 2013): "Eisenhower got along very well with my father… It was a much friendlier relationship than [Eisenhower had] with [Vice President] Nixon." Joseph Burns added the caveat that this relationship really began when Eisenhower took office, remarking of the pre-1953 situation: "They didn't know each other. I've seen articles suggesting that, because Eisenhower was president of Columbia, he knew my father from Columbia days. Actually, my father may have been once on a receiving line and shook his hand, and that was about it." Arthur Burns' own recollection singled out his main pre-1953 interaction with Eisenhower as being when Eisenhower exchanged pleasantries with Burns and a group of other Columbia University officials who were members of the procession at Eisenhower's 1948 installation as university president (Burns, 1969, p. 6; Ewald, 1981, p. 168). The fact that Burns served in the Reagan Administration meant that he also ended up having a closer relationship with President Reagan than Friedman did.

[68] In the area of specific policy decisions, members of Reagan's election economic team such as Friedman, Alan Greenspan, and George Shultz were also critical of the Federal Reserve for having allowed monetary growth to take off since mid-1980 (see *Wall Street Journal*, November 11, 1980*b*, p. 1). This was not a matter on which Burns likely had much disagreement with Friedman.

[69] Also quoted in Gordon (1978, p. 351), using mimeographed version of the remarks—with "should" instead being "could profitably"—that Friedman had supplied him. For the Friedmans' own recitations of these remarks, see R.D. Friedman (1977, p. 24), Friedman (1986a, pp. 78–79), and Friedman and Friedman (1998, p. 453).

[70] For example, *Wall Street Week*, Maryland Public Television, April 27, 1984, p. 10 of transcript; *Wall Street Journal*, December 10, 1984, p. 16; *San Francisco Examiner*, March 18, 1985; *Wall Street Journal*, April 15, 1988, and July 5, 1989; and his remarks in Pringle (2002, p. 19). In addition, in a hearing held on January 26, 1983, Senator Steven D. Symms noted that "my friend, Milton Friedman, said to me one day that if you'd fire the Federal

Although his relations with Friedman had improved greatly in recent years, Burns in 1980 still found Friedman's hostility to the Federal Reserve as an institution jarring. He was concerned that Friedman would use his influence to curtail Federal Reserve independence, and, in that connection, he apparently took Friedman's references to the computer-operated policy literally.

Friedman was certainly not a sympathizer with central bank independence, and this was reflected in his attitude to the role he thought the new administration should play. It was publicly known in 1981 that Friedman was urging Reagan to be outspoken about monetary policy (*New York* magazine, April 27, 1981, p. 16). Of the Federal Reserve, Friedman remarked, "You have a large bureaucracy that's running itself." It had an "incredible capacity" to resist outside pressure, he suggested (*Evansville Press* (Indiana), May 22, 1981). He did not regard this capacity as a plus when, as in the present case, the pressure was in a direction he considered desirable: toward improving its monetary-control arrangements. Friedman also declared in May 1981: "After studying the Fed for [its] 67 years, I have no doubt that the United States would be better off if the Federal Reserve had never been established." (*Daily News* (New York), May 22, 1981.) In the same period, he stated that more Congressional control of the Federal Reserve was needed (*Jersey Journal* (Hudson County, New Jersey), May 22, 1981).[71] This observation was made before the 1981–1982 recession. It came at a time when prominent Congressional voices were perceived as supportive of the Federal Reserve being focused on tighter control of the monetary aggregates—something that would be transformed over the following year when the Congress largely became a source of calls for much easier policy.

But Burns' concerns that Friedman's views on Federal Reserve independence would be felt in Reagan Administration policy toward the institution would prove to be overblown. Friedman would be only an irregular and outside Reagan adviser, and on those occasions when he did see the president, it was typically in the presence of supply-side economists who felt that Friedman already had too much influence on the course of U.S. monetary policy.

As for the idea of replacing the Federal Reserve by a computer, Volcker was surely right to see it as a metaphor rather than a serious prescription. That was how he replied to Burns' concern at their meeting (Silber, 2012, p. 194). In the same manner, Ann-Marie Meulendyke, a former graduate student of Friedman's who by the early 1980s was serving on the New York Federal

Reserve Board and put a computer clerk over there to run it, so we could have some stability" (Joint Economic Committee, U.S. Congress, 1983a, p. 44).

[71] As he had in the past, Friedman pointed to the historical record as having convinced him that tighter Congressional control would have produced monetary policy patterns that were more economically stabilizing.

Reserve Open Market Desk, observed: "Friedman *publicly* used to say they should replace the Fed by a computer... He didn't [want that], really. I think he said that for attention… He had a much more nuanced and sophisticated understanding of money demand and supply to really think you could operate that way." Meulendyke associated Friedman's invocations of a computer-operated monetary policy with occasions "when he used to talk in non-professional environments. I remember seeing him on Louis Rukeyser's *Wall Street Week* once time, [saying] sort of, 'Well, you just have a machine and, well, every week it spits out *X* dollars of your money.' He didn't [really] expect that, or believe that, or favor it." To Meulendyke, the computer recommendation was a way of expressing Friedman's genuine wish to ringfence monetary policy decisions from "the discretion that came from feelings, perceptions, market chatter, and so forth." (Ann-Marie Meulendyke, interview, April 29, 2013.)[72]

For his part, although Friedman insisted that he was serious about the proposal (*American Banker*, April 30, 1986, p. 20), this may have been tantamount to saying he was serious about wanting a monetary-growth rule.[73] Meulendyke's view that Friedman formulated the suggestion so that it was attention-grabbing has merit. The imagery of replacing the Federal Reserve with a computer was, in effect, a restatement of Friedman's longstanding wish for an automated rule to determine monetary policy. It provided a vivid way of expressing in the electronic age Friedman's longstanding proposal that monetary policy responsibilities revert o the U.S. Treasury and thereafter be conducted according to a nonactivist rule.

Furthermore, despite his opposition to central bank independence on principle, Friedman's own practical policy prescriptions in 1980 and 1981 conditioned on the continuation of the Federal Reserve as an independent agency. Speaking in June 1980, he suggested that there be "announced by the Federal Reserve, with the concurrence and support of the president… a five-year program of gradually bringing monetary growth down to a level consistent with no inflation" (*Cover Story: Inflation—It's Only Your Money*, PBS, July 1, 1980). And in a lengthy talk "Monetary Policy: Theory and Practice," delivered at the Western Economic Association meetings in San Francisco on July 5, 1981, Friedman laid out an eight-point proposal of suggested changes to U.S. monetary policy operation that—although extensive and implying major limitations on policymakers' discretion—was conditional on the Federal Reserve

[72] In contrast, Paul Samuelson observed of Friedman's computer proposal: "A lot of people think he is not serious [when he makes it], but he means to be." (Quoted in *San Francisco Examiner*, March 18, 1985.)
[73] Similarly, in *Wall Street Journal*, November 17, 1989, Friedman remarked that it would be desirable "to put monetary policy on automatic pilot outside the control of the Fed or the Treasury or Congress."

remaining in existence.[74]

The new Treasury team

The departure of Jimmy Carter as president in January 1981 also brought with it the exit of G. William Miller as Secretary of the Treasury. He had served in that position for almost eighteen months since relinquishing the position of Federal Reserve chair. During that year-and-a-half period, Paul Volcker had enjoyed more prestige heading the central bank than Miller had experienced in his own 1978–1979 service in the position.[75] Miller himself acknowledged the contrast, observing: "I guess it was because I came into government as a businessman from the outside. That apparently made me fair game." (*Chicago Tribune*, October 14, 1980.)

Miller kept a very low profile once he left public office in January 1981. Benjamin Friedman, who knew Miller during his years in public office and saw him on occasion thereafter, remarked: "My reading is that this is a guy who had been extremely successful in business, and who understood that he'd had these two very high-level government assignments, and had not gone out in good reputation from either one of them." (Benjamin Friedman, interview, July 23, 2013.)

In Miller's place as Secretary of the Treasury came Donald Regan.[76] The broad backing that the Reagan Administration gave to Volcker's disinflationary monetary policy, including the mostly supportive stance taken by the president, ran in parallel with a practice by the Treasury under Regan of making regular public commentary on monetary policy—including numerous specific criticisms of policy operations and emerging short-term patterns. In the case of Regan, his main critical remarks on the Volcker Federal Reserve (as well as his public disputation with Volcker about fiscal policy) did not come until 1982.

In contrast, Beryl Sprinkel—who was serving under Regan in the Under Secretary for Monetary Affairs position in the U.S. Treasury that had once been held by Volcker—had after only about three months in office already acquired a reputation of "slugging away at the Fed in public"

[74] Friedman (1982c, pp. 116–117).

[75] Volcker did, however, go through periods during 1981 and 1982 of being out of favor in financial markets. For example, a new bout of volatility in the federal funds rate in late 1980 and the early months of 1981 led to the commentary: "Criticism of the Fed from bond dealers has reached a fever pitch." (*New York* magazine, April 27, 1981, p. 16.) See also the discussion titled "Henry Kaufman" at the end of this chapter.

[76] Regan, Volcker, and Friedman all attended the American Bankers Association meeting in San Francisco in October 1981. Friedman's participation in the conference included being the guest speaker at a luncheon hosted by Citicorp (*New York Times*, October 11, 1981).

(*New York* magazine, April 27, 1981, p. 16).[77] A reference to Sprinkel as a "devout disciple of Milton Friedman" was representative of the manner in which this former Friedman student was described in media accounts (*Financial Times* (London), June 30, 1981). Sprinkel's public criticisms of Federal Reserve policy centered on volatile monetary-growth outcomes and the control methods that he believed lay behind these outcomes. Although he acknowledged that "effective control of monetary aggregates… may not (and need not) be achievable on a weekly or monthly basis," Sprinkel contended that it was achievable "over a longer span" and, like Friedman and other monetarists, he argued that current Federal Reserve operating procedures, most notably lagged reserve requirements, frustrated the attainment of better short-run control (*Wall Street Journal*, August 5, 1981).

Stephen Axilrod, who as a senior Federal Reserve Board staff member in the monetary policy area was involved in the dialogues with Sprinkel, remarked: "I think the hostility felt by Sprinkel, who was not a hostile man—he was a lovely guy, actually—was simply because he believed that monetary policy, money supply, was the most important thing to control, and that we didn't know what we were doing—weren't controlling it. And that drove him crazy." (Stephen Axilrod, interview, April 24, 2013.)

Ultimately, Sprinkel's pattern of repeated public criticisms of the Federal Reserve was thought to have undermined the appearance of a united front by the U.S. government on economic policy.[78] Sprinkel's behavior was believed to have wrecked his chances of being considered a serious candidate when the administration contemplated whether to replace Paul Volcker when the Federal Reserve chair's first term was expiring in 1983 (*New York Times,* March 9, 1983).

James Tobin receives the Nobel award

1980 and 1981 were back-to-back years in which Nobel awards in economics went to leading American Keynesian economists. Each of these U.S. Keynesians had been at the opposite ends of debates with Milton Friedman. The 1980 Nobel winner, Lawrence Klein, had defended Keynesian economics against some of the reasonably early Friedman criticisms that appeared in

[77] For an early case in which Sprinkel publicly called for changes in Federal Reserve control methods and for the FOMC to "restore credibility to anti-inflationary efforts," see his testimony of April 8, 1981, in Joint Economic Committee, U.S. Congress (1981b, p. 7).

[78] At an early stage of Reagan's tenure, it looked like the criticism voiced by Treasury officials of Federal Reserve operating procedures would be expressed more definitively and authoritatively by the administration in a public document on economic policy. The president's outside advisers Arthur Burns and Alan Greenspan were reported as having successfully argued against the release of such a critique. See *The Times-Mail* (Bedford, Indiana), May 9, 1981.

the 1950s. The 1981 recipient was James Tobin, with his award announced on October 13.[79] Tobin had been a wide-ranging critic of Friedman's research in monetary economics—especially the work that had appeared in print during the 1960s, including that on the Phillips curve.

By 1981, Friedman and Tobin rarely saw each other, but they were well established as major sparring partners. When Friedman in 1970 had written of his "great respect and admiration for James Tobin's scientific ability," it was as a qualifying remark in the context of a strongly negative judgment of an article specifically challenging the Friedman and Friedman-Schwartz conclusions concerning money.[80] Over the subsequent decade, however, it was the monetarist challenges to Keynesianism, rather than the Keynesian challenges to monetarism, that were perceived as having largely prevailed—and so when Tobin won the Nobel, he remarked that, in "some circles," the formulation "discredited Keynesian" had "become one word, like 'damn Yankee'" (*Kansas City Times* (Missouri), October 14, 1981).

A new article specifically on monetarism by Tobin had appeared in the *Economic Journal* in March 1981 (Tobin, 1981b), and Tobin testified against monetary targeting at sessions of legislative committees in both the United Kingdom (on July 29, 1980) and the United States (on October 6, 1981, a week before the Nobel announcement).[81] Little wonder that when Tobin won his Nobel, press coverage highlighted his status as a critic of Friedman and of monetarism (*Chicago Tribune*, October 14, 1981; *Sunday Times* (London), October 18, 1981)—with one report headlined "Monetarism Critic Wins Nobel Prize" (*Daily Telegraph* (London), October 14, 1981).[82]

The year 1980 had also seen the publication, as the book version, *Asset Accumulation and Economic Activity*, of the 1978 Tobin lectures that were discussed in Chapter 8 above. The title of the book suggested that the book might concentrate on modeling financial markets.[83] It

[79] The following month, a U.S.-based research award made headlines when the MacArthur Foundation grants were announced (*Democratic and Chronicle* (Rochester, New York), November 18, 1981). One of the recipients, Michael Woodford, then an MIT graduate student, would later become a major name in monetary economics.

[80] The remark was in Friedman (1970e, p. 327), in the course of a response to Tobin (1970a).

[81] See Treasury and Civil Service Committee, House of Commons (1981, pp. 208–222) and Joint Economic Committee, U.S. Congress (1982a).

[82] Some of this coverage quoted Assar Lindbeck, the economics prize committee chairman, who on the day of the award talked to the media about Tobin's differences from Friedman with regard to the nature of the economy's adjustment to a restriction of aggregate demand (*The Post* (Palm Beach, Florida), October 14, 1981).

[83] Tobin (1980a). Likewise, although the press coverage of Tobin's Nobel focused on his long-running disputation with Friedman, the award itself arose mainly from Tobin's work on asset pricing, including *q*-theory, and portfolio choice. Material issued by the Nobel Foundation on the day of the award described his work as putting portfolio selection "into a general equilibrium theory [covering] financial and real assets." (*The Post* (Palm Beach, Florida), October 14, 1981.) Tobin's (1969) paper had not cited Friedman, although its concluding sentence (p. 29) gave the

nevertheless had considerable coverage of stabilization policy and it contained heavy criticism of Friedman's work, as well as of rational expectations macroeconomics—which Tobin categorized as a second generation of monetarism. The book prompted an article-length review/rebuttal, "Tobin on Monetarism," by Robert Lucas (1981). Much of Lucas' review was concerned with aggregate-supply matters such as the Phillips curve, rather than the somewhat separate Keynesian-monetarist debates on aggregate demand. On the matter of aggregate supply, Lucas underlined the role that the Friedman-Phelps Phillips-curve proposition had played in upending the price-setting specifications propounded by Tobin and others in the 1960s.

Lucas also stressed that the assumption of rational expectations need not, in itself, imply extremely ephemeral real effects of monetary policy, even though the rational expectations models in much of his own 1970s work had had that feature. Here, Lucas (1981, p. 564) pointed to studies by Fischer (1977), Phelps and Taylor (1977), and Taylor (1979a), which he characterized as having delivered "the Friedman-Phelps conclusions for the 'long run'" while leaving scope for activist demand management to improve on the fixed-monetary-growth rule in short-run stabilization policy."[84] Friedman had himself supervised some research of this kind late in his time at the University of Chicago, namely, the work of Jo Anna Gray.[85]

These models, especially once they were augmented with aggregate-spending equations grounded in optimizing behavior, later became known as part of the New Keynesian framework. It became standard to describe these models as being "Keynesian" in the short run because they had temporary nominal wage or price stickiness—with this incomplete adjustment meaning that monetary policy actions initially generated reactions in real variables like output, employment,

conclusion of the analysis as being negative regarding approaches that focused on the money stock. The material issued by the Nobel committee played up this contrast, too, again not naming Friedman in this context but stating: "Unlike many other theorists in the field, Mr. Tobin does not confine the analysis solely to money…" (*The Post* (Palm Beach, Florida), October 14, 1981.) Of course, the monetarist position did not actually limit the analysis to money. Rather, as Tobin (1969) had implicitly acknowledged, it treated the money stock or monetary growth as providing a valuable summary of the behavior of multiple asset yields.

In having a joint interest in macroeconomics and more purely financial topics, Tobin was dissimilar in his sensibility to Friedman, but had commonality with Paul Samuelson. Stanley Fischer observed (interview, August 30, 2013): "Samuelson was much more interested in [details of financial] markets and how to invest and things like that I don't think Milton was very interested in." See also Chapters 4 and 11 above.

[84] Fischer (1977) was included in Lucas and Sargent's (1981a, 1981b) volumes of readings on rational expectations. The development of this work was foreshadowed by Barro's (1976, p. 6) remark: "It would be of interest to incorporate price adjustment costs into a rational-expectations-type model."

[85] See J.A. Gray (1976a, 1976b, 1978), as well as the discussion of this work in Nelson (2018). In the years after Friedman's departure, monetary-economics research at the University of Chicago continued among graduate students and economics-department members—Lucas and Stokey (1984, p. 171), for example, cited a dissertation by Michener (1981) titled "Money, Growth, and the Theory of Interest"—although it was predominantly done under the assumption of price flexibility.

and real interest rates.

Lucas' own discussion (1981, p. 565), however, cautioned against the interpretation that nominal contracts of the kind Taylor and others as putting a Keynesian short run into rational expectations models. Rather, Lucas stressed that Taylor's results echoed Friedman in requiring "that one think of setting a policy as fixing a policy rule in advance, and having this rule be entirely trustworthy and understood by private agents." Taylor (1981, p. 146) himself added a further respect in which his framework was not Keynesian: he was studying money supply rules and not fiscal-policy rules—a "feature of the model which a few years ago would have made it seem monetarist from the start."

Lucas' review did not touch on an area of dispute between monetarists and Tobin that had figured heavily in public-policy argument but had not featured much in the rational expectations literature or other major 1970s research: the clash between cost-push and monetary analyses of inflation. This clash had, of course, been prominent in media discussions of the Keynesian-monetarist argument for years: the *Chicago Daily News*—a newspaper that had closed by the 1980s but that had been a mainstay of Friedman's years in the city—had once kicked off an editorial titled "Wages and Productivity" by observing that "Milton Friedman discounts wages as a cause of inflation" (August 7, 1970). And the issues raised in that debate highlighted a still further respect in which the Phelps-Fischer-Taylor nominal contracting models went in a direction of which Friedman approved: they treated the price level as endogenous—in contrast to many presentations, in classrooms, policy discussions, and research articles during the 1970s—while eschewing the price-flexibility assumption of the early rational expectations literature. And these model specifications implied a rejection of cost-push forces as a separate systematic influence on inflation.

For his part, James Tobin continued to stress wage-push views of inflation, stating on May 1, 1981: "Let there be no illusion. There is no way to reduce inflation in this country so long as wage increases proceed at 10 percent a year."[86] In contrast, in the same month, Friedman reaffirmed his view that nominal wage growth was only a symptom of inflation: "It isn't the wet street that caused the rain." (*The Daily Oklahoman* (Oklahoma City), May 19, 1981.) Likewise, with regard to strategies against inflation, Tobin continued to criticize the "purely monetary cure for inflation"—which he suggested might imply five years of economic stagnation (*The Australian*, October 19, 1981.) Shortly before his award, he argued that monetary policy

[86] Tobin (1981a, p. 10).

should be supplemented by “guideposts [such] as those used in the Kennedy Administration… But, of course, it’s clear that this [Reagan] Administration is ideologically and on principle opposed to any such program.” (*Chicago Tribune*, October 14, 1981.)

Although it was presented by Tobin as an ideological choice, the negative conclusion on incomes policy in Reagan’s economic circles heavily reflected analytical and empirical judgments about the source of the 1970s inflation and the ineffectiveness of economic policy: Beryl Sprinkel, for example, referred to “the inflation and inflationary expectations which have been generated by many years of excessive monetary growth” (*Financial Times* (London), June 30, 1981). George Shultz recalled: “In the advice we gave to the president, the one that was written [in November 1980], we all said, and he was perfectly convinced, that you couldn’t have a decent economy with the kind of inflation we had then—I might say ironically that we inherited that from the wage and price controls and too loose a monetary policy and so on. There was still a residue of the wage and price controls when Reagan took office. So one of the things we said was: ‘The first thing you ought to do is just abolish the controls.’ And he did.” (George Shultz, interview, May 22, 2013.) Specifically, as well as expediting decontrol of energy prices, Reagan sought and secured the abolition of the Council on Wage and Price Stability in 1981.

The first American Nobel economic laureate, Paul Samuelson, was, like Tobin, a longtime Friedman sparring partner. But he was pulling back from that role in the early 1980s. For close to fifteen years, Samuelson’s column appeared in different weeks from Friedman’s in *Newsweek*. But the Samuelson no longer appeared after May 1981. In his memoirs, Friedman indicated he did not know why the column ended.[87] But the reason was simply that Samuelson, who had turned 65 in 1980, was cutting back his writing commitments: the *Wall Street Journal* later accurately noted (February 14, 1984) that Samuelson “gave up” his column. Samuelson had already ceased, at the end of 1979, writing his twice-yearly *Financial Times* review of U.S. economic developments.[88] Terminating the *Newsweek* column continued this pattern.

Robert Solow recalled of Samuelson’s time as a *Newsweek* columnist: “I think Paul really wanted to have an influence on public opinion—and there it was, an offer. He may have thought: ‘If Milton and Henry Wallich [a columnist until 1974] are writing these things, there has to be a counterweight from somewhere, and it might as well be me.’ I think that may have been part of it. But Paul also loved to write. He enjoyed sitting down and doing that sort of thing—

[87] Friedman and Friedman (1998, p. 359).

[88] Samuelson, who had been contributing to the newspaper for over a quarter-century years in this fashion, published his final regular *Financial Times* economic review in the edition of December 31, 1979.

whereas I don't like it… It's not even always related to the ability to write well. Some people like to do that, and others don't."[89] In light of Samuelson's continuing research interests, however, he became ambivalent about the column: "he was conflicted about it. He wanted to do it. But he always wanted to do several things, and the column always had to have the highest priority, because it has a hard deadline. And Paul was never good at meeting deadlines—but he made that [deadline each time], because he had to meet it. So I think, after a while, he found it hurt him… But Paul was just incredible—the ideas came [to him] in large numbers, and he always had a backlog of things that he wanted to pursue—notions that he'd had, or that we had talked about, and that he wanted to get down to actually thinking them through, and proving, rather than suspecting. And so he always had a bunch of things in the back of his mind." (Robert Solow, interview, July 7, 2014.)

The year 1981 would have been a highly opportune time for Friedman, also, to have relinquished his own *Newsweek* column. That would not only have spared him the ignominy of being unceremoniously dropped by *Newsweek* in February 1984. It would also have prevented him from writing some of his least prescient and least well-conceived columns. The period from the start of 1982 to early 1984 would see him produce numerous worthwhile *Newsweek* pieces—but also several that did not hold up well as events progressed. Most notably, an early-1982 column on Chile's economic achievements was published just as the country's terms-of-trade and economic crashes were starting to come to the fore, and his columns from late 1982 through very early 1984 would contain predictions and warnings about U.S. inflation and recession that would prove far from prophetic.[90]

Monetary Trends

As indicated in the previous chapter, work on *Monetary Trends* continued at the start of the 1980s. "I'm still trying to finish a book on monetary trends that Anna Schwartz and I started ten years ago," Friedman noted to the *New York Times* a couple of months into the decade (*New*

[89] With regard to his own attitude toward being a regular op-ed writer, Solow observed: "I never wanted to do that. And I never thought that it was time well spent. But I'm the person who advised Paul Krugman not to accept the *New York Times*' offer to be a columnist—so I don't count as an astute person about this. But for myself, that's not something I would ever have wanted to do… because, first of all, I don't like to have something that I have to do regularly, every third week, or God knows, in the case of the [*New York*] *Times*, twice every week. No, I don't like that. And I don't like the act of writing, so I wouldn't want to tie myself to having to write. And I have some doubts about anyone's capacity to explain anything serious in the amount of space you get in *Newsweek* or in a newspaper. And I once spontaneously, I can't remember the occasion, but a friend—there's a friend of mine who loves it, and has quoted it [what I said] back to me: I once said that the shortest true sentence in economics is longer than the average person's attention span. And I mean that seriously. And so I've never liked the short column, or op-ed piece, as a way of expressing anything serious." (Robert Solow, interview, July 7, 2014.)
[90] See Chapter 14.

York Times, February 24, 1980*a*)—although twenty years would have been a more accurate number to give. “We’re hoping to get it out this summer. But that’s a hope, not a prediction.” Once again, Friedman underestimated the time that it would take to finalize the book, and *Monetary Trends* remained unreleased at the end of 1981. The book was nevertheless reaching the finishing line. In 1980, a full manuscript version of the book was circulated to economists.[91] Not yet having a truly new Friedman-Schwartz book to release, in 1980 the National Bureau of Economic Research instead published (through Princeton University Press) a softcover book, *From New Deal Banking Reform to World War II Inflation*.[92] This was simply a reprint of chapters 8 and 9 of the *Monetary History*. Unlike the previous paperback that had been an extract from the *Monetary History*—1965’s *The Great Contraction*—this one did not contain a new preface by the authors. The book was purely a reprint, albeit with chapters renumbered.[93]

In 1981, the *Monetary Trends* book really was finished and was scheduled for publication the following year.[94] Anna Schwartz presented a summary of the forthcoming book, to an audience consisting of members of the business and media worlds, at the NBER’s third annual research conference in New York City on October 19, 1981.[95] In a foreshadowing of the muted response that the book would receive on its 1982 release, Schwartz was disappointed at the lack of interest shown by the audience in the new Friedman-Schwartz work: there were no questions directed to her (Anna Schwartz, personal communication, September 25, 2008).

II. ISSUES RELATED TO MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1980–1981

CREDIT CONTROLS

As has already been indicated, the decade of the 1980s opened with Friedman reasonably pleased with the course that U.S. monetary policy had taken since the October 1979 introduction of New Operating Procedures. At the top of the Federal Reserve, Paul Volcker, likewise, looked with

[91] Lawrence Summers, at the time a frequent presence in the NBER’s Cambridge headquarters (National Bureau of Economic Research, 1981a, p. 15) cited the 1980 manuscript version of *Monetary Trends*, in Summers (1983, p. 217). Summers originally presented this 1983 study in September 1980 and issued in May 1981 for wider circulation as a working paper.

[92] Friedman and Schwartz (1980).

[93] But for a recent analysis that cites the book as a study in its own right, see Rouse, Zhang, and Tedeschi (2021).

[94] In early September 1981, Friedman (1981g, p. 1) cited the book as being in press. Friedman and Schwartz (1982a, p. 43) referred to 1980 as the current year. But evidence that the finishing touches on the book were made in 1981 turned up in some references to papers published that year (see for example. Friedman and Schwartz, 1982a, pp. 33, 34, 114) as well as to a couple of unpublished 1981 papers (pp. 634, 647).

[95] See National Bureau of Economic Research (1981b). Many of the attendees were NBER sponsors or NBER board members (Anna Schwartz, personal communication, September 25, 2008).

satisfaction at the response to date of both financial markets and the targeted monetary aggregates to the new policy arrangements. Volcker observed at the start of 1980 that a "period of turmoil and unsettlement" had been passed, and that "now banking and financial markets appear to be functioning in an orderly way" (*Kansas City Times* (Missouri), January 3, 1980). As for the monetary aggregates, he noted about a month later, they were "remarkably on target" (*Financial Times* (London), February 18, 1980).

This constellation of financial and monetary settings would shortly be badly upset. As indicated above, a renewed bout of financial market volatility occurred in the first quarter of 1980 in response to high inflation readings. It was the U.S. government (both the Federal Reserve and Carter Administration) policy response to these disturbances that would ultimately both create still more market turbulence and upend the Federal Reserve's newly improved track record in meeting its monetary targets. The 1980 credit controls—what Stuart Eizenstat, one of Carter's key domestic advisers, retrospectively agreed was a "monumentally bad idea" that put a "monkey wrench" into the Volcker disinflation strategy—were introduced by the president as part of his March 14 anti-inflation announcement.[96]

The controls' implications for monetary policy included what Friedman described (*Newsweek*, December 1, 1980) as a shift from a state of affairs in which M1 was on target from October 1979 to February 1980, to one in which "it all went to pieces"—with a deep mid-year undershoot, followed by a rebound that produced an overshoot of the target for the year as a whole. This sequence produced a lasting situation in which Friedman was harsh and outspoken critic, rather than a cautious supporter, of the Volcker stewardship of U.S. monetary policy.

The push for controls

The credit controls were instituted in the wake of widespread advocacy of a controls program in public discourse. Concern about the rapid growth of consumer credit growth during the late 1970s had been expressed in media and financial commentaries (see, for example, *Manchester Union Leader* (New Hampshire), March 1, 1979), and an editorial in the *New York Post* (February 28, 1980) called for Carter and Volcker to deploy "the standby authority they already have" to apply credit controls. Importantly, at the official level, Alfred Kahn had publicly and privately been making the case for direct credit controls for over a year (*The Courier-Journal*

[96] The quotations are from Eizenstat (2018, p. 347).

(Louisville, Kentucky), April 11, 1979; Schreft, 1990, p. 29; Eizenstat, 2018, p. 347).[97]

As already indicated, in his role as the administration's anti-inflation czar Alfred Kahn strongly espoused cost-push aspects of the inflation problem—but he also granted that, by the late 1970s, high U.S. inflation rate was, in part, also an excess-demand problem. In principle, this acknowledgment should have augured well for an arrangement in which the administration handed over aggregate-demand control to Volcker, aside from providing supportive fiscal restraint. But Kahn's particular perspective on the causes of increases in aggregate demand led him to encourage the administration to go beyond this and, in particular, to take initiatives in the area of credit control that would trespass on monetary policy.

Kahn was persuaded that U.S. private-sector aggregate demand, particularly consumption spending, had been driven by excessive growth in credit. "The strength in the economy until now has been phenomenal. It has been much stronger than anyone predicted, and it was pushed along by increased consumer credit," he remarked (*Dallas Morning News*, April 19, 1980). During the February 1980 period of heightened concern about inflation, Kahn was said to be "pressing for credit controls" as an immediate policy response (*Financial Times* (London), February 18, 1980). Credit controls would indeed form a major part of the administration's new anti-inflation package, announced the following month.[98]

The elements of the controls program

The controls announced on March 14 included items described in news reports as being restrictions on "certain types of consumer credit," including those provided in the form of "credit cards, check overdraft plans and unsecured personal loans" (*The Sun* (Baltimore), March 15, 1980*b*). These restraints, ostensibly voluntary, applied to both commercial banks and finance companies (Schreft, 1990, p. 35).

The efforts to limit consumer loans also included an overtly mandatory braking element in the

[97] He had originally done so on a television interview broadcast during the weekend of December 2–3, 1978, and at the time his comments had been repudiated by other administration personnel (*Detroit Free Press*, December 5, 1978).

[98] Schreft (1990, p. 25) concluded that the controls were put on "for political reasons." An inference that might be drawn, upon acceptance of this conclusion, might be that economic arguments, including those advanced by economists inside and outside the administration, did not figure heavily in the decision. Such an inference would not be correct, however, as a prominent administration economist—namely Kahn—argued on economic grounds for controls. Nor did Kahn advance credit controls as a quick fix. Instead, he described them in a May 1979 memorandum as being attractive "as part of a longer-term policy" (quoted in Schreft, 1990, p. 30).

form of a special deposit requirement on increases in a wide variety of specified categories of consumer credit applied to what the Federal Reserve Board (1980, p. 59) called "all types of lenders." That is, the requirement was that for each new extension of a specified type of lending, an institution needed to lodge with the authorities an amount equal to 15 percent of the loan amount. As the lodged amount earned no interest, the effect was to reduce the profitability of making new loans. In addition, a 15 percent special deposit requirement was imposed in proportion to new assets acquired by money market mutual funds (*San Jose Mercury News* (California), March 15, 1980; Federal Reserve Board, 1981a, p. 59).[99]

The control measures also included actions that were at the intersection of measures to restrict loan growth and measures to restrict deposit growth. In particular, the marginal reserve requirement that had been applied by the Federal Reserve Board to increases in large commercial banks' managed liabilities (that is, essentially their wholesale deposits) in the October 1979 monetary policy package—and which was discussed in Chapter 10's discussion of that package—was now raised from 8 percent to 10 percent. The same marginal reserve requirement was now also imposed on commercial banks that were not members of the Federal Reserve System (Federal Reserve Board, 1981a, p. 59; Schreft, 1990, p. 35).[100]

Friedman's attitude to credit controls

Friedman was a longstanding opponent of direct credit controls. This opposition went back to

[99] This applied to asset acquisition, not new loans, because the apparent intention of the restriction on MMMFs was not to reduce the financing of private-sector borrowing but instead to reduce the extent to which MMMFs could bid funds away from (that is, gain lender-depositors at the expense of) thrift institutions (Schreft, 1990, p. 38). At the time, MMMFs, which were not subject to the U.S. government's interest-rate ceilings had emerged as the entities offering the highest-yielding deposit-like instruments to the U.S. retail market (*San Jose Mercury News* (California), March 15, 1980). In offering partial justification for the special deposit requirement, Volcker implied that MMMF expansion was more inflationary than expansion of thrifts' accounts (*Wall Street Journal*, March 17, 1980). Nonetheless, MMMF and thrift accounts were treated largely symmetrically in the official monetary data—with both appearing in the new M2 aggregate, as part of the redefined series' expanded conception of time-deposit-like assets of the private sector.

[100] Nonmember commercial banks (as well as thrift institutions—restriction of whose activity was, as already indicated, not a main concern of the credit controls, as the controls were not intended to discourage housing lending—and other depository institutions, such as credit unions) were shortly to become subject to regular reserve requirements, once the Depository Institutions Deregulation and Monetary Control Act of 1980 was passed on March 31, 1980 (Goodfriend, 1983, p. 345; Meltzer, 2009b, p. 1052). Ahead of this occurring, the credit controls, using existing legislative authority, applied marginal reserve requirements on many of these institutions. The use of marginal reserve requirements as a control weapon contrasted with the reduced emphasis on regular reserve requirements in monetary policy in the same period. In November 1980, as required by the Monetary Control Act, regular reserve requirements were lowered (F.J. Levin and A. Meulendyke, 1982, p. 402). As it did in its preceding operating procedures, the FOMC essentially offset these changes in ordinary reserve requirements through open market operations.

his earliest days as a monetarist in the early 1950s.[101] In a 1952 statement of his position that appeared in both the *American Economic Review* and a Congressional hearings volume, Friedman had expressed his strong disapproval of controls and his preference for movements in interest rates as the means of directing credit allocation.[102] He opposed the idea of government controls on the allocation of credit to businesses and consumers alike. Indeed, Friedman had criticized a key monetary economist in the prior generation of teachers at the University of Chicago—Henry Simons—for wishing to curtail corporations' ability to issue securities (see Nelson, 2020a, Chapter 2). And in late 1956 Friedman had provided a long critique of consumer credit controls for a conference organized by the Federal Reserve Board whose proceedings were published in mid-March 1957.[103]

Friedman was resolute on this point. "You should let the market determine the allocation of credit," he observed in the mid-1960s (*Christian Science Monitor* (Boston), December 31, 1966, p. 14). "I remain adamantly opposed to credit controls in any shape, form, or manner," Friedman remarked further, early in the 1970s (Instructional Dynamics Economics Cassette Tape 51 (May 27, 1970). In mid-decade, he was withering about seeing consumer spending as the basic reason for inflation: "I do not believe consumer behavior is to blame. How much people spend should be of their own choice." (*Times-Bulletin* (Van Wert, Ohio), July 27, 1974.)

Friedman's opposition to credit controls largely paralleled his critiques of price controls and of rationing. Specifically, credit controls amounted to attempting to allocate credit using a device other than through changes in the price of credit (the interest rate or, more correctly, a vector of interest rates). They were not only less efficient than this mechanism, they were also not really a *bona fide* substitute for it. That is, as with controls on goods prices, Friedman viewed credit

[101] See Nelson (2020a, Chapters 4 and 6; 2020b, Chapter 14). Friedman's (1951, p. 187) remark that "qualitative credit controls are not appropriate means of controlling inflation" was an early statement of his opposition. A complication involved in interpreting his statements on the matter is that, during the late 1940s and early 1950s, Friedman occasionally used the term "credit control" as a synonym for monetary policy, whose use he advocated and which he contrasted with direct controls on prices or on resource allocation (see, for example, NBC, 1948, pp. 9, 11). (Similarly, Arthur Burns in his *Prosperity Without Inflation* lectures used "general credit controls" to mean Federal Reserve monetary policy actions—see Burns, 1957, pp. 63, 64.) Just as in Friedman's later statements, his advocacy in the late 1940s and early 1950s of "credit" (that is, monetary) control contrasted with his opposition to direct or qualitative credit controls. Friedman alluded to his previous, now-obsolete use of the "credit control" terminology in Instructional Dynamics Economics Cassette Tape 51 (May 27, 1970).

[102] See Nelson (2020a, p. 382). This appeared in a solo-authored portion of Samuelson and others (1952, p. 390). In addition, on March 25, 1952, Friedman testified: "the voluntary credit-restraint program is fundamentally antithetical to our basic econom[ic] and social philosophy. Insofar as it has any effect, it does so through the exercise of arbitrary power[,] without either the economic check of competition or the political check of responsibility to the electorate." (Joint Committee on the Economic Report, U.S. Congress, 1952b, p. 691.)

[103] Friedman (1957b). This paper was not heavily cited and was not reprinted in Friedman's later collections, but he recalled this "lengthy" paper in Instructional Dynamics Economics Cassette Tape 40 (December 17, 1969).

controls as ultimately futile—indeed, he grouped price controls and credit controls together even in 1951 when he stated that "such controls repress rather than remove inflationary pressure."[104]

With regard to credit controls specifically, Friedman believed that they would prove ineffective in controlling aggregate credit. Direct controls might suppress certain types of lending—although, even in those lending categories, the financial sector would ultimately find ways of bypassing the controls—but the funds that the authorities blocked from used for lending in controlled categories would largely end up being channeled by the private sector into other forms of lending.[105] Furthermore, insofar as policymakers were persuaded that controls *were* effective, they would be encouraged to become more stimulative (or would be convinced that they could be less restrictive than otherwise) in their setting of standard monetary policy tools.[106] What Friedman said in January 1980 with regard to the U.S. experience with interest-rate ceilings and other regulations—that the "stress of competition subsequently forced innovation" in the financial system that undercut the officially-imposed limitations—reflected also his view concerning the cumulative private-sector response to credit controls.[107]

But, as his description of the experience with rate ceilings also implied, Friedman realized that the process of private-sector evasion of mandatory credit controls took time.[108] In light of this,

[104] Friedman (1951d, p. 187).

[105] See, for example, Friedman (1970f, p. 22). See also the discussion of Anna Schwartz (in *The Banke*r (London), February 1985), who used this argument to critique the United Kingdom authorities' use of marginal reserve requirements during 1973–1980. As of the early 1970s, a senior Federal Reserve of New York official, Richard Davis, contended (1971, p. 76) that credit controls would have some definite aggregate-demand effect because a fraction of private-sector customers was shut out of bank-credit access and had no ready alternative: "there will be at least some cuts in [receipt of] total credit flows and in spending on output by businesses and consumers whose access to bank borrowing has been reduced." Although very likely valid to this day as a description of the short-run situation, and certainly applicable to the months of the credit-controls period of 1980, Davis' position was of more questionable validity as a long-run proposition even in the 1970s. The volume of U.S. nonbank credit was large by the early 1970s, as Friedman (1972c, p. 192) stressed—and, thanks to continuing financial innovation, this would remain so even though some of the institutions to which Friedman was referring in 1972, such as thrift institutions, increasingly came to be treated as banks in monetary analysis. And although many in the private sector lacked the capacity to issue debt directly to financial markets, many of the same participants nevertheless had, both in the 1970s and later, considerable scope to gain access to nonbank credit through other means. For example, some nonbank financial intermediaries offered bank-like lending services, while commercial banks themselves could participate in credit extension through off-balance-sheet activities—as U.K. banks did during the episode that Schwartz's 1985 piece highlighted.

[106] This was another parallel with wage and price controls—the main difference being that credit controls were intended to limit aggregate demand and so might be part of a demand-focused perspective on inflation.

[107] Friedman (1980d, p. 81).

[108] The tendency for financial markets to innovate in the face of regulation was often presented as an argument not only against credit controls but against some of the money-based analysis that Friedman advanced. For example, as discussed in Tavlas (1977), some Keynesian critiques of monetarism argued that the private sector would find ways of arbitrarily raising the money multiplier or velocity if the central bank held down the monetary base or the money stock, respectively. To Friedman, these critiques of money-oriented economic analysis did not apply if the central

Friedman recognized that considerable time would elapse before escape valves developed in response to sweeping credit controls such as those imposed in March 1980. Accordingly, upon the announcement of the controls, Friedman granted that they would indeed affect overall credit. But he emphasized that they were "a very inefficient and undesirable device for reducing spending by consumers and by business." (*Carter and the Economy: Other Views*, CBS, March 26, 1980.)

As this statement indicated, Friedman was highly critical of the 1980 credit controls from the moment they were implemented. Among the various criticisms he marshaled against the controls were: that they contradicted President Carter's claim to be in favor of deregulation (*Carter and the Economy: Other Views*, CBS, March 26, 1980). He stressed also that their underlying rationale drew in part on the fallacious real-bills doctrine, according to which certain types of spending were particularly inflationary in character (so that control of inflation required discouraging these spending categories).[109] This was, of course, in contrast to Friedman's own view that inflation responded to total spending. He also believed that the credit controls and new regulations announced in conjunction with them would have adverse effects on U.S. saving and investment (*Newsweek*, April 14, 1980).

Friedman also called the wide-ranging 15 percent marginal reserve requirement that was part of the March 1980 package "crazy" and "absolutely absurd." He was withering about "what I regard as evasive tactics" of couching the measure in terms of managed liabilities (a term Friedman himself rarely used)—"it's all clothed in the complicated jargon of financial institutions, and nobody understands it." (*Fort Worth Star-Telegram* (Texas), March 27, 1980.) The credit controls' targeting of credit card usage was "fundamentally a tax measure," so the credit-controls measures in question were "clearly and simply credit-card taxes—and they should be called by their rightful name." (*San Antonio Light* (Texas), March 30, 1980.)

Other economists' attitudes

As a whole, however, the economics profession outside the administration had a mixture of views regarding credit controls. Ahead of the controls' imposition, Paul Samuelson (*Newsweek*, March 3, 1980) noted that eminent Wall Street economist Henry Kaufman had spoken out in

bank concentrated its approach to monetary control on measures that altered the volume of reserves, which the banking system had an interest in holding, and avoided reliance on more evasion-prone devices like changes in reserve requirements. For further discussion, see Nelson (2020a, Chapter 2; 2020b, Chapter 14).

[109] Friedman (1982c, p. 103).

favor of the introduction of controls.[110] In addition, a longstanding view of credit controls was that it as an attractive alternative to explicit increases in interest rates. In part, this was simply because of the desire to achieve a certain amount of aggregate-demand restriction at lower interest rates (see, for example, Davis, 1971, pp. 66–67). This argument was influential within the Carter Administration in the leadup to its March 1980 decision to impose controls (see Eizenstat, 2018, p. 347). An additional rationale, reflecting a view inherited from early Keynesian economics and still very prevalent in U.S. economics circles in 1980 (see Slifman and McKelvey, 1981, p. 26) was that the non-housing component of household spending was interest-insensitive, and so direct credit controls provided a more effective tool for curbing such expenditure.[111]

In a *Newsweek* column (March 3, 1980) that appeared when credit controls were being contemplated, Samuelson made clear that he thought that such controls could have usefully been deployed in place of interest-rate increases during key periods of U.S. monetary policy tightenings in the 1970s. His column was, however, much more circumspect about imposing controls in 1980's conditions, as Samuelson expressed doubt that controls would actually focus on the lending that supported the category of consumer expenditure—purchases of nondurable products—that he currently thought excessive, while they might further lower business in the already-weak housing and automobile sectors. But the actual March 14 package seemingly addressed Samuelson's reservation by exempting consumer loans in the areas of housing and automobiles from the voluntary credit-control program (*The Sun* (Baltimore), March 15, 1980*a*, p. A1). In addition, by addressing nonbank as well as commercial bank activity, by including special deposit requirements, and by circumscribing wholesale-deposit raising through the enhanced marginal reserve requirements, the controls also seemed to take into account concerns, as expressed by economists who were inclined toward controls (see *New York Times,* March 14, 1980), that an announced program might be cosmetic and that, in order to be effective, a program needed to put pressure on banks' capacity to finance loans on the margin, to include specific disincentives to consumer loans, and to be broad-based.

Once the controls were in place, Samuelson assessed that the credit-control package would succeed in restraining aggregate demand by "a bit" and doing so in the context of lower interest rates (*Newsweek*, April 28, 1980) and, after they had been in force for about 50 days, Samuelson

[110] Samuelson did not give a specific source, but *Decatur Sunday Herald and Review* (Illinois) (June 8, 1980) and Schreft (1990, pp. 32, 50) specifically pointed to a speech that Kaufman had given to this effect at a Los Angeles event on February 21, 1980, held by the American Bankers Association. Although it was cited by Schreft as though it was unpublished, the speech had actually been printed in *American Banker*, February 28, 1980.
[111] On Friedman's opposition to this view, see Nelson (2020a, Chapter 5).

told an interviewer that he was impressed at the effectiveness of the controls (*New York Times*, May 9, 1980, p. D6). The interviewer gave the impression that, at this point, Samuelson approved of the credit controls' imposition and of their continuation. Before long, however, as the scale of the reaction of the economy to the controls in spring 1980 became apparent, Samuelson was also noting the "plunging" economy and "slumping real incomes" and implied that the administration had not anticipated how restrictive its credit-control measures would prove to be (*Newsweek*, May 19, 1980).

A critic of the controls from the moment of their imposition was Arthur Burns, who attacked both Carter's decision to invoke credit-controls legislation and the legislation itself: "the Credit Control Act of 1969… is a demagogic piece of legislation, one that has put dictatorial power into the hands of the Federal Reserve Board, a power that no agency of government should have… This is stupid legislation, demagogic legislation that is potentially destructive of our economic system."[112]

Paul Volcker's posture regarding credit controls

Although it was the U.S. executive branch that made credit controls an action item in 1980, the terms of the Credit Control Act meant that it was the Federal Reserve Board that actually had to give the go-ahead to the institution of controls and to choose and design the form they would take. So when, as part of the anti-inflation program discussed above, President Carter announced direct controls on credit, he was indicating that he had formally requested the Federal Reserve

[112] From Burns' testimony of March 27, 1980, in Joint Economic Committee, U.S. Congress (1980, p. 137). See also *Kansas City Star* (Missouri), March 27, 1980. Although, of course, Burns changed his mind over price controls over the years, he had been reasonably consistent on the subject of credit control. In his *Prosperity Without Inflation* lectures, he had implied that the Federal Reserve's scope to alter credit expansion was an important part of its influence over the economy and that the growth of nonbank lending institutions had undermined this power, but he came out in favor of the view that the existing tools of the Federal Reserve likely gave it considerable scope to restrict aggregate demand (see Burns, 1957, pp. 51, 63). In 1977, he had suggested that Congress take steps to limit consumer credit card issuance. But he later noted that he had changed his mind and now believed that additional control powers were unnecessary (see Burns' testimony of March 17, 1980, in Committee on Banking, Housing and Urban Affairs, U.S. Senate, 1980a, p. 150). As for the Credit Control Act, he stated well before Carter invoked it that "the Federal Reserve is entirely capable of exercising a healthy influence over monetary and credit developments without this legislation" (May 10, 1979, letter to Senator William Proxmire, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1979a, p. 16). Finally, Burns was a longtime supporter of imposing reserve requirements on nonbank depository institutions (see, for example, Burns, 1973, pp. 18–19). But he believed that this matter was adequately addressed by the new Monetary Control Act of 1980, with no need to use the Credit Control Act of 1969 for this purpose (see his testimony of March 17, 1980, in Committee on Banking, Housing and Urban Affairs, U.S. Senate, 1980, p. 150). Later, in his posthumously published account of innovations in U.S. banking, Burns (1988, p. 22) gave examples of changes instituted during his tenure as Federal Reserve Chair in making the case for associating his period in office with financial deregulation and the relaxation of various credit controls.

Board to impose such controls—and that the members of the Board had agreed to do so and designed a controls system. The details of these controls were issued concurrently with Carter's announcement (*The Sun* (Baltimore), March 15, 1980*a*).

In his commentary on this sequence of events, Friedman recognized the reality that the controls were a presidential initiative that the Federal Reserve Board was expected to agree to, though he did note that "under the authority of the 'independent' Fed."[113] His centering of blame on the executive branch was evident in his references to "President Carter's ill-starred imposition of credit controls in early 1980" (*Newsweek*, July 25, 1983) and the president's "mistake."[114] But he criticized the Federal Reserve for the specific form of the controls (see the discussion above) and also for allowing the credit controls to spill over into a major monetary contraction (see below).

When working in the Nixon Administration, Paul Volcker had helped dismantle U.S. exchange controls. And while president of the Federal Reserve Bank of New York, Paul Volcker had similarly indicated his antipathy toward domestic credit controls when he observed that "the Federal Reserve likes to stand back from the actual decision of who gets how much credit: the bank-lending decision. It's not up to the Federal Reserve, it's not up to a government bureaucracy, to make that kind of decision… But it's not our job: We try to control the total amount of credit total amount of money. It's up to the banks, it's up to private enterprise, to say who gets that." (*Wall Street Week*, Maryland Public Television, November 14, 1975.) Similarly, as Federal Reserve Chair in October 1979, Volcker stated: "It is the job of banks and savings and loans and other lending institutions to make that kind of judgment as to what the need is of their individual customers." (*Issues and Answers*, ABC, October 29, 1979, p. 14 of transcript.)

When, therefore, President Carter asked the Federal Reserve Board to impose credit controls, Volcker agreed only with reluctance. He made his reluctance clear both in the public retrospectives he gave on the credit-control episode (for example, *New York Times*, September 19, 1982, p. 72; Eizenstat, 2018, p. 347; and Kohn and others, 2019, p. 135), but also, to some extent, in his public comments once the controls were in force—including in April 1980, when Volcker stated in an interview that "the decision to invoke the Credit Control Act, which [gave rise to] the direct control[s] on consumer credit, was not mine, that's the president's decision… I'd like to get rid of those as soon as we can." (*The MacNeil/Lehrer Report*, PBS, April 21, 1980.) In other public remarks, Volcker tried to reconcile his opposition to government-directed

[113] *Wall Street Journal*, January 30, 1981 (reprinted in Friedman, 1983a, p. 242).
[114] Friedman and Friedman (1984, p. 90; 1985, p. 90). See also Friedman (1984c, p. 399).

credit allocation with the controls program by remarking that the program still left detailed decision-making on individual loans with the lending institutions (*Wall Street Journal*, March 17, 1980, p. 2). Volcker also repeatedly indicated that he was willing to countenance credit controls because they were part of a toughened message that could engender "greater public confidence in the government's commitment to ending inflation," including via the inclusion of further proposed fiscal tightening in the package.[115]

Another contention that Volcker made in his retrospectives was that the Federal Reserve Board had formulated the credit controls with the aim that they would have little economic effect. In 1982, for example, he stated that "we introduced some rather mild controls or restraints on a limited portion of consumer credit" and that "the action that was taken was, frankly, the mildest we could think of."[116] He suggested that the controls then "had a larger impact than the action was designed to produce."[117] In these later accounts, he attributed the sizable effect observed in practice in large part to public sentiment against consumer borrowing, a sentiment encouraged by the administration's warnings (see, for example, *New York Times*, September 19, 1982, p. 72).[118]

The controls movement was indeed associated with moral suasion on the part of the administration that associated consumer borrowing with reckless or unpatriotic conduct, and this perspective was reflected also in reports describing the credit controls as an attempt to discourage people from living beyond their means (*The Sun* (Baltimore), March 15, 1980*a*).[119] However, even beyond this packaging dimension of the controls, the actual Federal Reserve

[115] With regard to Volcker's expression of this sentiment, see, for example, his remarks in *New York Times*, September 19, 1982 (p. 72), in his testimony of July 14, 1983, in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1983a, p. 22), in Smith (1989, p. 25), and in Kohn and others (2019, p. 137). The quotation is from Volcker's letter of April 7, 1980, in Joint Economic Committee, U.S. Congress (1980a, p. 129).

[116] The quotations are, respectively, from Volcker's testimony of July 20, 1982, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1982b, p. 36) and *New York Times*, September 19, 1982 (p. 72). Similarly, in Kohn and others (2019) he stated: "We developed this idea that we'll have credit controls, but we will make them as little disruptive as we possibly can."

[117] From Volcker's testimony of July 20, 1982, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1982b, p. 36).

[118] Similarly, in his testimony of May 5, 1982, in Committee on the Judiciary, U.S. House of Representatives (1983, p. 84), Volcker stated that although the Federal Reserve implemented "credit control in ways that we thought were relatively mild," the controls were followed in terms of macroeconomic behavior by "a much bigger reaction—through psychological reactions, in some cases—than anyone had counted on."

[119] It may have been in reaction to such rhetoric that Ronald Reagan commented at the time: "I don't think inflation comes from too many people buying things." (*New York Post*, March 15, 1980.) Actually, the monetary view of inflation that Reagan largely espoused implied that inflation did result from excessive spending. Reagan may have meant "people" in specific reference to U.S. consumers. This would be consistent with his later statement, "Inflation is not caused by workers or businessmen or consumers or producers. It is caused by government. The price of goods can go up only if the government creates too many dollars." (*The Sun* (Baltimore), May 1, 1980, p. A8.)

Board credit-controls program was extensive, and Volcker's announcement of the program did not suggest that it was intended to have only a minimal or cosmetic effect. He stated in a March 15 briefing to the media (*Omaha World-Herald*, March 16, 1980, p. 1A): "The thrust of this thing is to put restraint on consumer credit, and it will be broadly felt." Volcker also indicated that "we would like some restraint on the use of credit cards." He was reported as suggesting at the briefing that "consumers soon will see credit-card companies calling back some cards, refusing to issue new ones, speeding up repayment schedules and tightening the terms of their lending agreements" (*Kansas City Star* (Missouri), March 16, 1980). Such restrictive behavior by lenders did occur, along with other measures such as the institution of new credit card fees (*Washington Post,* October 5, 1980, p. G6; Slifman and McElvey, 1981, pp. 24–25). The contraction of credit was then compounded by customer-initiated cutbacks on card usage, in part because of the factor that Volcker's retrospectives so stressed: the administration's public campaign against household borrowing (Slifman and McElvey, 1981, p. 25; Eizenstat, 2018, p. 347).

As already implied, Volcker granted that, his intentions notwithstanding, "the bottom fell out of consumer credit" (A. Smith, 1989, p. 25) and that, the economy, too, responded strongly negatively. Volcker would acknowledge that, although he had "thought we were leaning over backwards to moderate [the amount of demand] restraint [stemming] from the credit controls," in fact the U.S. "economy really came down" once they were introduced (Kohn and others, 2019, pp. 139, 138). This development contrasted with Volcker's conjecture on television in April, when acknowledging the recession, that it might prove to be mild (*The MacNeil/Lehrer Report,* PBS, April 21, 1980; *Wall Street Journal,* April 22, 1980). "Paul accused me: I remember once he said, 'You didn't tell me we were going to have a big recession,'" Stephen Axilrod, a Board staffer and senior subordinate of Volcker's, recalled (interview, April 24, 2013). Volcker himself would later judge that "the recession was created by credit controls" (A Smith, 1989, p. 25)—although this proclamation must be qualified by the fact that the official, NBER-designated recession-start date is January or February 1980—after Volcker had intensified monetary tightening, but before the March introduction of controls.

The Federal Reserve Board publicly rationalized its support for the credit-controls program in terms of the need to make further improvements to its October 1979 monetary-control methods (Federal Reserve Board, 1981a, p. 59). The perceived need to bolster monetary control followed some uncertainty about Federal Reserve control that had materialized in the February-March period when it appeared that the slowdown in monetary aggregates seen since October was being upset.[120] Indeed, as already indicated, the announced March 1980 controls package included marginal reserve requirements that were proximately directed against bank *deposit* growth rather than its extension of credit or lending. As he had been critical of the reserve-requirement aspect of the October 1979 package, Friedman was critical of the monetary-control rationale of the enhanced marginal reserve requirements on banks and the new requirements on nonmember banks. Their imposition, he insisted, "is *not* required to enable the Federal Reserve to control the money supply." (*Newsweek*, April 14, 1980.) Friedman continued to favor concentration on the volume of reserves as the means of securing monetary control.

Another monetary-control item that was announced along with the credit-controls package was a surcharge on the discount rate that large banks that frequently borrowed from the Federal Reserve Board (Federal Reserve Board, 1981a, p. 59). As it was introduced along with the credit controls, this measure would, naturally enough, be treated by some accounts (for example, Schreft, 1990, p. 35) as part of the credit controls. However, in two respects, it differed importantly from the credit controls. First, it would be dropped earlier than most controls (in May) but then would be reintroduced in the post-controls period in September 1980 (at 2 percentage points, raised to 3 points again in November and later to 4 percent) and would remain in force until November 1981 (Federal Reserve Board, 1981a, pp. 16, 80–81; *Journal of Commerce*, September 17, 1981; Cook, 1989, pp. 18–19; Hetzel, 1984a, p. 34). Second, it fit into the Federal Reserve's preexisting conception of its New Operating Procedures which, as discussed in Chapter 10, involved seeing the FOMC's nonborrowed-reserves control as amounting, beyond the short run, to a *total-reserves* control regime. Consistent with this perspective, the Board stated that the "surcharge provided added incentive for affected institutions to adjust their loans and investments more promptly" (Federal Reserve Board, 1981a, p. 81), so that, even under the lagged-reserve-requirement regime, FOMC actions on nonborrowed reserves should be manifested more rapidly in the behavior of total reserves and banks' deposit liabilities.[121]

Friedman recognized the ultimate focus by the Federal Reserve on managing total reserves and approved of it.[122] But he rejected *ad hoc* adjustments to the discount rate as the means of

[120] Volcker would later note that "the run-up in some of the monetary aggregates" in the January and February data on money had impaired public confidence in the Federal Reserve's new procedures (November 19, 1980, testimony, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1981a, p. 32).

[121] The process would involve commercial banks finding it desirable to hold more excess reserves as well as a greater fraction of their interest-bearing assets in a form that could be disposed of quickly. These were factors that would tend to lower the amount and time span of the commercial banking system's discount-window borrowing.

[122] For example, Friedman (1982d, p. 74) wrote of the Federal Reserve's 1979 change: "Its objective of seeking to control total reserves rather than interest rates was an excellent one."

securing total-reserves control. He did approve of making the discount rate a penalty rate (*Newsweek*, July 15, 1974) but wanted this to be a practice carried out on a routine, continuous basis. In particular, Friedman wished the discount rate to be made a floating rate, linked to a short-term market interest rate like the U.S. Treasury bill rate (in which case the penalty embedded into the discount rate could take the form of a constant spread over the bill rate—the spread Friedman himself suggested being 200 basis points).[123] And, of course, he wanted contemporaneous reserve accounting introduced, so that total reserves or the monetary base could be set in advance by the Federal Reserve (*Newsweek*, July 28, 1980).

Monetary contraction during the controls period

As they were announced on March 14 and were fully removed on July 3, the period in which the credit controls were in force coincided closely with the second quarter of 1980. The forces, associated with the credit controls, that were tending to reduce credit supply and demand were manifested in a dramatic reversal in the pattern of consumer indebtedness, with April 1980 seeing $2 billion more repaid than added—the first case of a net repayment of households' aggregate debt since May 1975 (*Kansas City Star* (Missouri), June 16, 1980). The controls also dramatically affected the money stock. The controls episode would be associated with a sharp slowdown in the monetary aggregates in the second quarter. And in what may have been a belated overreaction to the period of monetary contraction, money then expanded very rapidly after May 1980.

The major repercussions for the behavior of the monetary aggregates in the second quarter are evident in Table 2. M1 fell heavily in April. This pronounced decline was not immediately

[123] See Friedman (1980a, p. 5; 1982c, p. 117). (The 200-basis-point number was in the 1980 discussion, while in Friedman, 1981c, p. 4, he suggested a 200 or 300 basis-point spread.) This, of course, was Friedman's recommendation in the event that (contrary to some of his more sweeping reform proposals) the discount window was continued. Along lines similar to Friedman's criticism of the Federal Reserve's management of the discount window, Goodfriend (1983, p. 354) criticized the Board's "irregular use of the discount rate surcharge" and its nonprice methods of managing lending—and suggested that these be replaced by a consistent, and publicly stated, policy on the spread between the discount rate and short-term market interest rates. He argued that it should do so whether it used the federal funds rate or a reserves series as its target in its operating procedures (pp. 352, 355). In a similar vein, William Poole responded to the discount-rate surcharge by stating: "What we need is assurance that money growth will be controlled over the long haul—that stopping disturbing reserve creation through then discount window is not just a problem for today but forever. If bank borrowings at the discount window sometimes produce a problem in money control, then the solution is a permanent reform in the discount window. What should have been done is to tie the discount rate to market rates of interest, setting it, say, at a level one percentage point above the average of the three-month bill rate in the previous week." (*American Banker*, March 26, 1980.) This recommendation largely foreshadowed the way in which the Federal Reserve Board set discount rate starting in 2002—although, by then, the FOMC was announcing a public target for the federal funds rate, so the discount rate was expressed as a spread, usually fixed at 100 basis points, over that target.

Table 2. Monetary growth in 1980, by quarter

	Annualized quarterly growth rates			
	1980:Q1	1980:Q2	1980:Q3	1980:Q4
M1	7.5	–2.2	14.7	10.5
M2	7.2	6.1	12.6	8.8

Source: Computed from quarterly averages of data in Federal Reserve Bank of St. Louis' FRED portal.

made up for—the subsequent rebound, though starting in May, mainly being felt in the third-quarter growth rate—and so the decline dominated the behavior of M1 growth in 1980:Q2 computed on a quarterly-average basis. During this period of M1 contraction, M2 continued to grow, although at a slower pace.

Despite his continuing general preference for M2 over M1, Friedman highlighted the behavior of M1 during the credit-controls period and, in particular, the contraction in that aggregate that was concentrated in April 1980. For example, a mid-July *Newsweek* column on the subject of "Monetary Overkill" plotted M1 growth and noted that the "decline in the rate of monetary growth from February to May is the largest three-month deceleration in monetary growth since at least 1959."[124] And a year after the controls episode, he continued to note the "unprecedented decline in [the] money supply" in February-May 1980 (*Boston Globe*, April 1, 1981, p. 39).

Friedman's focus on M1 may have been in part because, as in the 1966 credit crunch, the very weakness of M1 in relation to M2 may have itself been an alarm bell pointing toward contractionary forces. But this focus also arose from the fact the Volcker 1979–1982 Federal Reserve, despite continuing with multiple monetary targets, was most concerned with M1—Friedman would characterize the October 1979 change as one in which "the Fed pledged allegiance to M1 control" (*Wall Street Journal*, August 20, 1985)—and so it was on that series' behavior against which the FOMC's targeting success was most straightforwardly assessed during this period.[125]

[124] *Newsweek*, July 14, 1980. The reference to 1959 was made because the newly redefined M1 data started in January 1959.

[125] Even before the October 1979 change, Volcker had called the M1 "the most central" of the monetary targets (in testimony of September 26, 1979, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1979c, p. 21).

As it turned out, the growth rates of nominal income and money were by no means in lockstep over 1980, irrespective of whether M1 or M2 was consulted as the monetary series. But the fall in M1 during February-May 1980 accurately conveyed, albeit with only a couple of months' advance notice, the fact that a severe economic contraction was ahead, while M2 gave too sanguine a signal. Some reasons why M1 deviated so much from M2 in this period are considered later. For now, it is worth considering why M1 fell.

Reasons for the M1 decline: bank loan repayment and weakness in reserves

In his *Newsweek* analyses of the time, Friedman was inclined to take the decline in M1 as an avoidable event but not to go into a detailed account of how it occurred. There was, in fact, not a single reason for the decline, as it reflected both Federal Reserve and private-sector actions (or inaction). In terms of the factors driving the decline in M1, weakness in the aggregate quantity of reserves supplied to the banking system by the FOMC (and the Federal Reserve Board) did play a role in the M1 decline. But even more so, the lack of expansion of reserves was significant because it meant that little offset was provided to the main source of the M1 fall—a step-down in the M1 multiplier.

Mechanically, the lower M1 multiplier was principally associated with a rise in the private sector's currency-to-checkable-deposits ratio. At the time, some observers, including monetarists such as Karl Brunner, interpreted this move in the currency/M1 deposit ratio as reflecting a decision by the nonbank public to mediate more of their transactions through currency rather than through credit on account of the new impediments to attaining credit.[126]

Although this factor likely played a role, it probably did not amount to the most important factor driving the decline in M1 deposits and the money multiplier. It seems unlikely that households in 1980 would have retreated so rapidly into a cash-centered mentality in response to the credit controls. Instead, it is more plausible that the decline in the M1 multiplier was an arithmetic reflection of another response to the credit controls: a process of heavy deleveraging by households, who reduced their indebtedness to banks and in so doing lowered their checking deposit balances. Such deleveraging was not a mechanism typically stressed in historical accounts of money-multiplier behavior of the kind given by Friedman and Schwartz, but it was

[126] See Brunner (1980, p. 22). For other accounts stressing this factor, see Weintraub (1981, p. 17) and Newton (1983, p. 100). Jerry Jordan offered the same interpretation, albeit somewhat more cautiously, when he noted (in his written answer of August 27, 1982, in Joint Economic Committee, U.S. Congress, 1982b, p. 93), that, during the controls period, "the public appears to have decided to hold a larger share of their money balances in the form of currency as a result of the limitations imposed on the use of credit cards."

not inconsistent with their characterizations of the basic factors affecting the multiplier.

Paul Volcker appealed to the deleveraging process in a 1982 retrospective: "The marked contraction in borrowing after the [credit-controls] program was instituted, and the resurgence in borrowing as it was unwound, led to sizable fluctuations in money balances and interest rates—first downward then upward."[127]

A more precise statement of the sequence of moves in interest rates and money would acknowledge that interest rates initially *rose* in March-April 1980: Volcker himself recalled that controls initially "pushed [rates charged on] instalment loans from 14 to 15½ or 16 percent" (A. Smith, 1989, p. 25), and short-term market interest rates rose to even higher rates than this. For example, in early April 1980 the federal funds rate reached a then-record value of 19.38 percent—the same period in which the administered, though market-linked, prime rate reached 20 percent (*Burlington Free Press* (Vermont), April 5, 1980*a*, *b*). But the period in which the credit controls were associated with rising yields was about three weeks.[128] Beyond this point, interest rates fell dramatically through July, before rebounding over the rest of the year. As for monetary growth, as measured by M1, it was negative in the spring of 1980, before turning around dramatically and registering very high rates over the rest of the year.

The joint movement of interest rates and money during 1980 will be discussed at length below. Ahead of that, it is worth focusing on the borrowing-to-deposits, or deleveraging, mechanism to which Volcker referred and that he stressed as the key factor behind the decline in the money stock during the credit-controls episode.

There was considerable merit in Volcker's interpretation. The weakness in M1 and M2 in this episode did, indeed, seem to reflect an initiative of the private sector in reducing its outstanding bank loans. That households might be able to reduce the nominal quantity of money outstanding by repaying bank loans was a longtime argument invoked against the quantity theory of money, both on the grounds that it provided for a separation of central-bank actions and money stock determination and on the grounds that it provided apparent grounds for believing that households might keep money balances and spending in line with each other by adjusting their deposit balances rather than their expenditures (see, for example, Kaldor, 1970; A.J. Brown and J.

[127] From Volcker's written answer of October 19, 1982, in Joint Economic Committee, U.S. Congress (1982b, p. 32).

[128] Wallich (1980b, p. 8) noted: "Interest rates rose further following the March 14 [introduction of] controls but quickly peaked and thereafter dropped dramatically."

Darby, 1985, p. 385).

But loan repayment was, in fact, a factor that Friedman accepted as an influence on the money stock, particularly in the short run—though not as a reason for denying the central bank's ability to steer the money stock. The notion that lowering their indebtedness to banks was an option available to individual members of the private sector as a means of reducing money balances was acknowledged by Friedman in the early 1970s in his solo- and coauthored work.[129] The reason he nevertheless favored analyses that traced the behavior of monetary aggregates to central bank actions and that linked money and spending lay in the distinction between *individual* and *aggregate* means of eliminating excess money holdings. In particular, if an individual household reduced its deposit balance by the repayment of a loan extended by the bank, the banking system's reserve/deposit ratio would be increased, and banks could be expected to restore the ratio (and so realign the money stock with the amount of base money outstanding). This argument would be advanced explicitly by Anna Schwartz (1986a), as it was by earlier defenders of the monetarist account of money-supply behavior such as Masera (1971).[130]

Nevertheless, loan repayment probably played a major role in the mid-1980 M1 contraction. For one thing, the replenishment of the former reserve-deposit ratio by banks did not necessarily occur immediately in response to a large-scale loan repayment (of the kind seen in the six weeks after credit controls were imposed in mid-March 1980).[131] For another thing, commercial banks

[129] See Friedman (1970c, p 195) and Friedman and Schwartz (1970, p. 149). See also Friedman and Schwartz (1982a, pp. 18, 58, 261) and Friedman (1968c, p. 441).

Judd and Scadding (1981, p. 21), although concluding that "large swings in bank loans, induced primarily by the Special Credit Control Program, were the major source of money's variability in 1980," claimed: "This explanation has no role in conventional models." Their grounds for asserting this was that "conventional models" had agents being on their money demand function at all times and so required fluctuations in money balances to be accounted for by real income, prices, and interest rates. In a simultaneous-equation model, however, there need be no contradiction between the banking system being a source of fluctuations in the nominal money stock and that money stock being equal to the nominal quantity of money demanded. So the explanation Judd and Scadding offered was reconcilable with such a conventional model. Furthermore, in such a model, a change in the nominal money stock might be consistent with the continuous equalization of money demand and supply money, even when there is little change in current values of real income, prices, and interest rates. In particular, this might be the case if the expected future values of real income also matter for money demand (as it did in Friedman's theory of money demand: see Nelson, 2020a, Chapter 6).

[130] The mechanism by which loan repayment might play an important role in money stock contraction was advanced by some proponents of the quantity theory, as well as by critics. See Demeulemeester (2022).

[131] Paul Volcker gave a loans-based explanation of the M1 contraction in a retrospective quoted above as well as in *New York Times* (September 19, 1982, p. 72), in which he stated that "the economy had this abrupt fall, and the money supply fell very rapidly with it… [in] reaction precisely to the consumer-credit controls. Consumers suddenly thought they'd better not use their credit cards—or consumer credit at all. But they had bills to pay. And so they drew down their cash balances. So you had this wild decline in the money supply for six weeks or so." In a challenge to Volcker's account, Greenfield (1987, p. 225) suggested that the Volcker quotation just given implied, incorrectly, that "the mere expenditure of money causes it to be extinguished." But the rundown in consumer credit

might during the credit-control period have been discouraged from replacing repaid loans with new loans and may instead have bought more securities. This would certainly have tended to replenish the stock of money as a whole—and may help account for the resilience of the M2 money stock during the controls period. But it may not have necessarily restored M1 one for one in a prompt manner.[132] The upshot is that loan repayment by households likely was an important force reducing M1 in the short run and that countervailing actions by banks to restore the size of their overall earning assets may have been more successful in bolstering M2 than M1.

With loan repayment a force making for a decline in the M1 multiplier, central bank action in the form of provision of a sufficient increase in the stock of bank reserves would have been needed to keep M1 up, or rapidly restore its decline, in the credit-controls period. This did not happen. That it did not happen contributed importantly, Friedman believed, to the severity of the second-quarter U.S. economic decline (*Newsweek*, July 14, 1980). He would contend that the contractionary economic effect of the credit controls had been strongly reinforced by the fact that the FOMC allowed the controls to produce pronounced weakness in retail-deposit series monetary aggregates like M1 or M2.

The Federal Reserve in March-June 1980 did not maintain bank reserves at a level sufficient to maintain M1, let alone keep it growing along its target path. Indeed, the FOMC allowed total bank reserves (seasonally adjusted) to decline during the credit-controls episode. It is therefore appropriate to conclude that although the M1 multiplier was clearly disturbed by the credit controls in a way that put downward pressure on M1—as discussed above and as shown in detail by Rasche and Johannes (1987, pp. 151–158)—the FOMC's actions during the episode on

to which Volcker referred was repayment of bank loans—and such "expenditure" would have the (initial) effect of extinguishing money—as households in that process would simultaneously reduce their debits and credits *via a vis* the commercial banking system. Consequently, this Volcker quotation, as well as others in the same vein (including his statement on July 21, 1980, that, in the early controls period, "consumers and others hastened debt repayment at the expense of cash balances"—see Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1980b, p. 100) did not reflect fallacious reasoning.

A Volcker quotation on the matter that was more vulnerable to criticism was his statement in July 1980 that "the demand for money subsided… *because* consumers and others hastened debt repayment" (July 23, 1980, testimony, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1980b, p. 9; emphasis added). Although it was legitimate to explain a decline in the nominal money supply in terms of debt repayment, the demand for real money balances should not have been taken as having necessarily moved down by an equal amount.

[132] Although, on a sustained basis, bank lending should have a closer numerical and accounting relationship with broader monetary totals than with an M1-type aggregate, it was widely accepted that, in the United States in the early 1980s, M1 deposits had a fairly close short-run relationship with the behavior of bank loans, on account of the fact that the creation of a new bank loan typically entailed the crediting of the loan funds to the borrower's checking account. For example, Tobin (1983a, p. 168; p. 534 of 1996 reprint) noted that "an M1 bulge accompanies an expansion of [bank] lending," while Judd and Scadding (1982, p. 870) referred to a sequence involving the "lowering [of] bank loans, which reduces M1."

balance not only did not offset this downward pressure, they also had the effect of reinforcing the M1 decline by allowing commercial banks' reserve balances to decline somewhat.

FOMC members actually indicated publicly at the time about the fact that it would not try to correct the M1 decline promptly. This approach was foreshadowed in a speech by Federal Reserve Board governor Emmett Rice given on May 7, 1980, as the data showing M1 declines were coming in. Although Rice acknowledged that more rapid growth in reserves would help bolster monetary growth and so rapidly offset the M1 decline, Rice pointed to the possibility that large injections of reserves to bolster current monetary growth might generate "a buildup of liquidity" (Rice, 1980, p. 8) and so, he implied, to over-rapid monetary growth in the future.[133]

This point of view, however, neglected the apparent reality that—despite some forces making for lags between movements in reserves and monetary growth in this period—reserves and money primarily had a contemporaneous or same-quarter relationship. Statistically, M1 and M2 growth rates were related to same-period monetary base growth in sample periods through the very early 1980s (see Hafer, 1981, p. 13). Furthermore, analyses of the components of the M1 and M2 money multipliers in the United States (including the revised definitions of these series) supported the notion that movements in reserves growth would likely have been reflected promptly in monetary growth, particularly M1, during the nonborrowed reserves regime of 1979–1982 and during the previous funds-rate-oriented regime (see Rasche and Johannes, 1987; Rasche, 1993b, pp. 44–47; and Feinman, 1993, p. 569).

In addition to this statistical evidence, there were basic economic grounds for believing in a dependence of deposit growth on reserves growth. Paul Volcker himself indicated in his testimony in the early 1980s that the Federal Reserve actions had a substantial same-quarter influence on monetary growth and that policymakers could have contained the 1980 M1 decline more rapidly than they chose to.[134]

[133] Essentially the same argument was made by Higgins (1982) in a research paper that defended the New Operating Procedures as practiced under Volcker against Friedman's criticisms as articulated in *Newsweek*, December 1, 1980. But, in summarizing his defense in the statement that "Federal Reserve actions affect monetary growth with a lag" (p. 3), Higgins obscured the fact that he and other defenders of FOMC policy did not dispute the point that Federal Reserve actions *also* affected monetary growth in the same month or quarter.

[134] See, for example, Volcker's statement of June 15, 1982 (in Joint Economic Committee, U.S. Congress, 1982c, p. 455): "The money supply, in the short run, responds to our actions, our current actions and, more importantly, the actions that we took a few weeks or months ago." See also his discussion, in testimony of July 22, 1980, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1980b, p.100), of the Federal Reserve's reaction to the spring 1980 M1 decline.

Finally, the basic movements of total reserves during 1980 was consistent with the behavior of M1: and in particular, as Gilbert (1983, pp. 45–47) showed, the amounts of total reserves supplied by the FOMC in the spring of 1980 were below those estimated as the level consistent with achievement of the M1 target, and those supplied later in the year were above the target-consistent level. It followed that allowing reserve growth to be rapid during the credit-controls period, then slowing reserve growth (instead of speeding it up) after the controls period would have been associated with more stable monetary growth over the year. Such behavior lent credence to Friedman's later judgment (*Wall Street Journal*, February 1, 1982) that the FOMC knew "how to produce stabler growth" but did not attach the priority that he did to this aim. And the target deviations in large measure reflected conscious FOMC decisions. For, notwithstanding its interest in M1 targeting and focusing FOMC policy on total reserves for that purpose, the FOMC consciously took measures in the course of the year that veered reserves away from the levels judged *most* likely to meet the M1 target. So credit controls were not the clinching reason for either the magnitude of the quarterly undershoot of the 1980 M1 target during 1980:Q2 or the notable overshoot of the M1 target for the year as a whole.

The reason they did so was also mentioned by Rice and would later be focused on by Friedman in one of his written accounts (*Wall Street Journal*, January 30, 1981): the federal funds rate constraints. As Jerry Jordan characterized monetary policy during the credit-controls period: "The Federal Reserve was not willing to see market interest rates drop as drastically as market forces seemed to imply during the first few weeks of the controls, so they provided fewer (drained more) reserves from the banking system."[135] Rice (1980, p. 2) noted that the March 1980 FOMC meeting had set a 13 to 20 percent tolerance range for the federal funds rate to accompany the monetary target. By the time of Rice's speech, the FOMC had, at its meeting of April 22, 1980, cut the band to 13 to 19 percent (Federal Reserve Board, 1980, p. 115; Federal Open Market Committee, 1980a, p. 35). It was these high-valued ranges agreed to in the March and April meetings that led, in the face of the large decline in demand for credit during the controls period, to the decline in reserves to which Jordan referred and to monetary policy's failure to provide a prompt reversal of the M1 decline.

Again, policymakers provided their rationale for this behavior publicly. In addition to his reluctance, already noted, to providing reserves that he felt could unduly boost future monetary growth, Rice (1980, p. 7) cited the repercussions for short-term interest rates. Rice pointed toward the possibility that, as financial markets traditionally interpreted funds-rate declines to be

[135] Jerry Jordan, written reply of August 27, 1982, in Joint Economic Committee, U.S. Congress (1982b, p. 93).

relaxations of policy, allowing a large decline "might be interpreted by market analysts as indicating an abrupt shift by the Federal Reserve towards monetary ease." This, he suggested, was a reason policymakers should not authorize a major increase in the supply of reserves and a much lower interest-rate target range, even though this combination would be more consistent with meeting the monetary target than would a combination of a smaller reserves increase and a little-changed target range. This reasoning was also cited in the published record of the policy actions for the April 1980 FOMC meeting (Federal Reserve Board, 1981a, p. 117).

In the event, the FOMC at a subsequent special meeting on May 6, 1980, adjusted the federal funds rate tolerance range to a much lower band (Federal Open Market Committee, 1980b). This helped bring the monetary contraction to an end and, even ahead of this, the week ending April 28, 1980, saw M1 reach its trough. The fact that the FOMC did cut its tolerated interest rate sharply in May, along with the associated sharp fall in the actual federal funds rate in May and June, underlies latter-day interpretations of monetary policy in 1980 that portray the credit-controls period as one of extreme monetary ease. However, it should be stressed that the May policy adjustment was a rearguard action taken to *arrest monetary contraction*—and so make monetary policy more consistent with achievement of the preexisting monetary-aggregate target. Indeed, the M1 series was not even back to its pre-credit-controls value until mid-June 1980.[136] Therefore, the amount of policy adjustment made was on the low side compared with that needed to achieve the M1 target.

The contention that the FOMC's federal funds rate targets were too high in April-June 1980—though too low later in the year—for achievement of the monetary target was articulated by numerous commentators at the time (for example, by John Paulus in *New York Times*, October 5, 1980).[137]

[136] See the weekly data on M1, available in the Federal Reserve Bank of St. Louis' FRED portal at *https://fred.stlouisfed.org/series/M1#*.

[137] In contrast, the account given in Meltzer (2009b, pp. 1054–1056) articulated the latter-day interpretation of 1980 developments in which the FOMC and Volcker were too loose in this period—and, in particular, that they lost their resolve in April-June, abandoned their disinflation strategy, and had "to start again" in late 1980 (p. 1054). In contrast to this interpretation, the April and early May decisions by the FOMC (its April 22 meeting and May 6 telephone meeting) did not entail a reversal of policy in the direction of an easier stance and, especially in the case of the April meeting, amounted to a case of the Committee not providing enough reserves to shore up and restore M1 deposits. Meltzer recognized this nature of these decisions—"Reluctance of the FOMC to allow to funds rate to decline reduced reserve[s] growth" (p. 1056), only to contradict this point on the same page by summarizing the FOMC's second-quarter-1980 policy as follows: "the Federal Reserve responded to high interest rates… by giving up its anti-inflation policy." (Even if Meltzer's summary covered the later May and June period when the FOMC was indeed taking actions that added to monetary growth, the summary would not be accurate, as it occurred against a background of interest rates *lower* than had been experienced in recent years.) This matter is discussed further below when the latter-day interpretation of FOMC behavior in 1980 is critically considered.

Friedman discussed the matter on January 22, 1981, at one of his Oppenheimer and Company briefings in New York City. His remarks on the matter were summarized as follows: "If one were to ask the Fed governors what their mistake was last spring, they would probably respond that it was to let interest rates decline too fast. That was not [in fact] their mistake. Rather, their mistake was to push the fed funds rate too high initially, then to keep it too high for too long and subsequently to push it too low."[138]

With regard to M2, its growth slowdown, to 7.3 percent in 1980:Q2 (see Table 2 above), appears fairly mild. But this mildness stemmed partly from the initial conditions: the M2 was growing strongly in late 1979 before the October 6 policy changes, and it had then stepped down. So a distinct slowdown in the aggregate was already in force before the credit controls were instituted. The further M2 slowdown associated with the credit controls showed up more sharply in monthly data than in the quarterly average. In particular, as Friedman pointed out in 1981, the trough in six-month growth of M2 growth of 5.8 percent (annualized rate) in April 1980 represented a halving of M2 growth from late-1979 rates.[139] As of this time, M2, like M1, was on a trajectory that suggested that it would undershoot the level associated with its target range for 1980 (Volcker, 1981, p. 17).

Friedman reacts

As already noted, Friedman did not give a very detailed analysis of how credit controls had come to affect the money stock. But he was emphatic that they had done so, and he specifically pointed to the behavior of M1. In retrospectives, he would be scornful of explanations that treated the M1 contraction as simply an accommodation by the commercial banking system of a downward shift in real money demand.[140] Underlying money demand, he contended, had *not* contracted—so what was observed was a contractionary shock to the money supply, with adverse repercussions for real economic activity.

Friedman would later stress that the decline in the money stock actually preceded the imposition

[138] Oppenheimer and Company (1981a, p. 7).

[139] *Wall Street Journal*, July 30, 1981. The 5.8 percent figure remains valid on modern revised data for M2 (despite notable changes in the M2 definition after 1980 that have been applied retrospectively to the modern series). Friedman would also point to a sharper slowdown in M2 growth—to 0.1 percent annualized growth—in the narrower timeframe of February-April 1980 (Oppenheimer and Company, 1981b, p. 5). This number was partly a reflection of the fact that data as of 1981 indicated that M2 had declined slightly on average in April 1980 (Council of Economic Advisers, 1981, Table B–59, p. 301). Modern M2 data suggest instead a slight rise in that month.

[140] See Friedman (1982d) and *Wall Street Journal*, January 30, 1981.

of credit controls (*Newsweek*, December 1, 1980).[141] The decline in M1 began in the second half of February and then intensified after the mid-March 1980 imposition of credit controls.[142] This record led Friedman to condemn Federal Reserve policy at the conference that he and Volcker attended in New Orleans in early June 1980. During his conference appearance, Friedman called for the Federal Reserve to abolish credit controls, abandon entirely its tolerance band for the federal funds rate, and boost the quantity of bank reserves to get money back on target (*American Banker*, June 3, 1980). Confronted by reporters about Friedman's critique, Volcker indicated that he was not impressed, simply saying, "Oh, Milton," before heading for the restrooms (*Wall Street Journal*, June 3, 1980).

The parting of the ways between Friedman and Volcker that occurred during the controls period was permanent. It was only on rare occasions that Friedman had nice words to say about Volcker in the remaining period of Volcker's tenure as chair—even after Volcker had got inflation down.

Friedman took strong exception to the conduct of monetary policy during the period of credit controls. Instead of insulating the money stock in the face of a credit contraction, the Federal Reserve had permitted a monetary contraction, manifested most clearly in the decline in M1. Just as he and Schwartz had criticized the Federal Reserve for allowing the money stock to decline when loans fell during the Great Contraction, Friedman saw the (far milder) absolute decline in M1 observed during the 1980 credit controls as an indictment of the Federal Reserve. This aspect of the credit-controls episode, Friedman suggested, should be blamed on the Federal Reserve rather than on Jimmy Carter. He observed in 1985 that "the volatility resulting from the credit controls should be blamed on President Carter" but "the Federal Reserve was responsible for exacerbating the effects of the credit controls by permitting an excessive decline in the money supply in response to the imposition of the controls."[143]

Reflecting this diagnosis, in his early June 1980 conference appearance Friedman attacked what

[141] Dornbusch and Fischer (1987, p. 450) gave the annualized M1 growth rates (according to data at roughly the time Friedman wrote his December 1980 *Newsweek* column) as: February 1980, 10.4 percent; March 1980, −0.3 percent; April 1980, −13.2 percent; and May 1980, −1.2 percent. Pierce (1980, p. 255) stated that M1 "fell sharply in February, March, and April of 1980 and then rose rapidly in May [1980]." This characterization was roughly consistent with the Dornbusch-Fischer numbers because the February decline and May rise were intra-month turnarounds in M1 that showed up initially in the weekly M1 data and not clearly in the monthly averages. However, modern weekly data on M1 show the series' decline being completed by the start of May 1980, and modern monthly-average data show seasonally adjusted M1 rising in May.

[142] Again, see the weekly data (available at the Federal Reserve Bank of St. Louis' FRED portal at *https://fred.stlouisfed.org/series/M1#*).

[143] Friedman (1985b, p. 58).

he labeled the “incredibly restrictive” monetary policy in force, which he saw as partly reflecting the Federal Reserve’s actions in limiting the decline in the federal funds rate (*American Banker*, June 3, 1980; *Wall Street Journal*, June 3, 1980). Writing for *Newsweek* about a month later, Friedman pronounced a negative verdict on the execution of the new operating procedures, declaring that the erratic money growth of the preceding months was a “disgraceful performance” that “confirms the doubts rather than the hopes” about the new regime (*Newsweek*, July 14, 1980).

Economic contraction intensifies

By the time this column appeared, the degree to which the monetary contraction had a counterpart in national economic contraction. As far as Friedman was concerned, the recession had really begun in 1979, because of the slowdown in economic growth that occurred then (*San Diego Union* (California), July 14, 1981).[144] He suggested that the first quarter of 1979 might really be regarded as the time of the cyclical peak of the economic expansion that began in 1975 (Oppenheimer and Company, 1980, p. 1; 1981b, p. 1). This 1979 slowdown had followed a weakening of monetary growth, as discussed in Chapter 10. The reduction in monetary growth that approximately coincided with the October 1979 policy change was what Friedman would come to believe was what ushered in what the NBER classified as the peak of the U.S. economy in January 1980. With regard to this episode and others during the 1979−1982 period, he would judge the lags from monetary policy to output as unusually short: usually only about a quarter.[145]

Friedman accordingly viewed the spring-1980 steepening of the economic decline in terms of the decline in M1 that began in February 1980. The U.S. economy showed a historically severe contraction in 1980:Q2, in the wake of the imposition of controls: initial estimates showed real GNP contracting at an annualized rate of 9.1 percent (*Financial Times* (London), July 19, 1980). Over numerous subsequent GNP data vintages, the second-quarter 1980 contraction was measured as being as high as 9.9 percent at an annual rate (see, for example, Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1982a, p. 341; Goodfriend, 1993, p. 11). Even in light of subsequent revisions (including the switch from real GNP to real GDP as the output measure), data today show a deep fall in output 1980:Q2: an 8.3 percent annualized output decline during that quarter.

[144] See also Friedman (1984b, p. 36).

[145] See, for example, *Newsweek*, July 12, 1982, and August 23, 1982, as well as Friedman (1983e, pp. 9–10; 1984b, pp. 28–30; 1984c, p. 399; 1985b, pp. 54–55). See also the discussion titled “Henry Wallich” below.

Friedman readily granted that part of the output decline reflected the effect of credit contraction on the economy. Consistent with this attitude, in 1982−1983 he told Ben Bernanke that he believed that credit supply shocks could exert an important influence on the economy for a given money stock (Ben Bernanke interview, February 19, 2014), and Friedman's 1984–1985 retrospectives on the 1980 credit controls would grant them a sizable role in generating the sharp 1980:Q2 fall in output.[146] He did view credit as having a much looser relationship with the economy than money did, however, in part because he believed that the demand for credit had much instability.[147] But that the Carter credit controls had been a contractionary force for aggregate demand was a matter he did not dispute.

In May and June, as monthly indicators started to bring out the extent of the economic decline, *Business Week* (May 12, 1980) congratulated Friedman for accurately predicting that a recession would occur in the first half of the year. Friedman himself focused on the recent deepening of the recession. He pointed to the rise in the U.S. unemployment rate from 6.2 percent to 7.8 percent in March-May 1980 and suggested that it was linked to the February-May decline in M1 (*Newsweek*, July 14, 1980).

Removal of the controls

By the time Friedman's column appeared in print, the credit controls had been lifted (*Kansas City Star* (Missouri), July 3, 1980). The complete removal of the controls just before Independence Day 1980 followed an earlier substantial relaxation of the credit controls (including the marginal reserve requirements) in the second half of May (*Financial Times* (London), May 23, 1980; Meltzer, 2009b, p. 1056).

Speaking four months after the end of the controls, Federal Reserve chair Volcker described the experiment as having "suggested to me, strongly, again, the difficulties of pursuing credit control as anything like a permanent policy… There is even difficulty in pursuing it as a temporary policy."[148] In mid-1981, he assessed the long-term reaction to credit controls in a manner that echoed Friedman's longstanding critique of such controls—that they generated evasion—with

[146] See, for example, Friedman (1984c, p. 399).

[147] See *Wall Street Journal*, January 30, 1981 (specifically, the portion reprinted in Friedman, 1983a, pp. 243–244) as well as Nelson (2013, p. 64; 2020a, Chapter 6).

[148] From Volcker's testimony of November 19, 1980, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1981a, p. 53). Similarly, Volcker remarked in February 1981 that the credit-controls episode had brought out "some of the real problems that arise" from direct intervention in credit allocation (testimony of February 25, 1981, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1981a, p. 11).

Volcker observing that they "don't work anymore because the market is too fluid" (*Wall Street Journal*, June 2, 1981). He mentioned the evasion problem again in Congressional testimony shortly afterward, in a discussion that also noted of credit controls: "we found out quite clearly last year that, first of all, that shouldn't be done."[149] The federal government's legal power to impose credit controls would be repealed in 1982 (Schreft, 1990, p. 47; Meltzer, 2009b, p. 1056). In that same year, Volcker reaffirmed that in addition to the "philosophical problem" associated with credit controls that they violated a key premise of economic policy (that specific decisions on credit allocation should be left to the market), there also existed the "horrendous practical problem" that "credit controls are extremely hard to enforce."[150]

Money versus interest rates, monetary policy stance, and Volcker's resolve

Gordon (1993, p. 261) remarked that "the Volcker-led Federal Reserve Board… maintained a steadfast anti-inflationary policy during the 1979–82 period." As of 1993, this was a seemingly uncontroversial statement about the conduct of monetary policy during Volcker's stewardship of monetary policy (as well as of the behavior of his Federal Reserve Board and FOMC) over the course of 1979–1982. The Gordon statement also likely still probably corresponds to the popular consensus regarding that period. But it has come under attack in latter-day accounts by researchers.

Paul Volcker had observed in early 1980 (*Kansas City Times* (Missouri), January 3, 1980) that a key question was: "Will the Fed stick with it?" According to some prominent latter-day interpretations of monetary policy developments during 1980, the answer to Volcker's question was "no." These later accounts have suggested that the Federal Reserve abandoned its October 1979 disinflationary strategy in mid-1980 and so had to restart with a new launch of a disinflationary policy in the later months of 1980. This interpretation of the course of monetary policy over 1980 was advanced forcefully by Goodfriend (1993, pp. 10–11), endorsed by Erceg and Levin (2003, pp. 919–920), and elaborated by Goodfriend and R.G. King (2005)—being embedded in these authors' declaration that the "Volcker Fed's initial inflation-fighting effort was abandoned in mid-1980 with the onset of credit controls" (2005, p. 986).

Although Milton Friedman certainly himself thought Federal Reserve policy went astray during

[149] From Volcker's testimony of July 21, 1981, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1981c, p. 251).
[150] From Volcker's testimony of May 5, 1982, in Committee on the Judiciary, U.S. House of Representatives (1983, p. 84).

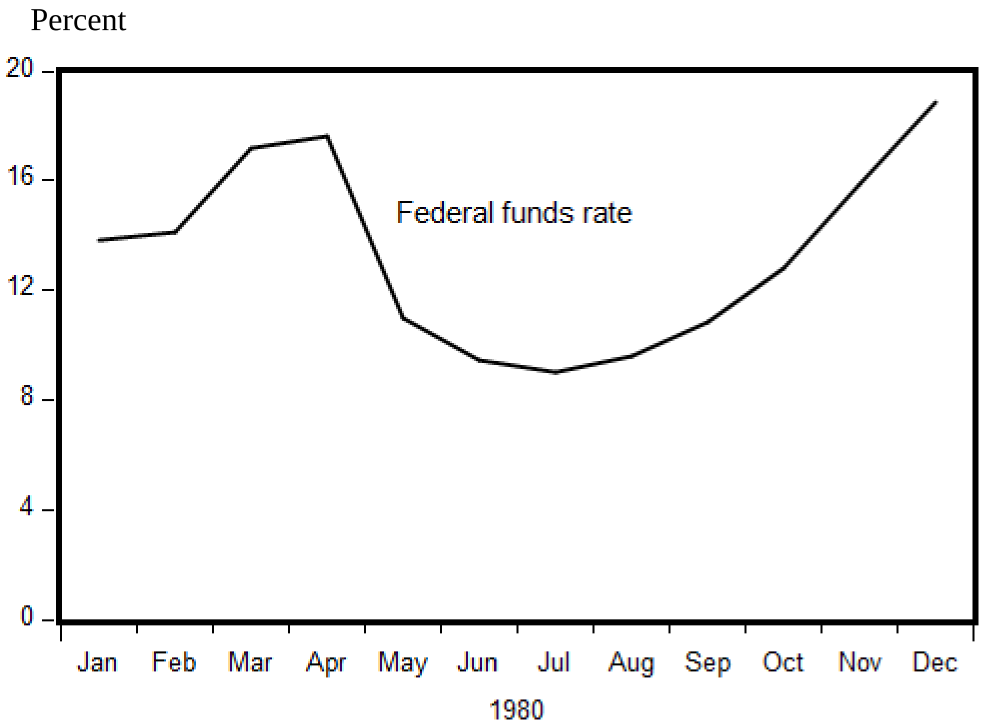

Figure 3. Federal funds rate (monthly average), January-December 1980.
Source: Federal Reserve Bank of St. Louis' FRED portal.

1980—and said so at the time—the analysis of events that he put on the record does not actually support the latter-day interpretations just described.

The reason is simple: the latter-day interpretations rely on the fact that the federal funds rate fell sharply in the controls period—see Figure 3—and take this fact as the judge and jury in assessing whether monetary policy was loose or tight. Friedman, in contrast, took the monetary-aggregate contraction of around mid-1980 as implying that monetary policy was "incredibly restrictive."[151]

In November 1980, in one of his earliest retrospectives on the credit-controls episode, Paul Volcker noted the conflicting signals given by interest rates and money and also anticipated the latter-day literature that would accuse him of losing resolve in 1980: "during that period we were subject to rather conflicting criticisms… There were those who said [that] this decline in the money supply was gravely exacerbating [the] recession. There were others who said, 'No, you have given up on any monetary restraint because interest rates are going down so far. You are much too easy.'"[152] Friedman was among those articulating the first of these criticisms, while the

[151] See the discussion above.
[152] From Volcker's testimony of November 19, 1980, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1981a, p. 54).

accounts cited above in economic literature since 1993 have joined the "others" claiming that Volcker's FOMC was "much too easy" in mid-1980.[153]

The position taken here is that, as Friedman suggested, monetary policy *was* tight during the credit controls period, and that, as Volcker implied, and contrary to the position taken in the research literature starting in 1993, the October 1979 FOMC disinflationary strategy *was not* abandoned in mid-1980 and then restarted. The basis for subscribing to this position is given in detail in Meade and Nelson (2023) with a focus on statements made by Volcker at the time. It will be covered more briefly here, with a focus instead on Friedman's position on evaluating monetary policy stance and on the behavior of the monetary aggregates.

As the contrast between the behavior of money and interest rates in mid-1980 indicates, the judgment that monetary policy *was* tight during the credit-controls period in part relies on favoring (for that period) monetary aggregates as the indicator of monetary stance.[154] But the same judgment that monetary policy was tight can be made by appeal to the behavior of interest rates, provided that one appeals to three points about interest rates that Friedman stressed.

The first is that the neutral or natural rate of interest has a cyclical component. Friedman frequently cited the fact that interest rates tended to fall in recessions as the demand for credit declined. In January 1980, he affirmed that this was still his view—his main qualification being that increasing U.S. government demands for credit in recession periods tended to make the decline more limited than was previously the case.[155]

The contention that interest rates tended to fall during recessions, for a given stance of monetary policy, was not special to Friedman. Indeed, during 1980, Paul Samuelson and James Tobin both explicitly took interest rates as having a procyclical character that meant that the neutral interest rate fell during an economic downturn. Samuelson remarked (*Newsweek*, April 28, 1980) that the "Federal Reserve will not resist a drop in interest rates brought on by a general weakening of the economy," and James Tobin observed: "A good way to have low interest rates

[153] The challenge of interpreting the stance of monetary policy in this period has recently been stressed by Blinder (2022a). Baumol and Blinder (1982, p. 248) earlier noted that "the Fed came in for heavy criticism for letting rates fall so quickly," while late in the year "again the Fed was vilified." Friedman certainly was among those criticizing policymaking in late 1980, but with regard to the initial interest-rate decline, his position was in effect that it should have been more rapid and larger—but then should have reversed course more quickly (as this sequence of moves would likely have been that consistent with keeping M1 on target).
[154] It is, also, however, consistent with the public and internal record of policymakers' statements over 1980. See, in particular, the Solomon statement given below.
[155] Friedman (1980d, p. 84).

is to have a good severe recession. There is no doubt about that."[156]

The Volcker Board and FOMC, too, had been explicit from the beginning that its New Operating Procedures would see interest rates decline during recessions. For example, well ahead of the credit controls' imposition, Volcker had stated: "Interest rates can and will respond to credit demands, to economic conditions and, over time, to inflationary expectations without any change in the basic thrust of [our] monetary policy," while adding that in the event of a 1980 recession, "historical patterns would suggest some moderation in interest rates would naturally accompany [it]" (*Kansas City Star* (Missouri), January 3, 1980).[157] Volcker interpreted the superimposition of credit controls onto the 1980 recession in this light: *New York Times* (September 19, 1982, p. 72): "Interest rates, fell like a stone, too."[158] Indeed, right after the end of the credit-controls episode, he explicitly poured cold water on the notion that "the interest rate declines were in some manner 'contrived' or 'forced' by the Federal Reserve," and he rejected interpretations in which the slide in rates "reflects some weakening of our basic commitment to disciplined monetary policy and the priority of the fight on inflation."[159] He had earlier remarked that "interest rates have not in any sense been 'forced' lower—nor will they be at the expense of excessive growth in money and credit, at the risk of a resurgence in inflation and inflationary expectations."[160]

The second point regarding interest rates in the credit-controls episode is that credit controls, in effect, put a component into the price of credit not necessarily registered into market interest rates.[161] Friedman cast the credit controls as a tax on consumers' use of credit (see *Newsweek*, April 14, 1980, and the discussion above).[162] From this perspective, during the period of the controls program, market interest rates were only the *pre-tax* price of credit—the (unobserved) *post-tax* price, incorporating nonprice methods of restriction on credit supply, being much higher.

The third point is that interest rates relevant for spending likely did not fall as much as market rates. In his January 1980 comments in Florida, Friedman spoke of the past situation: the dearth

[156] From Tobin's testimony of July 29, 1980, testimony, in Treasury and Civil Service Committee, House of Commons (1981, p. 211).
[157] See also Nelson (2021) for earlier Volcker quotations to this effect.
[158] Similarly, M.K. Evans (1983, p. 76) observed of the interest-rate fall: "To a large extent[,] that decline was due to the recession and the increase in the saving rate…"
[159] From Volcker's testimony of July 9, 1980, in Committee on Banking, Housing and Urban Affairs (1980b, p. 8).
[160] *Washington Post*, May 16, 1980, p. B2. Also quoted in *New York Times*, May 21, 1980.
[161] As already indicated, retail-level interest rates did rise in the early credit-controls period. But it may be that, even in this period, the rise in recorded interest rates understated the increase in the cost of obtaining household credit.
[162] Volcker himself remarked of one of the restrictions on credit-card loan provision: "In effect, it was a 10 percent tax if they went above the previous peak." (In Kohn and others, 2019, p. 138.)

of monetary research after the Great Depression and the eventual inclusion of a staff model that allowed monetary policy to have effects on aggregate demand, "a large-scale econometric model in which monetary forces played at most a bit part, entering only via a narrow range of interest rates."[163] Key observed market rates, particularly the federal funds rate and Treasury bill rate—both of which essentially price low-risk loans to an actor (the government or a banking institution) not part of the nonbank private sector—do not always summarize all the variation in economically relevant (in particular, expenditure-sensitive) interest rates. In connection with this point, Bernanke (1988, p. 10) argued that the fact that key short-term market interest rates declined in the credit-controls period provided an example of monetary policy tightening even while observed interest rates fell. In keeping with this, and with Friedman's longtime arguments, the contraction of, or weakness in, monetary aggregates in 1980 also gave an accurate signal that monetary policy was tight. And this tightness showed up much more clearly in money and some other financial indicators than it did in standard market interest rates such as the federal funds rate and the Treasury bill rate.[164]

The preceding three points can be compressed into the proposition that the short-term natural rate of interest fell in the credit-controls rate—so that the value of the federal funds rate consistent with bringing inflation down was itself unusually low in real and nominal terms in this period. This interpretation is also consistent with Blanchard and Watson's (1986, pp. 131, 135) finding, on the basis of M1 behavior, that mid-1980 saw a major contractionary shock to monetary policy. In marked contrast, measures of policy shocks based on interest-rate reaction functions, such as Smets and Wouters (2007), have suggested that monetary policy was easy in mid-1980.[165] On the argument just given, these interest-rate shock estimates are unreliable as measures of monetary policy disturbances in 1980. The upshot is that monetary aggregates gave the correct signal of output decline and policy tightness during 1980. The monetary totals both contributed to and reflected the tightening of financial conditions seen during the episode.[166]

[163] Friedman (1980d, p. 81).

[164] One way of putting the matter is that monetary aggregates and the "interest rate" should give the same information provided that the "interest rate" is interpreted as a vector of yields and also incorporates movements not necessarily recorded in measured interest rates. Real money balances may move inversely with that broad concept of the interest rate either because the latter enters the money demand function or because (as suggested earlier) the expected-income variable that matters for money demand depends on the broadly defined interest rate.

[165] Romer and Romer (2004, p. 1064) also found expansionary monetary policy shocks in mid-1980 using the federal funds rate as the monetary policy variable, and (contrary to the interpretation offered here) they saw the FOMC as making a "decision to ease aggressively" in April 1980 (p. 1071). Their surrounding discussion (pp. 1071–1072), however, indicated that the interpretation of the 1980 observation was clouded by the credit controls and that the April 1980 policy move indeed exhibited some properties of a *contractionary* policy shock.

[166] Another standard that Goodfriend and R.G. King (2005, pp. 986–987) used in evaluating the tightness of monetary policy in 1980 was inflation. They apparently viewed high inflation in 1980 as reflecting monetary ease during 1980, and the decline in inflation that took place from late 1980 as establishing that sustained monetary

None of this is to deny that the Federal Reserve eventually eased policy decisively in 1980. Indeed, Friedman would argue (*Wall Street Journal*, June 28, 1982) that some of the decline in short-term interest rates that took place in May-June 1980 reflected the Federal Reserve's injection of reserves after its initial withdrawal of reserves. This policy shift contributed to the scale of the decline in interest rates in mid-1980. But the actual lower level of interest rates at which this decline occurred largely reflecting the downward effect of the credit controls, and associated slump in credit demand, on interest rates.

The monetary rebound and overall 1980 performance

Friedman's analysis did not support the notion that monetary policy was easy in the credit-controls period. But Friedman would contend that the Federal Reserve in the course of 1980 did ultimately move to excessive ease, in allowing a dramatic rebound in M1 over the course of the post-controls months.

During the late summer of 1980, in the wake of the second-quarter contraction in M1, Friedman suggested that M1 would almost certainly undershoot its target range for the year (*The News* (Frederick, Maryland), August 20, 1980). Around the same time, Paul Volcker expressed a similar expectation.[167] Yet the upsurge in monetary growth that started in May would be so great, and prolonged, that, as already indicated, M1 actually overshot its target for the year (see Table 1 above).

Even late in the year, it still looked possible that the FOMC would come just inside its M1 targer range for 1980 as a whole. One of the Board's governors, Nancy Teeters, acknowledged that the profile of M1 growth in the course of the year had been a little like "the Loch Ness monster" due to its gyrations, but suggested that the annual target would probably be met, and also declared: "I don't see how anyone could be more monetarist than we have been in the last thirteen or fourteen

restriction only began in earnest in late 1980. This judgment concerning 1980 policy would be echoed by Nakamura and Steinsson (2018, p. 72). These judgments were, however, inconsistent with the consensus that, in the United States, monetary policy actions typically do not have a major effect on the same-year inflation rate. Instead, in the United States, monetary policy is widely considered important mainly for *future years*' inflation rates. For further discussion, including an analysis of the role that Friedman's work played in forming this consensus, see Nelson (2020b, Chapter 15).

[167] See his recollection in Federal Open Market Committee (1981a, p. 25). Volcker may have been recalling his testimony of July 22, 1980, in which he did not predict an outright miss (that is, a full-year outcome outside the target range) but stated that "the FOMC is in fact prepared to contemplate [the possibility] that M1 measures [that is, old and new M1] may fall significantly short of the midpoint of their specified ranges for the year." (In Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1980b, p. 100.)

months." (*Chicago Tribune*, November 22, 1980.) By late September, however, the likelihood of an overshoot had already been emerging clearly enough that Karl Brunner (1980, p. 22) was noting that the "massive decline" in M1 had been replaced at the end of April with the setting-off of "massive expansion" in the aggregate. In suggesting that the M1 target would be hit, Teeters had been counting on a late-in-the-year monetary slowdown that did not materialize.[168]

Friedman would acknowledge that the Carter-initiated credit controls "really messed everything up" in 1980.[169] But he held the Federal Reserve accountable for the monetary upsurge seen in the final six to eight months of the year. The FOMC allowed "an excessively rapid growth in the money supply in connection with [the controls'] elimination," he suggested.[170] Simply increasing the monetary base "at a steady rate" over the year, he argued near the end of 1980, would not have forestalled fluctuations in M1 growth over the year but would have led to "far milder" and shorter deviations from the 1980 target (*Newsweek*, December 1, 1980).

The Federal Reserve did not carry out such a policy. Instead, as indicated above, it permitted total reserves growth to be rapid after April 1980 in a manner that ultimately encouraged an M1 overshoot. Gilbert and Trebing (1981, Chart 4, p. 12) graphically demonstrated that the rebound of M1 in the second half of 1980 was so great that the level of M1 ultimately overshot the level implied even by the outer bound of the FOMC's annual M1 growth target.

The U.S. economy itself would subsequently be judged to have troughed in July 1980, before entering an expansion that continued until mid-1981. The expansion was rapid: Volcker noted in November, "contrary to expectations, total industrial production has increased… at an annual rate of 16 percent since its low in July."[171] Friedman would trace output's July 1980 bottoming-out to the end-April trough in monetary growth (*Newsweek*, July 12, 1982).

Although relieved that the monetary contraction was over, Friedman deplored the overdone nature of the recovery of monetary growth. In a harsh indictment, Friedman remarked in late October 1980 with regard to the "most rapid rate of growth" in money in modern U.S. history: "That's hard to explain—except as a reaction to political forces. It's not only the Supreme Court

[168] Volcker blamed depository institutions' behavior ahead of scheduled changes in banking laws, including the legalization in early 1981 of nationwide NOW accounts, for some of the overshoot. He suggested that this factor added 0.5 to 0.75 percentage points to 1980's growth in (new) M1. See Volcker (1981, p. 16).
[169] The quotation is from H.R. Heller and others (1984, p. 44).
[170] Friedman (1985b, p. 58).
[171] From Volcker's testimony of November 19, 1980, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1981a, p. 54).

that follows the election returns and, indeed, in this case, precedes and tries to affect them." (*Merced Sun-Star* (California), October 30, 1980.)[172] His disaffection for Volcker had become so great that he was now willing to make characterizations of the current Federal Reserve chair of the kind he had once defended Arthur Burns against—when Burns and his FOMC were accused of having had electoral motives during the Nixon era.

This election-focused interpretation on Friedman's part of 1980 Federal Reserve behavior overlooked the fact that, in the fall of 1980, the central bank *did* take actions designed to curtail monetary growth. These included very public measures like the October rise in the discount rate that, as discussed in the previous section, generated President Carter's criticism. In 1983, Volcker would cite the discount-rate rise as among the pieces of evidence indicating that there was "no question" that the FOMC took restrictive moves in the July-October 1980 period, ahead of the election.[173]

This reality meant that the Federal Reserve was really more vulnerable to Friedman's longstanding criticism of flawed operating procedures than to his more spur-of-the-moment accusation of political motives. With regard to operating procedures, the Federal Reserve's discount-rate increase in September 1980, like the resumption of the discount rate surcharge the following November, epitomized the Federal Reserve's continuing reluctance to bear down on total reserves in the short run. Instead, the FOMC and Federal Reserve Board under Volcker maintained a fairly permissive attitude toward the behavior of total reserves, preferring not to use its nonborrowed-reserves control as a means of delivering more precise short-run control of aggregate reserve balances. Rather, the Federal Reserve used price-oriented control methods, in the form of adjustments to the discount rate, to influence the medium-term behavior of borrowed reserves and hence total reserves. This approach likely *did* contribute to much slower monetary growth in 1981, but did not prevent what Mussa (1994, p. 142)—in a detailed account whose judgment on the course of monetary policy in 1980 closely aligned with Friedman's—a "sharp upturn [in M1 growth]… during that summer and early autumn of 1980."

A corollary of the Federal Reserve's willingness to allow reserves to grow rapidly in the second half of 1980 was that the FOMC's associated tolerance range for the federal funds rate in the second half of 1980 was not as high as needed to keep M1 from overshooting its target for the

[172] A couple of days earlier, Friedman had similarly remarked of the rapid monetary growth: "It's hard to think of it in other than political terms." (*Pittsburgh Post-Gazette*, October 28, 1980.)

[173] See Volcker's testimony of July 14, 1983, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1983a, p. 22). In mid-year, the Federal Reserve Board had cut the discount rate: by 100 basis points on May 28 and by the same amount on June 12, bringing the rate to 11 percent (*Kansas City Time*s (Missouri), May 29, 1980, and June 13, 1980).

year. It was raised very substantially, and the actual federal funds rate likewise rose strongly (see Figure 3). Other rates showed the same pattern, with Baumol and Blinder (1982, p. 249) noting of the three-month Treasury bill rate and the prime rate, "rates skyrocketed again in the second half of 1980."

Volcker would describe FOMC policy in this period as one of "progressively moving against the increase in the money supply," with the result that "interest rates were rising" in the second half of the year.[174] The FOMC seems to have taken the view that its approach of permitting these interest-rate increases provided an adequate signal of a reining-in of monetary growth in future months—and that this approach was preferable to a more concerted attempt at short-run M1 control.

Friedman acknowledged that interest rates had risen greatly in the second half of 1980, and that this reflected efforts by the Federal Reserve to rein in an explosion in monetary growth.[175] Indeed, the monthly average of the federal funds rate, which reached a low for the year of 9.03 percent in July, averaged 18.9 percent in December. But Friedman suggested that the Federal Reserve had, in effect, taken actions that boosted the values of the short-term interest rate that were *consistent with* moderate monetary growth by kick-starting the economy (and so the demand for credit) so heavily via the midyear surge in monetary growth (*Newsweek*, December 29, 1980; *Wall Street Journal*, June 28, 1982).[176] The "stronger-than-anticipated recovery" in the economy had complicated matters, he granted, but the high speed of that upturn was "entirely the Fed's doing" (*Wall Street Journal*, January 30, 1981).[177]

Speaking in late November, Friedman declared that the past six months' monetary surge amounted to a "terrible performance" by the Federal Reserve (*Dallas Morning News*, November 22, 1980). A column on monetary variability that Friedman wrote shortly before this remark was

[174] Again, see Volcker's testimony of July 14, 1983, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1983a, p. 22).

[175] See *Wall Street Journal*, January 30, 1981 (as reprinted in Friedman, 1983a, pp. 245–246).

[176] Friedman did not claim—as some others did in analyzing this period (see, for example, M.K. Evans, 1983, pp. 66–67) that nominal interest rates' *initial* response to higher growth in the stock of money or in reserve balances consisted of an increase. Although he indicated that the downward pressure on interest rates associated with monetary expansion seemed to have become quite brief (*Newsweek*, December 29, 1980), he continued to believe in the empirical relevance of this liquidity effect (*Wall Street Journal*, January 30, 1981, as reprinted in Friedman,1983a, p. 245). He would conclude that, whereas in the past the liquidity effect had been evident in U.S. interest rates for about six months, it was the case that "from October 1979 to July 1982, … [it] appears to have lasted only two or three months, after which interest rates moved in the same direction as monetary growth." (Friedman, 1983e, p. 3.) Friedman's analysis of the factors driving interest rates in this period will be considered further in the next two chapters.

[177] Reprinted in Friedman (1983a, pp. 241, 242).

acidly titled “The Fed Fails—Again.” (*Newsweek*, December 1, 1980.)[178] Writing in December, Friedman indicated that he saw monetary instability as the prime culprit behind the economy’s fluctuations during 1980.[179]

A great deal of Friedman’s unhappiness with monetary policy during 1980 stemmed from the fact that he felt it threw away the opportunity for a program of gradual reduction in monetary growth, in which the output and unemployment costs of disinflation were limited to the early stages of the program. He did not see a gradualist policy as sidestepping the need for a recession. But in light of the fact that he had little time for classifying only periods of absolute declines in output as recessions, he emphasized that the period of economic weakness would be longer than the period of real GNP contraction. Friedman saw the period of “relatively high” unemployment being about one or two years, accompanied by “relatively slow” growth.”[180] But he thought that, under a gradualist program, the period of output decline could probably be contained to nine months to a year (*The Register* (Santa Ana, Orange County, California), December 23, 1979, p. E11). He also suggested that it could nevertheless be as long as eighteen months and would involve a total output decline of perhaps 2 percent (*San Jose Mercury News*, February 12, 1979).[181] Following this, there could ensue “a very healthy and vigorous expansion along with continued decline in inflation” (*The Register* (Santa Ana, Orange County, California), December 23, 1979, p. E11)—inflation presumably declining because the recovery was sufficiently restrained to keep a negative output gap in its early stages—and the elimination or near-elimination of inflation altogether five years after the program began (*U.S. News and World Report*, December 15, 1980, p. 51).[182] Instead, however, the first half of 1980 had seen an economic and monetary crash, followed by an overreaction in the form of a too-rapid rebound of monetary and economic growth.

[178] Ito (1999, p. 197) cited this column as the basis for his statement, “As early as December 1980, Friedman complained that the Federal Reserve had failed to keep its promise of October 6, 1979.” Although Ito was correct to date Friedman’s dissatisfaction with the post-1979 regime to 1980, he was incorrect to imply that origin dated to December 1980. Rather, as we have seen, Friedman was publicly criticizing the post-October 1979 monetary policy record as early as *June* 1980 and was expressing reservations even in March.

[179] *U.S. News and World Report*, December 15, 1980, p. 50 (reprinted in Friedman, 1983a, p. 193). Friedman also connected the economic fluctuations in monetary fluctuations in his retrospectives on the period: in *Wall Street Journal*, July 30, 1981, he linked personal income variations during 1980 to prior variations in M2, in *Newsweek*, July 25, 1983, he pointed to the instability in M1; and in Friedman (1983e, p. 9; 1984b, p. 29; 1985b, p. 55), he compared the economic fluctuations observed during 1980 with the prior behavior of both M1 and M2.

[180] *U.S. News and World Report*, December 15, 1980, p. 51 (reprinted in Friedman, 1983a, p. 195).

[181] After the 1979–1982 episode, Friedman likewise concluded that a steadier disinflation program—one with no credit-controls interregnum and a more stable downward path of monetary growth—would likely have involved an eighteen-month recession from January 1980 to July 1981 (Friedman, 1985b, p. 59), with a smaller peak in the unemployment rate than what actually occurred in 1982, as well as an extra year or so to attain the inflation rate that was reached in 1982.

[182] Reprinted in Friedman (1983a, p. 195).

The notion that 1980 had seen a wasted recession—in the sense that the disinflation induced by the recession had been undone by an excessive subsequent rebound—had been expressed in the financial press by commentator Sylvia Porter as early as September (*Daily News* (New York), September 21, 1980), and by Arthur Burns in a talk in late October 1980 (*Cleveland Press*, October 27, 1980). This interpretation echoed concerns that Friedman expressed, though it likely did not correspond entirely to his own position. And as a verdict on monetary policy in 1980, it should not be pressed too far. In particular, it does not appear appropriate to imply that as of the end of 1980 the Federal Reserve had dissipated all of the tightening since October 1979 and thus had lost a full year or more in its fight on inflation. On net, monetary policy *did* tighten when 1980 as a whole as compared with 1979. One can agree that the late-1980 monetary expansion, or 1980–1981 economic expansion, made the 1979–1982 disinflation more disorderly, without agreeing that that there was *no* restrictive pressure on aggregate demand, on net, produced by the Federal Reserve from October 1979 through December 1980.

Rather, monetary growth did show signs of having come down on average in 1980, as discussed above, even though the year's target was overshot. The tightness of average monetary policy in 1980 is also registered in the fact that nominal GDP growth continued to decline in that year. It seems appropriate to conclude that, over 1980 as a whole, the monetary restraint undertaken in late 1979 was not wholly reversed, but that it was not consolidated upon very much. That is, because of the relaxation of monetary policy after July 1980, disinflationary pressure was not increased as much as the Federal Reserve had planned for the year or as much as Friedman thought desirable.[183]

But, because 1980 witnessed, in total, only a limited firming of monetary policy, the subsequent further restriction in 1981—discussed below—came as a sudden tightening of the screws rather than as part of an ongoing progressive restriction of aggregate demand.

As for Friedman, he was thoroughly unhappy with U.S. monetary policy at the end of 1980: monetary growth still seemed to be surging, and, under current policy settings, the economy was

[183] Schreft (1990, p. 43) suggested that the credit controls had the effect of sharply lowering inflation for the period during which the controls were in effect. As already indicated, however, it is nonstandard to attribute movements in U.S. inflation to same-year movements in monetary policy (or to other same-period changes in aggregate-demand policy). And, as Friedman emphasized at the time, U.S. inflation was almost bound to fall off from its early 1980 peak as the oil price increase had been completed and the lagged impact of the mid-to-late 1970s monetary ease left the index. Furthermore, some of the decline in inflation in mid-1980 also reflected, not unusually short lags of the effect of demand restriction on inflation, but a mechanical decline in the CPI from the decline in the mortgage rate that occurred in the controls period. This factor contributed importantly to the occurrence of a near-zero increase in the CPI in July 1980 (see M.K. Evans, 1983, p. 76).

likely to move further away from the setting of aggregate demand needed to bring inflation down substantially.

FISCAL POLICY AND UNPLEASANT MONETARIST ARITHMETIC

As discussed in the previous chapter, the fiscal policy aspect of the Reagan economic program announced in 1981 envisioned a joint step-down in the ratios of government spending and taxes to national income over coming years—although the administration entered office with much more specifics regarding how to implement this plan on the tax-cutting side than on the government-spending restraint side.

In this environment, Paul Volcker was a major figure trying to encourage on Congress and the administration to make substantial progress in reducing government spending. He had been inclined against a broad-based tax cut—especially one whose passage preceded the enaction of commensurate spending restraint. Indeed, Volcker had privately cooperated in, and publicly supported, the opposite approach to fiscal policy that the Carter Administration had taken in 1980, when it aimed for a balanced budget, to be achieved in the early 1980s via a package that had no major tax cuts but that included slower increases in domestic federal spending than what was previously planned.[184] In March 1980, he stated: "I don't think it's responsible to go ahead with the tax cut right now, when we have no assurance of the expenditure cut."[185] But, when the 1980 recession emerged, the Carter Administration itself proposed a tax cut, as part of a package offered both in Congressional hearings and at the presidential election.[186] And, of course, Ronald Reagan was elected promising a large-scale tax cut.

By 1981, Volcker had acknowledged that the political momentum for a tax cut had become inevitable. "I think the tax cut is highly desirable," he testified a week after Reagan took office—although he would subsequently add the qualification that the kind of cut that he supported was one involving "prudent tax reduction."[187] The Federal Reserve chair particularly

[184] See Volcker's discussion in Kohn and others (2008, pp. 79, 134–135).

[185] From Volcker's testimony of March 24, 1980, in Committee on the Budget, U.S. Senate (1980, p. 272).

[186] Meltzer (2009b, p. 1065) implied incorrectly that President Carter quashed any suggestion that he include a tax cut in his 1980 economic packages. Meltzer was relying on a recollection of Charles Schultze that evidently was really in reference to an earlier timeframe (specifically, before the summer of 1980). Indeed, although Meltzer specifically suggested that Carter vetoed the idea of a new investment tax credit, such a new credit (as well as personal tax cuts) *was* included in Carter's September 1980 economic program (see Committee on the Budget, U.S. House of Representatives, 1980, pp. 16–19, 27–28).

[187] The quotations are from Volcker's appearances of January 27, 1981, in Committee on Appropriations. U.S. Senate (1981, p. 113), and of June 25, 1981, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1981d, p. 272). Presumably owing to this qualification, Volcker later remarked: "I do not think I

stressed that he would “like to see them [spending and tax reductions] locked in together... or see the spending cuts come first, …[as] my basic concern would simply be that the expenditures be brought into tandem [with lower taxes].”[188] Such a fully synchronized cut in taxes and spending was something to which Friedman, too, was agreeable. But it was not on the agenda in 1981, and in its absence, Volcker stated that “there is room for a responsible U.S. tax reduction” but called for the executive and legislative branches to make good on the “major change” in government spending path that was also part of the stated administration fiscal plan (*Washington Post*, June 6, 1981, p. D8).[189]

A key concern that Volcker therefore expressed was that “I don’t know how firmly in place” were plans for domestic federal spending cuts that were of the same order of magnitude as the tax revenue reductions.[190] Volcker pressed this point over the subsequent months, including after the tax cut was passed. Enaction of tax reductions “concern[s] me without the spending cuts,” he observed in a television interview, “which is why I put so much emphasis on the spending cuts.” (*The MacNeil/Lehrer Report*, PBS, August 20, 1981.) Volcker testified on September 16 that the “tax cut has been done. Now the job is to get the spending in line with what has been done on the taxes.”[191] So while Volcker, like Friedman, wanted spending and taxes simultaneously reduced, the Federal Reserve chair was wary about taking a step that Friedman welcomed: proceeding with the tax cuts in the absence of definite decisions regarding spending cuts.

This difference in position led to a major divergence between Friedman and Volcker after 1981, once it had become clear that spending restraint on a scale needed to balance the budget would not be forthcoming in the near future. Whereas Friedman favored proceeding, over time, with the phase-in of the tax cuts and allowing the resulting revenue shortfall to exert pressure on Congress and the administration to cut spending, Volcker from 1982 onward became an advocate not only of spending reductions but also of tax increases to achieve a balanced budget.

would characterize my attitude [to the 1981 tax bill] as having been supportive.” (Testimony of February 23, 1982, in Committee on Ways and Means, U.S. House of Representatives, 1982c, p. 374.) He would also stress that the negotiating process regarding the tax cut had made it notably larger (see Volcker’s testimony of July 14, 1983, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1983a, p. 42).

[188] From Volcker’s testimony of March 3, 1981, in Committee on Ways and Means, U.S. House of Representatives (1981, pp. 380, 381).

[189] In March, the Federal Reserve Board’s *Federal Reserve Bulletin* had used unusually strong language on its contents page when it summarized earlier testimony Volcker had given to this effect as: “the linchpin of the whole [Reagan] economic program is massive progress in cutting back the upward surge in federal expenditures.” (Federal Reserve Board, 1981b, p. 193, summarizing Volcker, 1981b).

[190] From Volcker’s testimony of June 25, 1981, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1981d, p. 272).

[191] Testimony of September 16, 1981, in Committee on the Budget, U.S. Senate (1982a, p. 86).

In making the case for tax increases, Volcker would frequently cite the high interest rates prevailing in the United States and point to upward pressure on those rates arising from federal budget deficits.[192] One source of upward pressure, and the one that Volcker mainly cited in his warnings during the Reagan years, was the crowding-out effect. This effect—one believed relevant for real interest rates—will be discussed in later chapters, when Friedman's revised view of the deficit/real-interest-rate relationship will be discussed. The second alleged source of upward pressure on interest rates coming from budget deficits pertained to the inflation-expectations component of nominal yields. Volcker referred on occasion to the possibility that high interest rates in the 1980s reflected market judgments that U.S. budget deficits would compromise the ability of the Federal Reserve to execute a disinflationary monetary policy, by putting pressure on the central bank to monetize the deficits.[193] This link between U.S. monetary and fiscal policy will be the focus of the discussion that follows.

Volcker, therefore, wanted a fiscal consolidation in part because it would help reduce concerns that a large-scale monetization of budget deficits would eventually occur. He would indicate nevertheless that his own approach to monetary policy would *not* involve accommodation. He rejected the notion that budget deficits precluded the Federal Reserve from meeting its monetary targets.[194] And he indicated that nor would the Federal Reserve would not be making a policy choice to accommodate the deficits as a policy choice.[195] "I can assure you that the Federal Reserve has no intention to simply accommodate expanded federal borrowing," he wrote to a member of the public in February 1981.[196] In particular, with regard to monetary policy in future years, Volcker noted that his intention was that the monetary-growth targets would be set with a price-stability goal in mind and that this would be so "almost irrespective of fiscal policy" (*Washington Post*, June 6, 1981, p. D8).

[192] Among many examples, see Volcker's testimony of February 5, 1981, testimony in Joint Economic Committee, U.S. Congress (1981a, pp. 10, 21) and of March 3, 1981, in Committee on Ways and Means, U.S. House of Representatives (1981, p. 357).

[193] He had referred to these links ahead of the Reagan years, too. For example, in testimony given on September 5, 1979 (in Committee on the Budget, U.S. House of Representatives, 1979b, p. 299), Volcker stated: "action to increase the deficit complicates our problems in restraining growth in the money supply... It will, at the very least, produce more pressures on interest rates as we attempt to restrain the money supply."

[194] See Volcker's testimony of February 5, 1981, in Joint Economic Committee, U.S. Congress (1981a, p. 19).

[195] See Volcker's testimony of March 3, 1981, in Committee on Ways and Means, U.S. House of Representatives (1981, p. 368).

[196] Letter of February 12, 1981, to J. Gassaway. Similarly, in a letter to G.H. Treinces of July 27, 1983, Volcker wrote (p. 2): "I assure you, however, that the Federal Reserve does not intend to abandon its anti-inflationary stance in the face of large deficits: that is, we will not simply monetize the debt by permitting our open market purchases to absorb whatever volume of government debt is coming to market." Scans of both letters are available at *https://fraser.stlouisfed.org/archival-collection/paul-a-volcker-papers-5297*.

Friedman, as will be seen, strongly concurred that disinflationary monetary policy could indeed proceed in the face of major budget deficits. But he dissented from Volcker on the urgency of reducing fiscal imbalances. In particular, Friedman believed that deficit reduction was undesirable (and ephemeral) if effected by tax increases.

On this matter, starting very early in the Reagan years, Friedman would also find himself at odds with two economists who shared his monetary view of inflation and who had been allies of his in many of the economic-research debates of the 1970s: Robert Lucas and Thomas Sargent. In prominent writings that appeared starting in 1981, both Lucas and Sargent indicated that they believed that the behavior of U.S. budget deficits had a strong bearing on the Federal Reserve's scope to deliver price stability.

"*Milton is putting too much emphasis on money*"

A few weeks after the Reagan tax cuts were passed as the Economic Recovery Tax Act of 1981, Lucas wrote two op-ed articles for the *New York Times* (August 26 and 28, 1981) criticizing the tax cuts.

Lucas actually had considerable common ground with Friedman on the positive economics concerning taxes and fiscal policy.[197] As discussed in the previous chapter, he welcomed the incentive aspect of tax cuts. And, like Friedman by the 1980s, he did not emphasize a crowding-out danger associated with budget deficits. But another assessment that she shared with Friedman—that the tax cuts as likely to reduce tax revenue, on net, for years into the future—underlay Lucas' opposition to them.

Lucas viewed the enactment of tax cuts as inconsistent with U.S. fiscal principles dating back to Alexander Hamilton, according to which only emergencies like wars and depressions justified sustained large deficits. The crux of the matter was that Lucas saw the approaching budget deficits as likely to prompt inflationary monetary growth. "Though Milton Friedman's aphorism that 'inflation is always and everywhere a monetary phenomenon' is true (or as true as economic aphorisms get)," Lucas wrote, "it is equally true that sustained monetary expansions are always and everywhere a consequence of printing money to cover the difference between government

[197] See the previous chapter.

expenditures and revenues." (*New York Times*, August 26, 1981.)[198]

A few weeks later, Friedman gave an analysis of Reaganomics in the September 21 issue of *Newsweek* and offered a very different assessment from that Lucas had given. Friedman was writing at a time when there was still considerable hope that the Reagan Administration would secure large-scale spending reductions. Still, he conceded that many were asking the question: "Don't tax cuts plus increased military spending mean even bigger future deficits?" Friedman maintained that the prospect of future budget deficits should be "rejected out of hand" as an explanation of high interest rates, because such pressure should produce an upward-sloping yield curve instead of the inverted yield curve currently being observed (*Newsweek*, September 21, 1981). In contrast, Lucas saw the prospect of large deficits under Reagan, and the related likelihood of high future monetary growth, as being responsible for high longer-term interest rates—and maintained that, in his analysis of future U.S. economic developments, Friedman was paying attention to recent patterns of monetary growth and neglecting the repercussions of budgetary imbalance for future monetary policy choices. "Milton is putting too much emphasis on money and not enough on the deficit," Lucas remarked (*Business Week*, September 14, 1981, p. 29). Recognizing that the federal spending reductions required to balance the budget were not going to be legislated, Lucas would further observe: "There is no way they can cut the budget enough. We've got to reconsider the tax cuts." (*Business Week*, October 5, 1981, p. 116.)

The contrast between Friedman and Lucas' views was underlined when a thrift-industry newsletter presented a symposium of views on why interest rates were so high. Lucas' and Friedman's diagnoses of the interest-rate situation in adjacent columns in the article. Lucas suggested that fiscal policy needed to tighten for interest rates to come down. Friedman, in contrast, said that nothing had to happen: nobody really understood why rates were so high but they should come down by themselves, as their current levels were, he suggested, currently out of kilter when judged by their historical connections to monetary growth and inflation (*Savings & Loan News*, October 1981, p. 68).

How do Friedman's and Lucas's retrospective positions stand up in retrospect? As discussed in detail later in this chapter, Friedman's analysis of monetary policy during 1981 was flawed by his failure to recognize until very late in the year that monetary policy was tight. Once monetary policy is appropriately acknowledged as having been tight in this period, high real and nominal

[198] Lucas had foreshadowed his coming opposition to the 1981 tax cuts by stating in *Business Week* (August 11, 1980): "The most important question is what impact a tax cut or talk of a tax cut would have on inflationary expectations."

short-term interest rates during 1981 do not seem hard to explain in monetary terms.[199]

Friedman appears to have been on firmer ground in suggesting during 1981 that longer-term interest rates were higher than what was justified by fundamentals: U.S. longer-term interest rates during this and surrounding years seem to have exhibited abnormal volatility, being, in particular, overreactive to movements in short-term interest rates. Furthermore, participants in U.S. Treasury securities markets in the first half of the 1980s seem not to have processed particularly quickly the information available in current monetary policy developments about future inflation and did not adequately anticipate, and appreciate the permanence of, the disinflation of the 1980s (Kozicki and Tinsley, 2005). In principle, the fact that longer-term rates lagged behind the disinflation could have reflected a market assessment, particularly from the second half of 1981 onward, that fiscal developments would eventually lead to a revival of inflation.[200] But studies specifically concerned with the relationship between fiscal policy and market yields have found that budget deficits and debt did not seem to account for a great deal of the behavior of U.S. longer-term interest rates in the 1980s.[201]

Unpleasant monetarist arithmetic

That the advent of very large budget deficits under the Reagan Administration invited inflation was also seemingly supported by the formal analysis provided by two other members of the rational expectations school, Thomas Sargent and Neil Wallace, in what would become one of the most famous articles in monetary economics—and certainly the most well-known paper to have "monetarist" or "monetarism" in the title. "Some Unpleasant Monetarist Arithmetic" appeared in the Fall 1981 issue of the *Federal Reserve Bank of Minneapolis Quarterly Review*.[202] Sargent and Wallace's analysis of the government budget constraint stressed the limits on the scope for monetary base growth to be set independently of budget deficits. They showed that, under specific assumptions about the values of real interest rates and real economic growth, the central bank might well be obliged to monetize budget deficits.

The title of the paper captured the mixture of messages toward Friedman. The analysis was

[199] Evidence suggesting low real and nominal monetary growth helped account for high short-term real interest rates in this period would be provided by Richard Clarida and Benjamin Friedman (1984), among others. See also the next chapter.
[200] This was conjectured by Sargent (1985, p. 249) who, in comments given at a conference in June 1983, remarked that "the high [U.S.] long-term nominal interest rates that prevailed in 1981 and 1982 can be interpreted as reflecting the market's guess that large deficits would persist and eventually be monetized in large part."
[201] For further discussion, see Chapters 14 and 15.
[202] Sargent and Wallace had part-time advisory affiliations with Minneapolis' reserve bank.

"monetarist" in the sense that the authors used a framework in which the path of the quantity of money (specifically, the monetary base) governed price-level behavior. In this analytical framework, in which the consolidated intertemporal government budget constraint played a central role, Sargent and Wallace reached the "unpleasant" conclusion that fiscal deficits cast a shadow over monetary policy—and could govern the behavior of monetary growth. In scenarios that the authors emphasized, attempts by the central bank to insulate the money stock from Treasury borrowing would, over long periods, be fruitless, and so the course of public debt would drive money and the price level. This was arguably unpleasant news not only for policymakers but also for Friedman himself, in light of his traditional downplaying of the role of fiscal policy, including budget deficits, in the theoretical analysis of inflation. The analysis did not contradict his statement that "monetary restraint is a sufficient condition for controlling inflation" (*The Times* (London), March 3, 1980), but it suggested that particular settings of fiscal policy could preclude longer-term monetary restraint. Along these lines, Dornbusch and Fischer (1987, p. 649) suggested that the Sargent-Wallace arithmetic is "unpleasant for monetarists because it suggests that budget deficits may have more to do with the eventual inflation rate than [does] the current growth rate of money."[203] The paper therefore amounted to a monetarist analysis that some monetarists might find unpalatable.

Neil Wallace remarked that "the title is, you know, sort of meant to be vague. You're sitting around, you know, [thinking:] 'What's "unpleasant"? Is it monetarism? Is it the arithmetic?' (*laughter*) So it's sort of meant to be intentionally ambiguous." (Neil Wallace, interview, March 15, 2013.)

In light of their emphasis on the link between money and public debt, the authors concluded that Friedman's 1948 policy rule, which interlocked monetary and fiscal policy, might well be a more appropriate way in which to conduct economic policy than the constant monetary growth rule that Friedman had, over the previous quarter-century, strongly pushed (Sargent and Wallace, 1981, p. 9).

Friedman later conjectured that the Reagan deficits were "doubtless the inspiration" for the

[203] As the Dornbusch-Fischer discussion indicated, the Sargent-Wallace (1981) analysis was quite rapidly introduced into advanced undergraduate textbook presentations of macroeconomic matters. This partly reflected the fact that, although the paper contained considerable technical material, the central messages of the Sargent-Wallace analysis were reader-friendly, mainly requiring reference to the public sector's intertemporal constraint: "it doesn't take much of a fancy model to see those things," Wallace observed (interview, March 15, 2013). Indeed, Sargent recalled that Wallace worried that their findings were too obvious: "he didn't think that was a paper." (Thomas Sargent, interview, January 24, 2014.)

Sargent-Wallace paper.[204] Along these lines, Wallace recalled that, in finalizing the paper, the authors "did some editing of the introduction, which really changed the tone of the thing… [and] it was sort of meant to be a story about current events, and how tight monetary policy, and selling of debt at high interest rates—something about what was going on in the early years of the Reagan Administration." (Neil Wallace, interview, March 15, 2013.) Indeed, Sargent, like Lucas, was outspokenly critical of the tight-money/fiscal-ease combination that emerged once the large budget deficits of the Reagan era emerged. He suggested that it made U.S. anti-inflationary policy "not credible" and, like Lucas, he saw it as poised to lead to high inflation over the long term.[205]

Friedman's views on the connection between deficits and monetary growth

Friedman's specific response to the "monetarist arithmetic" analysis will be discussed presently. Ahead of that, it is worth asking: Why did Friedman's position differ on the implications of deficits even from those who agreed with him on the monetary-growth/inflation link?

Friedman shared Lucas' view that really large inflations had, empirically, reflected monetization of budget deficits. Deficits had, Friedman acknowledged, been a major influence on U.S. monetary growth during wartime periods (*Newsweek,* February 23, 1981). Indeed, some of his earliest empirical studies in the early 1950s in his monetarist work had focused on wartime episodes for this reason.[206] But Friedman had, also starting in the early 1950s, amassed a stream of statements that highlighted the feasibility of separating monetary policy and fiscal policy (see Nelson, 2020a, pp. 319–320). He had taken the view that, for deficits of the size observed in the post-World War II period in the United States, the monetary authorities had sufficient power to offset the influence of these deficits on monetary growth and so on inflation.[207]

During the 1960s and 1970s, Friedman had indeed quite often accepted that the deficit was, *in practice*, a source of monetary growth in the United States.[208] But he emphasized that this

[204] Friedman (1987e, p. 218).

[205] The quotation is from Sargent (1985, p. 249). See also his remarks in Sargent (1982), in *New York Times*, August 10 and 12, 1983, and in Klamer (1984, pp. 71–72).

[206] See, in particular, Friedman (1952a).

[207] See, for example, Friedman's remarks of January 31, 1952, in Joint Committee on the Economic Report, U.S. Congress (1952a, p. 334). See also Nelson (2020b, pp. 245–246) and Chapter 2 above.

[208] See, for example, *Newsweek*, April 24, 1978. In *The Sun-Telegram* (San Bernardino, California), September 26, 1977), Friedman started out by saying, "Federal deficits are likely to be financed by printing money," but he went on to give financing by bond issuance without accompanying inflation as another plausible scenario. One of the final major occasions on which Friedman stressed an empirical deficit/monetary-growth link in the United States was in a passage in his March 1980 memorandum for the Reagan campaign, describing it as a "major factor fostering

situation was avoidable and did not even think that, as a practical matter, U.S. double-digit inflation in the mid-1970s had much to do with accommodation of budget deficits.[209]

The fact that Friedman did, nevertheless, make statements suggesting that the Federal Reserve had in practice accommodated deficits may have had a role in shaping Ronald Reagan's pre-1981 statements in the area. The future president frequently referred to the U.S. budget deficit as a major source of inflation. Several examples were provided in Chapter 5 above. Another example was his statement in a talk-show appearance: "I think the answer to curing inflation is a balanced budget" (*The Tonight Show Starring Johnny Carson*, NBC, January 3, 1975). In the months ahead of the 1980 presidential election, Reagan affirmed that he saw this process as operating via the response of monetary growth to deficits: "Deficits tempt [the government] to print new dollars, instead of paying back its debt with honest money." (*The Sun* (Baltimore), May 1, 1980, p. A8.) Of course, once he was president, Reagan became much more comfortable with large budget deficits and more inclined to see deficit containment and inflation control as largely distinct matters.

In contrast to Reagan's characterization of the matter, Friedman had, even in the 1970s, not subscribed to a belief in an inevitable link between inflation and deficits. He rejected the notion that accommodation of deficits could account for key episodes of high U.S. monetary growth, and like other monetarists, he emphasized that there was no necessity for money growth to be associated with public borrowing.[210] Friedman reaffirmed in early 1981: "On the technical level… there is no necessary connection between deficits and monetary growth... [and] there is

excessive monetary growth" in recent years—perhaps referring here to the 1976–1977 monetary explosion (Friedman, 1980a, p. 7). He dropped this passage in his March 1981 revision of the memorandum prepared for the new administration (Friedman, 1981c). In *Newsweek*, February 23, 1981, he seemed to see upward pressure on monetary growth as stemming from public spending regardless of whether that spending was deficit spending, although he also implied that it was technically feasible to resist this pressure.

[209] In a recent op-ed, Thomas Sargent and William Silber (wsj.com, March 3, 2022) suggest the opposite by pointing to Friedman's remark: "Financing government spending by increasing the quantity of money is often the most politically attractive method to both presidents and members of Congress." (*Newsweek*, January 9, 1978.) But because Friedman emphasized bracket creep—a link between high monetary growth and financing of the budget via ordinary taxes—as a U.S. fiscal-policy windfall that routinely arose from inflation, the indicated statement did not, in fact, refer solely or even mainly to deficit monetization. In a similar vein, Friedman's identification in *Newsweek*, February 23, 1981, of government spending as a "chief culprit" behind inflation seemed to reflect mainly his attachment to the idea that inflation provided a means of raising tax revenues. This interpretation is consistent with his later observation (Friedman, 1985e, p. 16) that, in the United States "in recent decades," bracket creep had been "very likely far more important" than deficit monetization.

[210] In addition to what follows, see McCallum and Nelson (2005) on the views of Friedman as well as other monetarists such as Schwartz, Brunner, and Meltzer. Also notable is the statement of Jerry Jordan of June 9, 1976 (in Committee on Banking, Currency and Housing, U.S. House of Representatives, 1976b, p. 95): "If you don't finance deficits by monetary growth, you will not produce the kind of inflation we have had. If you have rapid monetary growth, even if you have balanced budgets or surpluses in the budget, you are going to have inflation."

no technical reason why the Fed must monetize deficits." (*Wall Street Journal*, January 30, 1981).[211]
Monetary growth had, he held, in practice reflected many factors other than deficits. This was true, Friedman suggested, both in the case of the United States and in the case of other advanced nations (*Newsweek*, February 23, 1981).[212] The loose relationship across these countries between monetary growth and deficits translated into a loose historical connection between inflation and budget deficits: Friedman accordingly observed in 1969 that there existed "many examples of government deficits without inflation."[213] And, in 1981, he indicated that he viewed the U.S. experience since 1969 as providing examples of the converse also being true—with 1974 and 1979 being given as examples in which high inflation rates did not coincide with large deficits (*Newsweek*, February 23, 1981).[214] Consequently, as of the early 1980s Friedman was less, not more, inclined than in earlier decades to see monetary growth and budget deficits as intertwined for the United States.

His own frequent references during the late 1970s and early 1980s to government spending control as facilitating monetary restraint did not actually amount to an acceptance that deficit reduction should accompany reductions in monetary growth. "I do not believe that balancing the budget is the key problem," Friedman remarked in March 1980.[215] Indeed, the following June, he criticized the Thatcher Government for having put reductions in the share of the U.K. budget deficit (the public sector borrowing requirement, or PSBR) to GDP alongside reductions in monetary growth in the multi-year public anti-inflation strategy.[216]

Friedman's position was actually that much lower public spending would facilitate lower monetary growth even if the lower spending level was accompanied by larger deficits

[211] Reprinted in Friedman (1983a, p. 243). James Tobin had a similar view. He testified to a U.K. parliamentary committee on July 29, 1980: "It is entirely possible for a government, including this government, to choose what happens to money supplies under its control, independently of what happens to the public sector borrowing requirement." (In Treasury and Civil Service Committee, House of Commons, 1981, p. 209.)
[212] Friedman pointed out that Japan had been running a budget deficit of 6 percent of GNP yet had a good record in "achieving monetary growth targets and holding down inflation" (*Wall Street Journal*, January 30, 1981, reprinted in Friedman, 1983a, p. 243). Japan's experience underlined the separability of deficits and monetary growth, while also demonstrating the proposition that a deficit gave rise to inflation "only if it is financed by printing money" (*The Daily Oklahoman* (Oklahoma City), May 19, 1981).
[213] Friedman (1969c, p. 107).
[214] Of course, any conclusions drawn from static comparisons like these were clouded by the fact that. if inflation in period *t* was traceable primarily to monetary growth in period *t*–*k*, the deficit that might be the most relevant driver of monetary growth would presumably be that in period *t*–*k*, not that in period *t*.
[215] In Friedman and Samuelson (1980, p. 29).
[216] See Friedman (1980b, pp. 56–57; p. 52 of 1991 reprint), in which his objections included (1980b, p. 56; p. 52 of 1991 reprint): "There is no necessary relation between the size of the PSBR and monetary growth."

(*Newsweek*, March 24, 1980, and February 23, 1981). Theoretical support for this position would be provided, to some extent, by an article that appeared around this time. Barro (1981), building on his earlier work, showed that reductions in government spending, if temporary, lowered the interest rates the private sector faced, while permanent reductions in spending permanently lowered households' lifetime tax burden, even when not accompanied in the short term by lower taxes.

Responding to Sargent and Wallace

When it was suggested to him in July 1982 that "Milton Friedman wouldn't like very much the conclusions you draw with Neil Wallace," Sargent would not be drawn on the matter, simply replying: "Ask him."[217] Friedman would eventually go on the record on the matter. He would welcome the Sargent-Wallace analytical framework but reject the conclusion they took from their "unpleasant monetarist arithmetic."

Friedman would encounter the Sargent-Wallace argument when Sargent presented it at a couple of conferences that Friedman attended in the early 1980s.[218] By mid-1986 he indicated he knew the paper, although he seemed not to be very familiar with the debate that the paper had generated.[219] He soon confronted the paper directly in print when he reviewed a collection of Sargent's papers that included Sargent and Wallace (1981).[220] Sargent remembered that "from my point of view, he wrote a very sympathetic review" (Thomas Sargent, interview, March 26, 2014). Friedman indeed declared Sargent's (1986) collection *Rational Expectations and Inflation* to be a "splendid book."[221] But he did take the opportunity of the review to indicate that he was not himself convinced by the Sargent-Wallace argument.[222]

Friedman accepted the logic of the Sargent-Wallace argument: it, after all, used the accounting identity of the government budget constraint as its starting point, appended with hypotheses about how different agencies of government interacted. Indeed, Sargent's perspective was that, via Friedman's 1948 and subsequent work, "he's the one who taught us that arithmetic, you know? All this stuff about: Who's the dominant player—the fiscal authority or the monetary

[217] See Sargent's exchange with Arjo Klamer, in Klamer (1984, p. 73).
[218] Specifically, the March 1982 Federal Reserve Bank of Atlanta conference on supply-side economics and the June 22–24, 1983 monetary policy conference at the Bank of Japan. See Sargent (1982, 1985).
[219] See his July 1986 observation in Darby and others (1987, p. 29).
[220] Namely, Sargent (1986), reviewed by Friedman (1987e).
[221] Friedman (1987e, p. 218).
[222] As well as covering the reprint of Sargent and Wallace (1981) in his book review, Friedman (1987e) considered the presentation of the Sargent-Wallace argument in the book's reprint of Sargent (1985).

authority?" (Thomas Sargent, interview, March 26, 2014.) In the early 1980s, Friedman himself was still someone inclined to remind audiences with regard to federal spending, "one way or another we're going to have to finance that spending." (*Austin American-Statesman* (Texas), May 24, 1981.)

Despite this common ground, Friedman rejected Sargent and Wallace's application to the situation of the United States in the 1980s—and, in particular, the implications that they drew for monetary growth and so for inflation.

On Friedman's interpretation, their arithmetic was not "unpleasant" for monetarism. An indefinitely rising government debt-to-income ratio, Friedman accepted, would hinder the government's ability to market debt and ultimately trigger monetization of the debt.[223] But he judged that this scenario was not relevant to the United States in the 1980s. Friedman refrained from relying on the "starve-the-beast" argument as the basis for rejecting the Sargent-Wallace position—though his review did note that he saw improved control of government spending as being likely to be a result of major budget deficits.[224] To rely on this argument would, however, not answer the case Sargent and Wallace considered in which future years' "primary" (that is, net of interest payments) budget deficits were taken as given.

Instead, key to Friedman's case against Sargent and Wallace (1981) was their reliance on the assumption that the real interest rate exceeded the real output growth rate. This condition had been met in the United States in 1981, but Friedman argued that real interest rates had been unusually high in that year and the immediately following years. Rates had come down notably by the time Friedman wrote his review in 1986, and he predicted that they would fall further in relation to the economic-growth rate. Friedman stressed that such a decline would relieve the strains associated with U.S. government debt and deficits.[225]

The historical U.S. empirical reality, Friedman stressed, was one in which the real interest rate

[223] See Friedman (1987e, p. 220). He also accepted it as a description of extreme situations, observing that "a large enough deficit continued for a long enough time would result in the destruction of our monetary system." (Quoted in *Newsday* (Long Island, New York), November 22, 1985.) In addition, Friedman (1987d, p. 17) noted that "[e]ssentially all major inflations, including hyperinflations" had been associated with very large-scale use of money financing in place of explicit taxes to finance public spending. In the 1990s, he also observed that there were "structural deficits over a long period are a great danger and should be avoided" (*Wall Street Week*, Maryland Public Television, February 21, 1992, p. 6 of transcript) and stated (Friedman, 1993c, p. 16): "Government debt is a problem in the long run. Obviously, if the government continues to run deficits for a long period of time, it will sooner or later have to monetize them."

[224] Friedman (1987e, pp. 220–221).

[225] Friedman (1987e, p. 221).

was on average below the real growth rate. This had not been the situation prevailing in 1981 but it had since been reestablished. With the real interest rate below the real output growth rate, budget deficits and noninflationary monetary growth could be an ongoing combination, with no collision occurring. Dornbusch and Fischer (1987, p. 650) described the case in which the real interest rate lay below the real growth rate as one implying that "debt finance is viable for the long term, and the hard choice posed by the Sargent-Wallace example can be avoided." Friedman characterized this case in similar terms: "the vicious cycle becomes a virtuous cycle: under the assumed monetary and fiscal policy, the ratio of debt to income declines even though all interest payments are financed by borrowing."[226] In these conditions, quite sizable budget deficits could go on indefinitely and coexist with ongoing monetary and price stability. This setting of the real interest rate in relation to the real growth rate has also been underlined by Blanchard (2019) as the empirically realistic one in the case of the United States.

Thus, for the analysis of inflation for periods other than extreme cases, Friedman's position in the wake of the Sargent-Wallace work was the same as he described it in April 1981: "No, I don't think monetary policy has to be backed up by fiscal policy at all. I think monetary policy can curb inflation… [S]o you can separate monetary policy from fiscal policy." (*New Zealand Herald* (Auckland, New Zealand), April 18, 1981.) And so, for the United States, he regarded the message of Sargent-Wallace as of doubtful inapplicability over long stretches of years.

The different views in retrospect

The scenario that Lucas and Sargent feared of deficit-driven U.S . inflation indeed did not occur. In retrospect, Lucas would say that he had underestimated the extent to which U.S. budget deficits would be compensated by increases in other taxes during the 1980s and early 1990s instead of by the inflation tax (that is, by monetization).[227] As for Sargent, he noted that he was concerned in the early 1980s that high inflation was in the future and stressed that the double-digit longer-term interest rates of the early 1980s were testament to the fact that he was not alone: "A lot of people thought there was going to be a lot of inflation at the time." He noted of his and Wallace's work that "what we were worried about was that, if they [deficits] continued in a sustained manner, inflation could emerge," and that "the intriguing thing is that it's not just one

[226] Friedman (1987e, p. 221). See also his remark in Darby and others (1987, p. 29). In late 1981, Friedman indicated that he believed the appropriate value of the real interest rate in the long term was about 3 percent (*Newsweek*, October 19, 1981), roughly two or three percentage points below the very high values prevailing in 1981.

[227] See his remarks in Snowdon, Vane, and Wynarczyk (1994, pp. 224–225).

year's deficit or two years' deficit—it's a sustained thing. That was the message. So you can't disprove it by saying, 'Well, we had a big deficit for a couple of years and nothing happened.' Because you've got lots of room—lots of room—to run up the government debt." (Thomas Sargent, interview, March 26, 2014.)

Sargent and William Silber have recently stressed that there was a concerted effort to reduce U.S. budget deficits after the mid-1980s. They suggested that this development forestalled the monetization, and high-inflation, outcome that many feared would eventually come from the Reagan deficits, and in so doing allowed the Volcker disinflation of the early 1980s to be permanent (wsj.com, March 3, 2022).

This account of why monetary policy was able to be noninflationary after 1979 is one in which fiscal retrenchment created conditions for monetary policy to generate price stability.[228] Such an account of the ending of inflation likely neglects the extent to which improvements after the late 1970s in official U.S. economic doctrine and the Federal Reserve policy reaction function gave rise to a monetary policy strategy that, across a wide range of budget deficits, implied less inflationary outcome for the economy than before. This alternative account would be more in line with Friedman's portrayal of what monetary policy was capable of doing on its own.

As already indicated, for purposes of economic analysis Friedman did not see the Sargent-Wallace scenario of fiscal policy overwhelming monetary policy as relevant, once empirically realistic values of the real interest rate and the real growth rate were used. Furthermore, as a matter of practical policy, Friedman rejected the notion that the Reagan regime had amounted to a new ascendancy of fiscal policy over the course of monetary policy—in part because he did not see Reagan's policy mix as implying an indefinite rise in government debt to income.[229]

In contrast to Lucas, Sargent, and Wallace, who viewed a long-lasting rise in the ratio as in prospect and as leading to a loosening of monetary policy, Friedman saw only a brief increase as likely to occur, and he believed that the rise that occurred would trigger not more rapid growth in money but slower growth in government spending. That is to say, as already indicated, he believed starve-the-beast forces would operate. In 1978, Friedman indicated that he was willing to see a $100 billion deficit in the short run if it acted as a restraint on current and future

[228] Neil Wallace (interview, March 15, 2013) also underlined the converse point: "I don't know—have we ever seen an inflation where the country's sort of 'fiscal house' was in order? I don't think you could find one."
[229] See Friedman (1987e, p. 221).

government spending.[230] Under Reagan, deficits of this size began to materialize: the deficit was $79 billion in fiscal-year 1981 (which ended before most of the Reagan tax cuts started to be phased in) and then $128 billion in fiscal-year 1982 (Council of Economic Advisers, 2011, Table B−78, p. 283). In the face of such deficits, the fiscal authorities would, Friedman believed, ultimately make a correction to their own behavior via spending retrenchment. Sargent acknowledged this as a possibility (Sargent, 1982, p. 113), while regarding an eventual adjustment of monetary growth to the deficits as being more likely.[231] Holding the opposite assessment, Friedman did not consider a large expansion of the budget deficit as inconsistent with achieving disinflation through monetary policy.

As it turned out, at the time when Friedman's book review appeared—February 1987—Volcker was only six months away from the end of his tenure at the Federal Reserve. As chairman, Volcker had accepted the responsibility for inflation to a far greater extent than his predecessors had. Nevertheless, Volcker's frequent calls for reductions in the budget deficit—including, after 1981, via tax increases—likely figured in Friedman not excluding Volcker when, in 1984, he complained of successive "chairmen of the Fed blaming all the nation's ills on fiscal policy."[232] By this time, Friedman had, as discussed in later chapters, substantially disassociated himself from a proposition he had once pushed and of which Volcker was a strong advocate: that budget deficits, for given monetary policy, significantly raised real interest rates.

III. PERSONALITIES IN DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1980–1981

HENRY WALLICH

In December 1980, Friedman was one of several Nobel economists making contributions to a *U.S. News and World Report* symposium. At a time when the U.S. presidency was transitioning

[230] Friedman (1978c, p. 12); also in Friedman (1978b, p. 16).

[231] Sargent would note, accurately, that Friedman's starve-the-beast argument was not strict positive economics but instead amounted to a hypothesis about policymaker behavior: "that's a political-economy argument. It could be true… Is it possible that big deficits will cause government expenditures to go down? That's all sort of beyond macroeconomics—or at least it's beyond the macroeconomics in those models. It's doing amateur political science." (Thomas Sargent, interview, March 26, 2014.) See also the conference discussion in Sargent (1982, p. 108), in which Sargent remarked that Friedman had essentially expounded the starve-the-beast argument at the conference, that this was consistent with an approach in which fiscal policy and monetary policy were linked, and that the starve-the-beast position essentially made "political judgments, which I don't feel as an economist I can tell you much about." Sargent interpreted Friedman's advocacy of a constant-monetary-growth rule as partly resting on the same judgments (see Sargent, 1982, pp. 111, 113).

[232] Friedman (1984b, p. 43).

from Carter to Reagan, each symposium contributor was requested to give their economic-policy recommendations for the years ahead. The umbrella title for the symposium—"Prosperity Without Inflation"—was poignant, as it matched the title of the book of lectures given in 1957 by someone who was not a participant in the symposium, but who had been a key figure in U.S. economic policymaking during the 1970s: Arthur Burns.

Burns' lectures had been given over the course of October 1957 during a period of perceived serious inflation—a situation reflected in the fact that the word "inflation" appeared in the title of three of the four chapters.[233] But, poor as conditions were thought to be in 1957, by key criteria they were unambiguously worse by December 1980. In terms of long-run output behavior, low or negative productivity growth had become a serious concern in 1980, as discussed in the previous chapter. In terms of the business cycle, the U.S. economy was in an expansion phase at the end of 1980, after the recession in the first half of the year—but the December 1980 unemployment rate was, at about 7.5 percent, three percentage points higher than in October 1957 and about equal to the high that the rate would reach at the trough of the 1957/1958 recession. And inflation, although off its early-1980 highs on the CPI criterion, remained in double digits, whereas the rate thought worrying in 1957 had been in the vicinity of 3 percent.

The deterioration in U.S. economic conditions in 1980 compared with the 1950s was highlighted particularly in the behavior of short-term interest rates. In October 1957, the three-month Treasury bill rate averaged 3.58 percent and the federal funds rate, 3.5 percent. Then, in December 1969, these rates had averaged 7.82 percent and 8.97 percent, respectively. It was this elevated level of interest rates that led Arthur Burns, poised to take over the U.S. central bank, to remark that he would consider "myself, and the Federal Reserve, a failure," if interest rates stayed high over the next few years (*Financial Times* (London), December 19, 1969, p. 5). As it happened, interest rates did fall over much of the following three years, only to reach new, double-digit highs in 1973 and 1974, and they were on the rise again when Burns left office in the first quarter of 1978.

The December 1980 values of interest rates would, however, put in the shade even those observed during Burns' tenure. As earlier noted, the federal funds rate averaged 18.9 percent in December 1980, while the Treasury bill rate averaged 15.49 percent. The most headline-worthy

[233] As it turned out, this period was on the eve of a period of lower inflation, with Burns later noting: "During the 1950s, the notion that creeping inflation was endemic to a modern economy was widely held in the United States. Yet the period from 1958 through 1964 was marked by reasonable price stability." See Burns (1970, p. 2; p. 5 of printed version) (also in Burns, 1978, p. 92),

interest-rate development of all would be with regard to the rate charged by banks on major corporate loans. The prime rate, which had moved into double digits in October 1978, reached what would be remembered as "a historic high of 21.5 percent in December 1980" (*St. Petersburg Times* (Florida), January 12, 1983)—surpassing the 20 percent rate reached the previous April.

The heights that interest rates reached in late 1980 confounded policymakers' prior expectations. Not only was it the case that, as Volcker later remarked, during 1979–1982, "Interest rates went up much further than I would have thought" (*Time*, January 23, 1989, p. 49).[234] It was also the case that the U.S. policy authorities had not expected interest rates to retrace all of the decline that they had exhibited during the middle of the year. When interest rates started coming down in April-May 1980 and the fall had continued over the summer, they perceived much of the decline as permanent: Volcker later recalled that "rates went up immediately [in October 1979]… [and] had a real spike early in 1980," but that view of the declines in the spring and summer, "It looked [to us], at that point, mistakenly, as though that interest-rate episode was short, but over."[235] Indeed, in late April 1980, Volcker had publicly suggested that the U.S. economy might have passed the peak in interest rates (see Nelson, 2021), and in the summer of 1980 he had suggested that firm policies were needed to assure conditions "consistent with maintenance of present—much less, lower—interest rates" (*American Banker*, August 7, 1980). Likewise, in the early fall of 1980, Secretary of the Treasury Miller motivated new economic-policy proposals as being designed "to sustain the gains that have already been made in reducing interest rates."[236] These statements had been rendered obsolete by the new, end-year, peaks in U.S. interest rates.

The 21.5 percent record prime rate, in particular, would be cited in retrospectives as an encapsulation of the tight-money policy of the early Volcker years (see, for example, *New York Times*, June 15, 1983, p. D1). The number would also crop up when the Carter Administration's economic record was recalled in political discourse. As it happened, in the case of another much-watched interest rate, the peak occurred *after* the Carter years: The federal funds rate

[234] He also testified on April 12, 1983, "I did not, just in the interest of full disclosure, think that interest rates were going to be as high as they were for as long as they were." (In Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1983a, p. 41.)

[235] From Volcker's testimony of April 12, 1983, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1983a, p. 44).

[236] From Miller's September 9, 1980, testimony, in Committee on the Budget, U.S. House of Representatives (1980, p. 104).

would rise further after December 1980, its peak of 19.1 percent occurring in June 1981.[237] But 21.5 percent really was the peak for the prime rate, and the fact that it occurred during the Carter presidency would prove a useful political point for Republicans—allowing them to associate high interest rates with the previous incumbent rather than with the early Reagan years. George H.W. Bush, for example, would explicitly cite the interest rate of "21½ percent, if you can believe it" in 1984's televised vice-presidential television debate (October 11, 1984). Less specifically, Volcker himself alluded to the number when he recalled of interest rates during the NOP era, "I certainly didn't expect them to go to 21 percent."[238]

Federal Reserve Board governors in the Volcker disinflation

Milton Friedman did not blame Carter primarily for the range of values that nominal interest rates had reached by the end of 1980. Insofar as a single person was responsible in Friedman's estimation for nominal rates being in high double digits, it was likely Burns. Burns had been out of office for nearly three years by December 1980, but as Federal Reserve chair he had presided over the second monetary explosion of 1976–1977 that Friedman believed had given the United States double-digit inflation in 1979–1980.

This high inflation rate set a high base around which nominal interest rates were set to fluctuate in 1980. But when it came to the particularly high interest-rate levels prevailing in late 1980, Friedman put the blame on the more recent stewardship of the Federal Reserve. He saw the severe second-half runup in interest rates as being the fault of 1980's monetary policy. In particular, as already indicated, Friedman regarded the new highs in interest rates recorded late in the year as levels that could have been avoided by a policy that instilled a steadier path of monetary growth over the course of the year. Specifically, he felt that such high interest rates could have been forestalled by an approach that had made the pickup in monetary growth that had occurred after May much slower and so, in the course of obtaining this outcome, had not allowed the U.S. economy to gain the head of steam that it did once credit controls were relaxed.

For letting monetary growth get out of hand in late 1980, and for a general lack of interest in pursuing policies that would have delivered more stable monetary growth, Friedman centered his criticism on the members (governors) of the Federal Reserve Board—who, he believed, set a tone followed by the overall Federal Open Market Committee that set monetary policy. The

[237] This peak would, unlike the late-1980 values of the federal funds rate, be associated with very low growth rates in money.
[238] In A. Smith (1989, p. 24).

Board governors he listed in his indictment included, of course, Volcker. But, in an interview given in late March 1981, Friedman also named three other members of the Federal Reserve Board: Governors Lyle Gramley, Charles Partee, and Nancy Teeters, who he suggested were all "dyed-in-the wool Keynesians, and Democrats, and overpower the rest of the [Federal] Open Market Committee. That's part of the problem." (*Boston Globe*, April 1, 1981, p. 45.)

Of the three governors whom Friedman named, Gramley had largely switched to being a team player with respect to a policy of sustained monetary restraint. Gramley had indeed been one of the senior Federal Reserve Board staff involved in the public and internal fightback against monetarism in the 1960s. But he had become very dissatisfied with the setting of monetary policy during the Burns years and had been further disillusioned with U.S. stabilization policy traditions when serving as a senior member of the Carter Administration's economic policy team.[239] Gramley therefore had a lukewarm attitude when, as a member of the Council of Economic Advisers, "I was sent over—as a monetary guy on the council, I was sent over to try to change Paul Volcker's mind" after Volcker introduced the October 1979 policy tightening. As Gramley noted of his effort at persuading Volcker, "That didn't work." (Lyle Gramley, interview, June 24, 2013.)

When, therefore, Carter nominated Gramley to membership of the Board of Governors, Gramley was generally in the Volcker camp. He continued to have deep reservations concerning monetarism and did not regard highly the criticisms of Federal Reserve policy that came from Beryl Sprinkel—of whom Gramley observed: "He had rather naïve notions that, somehow, all you had to do was set the money supply at a constant growth rate and everything then would be wonderful." (Lyle Gramley, interview, June 24, 2013.) But Gramley was supportive of Volcker's intention to institute a sustained restriction of aggregate demand. "We're trying our best," Gramley observed. "We can keep this up for quite a while. This is a pretty tough bunch of guys. We're going to hang tough." (*Chicago Tribune*, December 11, 1980.) Friedman's criticism of Gramley had, by 1981, been overtaken by events.

In contrast, Governors Charles Partee and Nancy Teeters better conformed to Friedman's picture of policymakers resistant to monetary restriction. Partee indicated publicly that he was not convinced of the merits of more precise control of within-year variations in monetary growth (*Time*, October 13, 1980). More fundamental, from the point of view of achieving disinflation, was the fact that Partee, in internal FOMC deliberations, questioned the rationale for longer-term

[239] See Biven (2002, p. 207) and Nelson (2020b, Chapter 15).

monetary restraint. Volcker publicly made clear that the Federal Reserve's restrictive course was designed to put downward pressure on inflation through the creation of economic slack.[240] But at the December 1980 FOMC meeting, Partee cast doubt on the slack-to-inflation channel, questioning why the Committee had shifted from an approach that sought to avoid excessive demand to one in which they actively sought a substantially negative output gap for inflation-control purposes (Federal Open Market Committee, 1980c, p. 53). At the same meeting, Teeters, who from early in her tenure had been stressing cost-push factors as driving inflation (see Nelson, 2005), seconded Partee's remarks and lamented a situation in which policymakers seemed to be deciding "how bad we are going to make the economy" (Federal Open Market Committee, 1980c, p. 55).[241]

These criticisms were manifested in Teeters (though not Partee) registering a dissenting vote at this FOMC particular meeting. And by the time of this FOMC meeting, Teeters had overtly opposed interest-rate rises by voting against discount-rate increases that the Federal Reserve Board instituted in November and December 1980 (*Chicago Tribune*, November 22, 1980; Meltzer, 2009b, p. 1064). "I think interest rates have gone too high," Teeters had observed publicly, in explaining her votes (*Chicago Tribune*, December 11, 1980).

The critical posture toward the FOMC's strategy that Teeters and Partee expressed would spill over into votes taken at the FOMC meeting in early July 1981. As discussed in Chapter 8, Friedman favored the Federal Reserve announcing a four-year (later changed to five-year) plan of reducing monetary growth each year to rates consistent with price stability. At his confirmation hearing in July 1979, Paul Volcker indicated that he did not want to take as explicit a stand at this, owing both to his reservations about making evaluations of trend velocity growth several years ahead and to specifying a year in which to attain price stability (see Committee on

[240] For example, early in his tenure as chair, Volcker remarked (*American Banker*, September 5, 1979): "The more the slack in economic activity, the more the downward pressure on prices." At the start of 1981, he testified (Volcker, 1981a, p. 19): "Given enough time, sluggish business performance should itself tend to restrain inflation." Consistent with the output-gap mechanism being a key channel through which monetary policy worked on inflation, Volcker also remarked: "We hope and expect that the restraints on money and credit over time will develop other forces in the economy that will work against inflation. That is the lesson of history." (Testimony of March 27, 1981, in Committee on the Budget, U.S. House of Representatives, 1981, p. 262.)

[241] Meltzer (2009b, p. 1061) claimed, without providing a source, that Teeters was among those who "used a Phillips curve, so they believed that lower inflation required higher unemployment, especially in the short run." The Meltzer account of the Great Inflation and the disinflation often mischaracterized the views on inflation held by Federal Reserve policymakers, and this was a case in point. It was *Volcker's* position that subscribed to a (short-run) Phillips-curve link between unemployment and inflation, whereas Teeters cast doubt on this position by espousing cost-push perspectives on inflation. Note also that Meltzer's claim that differences among FOMC participants on the acceptable unemployment costs of a disinflation "were never explicitly discussed" overlooked the explicit discussion by Governors Teeters and Partee of these matters at the December 1980 FOMC meeting (see Federal Open Market Committee, 1980c).

Banking, Housing, and Urban Affairs, U.S. Senate, 1979b, pp. 13, 15). He nevertheless expressed sympathy with such an idea and affirmed that the FOMC should seek price stability in steps "over a period of years."[242] Subsequently, a persistent theme of Volcker's public remarks during his first term as chairman was the multi-year and maintained nature of the Federal Reserve's policy course. In December 1980, for example, he stated that the Federal Reserve would "contain persistently the growth of money and credit, by controlling its supply, to amounts commensurate with a return to price stability."[243] The following month he remarked (Volcker, 1981a, p. 20): "Credibility in policy commitment will have to be earned by performance maintained through thick and thin." And in April, as the 1981 speaker in an annual lecture series to which Friedman had been the 1978 contributor, Volcker observed: "We plan to make the needed reduction [in monetary growth] not all at once, but over a period of several years."[244]

As part of the process of committing monetary policy, Volcker arranged in 1980 and 1981 for the FOMC to make, in the middle of the year, a tentative decision on what the next year's monetary-growth targets would be. In July 1981, the FOMC did make such a decision regarding its 1982 targets. But Governor Teeters dissented in the vote. One of the grounds she gave for her dissent—that it was too soon to decide the appropriate number—would receive some vindication in the fact that M1 velocity indeed veered away from its trend in 1982. But in mid-1981 this trend shift was not widely expected, and Teeters' reluctance to support the 1982 monetary targets likely lay not solely in technical considerations but in her continuing basic doubt about the merits of an aggregate-demand-centered fight on inflation—a likelihood reflected in the fact that she stated that she would have preferred keeping the same target in 1982 to lowering it (see Federal Reserve Board, 1982, p. 119). The notion that there was a body of opinion on the FOMC that wished to stop short of further years of demand restriction was also evident in Governor Partees' dissenting vote at the end of the same July meeting, when, in the vote on the immediate policy decision, he indicated his wish a less firm monetary policy stance than what had received the majority vote (Federal Reserve Board, 1982, p. 121).

Henry Wallich and inflation

[242] From Volcker's letter, submitted in connection with his confirmation hearing (July 30, 1979), in Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1979b, p. 15). See also Volcker's testimony of November 13, 1979, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1980a, pp. 29–30).
[243] Volcker (1980a, p. 5). Earlier, Volcker had remarked that "we have simply said that we think we have to continue working with the general intention of getting these [monetary] growth trends back to a level that we think is consistent with price stability." (September 10, 1980, testimony, in Committee on the Budget, U.S. House of Representatives (1980, p. 166.)
[244] Volcker (1981c, p. 4).

Notably absent from Friedman's list of Board governors who he thought were setting the wrong tone was Henry Wallich—a member of the Board since 1974. Wallich had a far more extensive public record of espousing cost-push drivers of inflation and the merits of incomes policy than either Teeters or Partee did. Indeed, during his years as a *Newsweek* economics columnist and an academic, when the Nixon wage-price controls had been in place for about five months, Wallich saw value in the controls and praised the president's 1971 *U*-turn on the matter (*Newsweek*, January 31, 1972, p. 36). Most prominently, he had been one of the proponents and developers of the "tax-based incomes policy" (TIP) idea in the 1970s and had continued to push the notion in his first five years as a policymaker. By the early 1980s, however, Wallich was no longer selling TIP hard. In a speech given in late October 1981 titled "Are There Alternative Ways of Fighting Inflation?," he did not repudiate the TIP idea but accepted that it was now off the national agenda (Wallich, 1981a, p. 15).

What likely underpinned Friedman's implication in 1981 that Wallich was a force for good in policymaking was the track record he had established at the Federal Reserve since 1974. When Wallich, one of Friedman's fellow *Newsweek* columnists right up to that point, was nominated to be a Board governor, Friedman praised Wallich's past research but added a sting in the tail by remarking that Wallich was "not a man of great personal force" who would provide a "truly independent" voice in monetary policy deliberations (Instructional Dynamics Economics Cassette Tape 138, January 16, 1974). Wallich defied this expectation by not only supporting FOMC policy tightenings but also frequently entering dissenting votes that indicated a wish to go further in the direction of policy firming. He did so in the Burns and Miller years, during which "he was a little out of phase," recalled Stephen Axilrod, who as a senior staff member in the monetary area was Wallich's office next-door neighbor at the Board (Stephen Axilrod, interview, April 24, 2013).[245] Indeed, Wallich was one of the majority of governors who passed a discount-rate increase over G. William Miller's (and Partee's) opposition in the much-recalled Board vote of June 30, 1978 (Federal Reserve Board, 1979, p. 97).

Wallich was far closer to being on the same wavelength with the top Federal Reserve leadership during Paul Volcker's years as chair. Reflecting this fact, Wallich's departure from the Board at the end of 1986 after nearly thirteen years in office prompted the headline "Volcker Loses Ally At the Fed" (*Journal of Commerce*, December 17, 1986). Wallich was not, however, a mechanical supporter of Volcker's policy position. Even in the key Volcker disinflation period

[245] Axilrod further noted that, long before Wallich became a Board governor, "He and I wrote a joint article when I was a young kid." This was Wallich and Axilrod (1964).

of the early 1980s, Wallich registered multiple dissenting votes in Board and FOMC decisions related to monetary policy.

Wallich, nevertheless, differed markedly from Governors Partee and Teeters in posture. In his own votes of dissent, Wallich's preferences were tilted in the direction of a *tighter* policy than that favored by the majority. In the latter months of 1980, for example, Wallich, in FOMC votes in September, October, and early December, favored tighter policy settings than those that the majority of the Committee supported (Federal Reserve Board, 1981b, pp. 144, 151, 158). During his research career (primarily as a professor at Yale University), Wallich had been a critic of Friedman's on numerous occasions.[246] And his persistent belief in tighter monetary policy in the early 1980s reflected no full-scale conversion to monetarism. True to Friedman's maxim, noted at the start of this chapter, that none of the Board members in office in October 1979 were monetarist, Stephen Axilrod observed that "there were no monetarists on the Board, actually," and added specifically with regard to Wallich: "Henry was the last person I'd ever think of as a monetarist... I don't think he had any positive attitude [toward monetarism]. He was an anti-inflationist. That's very different." (Stephen Axilrod, interview, April 24, 2013.) In a similar vein, Paul Volcker (interview, October 16, 2013) recalled of Wallich: "I don't know that he disliked Friedman personally, but I guess he saw him as something of an intellectual rival… Henry Wallich's obsession was in dealing with inflation. I don't remember him being in any way a convinced [monetarist]. He certainly was not a pure monetarist on the Friedman scale."

Indeed, despite the fact of their common ground on the need for monetary restraint, Wallich continued to mark his position out from Friedman's in the early 1980s. One of the ways in which Wallich did so was via his emphasis, in common with Nancy Teeters, on the importance of interest rates. Even though the FOMC was now conducting policy in terms of nonborrowed reserves, Wallich continued to highlight interest-rate aspects of the policy decision. In his December 1980 dissent from the FOMC vote, for example, Wallich gave as his grounds for dissent his wish that the Committee specify a higher tolerance band for the federal funds rate range they associated with their decision (Federal Reserve Board, 1981b, p. 158). More generally, as will be seen, he saw monetarism as far from having won the debate in interest rates versus monetary aggregates as indicators of monetary policy stance. The year 1981 would add important empirical evidence pertaining to this debate, as the U.S. economy would enter one of its worst postwar recessions.

[246] An early manifestation was Wallich's (1962) review of *Capitalism and Freedom*, in which his criticisms included Friedman's advocacy of floating exchange rates.

Renewed recession in 1981

In the final few months of 1980, the evidence that the recession that had begun early in the year was proving to be short, together with the post-April take-off in monetary growth, convinced Friedman that the United States would need another recession before long if a disinflationary path was to be resumed. In mid-October, speaking in Singapore, he remarked that he would "not be surprised to see recession in the U.S. resume, although it is tapering off at the moment" (*The Citizen* (South Africa), October 17, 1980). Back in the United States late in the month, and with evidence already emerging of an excessively rapid U.S. economic recovery, Friedman suggested that renewed recession was likely in 1981 as the monetary authorities restored downward pressure on aggregate demand (*Pittsburgh Post-Gazette*, October 28, 1980). A "rather serious recession" lasting up to eighteen months would likely be needed, he observed when speaking shortly afterward at Stanford University, at an American Electronics Association event (*Merced Sun-Star* (California), October 30, 1980).[247] After the election, the message that the Reagan Administration would need to preside over a recession in 1981 was a somber aspect of Friedman's otherwise optimistic contribution to the near-close-of-year *U.S. News and World Report* symposium.[248]

Because he did not believe that expansions automatically gave rise to recessions, this was not an unconditional forecast: instead, it embedded an assumed change in the course of monetary policy. Specifically, Friedman was presuming that monetary growth would be slowed down from its very rapid second-half 1980 rates. It was because such a decline was not preordained at the end of 1980 that Friedman added some qualifications to his projection of a recession projection. "I think the most likely scenario is that the recession will resume in 1981," he remarked at the start of the new year (*San Francisco Examiner*, January 4, 1981). "That's just one possible scenario, and I think it is the most likely one… [but] I am a very poor forecaster." But he reaffirmed seven weeks later that "1981 is going to be a very difficult year" (*Arizona Republic* (Phoenix), February 24, 1981).

"There's no easy way out of the mess we've gotten into, which means that 1981 will be a difficult year," Friedman observed in a mid-January briefing to Oppenheimer and Company. This briefing was being delivered at a time at which Friedman was nevertheless optimistic, as the

[247] He repeated that a recession was needed in his address at the Commonwealth Club of San Francisco on October 31 (Friedman, 1980c, p. 244).

[248] *U.S. News and World Report*, December 15, 1980 (p. 51), reprinted in Friedman (1983a, pp. 194–195). See also the Friedman analysis in *Newsweek*, December 29, 1980, discussed earlier.

Reagan Administration was coming into office. Against this background, Friedman stressed that the sequence he sketched in which a 1981 recession occurred was one he favored, and that others should, too—as it offered the prospect of a noninflationary recovery being able to take place in the post-recession period. "I'm confident we can get the economy growing again and inflation down to 3 percent to 5 percent a year if we pursue proper policies." (*Washington Star*, January 25, 1981, p. A13.) The briefing also underlined, however, Friedman's disaffection with Paul Volcker after 1980's monetary events. Friedman had come a long way from his December 1979 position of being "reasonably well satisfied" with monetary policy under the New Operating Procedures (*The Register* (Santa Ana, Orange County, California), December 23, 1979, p. A11) and was back to his more customary posture of being a harsh critic of current Federal Reserve policy. Disillusioned with Volcker on account of 1980's events, Friedman in early 1981 called the Federal Reserve "the weakest link in the chain" (*Washington Star*, January 25, 1981, p. A13).

In the next several months, Friedman saw little evidence of the needed reduction in monetary growth. When it came to the imperative to "keep the money supply growth down," Friedman concluded in May that so far "the Federal Reserve hasn't been doing it" (*The Daily Oklahoman* (Oklahoma City), May 19, 1981).

In early July, at the Western Economic Association meetings in San Francisco, he indicated that empirical evidence was building against his long-term support for gradualist disinflation policies, because it seemed that such multi-year programs could not be adhered to. As far as other countries were concerned, Friedman now acknowledged: "The successful cases of curbing inflation by controlling monetary growth I know of have all been [achieved by] sudden reductions." With regard to the United States, he had reacted critically in the past when (as in 1969, for example) the Federal Reserve had departed from gradualism by shifting abruptly, instead of in stages, from monetary growth rates consistent with elevated inflation rates to rates implying price stability. He had also, as discussed above, been highly critical when monetary growth turned negative in the spring of 1980. In both cases, the shift to very low monetary growth had been followed by monetary breakouts: the early 1970s had seen disinflation abandoned altogether, the year to mid-1981 had seen rapid average growth in both monetary aggregates and nominal income. He consequently could not, in 1981, point to a very clear-cut case in which a gradualist disinflation had been realized. Friedman stressed, nevertheless, that he still believed in a gradualist policy as a means of reducing the real costs of a restriction in aggregate demand. (*San Francisco Examiner*, July 6, 1981, p. D8.) But the 1980 experience had left him with further doubts that a multi-year disinflation, involving an initial protracted recession, was something that could be carried out in practice.

Notwithstanding this doubt, even in mid-1981 Friedman did allow himself a central scenario in which disinflation was pursued in the United States from now on. In May 1981, although, as already indicated, he did not yet perceive a slowdown in monetary growth as being currently underway, he nevertheless ventured a prediction: "We'll have a recession [starting] over the next two quarters" (*The Daily Oklahoman* (Oklahoma City), May 19, 1981). His basis for so suggesting was that, for all Friedman's complaints about actual monetary outcomes, and his view that the Federal Reserve was responsible for avoidable short-run fluctuations in monetary growth, he mostly accepted that Volcker did want to bring down inflation and would take steps to hold down *longer-run* monetary growth to that end. Volcker, Friedman noted, was determined to stay in office until "he turns the Fed around. I hope he achieves it." (*Boston Globe*, April 1, 1981, p. 45.) But as of the end of the first quarter of 1981, "his record" had not established that Volcker had achieved this aim: "I want him to put up or shut up." (*Boston Globe*, April 1, 1981, p. 39.)

Although Friedman would relent little in his criticism of the Federal Reserve, developments in 1981 were to see clear evidence of monetary restraint. This was manifested in the renewed recession that Friedman believed was likely. And, consolidated by further restriction over much of 1982, it would eventually lead into the noninflationary recovery starting late in 1980.

Paul Volcker was candid in the public record during 1981 that he still had to show definite results. He was not of the view that he or the FOMC had lost resolve during 1980. On the contrary, Volcker remarked in early 1981 that he "hope[d] 1980 was instructive in demonstrating that we do take the targets seriously, both as a means of communicating our intentions to the public, and in disciplining ourselves."[249] Other public remarks Volcker made during 1981, indicated, however, that he shared a prominent concern—related to *future* policy settings—that both Friedman and Ronald Reagan would also highlight on numerous occasions in 1981 and 1982. This related to the fact that the private sector was skeptical that the authorities would, as Volcker put it in March 1981, "maintain the monetary policy that is necessary to deal with inflation."[250] As already noted, Volcker saw public undertakings about continuing, multi-year, reductions in monetary growth as an important part of the process of shaking off this skepticism. More generally, he remarked in early 1981 that "we have to demonstrate we can turn the corner on inflation," even when the necessary policy actions involved "short-term sacrifice" for the country.[251]

[249] Testimony of February 26, 1981, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1981b, p. 4).
[250] Testimony of March 3, 1981, in Committee on Ways and Means, U.S. House of Representatives (1981, p. 354).
[251] Testimony (February 5, 1981), in Joint Economic Committee, U.S. Congress (1981, pp. 13 and 7, respectively).

In articulating their resolute attitude toward disinflation, both Volcker and Wallich in 1981 ended up echoing—albeit in more diplomat language—many of the indictments of past U.S. stabilization-policy experience that Friedman had made during the 1960s and 1970s. Both of them disliked the polemical manner in which Friedman and other monetarists had critiqued the historical performance of the post-Accord Federal Reserve. "I find it regrettable that their analysis causes them to concentrate their criticism on the Fed," Wallich remarked at the end of 1981. "Allies fighting for a common cause [eliminating inflation] ought not to fight among themselves."[252] But when instilling sustained aggregate demand restriction over the course of 1981, Wallich and Volcker each largely paralleled Friedman's criticisms of past policies and of prior traditions of inflation analysis. For his part, Wallich remarked in an end-1981 talk on inflation: "I believe it is the Keynesian thinking of our times that, permeating most of the older generation of economists and through them [the] government and the public, that is fundamentally responsible for much of our inflation." [253]

As for Volcker, although he did not criticize his predecessors Martin, Burns, and Miller (with each of whom he had worked) by name, he repeatedly restated Friedman's criticism of past policy *U*-turns.[254] Volcker stressed in his aforementioned testimony of March 3, 1981, that "we will not back off on monetary policy, because that would put us right back in the situation" that the economy had encountered in prior years.[255] He added on July 21 that "after a decade and more of disappointment, there is persisting skepticism and doubt about the ability of the nation to persevere in an anti-inflation program. I believe that skepticism is unwarranted, but we must

[252] Wallich (1981c, p. 4).

[253] Wallich (1981c, p. 1). This criticism of Keynesian approaches was made at the incongruous location of a session in memory of Arthur Okun,

[254] Volcker had served on the FOMC with Burns in 1975–1978 and with Miller in 1978–1979. He had also held senior Treasury positions in 1961–1965 and 1969–1974 when Martin and Burns headed the Federal Reserve and, of course, headed the Federal Reserve while Miller headed the Treasury for nearly eighteen months through early 1981. Meltzer (2009b)—who over time, unlike Friedman, became a strong proponent of ringfencing the Federal Reserve as much as possible from the U.S. Treasury—implied that there was something extraordinary about Volcker appearing in the same Congressional hearing as Miller, observing (p. 1050) that "Volcker even accompanied members of the administration when they testified in Congress about budget cuts." In fact, the hearing in question (which Meltzer did not specify), held on February 1, 1980, was one of the Joint Economic Committee's sessions on the *Economic Report of the President* (annual hearings in which the Federal Reserve chair and U.S. Secretary of the Treasury each traditionally testified), and Miller and Volcker testified consecutively in the same JEC hearing session, with Miller being discharged as a witness before Volcker began his testimony. The two had actually testified consecutively before (on October 17, 1979): see Joint Economic Committee (1980b). And as Under Secretary of the Treasury, Volcker had, on September 26, 1969, testified jointly (actually appearing on the same panel as him and answering questions alternately) with Federal Reserve Chair Martin (see Committee on Banking and Currency, U.S. Senate, 1969).

[255] In Committee on Ways and Means, U.S. House of Representatives (1981, p. 354). Similarly, later in the month, Volcker testified: "After years of false starts in the effort against inflation, there is widespread skepticism about the prospects for success." (Testimony of March 27, 1981, in Committee on the Budget, U.S. House of Representatives, p. 241.)

make that claim good by our actions."[256]

By the time of this last remark, Volcker had, in effect, made good on his words through deeds, as the FOMC had implemented a major monetary restriction. In the months leading up to this point, in late 1980 and early 1981, Volcker gave indications that he was willing to countenance another recession rather than forestall one. Charles Partee at the FOMC meeting in the second half of 1980, while supporting that meeting's vote, was concerned that the Committee was headed in this direction under Volcker, stating: "As far as I'm concerned, we really ought not to plan a policy that produces less than zero [real output] growth."[257]

For his part, Volcker in early December had publicly acknowledged "renewed and understandable concern about the sustainability of the recovery."[258] But over subsequent months, Volcker—although not casting himself as regarding a recession as a necessary, and deliberately planned, part of a sustained inflation fight—implied that such a recession might well materialize. Abel and Bernanke (1992, p. 329) would later remark, "Many economists claim that this recession [that in 1981−1982] was knowingly created by the Federal Reserve in order to reduce inflation," and Volcker's own public stand during the time of the relevant policy decisions was not out of step with this interpretation.[259] Notably, on April 15, 1981, and in contrast to the mixed messages coming from the administration regarding whether a decline in dollar aggregate demand was part of U.S. economic strategy Volcker confirmed that monetary policy plans did entail an ongoing slowdown in the growth of U.S. aggregate nominal income.[260]

At just this time, monetary growth was poised to move to rates consistent with restriction. This was so especially in the case of M1 growth (see Table 3). M1 growth exhibited rapid growth in March and April—strength that, because of quarterly averaging, slows up primarily in the second-quarter number in the table. But its course turned thereafter, as is especially apparent in the third-quarter number in Table 3. Indeed, in a vector autoregression analysis that used M1 as the primary monetary policy variable, Blanchard and Watson (1986, p. 135) found that 1981:Q3 saw one of two "very large" contractionary monetary policy shocks in the 1979−1982 period (the

[256] In Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1981b, p. 167).
[257] Federal Open Market Committee (1980c, p. 53).
[258] Volcker (1980a, p. 3).
[259] As of November 1980, the assessment of leading forecaster Michael Evans was different: "The Fed doesn't want to tighten policy enough to cause another downturn. Volcker was genuinely appalled by the wreckage spawned this spring by the 20 percent prime rate and credit controls, and [he] will go to great lengths to avoid a repeat performance." (*Wall Street Journal*, November 11, 1980, p. 1.) Claiming that Volcker would "go to great lengths" certainly overstated matters, but Volcker's statements during the 1980–1981 expansion did not imply that that expansion had to end (as opposed to suggesting that it was too rapid).
[260] Volcker (1981c, p. 9).

Table 3. Monetary growth in 1981, by quarter

	Annualized quarterly growth rates			
	1981:Q1	1981:Q2	1981:Q3	1981:Q4
M1	5.6	10.8	1.1	5.0
M2	7.0	11.3	7.3	10.7

Source: Computed from quarterly averages of data in Federal Reserve Bank of St. Louis' FRED portal.

other one occurring during the credit-controls episode of 1980).[261]

That third-quarter observation, in turn, essentially reflected a change in the course of M1 growth that had begun around late April. Friedman himself would contrast 11.1 percent M1 growth in the year to April 1981 with annualized growth of –0.2 percent in the six months to October 1981 (*Newsweek*, August 23, 1982).[262] Similarly, Maxwell Newton, a New York-based financial columnist with strong monetarist leanings, would use M1 growth as the basis for dating the Federal Reserve's tightening as starting in April 1981 (Newton, 1983, p. 30) and would subsequently refer (*The Australian*, July 4, 1984) to the "infamous freeze on money growth that lasted for most of the period between April 1981 and July 1982." Likewise, Lawrence Roos, who served as president of the Federal Reserve Bank of St. Louis throughout the 1979–1982 period and so participated in FOMC deliberations, would characterize M1 growth as having been brought down to a near-zero rate by the fall of 1981 after its rapid growth in the second half of 1980 (*Wall Street Journal*, April 7, 1983).

By late July, these developments were sufficiently in train that Volcker observed publicly that "you [we] have a very restrained monetary policy now."[263] As already indicated, it was also in July 1981 that Volcker faced dissenting votes at the FOMC meeting by Governors Partee and

[261] One of the authors later used interest rates as the variable from which to extract monetary policy shocks, and because interest rates already rose in the second half of 1980, this practice resulted in an earlier dating of tightening, with Bernanke, Gertler, and Watson (1997, p. 121), referring to "the autonomous tightening of monetary policy in late 1980 and 1981."

[262] Using quarterly data and a later revision of the monetary series, Friedman (1985b, p. 55) gave M1 growth as moving from 10.1 percent in the year to 1981:Q2 to an average of 3.9 percent annualized growth in the two quarters 1981:Q3 and 1981:Q4.

[263] Testimony of July 22, 1981, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1981b, p. 9). A. Smith (1989, p. 23) observed: "Volcker frequently says 'you' when many of us would say 'I' or 'we.'"

Teeters on different aspects of the Committee's decision to continue tightening. But the restriction in M1 growth, already well in process, continued, and Volcker kept majority support.

In terms of interest rates, policy was also unambiguously tight. In contrast to the situation prevailing over May-November 1980, when monetary growth and interest rates gave different signals about whether monetary policy was tight, interest rates and monetary growth both indicated tight monetary policy over May-November 1981. In real terms, interest rates moved up even more steeply, as inflation was declining in this period and beyond. Nominal interest rates, however, peaked in mid-year, with the federal funds rate's average value being 19.1 percent in June 1981, followed by 19.04 percent in July. Meltzer (2009b, p. 1082) was therefore not correct in characterizing the spring of 1981 as a period in which the FOMC was "increasing interest rates in the middle of a recession," as the recession did not officially start until business activity peaked in July 1981.[264] It is nevertheless true that the FOMC tightened in 1981 while unemployment was still high after the previous year's recession.[265]

Once the recession was in process in the second half of 1981, M1 growth remained low, but nominal interest rates came off their peaks. In late October, Volcker noted that "in the last few months… short-term interest rates have come down considerably."[266] This decline continued, with the average federal funds rate lower in each successive month from July to December 1981, when it stood at 12.37 percent. Over this period, the FOMC was acquiescing in rate declines as M1 came inside its target range. Indeed, in the fourth quarter of 1981, it encouraged higher growth in reserves and money to forestall a more serious undershooting of the M1 target.

Of Volcker's policymaking colleagues, Henry Wallich was something of a holdout in this pattern. His instinct that the Federal Reserve should send signals about a tight policy stance through maintained high interest rates made him reluctant on occasion to accept interest-rate declines even when his colleagues judged these to be consistent with monetary-growth developments. So in 1981, as in 1980, Wallich's votes displayed an interest in maintaining the pre-1979 practice of registering policy stance through interest rates—and, in particular, conveying a maintained tight monetary policy by resisting interest-rate declines. In this vein, at

[264] Another characterization of the 1981 rate rises as having occurred during a recession was Blanchard (1984, p. 211). Real GDP growth was negative in 1981:Q2. On this basis, one might well categorize the spring of 1981 as a recession period, but not, as Meltzer did, the "middle of a recession," unless one counts 1980–1982 or 1979–1982 as one long recession (as some, including Friedman, preferred to do).

[265] The Federal Reserve Board (not the FOMC) had raised rates during a recession when in February 1980, before the recent business cycle peak had been recognized, it increased the discount rate.

[266] Testimony of October 29, 1981, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1981c, p. 553).

the February 1981 meeting not only did Wallich vote for a lower monetary-growth target range (Board of Governors, 1982, p. 95) but also, in the vote on the immediate policy decision, wanted a higher federal funds rate range to reinforce the message of monetary restraint (p. 98). And on September 21, 1981, Wallich dissented when his fellow governors voted a 100-basis point cut in the discount-rate surcharge. The basis for his dissent was recorded as: "Mr. Wallich voted against this action because he felt that, given prevailing economic and financial conditions, a reduction might convey a misleading signal regarding the outlook for monetary policy."[267]

Money versus interest rates in interpreting post-October 1979 developments

It was not just the signal that the Federal Reserve might be sending to the private sector regarding its monetary policy stance that made Wallich uncomfortable with declines in short-term interest rates—it was also the potential effect that such declines had on the reality of monetary conditions. He worried that Federal Reserve acceptance of lower interest rates might actually imply a looser policy stance than the anti-inflationary posture that the Board and FOMC sought, even in the context of an unchanged monetary target. For although, as discussed below, Wallich was a supporter of monetary-growth targets during the early Volcker years, he remained far from convinced that money was a better monetary policy stance than interest rates.

Wallich communicated his own stand on this matter prominently in his public remarks during 1981. In so doing, notwithstanding his caution nearly two decades earlier that it was a "difficult and hazardous exercise to match wits with Milton Friedman" (Walllich, 1982, p. 40), he decided it was once again time to challenge Friedman explicitly.

The new debate occurred despite considerable convergence on Wallich's part over the years toward Friedman's economic mindset. By 1981, Wallich had acquired considerably greater confidence in monetary policy's importance than he had once displayed. In 1968, he had indicated that he was opposed to a monetary-growth rule but asserted that as "[i]t is a mild effect that money exerts on economic activity," a monetary-growth rule would not make too much difference to the behavior of the economy: it would not prevent depressions from occurring and would by itself not add much to cyclical fluctuations, even if, as Wallich feared, it led to much more variable interest rates.[268]

By the mid-1970s, Wallich's estimation of monetary policy's effect on aggregate demand was

[267] Federal Reserve Board (1982, p. 82).

[268] Joint Economic Committee, U.S. Congress (1968a, pp. 13–16; quotation from 16).

much higher. Furthermore, he had moved, despite his ongoing interest in incomes policy, toward Friedman's positions on both incomes policy. Just five days after the October 1979 change in Federal Reserve procedures, Wallich remarked publicly: "Greater steadiness is needed. In particular, we need steadiness in reducing the growth of the monetary aggregates in order to wind down the inflation."[269]

Nevertheless, against the background of this shift to a greater emphasis on monetary policy's importance, Wallich remained critical of Friedman's belief in money as an indicator of policy stance, particularly in the context of cyclical movements. This was a matter that had erupted about a year into Wallich's tenure as a Board governor, in the Federal Reserve's exchanges with Friedman, around the time of the 1975 economic trough, on how to interpret low monetary growth when nominal interest rates were well down from their highs. In that period, Wallich had remarked in New York that "money works through interest rates" (*Washington Post*, May 22, 1975). He had tended to discount the signal given by monetary aggregates in that period and, as discussed in Chapter 5 above, had argued—contrary to Friedman—that the course of interest rates had provided a better guide to future economic developments than had M1 or M2.

Friedman was, of course, not averse to thinking of monetary transmission in terms of interest rates at a conceptual level, if these were interpreted broadly. But as far as empirical analysis was concerned, in the absence of an appropriately broad-based index of rates, he favored looking at money.[270] But Wallich, unlike Friedman, was satisfied that the most observable market interest rates contained the most relevant information. This was made clear in their respective remarks at the International Monetary Conference in New Orleans in early June 1980. The event occurred during the credit-controls period and came in the wake of spring declines in money, interest rates, and production. Declines in interest rates, he remarked, were "not what I would call moving from an easy money policy," as monetary growth had as yet only turned mildly positive. "If you are going to write about monetary policy," he told the financial journalists and participants present, "[then,] for God's sake talk about what's happening to the monetary

[269] Wallich (1979, p. 3). Wallich also largely accepted a durable regime of floating exchange rates, after having been a critic of Friedman's on international monetary arrangements and a self-described "defender of fixed exchange rates" (Wallich, 1969, p. 360) during the Bretton Woods era. He did, however, defend the U.S. government's interventions in the foreign exchange market in the post-Bretton Woods period, doing so against Friedman's criticisms (see Chapter 9 above). With regard to the criticisms of floating rates (and of domestically-focused monetary policy arrangements) that Wallich articulated during the 1960s, see also the 1962 item noted earlier (Wallich,1962, p. 40), as well as Joint Economic Committee, U.S. Congress (1968a, pp 14, 18–19), and Friedman's discussion of their December 1968 American Economic Association roundtable exchange in Instructional Dynamics Economics Cassette Tape 9 (January 1969).

[270] This was in reference to analysis of cyclical fluctuations. With regard to longer-run price behavior, there were further grounds for using money as the monetary policy variable.

aggregates." (*State-Times* (Baton Rouge, Louisiana), June 3, 1980.) In contrast, Wallich (1980a, p. 4) argued that it was the interest rate that "in the last analysis" gave the central bank an influence on the economy, and that even under current reserves-focused operating procedures money and credit were essentially "controlled by means of the interest rate."

Wallich's conviction that market interest rates provided useful indications of monetary policy stance would be reinforced by the unfolding experience with the post-October 1979 policy regime. Despite Friedman's own conviction that 1980's events had underlined the importance of variations in monetary aggregates in understanding economic fluctuations, *New York Times* economics columnist Leonard Silk had argued that the "year was, on the whole, a bad one for monetarists" and suggested that the year might revive Paul Samuelson's dictum that central banks should keep their eyes on money and interest rates alike (*New York Times*, December 31, 1980). Wallich's interpretation of 1980's course was very much on these lines. Indeed, he felt that the merits of money as a guide to the business cycle had been undercut by recent patterns in the data. Accordingly, in a speech given in early April 1981 speech, Wallich (1981b) offered a challenge to monetarists' position that monetary growth outperformed interest rates as a signal of future economic developments.

In his speech, Wallich cited several features of financial developments in 1980, including the fact that interest rates had declined in the second quarter and rebounded in the third quarter. He added, on the basis of the output data available to him at the time, that the economy had begun to recover in the third quarter of 1980. "To make the case that [changes in] the money supply, rather than the interest rates, move the economy, one would have to assert that the money supply affected the economy with a zero lag," Wallich claimed. "…Given Friedman's time-honored dictum that the money supply affects the real sector with a long [and] variable lag, this seems hardly plausible."[271] The experience supported, he suggested, the "seeming prevalence of an interest-rate rather than a money-supply[-based] evaluation of monetary policy."[272]

In invoking Friedman by name, Wallich had thrown down the gauntlet: he was suggesting that Friedman had to accept either that monetary growth had given the wrong signal concerning the 1980 recovery or that, on this occasion, there had been no lag from monetary growth to economic activity. *New York* magazine would pick up Wallich's critique, noting of the 1980 recovery (April 27, 1981, p. 20): "The monetarists have a hard time explaining that one."

[271] Wallich (1981b, p. 9; pp. 89, 90 of 1982 printing).

[272] Wallich (1981b, p. 8; p. 89 of 1982 printing).

It turns out, however, that substantial revisions to U.S. quarterly national-income data have implied a changed pattern of behavior of output over 1980—one that removes the force of Wallich's indictment. The initial real GNP reading for 1980:Q3 did show a rise in that quarter (*Wall Street Journal*, October 27, 1980). But modern vintages of output data now imply that the 1980 recession actually featured two successive quarters of decline, namely 1980:Q2 (about minus 8 percent annualized for modern real GDP data, as already noted) and 1980:Q3 (–0.5 percent annualized), with the return to positive growth only beginning in 1980:Q4 (7.7 percent annualized).[273] As the rebound in growth rates of monetary aggregates occurred in midyear, the revisions have given money an approximately one-quarter lead over output during the 1980 recovery—not the zero lag that Wallich held would contradict a Friedman-type account of events. Friedman himself would grant that the credit-controls period saw some "significant discrepancies" between monetary growth and nominal income growth.[274] It nevertheless saw a positive relationship between the two series, with money still leading income to some extent.

Some perspective on the respective quality of money and interest rates as respective indicators of the direction of economic activity in this period is given in Tables 4 and 5, which concern the 1979–1982 period. The tables report the correlations, with quarterly growth rates of nominal and real GDP respectively, of various monetary variables: current and lagged quarterly monetary growth (both M1 and M2) and the federal funds rate. Both levels and first differences of interest rates are considered, as well as both nominal and real interest rates—the latter obtained by subtracting the contemporaneous four-quarter CPI inflation rate. This inflation rate and the GDP and monetary growth rates are obtained as log differences.

In interpreting the tables, two points should be mentioned. First, as monetary growth was so variable from 1979 to 1982—with increases in M1 and M2 each actually having mildly *negative* first-order autocorrelation in the thirteen quarters through 1982:Q4—the correlation of income growth and a particular lag of monetary growth may not capture well the money/income relationship if, in fact, income is positively correlated with a *distributed lag* of money. In

[273] Meltzer (2009b, p. 1043) treated it as a fact that 1980 did not see two consecutive quarters of negative economic growth and went on to assert (p. 1057) that the Federal Reserve Board staff was in error in forecasting a continued decline in output in 1980:Q3. These statements, too, rested on the early-1980s vintages of the national accounts in which the recession was confined to the single quarter, 1980:Q2. As indicated, these vintages of data have been superseded by subsequent data revisions.

[274] Implicit in Wallich's (1981b) discussion was the notion that the 1980 period was a legitimate test case for comparisons of money growth and interest rates as indicators, notwithstanding the presence of the credit controls as a factor that affected the relationship of these variables to economic activity. Elsewhere, Wallich (1980b, p. 8) acknowledged that credit controls were a disturbing element that made it difficult to ascertain the underlying relations between monetary variables and the economy over this period.

Table 4. Nominal income growth and monetary variables, 1979 to 1982

	Nominal GDP growth, 1979:Q4–1982:Q4		
	Correlations with monetary growth		
Monetary variable	No lag	One quarter earlier	Two quarters earlier
M1 growth	0.026	0.572	0.102
M2 growth	–0.187	0.394	0.145
M1 growth, 2-quarter average	0.489	0.562	0.252
M2 growth, 2-quarter average	0.178	0.446	–0.358
	Correlations with interest rates		
	No lag	One quarter earlier	Two quarters earlier
Nominal federal funds rate	0.450	–0.168	–0.582
Real federal funds rate	0.007	–0.352	–0.698
Change in nominal funds rate	0.353	0.401	–0.493
Change in real funds rate	0.504	0.513	–0.425

Source: Federal Reserve Bank of St. Louis' FRED portal. Growth rates are calculated as percentage changes in quarterly nominal GDP and in quarterly averages of M1 and M2. Interest rates calculated as quarterly averages. Changes in interest rates are calculated as first differences of these averages. Real rates are obtained by subtracting four-quarter CPI inflation rate.

recognition of this point, Table 3 also reports correlations of income growth with a two-quarter moving average of monetary growth. Second, Wallich (1981b) presumed that the short-run relationship between interest rates and income should be negative, and so the negative interest-rate/GDP correlations in Table 4 will be focused on in what follows.[275]

The tables show that the best correlation between interest rates and income, of –0.698, is between nominal GDP growth and the real interest rate's level two quarters earlier. But other interest-rate/GDP-growth correlations at this lag tend to be strong, in the cases of nominal income and real income alike. So Wallich's contention that market rates performed well as an

[275] A positive relationship between nominal interest rates and nominal income growth might be expected to characterize the longer-run connection between the two series, on account of the Fisher effect.

Table 5. Real income growth and monetary variables, 1979 to 1982

	Real GDP growth 1979:Q4–1982:Q4		
	Correlations with monetary growth		
Monetary variable	No lag	One quarter earlier	Two quarters earlier
M1 growth	0.108	0.578	0.091
M2 growth	–0.184	0.442	0.233
M1 growth, 2-quarter average	0.573	0.557	–0.269
M2 growth, 2-quarter average	0.223	0.558	–0.268
	Correlations with interest rates		
	No lag	One quarter earlier	Two quarters earlier
Nominal federal funds rate	0.377	–0.212	–0.527
Real federal funds rate	0.188	–0.168	–0.506
Change in nominal funds rate	0.562	0.307	–0.602
Change in real funds rate	0.487	0.494	–0.462

Source: Federal Reserve Bank of St. Louis' FRED portal. Growth rates are calculated as percentage changes in quarterly nominal GDP and in quarterly averages of M1 and M2. Interest rates calculated as quarterly averages. Changes in interest rates are calculated as first differences of these averages. Real rates are obtained by subtracting four-quarter CPI inflation rate.

indicator in this period was well founded.[276] It is not, however, the case that monetary growth was a poor indicator in the same period or that it showed no leading-indicator relationship with income. The contemporaneous correlation between M1 growth and real income growth is good at 0.573, and an average lag of 0.5 quarters delivers a correlation of about 0.56 for both M1 and M2. And with regard to nominal income growth, the correlations (about 0.56 for M1, about 0.45 for M2) are best when money is ahead of income by one quarter or 1.5 quarters.

The significant correlations in the tables partly reflect the fact that interest rates and monetary

[276] It is also evident that it is straightforward to find an empirical delay between interest-rate movements and output fluctuations in the 1979–1982 period—in contrast to Nakamura and Steinsson's (2018, p. 73) suggestion that the series' relationship in this period was simply contemporaneous.

growth both accurately predicted an event that was beyond the period Wallich (1981b) considered: the recession of 1981−1982. As discussed above, this event started in or after July 1981 and followed a span of months in 1981 during which monetary growth had been low and interest rates had remained high. Interest rates had, in fact, been rising for a year before the recession began, and in mid-1981 there were news articles speculating prematurely that the economy had become resistant to high interest rates, with forecaster Michael Evans being quoted as saying: "The economy is doing an excellent job of acting as though high rates don't exist." (*New York Times*, May 25, 1981, p. D1.)

From Friedman's perspective, the phenomenon of high interest rates during the 1980−1981 expansion was less of a puzzle, as he drew a distinction between interest-rate increases accompanied by slow monetary growth and those occurring in conditions of rapid expansion in money. The interest-rate rise in the second half of 1980 fell into the latter category and so, to Friedman, it reflected not a tightening of the monetary policy stance but the existence of strong economic momentum. As already indicated, in the first half of 1981 a more clear-cut picture of tight money emerged: high interest rates continuing while, about three months before the 1981−1982 recession began, monetary growth moved into a decided decline. This development, too, gave rise to press conjecture that "Milton Friedman may have to rethink his theory" because that theory implied "we should soon be entering a recession" yet "many forecasters say that no recession will take place this year." (*New York* magazine, April 27, 1981, p. 20.) In the event, of course, the monetary indicators, including monetary growth, over 1981 gave an accurate indication of forthcoming economic developments.[277]

Reserves versus interest rates as instruments

In retrospect, therefore, it would appear that Friedman and monetarism came out well with regard to the specific empirical challenge laid out by Wallich in 1981 on money's capacity, *vis a vis* interest rates, to predict future economic activity in the New Operating Procedures, or NOP, era.[278]

[277] This is not to say that Friedman was wholly vindicated. The *New York* article presumed that he was emphasizing M1 in this period, but at the time, he was discounting M1 and stressing the behavior of M2, whose slowdown in 1981 was less dramatic than that of M2 (see Table 3).

[278] At the time, one of the individuals pressing the significance of quarter-to-quarter movements in money was Beryl Sprinkel. Wallich felt that, in a series of discussions with Wallich in the leadup to the May 1981 FOMC meeting Sprinkel, had been harping on money supply stability and the Federal Reserve's responsibility for instability in monetary growth. See Meltzer (2009b, p. 1092).

Beyond predictive power, however, some more general themes that Wallich raised over this period in opposition to the monetarists during the early 1980s would prove a greater challenge to them. In particular, notwithstanding the introduction of the NOP, there remained considerable opposition to the position that Friedman restated in 1981 as: "The government shouldn't do a thing about limiting interest rates." (*The Daily Oklahoman* (Oklahoma City), May 19, 1981.) Not only would concern with the behavior of interest rates prove durable among policymakers, but, also, economic thinking outside central banks offered new support for this concern—and, in particular, the usage of interest rates as a policy instrument.

In his Western Economic Association talk in San Francisco in early July, Friedman portrayed the choice of policy instrument as being a settled matter in terms of economic analysis. He suggested that there was "wide agreement" that "the [monetary] base, or components of the base" should serve as the policy instrument.[279] But he underestimated the strength of the opposing view. It became clear that many remained unconvinced by his position that central banks should manage interest rates was a misguided notion that had no place in the 1980s.

For his part, with regard to the issues of operating procedures, Henry Wallich continued to give clear indications of his doubts about the monetarist case for a reserves-type instrument. Just before the October 1979 change in operating procedures, an article coauthored by Wallich appeared (in the *Federal Reserve Bulletin*) that "no single formula or operating target can be relied on to work effectively in all circumstances."[280] Indeed, although he supported the change in operating procedures in 1979, Wallich continued to defend the notion of the short-term interest rate as a monetary policy instrument in way that put him at odds with monetarists and that made clear that he did not regard the choice of instrument as having been decisively settled. As already indicated, he regarded the interest rates associated with reserve settings as significant in signaling policy. He believed that this should figure in policy decisions explicitly, arguing: "Evaluation of the thrust of a policy should always include examination of the interest rates it produces, even though these interest rates should not be the primary target."[281]

Wallich was not alone among Federal Reserve policymakers on this score. In the fall of 1980, Stephen Axilrod of the Federal Reserve Board's senior staff disclosed with regard to FOMC meetings: "My principals [the policymakers] want to know: 'If the money supply does this, what is going to happen to interest rates?'" (*Wall Street Journal*, November 11, 1980, p. 19.)

[279] Friedman (1982c, p. 101).
[280] Wallich and Keir (1979, p. 690).
[281] Wallich (1982a, p. 33).

In the research world, too, developments around this time were taking place that tended to challenge at least the more visceral monetarist arguments against an interest-rate-based monetary policy approach. Analytical results in research papers by Parkin (1978) and McCallum (1981) formalized the notion that a nominal interest-rate feedback rule that responded appreciably to nominal variables could provide a model solution for the price level (and its rate of change).[282] Friedman and other monetarists had already acknowledged this point verbally (see Nelson, 2020a, Chapter 8). But it had not been established formally: even James Tobin, despite his support for an interest-rate management as a practical policy, had typically treated central banks as operating on reserve quantities or the monetary base in his formal analysis. Furthermore, analytical results produced by Sargent and Wallace (1975) that cast doubt on rules centered on interest-rate reaction functions had been widely noted—a situation that likely led the 1981 John Taylor study, mentioned earlier, to imply that "explicit interest-rate rules" had gone off economists' agenda (Taylor, 1981, p. 146). The fact that interest-rate rules were now being established as capable of delivering stable and determinate outcomes for nominal variables offered the prospect that the matter of choosing between these rules and monetary-growth rules might be reconsidered.

The smokescreen interpretation revisited

Indeed, some observers have argued that the FOMC never, even in 1979–1982, actually stopped using the federal funds rate as it main policy instrument. Bernanke and Mishkin (1992, p. 193) went in this direction when they argued that "the new operating procedures and the greater (putative) attention to monetary targets were a useful smokescreen that obscured the link between the Fed's actions and the painful increases in interest rates." But numerous other, more categorical claims that the 1979–1982 policy regime was a charade, and that the FOMC was still covertly managing the federal funds rate, have been advanced. For example, as discussed in Chapter 10, the Carter Administration's Charles Schultze came to view the 1979 Federal Reserve policy change along these lines.[283]

The "smokescreen" interpretation of monetary policy over 1979–1982 has become very prevalent in economic research's interpretations of those years. One twenty-first century example in this vein was Mishkin's (2001, p. 2) statement that "the 1979 policy shift... was a

[282] It was later shown in Woodford (1999b) that, in sticky-price models, even an interest-rate rule that responded solely to an endogenous real variable (such as the output gap) might deliver determinacy of nominal variables, owing to the short-run link between nominal and real variables associated with periods of price stickiness.

[283] In addition to the Schultze statements referred to in that chapter, see his remarks in *Wall Street Journal*, June 3, 1987.

smokescreen to obscure the need of the Fed to raise interest rates to very high levels to reduce inflation."[284]

As for Milton Friedman, it was suggested in Chapter 10 above that he ultimately came against the smokescreen interpretation. This point will be elaborated on below. In order, however, to set the scene, it is worth indicating why, in considering U.S. monetary policy from 1979 to 1982, the smokescreen interpretation should *not* be the favored one.

As a preliminary matter in documenting this judgment, it is worth acknowledging that a factually undisputed aspect of the U.S. central bank procedures during the 1979–1982 period—and an element that made those procedures inimical to Friedman—was that there remained *some* Federal Reserve administration of interest rates in 1979–1982. In particular, the Federal Reserve Board continued, against Friedman's wishes, to make explicit policy decisions about the setting of the discount rate. As already indicated, these decisions received considerable publicity and were taken in much media coverage as amounting to Federal Reserve steering of market interest rates.[285] For example, in discussing the Board's increase in the discount rate to 12 percent in February 1980, Louis Rukeyser described it as an adjustment to the "country's most basic interest rate."[286]

In addition, of course—and again to Friedman's disapproval—over this period the FOMC specified tolerance bands for the federal funds rate tolerance alongside its reserves targets. The interpretation, however, varied among FOMC members about whether these rate bands had the status of targets. Reflecting his interest-rate-focused perspective, Governor Wallich (1980b, p. 9) indicated in June 1980: "In setting its short-run money supply targets, at monthly FOMC meetings, the Federal Reserve had established upper and lower limits for the federal funds rate (interbank rate)." In contrast, Volcker insisted that "nonborrowed reserves are our proximate policy tool."[287] He characterized the ranges merely as an expectation of the range of fluctuation in rates likely to accompany the planned nonborrowed-reserve supply: "we don't have any real

[284] Earlier, Anna Schwartz was associated with the same interpretation of the 1979–1982 monetary arrangements. A 1984 *Wall Street Journal* article attributed to her the view that in 1979–1982, "the Fed embraced monetarist principles as a smokescreen for raising interest rates and reducing inflation." (*Wall Street Journal*, December 10, 1984, p.16.) See also Batini and Nelson (2005) for a discussion of the "smokescreen" argument in the context of U.K. monetary policy.
[285] In the press, one instance of this was *Kansas City Time*s (Missouri), May 29, 1980,
[286] *Wall Street Week*, Maryland Public Television, February 8, 1980, p. 9 of transcript.
[287] Testimony of February 25, 1981, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1981a, p. 27).

targets for interest rates," Volcker remarked in July 1981.[288]

Volcker was resolute in contending that the NOP was not a charade. And his position that the nonborrowed-reserves regime was *bona fide* has been bolstered by studies of FOMC transcripts for the 1979–1982 period.[289] The transcripts have provided evidence that the Committee (many of whose members had little inkling that their remarks would eventually be made public) genuinely made policy decisions in terms of quantities and was not simply targeting the federal funds rate by stealth. Certainly, the 1980 credit-controls period was a time when, as discussed earlier, the FOMC allowed a concern with stabilization of the federal funds rate to override its usual manner of setting reserve levels. And, as also indicated, Governor Wallich voiced a wish to put more weight on the federal funds rate. But the FOMC as a whole for most of the period from October 1979 through summer 1982 was oriented on nonborrowed reserves.

The smokescreen interpretation of the 1979–1982 period was also largely eschewed in the historical account of Meltzer (2009b), although Meltzer entered the caveat (p. 1100) that there was some evidence in the second half of 1981 of an underlying FOMC policy of stabilizing the federal funds rate. But the plausibility of the suggestion that the FOMC went back to interest-rate management after mid-1981 is crucially undermined when one considers that short-term interest rates underwent an unwelcome rise in 1982 that an interest-rate-managing approach would have prevented from happening.

The Meltzer is also inconsistent with what was said in key internal Federal Reserve deliberations in late 1981. A notable item here was Governor Wallich's remark at the November 17 FOMC meeting that the FOMC should not expose ourselves to the charge that we're managing interest rates[,] when [in fact] we're not doing that."[290]

Avoiding interest-rate decisions

The smokescreen interpretation should be distinguished strongly from a related but different interpretation: that the move to the NOP *facilitated* the achievement of the interest-rate adjustment necessary for monetary control and disinflation, and that it did so not just on technical but also on political grounds. In particular, with respect to the political grounds, the

[288] From Volcker's testimony of July 21, 1981, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1981c, p. 251).
[289] See, for example, Lindsey, Orphanides, and Rasche (2005) and Thornton (2006).
[290] In Federal Open Market Committee (1981b, p. 54).

NOP reduced the need for policymakers to take a stand on the required degree of interest-rate adjustment, and it created something of an arm's length relationship between policymakers and short-term interest rates.[291] A member of the Carter Administration, Stuart Eizenstat, saw the Federal Reserve's adoption of the NOP in such terms: "It was a way to deflect public attention from interest rates." (*New York Times*, June 15, 1983, p. D6.)

It is highly plausible that Volcker adopted a *bona fide* change to quantity-based operating procedure but also saw the attraction in separating himself from overt interest-rate decisions. Indeed, just a few weeks prior to the NOP introduction, Volcker had implied that interest rates needed to move higher but also indicated his discomfort with being held responsible for interest rates: "I don't like these interest rates any more than you do, I'm sure… But it's all we can do with [respect to] inflation."[292]

Lyle Gramley's assessment was: "Well, that's really a political question, and I think Paul always felt that there would be no way that the Fed could 'take credit' for raising interest rates as high as they had to go to break the back of inflation, that there had to be, in effect, a different course, a different *modus operandi* in policy—one that focused on the money stock and let interest rates be set by the market. He thought that, politically, this is the only way it could happen." (Lyle Gramley, interview, June 24, 2013.)

And Wallich himself said that the arrangement "helped to shield the central bank against the criticism that would be associated with a policy of setting interest rates high enough to curb inflation."[293]

Once in the NOP regime, Volcker could accurately, but not altogether forthrightly, state: "We are not deliberately trying to push up interest rates. What we are trying to do is to restrain the growth of credit, restrain the growth of money, because we think, ultimately, that is fundamental to inflation."[294]

Volcker, in fact, made numerous statements during his tenure that could be construed as

[291] This line of reasoning was also discussed in Chapter 10.
[292] From Volcker's testimony of September 26, 1979, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1979c, p. 33).
[293] *Wall Street Journal*, December 10, 1984, p. 16. (The article stated that Wallich was referring to monetary targeting, but the remark seemed to be in the context of a discussion of the NOP, or perhaps of post-1979 U.S. monetary policy *in toto*.)
[294] From Volcker's testimony of March 20, 1980, in Joint Economic Committee, U.S. Congress (1980a, p. 111).

implying that the FOMC did not, and perhaps could not, control interest rates. That said, even during 1979−1982 he did not deny, when pressed, that Federal Reserve actions importantly affected short-term interest rates and that its monetary restriction was making interest rates in the short run higher than they otherwise would be.[295] He also indicated that he believed that higher (real) interest rates lowered private-sector spending and economic activity.[296]

It should also be stressed that many of Volcker's statements about the general endogeneity of interest rates went beyond the issue of choice of instrument and relied on well-founded economic arguments about the medium- and longer-term determination of interest rates. Throughout his years at the Federal Reserve, Volcker made innumerable references to the Fisher relationship between nominal interest rates and inflation. This relationship was, of course, something that Friedman had pressed on the Federal Reserve for many years, including in the 1960s when policymakers did not stress it. And Friedman reemphasized the point at the start of the NOP period: "If the Fed stays with its policy, if it brings rates of monetary growth down, then the interest rates will come down." (*San Francisco Examiner*, October 18, 1979.)

In his accounts of interest-rate behavior in his various public remarks during the NOP years, Volcker also stressed other aspects of interest-rate behavior that Friedman and other monetarists had articulated—including the notion that interest rates should tend to decline in recessions and that the liquidity effect of monetary expansion on interest rates was transitory.[297] So, in

[295] For example, writing on April 7, 1980, Volcker stated (Joint Economic Committee, U.S, Congress, 1980a, p. 130) that "the dominant element in the general rise of nominal interest rates in the industrialized nations over the past year has been the need all countries have felt to come to grips with inflationary pressures by exercising monetary restraint." Volcker also testified on April 2, 1980 (in Committee on Finance, U.S. Senate, 1980, p. 10): "We can, with[in] a certain range, have a strong influence on interest rates." And, in testimony of October 29, 1981, Volcker remarked of market interest rates that "if we influence anything in the short run, we influence short-term rates." (In Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1981c, p. 553.) The Federal Reserve Board's official public economic reports were also occasionally forthcoming on the matter. For example, a February 1981 publication referred to the fact that upward pressure on short-term interest rates had been "reinforced by reductions in the path for nonborrowed reserves and by increases in the discount rate and imposition of surcharges on frequent [discount] borrowing." (Federal Reserve Board, 1981c, p. 203.)

[296] For example, on July 22, 1981, Volcker testified that "high interest rates have some restraining influence" (Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1981b, p. 87). See also his comment (quoted in *Wall Street Journal*, July 22, 1981) that interest rates were "high enough to have a restricting effect on the economy," as well as numerous Volcker remarks made during 1980 on the interest-rate/output relationship.

[297] On pre-1979 Volcker remarks to this effect, see Chapter 10 above. Volcker reiterated these themes on many occasions from 1979 to 1982. With regard to the transitory nature of the liquidity effect, see, for example, his testimony of April 2, 1980, in Committee on Finance, U.S. Senate (1980, p. 8). His indications that the cyclicality of interest rates was consistent with an unchanged monetary-growth strategy included a number of the statements considered above and in Nelson (2021), as well as Volcker's articulation of his expectation with regard to the NOP: "Interest rates can and will respond to credit demands, to economic conditions, and, over time, to inflationary expectations without any change in the basic thrust of our monetary policy." (*Kansas City Star* (Missouri), January 3, 1980.)

emphasizing factors influencing interest rates other than the impact effect of a monetary policy action, Volcker was not dissembling. He was, instead, relying on established economic analysis, including monetarist work.

It was the endogenous drivers of interest rates that underlay Friedman's longstanding position (see Nelson, 2020b, Chapter 6, and Chapter 5 above) that, except via the Fisher effect, central banks could not really control interest rates.[298] Of course, neither the ultimate endogeneity of interest rates over the medium and longer runs, nor consciously making interest rates endogenous in the short run (by eschewing them as a policy instrument) meant that the central bank's behavior had no repercussions for interest rates. On the contrary, such repercussions were bound to occur in both the short run and the long run—a point that was acknowledged by Friedman in 1980 when, under a base-control operating regime, the course selected for the monetary base would influence the course of interest rates.[299] And with respect to how interest rates behaved during the new regime, Friedman would note (*Newsweek*, June 28, 1982) a "dramatic change" since 1980 in the relationship between short-term nominal interest rates and inflation, putting to an end a stretch of years in which interest rates had "consistently been lower than the inflation rate."

Indeed, the fact that Federal Reserve operations had a systematic effect on the federal funds rate during the nonborrowed-reserves regime was used by Bernanke and Blinder (1992), Rotemberg and Woodford (1997), and Taylor (1999a) to approximate the characterization of monetary policy during the NOP years by a federal funds rate-setting regime—even though these authors accepted that the nonborrowed reserves regime was a genuinely different operating procedure.[300]

In terms of his own interpretation of the NOP, during the early 1980s Friedman occasionally seemed enamored of the smokescreen interpretation. He appeared in early 1981 to believe that the Federal Reserve was really still using the federal funds rate as its instrument (see *Wall Street Journal*, January 30, 1981).[301] Within months, however, he had accepted that this was not the

[298] In a related vein, Karl Brunner (in his written statement of December 4, 1979, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1980a, p. 114) had suggested that central-bank management of interest rates was distracting because interest rates ultimately depended on the state of the economy: "an interest target policy tends to misdirect public attention. Short-run changes in interest rates tend to be attributed to the policymakers even when they dominantly reflect current market forces."
[299] See Friedman (1980b, paragraph 11, p. 57; p. 53 of 1991 reprint). See also Nelson (2020a, Chapter 8) for further discussion.
[300] This work is discussed further in Chapter 18 below.
[301] Similarly, in *Newsweek*, February 23, 1981, Friedman referred to "the Federal Reserve's unfortunate propensity to operate through interest rates," and he did not identify this behavior solely with the past.

case, and in a *Newsweek* column of August 31, 1981, he granted (as he had in the previous month's public lecture) the October 1979 change in operating procedures as *bona fide* and—that premise having been granted—urged further reforms of Federal Reserve monetary-control techniques.[302]

In September-October 1981, Friedman was involved in several discussions in which he expressed puzzlement at why interest rates (including short-term rates) were so high, and in which he clearly treated current rate levels, and recent movements in rates, as reflecting market forces rather than deliberate FOMC choices.[303] His process of trying to interpret interest rates in terms of the market forces driving them continued in 1982.[304] In sum, Friedman after early 1981 essentially dropped the smokescreen interpretation of the 1979–1982 regime.

Money's important role in policymaking

For all the dispute among observers about the character of the 1979 change, there would emerge considerable agreement that, from October 1979 and through mid-1982, the Federal Reserve devoted more effort than before to hitting its M1 intermediate target. As a Congressional study put it in 1985: "While some monetarists have questioned the FOMC's commitment to M1 targeting, the FOMC did place more emphasis on controlling the growth of M1 than it did prior to 1979."[305]

During the late-1980 period when an M1 target overshoot for the year had become inevitable, Volcker had affirmed that "the Federal Reserve, for its part, intends to maintain the restraint on money and credit growth needed to wind down inflation.[306] A year later, he was in a position to

[302] Friedman's numerous analyses appearing during the mid-1980s accepted that 1979 had indeed witnessed a change in Federal Reserve operating procedures. Later, in Taylor (2001, p. 107), Friedman remarked on the 1979–1982 period in a way that could be regarded as endorsing the "smokescreen" interpretation (as he characterized Volcker as using a "shield"). But that observation could also simply be regarded as making the point—noted above as being distinct, and as not taking the NOP to be a smokescreen—that the regime made major interest-rate adjustments easier to enact.
[303] See, for example, *Newsweek*, September 21, 1981, and *New York Times*, October 11, 1981. See also the discussion above of the *Savings and Loan News* symposium, published in this period, that concerned the forces that had been driving interest rates of late.
[304] His 1982 interventions on this matter were perhaps the more significant, as by that point Friedman had accepted that monetary policy had been very tight under Volcker in 1981, and so he had a better grasp of the policy stance the FOMC had taken in that year.
[305] Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1985, p. 62). Subsequent studies providing conclusions or results that lined up with this statement included Feinman and Poole (1989, pp. 67–68), Karamouzis and Lombra (1989), Benjamin Friedman (1997, p. 152), and Meltzer (2009b, p. 1087).
[306] Volcker (1980a, p. 13).

indicate that such promised restraint had been delivered. Indeed, the 1981 monetary-growth outcome essentially implied that the 1980 overshoot had been wound back, and even Friedman would acknowledge that, on average from October 1979 through the end of 1981, the Federal Reserve had met its M1 targets.[307]

Henry Wallich had served on the FOMC throughout this period and been a leading proponent of restraint. Furthermore, despite his doubts about monetary aggregates as an economic indicator, he had continued to see merit in the monetarist emphasis on the centrality of monetary policy to inflation. Even during the period from 1982 onward, when there was enhanced disaffection with monetary targets inside and outside policymaking, Wallich pointed to the benefits of a policymaker focus on money. In 1983, for example, he was quoted as saying: "Money supply targets have proved to be more visibly related to the rate of inflation and less susceptible to gross error."[308] And in 1984, while still very critical of monetarists, Wallich would remark of the monetarists that "they basically have a good idea."[309]

Wallich's dialogue with Friedman might have moved into FOMC meetings if press speculation in May 1981 had proved accurate. The prospect that Friedman might join Wallich and Volcker on the Federal Reserve's Board of Governors was raised in that month, when the media reported the fact that Friedman had been subject to an FBI investigation for the purposes of obtaining U.S. government security clearance. The media suggested that this development might be a sign that President Reagan was poised to nominate Friedman to become a Board governor. In fact, the investigation was being carried out merely to allow Friedman's admission into Reagan's occasional economic-advisory group. In any event, Friedman told the media, he would likely turn down a Board nomination offer, because it would imply another move of location: "I prefer San Francisco. I hope the problem [of being offered a nomination] never arises, [and] I do not have any expectations [that it will]." (*Aberdeen American News* (South Dakota), May 22, 1981.) The only Board appointment that he would accept, he had earlier made clear, would be to become Federal Reserve chair. "It's the only job [offer] I'd have to think deeply about. If I said 'no,' then I'd have to shut up about monetary policy. That would be a very high price to pay." (*Boston Globe,* April 1, 1981, p. 39.)

[307] For further discussion, see the next chapter.

[308] *Wall Street Journal*, June 20, 1983, p. 19. Wallich's skeptical attitude toward Friedman continued, however, and on two occasions from late 1983 onward he received briefings, arranged by Stephen Axilrod and provided by Neil Ericsson, on the Hendry and Ericsson (1983) critique of *Monetary Trends* and on follow-up work by Hendry and Ericsson.

[309] *Wall Street Journal*, December 10, 1984, p. 16. Wallich added that monetarists "basically carry it to an extreme—where it is unworkable."

It therefore transpired that Friedman's and Wallich's careers in monetary analysis had involved multiple common experiences—each of them been economic researchers, policy advisers, and *Newsweek* columnists—but, of the two of them, only Wallich served as a policymaker. Reflecting this fact, following the publication of some of Wallich's statements in the 1982 collection *Monetary Policy and Practice: A View from the Federal Reserve Board*, Friedman added the collection to his list of recommended reading, as a representation of central bankers' views, in his 1986 revised *Encyclopaedia Britannica* entry on money.[310]

HENRY KAUFMAN

In the April 1981 speech, discussed above, in which Governor Wallich argued for "interest rates as the principal measure of monetary ease and tightness," he cited the content of Wall Street financial-market newsletters as evidence of the fact that, among many professionals who regularly commented on monetary policy stance, the monetary aggregates continued to be given less weight than interest rates. In particular, Wallich noted that, on the basis of reading of "the press and particularly the never-ending flood of bank, investment-house, and other letters," it was clear to him that "the interest-rate school by far predominates."[311]

Among the producers of this "flood" of financial market commentary, the most eminent of them by the early 1980s was Henry Kaufman—a partner in, and the chief economist of, the Salomon Brothers investment bank/bond dealer. There had been a time in the late 1960s when, of all the remarks on the U.S. economic outlook made by people not in government, those by Milton Friedman seemed to be the most prone to generate a reaction in financial markets.[312] But that time had long passed, and by 1980–1981—notwithstanding the further progress that Friedman had made over the intervening years in acquiring influence on economic policy and general U.S.

[310] Friedman's entry (1986c, p. 329), in referring to Wallich (1982b), incorrectly suggested that Wallich had already left the Federal Reserve Board, but at the time of the appearance of Friedman (1986c), Wallich was still on the Board. Wallich, who would pass away in 1988, had been in poor health for well over a year before his resignation from the Board in December 1986. His stretch of poor health may have been Friedman's basis for mistakenly believing that Wallich had already retired.

During his years as an active policymaker, Wallich, a longtime and heavy user of tobacco, had routinely smoked in Federal Reserve policy meetings. This proved no problem under the leadership of Arthur Burns (a pipe smoker) and Volcker (a cigar smoker until 1987), but it had encountered resistance during the tenure as chair of G. William Miller, who attempted to institute a no-smoking policy. Lyle Gramley recalled (interview, June 24, 2013): "Bill Miller just strongly felt that no smoking should take place in the Board Room [in which both Board and FOMC meetings were held in that era]. And they were in the middle of a Board meeting one day, and Henry pulled out his cigar—or his pipe, I'm not sure which one—and Bill Miller said to him, 'Governor Wallich, would you smoke in church?' So Wallich said, "This isn't church.'"

[311] Wallich (1981b, p. 10; p. 90 of 1982 printing).

[312] See Nelson (2020b, Chapter 15).

economic thinking—others dominated him in the capacity to move markets.

Kaufman was the one who had most left Friedman in the dust. By 1980, the influence on financial pricing that not only Friedman but also many other economic commentators could generate was far outweighed by what Kaufman was capable of. The scale and frequency of Kaufman's market impact during this period were captured by the *Washington Post* (December 7, 1980, p. G1): "Pronouncements by Henry Kaufman, a financial expert, regularly send the stock and bond markets into spasms."

By this juncture, Kaufman's leading status among Wall Street analysts was very well established indeed. Quite early in the preceding decade, Friedman himself had already noted (Instructional Dynamics Economics Cassette Tape 106, August 24, 1972) that a "very respected commentator is Henry Kaufman, of Salomon Brothers, who puts out [the newsletter] *Comments on Credit* once a week." By November 1981, when Friedman gave a briefing to Oppenheimer and Company clients in Los Angeles on the monetary and economic outlook for 1982, he found it useful to compare his own scenarios with highly publicized projections that Kaufman had recently issued and to discuss the circumstances under which Kaufman's forecast might be realized (*Los Angeles Herald-Examiner*, November 5, 1981, p. C14; Oppenheimer and Company, 1981b, p. 12). Friedman added in 1984 of Kaufman that "he's a great guru—when he speaks, everybody listens." (*Fortune,* March 19, 1984, p. 34.)

A leading "Fed watcher"

The last remark was made at a point when Friedman felt that the degree of attention given in the media and in financial circles to Kaufman's analyses had become excessive. Friedman—a regular recipient of the *Comments on Credit* newsletter—had, nevertheless, himself concluded long before that the esteem Kaufman received was well deserved. "Anything that Henry Kaufman has to say is worth listening to," Friedman remarked in 1976, while also declaring Kaufman to be a "very perceptive observer of the economic scene." (Instructional Dynamics Economics Cassette Tape 197, August 11, 1976.)

Friedman explained that Kaufman was "in charge of the whole Salomon Brothers series of statistical reports on interest rates and the like" (Instructional Dynamics Economics Cassette Tape 197, August 11, 1976). Filling out this description, a 1979 newspaper profile of Kaufman stated that he oversaw "a staff of about seventy that gathers statistical evidence of the economy's performance" (*San Francisco Sunday Examiner and Chronicle*, June 10, 1979, p. 11C). Of course, an enormous amount of the material that Kaufman's department processed pertained to financial-market data rather than national economic time series. Indeed, even by 1977 Kaufman had a small office computer providing live financial information (*Kansas City Star* (Missouri), May 20, 1977, p. 12).

In keeping with his role as an analyst for an investment bank, much of Kaufman's specific concerns involved included monitoring of debt-issuance and other corporate capital-raising operations. Inevitably, however, analysis of the macroeconomy and monetary policy figured heavily into his analysis, and there was considerable demand by Kaufman's subscribers and by media interviewers for him to provide analysis of the national economic outlook in addition to his consideration of individual firms' debt and equity offerings. Kaufman consequently emerged as a general commentator on macroeconomic and monetary policy developments, rather than being merely a specialist on conditions in specific financial markets.

Much of Kaufman's value added, as he saw it, was in combining numerical information with non-numerical material: "he spends hours absorbing or pursuing economic information—in print and [in] an endless round of telephone conversations," the 1979 profile observed (*San Francisco Sunday Examiner and Chronicle*, June 10, 1979, p. 11C).[313] Kaufman's analysis entailed the processing of dispersed anecdotal and—in a still largely pre-digital age—hardcopy information. "You see the money banks raise money to finance loans, you see the corporations raise money to pay for expansion, you see very clearly the operation of our central bank. There are numbers running through all the time, [but] you don't have to feed it into a computer—you have it all in your own memory bank," Kaufman explained (*Kansas City Star* (Missouri), May 20, 1977, p. 13). At the end of each week, after his *Comments on Credit* was written and printed, it would be mailed by Salomon Brothers to subscribers (*Fort Lauderdale News* (Florida), October 31, 1981).

With much of Kaufman's stemming from his provision of a paid subscription service, he

[313] On this score, Kaufman differed from Friedman, whose preference for working at his home office during his California years reflected a belief that he could only get sustained work done when the likelihood of protracted phone interruptions was controlled (Gloria Valentine, personal communication, January 15, 1992, and interview, April 1, 2013)—although, in the event, he gave many telephone interviews. Similarly, near the end of the Friedmans' years living in Chicago, Rose Friedman observed: "He says home most mornings—presumably, to work. But there are always telephone calls, and instead of 'answering quick,' like I do, he starts talking [at length]." (*Sunday Sun-Times* (Chicago), October 31, 1976.)

Kaufman's office routine had closer parallels to that of Paul Samuelson, who once referred to "my telephone calls of last week" to keep up to date on current economic discussion (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 17, January 1969). In addition, Robert Solow noted that, when Samuelson wrote on economic developments in a European economy, "I suspect it was mostly [information obtained] from conversation. And when he got an interest in something like that, he would call up a friend and talk for half an hour on the phone." (Robert Solow, interview, April 3, 2015.)

rationed his public appearances. Indeed, at the end of the 1970s, he was characterized as being "virtually unknown to the general public" (*San Francisco Sunday Examiner and Chronicle*, June 10, 1979, p. 11C). Kaufman had a lower media profile than Friedman—especially outside business-news outlets—and made fewer television appearances and speeches. Furthermore, although his speeches attracted wide attention, his approach to speeches differed from Friedman's. By 1980, Friedman was accustomed to using the lecture circuit frequently as a means (a remunerative one for him) of repeating established themes. Kaufman's speeches, in contrast, had more the status of major occasions on which he aired evolutions in his thinking. "I write my speeches out completely. The process isolates me and forces me to reflect. I find it a thorough, rigorous discipline." (*San Francisco Sunday Examiner and Chronicle*, June 10, 1979, p. 11C.)

Kaufman's description of his speechwriting process also pointed up a difference in the way he saw himself and the conception Friedman had of someone in Kaufman's position. "Henry Kaufman is an extremely able and intelligent man," Friedman remarked (Instructional Dynamics Economics Cassette Tape 126, August 2, 1973). "There is probably no person in the country who has a more intimate familiarity and expertise with bond markets than he has—with credit markets, in the day-to-day, operating sense. I'm not speaking [of him] as [a producer of formal research]. He is not an academic economist, [and] he has made no theoretical contributions or historical studies of that kind." In contrast to this evaluation, and despite the hothouse atmosphere of Wall Street, it was said that Salomon Brothers "thinks of Kaufman as its academic in residence" (*San Francisco Sunday Examiner and Chronicle*, June 10, 1979, p. 11C)—an image reinforced by Kaufman's characterization of himself as "going through my own analytical process" (*Kansas City Star* (Missouri), May 20, 1977, p. 13).

Kaufman would also have been unlikely to have seen his own interests as quite as parochial as Friedman characterized them to be. After granting of Kaufman that "in terms of an understanding of the day-to-day operations of the market, it would be hard to find anybody who is his superior," Friedman emphasized the distinction between being interested in the financial markets and being concerned more directly with monetary matters: "Mr. Kaufman is with Salomon Brothers, which is a major factor in the credit markets, and what he's really concerned with is credit and interest-rate developments. And he's interested in the monetary aggregates only [insofar] as they give him some line to [understanding] credit developments." (Instructional Dynamics Economics Cassette Tape 126, August 2, 1973).

Kaufman would cast doubt on the "only" part of this characterization when, as detailed below, he took a public stand in advancing his own monetary policy proposals—in so doing directly challenging money-focused policies of the kind Friedman favored.

Even aside from his normative proposals, much of Kaufman's newsletter analysis concerned topics prominent in Friedman's economic commentaries. A key reason for this was straightforward: watching and understanding monetary policy was such an important part of his job. In January 1976, Friedman noted the information one could receive "[i]f one reads the *Federal Reserve Bulletin*… as a small number of us religiously do."[314] But such monitoring of Federal Reserve publications was becoming widespread by the early 1980s. Furthermore, the *Federal Reserve Bulletin* was only one of many Federal Reserve public documents providing policymakers' positions, and financial institutions became more focused on gleaning information from this material. In a period described by Goodfriend (1986, p. 63) as seeing the U.S. financial world engage in the "widespread use of 'Fed watchers,'" Kaufman was at the cutting edge of "Fed watching," though it was far from the be-all and end-all of his job.

A good deal of Kaufman's reputation, encapsulated as "one of Wall Street's foremost authority on interest rates" (*Kansas City Star* (Missouri), May 20, 1977, p. 12), stemmed from his analysis of current and prospective FOMC policy concerning the setting of the federal funds rate. In this analysis, although he granted "market forces" as an influence on U.S. interest rates generally (*Kansas City Star* (Missouri), May 20, 1977, p. 13), Kaufman recognized the reality that, through 1979, the Federal Reserve dominated the behavior of short-term interest rates through conscious decisions regarding the level of the federal funds rate. He had maintained this perspective on FOMC behavior during the years from 1972 to the mid-1970s, during which the Burns FOMC professed to have switched from a funds-rate policy to the employment of reserves against private deposits (RPD) as its operating instrument.

When, soon after this alleged shift in operating regime occurred, Kaufman continued to describe monetary policy in terms of selected values of the federal funds rate, Friedman displayed conflicting reactions upon reading what "this very knowledgeable observer is saying to us" in his newsletters. On the one hand—wanting the regime shift to be genuine—Friedman seemed irritated that Kaufman was characterizing FOMC policy in terms of funds-rate setting. But, on the other hand, Friedman was willing to acknowledge that—as a factual matter—Kaufman might

[314] Friedman's testimony of January 22, 1976, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1976a, p. 2179).

be right that the Federal Reserve had *not* really moved to a quantity instrument and was in practice still closely managing short-term interest rates (Instructional Dynamics Economics Cassette Tape 106, August 24, 1972).

The latter reaction on Friedman's part proved to be the more appropriate one and, in retrospect, Kaufman's newsletter analysis evidently provided an early tip-off regarding a fact that Friedman accepted later: that the RPD arrangements never became a *bona fide* abandonment by the FOMC of funds-rate targeting.

Kaufman on monetary policy performance in the 1960s and 1970s

With regard to a wider issue—the implications that monetary policy actions had for the behavior of aggregate demand and inflation—Friedman remarked of Kaufman (Instructional Dynamics Economics Cassette Tape 126, August 2, 1973) that "his own views have changed drastically over the past few years." This was certainly true. Kaufman had attended the Walter Heller/Friedman debate on the relative strength of monetary policy and fiscal policy, held in November 1968 at New York University (from which Kaufman had received his Ph.D. in 1962). During the same late-1960s time frame, Kaufman went through a personal rethinking on the subject of the monetary policy versus fiscal policy: he had initially believed that the Johnson Administration surtax would have strong restrictive effects that overwhelmed the accompanying monetary easing—"I definitely erred," Kaufman would conclude (*San Francisco Sunday Examiner and Chronicle*, June 10, 1979, p. 11C). Like Henry Wallich, Kaufman would voice many disagreements with Friedman and monetarism but, also in common with Wallich, he agreed with much of monetarists' critique of the U.S. stabilization-policy record in the 1960s and 1970s.

In particular, Kaufman would conclude that monetary policy had been too loose in the 1970s—so he held the Federal Reserve in large part responsible for what he described in November 1976 Congressional testimony as "the deeply imbedded memory of the bitter experience of the past decade—of accelerating inflation and of sharply rising interest rates."[315] This included what, in a June 1975 speech, Kaufman (1975, p. 7) called "its mistake of 1971–73" of policy ease that "was the harbinger of double-digit inflation and the financial stringencies of 1974."

[315] Testimony of November 15, 1976, in Committee on Banking, Housing and Urban Affairs, U.S. House of Representatives (1976, p. 53).

A former Federal Reserve economist himself, having worked at the Federal Reserve Bank of New York from 1957 to 1961 (Europa Publications Limited, 1986, p. 818), Kaufman would note that he had seen the profile of the national central bank rose dramatically during his years in private financial markets: “When I joined Salomon Brothers in January 1962… the head of the Federal Reserve, William McChesney Martin, Jr., was hardly the household name Paul Volcker has become.”[316]

Kaufman would know all of Martin’s 1970s successors. Of the first of them, he summarized his impressions as follows: “Arthur Burns was a highly educated individual, [former] head of the Council of Economic Advisers, an expert on business cycles, and so on. But he could not bring himself to tighten money, to restrain the system on a timely basis, if at all. I knew Arthur Burns, and he had a great persona. A great style. Very impressive when you met him... Arthur had that style of smoking a pipe, puffing, listening—thinking, apparently, great thoughts. But… I think monetary policy was much too loose in the latter part of the ’60s and in the ’70s.” (Henry Kaufman, interview, October 14, 2014.)

With regard to the tenure at the Federal Reserve of Burns’ successor, G. William Miller, Kaufman noted: “Bill Miller was out of place. He was a very good industrialist. I met him a couple of times in New York when I was at Salomon Brothers, and some of us had lunch with him. He was a very nice man… But… he had no background in financial markets. [In order to head the Federal Reserve effectively,] you don’t have to be necessarily someone that’s deeply schooled on monetary policy or theory. But, at a minimum, you have to have deeper feelings about the interrelationship of financial markets. He lacked that. He lacked it completely.” (Henry Kaufman, interview, October 14, 2014.)

After Miller had been in office for close to eight months, Kaufman expressed dissatisfaction at the gradual pace of policy tightening. In one of his widely-reported speeches—this one given to the American Bankers Association convention, held in Honolulu—Kaufman remarked that “a further sharp rise in interest rates is unavoidable”—the choice facing the authorities, as he saw it, was whether to engineer the increase as part of concerted actions against inflation, or to acquiesce in the increase once inflation had added greatly to upward pressure on nominal interest rates (*Kansas City Times* (Missouri), October 26, 1978). A further eight months along, Kaufman was concerned that the requisite major tightening was not in sight: “There has always been the perception in this country that, whenever inflation got too high, the government would step in

[316] Kaufman (1986, p. 3).

and break its back," he observed. "Today, that perception is gone. Americans have lost confidence in their government. They assume inflation will continue." (*San Francisco Sunday Examiner and Chronicle,* June 10, 1979, p. 13C.)

On July 23, 1979, Kaufman elaborated on this point in Congressional testimony. He remarked that "it should not be surprising to know that inflationary expectations are, by many indicators, more deeply entrenched in the United States today than at any other time since the Second World War. Almost every cyclical low and high in the inflation rate has been above the preceding cyclical troughs and peaks. The secular rise in U.S. inflation is now perceived not only by the trained economist but also by the public in general. This dismal trend has led many Americans to conclude that only temporary relief can be expected from governmental policies."[317]

This statement was given two days before Paul Volcker's nomination as Federal Reserve Chair. Kaufman, who stated "I think it's a fine appointment" (*American Banker,* July 26, 1979), also praised Carter: "The president has appointed someone with great strength in the international monetary area, and it is to Mr. Carter's credit that he is willing to appoint someone who [at FOMC meetings] has taken a contrary view to [that of] the chairman [Miller]." (*Financial Times* (London), July 26, 1979.) Volcker was a close contemporary of Kaufman's—Volcker was born in September 1927, Kaufman in October 1927—and they had both worked nearby each other in the Wall Street area in the years since Volcker became head of the Federal Reserve Bank of New York in 1975.[318] Both at the time and in retrospect, Kaufman judged that economic policy had reached a poor state after the Burns and Miller years. "And then it was Paul Volcker's job to bring things back into line." (Henry Kaufman, interview, October 14, 2014.)

Kaufman on money and credit

In a 1973 discussion, Friedman had emphasized the degree to which Kaufman had absorbed monetarist ideas in his analysis of monetary policy: "Five years ago, he emphasized solely interest rates… [Now, he] puts great emphasis on monetary aggregates." (Instructional Dynamics Economics Cassette Tape 126, August 2, 1973.) However, another point that Friedman made concerning Kaufman's line of work at Salomon Brothers—that much of his concern was with credit—had a parallel in Kaufman's macroeconomic analysis and meant that in

[317] From the written portion of Kaufman's testimony of July 23, 1979, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1979d, p. 63).

[318] Although both were based in New York City's financial district, neither Volcker nor Kaufman was located in Wall Street proper during the 1975–1979 period. The New York reserve bank building was in Liberty Street, while Kaufman's Salomon Brothers had its offices at 1 New York Plaza (Europa Publications Limited, 1986, p. 818).

practice Kaufman was often a critic of monetarism and "the simple approach of targeting money" (Henry Kaufman, interview, October 14, 2014). He agreed with Friedman and others that the 1970s inflation reflected excessive aggregate demand. But the key quantity series on which Kaufman focused were credit aggregates, not the money supply.

In his June 1975 speech indicting Federal Reserve policy in 1971−1973, Kaufman (1975, p. 7) specifically identified the problem as having been one of a "massive credit reflation" that was accommodated by Federal Reserve policy, with the authorities having "validate[d] an explosion in the demand for bank funds" during those years of economic expansion. It was therefore commercial banks' credit or loans, rather than their deposits, on which Kaufman's analysis of inflationary developments focused. This orientation was evident in the quantity he highlighted—a "debt proxy," which (like the Federal Reserve's "bank credit proxy" of the 1960s) was composed of bank deposits but whose significance he believed lay not in its monetary properties but in its status as a reflection of what was happening to credit (*San Francisco Chronicle*, May 19, 1975). Until the early to mid-1960s, such a debt or credit proxy would not give signals very different from M2. In that earlier period, therefore, a focus on that series need not have led to evaluations of the monetary policy stance that Friedman was making. Indeed, in his November 1976 testimony, Kaufman still seemed to think that M2 and credit were in sufficient agreement that he was willing to see monetary targeting (which had just been made formal FOMC policy) continue. His stipulation was that either M2, or M3 (which in the 1970s corresponded largely to the later, 1980, official redefined M2 series) should receive "greater emphasis for policy guidance" than M1.[319] Consequently, at this stage, Kaufman's practical policy prescription was not actually much different from Friedman's, even though the basis for Kaufman's prescription was an emphasis on credit rather than money.

The reality, however, was that M2 (even in the widened definition it had starting in 1980) was an unreliable indication of the behavior of commercial bank credit, on account of the large magnitude of, and wide variations in, bank wholesale deposits (as well as in banks' nondeposit liabilities).[320] In addition, Kaufman's concept of the debt proxy was intended to cover both bank

[319] From Kaufman's testimony of November 15, 1976, in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1976, p. 54).

[320] The sum of M1 and all commercial bank time deposits was what Friedman, and some Federal Reserve sources, called M2 in the 1960s. But in light of the advent of wholesale certificates of deposits, Friedman in the late 1960s moved to a broad-money definition that excluded large time deposits, and, when, in 1971, the Federal Reserve Board created an official M2 series, it (appropriately, in Friedman's view) was defined to include only retail deposits. See Nelson (2020b, Chapter 14) for discussion. The 1980 official redefinition of M2 included some wholesale deposits thought at the time to be particularly substitutable with demand deposits, but these items were progressively removed from M2 in later redefinitions.

and nonbank credit, in order to encompass various borrowings by corporations and the U.S. government in securities markets, and this feature further marked out his series from M2.

During 1979, Kaufman stressed the importance of the distinction between his own credit-focused series and was active in advancing his series as the preferred target for monetary policy. "I continue to believe that the Fed should abandon its slippery money supply approach," he remarked in July 1979 Congressional testimony. "…The long-term target should be the 'Debt Proxy'—which is the sum of credit market instruments, deposits and currency held by private domestic nonfinancial investors."[321] This series, which he also labeled M7 (*South China Morning Post* (Hong Kong), July 25, 1979) or total credit market debt outstanding (*South China Morning Post* (Hong Kong), October 10, 1979), was far wider than any of the retail-deposits-focused monetary series, like M1 or M2, advanced by major monetarists.

Kaufman accompanied his advocacy of broad credit targeting with criticisms of analytical approaches that emphasized money. He suggested that monetarists' emphasis on monetary aggregates had been rendered out of date by financial innovations: "it's extremely difficult today to define what 'money' is." Kaufman contended that conventional monetary aggregates were obsolete because "the consumer thinks of money in terms of his credit card, and even in terms of the equity value that he has in his house and that he thinks he can monetize." (*San Francisco Sunday Examiner and Chronicle*, June 10, 1979, p. 13C.)[322] These comments indicated both a belief in the obsolescence of traditional money definitions and a view that it was, in any event, predominantly through credit channels that monetary policy had effects on the economy. Correspondingly, Kaufman judged in retrospect that "the great problem of monetary authorities has always been—and, I suspect, continues to be—that it is difficult for monetary authorities to understand the consequences of what new credit instruments will bring, new rating techniques will bring, and so on." (Henry Kaufman, interview, October 14, 2014.)

As already discussed in this chapter, monetarists maintained that, empirically, the credit channels of monetary policy were less important than those connecting money creation, various asset prices, and spending. They also questioned specific points raised by those citing the importance of credit. For example, in response to the suggestion that the existence of credit cards really challenged the case against a focus on monetary aggregates, monetarists would note that a credit-

[321] From the written portion of Kaufman's testimony of July 23, 1979, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1979d, p. 72).

[322] In a similar vein, Kaufman wrote later in the year: "Individuals exercise their credit cards as if they were disbursing money. They judge their liquidity in part by how much they monetize the equity value of their homes." (*South China Morning Post* (Hong Kong), October 10, 1979.)

card transaction was settled when a payment was made via a bank-deposit transfer, and that credit cards had more of a trend effect on velocity than a pernicious effect on the business-cycle money/income relationship.[323]

Kaufman's advocacy of credit concepts was unusual at the time, however, in its empirical concreteness. He was arguing that credit mattered more than money when the aim was the understanding of economic development. And he was buttressing this argument by advancing a specific time series that was meant to encompass the behavior of aggregate credit. Kaufman, furthermore, did so in a manner intended to displace monetary aggregates, as he was making the case that his empirical credit-focused series not only accounted for the behavior of nominal aggregate demand than money did (*San Francisco Chronicle*, May 19, 1975) but also explained the United States' inflation experience in recent years: "The real story behind our severely aggravated inflation is that the growth of credit has been proliferating…" (*South China Morning Post* (Hong Kong), October 10, 1979.)

This stand amounted to a specific empirical challenge to Friedman, who in 1965 had contrasted money and credit partly on the grounds of the concreteness of definition: "it has so far proved impossible to find a satisfactory objective quantitative measure of credit conditions that would command widespread acceptance" by policymakers, he had suggested in 1965.[324]

During the same late 1970s and early 1980s period in which Kaufman emphasized credit in his financial-market writings, Harvard University's Benjamin Friedman in academia was developing a parallel case—one he expounded in formal research—for the use of an empirical series on broad credit series as a guide to understanding U.S. aggregate income behavior and to the setting of monetary policy.

The travails that these credit aggregates subsequently experienced in the 1980s proved, however, to be great. Consequently, after the FOMC showed tentative interest in integrating them into their regular policy deliberations, the influence of the credit aggregates on policy discussions receded. This course of events, which primarily involved monetary policy developments that began in 1982, will be discussed further in the coverage of Benjamin Friedman in the next chapter.

[323] See, for example, the discussions in Jackson and McConnell (1985, p. 250), McCallum (1989a, p. 20), and Brunner and Meltzer (1993, p. 97).
[324] From Friedman's memorandum to the Federal Reserve Board of October 7, 1965, in Friedman (1968a, p. 144).

Kaufman, the New Operating Procedures, and the 1980 credit controls

Kaufman had argued for credit targeting when meeting the Federal Reserve Board in early June 1979, under Chairman Miller.[325] And a few months later, just as Chairman Paul Volcker was switching to the New Operating Procedures, Kaufman wrote publicly that "the Federal Reserve Board, by focusing… upon a very narrow statistical concept called the M1 money supply" was not targeting something "relevant to today's economic and monetary reality."[326]

Consequently, Kaufman did not view Paul Volcker's October 1979 shift as something altogether to be welcomed: it was the kind of dramatic move Kaufman favored and contained some indications of an increased Federal Reserve interest in reducing credit growth, but it primarily focused on monetary aggregates. Furthermore, when in early 1980 financial markets were reacting negatively to new high inflation data, Kaufman was a leading figure among those calling for further policy action, arguing that "we have... seen that the policy posture of the government is not to launch a frontal attack on inflation." (*San Jose Mercury* (California), February 11, 1980.) "I find nothing in the present policies of our government to suggest that any of these problems will be dealt with decisively," Kaufman remarked in a speech on February 21, in which he grouped Volcker's monetary policy alongside the Carter Administration's policies as simply "efforts to muddle along" (*American Banker*, February 28, 1980).

In this same speech, as has already been noted, Kaufman called for the introduction of consumer credit controls. This chapter's coverage of credit controls indicated that a move of this kind already had prominent proponents within the administration. But Kaufman's public interventions in early 1980—including his stress on the bond market's severe inflation concerns—were nevertheless believed to have been a key factor leading to the Carter Administration's March anti-inflation package that included invocation of the Credit Control Act (*Washington Post*, June 1, 1980, p. F1).

Once the controls had been in force for nearly three months, Kaufman recognized that they had given rise to the "sharp tailspin" of the economy and predicted that the Federal Reserve Board would shortly remove the controls altogether (*Chicago Tribune*, June 13, 1980).[327] Even before the credit-controls episode had been completed, Kaufman recognized that "history will record it as a kind of overkill," although he defended the controls' introduction, maintaining that

[325] Kaufman's (1979) accompanying memorandum would be cited by Modigliani and Papademos (1980, p. 113).
[326] *South China Morning Post* (Hong Kong), October 10, 1979.
[327] By this stage, the Board had already instituted its large-scale winding-back of controls in May.

something dramatic had indeed needed to be done in March 1980 in order “to really shake inflationary expectations” (*Washington Post*, June 1, 1980, p. F4).[328]

Kaufman did revise his view about the gravity of Volcker’s 1979 policy switch and would come to recognize it as a turning point in the effort to get U.S. inflation down. But he maintained that the emphasis on money embedded in the NOP was the wrong approach. Putting matters in the context of the 1968 Friedman/Heller debate that he had seen firsthand, Henry Kaufman remarked (interview, January 22, 2015): “in 1979, Paul Volcker more or less said, ‘Today I am a monetarist,’ by letting interest rates rise to wherever they would go, based on the provision of only a specified amount of new reserves. So, in that sense, Milton Friedman won the debate. You could also argue that Walter Heller lost the debate because we never did achieve, thereafter, a fiscal policy flexibility. But eventually, Milton Friedman lost the debate, too, because it was very difficult to define what money is.”

Friedman on M1 versus M2 in 1981

Friedman’s own public statements during 1981 would highlight the difficulties of defining money, as he would prevaricate between emphasizing M1 and emphasizing M2.

Henry Kaufman would later complain: “The fact that there are several different money supplies, often moving in different directions, is generally dismissed by monetarists as unimportant.”[329] There were, it should be noted, important occasions in the past when Friedman had acknowledged serious discrepancies between different monetary aggregates. Notably, in early 1971, when M1 growth had been slow and M2 growth rapid, he had noted: “Neither figure tells the whole truth about the money supply, and I’d like to see those rates coming closer together.” (*The Evening Sun* (Baltimore), February 19, 1971.) That particular episode of discrepancy had proved to be brief and, before too long, both aggregates were giving the same signal of extreme ease—their high growth rates registering the first monetary explosion of the 1970s (see Nelson, 2020b, Chapters 14 and 15). The story that each aggregate told regarding monetary policy in the 1970s as a whole was, also, generally the same, and, just ahead of the early-1980 redefinition of the aggregates, Friedman observed: “Personally, I have found the least unreliable indicator to be what’s called M2… But the plain fact is that all of the monetary aggregates tend to move more or

[328] Kaufman indicated that his preferred method as a permanent means of control of credit growth consisted not of direct controls but instead of active policymaker use of a minimum capital ratio imposed on financial institutions (*Globe and Mail* (Toronto, Ontario, Canada), September 12, 1981). He had earlier mistakenly thought that the Federal Reserve would use such a tool actively in order to limit growth in bank credit (see Kaufman, 1975, p. 7).

[329] Kaufman (1986, p. 27).

less together."[330]

The 1980 redefinitions of the monetary aggregates largely helped Friedman's case. After this redefinition, it was true to state of the new aggregates at the start of the 1980s, as it had been of the old aggregates in the mid-1970s, that the velocities of the two series had different trends (about a 3.5 percent per year increase in the case of M1 velocity, and little increase at all in M2 velocity) but that their growth rates moved together on a quarterly basis. In his July 1981 Money, Credit, and Banking lecture in San Francisco, Friedman ventured to declare: "In general, the various *M*s show similar movements but at times there have been sharp differences, primarily as a result of 'disintermediation' or 'reintermediation' produced by the varying impact of Regulation Q."[331] This was not a bad generalization regarding past patterns, but it would not characterize behavior in the 1980s well. During that decade, Regulation Q's ceilings on time deposits in M2 would be phased out—yet M1 and M2 would frequently give different signals. In the wake of that experience, Friedman would acknowledge in July 1991 that "the various totals do behave quite differently."[332]

In his July 1981 lecture, Friedman indicated that his own preference continued to be for targeting M2.[333] In favoring M2 over M2, he continued to be in the minority. As already indicated, the FOMC oriented its nonborrowed reserves instrument primarily with a view to achieving the target for M1 (or "M1B," as the new definition of M1 was often called during 1981—in order to distinguish it from the old M1 series, M1A, which was being phased out). The M1 series was also the aggregate emphasized in most financial-market and media commentary on money, as well as the series used as money in much formal economic research.

In the first eighteen months or so of the new operating procedures, the M1/M2 distinction did not figure very heavily: the two aggregates told essentially the same story—with the important exception, already noted, that M1 underwent a much more drastic contraction during the period of credit controls. Notwithstanding his own preference for M2, Friedman was happy to endorse M1 targeting (*Fortune*, October 6, 1980, p. 45). He reiterated the position that a single monetary target, including for M1, was preferable to the Federal Reserve's practice of announcing targets for both M1 and M2 (*Wall Street Journal*, January 30, 1981.)[334] He also maintained in May 1981

[330] *Wall Street Week*, Maryland Public Television, January 4, 1980, p. 11 of transcript.
[331] Friedman (1982a, p. 108).
[332] Milton Friedman, letter to the author, July 16, 1991.
[333] Friedman (1982a, p. 117).
[334] As reprinted in Friedman (1983a, p. 248). The surrounding passages of this op-ed focused on M1 and took for granted that this aggregate was the principal focus of the Federal Reserve's monetary targeting.

that, as he believed had been the case under Arthur Burns, the Federal Reserve continued to announce targets regarding multiple monetary aggregates for reasons of obfuscation—as a means of avoiding accountability for deviations of any particular monetary series from its announced target (*Austin American-Statesman* (Texas), May 24, 1981).[335]

But, as greater discrepancies between M1 and M2 behavior emerged in 1981, Friedman would himself initiate a series of veering movements, alternatively favoring M1 and favoring M2. This pattern of behavior would not stop until 1986. And, as discussed in the next chapter, this behavior would see him produce a good number of poor forecasts and inconsistent statements over the first half of the 1980s.

The struggle in Friedman's assessments regarding the weight to give to each of the two main aggregates would unfold during the summer and fall of 1981, when a fresh discrepancy between M1 and M2 growth rates emerged. Friedman's initial reaction was to downgrade the importance of M1 and to emphasize M2. Indeed, in mid-1981, Friedman pointed to the behavior of M2 since 1979 as evidence that the Federal Reserve had not tightened monetary policy, on average, in recent years (*Newsweek*, June 15, 1981).[336] This was the first of several serious misjudgments that Friedman would make in his monetary policy analysis in five years to mid-1986. Many of them had to do with his not grasping the degree to which changes in the opportunity cost of holding money were affecting the various aggregates: creating discrepancies between them (as in 1981 and 1985) or producing super-rapid growth in both series (as in 1983).

As of the summer of 1981, Friedman's suggestion that monetary policy had not tightened very much was not too serious an error, as both M1 and M2 aggregates had been growing rapidly in the year to late spring. The very severe slowdown in M1 growth had only begun around May, too late to be considered in Friedman's June 1981 *Newsweek* column. But from May through the fall of 1981, M1 growth was, as already discussed, very slow. Dismissing this behavior meant neglecting a sign of a significant tightening of the monetary policy stance.

In time, therefore, Friedman would concur with the assessment that 1981 had witnessed a sharp tightening in monetary policy. Indeed, by late 1982 his position was: "From April to October 1981 monetary growth collapsed, aborted the expansion[,] and brought on the recession"

[335] Similarly, when, in his July 1981 lecture, he recounted the origins of monetary targeting in the 1970s, Friedman (1982a, p. 108) characterized the adoption of multiple targets in terms of the attraction to the central bank that "it could shift attention back and forth from one *M* to another, depending on which one put the Fed in the best light."
[336] See also his remarks in *The Daily Oklahoman* (Oklahoma City), May 19, 1981.

(*Newsweek*, December 27, 1982). But, in the first four of five months during which this monetary slowdown was in process, he largely missed the boat. What was clouding the picture was the growing discrepancy between the movements of M1 and M2. Growth rates of M1 and M2 had basically given qualitatively similar signals during 1979 and 1980 about monetary developments. But in 1981, however, M2 growth stayed in high single digits, while M1 growth fell sharply. M2 growth also slowed down, but less dramatically (see Table 3 above), so its twelve-month growth remained fairly high. Richard Davis, the longtime Federal Reserve Bank of New York monetary specialist, went so far as to say: "Broad money measures continued to grow rapidly and to exceed somewhat our targets even as the economy was moving into a fairly steep recession."[337]

Although M1 deposits were certainly being affected by the introduction of nationwide NOW accounts (checkable deposits that, unlike demand deposits at the time, bore interest, and which had previously been limited to specific parts of the country), this factor, which if anything would draw funds into M1, was hardly responsible for *low* M1 growth. This fact was borne out by the Federal Reserve's published series: it provided data on both M1 and an M1 series adjusted for the introduction of NOW accounts—and, sometimes, it seemed to be this shift-adjusted series that was the FOMC's focus during the year—and both the unadjusted and shift-adjusted series showed M1 growth falling to low rates in 1981, the shift-adjusted series especially so.[338] The behavior of M1 was accurately reflecting tight monetary policy, while M2 was not. The M2 growth pattern over 1981 was not outlandish *vis a vis* nominal income—and Gordon (1983, p. 87) would find that, statistically, income equations using M2 performed well in out-of-sample nominal GNP projections from 1979 to 1982—but it was nevertheless on the high side and did not convey the degree of slowdown actually taking place in nominal income in 1981 as well as in 1982.

Friedman maintained his preference for M2 into the fall of 1981. As justification for favoring M2, Friedman cited the fact that retail money market funds (MMFs) were in M2 but not M1, and so the narrower aggregate was understating true monetary growth. He stated in a memorandum in early September: "Narrow monetary totals clearly understate monetary growth because of institutional changes, so M2 is probably the least misleading aggregate."[339] Friedman elaborated on this shortly afterward in his column: "Institutional change, notably the explosion in money-

[337] Davis (1982, p. 69).

[338] See, however, the earlier discussion of the doubts expressed about whether the shift-adjusted series was preferable to the unadjusted series in monetary analysis.

[339] Friedman (1981g, p. 3).

market mutual funds, has rendered narrow monetary aggregates misleading."[340] Indeed, he concluded that "money has not been tight by past standards." (*Newsweek*, September 21, 1981.) What Friedman inadequately accounted for was the consideration that, while there likely was merit in including money market funds in M2, their inclusion also complicated the interpretation of M2 because the attractive return on MMF assets bolstered the real demand for M2 in high-interest-rate years like 1981. Indeed, the own rate on M2 averaged 4.59 percent in 1978, 5.99 percent in 1979, 7.16 percent in 1980, and 9.07 percent in 1981.[341]

Consequently, although M2 growth was, as Friedman put it, indeed quite "flat" over 1978–81 in its four-quarter growth rates (*Newsweek*, September 21, 1981), at 8 or 9 percent, that flatness concealed a tightening in monetary policy. In M2 terms, the 1979–1981 portion of this tightening was evidently of a magnitude sufficient to maintain the sizable reduction in M2 growth achieved in 1977–1978 and to frustrate the strong upward pressure on the nominal quantity of money demanded that was coming from high rates of nominal spending growth and attractive returns on those assets included in M2. M1, in contrast, continued in the early 1980s to consist of assets that bore no interest or interest rates that were uncompetitive with market rates, both short-term and long-term.[342] It was consequently easier for M1 growth to register the sharp monetary policy tightening that occurred in 1981.

The Federal Reserve seemed to understand that, in light of the financial innovations occurring in the early 1980s, a disinflationary policy largely entailed keeping M2 growth down (rather than reducing it) while making sure that M1 growth fell. In this light, the FOMC's M2 target range for 1981, a growth rate of 6 to 9 percent, was the same as that for 1980 (see Table 1). In the event, because the M1 target was essentially met but the M2 target was somewhat overshot, the divergence of M1 growth from M2 growth in 1981 was even greater than envisioned in the setting of the FOMC's monetary targets.[343]

Indeed, Friedman's focus on M2 may have strengthened Volcker's hand during 1981. Volcker

[340] The "mutual" in this label, although not uncommon in use, was also often viewed as redundant. The MMF name that was often employed in reference to these accounts—which were excluded from M1 but included in M2—did not include "mutual," a word that was associated with institutions whose asset investments includes equity holdings. MMFs were instead involved in investment in short-term securities like Treasury bills. See, for example, R.G. Anderson and K.A. Kavajecz (1994, p. 3).

[341] Computed as averages of the monthly series on the M2 own rate, available at the Federal Reserve Bank of St. Louis' FRED portal (*https://fred.stlouisfed.org/series/M2OWN*).

[342] Because of the low return on NOW accounts, this was the case even with NOWs newly included in M1.

[343] Davis (1982, p. 69) noted that the behavior of M1, the monetary base, and the economy all pointed to tight policy, reducing the Federal Reserve's concern about possibly overshooting the M2 growth target.

remarked to a questioner at a hearing on July 21, 1981: "You point out that M1 has been running low this year, as I indicated it has. I was interested in seeing one of Milton Friedman's articles in *Newsweek* a few weeks ago [the June 15 edition] where he said, 'Don't forget, M2 is running quite high.' I was well aware of that before reading the article—but it was interesting that he was attaching importance to M2 as something you had to look at, while he has at other times focused on other aggregates."[344] By pointing to judgments that monetary policy may have not tightened very much, Volcker was likely better able to fend off the pressure to ease policy during the year. "Well, in one sense, we were on the same side," Volcker observed of Friedman—but added that, with respect to monetary policy under the NOP, "he was always very critical, right through the period." (Paul Volcker, interview, October 16, 2013.)

By late 1981, Friedman had joined the camp that money was indeed tight. In his November presentation to Oppenheimer and Company, while noting that M2 growth had not changed much since 1979, he pointed also to the slowdown in M1 and, though lamenting the variability of M1 growth from quarter to quarter, granted that, as the level of M1 had shown a tendency to revert to its implied target path since October 1979: "The problem is not one of average performance."[345] The presentation, as discussed earlier in this chapter and in more detail below, also recognized the fact of the current recession, associated it with recent slow growth in nominal aggregate demand. and cast the authorities' challenge as one of avoiding a *U*-turn toward ease in 1982. This narrative rested on an acceptance that monetary policy was indeed tight, as the M1 aggregate suggested.

Friedman consolidated his move to a considerable weight on M1 with his final column for the year (*Newsweek*, December 21, 1981). The column's analysis focused on M1 and cited its slower growth rate since October 1979, although it did also include a paragraph noting that M2 had not slowed. In increasingly accepting that monetary growth had slowed down, Friedman had essentially rendered moot his evaluations in May through September 1981 that U.S. monetary policy had not actually been tighter under Volcker.

[344] From Volcker's testimony in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1981c, p. 231). This was one occasion on which Volcker himself brought up Friedman, in contrast to the usual situation of being asked to reply by an interlocutor to a Friedman's criticism of current Federal Reserve policy. Oe of the many examples of the latter situation arose early in 1981 when Volcker, in replying to a letter from a member of the public, wrote: "I assure you that I am aware of Mr. Friedman's position on monetary policy and that we have met and corresponded on the issues involved many times." (Letter of January 23, 1981, to D. Kaplan, available at *https://fraser.stlouisfed.org/archival-collection/paul-a-volcker-papers-5297*.)

[345] From the synopsis of Friedman's remarks given in Oppenheimer and Company (1981b, p. 3). At this stage, however, Friedman still endorsed M2 as the more reliable of the two series (p. 4).

Friedman's switch from M2 to M1, which was solidified in his 1982 commentaries, made economic and monetary developments in 1981 easier to explain and would also serve him well for output and inflation predictions during 1982 and 1983. It would also receive support in retrospectives on 1981 that emphasized the course of M1. Notably, Eichenbaum (1992, p. 232) would contend: "M1 substantially undershot its range in 1981… [T]he reduction in inflation [after 1981] was due to the shock of M1 significantly undershooting its target."[346]

But if Friedman had not made this switch, or if he had paid more attention to the factors producing discrepancies between the growth rates of M1 and M2, he would probably have been able to avoid some of his worst—and most-publicized—economic forecasts in the period from 1982 to 1986. In those years, his reliance on M1 led him to the erroneous conclusion that the Volcker regime had turned into one of excessive monetary ease.

Friedman had himself acknowledged that it was "so confusing" that there were multiple monetary aggregates (*Austin American-Statesman* (Texas), May 24, 1981). The problems that he, like others, would have in trying to distill a consistent and reliable description of monetary policy developments in the 1980s on the basis of a single aggregate would take its toll on the reputation of monetarism—leading to a situation described by Henry Kaufman: "M1A, M1B—well, all that disappeared into the history books of monetary policy." (Henry Kaufman, interview, October 14, 2014.)

Interest rates and financial market participants

In addition to being critical of the focus on monetary aggregates associated with the New Operating Procedures, Kaufman had serious reservations about the accompanying change in the Federal Reserve's means of implementing monetary policy: its shift in its main operating target from short-term interest rates to nonborrowed reserves.

In an October 1980 speech, Kaufman granted that there were some arguments suggesting the "soundness of this action" of changing policy instrument. But he stressed that the move to the NOP had been associated with greatly increased interest-rate variability. The FOMC's use of a federal funds rate instrument had, Kaufman suggested, provided an "anchor of stability"—allowing market participants to identify policy decisions with movements in that rate, and providing a benchmark rate to which market interest rates—but now that anchor had been lost.

[346] Eichenbaum's reference to an undershoot was on the basis of looking at the target and outcome with regard to growth in shift-adjusted M1.

Furthermore, in practice, short-term interest rates had been highly variable under the NOP—something Kaufman attributed to the greater focus on monetary quantities: “These monetary variables gyrate widely and, as a consequence, so do interest rates.” (*The Sunday Oregonian* (Portland), October 19, 1980.) Unlike Friedman, who believed that the 1979 changes had not gone far enough in moving from interest rates to quantities in the setting of monetary policy, Kaufman’s implication a year after the changes was that they had gone too far,

These sentiments were echoed elsewhere in financial markets, and Tobin (1981b, p. 33) made mention of the fact that interest-rate gyrations observed since October 1979 were costing monetarists support among financial market participants. With the major fluctuations in interest rates observed in 1980 and again in 1981 having been widely perceived as the result of the change in operating procedures, signs of a pronounced backlash against monetarism emerged in financial analysts’ commentaries—notably a February 1981 article in the *Morgan Guaranty Survey* (the financial circular to which Friedman had contributed in past years) titled “The Pitfalls of Mechanical Monetarism” (Morgan Guaranty, 1981).

Friedman and other monetarists conceded that, on a daily and weekly frequency, reserves-oriented monetary control approaches implied sharper fluctuations in short-term interest rates than would prevail under a federal funds rate operating target.[347] But they rejected the conclusion that severe gyrations of interest rates such as those observed in the United States during 1980 and 1981 were a corollary of the employment of bank reserves as an instrument. In particular, as we have seen, Friedman regarded a major contribution to 1980’s interest-rate volatility as having come from the credit controls, while he believed that the runup in interest rates during the second half of 1980 was due to the Federal Reserve letting the post-controls rebound, in both monetary aggregates and the economy, get out of hand. In addition, as discussed in the next chapter, Friedman pointed to lagged reserve requirements and nonfinancial firms’ distress borrowing as additional contributors to adverse behavior of short-term interest rates during the 1981–1982 recession.

A particular point that Friedman stressed from late 1981 was the possibility that, beyond periods of a month or so, more stable monetary growth might be associated with *more* stable interest rates. In making this case, Friedman pointed to the fact that ups and downs in M1 growth since 1979 had seen interest-rate moves in the same direction occur in the same direction, with a brief

[347] For a concession of this kind from Friedman during the 1980–1981 period, see Friedman (1980b, p. 59, paragraph 18; p. 56 of 1991 reprint).

lag (*Newsweek*, December 21, 1981).[348]

The monetarists were nevertheless having little success in getting their interpretations accepted and, instead, the 1979 shift to a reserves instrument was perceived as the source of the interest-rate fluctuations. For their part, policymakers insisted that still greater interest-rate volatility would occur if monetary growth was stabilized further, and that this would be so both under existing control procedures and under the modified operating procedures that monetarists were calling for.[349]

The business of forecasting interest rates continued irrespective of whether policymakers set short-term interest rates. And Henry Kaufman, who had already established himself as a leader in this field during the pre-1979 period of Federal Reserve funds-rate management, continued to set the pace as an interest-rate forecaster during the 1979–1982 reserves-based policy regime. He had already been applauded for the accuracy of his predictions that interest rates would reach new highs in 1979 (*San Francisco Sunday Examiner and Chronicle*, June 10, 1979, p. 11C). "Henry Kaufman… is considered a seer about interest rates," a late 1979 news piece observed. "His accuracy might not be perfect, but his reputation seems almost to be." (*The Huntsville Times* (Alabama), December 16, 1979.) Kaufman's prediction in late 1980 that interest rates would "remain historically high" would consolidate his forecasting reputation when it was borne out during 1981.[350]

In October 1981, Kaufman made predictions of a major increase in U.S. interest rates in the second half of 1982. The key basis for the forecast was the crowding-out effect Kaufman saw as likely to result from the very large U.S. budget deficits that were on the brink of emerging.[351] The possibility of major crowding-out effects on U.S. interest rates was something Kaufman had

[348] This was a point concerning the connection between monetary growth and interest rates rather than reserves and interest rates. But, as discussed earlier, monetary growth enjoyed a significant quarter-to-quarter connection to changes in total reserves during these years.

[349] For example, Wallich (1981b, p. 4; p. 87 of 1982 printing) stated: "Tighter control of the money supply by the Federal Reserve would indeed be possible… [but] would in turn lead to wider fluctuations in interest rates…"

[350] See *Corpus Christi Caller* (Texas), December 10, 1980. A year earlier, in December 1979, Kaufman had (accurately) predicted that the prime rate would exceed 17 percent in 1980 (*Boston Herald*, December 11, 1979). During the course of 1980, Kaufman was widely hailed also for recognizing the forces making for a downturn in interest rates after the credit controls were imposed, and his revised interest-rate projections likely importantly influenced the timing of the start of that intra-year decline (*American Banker*, April 17, 1980; *Decatur Sunday Herald and Review* (Illinois), June 8, 1980). It is also notable that, in line with the retrospective analysis of the 1980 credit-controls episode given earlier in this chapter, Kaufman at the time did not regard the mid-year decline in interest rates as a signal on Paul Volcker's part that he had reduced his commitment to monetary targeting or to achieving disinflation (see *New York Times*, May 21, 1980).

[351] See *Seattle Times* (Washington state), October 13, 1981. Kaufman also raised the possibility that the deficits might give to a combination of higher interest rates and Federal Reserve monetization of the new borrowing.

stressed in the early 1970s, when he foreshadowed accurately that the fiscal austerity then being adhered to by the Nixon Administration might not last long.[352] With budget deficits now poised to expand greatly and the Federal Reserve taking the position that it would not accommodate those deficits, the picture that Kaufman painted for the Reagan years was one of "the butting of heads between a stimulative fiscal policy and a Fed policy of monetarism" (*American Banker,* October 19, 1981).

Milton Friedman had, of course, been a proponent of standard crowding-out ideas in the 1960s and the first half of the 1970s.[353] But by the late 1970s, as has been discussed in previous chapters, he had gone cool on these ideas. So when, in November 1981, he commented on Kaufman's interest-rate projections for 1982, Friedman did not dismiss the possibility of their being realized, but he disagreed with the rationale that Kaufman had offered for them. If interest rates took off in 1982, Friedman argued, it would not be because of the occurrence of large budget deficits alongside a nonaccommodative Federal Reserve. Rather, if a decided rise in interest rates occurred, it would follow a combination of fiscal and monetary expansion—a combination reflecting a *U*-turn by the authorities: an abandonment of disinflation in favor of policies designed to bring a quick end to the recession. These anti-recession moves, Friedman suggested, would before long raise interest rates by boosting inflationary expectations significantly. "If we reverse policy and the Fed causes a monetary explosion, Kaufman will be proved right," Friedman remarked (*Los Angeles Herald-Examiner,* November 5, 1981, p. C14). If, in contrast, such a 1982 *U*-turn was avoided and disinflationary policies persevered with, interest rates would fall substantially, not increase, over the year, Friedman suggested (Oppenheimer and Company, 1981b, pp. 8, 11–12).

In the event, as discussed in the next two chapters, interest rates did fall substantially in the second half of 1982. This outcome allowed Friedman to declare: "His forecasts of interest rates were terrible." (*Fortune*, March 19, 1984, p. 34.) Indeed, one commentary went further and congratulated Friedman for accurately predicting rates would decline in the second half of

[352] See *Chicago Tribune*, September 19, 1970, and *Manchester Union Leader* (New Hampshire), September 30, 1970.

[353] They were standard, that is, in the sense that they involved government borrowing raising real interest rates and putting downward pressure on real private-sector spending via that route. P.J. Miller (1983, p. 12) suggested that Friedman restated this standard argument in *Newsweek*, February 23, 1981. But that Friedman column did not actually reaffirm a belief in traditional crowding-out ideas. Instead, it articulated Friedman's revised view that the level of borrowing was less important as a variable curbing private-sector economic activity than was the total level of public spending, however financed. Consistent with this interpretation, Friedman later remarked: "Historically, there is very little relation between deficits and interest rates, but very much of a link between [government] spending and interest rates." (*Washington Times* (Washington, D.C.), May 10, 1983.)

1982.[354] But there was a false premise common to both Friedman's and Kaufman's discussions of the interest-rate outlook for 1982. Both of them presumed that the FOMC would continue to concentrate on managing a reserves aggregate and that short-term interest rates would consequently be market-determined—responding to this reserves policy as well as to private-sector forces. They would be both proved wrong on this score by the FOMC's move in 1982. Indeed, an earlier observation by Kaufman—one he made in April 1975 (see Birnbaum and Laffer, 1976, p. 238)—proved to be more applicable to 1982 than were his late-1981 forecasts. On that occasion, Kaufman had remarked that if the Federal Reserve let short-term interest rates go to 15 percent, "the focus of Congress will begin to shift from the aggregates to interest rates" and the monetary policy agenda would likewise be refocused on interest-rate management.

Within policy circles, a subtle sign of the desire to move away from a focus on quantities in monetary analysis came in November 1981 when the subtitle of the Bluebook—the policy-discussion briefing book prepared by Federal Reserve Board staff and circulated to policymakers ahead of each FOMC meeting—was changed from "Monetary Aggregates and Money Market Conditions" to "Monetary Policy Alternatives"—a change still in force 35 years later.[355]

Even as he continually voiced complaints about it, the day-to-day setting of monetary policy in the early 1980s reflected the considerable influence of Friedman's views. But the resilient tendency inside and outside the Federal Reserve for monetary policy actions to be viewed in terms of their implications for variables other than money—especially interest rates—cast doubt on whether arrangements in which the FOMC made decisions on reserves, rather than the federal funds rate, would really endure.

[354] See *The Weekend Sun* (Vancouver, British Columbia, Canada), December 31, 1982, p. A7.
[355] See Federal Reserve Board (1981d, 1981e) as well as *https://www.federalreserve.gov/monetarypolicy/fomc_historical.htm*.

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 13:
Friedman Stumbles: Debates on Monetary Policy and Macroeconomic Stabilization, 1982 to 1986

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I. EVENTS AND ACTIVITIES RELATED TO DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1982–1986

When he turned seventy years of age on July 31, 1982, Milton Friedman was well beyond the zenith of his research career. But he was at the height of his national prestige. One sign of the heights that Friedman had reached had come during the previous May, with the appearance of issues of two news weeklies—*Time* magazine and *U.S. News and World Report*—that had the same cover date (of May 10, 1982). At the same time as another of his regular columns was appearing in *Newsweek*, the other two weeklies were talking about him. The *Time* piece reported that Friedman's speaking fee for non-academic talks was $15,000, and *U.S. News and World Report* (p. 35) listed him as the twenty-second most influential U.S. citizen—ahead of First Lady Nancy Reagan, who was at the twenty-fourth spot.

Among the general public, Friedman's name recognition was considerable. This fact was underscored several months after the appearance of the aforementioned newsweekly pieces, when he was prominently mentioned in the characters' dialogue—and actually integrated into the plotline, as a speaker at a local event—in an episode, "Summer of '82," of a new network situation comedy, *Family Ties*, starring Michael J. Fox. Ahead of transmission, Friedman was sent a copy of the episode script by the production team, although he apparently did not see the episode when it was broadcast on ABC on October 27, 1982.[2]

[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 Nigel Duck, R.W. Hafer, and Lester Telser for their provision of documentary material. 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—Stephen Axilrod, Samuel Brittan, Robert Chitester, Martin Feldstein, David Lindsey, and Thomas Mayer—have passed away.

[2] Gloria Valentine (personal communication, January 7, 2009) viewed this *Family Ties* instalment "and, I might add, it was the only episode of *Family Ties* I ever watched. Professor Friedman was notified when the episode would run but I believe he was out of the country at the time. If I'm not mistaken, the writer of that episode sent Professor

Friedman's high-profile status was encapsulated in a description of him that saw print when the book in which it appeared, *Great Economists Since Keynes: An Introduction to the Lives and Works of One Hundred Great Economists*, was published in 1985, but which really pertained to how matters had stood a few years earlier. In the passage in question, Mark Blaug summarized the situation as follows: "As a result of his regular column in *Newsweek*, his TV documentary series *Free To Choose*, and the adoption by many governments around the world of his doctrine of 'monetarism,' Milton Friedman is probably the only living economist that absolutely everybody has heard of. He is, however, much more than an economic journalist and media personality. His numerous articles and books are studied by all serious students of economics and his many contributions to technical economics won him the Nobel Prize."[3]

As of 1982, therefore, Friedman was seemingly secure in the role of an eminent figure in public-policy discussions, while also maintaining some presence in the research world.

But the crash was soon right upon him. On a whole variety of fronts—his research, his influence on economic policy, his public-policy ventures as a television broadcaster and writer for the popular press, and his health—Friedman suffered setbacks in the years from 1982 to 1986. When Robert Heller introduced Friedman at a panel in mid-1984 by declaring, "He is probably the most distinguished economist alive," the praise clashed with the reality that 1984 was already emerging as one of Friedman's most ignominious years—in the talk that followed, Friedman himself would discuss a spectacular forecasting error he had recently made about the U.S.

Friedman a copy of the script of the episode. However, I don't recall Professor Friedman ever replying and I don't recall ever getting the script to file away. Whenever he was away from the office for a month, say, the pile of correspondence was obscene and got short shrift when he returned. One reason it got short shrift was that for first-class mail [received,] I wrote a short note acknowledging receipt of the letter and/or manuscript and said that he was out of the country, etc., etc. I never made a copy of my acknowledgment letters because I didn't want to add to the paper stack but [I did] put a penciled note on each letter that said 'Ack' with the date of acknowledgment."
The episode received a rerun in December 1982. Friedman also received a brief mention in the episode "Best Man," November 15, 1984.

In Australia, the initial broadcast of *Family Ties*' first season in the Melbourne and Sydney markets took place in 1983 and 1984, respectively. In Sydney, on account of the course of the later part of the storyline, the 1982 episode that heavily mentioned Friedman was judged unsuitable for its early-evening timeslot. The episode was skipped in the screenings of the series' first season and was not seen on Sydney primetime network television in the 1980s. In contrast, the same episode had already been shown in Melbourne, in the first full week of December 1983 (on Monday, December 5, 1983). This proved to be a poignant week for an episode focused on Friedman to be screened in Australia, as the floating of the Australian dollar (taking the place of a crawling-peg arrangement) was announced by the federal government at the end of the week, on December 9. The Friedman-related episode of *Family Ties* was also rerun a couple of times in Melbourne in 1986. See *The Age* (Melbourne), December 1, 1983, January 30, 1986, and October 16, 1986.

[3] Blaug (1985, p. 62). Friedman's fame continued, however, to have its limits. On April 8, 1982, the *New York Times* ran a profile of New York City-based arbitrator Milton Friedman. The article made no attempt to note the coincidence of names between this Milton Friedman and his more well-known economist counterpart.

economy—and one that would get worse. There would follow, in 1985 and 1986, a very sizable volume of commentary in the national and international business media to the effect that Friedman's longstanding monetarist positions had been decisively overturned by the economic results coming in during those years. The focus of criticisms had shifted from contentions that his policy prescriptions involved unacceptable economic sacrifices to suggestions that the relationships on which those prescriptions were based simply no longer existed.

The deterioration in Friedman's standing was captured in the *Los Angeles Times*' magazine supplement, in a long profile of him that was subsequently widely syndicated and that originally appeared at the end of 1986. The article reflected the ambivalence that had built up in recent years when it came to judging Friedman's imprint on economic thinking in policy circles. The article confirmed that he was still a much-in-demand speaker in the United States and that, on some big-picture items—such as the influence, on governments around the world, of free-market ideas and of arguments for floating exchange rates—he could still claim to have considerable sway and momentum.[4] But on the more specific matters of the influence of his thinking on U.S. macroeconomic policy, particularly monetary policy, and the weight that policymakers attached to his public and private commentaries, he had lost a lot of ground over the previous five years. The cover text blurb for the article summed things up as: "Around the world, Milton Friedman's stock has never been higher. So why has Washington stopped listening?" (December 14, 1986, p. 1.)

The state of affairs was not quite as dire as that: Friedman continued to have numerous close ties in the Reagan Administration, and through late 1986 there continued to be very occasional meetings at the White House of the President's Economic Policy Advisory Board (PEPAB). The fact of the PEPAB meetings and Friedman's membership of it continued to be reported in the press. A member of the public could therefore find out that Friedman continued to be one of Reagan's advisers.[5] They could even ascertain the specific times he met with Reagan, as PEPAB meetings were part of the president's public schedule. For example—contrary to Mallaby's (2016, p. 1) contention that the PEPAB "met in secret" with Reagan on January 23, 1986, and that the device of the CIA director's attendance of the meeting was used to "declare the meeting classified"—the PEPAB meeting held that day was actually listed in advance in the press, in routine newspaper reports of the president's work diary for the week ahead (*Alexandria Daily*

[4] Some of these items are taken up in the next two chapters, with this chapter focusing on Friedman's monetary views and his related predictions.

[5] For example, Friedman was described in the media as "a member of the President's Economic Policy Advisory Board" (*Dallas Times Herald*, March 21, 1985).

Town Talk (Louisiana), January 20, 1986).

A further sign that Friedman's influence on U.S. economic policy was far from exhausted came in the laudatory words about his intellectual achievements, along with a reference to his rapport with Reagan, that were given in the *Los Angeles Times* piece by another PEPAB member, who in 1987 the president would nominate to be the new Federal Reserve chair: Alan Greenspan (*Los Angeles Times*, December 14, 1986, p. 14). Nevertheless, Friedman's image as a major influence on the Reagan Administration did diminish after 1982, and as discussed in the next chapter, there was considerable basis for this change in perception, and especially so in Reagan's second term.

Perhaps most enduringly—and outside the Reagan Administration proper, but still within the U.S. government—Friedman's standing on monetary policy matters would be undermined badly from 1982 to 1986. Friedman was never a maker of monetary policy and had been an official adviser on it only peripherally. But his views on monetary policy received much attention. After 1982, this would less be the case. And, when he was mentioned, it was often—notably from 1984 onward—in the context of citing a bad prediction he had made. A combination of overconfidence and reasonable, but wrong, guesses, probably compounded by anger at the direction in which Federal Reserve policy had proceeded under Paul Volcker, led Friedman to make some very bad forecasts in the mid-1980s, and Friedman would be in the unaccustomed position of being trounced by his old Keynesian adversaries on the matter of predicting the future course of U.S. inflation.

Through these and related incidents, the tables would be turned. After many years in which he had successfully cast doubt, in his research and public commentaries, on the quality of actual U.S. monetary policy and of his opponents' monetary analysis, now it was the soundness of Friedman's own judgments on monetary matters that was heavily open to question, and both Federal Reserve policymakers and leading academics highlighted flaws in his recent analysis. Friedman was at bay through most of 1983 to 1986, and it was only at the end of that period when, after several years of floundering and backtracking, that he arrived at a tenable account of monetary developments that he could stick with. By that point, however, his standing as an authority on monetary matters had taken a considerable battering.

Monetary Trends: *no repeat of 1963's success*

Separately from his activities in public discussion of economic policy, Friedman was actually getting research completed and published at the start of the 1982–1986 period. Most notably, Friedman and Anna Schwartz's *Monetary Trends* (full title: *Monetary Trends in the United States and the United Kingdom: Their Relation to Income, Prices, and Interest Rates, 1867–1975*) was finally published.

The book was released in July 1982.[6] It therefore saw the light of day in the same summer in which *Grease 2* appeared in U.S. cinemas. Unfortunately, the parallels between the two sequels were not limited to their dates of release. Each of the sequels made an impact vastly less than that of their predecessors.[7]

The cover flap of *Monetary Trends* grouped it with *Monetary History* as part of Friedman and Schwartz's "now-classic series," but economists declined to treat *Monetary Trends* as a classic. "In retrospect, in terms of capturing people's attention, we reached a peak with *Monetary History*," Anna Schwartz would recall more than twenty years after the release of *Monetary Trends*. "The fact that *Trends* didn't really repeat the success of *Monetary History* was a big disappointment."[8]

Returning to this topic more than six years later, Schwartz was even more blunt. "It seems to me that *Monetary Trends* was dead from the moment it was published. Nobody really looked at it. It was just a failure. I know it [the lack of attention given the book] was something that bothered Friedman." (Anna Schwartz, personal communication, May 29, 2009.)

Contents of the book

Not counting its Chapters 1 and 12, which respectively foreshadowed and summarized the book's contents, the *Trends* book consisted of ten substantive chapters: 2 to 11. *Trends*' Chapter 2—essentially an updated and expanded version of the "Theoretical Framework" paper of 1970 and some of Friedman's 1971 and 1972 follow-up articles—outlined Keynesian versus quantity-theory accounts of nominal income determination and, to some extent, of price determination, too.[9] Chapter 3 outlined the NBER-originated procedure of "phase averaging" (the conversion of

[6] The July 1982 release date was given in Friedman and Schwartz (1982c, p. 3).

[7] On its London release, *Grease 2* was reviewed by Milton Shulman (a public-affairs writer whose name had often been transposed with Friedman's in U.K. discussion over the previous decade). A remark in Shulman's review on *Grease 2*'s makers would also be applicable to Friedman and Schwartz *vis a vis Monetary History*/*Monetary Trends*: "I suspect their luck will run out second time round." (*The Standard* (London), July 29, 1982.)

[8] In Nelson (2004, p. 406).

[9] Butler (1985, p. 127) claimed that Friedman and Schwartz (1982a) included ten pages on the first-round effects and transmission mechanism of monetary actions that had not appeared in the early-1970s publications. In fact, the new (as opposed to previously published and now lightly revised) material of this kind was about four pages (see

multiple annual data observations on a data series into a corresponding, single data point associated with an economic expansion or contraction) that the authors would use in their subsequent graphical and econometric analysis. Chapter 4 provided a very readable discussion of the authors' assembly of their U.S. and U.K. annual data and then went on to tabulate the corresponding phase-averaged data. Chapter 5 provided non-regression—graphical and correlation—analysis (albeit with the data in phase-average form) of money, nominal income, output, and prices, and their growth rates, for the United States and the United Kingdom.

The econometrics proper of the book began in Chapter 6, with a 100-plus-page study of the demand for money. The specification of the opportunity costs of money demand involved branching out on several dimensions. The authors strongly favored a semilogarithmic specification when including interest rates—at a time when this was not the most standard choice in the research literature.[10] They allowed for an own rate on money (albeit one measured indirectly). And, in line with an element of Friedman's restatement of the quantity theory, they included in their estimated money demand function a nominal rate of return on goods (although that rate—again, one measured indirectly—did not enter the estimated relations very powerfully).

Perhaps overshadowing much of the work is the fact that the authors allowed for a major upward shift in money demand in order to account for unusually low levels of velocity (in both the United States and the United Kingdom) in the interwar period.[11] The notion that economic uncertainty had produced an increase in the demand for money during the U.S. Great Depression had been part of Friedman and Schwartz's narrative account in 1963. But their use in 1982 of a dummy variable (representing uncertainty) in the money demand functions laid bare their reliance on this factor in their account of interwar developments. It is not, however, accurate to say of the Friedman-Schwartz money demand analysis in *Trends*, as Hendry and Ericsson (1991, p. 13) did, that "they do not formally *test* for constancy."[12] Page 232 of *Trends* reported, on the basis of *F*-tests, that, when intercept shifts were included, the estimated money demand function—which imposed constant slopes, alongside the shifts in intercepts, over the authors' century-long sample—exhibited no further significant instability over time in the case of the U.S.

Friedman and Schwartz, 1982a, pp. 26–29). The remaining coverage of these issues was largely a rewrite of material that had appeared in Friedman (1972a).

[10] An early case of a semilogarithmic money demand specification being estimated—albeit for bank reserves rather than the nonbank sector's money balances—was Frost (1966, 1971).

[11] See especially Friedman and Schwartz (1982a, pp. 4, 247–253).

[12] Emphasis in original.

equation, although it did do so in the case of the U.K. equation.[13]

Chapter 7, on the interrelationships between the two countries, although it was linked closely to Chapter 6, was almost totally detachable from the subsequent chapters. The chapter suffered from bringing in open-economy analysis only casually, but its content did underline Friedman's longstanding position that a floating-exchange-rate regime would not mean insulation of one economy from another's nonmonetary developments. Chapter 8, on nominal-income adjustment, was a meandering analysis that included a great amount of algebraic rearrangement of the authors' money demand equations as well as of other relationships, including directly estimated nominal-income equations. In the end, the chapter was largely concerned with estimating or simulating more elaborate versions of Friedman-Meiselman or St. Louis equations. Much better were Chapter 9, on the Phillips curve and monetary neutrality, and Chapter 10, on the money/interest-rate connection. A very short Chapter 11 had Friedman and Schwartz challenging the by-then-obsolete "long swings" literature, which had emphasized nonmonetary drivers of economic fluctuations.

The backdrop for the book's release

A fact that only received occasional acknowledgment in the book was that, since the previous economic-debate-centered book in the Friedman-Schwartz book series—1963's *Monetary History*—Friedman had, in good part because of his work with Anna Schwartz, himself become the center of monetary debate in the Western world. In the United States, this had really started in earnest in 1965–1966 (see Nelson, 2020b, Chapter 12) and had largely remained the case since, at least if public discourse was counted in the definition of debate. The proliferation of materials centered on Friedman that had appeared since 1963 included many items that had his name in the title. The first book with Milton Friedman's name in the title had been the William Hamovitch-edited collection *Monetary Policy: The Argument from Keynes' Treatise to Friedman* in 1966. And *Monetary Trends* frequently had occasion to cite another such book, the Robert Gordon-edited *Milton Friedman's Monetary Framework* (1974). *Trends* also cited articles that had Friedman's name in the title, including Levi and Makin's 1979 *Journal of Finance* article on "Fisher, Phillips, Friedman" and a 1976 *Journal of Monetary Economics* article, "Friedman's Dynamic Models: Empirical Tests" by Friedman's former student, Dean Taylor.[14]

[13] See also Friedman and Schwartz (1982a, pp. 249–252).

[14] See Levi and Makin (1979), D. Taylor (1976), and Friedman and Schwartz (1982a, pp. 66, 478).

Outside the book proper, the text of the dust jacket of the 1982 hardback (and back cover of the 1983 paperback edition) made a nod to events since 1963 by referring to Friedman as an economics Nobel winner and his current affiliation as “the Hoover Institution, Stanford University.” As for the book text itself, it gave some explicit acknowledgment of the course of the debate in the intervening years, when Friedman and Schwartz stated that the “resurgence of the quantity theory (renamed undescriptively ‘monetarism’) and the rejection of simple Keynesianism have been a reaction to the emergence of inflation and stagflation.” They also observed, in the same passage, that their own positions were no longer seen as “idiosyncratic and reactionary” and were “more nearly in the mainstream.”[15]

But the authors eschewed much discussion of policy-level debates in the course of *Monetary Trends*. There was one mention (p. 52) of the United Kingdom’s hardline-Keynesian Radcliffe Committee report of 1959 but then hardly any follow-up. The book frequently discussed Keynes, but it made no mention of Nixon or Arthur Burns when discussing the U.S. wage and price controls of 1971–1974. A Congressional submission that Friedman made in 1958 was cited but only as an example of earlier empirical work.[16] Pages 573 and 574 referred to 1980’s new peaks of short-term interest rates of 16 percent in the United States and of 18 percent-plus in the United Kingdom, as well as their inflation rates of 13 percent and 20 percent-plus respectively, but the authors did not mention Jimmy Carter, Margaret Thatcher, or Paul Volcker.

In his initial, mostly favorable, remarks on the book, Samuel Brittan made a virtue of the above-the-fray perspective the book had taken to historical personalities and policies: “The book is Friedman the Nobel prizewinner, not Friedman the television pundit.” But even Brittan noted that *Trends* “does not have quite the panache and sweep of the earlier volume [*Monetary History*].” (*Financial Times* (London), January 20, 1983.) The book was a study of the data whose purpose was distinct from that of *Monetary History*. But, in fulfilling its purpose, *Trends* did little to extend the previous Friedman-Schwartz U.S. historical analysis of the 1963 book up to 1975, and it made no effort to produce a historical narrative of U.K. developments.

“I didn’t see in *Monetary Trends* the sort of historical detail that made *Monetary History* so interesting… I remember reading *Monetary Trends* and not getting excited about it,” Stanley Fischer remarked (interview, August 30, 2013).

[15] Friedman and Schwartz (1982a, p. 70).

[16] Friedman and Schwartz (1982a, p. 57), citing Friedman (1958b).

Release and initial reception

The rapid fading of *Monetary Trends* from U.S. economists' radar did not reflect a lack of publicity when it came out. The *Wall Street Journal* had a feature on the book (August 31, 1982), while, in London, it was reviewed favorably in *The Economist* (November 13, 1982). In economic-research journals, too, *Monetary Trends* received considerable exposure on its release. The rollout of *Monetary Trends* included an article by Friedman and Schwartz on U.K./U.S. interaction in the inaugural issue of the *Journal of International Money and Finance* and one on money demand and the term structure in the *Journal of Political Economy*.[17]

Both these articles were essentially unaltered excerpts from *Monetary Trends*, and so the pieces cannot be said to have really been subject to formal review by the journals that published them. The appearance of these two articles was therefore good publicity for, but not a reliable stamp of quality regarding, *Monetary Trends*. But the fact that journal space was granted to Friedman and Schwartz for the articles did underline the prestige that the *Monetary History* had acquired by 1982 inside the economic-research world. This was the perception outside, too, with *The Economist* review of *Trends* stating (November 13, 1982, p. 110) that *Monetary History* "immediately became a classic of economic research."

Roughly five months after its appearance, the book received an extra potential fillip in three full-length reviews in the December 1982 issue of the *Journal of Economic Literature*, the American Economic Association journal whose launch Friedman had authorized back in the late 1960s.

The *JEL* issue in which the reviews appeared was dated December 1982. This would turn out to be a poignant date. In later years, the 1982/1983 threshold widely came to be seen—despite Friedman's efforts to resist the narrative—as one in which the kinds of relations between money and other variables that Friedman and Schwartz had documented in their work permanently became less reliable. Even Friedman would eventually grudgingly acknowledge that many commentators, when discussing monetary aggregates, had a "tendency to proceed as if all of economic history is divided into two parts: from the creation to 1983, and from 1983 to 1986."

[17] The *Journal of Political Economy* article was Friedman and Schwartz (1982b). It was Friedman's first publication in the *JPE* since his Nobel lecture in 1977 (and Anna Schwartz's first *JPE* article since 1947). Although it printed the 1982 article near the back of the issue, the *JPE* would make the most of this latter-day Friedman publication by including Friedman in a list of *JPE* contributors in an advertisement (published in *Econometrica* in January 1984) that asked: "What keeps *JPE* at the top?" (University of Chicago Press, 1984.) The *Journal of International Money and Finance* article was Friedman and Schwartz (1982c). It appeared in the first issue of that journal, which was initially coedited by Friedman's former student Michael Darby.

(*Wall Street Journal*, February 12, 1987.)

Of the three reviews, that of Goodhart (1982) concerned the book's material on the United Kingdom. The manner of proceeding chosen by the two other *Journal of Economic Literature* reviewers of *Monetary Trends* (who covered mainly the book's U.S. coverage) was revealing. Their approaches pointed up reasons why the book received little attention in subsequent years.

Robert Hall's (1982) review underscored the fact, stressed below, that the *existence* of strong money/nominal income relationships was not a strong interest of the academic monetary-economics world by the early 1980s.[18] A large part of those working in monetary policy research largely took these relationships for granted, especially for the long horizons that were the concern of the *Trends* study (though actual money/income patterns in the United States from 1982 onward would eventually force a reconsideration of this position). Rather, those in the rational-expectations monetary field concerned themselves with other issues, most notably the short-run behavior of output in response to monetary policy (a topic that *Trends* was ill-suited to cover) and whether the correlation might emerge even if monetary policy had little impact on income. Hall concerned himself with the latter issue in his brief review. He was, consequently, mainly disputing Friedman and Schwartz's interpretation of their findings and not the findings themselves. But his argument implied that these findings were not very interesting. Hall's position that money/income correlations are an artefact of the financial environment was one for which the natural-experiments approach of the *Monetary History* and, later, of Romer and Romer (1989) provided a better rebuttal than did the statistical work of *Monetary Trends*.

The other review, by Thomas Mayer, raised a number of specific criticisms. But an aspect of *Monetary Trends* that Mayer did single out for praise may be one reason why the book made little impact. In his opening paragraph, Mayer (1982a, p. 1528) remarked of *Monetary Trends*, "It is that rarity in economics—a true work of scholarship." These words were subsequently quoted in subsequent advertising for *Monetary Trends* and appeared on the back cover of the paperback version. But was a "work of scholarship" really something that would make the profession sit up and take notice? Scholarship is a distinct activity from research *per se*: the scholarship component of a written piece pertains to how the item puts its results in the context of the preexisting literature on the subject by relating its research findings to prior work. *Monetary History* had been scholarly (so much so that the cases in which it did not cite earlier

[18] In broad terms, this continued to be the case for the rest of the 1980s. See the discussion titled "Benjamin Friedman" later in this chapter.

relevant publications were all the more glaring) *as well as* making a large impact as a piece of research.

Monetary Trends was, generally speaking, scholarly.[19] The authors seemed keen to show that they had kept tabs on the literature that had appeared in the 1970s and very early 1980s. But this scholarship was not something that would gain their book attention. The lack of a research finding that had a punch comparable to *Monetary History*'s coverage of the Great Contraction, against the background of what was considered by 1982 an unfashionable choice of subject matter (long-run money/income relations), helped consign *Monetary Trends* to comparative obscurity, notwithstanding its status as the capstone of Friedman and Schwartz's study of money.

Phase averaging and statistical methodology

And for all their efforts to keep up with the literature, the Friedman-Schwartz volume had an antiquated air not only in its general subject matter but also in its methodology. In contrast to Friedman's disagreements with Arthur Burns during the 1970s on matters of monetary analysis and economic policy, the *Trends* volume that he and Schwartz were working on over that decade was a tribute to the extent to which Friedman was following in Burns' footsteps—at least when it came to the way in which economic data were processed to be made suitable for a study's analysis. Friedman and Schwartz used traditional NBER procedures and, in particular, a filtering/averaging procedure called "phase averaging." Their data on money, real income, nominal income, interest rates, and prices were, being annual observations, already highly time-aggregated. But, for both the United States and the United Kingdom, the Friedman-Schwartz data received further time aggregation via the attempt to convert each observation so that it corresponded to a particular recession or expansion that had occurred during the century of data.

This procedure, never prevalent in the economics profession, was archaic by 1982. When Gordon (1986b, p. 30) recalled that "the NBER continued to use the Burns/Mitchell [1946] methodology, most notably the recent volume by Milton Friedman and Anna Schwartz, *Monetary Trends* (1982)," he might have added that Friedman and Schwartz's study was virtually the last gasp of that methodology.

[19] For *Monetary Trends*, the gaps in the scholarship were mainly in Chapter 9's Phillips curve discussion: Samuelson and Solow (1960), Phelps (1967), Fischer (1977), and J.B. Taylor (1980a) all should have been cited. Also, the statement that adaptive expectations could coincide with rational expectations under certain specifications of a stochastic model was sourced to 1980 papers (p. 447), with no citation of J.F. Muth (1960).

In particular, Friedman and Schwartz followed the traditional NBER practice in averaging their annual data on money, nominal income, and other series, into corresponding "phase averages." Under this procedure, the NBER chronology of expansions and contractions (and an equivalent one for the United Kingdom) would dictate the number of observations to be used, and annual data would be condensed to a smaller number of observations, each of these new data points being the value that the series took during a contraction or expansion. This highly complicated way of transforming the data had next to no following by the early 1980s, and it would be criticized by econometricians such as Hendry and Ericsson (1983, 1991) and Stock (1987). It is significant that, although *Monetary Trends* has picked up many citations as a source of data, researchers overwhelmingly have used Friedman and Schwartz's tabulations of annual data, rather than the phase averages of the series that Friedman and Schwartz also reported and that they used in their regressions.[20]

The use of phase averaging led to some of the denser passages of *Monetary Trends* as the authors expounded the convoluted procedure (see, for example, their pages 79–80 and 357). Phase-averaging also frustrated some of Friedman and Schwartz's own research tasks. The high level of time aggregation entailed by phase-averaging made it difficult for Friedman and Schwartz to study dynamic adjustment patterns of nominal income and interest rates—which ended up being a major concern of *Monetary Trends*. In the early 1960s, when Friedman's estimates of money-to-income lags were likely inflated by his, and Schwartz's, concentration on monetary-growth/income-level comparisons, phase-averaging might not have seemed prejudicial to a study of the dynamic relationship between money and income. By the early 1980s, in contrast, he had accepted that much of the reaction of nominal income to monetary policy occurred within a year.

Consequently, *Monetary Trends*' Chapter 8, "Monetary Influences on Nominal Income," which studied the relationship between phase averages of money and income (and of the corresponding growth-rate series), was bound to fall short in its aim of studying the dynamic aspects of the money/income relationship and the 50-plus-page chapter went largely unremarked upon. For their chapter, "Money and Interest Rates," which mainly concerned the Fisher effect, Friedman and Schwartz had the sense to abandon phase-average data toward the end of their discussion and to consider, instead, monthly data on interest rates and inflation.

[20] Friedman and Schwartz computed, without publishing, annual-data versions of the relationships that they were estimating using phase-averaged data (Mayer, 1982a, p. 1534; Friedman and Schwartz, 1991, p. 41). Friedman and Schwartz (1991, p. 41) noted that the estimates of their money demand equation were not very sensitive to their use of phase averages, with annual data giving similar results (see Friedman, 1988a, p. 237). However, while this indicates that results obtained using phase-average data are not an artefact of transforming annual data to phase averages, it does not itself point to the desirability of phase-averaging the data.

Coverage of the credit process

It is worth discussing an aspect of the monetary economics of *Monetary Trends* that likely received unduly harsh treatment in the reviews of the book. The undoubted fact that Friedman and Schwartz treated money as a policy-determined variable in their analysis was interpreted by some reviewers not as a simplifying postulate but as reflecting lack of knowledge, or acknowledgment, on their part of the connections between money and credit or of policymakers' management of interest rates.

It should first be stressed that money-credit interactions and the implications of the chosen policymaker reaction function were longstanding themes of the monetarist literature. Karl Brunner (1983, p. 52) observed in response to similar critiques made around this time: "Monetarist studies have yielded important insights into the role of the Central Bank, the public, and the banks in shaping the behavior of monetary growth and the growth rate of bank credit." Brunner was, of course, mainly writing in reference to the Brunner-Meltzer work (although, in doing so, he should have used a more neutral word, like "results," than the self-glorifying word "insights").[21] But *Monetary History*, and Friedman's work in the intervening years, had covered these issues in detail, too: see the discussion in Nelson (2020a, Chapter 8), as well as the analysis of 1973−1979 federal funds rate policy, and the analysis of Friedman's commentary on it, that were provided in Chapters 2 through 10 of the present book.

Indeed, one could hardly fail to notice, if one perused Friedman's many commentaries on current monetary policy in the early 1980s, that the money supply process and the policymaker reaction function were important matters to him. This will be brought out again in Section II below, when Friedman's analyses of Federal Reserve policy in 1982−1986 are considered.

Friedman and Schwartz also covered these issues, although not in detail, in *Monetary Trends*—and did so more so than their critics allowed. Friedman and Schwartz did not make the omission attributed to them in the hostile review of Basil J. Moore (1983), who stated (p. 152) that "they neglect even to mention that [their] U.S. data are confined to deposits held with commercial banks!" Evidently, Moore missed the explicit statements to this effect on pages xxviii, 3, 126, and 261 of *Monetary Trends*. And although Tim Congdon (in *The Banker* (London), July 1983, p. 119) contended that Friedman and Schwartz's *Trends* discussion nowhere considered the role

[21] Indeed, Benjamin Friedman remarked, "the essence of the Brunner and Meltzer model was disaggregating [bank balance sheets] so that there was not just money but also credit, which of course [was an element of their analysis that] always appealed to me." (Benjamin Friedman, interview, May 10, 2013.)

of the interest rate in the money supply process, he elsewhere (p. 125) acknowledged a statement (on *Trends*' page 36) that the interest rate could matter for the money supply process. In addition, page 499 of *Trends* stated: "In Wicksell's model, the initial rise in the 'natural' rate would be resisted by banks, which would lead to a rise in… monetary growth…" And, of course, *Monetary Trends* was written during years in which Friedman repeatedly acknowledged (and complained) that (even holding constant the money multiplier) the management of the federal funds rate by the authorities made the demand for, and supply of, credit factors in the determination of the U.S. money supply, as this policy arrangement meant that developments in the credit market affected the amount of reserves provided by the Federal Reserve to commercial banks.[22]

The Hendry-Ericsson critique

At a panel of academic consultants held in London by the Bank of England in October 1983, David Hendry and Neil Ericsson produced a lengthy critique of the *Monetary Trends* econometric analysis. Their paper, which was brought into public debate in the United Kingdom (see D. Smith, 1987, pp. 149–150), was issued as a Bank of England publication in December 1983, and it appeared in revised form in one of the Federal Reserve Board staff research papers in December 1985.[23] A retitled, and substantially rewritten, version ultimately appeared, along with Friedman and Schwartz's reply, in the *American Economic Review* issue of March 1991.

As the Hendry-Ericsson critique was focused on the U.K. results of *Trends*, a discussion of it and the debate that it generated properly belongs in a study of U.K. rather than U.S. monetary debate.[24] But a few words are worth making on the impact that these likely had on discussions in U.S. monetary-economics circles in the 1980s. The debate received only limited coverage in print in the United States. But the title of the Hendry-Ericsson paper, "Assertion Without Empirical Basis," together with the paper itself and the tone of the reaction it generated, probably fostered a feeling that the *Trends* work was fragile. David Laidler's (1982, p. 293) review of *Trends* had stated that the book was "above all, careful. I can think of no work in applied econometrics whose authors have given as much attention as have Friedman and Schwartz to making clear to the reader where the data employed come from, how they have been adjusted,

[22] In addition, the text of *Monetary Trends* acknowledged that, in the modern era, monetary policy had been concerned with short-term economic stabilization (Friedman and Schwartz, 1982a, pp. 442, 453, 571).
[23] Ericsson, formerly a graduate student of David Hendry's, had been an economist at the Federal Reserve Board since late October 1983, having taken up that position a few days after the October 28 panel meeting in London.
[24] The present author first wrote a lengthy account of the Friedman-Schwartz/Hendry-Ericsson debate in a November 1993 manuscript prepared for a discussion held in Sydney with Neil Ericsson.

and how the adjustments in question may or may not affect the results generated." Similarly, the *Economist* review was titled "Spade-work," and it had suggested that the book showed why "Friedman will be remembered for his laborious and scholarly research on money" (November 13, 1982, pp. 109, 110). The appearance of the high-profile Hendry-Ericsson critique had, however, created the opposite impression. Anna Schwartz noted that Friedman objected to the critique in part because of the impression it likely—and, in Friedman and Schwartz's view, unjustly—created that *Trends* had been a "flimsy" study (Anna Schwartz, personal communication, May 29, 2009).

A sign of the negative influence of the Hendry-Ericsson critique on general perceptions, in the United States during the mid- and late 1980s, of Friedman and Schwartz's work materialized when, in their 1989 inspired by the study of the *Monetary Histor*y, Christina Romer and David Romer felt obliged to mention the Hendry-Ericsson critique and to indicate that it had not applied to the 1963 book: "Hendry and Ericsson (1987) criticize Friedman and Schwartz's econometric methods, focusing mainly on their later work."[25]

Romer and Romer were on solid ground, and their remarks lined up with Samuel Brittan's explanation to his readers, during the height of the U.K. reaction to the Hendry-Ericsson critique, that "David Hendry's critique is confined to one particular book of Friedman and Schwartz."[26] Both in the 1980s and later, Hendry and Ericsson wrote very little on the *Monetary History*, and they did not cite it in Hendry and Ericsson (1991). In fact, their research writings made scant effort to relate their critique to the objections made to monetarism by Keynesians over the years.

Lack of staying power

On the U.S. scene, why was *Monetary Trends* not another book that took the profession by storm? There were likely several factors involved. A major reason is that the agenda of the profession had changed. When the *Trends* project was begun, there was far more controversy among economists regarding whether certain long-run monetary relations held in the data than there was in 1982. The rational-expectations revolution had, as already noted, shifted the research agenda to short-run dynamic adjustment. Work by Friedman and Schwartz and other monetarists had already changed macroeconomic thinking substantially, and the lion's share of research in monetary economics was now concerned with rigorous modeling of short-run

[25] Romer and Romer (1989, p. 169). The authors were referring to a 1987 revision of the Hendry-Ericsson paper.
[26] *Financial Times* (London), January 19, 1984. Indeed, as Friedman and Schwartz (1991, p. 39) stressed, the critique focused on the U.K. results that were reported in one chapter of *Monetary Trends*.

dynamics or of money-holding, rather than the empirical evidence on long-run relations that concerned Friedman and Schwartz in their *Trends* study. Thomas Mayer felt that there was "a certain amount of tiredness" in U.S. economists' reaction to Friedman and Schwartz covering money/income issues again in *Monetary Trends*: "it didn't really have a fighting edge for U.S. readers, as the *Monetary History* did." (Thomas Mayer, interview, October 16, 2013.)

"I think that *Monetary Trends*, if it had come out when it was supposed to come out, which would've been around '68, something like that, around the time that *Monetary Statistics* came out [1970], the impact would've been a lot bigger," Michael Bordo observed (interview, July 24, 2013), adding that *Trends* was initially drafted in the era "before the Lucas revolution, before all the changes that occurred in macro... It was a complete complement to the other two books. And so, if those three books had come together, the impact would've been a lot different. *Monetary Trends* would've had a lot bigger impact. But [instead] it came out 'after Lucas.'"

To that extent, the long gestation period for the book counted against it. As the authors noted in the introduction, the extended period of completion of the book reflected not only several rounds of revisions but also "[o]ther digressions and interruptions affecting both authors."[27] In the case of Anna Schwartz these included membership of the Shadow Open Market Committee, a number of other NBER projects, and a health setback in the form of losing vision in one eye. In the case of Milton Friedman, the digressions and interruptions included becoming world-famous.

Friedman and Schwartz had revised the manuscript many times to try to keep in step with the literature and, up to a point, new data (eventually settling on a 1975 cutoff date): "it just dragged on, and on, and on," Michael Bordo observed (interview, July 24, 2013). Nevertheless, the early-vintage origination of the book was not altogether stamped out by the revisions that were made to the draft in the years through its 1981 finalization. Not only was the topic of the book somewhat out of fashion at the time of its release date. Crucially, the methodology of the book was also very dated, as stressed above. And despite the multiple occasions on which the authors were able to deliberate on and polish their manuscript, some glaring slips slipped through to the published version. Most of these were isolated items that reflected proofing oversights.[28]

[27] Friedman and Schwartz (1982a, p. xxiv).

[28] For example, the numbered list of principal economic findings jumps from no. 5 to no. 7 (Friedman and Schwartz, 1982). A version of the list that had no such jump (Schwartz, 1981) implied that the two sentences of findings reported as finding no. 5 in *Monetary Trends* should have been one-sentence findings reported as findings no. 5 and 6. Another glaring error appeared on page 403: "acceleration in nominal income" must actually be "acceleration in money." Other aspects of the book gave away the fact that different parts were written at different times. For example, the change in Friedman's attitude to data analysis during the writing of the book was felt in the conflicting statements in the book: in places, Friedman and Schwartz seemed to regard analysis with rates of change as much

Most of the chapters of *Trends* were nevertheless highly readable. In particular, the chapters on the authors' theoretical framework, the basic data, their pre-regression chapter providing basic plots and correlations all flowed smoothly. The chapter on money demand, in which the authors' preferred money demand equation was reached bit by bit, with different aspects of the specification (such as the scale and opportunity-cost variables) being arrived at sequentially, would be criticized by Hendry and Ericsson (1991, pp. 16–17) for embracing a methodologically flawed way of proceeding. This approach, nevertheless, had advantages as an expositional procedure. It provided a reader-friendly way of navigating through table after table of regression results.[29]

Tyranny of the Status Quo*: no repeat of 1980's success*

The *Economist*'s review of *Monetary Trends* had observed that the book shed light on "what Friedman has been doing away from the armchair [that is, his commentaries on current events] and the television studio." (*The Economist* (London), November 13, 1982, p. 110.) In 1983, post-*Trends*, Friedman was back in the television studio for a protracted period. He was making a new series, called *Tyranny of the Status Quo*.

The phrase was one Friedman had used for decades.[30] In 1983's context, however, it referred primarily to the Reagan Administration's loss of momentum in its efforts to reduce the size of the U.S. public sector.

Upon 1984 broadcast, it would transpire that the series, very much like *Monetary Trends* in 1982, would be a sequel that received a far lower level of attention than its predecessor (which, in the case of *Tyranny of the Status Quo*, was *Free To Choose*). Reflecting the Friedmans' disappointment at this outcome, in their memoirs would allocate only about half a page to the *Tyranny of the Status Quo* project, and they presented the experience as a minor coda to the

less reliable than analysis with levels (for example, pages 86 and 283–284). In other passages, the book reflected the preference (especially evident in Friedman's post-1966 work) for focusing the analysis on growth rates (for example, pages 333, 366, 428, 431, 432, and 442).

[29] Nevertheless, the exposition in this chapter had defects, perhaps reflecting the fact that it was written over a long period. In particular, Friedman and Schwartz stated a conclusion that "there is no justification for including both" short-term and long-term interest rates in the money demand function (p. 267), in apparent reference to the difficulties of putting both rates directly in an estimated money demand function. But their way of stating this conclusion was overly categorical and contradicted the argument made elsewhere in the chapter (pp. 263 and 284–294) that, in principle, *both* short- and long-term interest rates *should* matter for money demand. See Nelson (2020a, Chapter 6) for further discussion.

[30] See his remarks in Friedman (1957c, p. 74; 1962a, p. 68, 158, 182; 1962b, pp. 241, 242), in S. Tax (1967, p. 200), and in *Newsweek*, October 9, 1967.

odyssey of the *Free To Choose* series.[31]

In contrast to *Monetary Trends*, however, Friedman went into the *Tyranny* project knowing that it would be a considerably less ambitious enterprise than its predecessor. The making of the new series had no counterpart to the wide-ranging location filming associated with *Free To Choose*, and Rose Friedman would describe the production as "much more modest, much less expensive." [32] The series consisted of three episodes—seven fewer than the episode count of the U.S. version of the *Free To Choose* series—and, in contrast to the earlier TV show, it was essentially studio-bound.

A full-length series that followed the *Free To Choose* template was not going to happen. There was not the marketing clamor for a sequel to the 1980 series comparable to that for a follow-up to the book. As discussed in Chapter 11, although the *Free To Choose* series was widely known, and did very well by the standards of American public television, it was not a commercial blockbuster. Its book counterpart, in contrast, was a *bona fide* blockbuster.

The scale of the task of making a new full-scale series was also beyond the resources, both financially and in terms of the time commitment, at the disposal of the Friedmans and Robert Chitester. Making the original series had involved a multi-year process of obtaining financing and working out international television deals—exercises that the Friedmans had little wish to repeat. Even a shorter version of *Free To Choose* was ruled out: Chitester explained that, this time around, there was no prospect that they would "find a quarter of a million dollars to do a documentary" (*San Francisco Chronicle*, February 18, 1984). And the Friedmans and Chitester were additionally faced with a less favorable situation regarding potential broadcasters of the series. The entities that were willing to broadcast *Free To Choose* in 1980—the BBC and, as it turned out, U.S. public television, too—were disinclined to give airtime to a follow-up series.

The book's success was the catalyst for a follow-up series and book, as Robert Chitester recalled: "One way to look at it, in a kind of cynical way, is that it was actually motivated by [publisher] Billy Jovanovich thinking that this was a way to really exploit the huge success of [the book version of] *Free To Choose*. He and Milton came up with the idea, and [the] Harcourt Brace Jovanovich [publisher] put up the money to do it. But it was a very, very modest undertaking. My memory is that it was $100,000 to do three programs. On *Free To Choose*, in 1980 dollars,

[31] Friedman and Friedman (1998, p. 503).
[32] Friedman and Friedman (1998, p. 503).

or ’79 dollars, we spent about $270,000 per program, per hour. And, obviously, with inflation, you can see that that puts it in the $700,000 to $800,000 range today. And there [for *Tyranny*], we were producing these shows, three of them, for [a total of] $100,000.” (Robert Chitester, interview, July 9, 2013.)[33]

“PBS Nixes Milton Friedman Series” was a newspaper headline that appeared in March 1984, in reference to the fact that the public-television company had refused to give the *Tyranny of the Status Quo* program a slot on its national schedules (*The Marietta Daily Journal* (Georgia), February 21, 1984). After being rebuffed by PBS on the matter of broadcasting the programs, Chitester noted publicly, “I think they should have been willing to schedule them on the network,” and he criticized the grounds for rejection given to him by PBS—that the program was a talk show (*San Francisco Chronicle*, February 18, 1984).

In retrospect, however, Chitester assessed that it was not just dislike, in PBS quarters, of Friedman’s policy views that weighed against the likelihood that PBS would agree to show *Tyranny*. He had been able to get PBS to accept his programs before, but doing so had required “two factors, and neither of which were anywhere nearly as well-developed with *Tyranny* as they were with *Free To Choose*. One is: You have to produce high-quality stuff. And I mean BBC [standard]—You know, what is the standard that people judge public broadcasting on? Basically, it [material broadcast on PBS] comes out of the BBC—much of it. And it’s extremely high production value. *Free To Choose* met that test. [In contrast,] *Tyranny of the Status Quo* looks like a local television talk show…. That’s number one. And number two is: The content. And the content of *Tyranny* was much more sophomoric. And that’s a pun, in one sense—because it [the program format] was Milton talking to a group of college students.” (Robert Chitester, interview, July 9, 2013.)

This was true. Whereas the debate portions of the *Free To Choose* series had seen Friedman confront authorities on the subject matter covered by the episode in question, in *Tyranny of the Status Qu*o his interlocutors were undergraduate students, who had been invited to participate on the basis of interviews conducted on various U.S. campuses (*San Francisco Chronicle*, February 18, 1984). In the Friedmans’ memoirs, Rose Friedman admitted frankly that she and Milton Friedman did not like the programs when they watched them before broadcast and that they felt that Milton Friedman looked out of place in having a dialogue with college-level students. The

[33] Likewise, Friedman in Hammond (1989, p. 49) stated that the project wasn’t “on the same scale” as Free To Choose and arose because “Bill [Jovanovich] was anxious to have another book [by the Friedmans].”

reason she cited for their negative reaction to this situation was that Milton Friedman looked like he was lecturing the students, rather than exchanging views with them.[34]

The Friedmans' impression that Milton Friedman looked like he was in a classroom setting and seemed close to badgering his inexperienced opponents, rather than having a discussion with them, mirrored the grounds given publicly by Barry Chase, PBS' director of news and public-affairs programs, when he explained PBS' refusal to show the program. It was no surprise that Friedman would look like he was "batting 1.000" in such a "seminar setting," Chase suggested (*San Francisco Chronicle*, February 18, 1984).

The *Tyranny* television format also encouraged the myth that college professors' daily lives heavily comprised debating current policy issues with undergraduate students. In Friedman's case, this myth was especially ill-founded because, although his name was a mainstay in undergraduate economics texts, his own participation in the teaching of undergraduate courses had been trivial since the early 1950s, and he had given up university teaching altogether seven years before the *Tyranny* studio sessions were videotaped in 1983.

Chitester acknowledged of the *Tyranny* programs, "they were just very, very low-end, in terms of both their production values and the [on-camera] interaction of the people involved." He noted that, despite PBS' rejection of the series at the national level, "it did get some carriage," as "there were a number of public-television stations that took them" and broadcast the three episodes of *Tyranny* (Robert Chitester, interview, July 9, 2013.) All told, about a dozen public-television channels, including ones based in Boston, San Francisco, and Los Angeles, screened the programs (*San Francisco Chronicle*, February 18, 1984), which were offered to PBS stations on a satellite-distribution basis.[35] But this was a poor outcome in view of the fact that the program contained what Chitester felt, after *Free To Choose*, should be high-in-demand content: "It's Milton Friedman and his ideas" (*San Francisco Chronicle*, February 18, 1984). In the end, the series had very few viewers, and Chitester concluded that "it certainly did not receive the attention of *Free To Choose*." (Robert Chitester, interview, July 9, 2013.) There was also no parallel to the earlier series' international dimension, as the U.S.-centric *Tyranny* series was barely seen abroad.

By late April 1984, when Friedman was a couple of weeks into publicity rounds for his and Rose

[34] Friedman and Friedman (1998, p. 503).
[35] Friedman and Friedman (1998, p. 503).

Friedman's new book *Tyranny of the Status Quo*, the television program's limited screenings in the United States were already largely completed. The book version of *Tyranny* actually had little to do with the series, other than the title and some themes.[36] The Friedmans wrote it over the spring and summer of 1983, with some additions made in October 1983.[37] Its aim was to cover the first two-and-a-half years of Ronald Reagan's period in office.[38] A particular focus of the book was the administration's record on fiscal policy.[39] But, as discussed in Chapters 5, 15, and 16 of the present volume, *Tyranny*'s coverage also covered social issues like drugs and crime, as well as national defense.

With considerable justice, Rose Friedman could look back on the book as the best thing to come out of the *Tyranny of the Status Quo* project.[40] In 1989, Milton Friedman would remark that he thought that the *Tyranny of the Status Quo* book had a better claim than *Free To Choose* did as a companion and update to *Capitalism and Freedom*.[41] *Tyranny of the Status Quo* had some of the flaws of the *Free To Choose* book—in particular, it had inadequate documentation of data sources.[42] Overall, however, it was a more cohesive book than *Free To Choose*. It had the advantage of being able to focus on a specific subject (that is, the Reagan record). And although much shorter than *Free To Choose*, the *Tyranny of the Status Quo* book generally had more analytical depth than the 1980 book had exhibited. The 1984 chapter on inflation, although flawed by Friedman's conviction that a new surge in inflation was ahead, covered the monetary view of inflation more clearly than had been the case in *Free To Choose,* by providing a clearer contrast of oil-push views of inflation and Friedman's monetary view of inflation.[43]

[36] For the 1985 U.K. paperback version, even the title did not quite coincide with that of the series (which was not shown in the United Kingdom), as the cover and book spine added a "The" at the front of the title.

[37] The book had various references to events and media in the spring and summer of 1983 (see, for example, Friedman and Friedman, 1984, p. 170). On the October 1983 material, see the discussion of monetary policy in the next section.

[38] Se Friedman and Friedman (1984, p. 9).

[39] Friedman's views on Reagan's fiscal policy record in his first two years in office were covered in Chapter 11, while the two chapters that immediately follow this one consider aspects of U.S. fiscal policy from 1982 to 1986.

[40] Friedman and Friedman (1998, p. 503).

[41] In Hammond (1989, p. 49).

[42] Despite providing a lengthy discussion of the many official sources that needed to be consulted for constructing a series on total government expenditures, Friedman and Friedman (1984) did not really explain how they arrived at their government-spending-to-national-income estimates. Their estimate that, in the United States in 1980, total government spending (including state and local outlays) was about 40 percent of national income (Friedman and Friedman, 1984, p. 15). This calculation no doubt included estimates of spending by off-budget government enterprises, but it also resulted from their use of a net-product concept of income, rather than GNP, as the denominator. Friedman indicated that he was using this narrower definition of U.S. aggregate income in *San Francisco Chronicle,* September 14, 1984.

[43] Numerous aspects of the coverage of monetary policy and inflation in the book are considered in the course of the rest of this chapter. These include the book's coverage of cost-push inflation: see the discussion titled "Otto Eckstein" below.

That the *Tyranny of the Status Quo* book was superior to *Free To Choose* was also the assessment of the person who typed up the Friedmans' drafts of both books—Gloria Valentine. She observed: "I was sorry that it didn't catch on like *Free To Choose*. I thought it would." (Gloria Valentine, interview, May 7, 2013.)

As Valentine's comments implied, the *Tyranny of the Status Quo* book was much less commercially successful than the *Free To Choose* book. This was no doubt partly due to the lack of a strong television tie-in series. In addition, the Reagan years had resulted in a dissipation of the novelty in U.S. public debate of advocacy of smaller government.

Rose Friedman would claim that the book was still in print in 1998.[44] But it likely was not, either in the case of the 1984 hardbound version or the low-print-run paperback version that Harcourt Brace Jovanovich put out into the U.S. market in 1985.

Newsweek *drops Friedman*

Another indication that Friedman had passed the peak of his success as a popular writer had come shortly before the release of *Tyranny of the Status Quo* when, on February 13, 1984, *Newsweek* announced that Friedman and Lester Thurow were being dropped as the magazine's economics columnists, economic commentary being taken over by a full-time staff writer (*Wall Street Journal*, February 14, 1984). His 17½-year affiliation with *Newsweek* had been terminated.[45]

Heart attack October 1984, and cardiac surgery, January 1985

On top of these professional setbacks, Friedman's *annus horribilis* of 1984 was capped in mid-October. Friedman had been quoted in 1983 saying that he had "never had a heart attack," and that the bypass operation that he had gone through in 1972 had "been just incredibly successful.

[44] Friedman and Friedman (1998, p. 503).

[45] A potted biography of Friedman in *USA Today* (April 11, 1984) listed his current positions as including *Newsweek* contributing editor, but this was erroneous. The courtesy title of contributing editor was one that came, and went, with Friedman's period as a columnist for *Newsweek*. (Friedman confirmed this fact in Europa Publications, 2003, p. 189.)

In the aftermath of being dismissed by *Newsweek*, Friedman omitted his past role as one of the magazine's columnists from one biographical entry he supplied (Europa Publications, 1986, p. 522). A little later, however, when providing another such entry, he did include "Economic [sic] Columnist, *Newsweek*, 1966–84" among his past posts (A&C Black, 1987, p. 621).

I have never had a problem. Everything has been fine."[46] But on October 16, 1984, he suffered a heart attack while in New Orleans, where he had been scheduled to talk at the dedication ceremony of Tulane University's business school. Friedman's heart attack was described as mild, and early indications were that he would be discharged within a few days (*Toronto Star*, October 17, 1984; *The Evansville Press* (Indiana), October 17, 1984). But, in fact, he spent two weeks at Tulane Medical Center, followed by a spell at Stanford University's campus hospital.[47] He was released from the latter hospital on November 4 or 5, 1984, and for several weeks was unsure whether the next step would consist only of medication or would instead involve further surgery, either an angioplasty procedure or, as in 1972, full-blown open-heart surgery.[48] By early December, it had been determined that the third of these courses was necessary.[49] Friedman had a bypass operation on December 11, 1984. It was his second and final one, and it took place close to the twelfth anniversary of his first operation.[50]

As had been the case with his 1972 operation, Friedman made a good recovery. He did, however, necessarily have an extended period of further convalescence.[51] Friedman had missed many conferences and published debates related to his own research over the years, so his absence from these forums during 1985 was by no means a sure sign of his inactivity. Nevertheless, his non-attendance of an American Enterprise Institute conference on monetary targeting held in Washington, D.C., on February 8, 1985, and his non-participation when, over the spring of 1985, *The Economist* published a series of essays on monetarism over the spring of 1985 underlined the fact of his convalescence.

Friedman's post-operation return to public activity was evidenced in March 1985, with an interview with the *San Francisco Examiner* (March 18, 1985), a talk in Dallas on March 20, and an op-ed on Israel published in the *Jerusalem Post* (March 15).[52] In April 1985, he penned a couple of op-eds on trade that would appear in the *San Francisco Chronicle* (see the next

[46] In G.R. Martin (1983, p. 57).

[47] See Rose Friedman's account in Friedman and Friedman (1998, p. 571).

[48] Letter from Milton Friedman to R.W. Hafer, November 8, 1984.

[49] Letter from Milton Friedman to Lester Telser, December 4, 1984.

[50] See Rose Friedman's account in Friedman and Friedman (1998, pp. 571–572). The date of December 11, 1984, was not provided there but was given in *Chicago Tribune*, January 27, 1985. Friedman had also mentioned the December 11 date in a letter to Lester Telser (of December 4, 1984). Between his stretches of time spent in hospitals, Friedman published an op-ed for the *Wall Street Journal* on international trade (November 27, 1984*b*).

[51] During this period, in January 1985, he did write an introduction to a book that criticized the record of welfare-state economies in Continental Europe. (See the sign-off date in Friedman, 1985g, p. xii.)

[52] Friedman gave a talk to on budget and trade deficits to an audience of 1,800 at Southern Methodist University on March 20, 1985 (*Dallas Morning News*, March 21, 1985). His letter to the editor, "Judging By the Record," dealing with economic policy in the 1970s in Israel, appeared in the *Jerusalem Post*'s edition of March 15, 1985.

chapter).[53] By May 1985, he was well enough to travel to Tokyo to be, as he had been in 1983, a keynote speaker at a Bank of Japan conference on monetary policy.[54] He then gave a popularized version of his Tokyo talk as a guest speaker at San Francisco's Commonwealth Club on June 28, 1985.[55]

The U.S. economics magazine *Challenge* put Friedman on the cover of its July/August 1985 issue. Its interior contents had a Friedman article titled "The Case for Overhauling the Federal Reserve."[56] Although the Federal Reserve Bank of Boston's president, Frank Morris, gave a speech on the following September 11 in which he described the *Challenge* piece as containing "Milton Friedman's latest views," this was not really the case.[57] The *Challenge* piece was a reworking of a segment of an article that Friedman had written nearly two years earlier.[58] But Morris' speech *did* relay more recent positions on monetary policy that Friedman had expressed—though not in a flattering light, as Morris went on to consider Friedman's spectacularly erroneous forecast of a 1984 surge in inflation alongside a new recession.[59]

These forecasting errors, and Morris' recounting of them, were part of a pattern in Friedman's public profile in the period since 1982. This pattern largely continued in 1986, as discussed in detail in the next section. All told, the period from 1982 to 1986 saw him involved in a string of misadventures, even leaving aside his health setbacks. He had hoped for a repeat of the success of *Monetary History* with *Monetary Trends*, but the new Friedman-Schwartz book had made little impression. In *Tyranny of the Status Quo*, the Friedmans produced an encore to *Free To Choose* that failed to stir up anything approaching the interest of the earlier enterprise, either as a television program or as a book. *Newsweek* had dropped Friedman, and he had lost clout with the Reagan Administration. And superimposed on these setbacks was the series of well-publicized and disastrously wrong forecasts that Friedman had made regarding inflation in the mid-1980s.

Even Friedman's post-operation interview, which helped to get the word out about his return to health, came with a sting in the tail that threw doubt on his continuing relevance. In discussing

[53] The pieces were syndicated: for example, the first of the op-eds subsequently appeared in *Boston Globe*, May 28, 1985, and the second in *Dallas Morning News*, May 6, 1985. They were published sequentially in *Houston Post*, April 28 and 29, 1985, and in *Seattle Times*, May 16 and 17, 1985.
[54] Friedman (1985e). See also the further discussion in the rest of this chapter of these conferences.
[55] Friedman (1985c).
[56] Friedman (1985f).
[57] The quotation is from F.E. Morris (1985, p. 8; p. 224 of 1987 reprint),
[58] It was drawn from Friedman (1984b), a book chapter written in 1983.
[59] F.E. Morris (1985, p. 8; p. 228 of 1987 reprint).

the fact that his policy prescriptions still encountered heavy resistance, it raised the specter that he was himself on the brink of passing from the scene altogether: “Old age (he’s 72) has brought mixed blessings to Milton Friedman,” one paragraph began (*San Francisco Examiner,* March 18, 1985). The reference to Friedman being old was picked up when a syndicated version of the same article was published under the headline “Old Age Is Generous to Economist Friedman” (*Houston Chronicle*, May 26, 1985). Then, in April 1986, Friedman granted a long interview to the *American Banker*, only for the interviewer to describe him in print as “in the twilight of a long and distinguished career” (*American Banker,* April 30, 1986, p. 20). And a couple of months later *New York Times* article (July 3, 1986, p. D7) on monetarism described Friedman as “now 73”—the “now” making it sound like it was a truism that 73 was a could-die-at-any-moment age.

For his part, Friedman after 1984 had no illusions about the fact that his peak period of activity was behind him—even in public-policy work. “I’m old in age,” he acknowledged on television in late 1985, by which time he had already reached the aforementioned age of 73. “That’s all right.”[60] But the increasing tendency for him to be described in terms of the past was compounded by the suddenness with which his monetary views had come to be regarded, quite widely, as having been discredited.

This must have been galling, and the same period nevertheless saw echoes of the defiance and perseverance that had characterized Friedman’s behavior in the long stretch of time, much earlier in his career, that had likewise been one in which the quantity theory of money was out of favor. The *American Banker* profile noted (April 30, 1986, p. 20) that “Mr. Friedman continues writing about monetary economics.” Likewise, a dictionary of economics that appeared in 1986 observed, after noting widespread opposition to the quantity theory of money: “Nevertheless, variations of the theory [the quantity theory], popularized in the United States by Irving Fisher, continue to be propounded by some economists, notably Milton Friedman…”[61]

When, in 1985, a different, and much more ambitious, dictionary/encyclopedia of economics called the *New Palgrave Dictionary of Economics* was being developed in the United Kingdom, the editors approached Friedman to write an entry on the quantity theory of money. He accepted the assignment and praised the *New Palgrave* project, which took some inspiration from a vintage (1910) economics dictionary but also aspired to be authoritative for modern readers: it

[60] *Wall Street Week,* Maryland Public Television, November 15, 1985, p. 9 of transcript.
[61] Ammer and Ammer (1986, p. 385).

sought essay-length subject entries that would be useful for economic researchers and would appear in a multi-volume set. John Eatwell, who was one of the coeditors of the *New Palgrave* dictionary, recalled of Friedman: "He loved the idea. He was one of our most enthusiastic 'first offers.' He not only wrote the ['Quantity Theory'] piece, [but also] he volunteered to write pieces on a couple of people [James Laurence Laughlin and Simon Newcomb] we'd never heard of. He was really enthusiastic. We had very warm correspondence with him. He was just great." (Lord (John) Eatwell, interview, January 4, 2015.)[62]

For his "Quantity Theory of Money" entry, Friedman produced a sprawling 78-page typescript, dated October 25, 1985.[63] He later turned to the task of paring back the manuscript, relying in part on suggestions sent to him by U.K. academic Nigel Duck, whom Friedman had sent a copy of the draft.[64]

The fact that he had produced such an ambitious work in response to the *New Palgrave*'s assignment, as well as other ventures (discussed in the next chapter) that Friedman made in the research world during 1985–1986, sent a clear message that he was not willing to be counted out. Similarly, the title given to a November 1985 episode of *Wall Street Week*—one in which Friedman appeared as a studio guest—offered a challenge to any perception that, post-heart attack, he had left public life: "The Return of Milton Friedman."

II. ISSUES RELATED TO DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1982–1986

MONETARY DEVELOPMENTS: THE YEARS OF CONFUSION

In a March 1984 television appearance, when recounting the course of monetary developments over the previous two years, Friedman interrupted himself, remarking, "I'm getting confused…"[65] Although he quickly regained his train of thought, Friedman's passing reference in the interview to his being in a confused state would serve as an apt verdict on much of the running commentary on monetary matters that he provided over the years from 1982 to 1986.

[62] For Friedman's entries on Laughlin and Newcomb were Friedman (1987f, 1987g). The dictionary included entries for selected living economists provided that they were age seventy by the 1985 date at which the project was in full swing. Alan Walters (1987) wrote Friedman's entry. Anna Schwartz, who turned seventy on November 11, 1985, just reached the age criterion, and her entry was written by her NBER colleague Robert Lipsey (1987).

[63] Friedman (1985d).

[64] Research by Duck cited in Friedman's *New Palgrave* entry (see Friedman, 1987d, p. 16), later published as Duck (1988), had applied some of Friedman and Schwartz's (1982a) specifications to cross-country data.

[65] *The MacNeil/Lehrer News Hour*, PBS, March 27, 1984, p. 4 of transcript.

Some of what Friedman said on U.S. monetary developments in these years would prove durable. But much of it would not, and Friedman himself would discard a good deal of it in 1986 upon conducting a new review of the evidence provided by U.S. monetary and economic data for the decade so far. By the time of that review, he had contradicted himself publicly multiple times, and made numerous zigzags, over the course of the various monetary policy commentaries that he had made since 1982. And—underestimating the degree to which the experience of the 1970s was applicable to unfolding developments—during the years from 1982 to 1986 Friedman had made gloomy and well-publicized forecasts regarding output growth and inflation that would be proved wrong.

The result of this behavior was the development—from 1982 to 1984—and then the reinforcement—especially in 1985 and 1986—of the widespread impression that the relevance of Friedman's monetary analysis for the U.S. economy had been undermined by events. Reinforcing this impression was the fact that the period from 1982 to 1986 saw a change in who had the upper hand in the long-running battle between central banks and monetarists. Monetarists became the side that was decidedly on the defensive.

An early setback

An early sign of the tumble that Friedman's reputation was going to take during the years covered in this chapter came after he launched a detailed critique of the Federal Reserve's open market operations. In his Money, Credit, and Banking lecture of July 5, 1981 (discussed in the previous chapter) and in a *Newsweek* column appearing late in the following month (in the edition of August 31, 1981), Friedman complained that the Federal Reserve was engaging a large number of redundant open market operations. The gross volume of open market operations, he complained, was being steeply inflated by "defensive" open market operations, or "churning," when a much smaller amount of open market purchases was required to deliver a specified volume of reserves. Both in the lecture and the column, Friedman contended that Federal Reserve securities transactions were of a size equal to one-quarter to one-half of all other market transactions in government securities.

The lecture appeared in print in early 1982. Two Federal Reserve economists, Fred Levin and Ann-Marie Meulendyke, were surprised by the claims regarding churning that Friedman had made in the article. Both of them had both been graduate students at the University of Chicago in the late 1960s, but their subsequent experience at the Federal Reserve, including at the Federal Reserve Bank of New York trading desk in the Open Markets Operation Function section, had led them to differ from Friedman in their attitude toward Federal Reserve operating procedures. And their knowledge of the details of open market operations indicated to them that Friedman was simply factually mistaken on the size of the churning, his error having likely resulted from a misreading of published tables. They duly submitted a comment on Friedman's paper to the *Journal of Money, Credit and Banking*, in which they indicated their desire "to set the record straight" (Levin and Meuldendyke, 1982, p. 399).

This comment appeared, with Friedman's reply, in the August 1982 issue of the *JMCB*, six months after the publication of the original lecture in the same journal. Friedman's response acknowledged the validity of Levin and Meulendyke's correction—"I goofed," he acknowledged.[66] He further indicated that, after taking into account the critique, he would now put Federal Reserve securities purchases at below 10 percent of open market transactions, rather than his lecture's estimate of 25 to 50 percent.[67]

Levin and Meulendyke's comment also took on Friedman on numerous other points made in the lecture. On these, he declared himself "completely unrepentant," and he proceeded to concentrate on these points for the rest of his reply.[68] Although Levin and Meulendyke felt that he was mistaken on some of these points, too, the exchange did not go into a new round.[69]

Even in the absence of any follow-up, this rare instance of a direct debate in print between Friedman and Federal Reserve economists would mark something of a turning point. Up to now, when the Federal Reserve was perceived as engaging in a public confrontation with Friedman, as in the Burns/Friedman public statements on inflation in 1973–1974, the Federal Reserve had come out second best. So, in the 1960s and 1970s, Friedman had most of the momentum, and it had largely been a case of the Federal Reserve had played catch-up—initially reacting defensively to Friedman's critiques and then adjusting its analysis and policy in light of those critiques. From the summer of 1982, when his exchange with the Federal Reserve Bank of New York economists appeared in print, the tables were turned. Friedman was increasingly being

[66] Friedman (1982e, p. 404). This would not quite be Friedman's last contribution to the *Journal of Money, Credit and Banking*, as he published a note on monetary-growth variability (see Friedman, 1983h). He continued to be listed as a member of the journal's advisory board (which was not really a true, in-being board) until the May 1997 issue.

[67] Friedman (1982e, p. 404).

[68] Friedman (1982e, p. 404).

[69] Levin took a private-sector position later in 1982. Meulendyke noted that writing the original comment in the first place was different from most of their day-to-day work and that "working at a Federal Reserve, you don't work seven hours a day and have free time the rest of the time. You have other stuff to do. And so I think we probably didn't feel like having an ongoing back-and-forth with Milton Friedman. You know, both Fred and I really respected him." (Ann-Marie Meulendyke, interview, April 29, 2013.)

brought to account for his monetary analysis by others.

The fragility of support for monetary targeting

The course of 1982 would be marked by long-remembered developments that moved U.S. monetary policy decisively away from key prescriptions associated with Friedman.

As indicated at the start of the chapter, the month of May 1982 was something of a peak in the public perception of Friedman as an influence on the course of national policy. In the area of the conduct of monetary policy, the imprint he had made in the United States and elsewhere was registered in the fact that a conference—not attended by him—was held by the Federal Reserve Bank of New York titled *Central Bank Views on Monetary Targeting*.

But though the fact of the conference itself was testament to Friedman's influence, elements of the proceedings of the conference provided signals of the breaks of central banks' practices from monetary targeting that would characterize the coming years. In particular, a remark that senior Federal Reserve Bank of New York official Richard Davis made at the conference proved to be prescient about the direction in which matters would go at the U.S. policymaking level, too. Davis (1982, p. 69) suggested that he already perceived "the first signal that the high-water mark of 'monetarism' in the industrialized world may be passing."

Some signals in this direction were already apparent in the mood within the Federal Reserve and in financial markets during 1981, as discussed in the previous chapter. But more decisive moves away from Friedman's prescriptions would start in the summer of 1982. Over the course of the 1970s, and especially in October 1979, the FOMC's operating and targeting practices had undergone significant shifts that were influenced by monetarists' calls for reform. In 1982, however, significant backtracking occurred in the conduct of U.S. monetary policy, and this process of shifting from monetarists' recommendations—and toward a return to interest-rate-centered policies driven by data other than monetary aggregates—would continue beyond 1982.

This is not to say that the course of events in U.S. monetary policy that took place in 1982 was a case of displacing arrangements that monetarists were themselves enthusiastic about. On the contrary, leading monetarists, including Friedman, were far from happy with the policy *status quo* prevailing at the start of 1982. As Friedman noted in midyear—during what proved to be the last days of the New Operating Procedures regime—"I have been sharply critical of Federal Reserve policy both before and after" October 1979 (*National Review*, July 23, 1982).

To outside appearances, however, Friedman was continually making negative remarks about Federal Reserve policy, with its 1979 and 1982 policy changes seemingly having little impact on his negative stance. A closer look at his commentaries does make clear Friedman's appreciation of the fact that the end of the New Operating Procedures in 1982 moved the Federal Reserve still further away from monetarist recommendations on the operation of monetary policy. He was also cognizant of the fact that the Federal Reserve's attitude to its intermediate target of monetary aggregates also changed materially at the same time. Indeed, monetary aggregates, once deemphasized in 1982, would never regain their previous prominence in U.S. monetary policy. Friedman was highly critical of the 1982 policy changes, and the years from 1982 to 1986—a period that most retrospectives, and indeed many contemporary accounts, saw as one that vindicated Volcker's 1979–1982 restraint, with the U.S. economy moved into a long period of noninflationary expansion—saw Friedman being as critical of Federal Reserve policy decisions as he had been in the 1970s. Furthermore, it would see him, until almost the end of the period, predicting a return to 1970s levels of inflation and suggesting that a new, corrective recession would soon be required.

Although they are obscured by the vast volume of negative statements made by Friedman from 1982 to 1986 about the U.S. central bank, it is possible to discern some favorable remarks that emanated from him over this period. One prominent example, discussed in the next chapter, came in the form of the applause he gave to the Federal Reserve for its part in the official rescue of the Continental Illinois Bank in mid-1984. A second example—to be considered further below when disinflation is discussed: By spring-summer 1982, Friedman was willing to acknowledge that the first two years of the post-October 1979 operating regime had seen a reduction in the average rate of monetary growth—and that the policy tightening was manifested in the decline in inflation from 1981 to 1982.[70]

A third example, to which the analysis below will also return, related to Friedman's long campaign to have the arrangement in which U.S. commercial banks were subject to lagged reserve requirements be replaced by a system of contemporaneous reserve requirements. His agitation on the matter dated back to the early Arthur Burns years. But Friedman became more active on reserve-accounting reform during the New Operating Procedures era. Furthermore, the criticisms that Friedman and other monetarists had made of lagged reserve accounting seemed to

[70] See *Meet the Press*, NBC, March 21, 1982 (pp. 7, 9 of transcript), Friedman (1982a, p. 58), and his *Newsweek* columns of July 12 and August 23, 1982. In addition, Friedman stated later in 1982: "The average monetary growth over the past two years has been lower. And it has succeeded in bringing inflation down. That is something that you have to give them [the Federal Reserve] credit for." (*The News and Courier* (Charleston, South Carolina), October 26, 1982, p. 13.)

be getting a high-level official response. During 1980 and 1981, the Federal Reserve had made a series of efforts, which Friedman considered too leisurely and too deferential to the perspective taken by leading commercial banks, to see if a move back to contemporaneous reserve accounting (CRA) was feasible and desirable.[71] This led, as discussed below, to a policymaker decision in June 1982 to reintroduce CRA. But Friedman was highly critical of a subsequent postponement of the planned CRA implementation date from May 1983 to February 1984 (*Newsweek*, December 27, 1982). Indeed, writing in late summer 1983, he was still expressing doubt that the announced change would actually proceed at all.[72]

Once, however, the move to CRA actually did occur on the new February 1984 date, Friedman would make a material change in his characterization of the Federal Reserve's attitude to monetary stability—and the change was in a favorable direction. He had previously suggested that he did not believe that the Federal Reserve really cared about achieving within-year stability in monetary growth (*Wall Street Journal*, February 1, 1982). After the move to CRA was announced but had yet to be implemented, Friedman granted with the regard to the Federal Reserve that "its repeated protestations that it was committed to a steady and moderate rate of monetary growth" might be sincere (*Newsweek*, December 27, 1982). And in March 1984, after the Federal Reserve had indeed reinstituted CRA, he went further, saying he had "no doubt they're trying" to achieve stable monetary growth (*The MacNeil/Lehrer News Hour*, PBS, March 27, 1984, p. 4 of transcript). In the same remarks, he promptly proceeded to criticize actual monetary policy performance. But even on this point he was capable of offering praise: in a panel appearance in Nevada in midyear, Friedman lauded the actual monetary-growth rates recorded so far in 1984 so far: "unaccustomed as I am to saying anything good about the Federal Reserve, I regard the approximate[ly] 7 percent rate of monetary growth since November 1983 as a reasonably appropriate rate of growth for the current period."[73]

Notwithstanding these instances of moving to a less critical stance, Friedman was predominantly negative in the posture he took toward the Volcker Federal Reserve in the years from 1982 to 1986. This was true not only for the last three-and-a-half years of that period—those in which Volcker was widely perceived as moving away from the monetarism-associated practices of his first three years in office. It also included the first six months of 1982. These six months

[71] Friedman (1981f; 1982a, p. 59) criticized the opposition of the large commercial banks to the reinstitution of contemporaneous reserve requirements.

[72] See Friedman (1984b, p. 39). A passage on page 59 dated the rewriting of this paper to August 1983. This was a substantial redrafting and finalization of an initial version of the paper that Friedman circulated in early April 1983 (including as an attachment to a letter sent to David Lindsey dated April 7, 1983, in Federal Reserve Board records).

[73] In Heller and others (1984, pp. 42–43).

essentially comprised the remainder of the 1979–1982 New Operating Procedures regime—an era that would come to be judged as having ended in the late summer and fall of 1982.

Monetarism versus the Federal Reserve's control methods

One point that Friedman was at pains to emphasize both during the tailend of the New Operating Procedures period and in the years thereafter was that he regarded the policy pursued starting in October 1979 as being far from monetarist. "The Fed uses monetarist rhetoric while pursuing a nonmonetarist policy," he wrote in mid-1982 (*National Review*, July 23, 1982). By rhetoric, Friedman was primarily referring to the fact that Federal Reserve official statements concerning monetary control had parallels with the monetarist literature in stressing restraint in the provision of reserves to the commercial banking system. But Friedman maintained that the difference from monetarist prescriptions regarding policy operations had remained substantive after October 1979.

As indicated in previous chapters, Friedman did not deny that there had been a change in operating procedures at that date. The Volcker Federal Reserve had, he acknowledged, indeed, "changed its operational goal from controlling interest rates" (*Newsweek*, August 23, 1982). But he remained highly dissatisfied both with the form of its control procedure and with the actual results, in terms of the record on money supply growth since October 1979. With regard to the results, he repeatedly noted that "monetary growth over short periods has become much more erratic" (*Newsweek*, August 23, 1982). And he traced this outcome in large part to the constellation of monetary-control arrangements on which the FOMC and Board had settled in October 1979. There had been material alterations to its operations made by the Federal Reserve at that time, he granted—the 1979 shift was not, as some had alleged, a smokescreen—but "the actual changes it made were inadequate to achieve its objective" as debilitating to its success.[74] So although there had been substance in the October 1979 move, what Friedman considered "the changes in procedures and regulations required to make the new method effective" in terms of improved control of monetary aggregates had not actually been taken (*Wall Street Journal*, February 1, 1982).

Most central—what Friedman called the "chief mistake" in the setting up of the New Operating Procedures was the long maintenance of lagged reserve requirements: he believed that these should have been abolished in favor of CRA as soon as the Federal Reserve adopted its new

[74] From Friedman's letter to Senator Roger W. Jepsen of August 16, 1982 (Friedman 1982d, p. 74).

procedures in October 1979.[75]

Friedman's emphasis on the desirability of CRA went alongside his conviction that, even under the system of lagged reserve requirements, it was in the power of the Federal Reserve to have generated a far smoother trajectory of monetary growth than that seen since October 1979 (*Wall Street Journal*, February 1, 1982). His grounds for this contention lay partly in his view that other changes beyond CRA—among them simplification of reserve requirements and the linking of the discount rate to a market interest rate—could, had they been instituted, have contributed to improvement in the achievement of the monetary targets.

But, as was discussed in the previous chapter with regard to 1980–1981 monetary developments, Friedman and other monetarists believed that, even in the existing set of arrangements, in the setting of its most routine tool—open market operations—the Federal Reserve did not seem to be choosing the purchase sequence most consistent (on the basis of information available at the time of the purchase decision) with delivering period-to-period stability in the monetary aggregates. The previous chapter discussed the fact that studies of the details of FOMC choices in 1980–1981 indicated that it could have done more to steer the overall level of reserves to the levels most consistent with the monetary-aggregate target and that, instead, the Committee made decisions that implied a different total-reserves outcome from this target-consistent path.[76] This was a point Friedman stressed in the mid-1980s, when he noted that the monetary-growth overshoot in the period through mid-1981 was a protracted miss that was due to reasons beyond imperfect control methods—and also stated that, even under current control arrangements, the notable three-month and six-month deviations of the money stock from target in 1981 and 1982 had similarly been avoidable.[77]

In this connection, Friedman acknowledged in August 1982, as he had previously, that the Federal Reserve nonborrowed-reserves procedure was intended as a means of controlling total reserves. But in practice, considerations other than delivering the overall levels level most consistent with immediate achievement of the monetary target played a part in the setting of nonborrowed reserves. Policymakers continued in 1982, as they had done since 1979, to decide on their provision of reserves through open-market operations on the basis of multiple considerations. They certainly were not following a policy that generated a nonborrowed-

[75] From Friedman's letter to Senator Roger W. Jepsen of August 16, 1982 (Friedman 1982d, p. 74).

[76] Even during the period when lagged-reserve-requirements were still in force, Baumol and Blinder (1982, p. 241) noted: "The Fed can now control bank reserves with great precision."

[77] Friedman (1984b, p. 39).

reserves, and hence total-reserves, trajectory that, on the basis of information available, would be most consistent with meeting monetary targets.

In his July 1981 lecture, Friedman had suggested that the Federal Reserve implement its reserve provision in a more rule-like manner: by breaking the year into blocks, and at the start of each block make an estimate of the money multiplier level likely to prevail over that period, then adding reserves or base money evenly over the period by the amount consistent with the monetary-aggregate target, conditional on a projected money multiplier for that period.[78] With regard to monetary developments in 1982, the exercises in Hafer and Hein (1983) suggested that a policy along these lines—choosing the most monetary-target-consistent reserves level or base-money level, conditional on a money-multiplier estimate for the forthcoming period—would indeed have kept M1 close to target.

The availability of this more straightforward approach to monetary control underlay Friedman's statement there was "an alternative policy which would work" (*Barron's*, October 25, 1982, p. 7) in achieving monetary targets in the post-1979 period. In addition, as it differed not only from the policy pursued under the New Operating Procedures but also from the permissive approach to the supply of reserves associated with the old funds-rate procedure, Friedman would contend that his preferred policy would have produced "considerably better" performance in terms of stability in monetary growth in comparison with the pre-1979 period.[79]

What became Friedman's overarching objection to monetary policy in 1979−1982 was the perception that Volcker's policy coincided with Friedman's prescription. Those years, Friedman granted, had come to be seen as those of "the so-called 'monetarist' policy of the Federal Reserve."[80] Friedman was frustrated by the fact that, although he believed that a determined effort at managing reserves would be effective in avoiding protracted undershoots and overshoots of a monetary-growth target, such a policy was, after 1982, unlikely to be adopted as it was perceived as being essentially the same as—and its introduction was ruled out by the

[78] Friedman (1982c, p. 117). See also Friedman (1984b, p. 40). His specific proposal in these two articles involved total security holdings (Friedman, 1982c, p. 117; 1984b, p. 40) or the monetary base (Friedman, 1982c, p. 101), but in those presentations and in others over this period he was also very receptive toward total reserves being the policy instrument (see for example *Wall Street Journal*, February 1, 1982; Friedman, 1982c, p. 101, 1984b, p. 39). As these proposals were made mainly alongside a prescription that a bank-deposit-inclusive monetary aggregate be targeted and that a projection of the money multiplier be made, they recognized the need to forestall or offset depletions in the stock of reserves associated with a rising volume of currency in the hands of the nonbank private sector.

[79] Friedman (1985b, p. 58).

[80] Friedman (1985b, p. 57).

widespread dissatisfaction with—"the 'misleadingly labeled 'monetarist experiment''" of 1979–1982.[81]

Because it fell short of the concerted quantity-based efforts to achieve the monetary targets that he advocated, Friedman deemed the 1979 change in policy to have been "cosmetic."[82] His label needs to be interpreted with caution—as already implied, "cosmetic" did not mean a smokescreen-like or chimerical policy change.[83] The cosmetic element lay crucially in the fact stressed above (and discussed further below when Volcker's own attitude is considered) that the Federal Reserve in 1979–1982 did not adopt policy settings in which its criterion for decisions was invariably delivering better short-run control of monetary growth. But the 1979 change in operations nonetheless had content, in the sense that it helped lead to substantially different patterns of policy behavior from those associated with the pre-1979 Federal Reserve. Friedman's key complaint was that, although "far-reaching," the 1979 shift, and the related changes in policymakers' reaction function, "did not go far enough to enable the new procedures to produce the desired results." (*Newsweek*, February 15, 1982.)

Short-run fluctuations in money

Indeed, in terms of the short-run behavior of monetary aggregates, the practical outcome of the New Operating Procedures was actually in the opposite direction of delivering more stability. Friedman explained in a *Newsweek* column on monetarism appearing close to his seventieth birthday (*Newsweek*, July 12, 1982): "Since the Fed adopted monetarist rhetoric on Oct. 6, 1979, monetary growth has been more volatile than in any comparable preceding period." On that occasion, he cited the Federal Reserve's recent approval of the reintroduction of CRA as a "hopeful augury" and so ended his column on a positive note. But soon it became clear that the move to CRA would not occur at all promptly, and Friedman, as well as condemning the delay of CRA to February 1984, declared that "Federal Reserve monetary growth in the past two-and-a-half years has swung widely from one side to another" and that he saw "absolutely nothing" in prospect to break that pattern (*Barron's*, October 25, 1982, p. 7).

Supporting Friedman's doubts about whether monetary-growth stability was a high priority of the Volcker Federal Reserve was the two-track character of Federal Reserve statements.

[81] Friedman (1985b, p. 57).

[82] *The MacNeil/Lehrer News Hour*, PBS, March 27, 1984, p. 4 of transcript.

[83] Earlier chapters (Chapters 10 and 12) have discussed Friedman's acceptance of the fact that the 1979–1982 regime did involve a material shift in policy operations.

Certainly, the FOMC set monetary-aggregate targets, and many of its records of policy actions over the period, as well as subsequent estimates of its reaction function, indicated a willingness to adjust its policy instruments with the aim of reducing deviations from the monetary targets. But running parallel with this tendency, even in 1979–1982, were statements by high officials that, in effect, conceded that higher-frequency stabilization of monetary growth was feasible but that cast doubt on the value of achieving this improved control of the money supply. Examples of such statements during the 1980 credit-controls period were given in the previous chapter. The same policymaker sentiment transcended that special episode, however. For example, at a hearing held on February 23, 1982, Chairman Volcker testified that, in the short run, the money stock was heavily affected by the private sector's balance-sheet decisions, and that there was little to be gained in monetary policy endeavoring to offset these fluctuations. "I conceive our job of trying to keep [the money stock] on an appropriate trend over a period of time… [If] we use or develop techniques to enforce more rigidity on the money supply in the short run, I think it is pretty clear [that] we would have more interest-rate instability."[84] Such statements were consistent with the reality that while the instability of within-year monetary growth was not an intentional aim of the FOMC in 1979–1982, neither was avoidance of it an overriding priority.

For Friedman, this reality, and the monetary variations it permitted, disqualified the post-1979 policy from being categorized as monetarist. He argued in October 1982: "Monetarism in the sense in which it has always been intended for policy purposes—that is, a very steady rate of monetary growth—has not been tried." Friedman conceded nevertheless that "it is widely perceived" that monetarism *had* been tried over the previous three years by Volcker and his colleagues (*Barron's*, October 25, 1982, p. 7). A couple of months earlier, he had written to a Congressional questioner that he might as well not be called a monetarist if monetarism was the policy associated with the New Operating Procedures: "An absolutely essential feature of a monetarist policy is steadiness in the rate of monetary growth. Since October 1979, monetary growth has been more unstable than in any other comparable period that I know about in the whole history of the Federal Reserve System. If this be monetarism, I am not a monetarist."[85]

The yo-yo economy

Before much data for 1982 on monetary growth had been racked up, Friedman had published a long op-ed in the *Wall Street Journal* titled "The Federal Reserve and Monetary Instability"

[84] In Committee on Ways and Means, U.S. House of Representatives (1982c, p. 380).

[85] From his letter of August 16, 1982, to Senator Roger W. Jepsen, in Friedman (1982d, p. 73).

(February 1, 1982). This piece presented an updated version of his *Newsweek* column of the previous December 28, which had documented the swings—whose most typical length was around ten weeks—of monetary growth since October 1979. As before, the reliance on weekly data meant that Friedman concentrated on M1 behavior—although, as discussed in detail below, by early 1982 he considered this focus to be a virtue, having become temporarily convinced that the modern M1 series was a more reliable aggregate than M2.

In London a week later, Friedman's former student Samuel Brittan gave readers of the *Financial Times* a mostly-critical account of the *Wall Street Journal* op-ed. Brittan's attitude was shaped by the fact that he had become convinced that the Volcker Federal Reserve was getting a raw deal from its monetarist critics and was, in fact, behaving more conscientiously and systematically than Friedman was suggesting. Indeed, in the same week in which Friedman's *Wall Street Journal* critique had appeared, Brittan had penned a *Financial Times* column titled "Here's to U.S. Monetary Policy" (February 4, 1982). In his subsequent piece on Friedman's op-ed, Brittan added to his defense of the Federal Reserve by suggesting that Friedman was not adhering to the spirit of his prior research writings. Brittan insisted that, in its concentration on high-frequency movements, Friedman's op-ed marked a break from the message of those writings. The Friedman he knew, Brittan maintained, would not have concentrated on such short-run fluctuations in money or inferred significance for the course of future national economic activity from such fluctuations (*Financial Times* (London), February 8, 1982).

This criticism, although an understandable reaction by someone like Brittan who had first been exposed to Friedman's monetary work in the early to mid-1950s, was not really well taken. It was true that much of Friedman's research had considered annual data or even lower frequency (and would continue to do so, in the soon-to-be-published *Monetary Trends* with Schwartz). But he had in 1970 signaled that the nature of the economic data that had accrued in the postwar period in the United States was different, as it lacked the "sharp ups and downs" of the interwar period—and that he was prepared, in that light, to concentrate to a greater degree on fluctuations that took the form of within-year movements, as well as put greater emphasis on episodes of slow rates of growth, rather than absolute declines, in real and monetary series.[86] The February 1982 op-ed, like many other analyses Friedman had made of the post-Accord Federal Reserve, was consistent with this perspective that "the ups and downs of the economy depend on the shorter-term movements of monetary growth." (*Commodities*, January 1983, p. 50.)

[86] Friedman (1970f, p. 16).

Brittan later (in *Financial Times* (London), March 21, 1983) advanced a related rebuttal to monetarists' criticisms of the Volcker years: "The so-called instability of U.S. monetary growth disappears if one looks at annual averages." In the same vein, in a paper presented at the Bank of Japan conference that Friedman attended in June 1983, Robert Gordon (1985, p. 64) found that the standard deviation of four-quarter M1 growth had risen only slightly, from 1.3 percent in 1971:Q1–1979:Q3 to 1.5 percent in 1979:Q4–1982:Q4.[87]

Again, however, Friedman's perspective was that, for the purpose of the study of within-year output and nominal income fluctuations, it was legitimate to look at within-year monetary growth fluctuations. As already indicated, he expressed some approval of the *average* rate of monetary growth delivered from October 1979 to the spring of 1982. But he viewed this verdict on average performance as consistent with his criticism of high post-1979 variability: His indictment lay in the fact that "over short periods, monetary growth has been excessively volatile, rising and declining at very rapid rates for periods of 10, 11, 12 weeks" and also that these had been "accompanied by longer-period ups and downs" such as the mid-1980 to mid-1981 surge and the two more recent six-month bouts of weak monetary growth (*Barron's*, October 25, 1982, p. 7). The various ups and downs had balanced out to deliver what Friedman acknowledged was roughly appropriate monetary growth on average since October 1979. But as he had put it in his *Wall Street Journal* and elsewhere, if a river averaged five feet in depth, that was no guarantee that a six-foot person could walk through it safely.[88] Friedman further argued that the fluctuations he was considering were long enough to matter: consideration of "three-month periods… is a very different thing" from a concentration on week-to-week movements in M1, he noted (*Newsweek*, June 28, 1982).

Friedman saw this within-year monetary variability as having an important bearing on the variability of U.S. real economic activity. He believed that this position had been borne out by the fact that, during the New Operating Procedures period, "you've had a series of economic moves out of line with past experience" (*Barron's*, October 25, 1982, p. 6). In particular, Friedman remarked, "the pattern of the last two-and-a-half years has been totally different from the pattern of the earlier postwar period" (*Commodities*, January 1983, p. 51). Actually, by the time he made this last observation—the end of 1982—the new experience of more sizable fluctuations in economic activity had spanned about three years. But Friedman's diagnosis for

[87] See also Gordon (1983, pp. 79, 84). The corresponding numbers implied by the modern data vintage are similar: 1.4 percent and 1.6 percent for the standard deviations of one-quarter annualized and four-quarter M1 growth rates, respectively.

[88] See *Wall Street Journal*, February 1, 1982, and Friedman (1982a, p. 58).

the post-1979 output instability remained the same as in August 1982, when that experience really was about two-and-a-half years in length. At that time, in a letter written for the public record, he had stated: "most of the sharp up and downs in real output have been the consequence of inappropriate changes in the rates of monetary growth. They are a result of bad monetary policy…"[89] In the same vein, Friedman urged in a column written during this period that it was the course of monetary policy—and not the fiscal policy developments that often got more attention in newspaper headlines—to which one should look for an understanding of "the recent sharp ups and downs in the economy" (*Newsweek,* August 23, 1982).

From mid-1981 to December 1982, the condition of the U.S. economy was mostly "down." As the early part of 1982 proceeded, it was clear that the United States was back in recession, even though the criterion of two successive negative growth rates in aggregate real output would not be met until the registering of the back-to-back observations of 1981:Q4 and 1982:Q1.

For his part, Friedman's perspective on matters was that the U.S. economy had never left recession since 1980 or even 1979.[90] In practice, of course, on many occasions he deferred to the standard categorization of recent experience as amounting to a recession-expansion-recession sequence. But he also referred disparagingly in a talk in March 1982 to "the so-called twelve-month expansion" of July 1980 to July 1981.[91] And, later in 1982, Friedman took the unusual step of publicly criticizing the NBER's dating committee for its designation of 1980−1981 as a period of recovery (*Purchasing's Hotline*, November 18, 1982). The single-recession classification scheme that he applied to this period remained the nonstandard one. It has nevertheless received considerable support over the years, including in Barro's (1984, p. 218) decision to "consider 1980−82 as one prolonged contraction" and in Furman and Powell's (2022) exclusion of July 1981 from the list of business cycle peaks since 1953.

Consistent with Friedman's classification of the early 1980s as one long recession, the cumulative behavior of U.S. output over the period was one of decline or stagnation. Indeed, on data prevailing as of early 1983, average real GNP in 1982 was about four billion dollars (in 1972 dollars) lower than its 1979 average value, and the (annualized) level of quarterly real GNP in 1982:Q1 was about 25 billion dollars (again, in 1972 dollars) below its value two years earlier (Council of Economic Advisers, 1983, Table B−2, p. 164).

[89] From his letter of August 16, 1982, to Senator Roger W. Jepsen, in Friedman (1982d, p. 73).
[90] Friedman (1984b, p. 36)
[91] Friedman (1982a, p. 58).

Nevertheless, Friedman by no means denied that there had been spurts of economic strength since 1980. In fact, as discussed in the previous chapter, he laid the blame at the door of the Federal Reserve for the disruptive nature of one of these spurts—on account of its allowing nominal aggregate demand to grow so rapidly in the second half of 1980 and into 1981, in a policy that, as Friedman saw it, let the U.S. economy zoom out of a short recession, canceling some of the anti-inflationary pressure created by that recession. He, in effect, granted the fact that things had not been all downhill for the economy in the contrast he made with the prior quarter-century of more spread-out cycles: "Since 1980, the pattern has been altogether different," Friedman remarked (*Barron's*, October 25, 1982, p. 6). "You have had short contractions, very short expansions, then again renewed contraction."

But as the Federal Reserve had resumed monetary restraint, and the U.S. economy went back into contraction, Friedman felt that, viewed as part of the experience as a whole since 1979, the spurts of output strength should not have been viewed as indications of *bona fide* recovery movements but, instead, as attesting to the enhanced volatility of the economy: "there is no precedent" in the post-World War II period for the brief recession-brief recovery-new recession pattern, he remarked: "the economy has been far more erratic… This is not a typical business cycle fluctuation." (*Wall Street Journal*, February 1, 1982.) Over a decade earlier, he had stated that he did not think that the United States had witnessed a "double bottom" economic contraction since 1932–1933 (Instructional Dynamics Economics Cassette Tape 85, November 3, 1971).[92] But the 1980 and 1981–1982 experiences had provided such an instance, and in October 1982 Friedman assessed that the preceding three years had seen essentially one "long drawn-out erratic recession" (*Barron's*, October 25, 1982, p. 7).

The previous February, Friedman offered an evocative label to the cyclical behavior observed under the nonborrowed-reserves regime—a label that allowed for the ephemeral periods of strength seen during the protracted downturn: "The yo-yo economy" (*Newsweek*, February 15, 1982). That yo-yo economy's course is brought out in Figure 1, which shows the period's volatility in nominal and real GDP by plotting the quarterly annualized growth of these series over a sample that includes the first half of the 1980s.

Although there were certainly other sources of turbulence over this period, with notable shifts taking place in both fiscal policy and oil prices, Friedman, as already stressed, was adamant that

[92] In that discussion and in Friedman and Schwartz (1963a, p. 324), both the "double bottom" terminology and its application to 1932–1933 were credited to Burns and Mitchell's business-cycle studies, including Burns and Mitchell (1946, pp. 82–83).

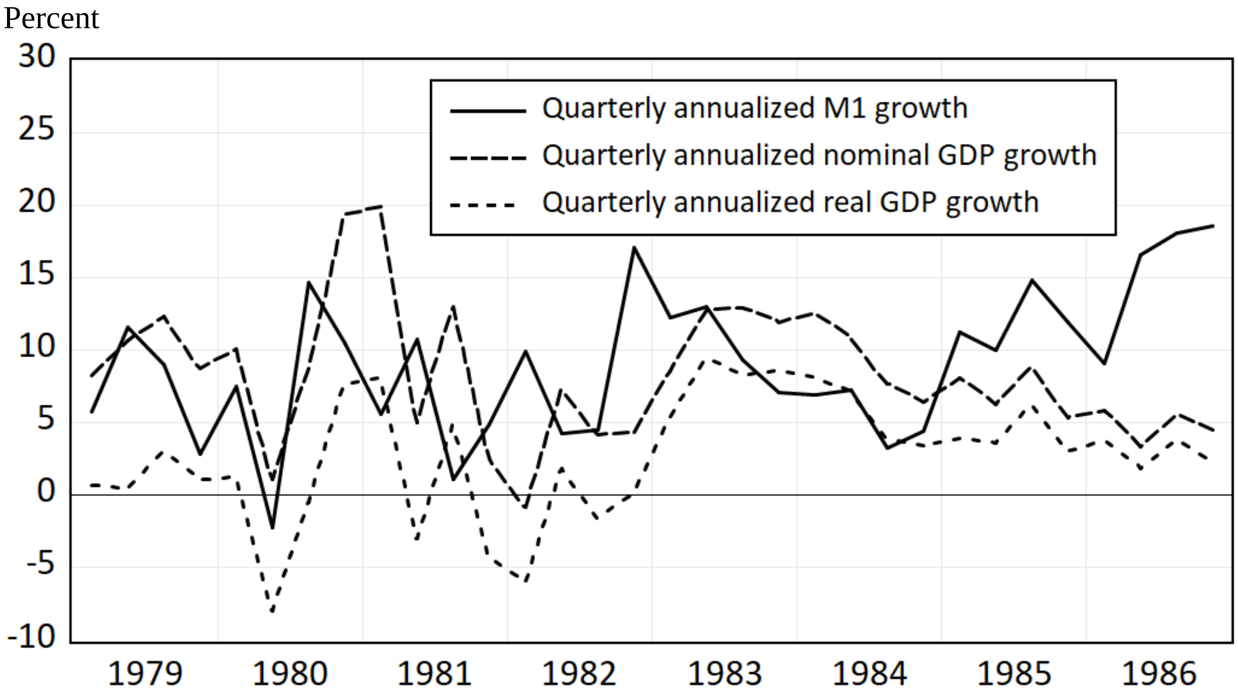

Figure 1. Quarterly annualized rates of nominal income, real income, and M1, 1979:Q1–1986:Q4.
Source: Federal Reserve Bank of St. Louis' FRED portal. Quarterly averages of M1 obtained from monthly data prior to computing growth rates.

the key source of the high economic variability since 1979 was what he described, in remarks in the city of Chicago in mid-1982, as an "extraordinarily erratic and volatile" Federal Reserve policy (*Chicago Tribune*, June 7, 1982).

In terms of measuring this variability in monetary policy, Friedman continued to look first and foremost at monetary aggregates and, in that light, in March 1982 he indicted "the unprecedentedly erratic performance of the Federal Reserve in respect to the quantity of money."[93] A key aspect of the present chapter's discussion is that, by this point of early 1982, and continuing through the mid-1980s, the M1 aggregate was what Friedman used as the main metric in judging monetary conditions. The annualized growth in the quarterly average growth rate of M1 growth is also plotted in Figure 1.

Friedman would later have cause to regret his reliance on M1. And the series, looked at in isolation, did not give a quantitatively reliable picture of monetary policy stance, or even of monetary aggregates' general behavior, in 1985 and 1986. One can nevertheless see good

[93] Friedman (1982a, p. 58).

reasons for Friedman’s focus on M1 in the earlier years of the 1980s: as the figure indicates, and as discussed further below, M1 growth in that period frequently foreshadowed the swings in aggregate nominal income and real output that would occur in the next few quarters. Although Paul Volcker argued on television that “I don’t think there is a body of economic analysis that suggests that that [monetary-growth variation] is important in terms of economic activity, unemployment, the rate of growth in the economy, all those things that really count,” the figure does suggest that much quarterly variation in M1 in this period had a counterpart in quarterly movements in national income.[94]

Friedman elaborated on the M1-variability/economic-variability link in mid-1983, when he used the occasion of his June 22 keynote speech at the Bank of Japan conference to provide his first research paper specifically concerned with U.S. monetary policy in the 1979–1982 period. By the time he produced this paper, he had the benefit of observations on money, income, and interest rates through the end of 1982 and slightly beyond—and so was able to make an early overall assessment of “the 1979–1982 pattern.”[95]

A good part of the analysis in the June 1983 keynote speech considered the issue of the monetary instability associated with the New Operating Procedures regime—an issue that Friedman had previously examined in various columns and op-eds during 1980, 1981, and 1982 via graphs and tabulations of growth rates for various subperiods. Although the keynote speech contained no data plots at all, it did contain, as its first table, an expanded and extended version of his summary of recent years’ developments.[96] A later table and accompanying text in the paper, in a section titled “Monetary Volatility,” turned to the matter explicitly in second-moment terms: specifically, variability as measured by the standard deviations of monetary growth and of growth in nominal and real income.[97] “This is a relationship that Anna Schwartz and I investigated for a period of close to a hundred years in an article published some two decades ago,” Friedman wrote, referring to February 1963’s “Money and Business Cycles.”[98] “I have subsequently extended that analysis.”[99] The write-up of this extension was reserved mostly for a

[94] Volcker made the remark on CBS’ program *Face the Nation* on February 14, 1982. See *New York Times*, February 15, 1982 (p. D2).

[95] The quotation is from Friedman (1983e, p. 10; 1984b, p. 30).

[96] Friedman (1983e, Table 1, p. 9).

[97] Friedman (1983e, Table 3, p. 12). The same table appeared in Friedman (1985b, Table 3, p. 57), that table’s numbers not having been recomputed in light of data revisions since 1983.

[98] Friedman (1983e) did not actually cite Friedman and Schwartz (1963b), but Friedman (1984b) did so when laying out his extension of the 1963 analysis.

[99] Friedman (1983e, p. 13). The same text appeared in Friedman (1985b, p. 58).

separate paper that Friedman worked on during the spring and summer of 1983.[100] But the keynote speech's table of standard deviations compactly summarized the enhanced instability in monetary growth after 1979: Friedman gave annualized quarterly M1 growth as having been 1.3 percent in the twelve quarters through 1979:Q3, and this had then risen to 4.7 percent in the twelve quarters to 1982:Q3.[101] This comparison was backed up by the quarterly evidence provided in Robert Gordon's paper for the same Tokyo conference. Gordon (1985, p. 64) found that the standard deviation of quarterly annualized M1 growth rose from 2.0 percent in the 1971:Q1–1979:Q3 to 4.7 percent in 1979:Q4–1982:Q3. Similarly, modern data show a rise in the standard deviation from 2.2 percent to 5.4 percent across these two periods.

Friedman's second-moments table in his 1983 talk also documented the greater post-1979 fluctuations in "economic growth"—the shorthand used in the table's title for growth in both real and nominal income growth rates.[102] His table showed the standard deviation of annualized nominal GNP growth rising from 3.7 percent in the twelve quarters to 1980:Q1 to 5.7 percent in the twelve quarters to 1983:Q1, with real GNP growth's standard deviation going up from 3.2 to 4.8 percent across the two periods. Likewise, Gordon (1985, p. 64) found that the quarterly annualized growth rate of nominal final sales (nominal GNP excluding change in inventories) had an increase in its standard deviation from 3.0 percent 1971:Q1–1979:Q3 to 4.9 percent in 1979:Q4–1982:Q4.

Interest rates and the 1981–1982 recession

One of the largest contributors to the high degree of economic variability experienced over 1979–1982 was the scale of the 1981–1982 recession. As well as seeing a major decline in real output, this was the only postwar U.S. recession in Friedman's lifetime that involved unemployment moving above 10 percent. Over 1982 and through the mid-1980s, Friedman would lay out his views on how monetary and financial factors that had contributed to the 1981–1982 recession's severity. In giving the basic story, Friedman, as already indicated, stuck to his usual mapping from movements in monetary growth to movements in nominal and real national income. Specifically, by early 1982, Friedman was persuaded by the behavior of M1 growth of the severity of the monetary policy tightening during 1981, and so he saw monetary

[100] See Friedman (1984b, pp. 31-35).

[101] In a presentation given at the end of the year, Friedman (1984c, p. 397) used slightly different dates and a later vintage of data to give a similar comparison. On that occasion, he reported the standard deviation of annualized M1 growth as having been 1.6 percent in the ten quarters through 1979:Q3 and then, in the subsequent ten quarters, rising to 5.6 percent.

[102] Friedman (1983e, Table 2, p. 12).

policy as behind the renewed (or deepened) recession that started in July 1981.

Friedman's discussions were notable, however, in the additional description they provided of how he believed the monetary slowdown had been transmitted to the national economy. In particular, Friedman was forthright about how important a role had been played by financial-market channels on this occasion. Specifically, he highlighted, as so many others did, the fluctuations in interest rates.[103] Friedman's discussions of the high variations in short-term interest rates put them in historical perspective: these were "gyrations without precedent" in the past century and more (*Newsweek*, February 15, 1982). And they came in the wake of a roughly fifteen-year period up to October 1979 which had been viewed at the time, correctly, as years of very large interest-rate fluctuations by U.S. standards. Yet since 1979, "rates have been far more erratic" even than that benchmark, Friedman noted in his *Wall Street Journal* analysis (February 1, 1982), adding: "I know of no prior example of the kind of interest rate fluctuations that have bedevilled the economy during the past two years." This emphasis on interest-rate volatility would get further impetus when, as discussed later, the first half of 1982 saw a notable new surge in market rates.

In these 1982 discussions of interest-rate experience, Friedman affirmed that a major channel through which he believed monetary growth variability since 1979 had been manifested in economic variability was via swings in U.S. financial market security yields. He posed and answered a key question in his *Newsweek* column on the yo-yo economy: "What accounts for this unprecedentedly erratic behavior of the economy? The answer that leaps to mind is the correspondingly erratic behavior of interest rates." (*Newsweek*, February 15, 1982).[104]

Notwithstanding, therefore, his heavy emphasis on monetary growth and its variability in his discussions of the early 1980s, Friedman's account of the economy's travails of 1979−1982 was basically consistent with many of the interest-rate-focused narratives that were advanced at the time, including Washington, D.C., correspondent Anatole Kaletsky's statement in the *Financial Times* (May 24, 1983): "In the U.S. itself[,] bond yields of over 15 percent and short-term interest rates of over 20 percent between late 1979 and 1982 flattened the domestic economy."

[103] A *New York Times* profile had erroneously claimed that Friedman believed that "interest rates… have a negligible impact on economic activity" (January 25, 1970, p. 22). This was never a correct characterization and likely stemmed from his frequent criticisms of using interest rates, particularly short-term nominal yields, as a summary of monetary policy stance. For further discussion of Friedman's views on the interest-rates/output relationship, see Nelson (2020a, Chapters 5 and 6).

[104] Likewise, Anna Schwartz remarked in April 1982 on the interest-rate benefits of a more stable monetary policy path: "Lower interest rates will set the stage for sustained real growth in the economy and a lasting reduction in unemployment." (In Women's Economic Round Table, 1982.)

Friedman's emphasis on the importance of interest-rate volatility for the 1979–1982 economic fluctuations was in harmony with his emphasis on monetary-growth variability, as he—in common with other monetarists in this period—believed that fluctuations in money were a key factor generating, not moderating, the heightened interest-rate variations. He contended that the fact that Federal Reserve policy had produced major variations in monetary growth served to "explain the wide swings in interest rates and the economy" (*Wall Street Journal*, June 28, 1982). In late 1983, with the benefit of more data, Friedman reaffirmed that "the new [1979–1982 monetary policy] approach produced enhanced volatility in monetary growth, and, as a consequence, in both interest rates and economic activity."[105]

On the numerous occasions when he split the 1979–1982 period into successive segments, Friedman brought interest rates in when considering each episode of rapid or slow monetary growth. His conclusion was that "the ups and downs in interest rates and in the economy… follow… the ups and downs in monetary growth" (*Wall Street Journal*, February 1, 1982).[106] Likewise, in midyear he stated flatly: "There is little doubt that the fluctuations in monetary growth account for the fluctuations in short-term interest rates." (*Newsweek*, June 28, 1982.)[107] He reaffirmed this view as further data on 1982 accrued, finding that movements in M1 growth had, after a lag of about a quarter, been associated with movements in the same direction in short-term interest rates, real GNP and nominal GNP (or in monthly proxies for aggregate economic activity).[108]

One difference in stress in his analysis from that in the past concerned the nominal/real interest-rate distinction. He had, before 1980, often deprecated high nominal interest rates as an indication of Federal Reserve tightness, as the corresponding real rates had, in fact, been low. That situation did not describe conditions in 1981–1982, however. Friedman (*Newsweek*, June

[105] See Friedman (1984c, p. 398).

[106] In these comparisons, Friedman highlighted a positive relationship between monetary growth and interest-rate movements a couple of months later. As discussed below, this was not inconsistent with the *contemporaneous* relationship between the two series being negative. Such an inverse short-run relationship would be consistent with monetary accelerations and decelerations initially generating a temporary opposite-signed response of interest rates as the liquidity-effect mechanism operated.

[107] In the previous year, Beryl Sprinkel had pointed to a table showing a link between monetary growth and movements in long-term interest rates (see his testimony of July 23, 1981, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1981c, p. 430). This fit in with Sprinkel's simplified story of the relation, one focusing on the Fisher response of interest rates. As already implied and as discussed further below, Friedman did not rely mainly on the Fisher relation in explaining the positive monetary-growth/interest-rate link, and his appeal to other mechanisms was appropriate in view of the fact that the link was still clearly evident when short-term interest rates, rather than long-term rates, were considered.

[108] See *Newsweek*, August 23, 1982, and Friedman (1983e, p. 9; 1984b, p. 29; 1985b, p. 55).

28, 1982) noted that, since 1980, U.S. real interest rates had turned "positive and attained levels rarely reached in the past."[109]

Nevertheless, Friedman's acceptance that in the early 1980s the United States' economic fluctuations clearly reflected the behavior of market interest rates went alongside a continued belief that expansionary monetary policy measures specifically intended to generate an immediate reduction in interest rates might be destabilizing for interest rates and the economy over longer periods (*Newsweek*, August 23, 1982).

Indeed, Friedman was not only not a newfound advocate of interest-rate centered policy, he also remained doubtful about the reliability of numerical knowledge concerning the linkage between interest rates (even adjusted for inflation) and aggregate demand. Even while acknowledging that the market interest rates and aggregate demand were related, he continued to believe that monetary growth recorded influences of monetary policy on spending that were not summarized in market interest rates (*Newsweek*, February 15, 1982). And he believed that the key to making output growth and interest rates more stable lay in making monetary growth more stable: "monetary instability, interest-rate instability, and economic instability—[they] are not unrelated. I believe they have all gone together."[110]

Reflecting Friedman's postulate—considered in detail later in this chapter—that monetary-growth variability raised the level of real rates, his position was that a more policy that delivered the same average rate of monetary growth over 1979–1982, alongside a much lower variance of monetary growth, would have reduced the level of interest rates and been associated with higher output as well as a more orderly, if slower, disinflation. But he did not associate any particular real interest rate with a specific state of the economy, and he still saw monetary aggregates as better summaries of Federal Reserve policy stance.

[109] Around this time, a key study by Mishkin (1981, p. 173) appeared of the empirical behavior of real interest rates. Alluding to a literature in which Friedman had been a leading figure, Mishkin noted: "Many economists have long warned that increases in nominal interest rates do not necessarily mean that money is tight because movements in the real rate might not be highly correlated with the nominal rate." Mishkin went on to note "even stronger" lesson that seemed to be implied by data through 1979: "an increase in nominal interest rates is associated with a lower real rate rather than the reverse. Thus[,] when nominal rates are high, it is more likely that we are in a period of "easy money[,]' with low real rates[,] than the contrary." This regularity, although it had indeed been an important feature of the U.S. through 1979, had been overturned by the time of the appearance of Mishkin's article. Nominal and real interest rates tended to move together under Volcker, including during the period of rising rates in the year to mid-1981.

Hence, in the mid-1980s, Schwert (1986, p. 284) could write that "the behavior of the real rate since 1979 is largely due to high and variable nominal rates."

[110] Friedman (1982a, p. 58). See also *Newsweek*, August 23, 1982.

Early 1982: a false dawn

"A year ago, the economic-forecasting community was nearly unanimous in predicting that the recession then underway would end by mid-1982," Friedman would remark in an early retrospective on the recession (*Newsweek*, February 7, 1983). His characterization was accurate: indeed, his own column a year earlier had noted the fact that "most forecasters" expected the recession that started in the second half of 1981 to be brief (*Newsweek*, February 15, 1982). Not only forecasters but also those in policy circles had also publicly expressed an expectation of a recovery in spring 1982 or shortly thereafter. Notably, Secretary of the Treasury Donald Regan referred in testimony of January 27, 1982, "that recovery in the spring that we are anticipating" (Joint Economic Committee, U.S. Congress (1982d, p. 248), and, in May, Congress' chief economist Alice Rivlin observed (American Society of Newspaper Editors, 1982, p. 130): "The hoped-for upturn in the economy still seems likely to us to occur—in the next few weeks."

But, as Friedman noted of the general expectation of a first-half-1982 trough: "That forecast was wrong." The 1981–1982 recession would, in the event, be an almost full-calendar-year recession as far as 1982 was concerned.

What Friedman did not mention in his 1983 retrospective was that he himself was one of those expecting a first-half-1982 expansion. "I believe [it] will be another short recession," he had stated at the time (*Wall Street Journal*, February 1, 1982). In late March, when he seemed to think that his expectation was being borne out, Friedman observed: "We may very well already have passed through the bottom of the recession. This is an area in which prediction is very unreliable… but I would not be surprised if, when business cycle theorists look back, they will say that January 1982 was the trough of this recession." (*The Vancouver Sun* (British Columbia, Canada), March 24, 1982.) By mid-year, in his July 12 *Newsweek* column he put a later date for the trough but still thought that it was in the past: "the recession probably bottomed in April 1982 and we are now in the early stages of an expansion."

Unlike some other forecasters of an early recovery, Friedman's projection did have the important rider that it was explicitly conditional on a sustained period of more rapid monetary growth. But this revival in monetary growth, although it did get started, proved ephemeral. Friedman's confidence in a short recession lay in the pickup in monetary growth that took place in late 1981 after the April-October 1981 stretch of an almost flat level of M1. He would give M1 growth as moving from an annualized rate of minus 0.2 percent in April-October 1981 to 15.3 percent in October 1981-January 1982 (*Newsweek*, August 23, 1982). Friedman saw the real data as responding to the change in the monetary climate in the form of the much-reduced rate of decline of industrial production in the first half of 1982. He also went beyond this reading to contend that "the very rapid monetary growth from October '81 to January '82… was indeed followed by a slight uptick in the economy" (*Barron's*, October 25, 1982, p. 7).[111] Supporting this view is the fact that industrial production on modern data actually shows a rise in one month of 1982 (February) before resuming its decline, which continued for the rest of the year. And real GDP data show a period of positive growth—in 1982:Q2—in between quarters of decline.

But the dating of the end of the recession to the first half of 1982 would prove premature. Friedman would contend that what he called a "putative recovery" (*Newsweek*, August 23, 1982) had not been allowed to proceed. He would regard a renewed shift to severe monetary restraint as being the culprit for having "cut short" the U.S. economy's exit from the recession (*Barron's*, October 25, 1982, p. 6). Specifically, Friedman cited a move down in annualized M1 growth from 15.3 percent in October 1981-January 1982 to only 1.3 percent from January to June 1982 (*Newsweek*, August 23, 1982)—a period of M1 weakness subsequently found to have continued through July 1982—as evidence that monetary policy had prevented a recovery from being able to take root.[112] Consequently, he viewed the 1981–1982 recession as including swings of the monetary/economic yo-yo: the initial decline, a short-lived uptick, and then a renewed downturn.

Breaks from the past in 1982: velocity and disinflation

Amid the criticism of the monetary volatility and of misses of the monetary targets—an

[111] Friedman's column (August 23, 1982) actually used the terminology of a "much lower rate" of decline—although this really meant a less negative rate of decline than previously.

[112] M1 growth of 1.3 percent in January to July 1982 was given in Friedman (1984b, p. 52), with page 39 of the same article characterizing this period as seeing one of the "monetary retardations" seen in 1979–1982. Earlier (*Barrons*, October 25, 1982, p. 6), he gave the annualized rate to six months to July 1982 rate as 1.2 percent. On modern data, the increase is 2.4 percent. The distinction between M1 in behavior up to January 1982 and subsequent months was underlined by the fact that Friedman (essentially referring to growth in the six months ending July) treated monetary growth in the first half of 1982 as low (Milton Friedman letter to David Lindsey, April 7, 1983 [p. 1], Federal Reserve Board records), while Federal Reserve Bank of Boston's president Frank Morris would treat "the first half of 1982" as a period of high M1 growth, because (with January included) the M1 growth rate for that period was "substantially above" the FOMC's 1982 target range (F.E. Morris, 1985, p. 4; p. 225 of 1987 reprint). This difference in numbers aside, Morris' 1985 recollection of the monetary policy climate in the middle of 1982 (before the July policy easings) seemed to be amiss, as he portrayed it as one in which the FOMC was already on course to overshoot its 1982 M1 target. But the predominant expectation at the time, fueled by the low rates of M1 growth seen since February, seems instead to have been that the FOMC was on course to meet the 1982 monetary-growth target. For example, it was reported that "money supply growth has recently slowed to within the Fed's limits." (*New York Times*, July 9, 1982.) Indeed, negative readings in the weekly data for M1 were said to have put "the Federal Reserve well within its target for money supply in 1982" (*Boston Globe*, July 13, 1982). Correspondingly, in its July 20 statement announcing a half-point cut in the discount rate, the factors that the Federal Reserve Board cited as justifying the reduction included "the relatively restrained growth of money and credit in recent months." (Quoted in *South China Morning Post* (Hong Kong), July 21, 1982.)

overshoot in 1980, and, on some reckonings, an undershoot in 1981—the New Operating Procedures begun in October 1979 was successful in delivering M1 roughly on the intended course. In particular, the implied target level of M1, obtained by joining the growth-rate target midpoints for each year, was close to the actual level in mid-1982. This feature was stressed in early retrospectives on the New Operating Procedures by Duesenberry (1983, p. 135) and Gordon (1985, pp. 64–65) and at the time by Samuel Brittan in his *Financial Times* column (July 15, 1982).[113] Volcker could therefore state accurately in a television interview in early 1982 that M1's declining-growth path was "very closely in line with what we set out to do" (*New York Times*, February 15, 1982, p. D1).

In 1982, Friedman himself concurred that averaging through the fluctuations, the FOMC had actually delivered on-target monetary growth. Previously, he had had little occasion to reach this judgment, as until the December 1981/January 1982 period he had been focusing on M2, rather than M1. In contrast to M1, for which its target misses roughly averaged out in the early years of the 1980s, the Federal Reserve overshot its M2-growth targets in both 1980 and 1981.[114] Consequently, in an interview given in late 1981, Friedman was still saying that, unlike many others, he thought that monetary growth had been too high, not too low, recently (*Human Events*, December 5, 1981, p. 6). But he changed his assessment once he switched to concentrating on M1. In February 1982, now measuring performance using M1, he described the rate of monetary growth achieved since October 1979 as "fairly good" (*Wall Street Journal,* February 1, 1982, and *Newsweek*, February 15, 1982). The following month, Friedman went slightly further, stating that "the average performance has been pretty good. In the past two or three years, the average rate of monetary growth has been held down."[115]

Having reached this assessment, Friedman maintained now that the indictment he would make of Federal Reserve policy was that "it's been too volatile" rather than being subject to criticism of having been "too tight or too easy" (*Chicago Sun-Times*, June 7, 1982). He would waver somewhat when it came to confining himself to this criticism. In the shadow of the substantial rise in M1 growth from October 1981 to April 1982, in mid-1982 he thought that monetary growth was "dangerously high" of late (*Wall Street Journal*, June 28, 1982), and in *Newsweek* (July 12, 1982), Friedman seemed unimpressed, even on the basis of M1 behavior, with the reduction in monetary growth achieved since 1979. He had already noted, however, that the very volatility of monetary growth since October 1979 made judgments about average performance

[113] Various reports in the U.S. business press in July 1982 (a couple of which are quoted later) made the same point.
[114] See Bernanke and Mishkin (1992, Table 1, p. 190) and the previous chapter.
[115] Friedman (1982a, p. 58).

tenuous (*Human Events*, December 5, 1981, p. 6), with new monthly observations potentially having a powerful effect on the calculation. That proved to be the case again, thanks to a slow rate of monetary growth after January 1982 brought M1 by midyear to the state described above of being on target on average since October 1979. Friedman consequently granted that from fall 1979 through summer 1982 monetary growth was lower than it had been before 1979 (*Barron's*, October 25, 1982, p. 7). And in the mid-1980s retrospectives—which continued to focus on M1 growth—that he wrote on the 1979–1982 experience, Friedman similarly acknowledged with regard to the introduction of the New Operating Procedures, "average monetary growth was lower after the change than before."[116]

In early 1982, Paul Volcker remarked: "If we get blamed for high interest rates, we at least ought to get some credit for the improved inflation statistics." (*Time* magazine, February 8, 1982, p. 53.) CPI inflation had peaked in April 1980. But its twelve-month rate had been in double digits as recently as October 1981. Several months later, however, an admirer of Volcker in financial markets observed that in much public discussion of the U.S. economy, "no one seems to notice that we are not talking about inflation anymore."[117] This shift of attention reflected not only the continuation of the recession, but the greatly improved behavior of inflation: its three-month rate had moderated to only about 5 percent in the fourth quarter of 1981 and improvement continued into 1982. At a panel appearance alongside Anna Schwartz on April 22, 1982, Volcker highlighted the "remarkable progress" on inflation seen in the past year.[118] A dramatic next step came the following morning, when the release of CPI data for March 1982 saw the first absolute decline in the index in seventeen years (*New York Post*, April 23, 1982). At the end of 1982, the twelve-month CPI inflation rate fell below 4 percent: see Figure 2.

Friedman was one who did—in the course of heavy criticism of other aspects of its performance—credit the Federal Reserve with reducing inflation. He observed that "inflation depends on average performance of the Fed" (*Commodities*, January 1983, p. 50), and so he regarded lower inflation as what would was implied by the reduction in monetary growth. As discussed later, by early 1982 Friedman was indicating publicly that he expected inflation to be much lower by the end of year. By late the following summer, with a large inflation reduction already highly apparent, he observed of the Federal Reserve (*Newsweek*, August 23, 1982): "It did succeed in bringing down the average rate of growth in the quantity of money. That is why inflation has come down so sharply."

[116] Friedman (1983e, p. 9; 1984b, p. 28; 1985b, p. 54). See also Friedman (1984c, pp. 399–400).
[117] H. James Toffey of First Boston Corp., quoted in *Time* magazine, March 8, 1982 (p. 76).
[118] Volcker (1982a, p. 3).

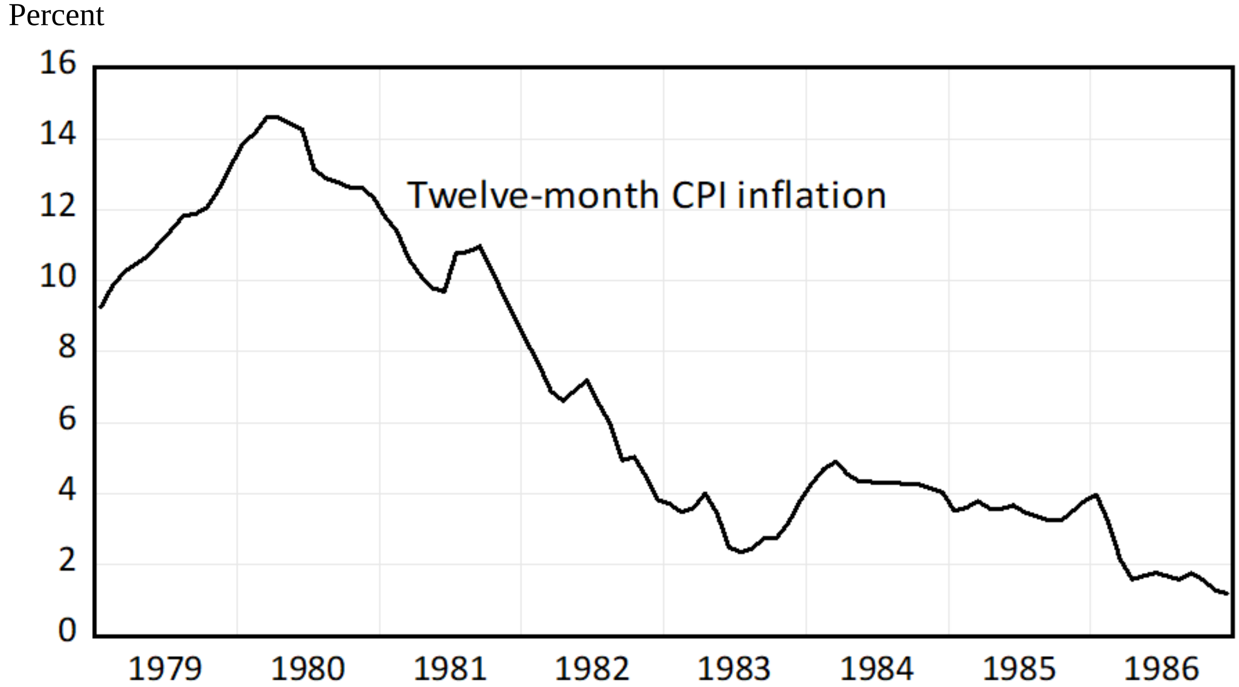

Figure 2. Twelve month U.S. CPI inflation, January 1979–December 1986.
Source: Federal Reserve Bank of St. Louis' FRED portal.

As Friedman noted, monetary growth had peaked before 1979.[119] This was true of both M1 (a 1978 peak of the twelve-month rate) and M2 (a 1977 peak). But, in the case of M1, the pattern of declining growth had clearly continued after October 1979, and in his M1-focused commentaries of the mid-1980s Friedman contended that the post-1979 reduction in the growth rate of that aggregate "accounts for the subsequent decline in inflation."[120]

Yet 1982, despite seeing a fall in inflation in the wake of a decline in monetary growth, would not be remembered as a year in which monetarist propositions were validated. Instead, as Friedman acknowledged in June 1983, it was the key year in an episode that was seen as "discrediting the implications of monetarist theory."[121]

The reason for this widespread perception was simple: the behavior of velocity. Samuel Brittan highlighted in his July 15 column something that would continue for the rest of the year: nominal income growth and inflation were falling much more than prior monetary growth would suggest. on M2 velocity, which indeed was showing an unusually large negative growth rate for 1982 as a

[119] See, for example, Friedman and Friedman (1984, p. 89; 1985, p. 89). They dated the peak in M1 growth to the second quarter of 1978.
[120] Friedman (1983e, p. 9; 1984b, p. 28; 1985b, p. 54).
[121] Friedman (1983e, p. 1).

whole (*Financial Times* (London), December 9, 1982; Brittan, 1983, p. 145).[122] But it was the fall in M1 velocity that was particularly striking and would dominate the discussion of velocity in the United States in policy circles and among economists for the rest of the year—and beyond, as indicated by the title "The 'Great Velocity Decline' of 1982–1983" of Judd and Motley (1984).[123] It was this M1 velocity decline that would provoke what Gordon (1983, p. 67) called "a new debate about monetary policy in general and monetary targeting in particular"—one that gave rise to both the immediate policy changes in the FOMC in 1982 and, over a longer period and via later rounds of the debate, a move away from a focus on monetary aggregates in research on monetary policy.

Visually and statistically, the M1 velocity decline was more significant than the fall in M2 Both M1 velocity and M2 velocity fell in 1982: see Figures 3*a* and 3*b*. Brittan would later focus velocity, as it represented a break from a long-term rising trend.[124] But, in any event, it was on M1 that the Federal Reserve and many outside observers tended to focus in this period when considering money, and so it was the behavior of M1 velocity, rather than of M2 velocity, that received the lion's share of attention in 1982–1983.

Friedman's stand in 1983 was that the M1 velocity decline "was entirely consistent with an unchanged demand for money [function]."[125] This was a claim that was not widely accepted at the time. But later demand-for-money studies have largely vindicated it, as they have suggested that the post-1981 shift in M1 velocity was predominantly due to the accompanying decline in market interest rates.[126]

[122] M2 velocity fell by more in the year to 1982:Q4 than did M1 velocity. Of the declines in the two velocity series, however, that in M2 velocity was considered less of an aberration because, of the two money-turnover series, only M1 velocity was historically stationary, and so it had both fallen and risen on numerous previous occasions. See Figure 3*b*.

[123] Baba, Hendry, and Starr (1992, p. 25) credited the "Great Velocity Decline" terminology to the Federal Reserve Bank of San Francisco (1983) volume. But they gave no page reference, and their attribution does not appear to have been accurate.

[124] The lag between monetary growth and nominal income growth meant that the conventional calculation of velocity overstated the decline. Friedman showed, however, that the decline in M1 velocity and the accompanying trend-break were still highly evident after allowing for the lag. See *Wall Street Journal*, September 1, 1983.

[125] Friedman (1983e, p. 10).

[126] Hoffman and Rasche (1996, p. 114) stated: "One hypothesis…, apparently initially advanced by Milton Friedman, is that the change in velocity behavior in the 1980s is a result of a break in inflationary expectations." Although they provided no specific reference, the first Friedman item in print that advanced this hypothesis was probably *Wall Street Journal*, September 1, 1983. He had earlier remarked in his January 14 PEPAB memorandum that "the reduction in inflation and interest rates" had lowered M1 velocity in 1982 (Friedman, 1983f, p. 7). Later, in testimony given on April 12, 1983, Paul Volcker had mentioned that it was possible that the disinflation might permanently alter the trend of M1 velocity (Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1983a, p. 40). In March 1983, Friedman's former student Michael Keran had also publicly advanced and endorsed the notion that "the rapid growth of M1 in the second half of 1982 was an appropriate

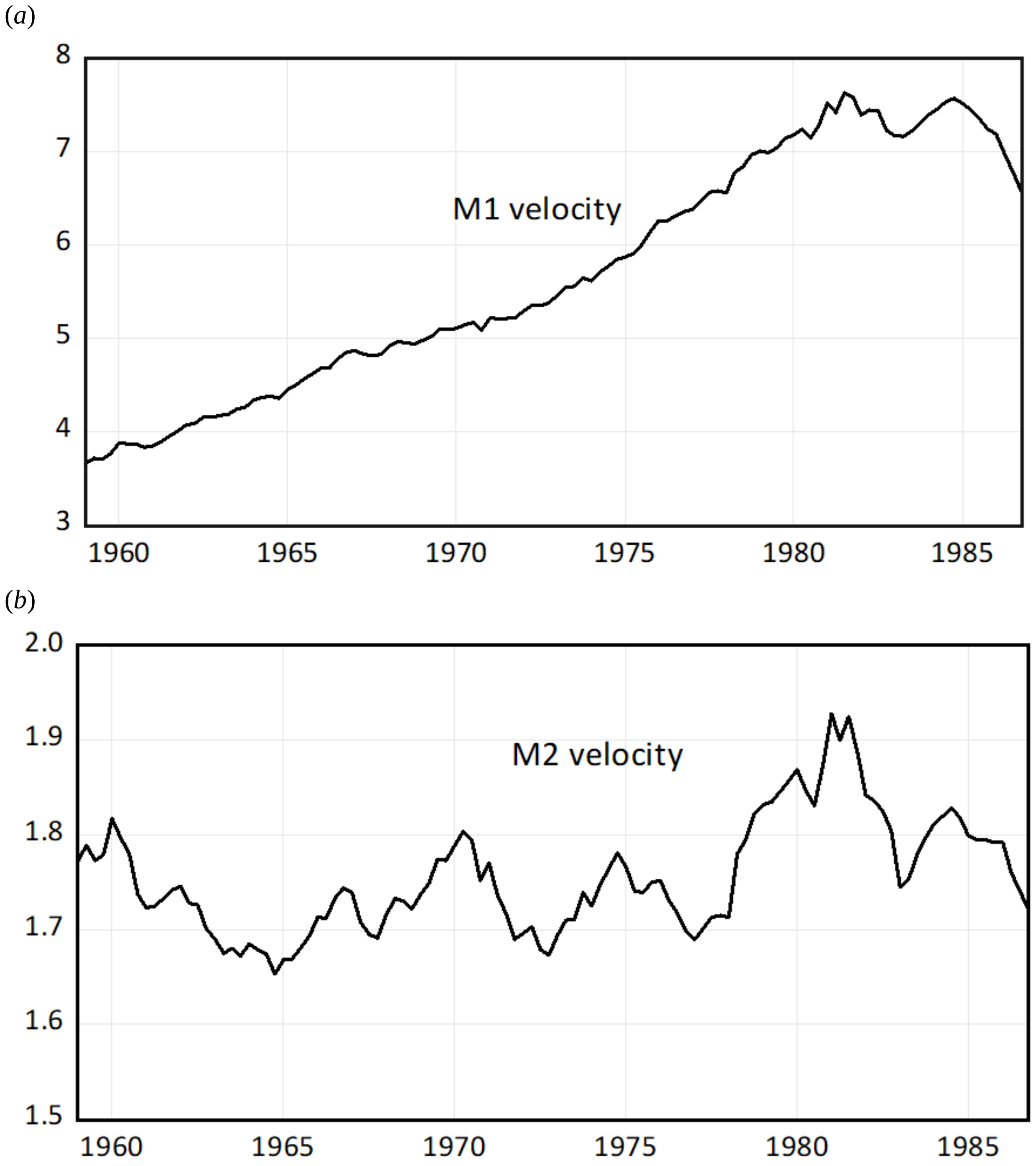

Figure 3. Velocity (nominal GDP divided by quarterly average of money) of M1 and M2, 1959:Q1–1986:Q4.

Source: Federal Reserve Bank of St. Louis' FRED portal.

response to the decline in inflation expectations," and that "standard economic analysis" suggested that this decline lowered the level of velocity (Keran, 1983; quotations from pages 3 and 1, respectively).

Before this later consensus developed, however, the Federal Reserve had moved substantially away from monetary targeting. And even had policymakers fully accepted that M1 velocity behavior was consistent with a stable demand for money function, they might well have moved away from monetary targeting in 1982, on the grounds that such targeting was not suitable in conditions of an ongoing change in the time-series behavior of velocity.

Alongside the 1982 decline in velocity, another reason why—as Friedman observed—many retrospectives on the period "interpreted the results as a failure of monetarism" was the severity of the 1981–1982 recession.[127] Alan Blinder, for example, recalled that, although he eventually came to admire Paul Volcker's record as chair, "I had cringed when he pledged allegiance to monetarism in 1979, objected to the excruciatingly high interest rates he caused or allowed in 1980–82, [and] complained vociferously about the depth and length of the 1981–82 recession" (*Business Week*, June 29, 1987).

In his answer to criticisms along these lines, Friedman indicated that he shared the view that the recession had been overly severe. But he traced its severity to the element of U.S. monetary policy performance of which he had been most critical: the "extraordinarily erratic" rate of monetary growth (quoted in Mann and Greenberg, 1984, p. 17). More orderly behavior of both economy-wide variables, he believed, would have resulted if the disinflationary policy (in the form of monetary restriction) had been phased in evenly over 1979 to 1983, instead of featuring moves to severe restriction in 1980, 1981, and 1982, interrupted by periods of more rapid monetary growth.[128] And when the recession did prove more severe than a gradualist program would have implied, he saw this larger-scale recession as, in turn, being associated with a larger decline in inflation than would otherwise have occurred.[129] In effect, Friedman was arguing that monetary policy had created a deep output gap concentrated in 1982 instead of a shallower output gap spread over 1980 to 1982. "On the average, we've brought down monetary growth," he stated in one retrospective, "but it was brought down in such an irregular and erratic pattern that it produced a much more severe, and longer-lasting, recession than would have been necessary if you had been able to do it in a gradual way."[130]

Friedman was essentially suggesting that "the extraordinary instability in monetary growth" had

[127] The quotation is from Friedman (1984c, p. 397).
[128] Friedman gave an early outline of how a gradual reduction in monetary growth in the 1979–1982 period would have produced better results for the U.S. economy in *Newsweek*, August 23, 1982. Elaborations of this alternative scenario appeared in Friedman and Friedman (1984, pp. 90–91; 1985, pp. 90–92) and Friedman (1985b, pp. 58–60).
[129] See, for example, *Newsweek*, July 25, 1983, and Friedman (1984c, p. 399; 1985b, p. 59).
[130] From his remarks in Mann and Greenberg (1984, p. 17).

contributed to the 1982 velocity decline—by producing such intense economic variability that the private sector increased its desired holdings of real money balances on precautionary grounds.[131] This explanation implied that the pace of decline in nominal income growth, especially in 1982, had been more rapid than what the authorities had intended. In particular, it was greater than what would, by general agreement, normally be implied by the monetary-growth reduction that had been produced. In accounting for this excess rate of nominal income decline, Friedman suggested that the uncertainty-induced increase in the demand for money had helped make the recession worse.[132] Relatedly, it had produced a larger decline in inflation than otherwise, although one that Friedman stressed could have been achieved after 1982 by a slower-paced but longer-lasting disinflation program.[133] He therefore saw the fact that monetary growth had been reduced erratically rather than smoothly as having made for an overly-abrupt disinflation, with a higher peak in unemployment, and so amounted to a case in which "deviations in practice from monetarist policies have had the adverse consequences predicted by monetarist theory."[134]

The emphasis that Friedman put on uncertainty as a factor depressing velocity in 1982 echoed his comments on earlier episodes—and particularly had a counterpart in Friedman and Schwartz's treatment of the early 1930s.[135] Friedman's pointing—particularly in the accounts that he wrote in the immediate wake of the 1979–1982 experience—to uncertainty as a factor driving down velocity in the early 1980s was somewhat at odds with the later work by Lucas (1988) on M1 and Hall, Porter, and Small (1991) on M2. These studies would suggest that velocity behavior over the first half of the 1980s could be accounted for by money demand functions that used standard explanatory variables, without the need to include proxies for uncertainty.

A possible reconciliation of the two approaches to understanding velocity might lie in the possibility that uncertainty-driven increases in money demand *were* a factor in operation during the early 1980s, but not by an amount sufficient to generate statistically significant money demand instability (particularly when a longer-run money demand function, which would tend to

[131] The quotation is from Friedman and Friedman (1984, p. 90; 1985, p. 90) For specific attributions by Friedman of the M1 1982 velocity decline to increased uncertainty, see Friedman (1983e, pp. 2, 10; 1983f, p. 7; 1984c, p. 399; 1985b, p. 59) and *Wall Street Journal*, September 1, 1983.
[132] See, for example, *Edmonton Sun* (Alberta, Canada), October 21, 1982; *Barrons*, October 25, 1982 (p. 7); and Friedman (1984f, p. 41).
[133] Friedman and Friedman (1984, pp. 90–91; 1985, pp. 91–92).
[134] Friedman (1983e, p. 1).
[135] As already noted, this was a feature both of their 1963 narrative of the Great Contraction and their 1982 econometric modeling of the same period.

have large residuals, is used as the benchmark). Consistent with this possibility, Friedman's own work on M2 money demand in 1961−1986, published in 1988, found a significant but small coefficient on a variable that was partly designed to proxy considerations related to uncertainty.[136]

Uncertainty may, therefore, well have played a role in reducing the velocity of U.S. monetary aggregates during the early 1980s, but to a lesser extent than Friedman's initial (1982 to 1985) analysis suggested. Better examples of uncertainty raising the demand for M1 and M2 would come with 9/11 and, after Friedman's death, in the late-2008 period (at the height of the global financial crisis) and following the outbreak of the coronavirus pandemic in 2020.

The path of U.S. interest rates during 1982

As discussed in the previous chapter, in late 1981 Henry Kaufman had made widely reported predictions that a surge in U.S. interest rates would occur in the second half of 1982. The scenario envisioned was one in which large U.S. federal budget deficits—which were widely expected to materialize in earnest in 1982 and, indeed, did do so—would collide with the policy of a Federal Reserve determined to achieve monetary-growth targets and lead to much higher market yields.

By the start of 1982, predictions along these lines had become standard. In a Joint Economic Committee hearing held on January 20, 1982—exactly one year into the Reagan presidency—former Carter Administration economic official Barry Bosworth indicated that in preparing his testimony, he had not seen any forecast "of a significant decline over the remainder of this year in interest rates." Bosworth noted that, for the second half of the year, the predictions pointed in the opposite direction: "almost all the markets are projecting the short-term rates six months from now will be higher than they are today. I don't think there's any disagreement over this issue."[137]

The first half of 1982 seemed to bear out this pessimism. If anything, it appeared that the predicted interest-rate increase was taking place ahead of schedule. The Friedman *Wall Street Journal* and *Newsweek* pieces that saw print in the first ten days of February 1982 were largely on the subject of interest-rate variability—and they appeared just as an interest-rate upsurge was

[136] Friedman (1988a, pp. 224–227).
[137] Joint Economic Committee, U.S. Congress (1982d, p. 130).

worsening considerably (see Figure 4). After averaging 12.37 percent in December 1981, the federal funds rate averaged 13.22 percent in January 1982. During the following month, it averaged 14.78 percent. And this average encompassed days during February 1982 in which the funds rate crossed 15 percent: in the week ended February 3, it averaged 15.19 percent, and in the week ended February 10, it averaged 15.61 percent (*Washington Post*, February 20, 1982, p. D8). On February 17, the administered, but market-sensitive, prime rate charged by banks—a rate that had been lowered significantly during 1981, was adjusted upward, from 16.5 to 17 percent, in recognition of the new upward pressure in market rates (*Charlotte Observer* (North Carolina), February 18, 1982). On the day after this well-publicized development, President Reagan remarked at a press conference: "One of my major concerns today is high interest rates. High interest rates present the greatest single threat today to healthy, lasting recovery." (*Detroit Free Press*, February 19, 1982, p. 13A.)

To Representative Henry Reuss—who had been one of the Congressional proponents of monetary targeting in the 1960s and 1970s, but who was also a frequent advocate of more expansionary economic policy—the appropriate response was straightforward: a change in Federal Reserve policy. Reuss called for a looser monetary-aggregate target alongside interest-rate reductions, to be effected, if necessary, by a Congressional instruction to the Federal Reserve (*Miami Herald*, March 16, 1982).[138] The Reagan Administration, however, did not join in the call for a central-bank-engineered cut in interest rates. Certainly, during 1982 Secretary of the Treasury Regan made numerous high-profile criticisms of Federal Reserve policy. Notably, in testimony before Reuss' Joint Economic Committee on January 27, 1982, he blamed the six months of very low M1 growth witnessed from April 1981 for the recession, and he noted that he had spoken out against this stretch of low monetary growth as early as September 1981.[139] Furthermore, echoing criticisms being articulated by Friedman and other monetarists, Regan complained about the wide variation in monetary growth being seen under the New Operating Procedures. Regan referred to "this lurching that they [the Federal Reserve] are doing"—"these rapid changes" in either direction, instead of "steady" growth.[140]

[138] On the Republican side, Senate Majority Leader Howard Baker also called for more expansionary Federal Reserve policy (*Time* magazine, February 8, 1982, p. 52).

[139] See Joint Economic Committee, U.S. Congress (1982d, pp. 241–242).

[140] See Regan's testimony of January 27, 1982, in Joint Economic Committee, U.S. Congress (1982d, pp. 223 [first quotation], 239 [second and third quotations]). Regan was also quoted during this period complaining about "erratic swings" in the money stock (*Time* magazine, February 8, 1982, p. 58). His outspokenness was shared by numerous other administration officials during 1982. Beryl Sprinkel, in particular, continued to be publicly critical. In February, he was quoted saying of Volcker: "he has got the same goals that we do. But you evaluate a man on goals and performance, and [as] between the two, performance is more important. What we want is stable, moderate growth of money, and the [actual] growth has been anything but stable." (*Time* magazine, February 8, 1982, p. 58.) In a later public intervention, Sprinkel reaffirmed, "We are very unhappy with the monetary policy we've been

Percent

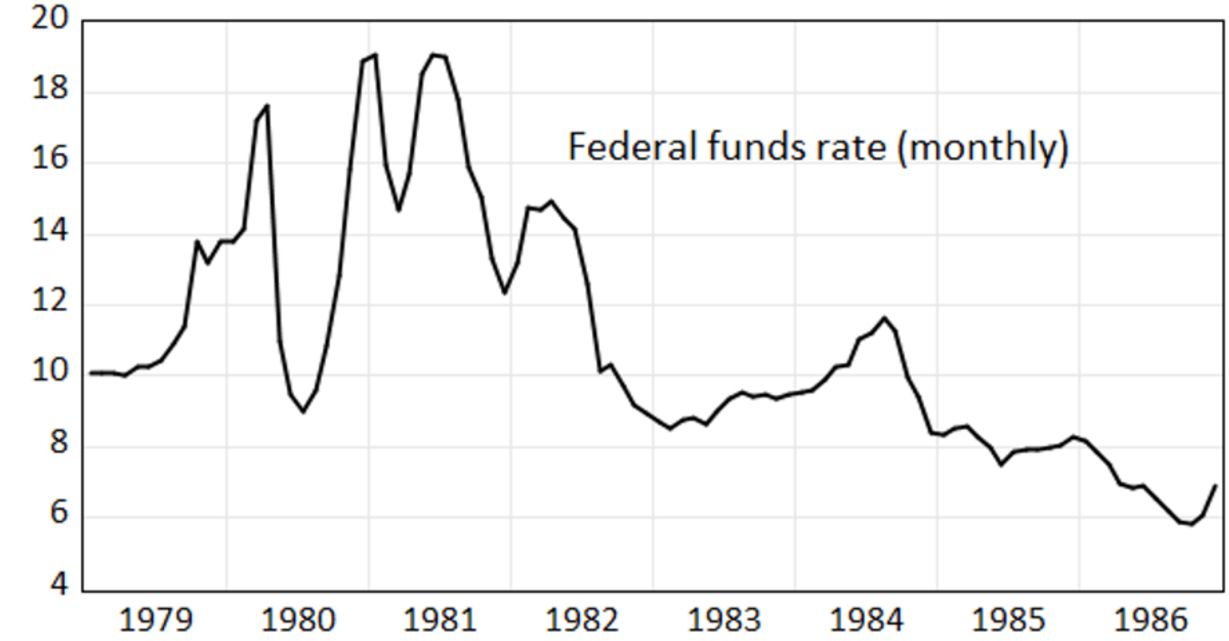

Figure 4. Federal funds rate (monthly average), January 1979–December 1986.
Source: Federal Reserve Bank of St. Louis' FRED portal.

Percent

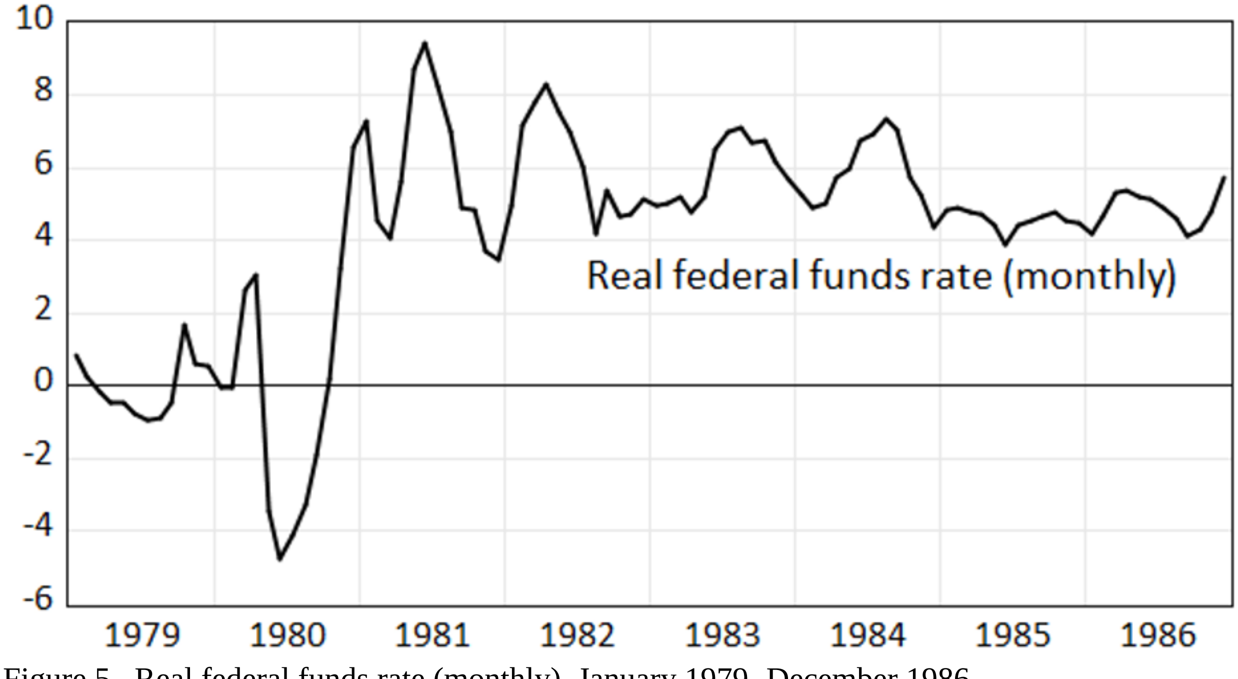

Figure 5. Real federal funds rate (monthly), January 1979–December 1986.
Source: Federal Reserve Bank of St. Louis' FRED portal. Computed as the monthly average of the federal funds rate minus same-period 12-month CPI inflation rate.

getting," in particular because fluctuations in monetary growth were in conflict with "what the Fed has promised us." (*Los Angeles Times*, June 23, 1982.)

But when urged to do so by Reuss, Regan did not call for the Federal Reserve to lower interest rates. Regan's endorsement of the idea of a more relaxed Federal Reserve posture was limited to a statement that the FOMC should seek to achieve the upper one-percent band of its target range for 1982's M1 growth of 2.5 to 5.5 percent Dm1 target, rather than aim for the midpoint of the range.[141] Regan continued to support the FOMC's basic announced strategy of multi-year phased slowdown in monetary growth: "what the Fed is trying to do, and something that we would support them in, is: Gradually, over a period of years, bring that money growth down from where it had been."[142]

A key reason for Regan's reticence in calling for action to lower interest rates was the fact that he shared a view that had become commonplace in financial circles—that a concerted step-up in monetary growth would soon be manifested in *higher* nominal interest rates. Regan himself specifically characterized the pattern seen in the decade to date as: "the more money you put into the economy, the higher your rates of interest seem to go."[143]

This line of thought underlined the degree to which the Fisher effect—largely ignored in practical discussions of interest rates in the 1960s except in those of monetarists—had been instilled into U.S. economic discussion by the early 1980s. An administration report that Reagan issued early in his presidency had stated: "Excessively rapid monetary growth cannot lower interest rates."[144] For much of 1981–1982, administration officials gave the impression that this message applied to any appreciable speeding-up of monetary growth and that it held in both the short run and the long run.

In fact, this maxim had doubtful applicability to the short-run behavior of U.S. interest rates, especially short-term rates. In particular, it highlighted the Fisher-effect mechanism to the exclusion of other influences of monetary policy that were likely of considerable importance for the behavior of interest rates at short horizons. Certainly, Friedman himself noted that since 1979 higher rates of monetary growth appeared to be reflected in upward pressure on short-term interest rates within a few months.[145] It was on the basis of Friedman's commentaries that

[141] See Regan's testimony of January 27, 1982, in Joint Economic Committee, U.S. Congress (1982d, pp. 223–224).
[142] Again, see Regan's testimony of January 27, 1982, in Joint Economic Committee, U.S. Congress (1982d, p. 224). Similarly, President Reagan stated in February 1982 that "we have made clear our support for a monetary policy that will steadily bring down inflation." (Reagan, 1982a.)
[143] From Regan's testimony of January 27, 1982, in Joint Economic Committee, U.S. Congress (1982d, p. 240). See also his surrounding discussion on pages 239-240.
[144] White House (1981a).
[145] This regularity was widely noted. For example, the *Washington Post* (December 12, 1982, p. H3) stated that "interest rates over the last two or three years… seemed to move in the same direction as money growth." Similarly,

broadcaster Louis Rukeyser referred in August 1982 to "the Friedman theory that interest rates rise following fast money growth."[146]

But this "Friedman theory" (really, his demonstration of an empirical regularity) was not necessarily—and, indeed, it likely was not—an endorsement on Friedman's part of the notion that monetary policy rapidly affected the expected-inflation component of short-term interest rates. Rather, he seemed to associate the fairly prompt upward pressure on interest rates in the wake of monetary accelerations (and downward pressure in response to decelerations), reflecting the dependence on economic activity of the private sector's demand for credit: economic expansions boosted credit demand, and upturns reduced it.[147] But even in the early 1980s, Friedman allowed for an interim period in which the short-term interest rate fell when the Federal Reserve, via an increased provision of reserves, stimulated monetary growth.[148]

The notion that expansionary policy certainly was associated with lower rates for an appreciable period would be confirmed by Cochrane (1989) who, upon focusing on higher-frequency movement in short-term interest rates, found that the liquidity effect—rates moving in the opposite direction of monetary-growth movements—was still present in the 1979–1982 period and was manifested in rates over a period of about twelve months.

These short-run influences still left inflation expectations as a fundamental influence on nominal interest rates—and with inflation readings turning for the better during 1981 and even more so in the early months of 1982, one that promised to be quantitatively important in the immediate interest-rate outlook. This was a point that united senior administration economic officials, Paul

speaking at a conference held on October 30–31, 1981, Lindsey (1983, p. 10) had referred to "the empirical regularity of movements in short-term interest rates tending to follow movements in money with only a brief delay."

[146] *Wall Street Week*, Maryland Public Television, August 27, 1982, p. 7 of transcript.

[147] See *Newsweek*, February 15, 1982, and Friedman (1983e, pp. 3, 9). As discussed in the previous chapter, Friedman specifically viewed the rise in interest rates in the second half of 1980 as resulting from what he sometimes called the "intermediate income effect" of a monetary expansion—a procyclical rise in interest rates due to a monetary expansion, and not a tightening of monetary policy. President Reagan offered a similar interpretation of this episode: "In the six months preceding this Administration's taking office, interest rates had risen rapidly, reflecting excessively fast monetary growth." (Reagan, 1982.)

[148] Like Friedman and figures in the administration, the Brunner-Meltzer monetarist camp was also strongly emphasizing, in this period, that the positive relationship between monetary growth had become a feature of the U.S. data even on quite short horizons: see, for example, Brunner and Meltzer (1983a, pp. 64–65). Indeed, Brunner (1983, p. 39) referred to "[o]lder monetarist analysis" as though the framework in that analysis, in which monetary expansion initially produced a liquidity effect (a framework that had just been reaffirmed in the interest-rate analysis of Friedman and Schwartz, 1982a), might be on the brink of being judged outmoded. A close look at the Brunner-Meltzer position articulated over this period indicates, however, that they still granted that higher monetary growth *would* lower U.S. interest rates on impact—so the liquidity effect continued to exist. Their position was, rather, that the subsequent move up in rates would occur "quickly" (Shadow Open Market Committee, 1982, p. 2)—and presumably more quickly than in the past.

Volcker, and Friedman—all of whom in the course of early 1982 expressed the view that it was a matter of time before nominal interest rates buckled in the face of the downward pressure coming from declining inflation expectations. In his testimony of January 27, Regan saw interest rates as poised to fall without a policy change: in his assessment, they were currently in an "ice jam" that would "break."[149] The Secretary of the Treasury was speaking at a time when the interest-rate increase in the year so far had been modest. And even on February 10, President Reagan was able to portray the Fisher effect as dominating the recent behavior of rates: "Since late last summer… interest rates have, on average, moved down somewhat in response to anti-inflationary economic policies." The further February spike in interest rates, however, undercut this message and raised doubts about the direction of the trend in nominal interest rates. Paul Volcker made an intervention on the matter that reaffirmed his assessment that the true trend was indeed downward. In a speech given on March 18, 1982, Volcker again invoked the Fisher effect and suggested that the coming decade should be one in which interest rates declined dramatically from their current levels.[150]

In his own remarks on interest rates, made several days after Volcker's, Friedman insisted that a substantial decline was in store even for 1982: "I expect that interest rates on the average will be coming down… [B]y the end of the year, short-term rates will be decidedly lower than they are now. The prime rate may well be in the neighborhood of something like 10 to 11 percent." (*The Vancouver Sun* (British Columbia, Canada), March 24, 1982.)

In the interim, Friedman did not rule out some further rise in interest rates. Indeed, after declining on average in March, interest rates experienced a renewed rise in April-May 1982 with the federal funds rate crossing 15 percent again in May. From a monetary policy perspective, the rise was not altogether surprising, as January-July 1982 had, as already noted, seen M1 growth go through another period of weakness.[151] And Volcker and his colleagues would conclude that they had underestimated the upward pressure on interest rates—and the downward pressure on aggregate demand—implied by their policy settings, as low monetary growth was being reinforced by the beginning of the decline in M1 velocity that has already been referred to. But there was also a prevailing expectation that, with U.S. inflation coming down rapidly, there should also be substantial downward pressure on yields due to the Fisher effect. At the time, therefore, the resilience of high interest rates in the first half of 1982 was widely regarded as a

[149] In Joint Economic Committee, U.S. Congress (1982d, p. 248).
[150] See Nelson (2021) for a discussion of this speech.
[151] Volcker himself noted the contribution that monetary restraint was making on the margin to the continuation of high interest rates in testimony he gave on March 2, 1982 (Committee on the Budget, U.S. Senate, 1982b, p. 273).

puzzle—and, indeed, real interest rates were much higher than those previously seen in the United States in recent times (see Figure 5). Alice Rivlin observed in the first week of May 1982 that "interest rates in real terms are extremely high—higher than they have ever been, I think for any sustained period."[152]

President Reagan continued to stress the downward pressure that should be coming as a result of the Fisher effect: "Interest rates shouldn't be higher than 10 percent," he remarked on a radio broadcast in late April (*Detroit Free Press*, April 25, 1982) Subsequently, in the face of the continued movement in interest rates in the opposite direction, the president noted that interest rates were still considerably lower than they had been when he came into office (*Detroit Free Press*, May 14, 1982). Similarly, William Gibson—a bank economist who, as a graduate student in the mid-1960s, had produced a dissertation on the Fisher effect under Friedman's supervision—observed in late May: "When all is said and done, rates have been drifting down." (*Dallas Morning News*, May 30, 1982, p. 1H.)

These observations were accurate: even after the increases recorded since the beginning of 1982, the prime rate and federal funds rate stood, in May 1982, about 400 to 500 basis points below their 1980−1981 peaks. But, as Reagan observed in May, "The inflation rate is drastically down since when we took office," and the nominal-rate decline was "not good enough," as real interest rates had risen very significantly in the first five months of 1982 (*Detroit Free Press*, May 14, 1982, p. 1A). William Gibson suggested that the second half of 1982 would see matters rectified considerably: "the fundamentals point toward lower rates," he remarked, while predicting that the Fisher effect would bring down interest rates down another 300 to 400 basis points by the end of the year (*Dallas Morning News*, May 30, 1982, p. 1H).

In the event, Gibson's forecast decline proved to be much smaller than the reduction in short-term interest rates that actually occurred over the rest of 1982. The larger-scale reduction in interest rates that Friedman had foreshadowed in March would end up being the more accurate prediction. Ahead of that steep decline, however, the revival of interest rates in the first five months of 1982 had caused great puzzlement at the policymaker level and had prompted the Federal Open Market Committee to question, and subsequently abandon, the New Operating Procedures that it had introduced in October 1979, along with the associated arms-length attitude toward the setting of short-term interest rates.

[152] In American Society of Newspaper Editors (1982, p. 127).

In particular, at the FOMC meeting of May 18, 1982, considerable dissatisfaction was expressed about the high level of short-term interest rates. Governor Henry Wallich observed that it was "somewhat surprising that we're not getting the declines in interest rates that one would expect, given the [declining] rate of inflation," and that recession should also be lowering interest rates by depressing credit demand.[153] Volcker stated at the meeting, "I'd like to get interest rates down," and indicated that he was receptive to giving "the market a little sense of a lead in that direction." But, as he had already implied in numerous public remarks on the matter, Volcker was concerned that central-bank management of interest rates might soon generate a backlash in terms of a move up in inflationary expectations, compelling the Federal Reserve to reverse the interest-rate decline.[154] In contrast, Governor Nancy Teeters viewed the disinflation as having been accomplished and favored ending the New Operating Procedures, moving back to an interest-rate instrument, and cutting key short-term interest rates to well below 15 percent, in view of the national unemployment rate being 9.4 percent.[155]

The Federal Reserve's reversion in operating procedures

Although there was little in the way of a formal Federal Reserve announcement to this effect, Governor Teeters' advice is now widely accepted to have, in effect, been taken within a couple of months of the May 1982 FOMC meeting. As Friedman was one of the first to grasp, direct FOMC management of the federal funds rate resumed during the summer of 1982.
From a 14.15 percent average in June 1982, the rate fell to 12.59 percent in July, and 10.12 percent in August. It rose slightly to 10.31 percent in September before averaging below 10

[153] Federal Open Market Committee (1982a, p. 21).
[154] See Federal Open Market Committee (1982a, p. 41). Volcker's sentiments at the meeting echoed those he had made in public testimony: "It will profit us nothing to embark on a heroic effort to bring interest rates down for three months to find out that we have undertaken a policy course that in the next six months sends them up." (Testimony of February 24, 1982, in Committee on Finance, U.S. Senate, 1982, p. 116.) Another occasion on which he expressed such concerns publicly was the April 22, 1982, event with Anna Schwartz, when Volcker observed: "increasing the money supply in a static situation where expectations are stable will come to be associated with declines in interest rates—that's true. But… you could not count on that response, I would suggest, today… [And] if it [the decline in interest rates] runs counter to the longer-term [forces,] it isn't going to last very long." (Volcker, 1982a, p. 19.) See also his remarks at the midyear FOMC meeting (Federal Open Market Committee, 1982b), quoted in Meltzer (2009b, p. 1112). This meeting was one of the final recorded occasions on which Volcker expressed this concern, before deciding to move rates down.
[155] Federal Open Market Committee (1982a, p. 27). A couple of months later, in line with her wish to move away from a policymaker focus on monetary aggregates, Teeters was (along with Lyle Gramley) in the minority opposing the Board's reintroduction of contemporaneous reserve accounting. "I don't see any public benefits at all," she remarked publicly on the day of the governors' vote. "[In moving to CRA,] we're trying to test a theory." (*American Banker*, June 29, 1982.) Board Vice Chairman Frederick Schultz had also argued that moves in the direction of CRA might give a signal of a Federal Reserve embrace of "pure monetarism" (*American Banker*, November 5, 1981), but he had resigned several months before the June 1982 CRA vote.

percent for the rest of 1982 and each month of 1983, with the lowest value being February 1983's average of 8.51 percent.

This fall in yields started at a time when the Henry Kaufman-influenced belief—that higher interest rates were still in the works for the rest of 1982—remained prevalent. For example, Donald Maude, chief economist of Merrill Lynch had said, "Any fall in interest rates is going to be extremely short-lived… I think we're in for a major pike upward in July." (*Dallas Morning News,* May 30, 1982, p. 1H.) Consequently, the actual course of interest rates—in particular, what was described as "[t]he "sudden—and largely unpredicted—collapse of interest rates" in July 1982 (*Financial Times* (London), May 24, 1983) led to congratulations going in Friedman's direction for his forecasting feat, with one commentary stating (*Barron's*, October 25, 1982, p. 6): "Friedman's most obvious success in the last year has been in predicting interest rates… [H]e said [they] would fall precisely when the contending school of capital market economists, influential on Wall Street, said they would rise." The grounds for seeing this development as a vindication for Friedman was, however, limited by the fact that the downward trajectory of interest rates in part reflected the return of the authorities to the practice he had criticized for so long—managing the federal funds rate.

Friedman's initial response was to attribute the interest-rate decline since midyear to the Fisher effect: "The reason for the fall in rates, I think, is fundamentally the decline in inflation, along with the decline in inflation expectations." He rejected the notion that the rate decline should be interpreted as being the result of the Federal Reserve's easing of monetary policy. (*Barron's*, October 25, 1982, p. 6.) By mid-January 1983, however, Friedman had changed his mind. He put recent years' events in perspective in a memorandum on monetary policy for the Presidential Economic Policy Advisory Board. This memorandum made clear that he now believed that the major decline in short-term interest rates that had begun in July had stemmed from a conscious FOMC move toward a more expansionary monetary policy.[156]

This interpretation proved to be the one that Friedman stuck with. In numerous retrospectives published over the 1983–1985 period, he would come to characterize the decline in interest rates in the summer and fall of 1982 as largely reflecting a monetary policy change that was also reflected in much more rapid monetary growth during the second half of the year.[157]

[156] Friedman (1983f, pp. 3–4).

[157] See Friedman (1983e, p. 10; 1984b, p. 30; 1985b, p. 55). This analysis underscored the fact that Friedman had never moved into the camp of rejection of the view that monetary expansion initially lowered rates. Neither had Federal Reserve policymakers or senior staff. Indeed, the notion that a short-run liquidity effect was still present

Of course, the role of monetary easing in facilitating this fall in interest rates did not preclude an important role for the Fisher relation in explaining the lower average level of nominal interest rates after mid-1982 than that prevailing in the first two-and-a-half years of the decade. And, numerically, Friedman's prediction of a 10 percent prime rate at the end of 1982 came close to being realized, with the prime rate reduced to 10.5 percent in February 1983 and staying at that level into late summer (*Pensacola News Journal* (Florida), July 20, 1983). These developments bore out not only Friedman's emphasis on the Fisher effect but also his downplaying of the influence of budget deficits on the interest rate. The budget deficit/interest-rate relation and Friedman's position on it will be discussed in detail in the next two chapters.

Friedman's analysis of interest rates in this period was borne out in another respect. He was, starting in his mid-January 1983 memorandum, one of the first observers to take July 1982 as the point at which the Federal Reserve abandoned its New Operating Procedures and went back to a concentration on setting target values for the federal funds rate. This switch back to funds-rate management proved permanent, being still the FOMC practice 40 years later. Much of what Friedman had to say about the interest-rate/monetary-growth rate link under the New Operating Procedures may well have been valid, but it had been rendered moot. There would be no more data bearing on how U.S. short-term interest rates behave when left to the market, as central bank management of those rates had returned permanently.

Friedman was initially skeptical that the Federal Reserve had "changed its policy in the slightest" (*Barron's*, October 25, 1982, p. 7).[158] Even an end-of-year column did not emphasize changes in operating procedures during the year (*Newsweek*, December 27, 1982) and, around the same time, he stated: "I don't believe the Fed has changed at all." (*Commodities*, January 1983, p. 49.)[159]

Shortly afterward, as he came to regard the second-half-1982 interest-rate decline as having been

underlay the Volcker and staff position that greater month-to-month stability in monetary growth from 1979 to 1982 would have produced still greater interest-rate variation (see, for example, Lindsey, 1986, p. 186).

[158] In an interview given a week or so later, Friedman similarly remarked: "I don't think there's been any change in policy whatsoever." (*The News and Courier* (Charleston, South Carolina), October 26, 1982, p. 10.) The same interview, in its edited published form, did not have Friedman giving a responsive reply when asked directly if he believed that the Federal Reserve had gone back to setting interest rates. But his other remarks in the interview implied that his judgment (at this stage) was that it had not.

[159] In addition, Friedman's (1983h) article took the New Operating Procedures as still being in force (p. 339). This paper was written during the fall of 1982 (see p. 341) and was lightly revised in December (as mentioned in a letter from David Lindsey to Friedman, December 15, 1982, Federal Reserve Board records). The initial draft, which included the same reference to the New Operating Procedures as in the published version, was dated November 5, 1982, and Friedman sent the draft to Paul Volker on the same day (Federal Reserve Board records).

a policy-managed move, Friedman became convinced that a change had indeed occurred. The FOMC, he declared on the basis of monitoring "its behavior," had "reverted to its pre-October 1979 policy of targeting interest rates."[160]

The reason this was something that had to be inferred was the oblique approach to public communications that the FOMC now took regarding policy operations. As Friedman observed in the January 1983 memorandum, having made this "major change in its operating procedures" in 1982, the Federal Reserve "has made no public pronouncement" actually indicating that this new change had occurred.[161] Friedman sometimes made overly sweeping statements that asserted Federal Reserve silence on a matter.[162] But, on this occasion, his characterization was correct: the Federal Reserve had exhibited public silence during 1982 with regard to the fact of the shift in operating procedures.

Indeed, Federal Reserve officials made numerous public statements implying that no change had occurred. A case in point was Paul Volcker's testimony to the Joint Economic Committee on November 24, 1982. In these remarks, Volcker referred simply to "bank reserves—our most important operating instrument"—without drawing a borrowed-reserves/nonborrowed-reserves distinction—and added: "I would not interpret what we are doing now as setting targets for the Federal funds rate. We used to do that prior to 1979… We do not target it now."[163] The FOMC's reticence on the matter was also reflected in the fact that a few weeks after Volcker's testimony, John M. Berry, a regular reporter on the topic of U.S. monetary policy, wrote: "The operational focus of the Fed approach was, and still is, … nonborrowed reserves." (*Washington Post*, December 12, 1982, p. H3.)

The absence of a public indication of a change, Friedman noted, was a major "contrast with

[160] Friedman (1983e, p. 10; 1983f, p. 4; 1984b, p. 30; 1985b, p. 54).
[161] Friedman (1983f, pp. 3, 4).
[162] For an example of such overstatements on Friedman's part, see the discussion of the pre-1951 rate-pegging policy in Nelson (2020a, Chapter 4). Another instance, already discussed in Chapter 2 above, was Friedman and Friedman's (1984, p. 96; 1985, p. 94) suggestion that they did not know of a Federal Reserve Board official acknowledging a policy error. Paul Volcker had, in fact, been critical of aspects of pre-1979 monetary policy. This included criticism of policy during the 1975–1979 period, in which he had been vice chair of the FOMC, and which had seen renewed monetary expansion in 1976 and 1977. On February 10, 1982, he testified: "As recently as the mid-1970s, coming out of a deep recession, we seemed to be moving in the right direction—and then lost our way." (In Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1982b, p. 18.) In his appearance alongside Anna Schwartz in New York City ten weeks later, Volcker was even blunter: noninflationary expansion was "went on in the early '60s: and [then] the Vietnam War came along and all the rest, but we did have a five-year period where that happened. It began to happen, in my judgment, in '75 and '76, coming out of the recession. And then, for a variety of reasons, we blew it." (Volcker, 1982a, p. 21.)
[163] In Joint Economic Committee, U.S. Congress (1983b, pp. 290, 311).

October 1979" when Volcker had publicly declared a change in procedures.[164] "On the contrary," Friedman further remarked, giving a characterization consistent with the evidence noted above, "it stated that it had not changed its procedures…"[165]

This practice of the Federal Reserve continued into 1983, with Anatole Kaletsky noting in the *Financial Times* (May 24, 1983) that "with the reticence characteristic of central bankers[,] Fed officials have publicly denied that any fundamental change in operating policy has taken place." Kaletsky's remark was underlined by an official Federal Reserve Bank of New York account that appeared around the same time as his piece. It stated: "Open market operations in 1982 continued to be aimed at achieving nonborrowed reserve levels stemming from the reserve-path targeting procedures."[166]

In the absence of a public declaration during 1982 of the termination of the New Operating Procedures, it has remained a murky matter regarding when these arrangements ended. Many accounts that have appeared in the past 40 years have concurred that the New Operating Procedures ended in the second half of 1982 but have conflicted with Friedman's dating of July in favor of a later date. Stanley Fischer, for example, wrote (1987, p. 14): "In August 1982, the start of the international debt crisis, the Fed announced the end of the monetary policy inaugurated three years earlier."[167] In other sources, a standard date for the change in operating procedures would become October (see, for example, Lindsey, 2003, p. 69).[168]

Even among proponents of the October date, however, there was uncertainty, reflected in Roley's (1986, p. 27) statement: "Most market observers argue that the Federal Reserve monetary policy operating procedures changed in October 1982, if not before." The continuing imprecision in the estimated date of the change in arrangements was also brought out in the fact that Meulendyke (1998, p. 53) dated the shift from nonborrowed-reserves to borrowed-reserves arrangements as having occurred in 1983, rather than 1982.

[164] Friedman (1983f, p. 4; 1984b, p. 28; 1985b, p. 54).

[165] Friedman (1984b, pp. 28, 30; 1985b, p. 54).

[166] Federal Reserve Bank of New York (1983, p. 41). The article went on: "These procedures [are] more fully described elsewhere," and it proceeded to cite items published during the 1979–1982 regime. Likewise, the *Morgan Guaranty Survey* (September 1983) stated: "The Fed's declared operating focus in controlling reserve availability is nonborrowed reserves…" (In Slesinger and Beeson, 1984, p. 155.)

[167] Of course, this particular account parted company with Friedman's interpretation because Fischer contended (albeit without documentation) that the end of the New Operating Procedures was actually publicly announced by the Federal Reserve.

[168] Friedman's own 1983 memorandum suggested initially that the date was "August or September 1982" (Friedman, 1983f, p. 1) before settling later in the text on the July 1982 timing.

But Friedman's July 1982 dating still seems to have been on solid ground. It is certainly true that at the *start* of that month, FOMC policy discussion seemed to be predicated on continuing to leave the federal funds rate as a market-determined rate and on not making a concerted effort to steer it down. Notably, at the Committee's meeting of June 30 and July 1, 1982, Vice Chairman Solomon stated that the markets did not expect the Federal Reserve to step in: "there's a gloom-and-doom atmosphere out there and very little expectation that interest rates will fall."[169] Contrary to these expectations, however, the federal funds rate did steeply fall within a short time (see Figure 4 above).

And it would seem that, consistent with Friedman's later characterization of this period, the FOMC engineered this decline. The July fall in rates did not occur as a result of a formal Committee vote. The main decision flowing from the June/July meeting, and of a follow-up discussion on July 15, was to maintain the existing range for 1982's M1 growth target (see Federal Reserve Board, 1983a, pp. 111, 113). But the subsequent July rate cut seems to have resulted from a decision by senior policymakers that took place in the context of the broad leeway given by the FOMC's existing policy directive. The June/July 1982 meeting had maintained a 10 to 15 percent federal funds rate range as the band expected to be consistent with its forthcoming open market operations (Federal Reserve Board, 1983a, p. 112).[170] This range was so broad that it did not need to be changed even if the Federal Reserve took the initiative to move rates substantially lower. In addition, the FOMC, in an early reaction to the fall in velocity, had decided at its June-July discussions that it would seek an outcome in which the year's M1 growth, rather than coming in at the center of its 1982 target range, was "near, or somewhat above" that range.[171]

In these circumstances, a concerted and policy-driven move in the federal funds rate to lower levels evidently did take place in July and August 1982. It therefore seems that the management

[169] Federal Open Market Committee (1982b, p. 52).

[170] In this vote, Governor Teeters dissented, again maintaining that the FOMC should actively reduce interest rates (see Federal Reserve Board, 1983a, p. 113).

[171] From the FOMC's June-July meeting's Record of Policy Actions, in Federal Reserve Board (1983a, p. 110). This record was released publicly in late August 1982 (*American Banker*, August 30, 1982), although the fact of the FOMC's willingness to accept M1 growth outcome "somewhat above" the target range had already been disclosed in the Federal Reserve's public communications during July, including in testimony given by Paul Volcker (including his July 20 statement, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1982b, p. 8).

With regard to the emerging change in M1 velocity, the Record of Policy Actions for the June-July deliberations noted that "in the first quarter, the decline in the income velocity of M1 was extraordinarily sharp" (Federal Reserve Board, 1983a, p. 109), but it also relayed policymakers' judgment that M1 velocity would start rising again once, possibly within months, economic recovery started (p. 110). In the event, policymakers were correct to see the economy bottoming out soon but incorrect to expect that M1 velocity would mirror the economy by turning up in 1982–1983.

of the federal funds rate began in July and was continued thereafter, and that this reduction was then ratified, and fortified, by the FOMC's choosing a lower federal funds rate band (7 to 11 percent) at its August 24 meeting (see Federal Reserve Board, 1983a, p. 119).

Friedman had long been aware of central banks' inclination to manage interest rates. Indeed, over twenty years earlier, in applying for funding that would support the relaunching of the Friedman-Meiselman project, he had appealed to this inclination by stating that the project related to "whether the instruments of monetary control should be reformed to improve the capacity of the monetary authority to control the stock of money or to control the level and structure of interest rates."[172] In the United States in 1982, the authorities' desire to manage interest rates had been heightened by the economic and financial-market climate and prompted a policy change in July of that year.

What about the later dates for this policy change that have been offered in the economic literature? Friedman certainly agreed with the view (embedded in the Fischer quotation given above) that the Latin American debt crisis that broke out in August 1982 gave rise to Federal Reserve easing.[173] But the easing as measured by the federal funds rate seems to have begun, and on a large scale, in July and to have been deliberately engineered.[174] The August moves consolidated the easing. As for the later, September/October 1982 dating of the change in operating procedures, that may be a prevalently cited date partly because a different, and publicly announced, policy change—the FOMC's deemphasis (discussed later) of the M1 target—occurred over that period.[175]

"The only issue was: How do we get off it?" Stephen Axilrod observed of the New Operating Procedures (interview, April 24, 2013). "I was a little leery about getting on it, because I

[172] Quoted in Fox and Shapiro (1963, p. ii). As Friedman's application was juxtaposing the priorities of improving monetary or fiscal instruments, his listing of money-stock control and interest-rate control in this context was not really presenting them as alternatives to one another. Instead, he was suggesting better control of money or interest rates as possibilities obtainable from refinement of monetary policy (as opposed to fiscal policy) instruments.

[173] See Friedman's remarks in *Newsweek*, January 16, 1984.

[174] For example, a report in the *American Banker* (July 12, 1982) stated: "The federal funds rate dropped to near 13½ percent toward the end of the week after holding as high as 15 percent. This was taken as a further sign that the Fed had eased and that interest rates had at last begun to decline." The article quoted a skeptical market commentator remarking: "I'm inclined to think the Fed has not changed, and it would be very premature for it to cut the discount rate." Within three weeks, however, the Federal Reserve Board had reduced the discount rate by a total of 100 basis points. (See page 10 of *https://www.federalreserve.gov/foia/files/20190829-changes-intended-federal-funds-rate.pdf.*)

[175] The tendency for these two, distinct, 1982 changes in FOMC conduct to be treated as one in discussions was compounded by slippage in terminology on the part of officialdom. A notable early example of this occurred when Chairman Volcker referred to the deemphasis of M1 as "an adjustment of operating procedures" (in his letter of October 19, 1982, to Senator Roger W. Jepsen, in Joint Economic Committee, U.S. Congress, 1982b, p. 26).

couldn't see how you could get off it, really, once you got on it." When expounding the post-October 1979 control techniques in a 1981 article, Axilrod and David Lindsey had speculated that "we will not eventually reach a point where interest rates will have to be given more consideration in [monetary] policy."[176] The specific scenario that Axilrod and Lindsey saw as likely triggering such a change in priorities was a large surge in M1 velocity. This did not occur in 1982. That period did, however, certainly see velocity and interest-rate instability that prompted the FOMC to return to interest-rate management.

This return occurred very covertly. The change in operating procedures in 1982 came to be classified by officialdom of the FOMC moving from one reserves instrument to another: from targeting of nonborrowed reserves to targeting (or, as it was formally termed, making an "assumption" about) borrowed reserves. As discussed presently, this change actually amounted to a much larger move away from monetarist policy prescriptions than the Federal Reserve's continued reference to reserves suggested. Again, however, even this characterization of the procedures—a move to a borrowed-reserves regime—was one that applied *ex post* and did not arise from a 1982 public announcement. Borrowed reserves were not mentioned in the meeting's Record of Policy Actions (that era's public FOMC meeting minutes) in 1982 (see Federal Reserve Board, 1983). Internally, by the end of the year targets for borrowed reserves (referred to as shorthand as "borrowing") were indeed voted on in FOMC meetings (see, for example, Federal Open Market Committee, 1982d, pp. 80, 91). But arriving at this arrangement did not stem from a clear-cut policy decision at an FOMC meeting. Meltzer (2009b, p. 1116) suggested that the period of fall 1982 saw the FOMC "deciding to target borrowing"—but gave no specific public statement or internal quotation relaying that decision. Rather, the meeting that he identifies as initiating borrowed-reserve targeting—a September 1982 teleconference—seems to have been a meeting to ratify a change in operating procedures that had already occurred (see Meltzer, 2009b, pp. 1115–1116).[177]

[176] Axilrod and Lindsey (1981, p. 252). The paper was presented at September 1980's AEA annual meeting.

[177] Thornton (2006, p. 2041) gave the date of the abandonment of nonborrowed reserves targeting as October 1982. Elsewhere, however, he also accurately noted (p. 2044) that Chairman Volcker's first reference at an FOMC meeting to the new practice as one of targeting borrowed reserves actually occurred earlier—at the September 24 teleconference meeting (Federal Open Market Committee, 1982c, p. 2) when Volcker stated that "the operational variable would essentially be borrowings." As rates had been falling for two months by this point, this move likely formalized a change in policy arrangements that had already been in place for a couple of months. Indeed, in August 1982 Benjamin Friedman wrote in reference to the Federal Reserve's using nonborrowed reserves as an operating instrument: "as of the time of writing there is some debate over whether it is still doing so." (B.M. Friedman, 1982c, p. 36; also in B.M. Friedman, 1983a, p. 136. See B.M. Friedman, 1982c, p. 1, for the date of the remark.) Similarly, a Reuters report in early August 1982 stated that "the Fed may be shifting its operational focus" and a commentator was quoted saying of the Federal Reserve, "Let's say it's paying a good deal of attention to the federal funds rate." (*South China Morning Post* (Hong Kong), August 8, 1982.)

The identification of borrowed reserves as the new operating target emerged only slowly in public Federal Reserve statements. One of the first official indications that the FOMC targeted borrowed reserves was given in February 1983 when, in answer to a question posed at a legislative hearing, Volcker remarked that "the marginal pressures on banks" should be measured by "not nonborrowed reserves but borrowed reserves, in the jargon of the trade—how much banks were borrowing or forced to borrow from the Federal Reserve."[178] Then, in May 1984, Governor Henry Wallich gave a speech (Wallich, 1984) on operating procedures that he presented as his personal interpretation of what had been in force since 1982 but which became the standard reference on the Federal Reserve's switch from operating on nonborrowed reserves to a focus on borrowed reserves (see, for example, Goodhart, 1989; Lindsey, 2003, pp. 69–70). Post-1984 official public reaffirmations of borrowed reserves as the official operating target since 1982 included that of Governor Robert Heller (1988).

The switch from a nonborrowed-reserves to a borrowed-reserves procedure was widely acknowledged by many observers as, in material terms, *not* really a move from one form of reserves instrument to another. It was, instead, an elliptical reversion by the FOMC to the use of a federal funds rate target. It therefore, as Friedman had suggested starting in early 1983, amounted to a reversal of the 1979 step in the direction favored by monetarists. The heart of the matter was stated by Goodhart (1989, p. 326): "a target for borrowed reserves implicitly represents an interest rate objective." This was long understood as being a property of borrowed-reserves targeting. Indeed, James Meigs, the one-time Friedman student whose dissertation had been on Federal Reserve operations, had testified in September 1973 that, owing to the relations usually prevailing between bank borrowing and market interest rates, targeting nonborrowed reserves was very like having a federal funds rate target.[179]

Even, therefore, while still articulating its policy publicly in terms of reserve series, the FOMC

[178] From Volcker's testimony of February 24, 1983, in Committee on the Budget, U.S. Senate (1983, p. 678). In addition, in the enclosures to his letter of October 19, 1982, to Senator Roger W. Jepsen, Volcker avoided specifically referring to the nonborrowed-reserves arrangement in the present tense. See Joint Economic Committee, U.S. Congress (1982b, p. 31). In another passage, he referred to the "reserve base" as the variable on which the Federal Reserve relied (p. 30).

[179] See Meigs' testimony of September 11, 1973, in Committee on Banking and Currency, U.S. House of Representatives (1973, p. 296). In his speech expounding the borrowed-reserves procedure, Wallich (1984, p. 26) asserted that "current operating procedures cannot be regarded as a form of rate-pegging." But the validity of this statement rested on taking "pegging" to mean rigid enforcement of a specific value for the federal funds rate. As Wallich acknowledged, the implication of his remarks was that the borrowed-reserves procedure might, in fact, well be consistent with keeping the federal funds rate in a narrow *range*. Furthermore, in practice during the post-1982 regime, achieving the borrowings target was made subordinate to keeping the realized federal funds rate in the range. See Federal Bank of New York (1990, p. 4).

was back to setting the federal funds rate. So in 1982 policymakers brought to an end the limited move away from that practice that they had made in 1979. True, and as discussed below, it would eventually become clear that the FOMC was setting the federal funds rate on different economic principles from those that had guided monetary before 1979. But it was nevertheless the case that, by late 1982, it had dropped the concentration on *bona fide* reserves targets to which it had seemingly moved in 1979.

It would take years before the Federal Reserve officials unequivocally described its post-1982 policy in terms of the federal funds rate. Indeed, in the initial years after 1982, there were numerous official *denials* of a funds-rate orientation. "We have no interest rate policy," Volcker testified on January 27, 1983.[180] Henry Wallich had stated in October 1982 in New York City: "There has been no change in Federal Reserve policy. We haven't switched to an interest-rate policy."[181] Such denials continued into subsequent years: for example, in the summer of 1984 Governor Charles Partee stated that the FOMC had "nothing" to do with the recent behavior of the federal funds rate, which had moved back into double digits (*Boston Herald*, September 18, 1984).[182] And Governor Lyle Gramley testified on June 8, 1984: "We do not have a target figure for the federal funds rate. We set objectives for monetary growth… and target on reserve variables."[183] Over these years, the FOMC's policy directives—published with a lag after each Committee meeting—referred to a range for the federal funds rate that was expected to prevail in the period ahead. But this had been the practice even in the period from 1979 to 1982, too. So it was possible to argue that short-term interest rates' status in the setting of monetary policy had not altered since that earlier period.[184]

Even internal FOMC discussions through the mid-1980s often referred to the management of the federal funds rate only obliquely (see Thornton, 2006). By 1990, however. it had become much more commonplace for Federal Reserve officials to refer to the federal funds rate as the policy instrument, even in their contemporaneously published remarks (see, for example, Kohn, 1990).

[180] In Joint Economic Committee, U.S. Congress (1983b, p. 80).

[181] Quoted in *Commodities*, January 1983 (pp. 49, 51).

[182] An element of validity in Partee's claim was the fact that the disruptions associated with the problems, and rescue, of the Continental Illinois commercial bank in the summer of 1984 led to a temporary alteration in the close relationship between borrowed reserves and the federal funds rate. See Lindsey (2003, pp. 91–92).

[183] In Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1984a, p. 81).

[184] When, in November 1981, the Committee had discussed removing a reference to the federal funds rate from the directive, Volcker agreed with Federal Reserve Bank of Boston president Frank Morris that it might be interpreted as implying that the Committee had "gone completely monetarist," with Morris quipping that it might be summed up by commentators as: "Milton Friedman has finally won!" (Federal Open Market Committee, 1981b, pp. 54, 55.)

But the switch back in 1982 to a federal funds rate-oriented operating procedure was, even by early 1983, apparent not only to Friedman but also to many commentators. Fred Levin, who had recently moved from the Federal Reserve Bank of New York to become a vice president of Citibank, and Sherman Maisel, former Board governor, both indicated in comments in late 1982 that they believed that the FOMC was managing short-term interest rates (*Commodities*, January 1983, pp. 49, 51). In the financial press, one columnist referred in mid-1983 (*American Banker*, July 11, 1983, p. 12) to "the closely controlled federal funds rate," while another referred later in the year to the Federal Reserve's "preoccupation with the targeting of the federal funds rate" (*The Australian*, September 12, 1983). As noted, the FOMC continued to publish the federal funds rate range it judged consistent with its monetary targets. The fact that the realized federal funds rates were highly consistent with that range did not go unnoticed (*Wall Street Journal*, July 12, 1983).[185]

By 1985, the regularity that short-term interest rates had become much less variable since mid-1982 was being noted in textbooks as indicating an unannounced shift back by the Federal Reserve toward managing short-term interest rates (see Baumol and Blinder, 1985, pp. 255–256). And with regard to 1989, a report to the FOMC observed that "market participants… closely followed movements in the federal funds rate to gauge the stance of [monetary] policy."[186]

The Federal Reserve had never in 1979–1982 let the federal funds rate be a wholly, unpegged, market-determined price. But it *had*, in that period, allowed the federal funds rate and other major short-term interest rates to fluctuate much more from month to month, and from quarter to quarter, than it did subsequently.

As a critic of the 1979–1982 operating procedures, Friedman had some difficulty settling on his reaction to the Federal Reserve's change in 1982. In part, of course, this reflected the fact that Friedman felt that the 1979–1982 procedures were an incomplete reform with too many holdovers of the pre-1979 arrangements. "The Fed is certainly not following my advice," he noted (*Commodities Magazine*, January 1983, p. 48), later adding, "I don't think people have

[185] Later, Marcia L. Stigum, in the third edition of her celebrated guide *The Money Market*, asked (p. 403) why, "under the current operating procedures, the level of borrowed reserves is used as a proxy for the funds rate," the FOMC did not overtly target the federal funds rate. Her answer, of course, was centered on the "smokescreen" attractions of an indirect approach, which, Stigum observed, helped avoid headlines such as "Fed Raises Interest Rates." As already indicated, however, by the time Stigum made these remarks, Federal Reserve officials were actually becoming more forthcoming in public about the fact that they essentially employed a short-term interest rate instrument.

[186] Federal Reserve Bank of New York (1990, p. 10).

abandoned monetarism because they never accepted it… How can you abandon something you don't accept?" (*Washington Post*, May 29, 1983, p. C4.) The ambivalence in his reaction was compounded by the fact that the Federal Reserve was not upfront in its public statements about having moved away from the New Operating Procedures.

Friedman nevertheless had to acknowledge that the nonborrowed-reserves targeting procedure was closer to his preferred method of conducting monetary policy than was a federal funds rate instrument. Events in 1982 amounted to what Friedman called the "shift to the earlier policy" of a funds-rate instrument rather than the full-fledged move to a total-reserves instrument he had been calling for.[187] And so in 1983, in remarks added to the printed version of a talk that he had given on U.K. television the previous September, Friedman described the summer of 1982 as the point at which "the Federal Reserve abandoned any element of monetarism and returned to its earlier practice of pegging an interest rate—the federal funds rate."[188] He would come to see the 1979–1982 experience as an aberration, rather than a stepping stone toward the total-reserves-control arrangements he favored: "the Federal Reserve has tended to focus on interest rates, rather than money. As far as monetarist policy is concerned, there has not [ever] been a monetarist policy." (*New York Times*, July 3, 1986, p. D7.)

David Lindsey was deeply involved in the Federal Reserve Board staff work on the nonborrowed-reserves arrangements throughout 1979–1982. He also frequently expounded and defended those arrangements in research conferences held over that period. Lindsey remarked of Paul Volcker: "he bought into the nonborrowed-reserves targeting, aiming at money, at just the right time. I think it was incredibly effective, and I think it wouldn't have happened if not for Friedman's influence. Volcker was very good at adjusting it to make it practical. I mean, he was a practical guy, unlike, I would argue, academics tend to be. Friedman never thought the Fed was doing anything worth a damn, and I think Friedman was wrong on that. I mean, Volcker was an absolute hero." (David Lindsey, interview, May 2, 2013.)

[187] The quotation appeared in Friedman (1983e, p. 10; 1984b, p. 30; 1985b, p. 54).

[188] Friedman (1983g, p. 54). Other monetarists were also quick to perceive the likelihood of a shift back to a federal funds rate operating procedure. For example, in October 1982 (*New York Times* October 12, 1982, p. D1), Karl Brunner was quoted saying: "Basically, Fed officials are primarily concerned with interest rates and are using interest rates to control monetary growth." Part of this, of course, had to do with the fact that monetarists were suspicious of the FOMC's continued usage of anticipated ranges for the federal funds rate during the New Operating Procedures period and so viewed the Federal Reserve as always being well disposed toward outright funds rate targeting. In this vein, when testifying on February 24, 1982, Allan Meltzer asserted that the Federal Reserve was still "trying to control interest rates or to set its targets in relation to interest rates… The Fed now tries to figure out what money growth will be by estimating what the interest rate will be at which the demand for money and the [desired] supply of money cross." (In Committee on Finance, U.S. Senate, 1982, p. 211.)

The FOMC deemphasizes M1

"Monetary policy in the United States is implemented by setting targets for several monetary aggregates," Friedman's old sparring partner, Lyle Gramley, remarked at a central banking conference on March 22, 1982. "The principal target has been the narrow money stock, M1."[189] Reflecting his status as a governor of the Federal Reserve Board, the Gramley speech was subsequently published in the Federal Reserve Board's main monthly publication, the *Federal Reserve Bulletin*, in July 1982. By the time these remarks appeared in print, however, they were rapidly being overtaken by events, with Federal Reserve monetary targeting, and M1 targeting in particular, increasingly being thrown into doubt by members of the Board and FOMC.

At first, monetary targeting was continuing in the midst of summer 1982's change in operating procedures. But monetary targeting would itself be subject to a major change in the second half of 1982. Some high-profile unhappiness with M1 had been registered by the Federal Reserve Bank of New York's president, Anthony Solomon, at the end of 1981. Solomon's criticism of the M1 aggregate was the opposite of that the FOMC would articulate in 1982: he felt that the economy was behaving more strongly than would be expected on the basis of 1981's low rate of M1 growth. In particular, Solomon declared that M1 in 1981 "seems to have been giving a misleadingly weak signal, even in view of the clear emergence of recession late in the year."[190]

Before very long, however, the decline in velocity that started around late 1981 was becoming evident, and policymakers were instead confronting a combination of nominal and real income growth slowing *more* than would be expected on the basis of past relationships with M1. Chairman Volcker's absorption of this emerging situation was reflected in the mixture of positive and negative remarks he made about monetary targeting in his panel appearance with Anna Schwartz on April 22, 1982. "We are going to maintain appropriate control of money and credit," he affirmed, and he added that "there is a central core of truth in the proposition that money, even halfway adequately measured, is going to bear some relationship over time with inflation."[191] But Volcker also referred to the "frailties" in the various monetary aggregates that meant that "we have consistently not wanted to put all our 'money,' so to speak—if I can use that phrase—on one measure of money… I think M1, recently, is perhaps subject to more question than some of the others, at least in terms of short-run fluctuations."[192]

[189] Gramley (1982, p. 396).
[190] Solomon (1982, p. 5). See also *Financial Times* (London), January 11, 1982. Much of Solomon's case was built on the fact, already stressed in the previous chapter, that M2 growth was quite strong in 1981.
[191] Volcker (1982a, pp. 7, 9).
[192] Volcker (1982a, p. 7).

Nearly six months later, in the wake of the decline in M1 velocity recorded in 1982 to date and the prospect that a number of financial developments—in the form of prospective major transfers into M1 accounts from outside M1—could reduce M1 velocity further in the period ahead, the FOMC decided that it would be deemphasizing its 1982 target for M1. The FOMC made this decision on the occasion of its October 5 meeting, at which it formally discontinued the 1982 M1 target (Federal Reserve Board, 1983, p. 127). Volcker gave a speech and press conference on the matter at the Hot Springs, Virginia, business conference shortly afterward (see Volcker, 1982b, 1982c).

In effect, this decision was equivalent to permitting a large overshoot of the original, and now abandoned, FOMC target for M1 growth in 1982. This overshoot indeed occurred: the M1 growth target range for 1982 had been 2.5 to 5.5 percent, but the outcome for the year was 8.5 percent (Dornbusch and Fischer, 1987, p. 396; Bernanke and Mishkin, 1992, Table 1, p. 190).

At the time, the move was cast by the authorities as also entailing a greater emphasis on the broader-aggregate targets, especially that for M2. For example, the Federal Reserve Board's annual report for 1982 stated (1983, p. 14) that "the FOMC at its meeting in early October decided to deemphasize M1, at least temporarily, as an operating guide for monetary policy, and instead, to place greater emphasis on M2 and M3 in the expectation that these measures would be less affected by developments in the fourth quarter."[193] But M2 was, in the December 1982–March 1983 period, affected by the introduction of a new M2 component—the competitive-yield Money Market Deposit Accounts (MMDAs)—and a large conversion of funds previously not in M2 into this new asset.[194] MMDAs' introduction, in Friedman's assessment in mid-1983, was among the developments related to financial innovations that had the potential to have the quantitatively largest distortionary effect on the monetary aggregates.[195]

The sizable shifts of funds related to the introduction of MMDAs therefore put the signal value of M2 in doubt almost as soon as the M1 target was deemphasized, with the result that Meulendyke (1998, p. 52) suggested that the FOMC's deemphasis of M1 was followed by a focus on M2 targeting "for [only] a few months at the end of 1982." This characterization probably overstated matters: although Volcker emphasized on numerous occasions during 1983

[193] See also the directives of 1982's October 5 and November 16 meetings (in Federal Reserve Board, 1983a, pp. 127, 133): these directives gave M2 and M3 growth-rate target ranges and, in contrast to the pre-October 1982 meetings, and no M1 target.
[194] The formal date of introduction of MMDAs was December 15, 1982 (Hess, Jones, and Porter, 1998, p. 484).
[195] Friedman (1983e, pp. 10–11).

that MMDAs' introduction had distorted M2 over the previous winter, and referred to the "enormous surge" in the aggregate during that period as essentially reflecting that institutional change, he did suggest that the behavior of M2 after early 1983 had considerable reliability.[196] The U.S. research community—never really won over by Friedman and Schwartz's endorsement in 1963 and 1970 of a measure of money broader than M1—continued to prefer M1 heavily until the late 1980s. And although M2 was taken seriously in 1983–1987 in Federal Reserve policy analysis, it is true that M2 never really attained prominence in policymaking in Volcker's second term. It continued to be targeted, and the targets mainly hit, in 1983–1987, but Benjamin Friedman (1988a, pp. 56–57) questioned whether M2 had been a major consideration in FOMC decisionmaking over those years.[197]

The FOMC would also extend the period over which it believed financial innovations were distorting M1. As noted, in October 1982 it had highlighted the fourth quarter of 1982 as the period ahead in which M1 was most likely to be distorted. The Committee proceeded, at its meeting held the following February, to set a target for M1 growth for 1983. In that year, however, Volcker and other Federal Reserve policymakers were soon again discounting M1 behavior, in the wake of a continuing decline in M1 velocity. In the middle of 1983, after double-digit annualized rates of growth of M1 in the first half of the year, Volcker announced that the 1983 M1 growth target would be rebased so that it applied only to a later segment of the year.

With this rebasing, Volcker was able to report in February 1984 that the FOMC's monetary targets for 1983 had basically been met.[198] But the practical effect of the rebasing was to ratify a substantial overshoot of the original M1 target for 1983. For the full year, M1 registered 10 percent growth—in contrast to the original target range of 4 to 8 percent (Bernanke and Mishkin, 1992, Table 1, p. 190).

The issue of the connection between financial innovation, financial deregulation, and the various

[196] See Volcker's testimony of February 7, 1984, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1984b, p. 72). See also Wallich (1983, p. 149) for the expression of a similar sentiment.
[197] Benjamin Friedman's subsequent finding (1997, p. 156) that FOMC responses to M2 growth became statistically significant in sample periods ending in 1983 through 1986 was, however, likely affected by his failure to allow for the fact that the FOMC made clear that its 1983 target applied really only to the final three quarters of the year—the first quarter being heavily affected by the introduction of MMDAs. As Secretary of the Treasury Regan noted at the end of that quarter: "Let us talk about the M2 target, for example. The Fed has said that it will wait and see where the February and March figures settle and then start the new growth bands from there." (*Financial Times* (London), March 30, 1983.)
[198] See his testimony of February 8, 1984, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1984a, p. 4).

monetary aggregates, and Friedman's reaction to them, will be considered below, ahead of a more detailed treatment in the next chapter. For now, it is sufficient to note that, with regard to M1 specifically, Friedman, in the initial months after the deemphasis of the M1 target, was receptive to the notion that the aggregate had been distorted by financial innovations (*Newsweek*, December 27, 1982). But, fortified by what he believed was the continued strong predictive power of M1, during 1983 he became highly critical of the FOMC's stress on distortions to M1. His commentaries were correspondingly negative about the abandonment-cum-overshooting of the 1982 M1 target and, to some extent, about Volcker's decision to rebase 1983's target.[199]

With regard to the stance of monetary policy, Friedman similarly moved from a cautious response in the fall of 1982 to a highly critical, and alarmist, perspective on the FOMC's change in posture. He seemed initially willing to take at face value the Federal Reserve's indication that it was not changing its strategy of bearing down on inflation (*New York Times*, October 12, 1982; *The Sunday Sun* (Baltimore), November 7, 1982). He certainly regarded monetary policy as having eased since July but saw the associated higher rate of monetary growth, if kept temporary, as capable of encouraging a recovery without producing renewed inflation, provided that the more rapid rate of monetary increase was not continued (*Newsweek*, December 27, 1982). As will be discussed presently, however, this posture changed in 1983 as Friedman, mistakenly, regarded the policy change as having proceeded so far that a new surge in inflation would result from it.

Contemporaneous reserve requirements are instituted

Amid its various moves from 1982 onward away from monetarist recommendations regarding its policy operations, the Federal Reserve proceeded with one of the reforms to its operating procedures that Friedman had urged, and on which he had accused it of dragging its feet.

"Contemporaneous reserve accounting?" Chairman Volcker had remarked publicly on April 22, 1982. "I think [a decision on] that will come before the Board in maybe four to six weeks."[200] The Federal Reserve Board meeting in question did not, in fact, occur for another nine-and-a-half

[199] Friedman treated the FOMC's July 1983 rebasing as an undesirable practice in Friedman (1984b, p. 37). But, as a practical matter, following the rapid growth of M1 in early 1983, he saw merit in trying to meet the 1983 M1 target on an annualized basis in the second half of 1983 rather than force the four-quarter rate back down into the target range (*Newsweek*, May 2, 1983). Consequently, Friedman had mixed reactions to 1983's rebasing: it represented a "partial restoration" by the FOMC of the pursuit of an M1 target, but it also represented a case of adjusting the target after the fact (*Wall Street Journal*, August 20, 1985).

[200] Volcker (1982a, p. 19).

weeks—being held on June 28 (*Wall Street Journal*, June 29, 1982*a*)—but its outcome was a victory for advocates of the restoration of contemporaneous reserve accounting (CRA).
What Friedman called the "huge push for contemporaneous reserve requirements" in the early 1980s (interview, January 22, 1992) had its culmination with the reintroduction, as a result of the Board's approval, of CRA into the U.S. banking system. This occurred on February 2, 1984, and the move ended the lagged reserve accounting arrangements in place since 1968. It would now be the case that commercial banks' reserve requirements would be a percentage of the reservable deposits prevailing two days earlier, rather than two weeks earlier.[201]

It was, however, a case of closing the barn door after the horse had bolted. CRA's reinstatement in 1984 was an anticlimactic development in view of the fact that, since the 1982 Federal Reserve decision to reintroduce CRA, the FOMC had changed its attitude toward both instrument setting and monetary targeting. The FOMC was much less focused on monetary aggregates than when the CRA debate had taken place, and it became even less so after 1984.

On the operational side, CRA was not used for the management of total reserves. "We have, for the time being… decided not to change our operating technique" in the wake of the institution of CRA, Volcker testified a few days after the 1984 changeover.[202] In fact, this was an understatement. The FOMC had moved away from the indirect control of total reserves that it attempted to pursue under the New Operating Procedures to a regime in which reserves provision was again guided by the need to stabilize short-term interest rates. A key aim of CRA was to allow the option of direct control of total reserves, but the FOMC by 1984 had no interest in exercising that option.[203] However important the contemporaneous-accounting/lagged-accounting distinction had been for monetary-aggregate and interest-rate behavior when the Federal Reserve genuinely had a target for a reserves aggregate, it really made little difference which arrangement was in force, provided that the Federal Reserve was targeting the federal funds rate.[204] As James Tobin

[201] CRA applied only to M1 deposits (see Lindsey, 1986, p. 191). The Federal Reserve was also targeting broader aggregates, but reserve requirements applying to the non-M1 component of these aggregates were already zero or low.

[202] Testimony of February 8, 1984, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1984a, pp. 90–91).

[203] An article by Marvin Goodfriend that appeared soon after the institution of CRA in 1984 concerned the "promises" of CRA. But the promises described required conditions the FOMC ended up not seeking: specifically, those in which it would "strictly control the volume of total reserves available to support deposits of the banking system" alongside "free-market federal funds rate adjustments" (Goodfriend, 1984, pp. 4, 7). As Goodfriend (1984, pp. 10-11) granted, if the federal funds rate was the policy instrument, CRA "will essentially make no difference."

[204] So when Volcker remarked of CRA, "I don't think it's going to make a big difference" (testimony of February 8, 1984, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1984a, p. 90), this was something on which monetarists could agree with him by 1984, because the FOMC had moved back to operating procedures in which CRA indeed made little difference.

put it (*The Economist* (London), April 27, 1985*a*, p. 24): "The irony is that these technical changes are occurring when their purpose, closer control of M1, is losing favor both at the Fed and outside."[205]

Lagged reserve requirements would be restored in 1998. Then, in March 2020, the Federal Reserve Board's lifting of all reserve requirements on depository institutions made the LRA/CRA distinction completely moot.

During the CRA debate of the early 1980s, Friedman had suggested that CRA had benefits for monetary control even when the federal funds rate was the instrument: as the link between banks' reserves demand and their current deposit creation was blurred, the central bank had a diminished perception of current developments in monetary growth. Hence, Friedman suggested that "the effect of lagged reserve accounting was to postpone the Fed's recognition of the need for a change" in the target value for the federal funds rate (*Wall Street Journal*, February 1, 1982).

But because monetary growth was of reduced importance in policy setting when the CRA was instituted, this feature of CRA did not much affect post-1984 policy decisions. But the funds rate was more responsive than before 1979 to other nominal variables. Consequently, the era of renewed federal funds rate targeting did have the property that Friedman in 1982 had stressed as desirable: prompt recognition of the need for a change" in the funds rate.

Interest rates and policy strategy

The enhanced responsiveness, especially with regard to actual or prospective pressure on aggregate nominal spending, of the post-1982 reaction function is something that deserves underscoring. In that connection, it is worthwhile to consider how the policy regime that emerged in that year has come to be seen in retrospect. That is the concern of the discussion that now follows. It serves as a backdrop for the consideration below of some of the immediate, and largely misguided, interpretations Friedman made of the emerging post-1982 reaction function—and of the associated faulty macroeconomic predictions that Friedman gave as a result of his misinterpretations of unfolding monetary developments. A look at key aspects of the post-New Operating Procedures reaction function from a longer-term perspective will bring out the fact

[205] Feinman (1993, p. 578) made a similar observation: "Ironically, by the time CRR [the system of contemporaneous reserve requirements] was instituted in 1984, the Federal Reserve had shifted its focus away from short-run control of M1 via a reserves-based operating procedure…. It also shifted its focus more toward M2…" The last point was not actually correct regarding 1984, which—as discussed later in this chapter—was a year in which the FOMC temporarily reemphasized the M1 target.

that—although Friedman was immediately inclined to perceive a return to 1970s-style policymaking in the post-1982 FOMC arrangements—those arrangements actually adhered to some key Friedman ideas on monetary policy and inflation, even while eschewing, and moving further away from, his specific, reserves- and money-stock-focused, policy recommendations.

After the dust had settled on the policy change in 1982, it would become possible to see that the post-1982 policy was very different from Friedman's 1983 labeling of the new policy arrangements of "pegging an interest rate," or, at least, that the interest rate was not really pegged beyond the very short run. The vigor with which the FOMC, over periods of a month to a quarter, adjusted its short-term federal funds rate target in response to nominal variables set the post-1982 period apart from the pre-1979 regime (see Clarida, Galí, and Gertler, 2000).

That adjustability of the interest rate would in turn reflect, in part, the fact that the Federal Reserve had not actually, as Friedman had suggested in 1983, "abandoned any element of monetarism." Importantly, it continued to accept the insight that inflation was a monetary phenomenon. This will be a point discussed further in later chapters.

Relatedly, monetary policy after 1982 continued to be focused on ending inflation. Consistent with its legislative mandate, which included a stable prices objective, and with its perception of what monetary policy was capable of, the FOMC's federal funds rate policy from 1982 and into the next century oriented federal funds rate policy closely toward price stability. This was an element of continuity over Volcker's tenure. At the April 1982 event in New York City at which Anna Schwartz was a panelist, held when disinflation was showing strong signs of setting in, Volcker remarked: "We're getting good price performance and was encouraged also by increasing "confidence that it's going to continue."[206] That continuation, as he saw it, should involve "the restoration of a sense of greater price stability" and, to this end, he affirmed that he wanted disinflation to continue: "The challenge as I see it is to take this period and make it a platform for building into the economy a trend toward a lower rate of price increase and eventual price stability."[207]

Almost a year later, with inflation having fallen to about 4 percent, Volcker indicated that, for one thing, an aim of the FOMC was to keep it down: "Our policies are designed to provide what reassurances we can that inflation is not going to up again."[208] But he further indicated on

[206] Volcker (1982a, p. 20).

[207] Volcker (1982a, pp. 3, 3–4).

[208] Testimony of February 24, 1983, in Committee on the Budget, U.S. Senate (1983, p. 648).

numerous occasions in the wake of the 1982 policy changes that he wished not only to maintain the reduction in inflation achieved in his third year in office but also to consolidate it. "I've said it on a number of occasions that I think the inflationary momentum that has come to grip the economy in the'60s and the '70's has been broken, and I do think the prospects are good for some further reductions—that we can build up a momentum toward price stability," he remarked at his press conference of October 9, 1982.[209] Similarly, in testimony given the following March, Volcker stated that one key policy aim was to dispel skepticism about the permanence of the decline in inflation.[210] But, he added that he regarded 4 to 5 percent inflation rates as costly for the economy and stressed "the critical importance of maintaining the momentum against inflation."[211] And in early 1984, Volcker remarked specifically: "We are seeking to bring about an environment of rough stability in the general price level."[212] "Paul wanted to get rid of inflation," Stephen Axilrod observed (interview, April 24, 2013). "He really wanted to."

The Federal Reserve's acceptance of the causes of inflation and the affirmation of the aim of price stability were also accompanied by a reaffirmation of the notion that price stability could be helpful to achievement of real goals. Numerous statements by senior Federal Reserve officials during the 1970s had stressed that inflation was costly for the behavior of real variables and had eschewed long-run-nonvertical Phillips-curve tradeoff ideas (as well as parallel notions that inflation stimulated economic growth). But policymakers' adherence at the time to cost-push views of inflation had meant that the price-stability/economic-stability link had not been adequately connected by policymakers to the setting of aggregate demand policy. This changed under Volcker, who had written in a letter of August 28, 1980, on the matter of monetary restriction: "These efforts do not in my mind reflect simply a concern about the need for price stability for its own sake—critical as that is. The experience of the 1970s strongly suggests that the inflationary process undercuts efforts to achieve and maintain other goals of growth and employment."[213] In the post-recession period, Volcker correspondingly observed: "We've paid the price once. Let's consolidate these gains. I think that's the only basis upon which we can expect the economy to have sustained growth. By the time I finish here [as Federal Reserve

[209] Volcker (1982b, p. 2). Also quoted in *The Sunday Sun* (Baltimore), November 7, 1982.
[210] Committee on the Budget, U.S. House of Representatives (1983, p. 4).
[211] Committee on the Budget, U.S. House of Representatives (1983, p. 5).
[212] From a written answer, added to Volcker's February 1984 testimony, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1984b, p. 92).
[213] See Volcker (1980b). Volcker had used a similar formulation in Congressional testimony (see, for example, the attachment in Committee on Banking, Finance, and Urban Affairs, United States, 1980b, p. 328). Indeed, remarks on Volcker's part in the same spirit recurred often in his tenure, including in a PBS interview given soon after the New Operating Procedures were introduced (see Lindsey, Orphanides, and Rasche, 2005, p. 207).

chair], I hope… [to see] a more stable economy, a more satisfactory economic performance, and a longer period of disinflation."[214]

Volcker's formulation aligned Volcker with Friedman's own hope that the post-1982 period would see the "long, healthy and noninflationary recovery we so desperately need" (*Newsweek*, February 7, 1983). Friedman also recognized that the ultimate inflation aim that Volcker relayed in his remarks during 1983 was one of "price stability."[215] But, as discussed later, Friedman—erroneously, it would prove—felt by the time Volcker made these remarks that the FOMC had already moved in ways that would prevent the desirable outcomes of a long expansion and a declining inflation path.

Subject to the acceptance of a price-stability-oriented monetary policy, on what nominal variable did the post-1982 FOMC focus in making its interest-rate policy? As already indicated, the Committee certainly drifted away from targeting monetary aggregates and, on that score, moved away further from monetarism. Monetary targets remained a part of U.S. monetary policy for the rest of the century—and, indeed, received some renewed attention during the early years of Alan Greenspan's tenure. But the truly central role that had been assigned to monetary targets in the period from 1979 to 1982 was not restored.

The receding in Federal Reserve discussions of monetary aggregates, and monetary targeting specifically, was captured in the fact that the Federal Reserve Bank of Boston found that it was topical to hold conferences on monetary-aggregate control in 1969, 1972, and 1980. But after the third of the conferences—which formed the basis for the volume *Controlling Monetary Aggregates III*—there was no conference or volume titled *Controlling Monetary Aggregates IV*.

To many observers in the 1980s, post-1982 policy arrangements were well approximated by nominal income (at the time, nominal GNP) targeting. Certainly, public discussions by Federal Reserve policymakers were marked by much more focus on the possibility of increased variability in velocity and a change in trend, and of indications that the FOMC was allowing for such velocity movements in adjustments of monetary policy stance.

[214] From Volcker's testimony of July 14, 1983, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1983a, p. 32). Other policymakers of this era expressed similar sentiments. For example, Wallich (1983, p. 154) referred to "those of us who regard price stability as an essential condition for long-run economic growth," and the Council of Economic Advisers (1983, p. 144) stated: "A major prerequisite for achieving our economic goals is control of inflation."

[215] See Friedman (1984b, p. 56). He had earlier noted "the long-term objective of the Fed to slow inflation or at least stop it from accelerating" (*Newsweek*, December 27, 1982).

In the United Kingdom, *Financial Times* columnist Samuel Brittan had already called for the adoption of nominal income targets (see Brittan, 1983, pp. 83–104, 125–155). He stressed (*Financial Times* (London), January 7, 1982) that "Friedmanite monetarists ultimately want to regulate money GDP" and had become convinced that monetary targets were not the best contribution that could be made for this purpose.[216] And in his parting of ways with Friedman on this point, Brittan was also willing to "leave a good deal of discretion" to monetary policymakers for the purpose of achieving the nominal-income goal (*Financial Times* (London), July 15, 1982). After the U.S. M1 velocity decline was in motion, Brittan suggested that Volcker's FOMC was targeting nominal income. Brittan argued that allowance for a decline in velocity should definitely be made because disinflation had reduced the opportunity cost of holding money.

The notion that U.S. policymakers were focused on nominal income targeting was reinforced by the supportive remarks regarding such a strategy that appeared in the *Economic Report of the President* in 1983. This strengthened the impression that the United States had moved to a nominal income targeting regime, and some commentators took this largely to be a fact (*Financial Times* (London), February 3 and 5, 1983; see also Emerson, 1984, p. 224). Although the *Economic Report* could not speak totally authoritatively regarding the Federal Reserve's monetary policy strategy, the notion that nominal income targeting, alongside a return of interest-rate management, might succeed the New Operating Procedures had also tentatively suggested in December 1981 by the FOMC's Anthony Solomon (1982, p. 15).[217]

Indeed, for a while, the notion that the FOMC was engaged in nominal income targeting was quite prevalent. In the early 1980s, James Tobin had become an advocate of nominal income targeting, albeit with incomes policy used as an additional weapon) (see, for example, Tobin, 1980b, p. 75), and he endorsed this strategy again when he and Friedman were both keynote speakers at the June 1983 Bank of Japan conference (Tobin, 1983b, p. 27). Tobin had been an outspoken critic of the New Operating Procedures regime, including in an August 1982 op-ed titled "Stop Volcker from Killing the Economy" (*Washington Post,* August 15, 1982). But after the 1982 Federal Reserve policy change, Tobin became a renewed supporter of U.S. monetary

[216] In contrast, another former Friedman student, David Laidler, although sharing some of Brittan's doubts about Friedman's criticisms of post-1979 monetary policy and his concern about instability in the money/income relationship, continued for the moment to take the view that "money growth targeting is probably as good a policy regime as we shall ever devise for stabilization purposes" (Laidler, 1983, p. 103).

[217] Furthermore, in March 1982, Federal Reserve Bank of Boston's president Frank Morris also indicated that he preferred nominal GNP targeting to M1 targeting. But he seemed to prefer targeting a credit series to either of these strategies. See F.E. Morris (1982a, pp. 82–84) and the discussion below titled "Benjamin Friedman."

policy, which he viewed as having moved largely in line with his own prescriptions: "In effect the Fed is targeting 'velocity-adjusted' monetary aggregates. That amounts to targeting nominal GNP."[218] And Franco Modigliani (1988, p. 401) interpreted the whole of post-1979 Federal Reserve policy as one of targeting nominal GNP.

John Taylor, then at Princeton University, had even in 1982 interpreted the whole Volcker period to date as one of targeting nominal income rather than monetary growth: "The *actual* change that occurred near October 1979 was that [the] Fed began, much more seriously than in earlier years, to reduce the rate of growth of *nominal GNP* in order to reduce the rate of inflation."[219] Later, he presented a similar interpretation for the years from 1982 to 1995 (see Hall and Taylor, 1997, pp. 488–493). Elsewhere, however, Taylor (1993) characterized the period from 1987 onward as being instead well approximated by a federal funds rate responding separately to inflation and the output gap, and this characterization was applied empirically to the whole period from 1982 to 1996 by Clarida, Galí, and Gertler (2000, pp. 160–164). The position that the FOMC from 1982 responded to these two variables separately, rather than pursuing a nominal income target, has become the most prevalent interpretation.

In the summer of 1982, Friedman confirmed that he continued to oppose direct targeting of inflation, as he felt that the lag between monetary policy actions and inflation made this strategy impracticable.[220] Publicly, Chairman Volcker expressed a similar sentiment in the fall of 1982.[221] As already indicated, however, he frequently articulated a price-stability goal. And this goal is thought to have figured heavily in the Federal Open Market Committee's reaction function under

[218] *The Economist* (London), April 27, 1985*a*, p. 27. In the mid-1980s, Tobin also took the Federal Reserve as having settled on a *de facto* long-run inflation objective of about 4 percent—a development of which he approved. As already indicated, however, in his public statements in 1982 and later, Volcker characterized monetary policy as actually seeking progress toward lower inflation rates than 4 percent.

[219] Taylor (1982a, p. 157).

[220] See Friedman's letter to Senator Roger W. Jepsen of August 16, 1982, in Friedman (1982d, p. 74). See also Nelson (2008, p. 98) for a related discussion of these issues.

In the past, Friedman had been concerned that the Federal Reserve might overdo monetary restriction by basing its policy decisions on the behavior of current inflation. Most notably, this was his fear in the wake of the monetary tightening that began in October 1968. Six months into that tightening, Friedman suggested that "the danger is that by not having patience, [not] waiting for the disinflation [in the pipeline] to manifest itself, [not] waiting for the monetary tightness to manifest itself, you remain in the danger of considerable overkill." (Instructional Dynamics Economics Cassette Tape 20, April 1969.) This remained his concern ten months later, when he suggested: "I think it may well be that we are going to have a *more* severe recession than would have been necessary, because the monetary authorities were so anxious to slow down inflation and had insufficient patience, so that they stepped on the brake too hard." (*Inflation: Causes and Control*, Learning Plans audiocassette (Tucson, Arizona), February 1970.) In the event, the 1969–1970 recession was less severe than he predicted, and the impatience shown by policymakers in the early 1970s was of a different nature—their feeling that the post-recession recovery was too slow and that policy settings needed considerable further relaxation.

[221] See his statement in Joint Economic Committee, U.S. Congress (1982b, p. 34).

his tenure.

In time, the fact that the Federal Reserve accepted a framework of centering monetary policy on inflation control would be understood as producing a major difference between Federal Reserve interest-rate policy after 1982 and its pre-1979 policy. But this conclusion would become widespread only much later than 1982, and Friedman's immediate reaction to 1982's developments was not to see it in these terms. Instead, as already implied, he recoiled at what was basically a reversion to 1970s operating procedures. Friedman had been pressing for institutional changes that would move toward an environment in which the Federal Reserve would set total reserves without regard for the implications for interest rates. Now, it had gone in the opposite direction.

A good prediction: Friedman on 1983's economic recovery

In 1982 and 1983, Friedman did better than most other economists in projecting the course of U.S. real output. Starting in mid-1982, during the recession, and through much of 1983, he established a good record of predictions regarding the timing and strength of the U.S. economic recovery—although this success would be quickly overshadowed by major forecasting mistakes on his part regarding U.S. economic performance in 1984.

During the period in which the 1981–1982 recession was nearing its trough, Friedman noted: "You never see much sign of recovery until it's on you."[222] He was nevertheless confident that economic recovery would soon begin. The basis for his belief that the U.S. economy would move from recession to expansion was the upward shift in monetary growth seen in the second half of 1982. Friedman's projection would be vindicated by the U.S. economy's behavior starting in late 1982 and continuing over the course of 1983.

This economic revival would render moot assessments such as that John M. Berry made in the *Washington Post* (December 12, 1982, p. H1): "Faster monetary growth has not generated the economic recovery it was supposed to nurture." Instead, and in keeping with Friedman's predictions at the time, the retrospective verdict on 1982–1983 would be that, in the words of Paul Krugman a couple of decades later (*New York Times*, June 11, 2004), the "decision to loosen up on the money supply in the summer of 1982… set the stage for the [economic]

[222] *Barrons*, October 25, 1982 (p. 7).

rebound a few months later."[223]

The "few months" in this case was about four months. As Friedman noted in June 1983, the U.S. "economy apparently reached its trough and started recovering in November 1982."[224] This dating of the trough is supported by the modern-day NBER business-cycle calendar.[225] It is also backed up by the fact that the real GDP observation for 1982:Q4 shows slightly positive growth.

The national economic trough had clearly been preceded by a change in M1 growth's behavior. The January-July 1982 period of mostly slow monetary growth, already discussed, was succeeded by what Friedman called "the monetary explosion from July 1982 to December 1982."[226] On modern data, this shift is manifested in annualized M1 growth of 12.6 percent in the six months to December 1982, compared with 5.0 percent in the six months to June 1982. Then, as discussed below, continued rapid growth in M1 took place over the first half of 1983.

In New York City on September 2, 1982, in a briefing to clients of Oppenheimer and Company, Friedman tentatively dated the end of the period of the slow M1 growth to late July 1982. The upturn in monetary growth had been in progress since then, he suggested, would probably be followed by an economic recovery starting in the fourth quarter of 1982.[227] Similarly, the following day, speaking to Oppenheimer's marketing staff, Friedman remarked (*Toronto Star*, September 7, 1982): "There will be a recovery getting underway in a few months. Maybe it's already started [or] maybe it will start in October or November."

The monthly data on spending and production suggest—consistent with the NBER dating— that the U.S. economy turned around in November or December 1982, with the unemployment rate then peaking slightly later, at 10.8 percent, in December 1982. Friedman's basis in September 1982 for nominating October as a possible date for the economy's turnaround had been his rule of thumb that, since October 1979, economic downturns and revivals had lagged the corresponding sharp movements in monetary growth by about three months (*Newsweek*, July 12,

[223] Similarly, much earlier, *Time* magazine (April 25, 1983, p. 96) characterized matters as: "last summer the Federal Reserve started allowing the money supply to grow, thus setting the stage for the present recovery." Paul Samuelson stated a little over a year later (*The Plain Dealer* (Cleveland, Ohio), June 2, 1984): "We should be grateful that the Federal Reserve brought the 1981–82 recession to an end by its abandonment of monetarism in mid-1982. Its controlled acceleration of the money supply in the following several months was one of the happier exercises in economic stabilization." See also the discussion of Samuelson's mid-1980s commentaries that appears in the discussion titled "Benjamin Friedman" later in this chapter.
[224] Friedman (1983e, p. 10). Also in Friedman (1984b, p. 30).
[225] See *https://www.nber.org/research/data/us-business-cycle-expansions-and-contractions*.
[226] Friedman (1983e, p. 11).
[227] Oppenheimer and Company (1982, p. 4).

1982). He later noted, however, that starting in 1982 the more familiar average lag of about six months from monetary growth to rates of increase in nominal and real income appeared to reassert itself—and that the return of this longer lag pattern helped account for the relationship between monetary growth and economic activity from 1982 to 1984.[228]

A short-term result of this process of reversion to more standard lag patterns was that high monetary growth and a weakening economy coexisted in the late months of 1982. Friedman maintained that this was a temporary condition: "the monetary explosion that started in July 1982 is almost certain to bring on a recovery starting within the next few months." (*Newsweek*, December 27, 1982.) This proved to be the case, and, as he submitted this end-of-year column, the economic recovery was actually in its very early stages.

As far as the strength and durability of the recovery were concerned, Friedman's confidence in a strong, multi-quarter rebound was fortified as more high readings on M1 growth came in. When the Federal Reserve deemphasized its M1 target in early October, Friedman did not, as has already been discussed, immediately deduce from the move that policymakers' operating procedures were going through an overhaul—"I do not know what the fuss is about," he observed (*New York Times,* October 12, 1982, p. D1). What the announcement did make clear, however, was that the FOMC would not be taking steps in the near future to move M1 growth down from its recent higher rates. So a scenario that Friedman highlighted as "more likely than not" in his September 2 briefing—that the M1 surge would be a short-lived one, around three months like that seen in late 1981 and early 1982—had, in effect, been made much more remote.[229]

With a longer stretch of M1 strength in prospect, Friedman was fortified in his recovery prediction. "Assuming that the Fed continues, as I believe it will for a time, with the monetary explosion that is now in progress, the economy will recover," he declared in October (*Barron's*, October 25, 1982, p. 7).

Furthermore, at a press conference held in the city of Chicago in early 1983, Friedman predicted that economic growth would be strong during 1983 (*Chicago Sun-Times*, January 19, 1983; *Sydney Morning Herald*, January 21, 1983). This posture contrasted with the tenor of the economic predictions being made in many quarters in late 1982 and early 1983. A headline in

[228] Friedman (1985b, p. 55).
[229] The quotation is from Oppenheimer and Company (1982, p. 4).

October 1982, "Experts See Slow, Sure Recovery," summarized the center of opinion in this period (*Detroit Free Press*, October 15, 1982; see also López-Salido and Nelson, 2010).

In a *Newsweek* column written in late January 1983, Friedman discounted the so-called consensus forecast, among watchers of the U.S. economy, of a weak recovery from the recession. The similarity in the numbers produced by major forecasters amounted to "misleading unanimity," he suggested. It was not a case of each forecaster making a separate judgment. On the contrary, he stressed, professional forecasters typically talked to one another and read each others' work. Each had a good conception of the consensus view—and there were incentives among professional forecasters not to deviate too far from the consensus. Friedman, in contrast, ostentatiously bucked the consensus forecast in his column's analysis. He insisted that "1983 will be a year of vigorous and rapid economic growth," citing the stimulus to the economy arising from the mid-1982 shift to strong monetary expansion (*Newsweek*, February 7, 1983).[230]

His column also highlighted the fact that "there has long been a systematic relation between the severity of a recession and the vigor of the subsequent recovery." In particular, as the 1981–1982 recession had been severe, it was likely that 1983's output behavior would complete a large " '*V*' that has a sharp decline and [then] a sharp rise." (*Newsweek*, February 7, 1983.) This was a point that Friedman reaffirmed in an interview in the spring: "The stage is certainly set for a robust recovery. The recession we have just been through has been long, drawn out, and relatively severe. History tells us that severe recessions are usually followed by vigorous expansions." (*Boardroom Reports*, May 1, 1983, p. 1.)

These analyses therefore applied to 1983's economy a postulate Friedman had articulated in 1964 of a recurrent *V*-shaped pattern in U.S. business-cycle dynamics—what he would later in the 1980s term the "plucking model"—in which deep recessions presaged rapid growth in output in the early years of the recovery. In 1964, his exposition had been explicitly advancing the property as supportive of a monetary perspective on the business cycle.[231] At that time, and again

[230] In his *Newsweek* column, Friedman stressed that the professional economic forecasters had an incentive to make a forecast that was distinctive enough to attract attention, but not far enough away from the center of opinion to attract adverse criticism if the forecast went badly. He had earlier expressed this opinion about the construction of forecasts in Instructional Dynamics Economics Cassette Tapes 68 (February 24, 1971) and 99 (May 17, 1972). Although the authors did not cite Friedman, his position on forecasts was substantially borne out by the study by Laster, Bennett, and Geoum (1999). The authors' summary of their results includes the statement (p. 293): "Those whose wages depend most on publicity produce forecasts that differ most from the consensus."

[231] In the 1983 column, the *V* property and Friedman's monetary view of the cycle were presented as "two general considerations" pointing toward the expectation of a rapid recovery. But his 1964 position that the *V* pattern was itself a corollary of his monetary view of the economy was embedded in the 1983 column's statement that it was rapid monetary growth that "assures" a sharp upturn.

in 1983, he saw the sharp-dip/sharp-rebound property as largely flowing from the practices of the U.S. monetary authorities. Although there had been changes in their reaction function over the decades, the authorities had repeatedly engaged in policy responses that implied major troughs in monetary growth followed by steep rebounds of monetary growth. Growth rates of nominal income and real output, Friedman believed, inherited this shape, thanks to their dependence on monetary developments. The plucking property of the business cycle was, therefore, not an intrinsic feature of real output dynamics but, instead, was a result of the short-run link between monetary policy and real economic activity.

Although a monetary, and essentially model-based, vision of economic behavior underlay his prediction of rapid expansion, Friedman stressed in his column that his prediction was not a formal one in the sense of being derived from "a detailed 'econometric' forecast" (*Newsweek,* February 7, 1983). Nevertheless, econometric models used by others at the time that had monetary aggregates prominently featured gave the same picture of a rapid economic expansion ahead. Especially notable here were the largely automated forecasts of Doan, Litterman, and Sims (1983), who used a ten-variable vector autoregression (VAR) estimated on data starting in 1948:Q1. The authors had M1 in their forecasting system, and their VAR model predicted "an extremely vigorous recovery" that, as they noted, implied a much more rapid 1983 economic expansion than did the February 1983 consensus forecasts (p. 29). They specifically contrasted their prediction, made on the basis of a VAR estimated through December 1982, of 8.8 percent 1983 real GNP growth with the Congressional Budget Office's forecast of 4 percent growth. These projections of strong growth were vindicated: modern real GDP data put growth in the four quarters to 1983:Q4 at 7.9 percent. The rapid expansion during 1983 was also registered in the extent to which the unemployment rate declined from 10.8 percent in December 1982 to 8.3 percent in December 1983.

"We have had a more rapid period of growth than was anticipated by virtually everyone at the beginning of the year," Paul Volcker testified to the Joint Economic Committee on October 20, 1983.[232] The following week, in remarks at a Hoover Institution event, Friedman underlined a key reason why Volcker had had to say "virtually." "I want to call to your attention that in October, November, and December of 1982, only one group of economists was predicting a very vigorous expansion in 1983, namely that small group of monetarists…"[233] In an interview that he gave a few months later, Friedman again boasted of the monetarists' success in predicting

[232] In Joint Economic Committee, U.S. Congress (1983c, p. 2).
[233] Remarks of October 25, 1983, in Friedman (1984a, p. 4). See also his similar comment, made a few weeks earlier, in Friedman (1984f, p. 39).

growth for 1983 in an interview: “Money watchers predicted a vigorous year to come. Every one of them was right.” (*Fortune*, March 19, 1984, p 34.)

Even outside the monetarist camp this success was acknowledged, with Samuel Brittan, remarking (*Financial Times* (London), December 1, 1983), “A well-known Governor of the Fed admitted to me the other day that the monetarists were the only economists to predict the strength of recovery.”[234] Brittan was very likely referring to Henry Wallich. Wallich was probably particularly impressed that monetary growth had sent the right signal about economic growth even in circumstances in which real interest rates remained high. He had been doubtful earlier in the year that would occur (see Meltzer, 2009b, p. 1132, 1134), and even in midyear had contended that, *vis a vis* monetary aggregates, “almost everything else we watch is telling us a different and more sensible thing” (*Newsweek*, June 27, 1983*a*, pp. 67–68).[235]

The interest-rate picture in 1983 was notable. The lack of further interest-rate declines in 1983 had confounded Secretary of the Treasury Regan’s statement that “interest rates can and will come down in ’83” (*Sydney Morning Herald*, January 25, 1983), and the rise in short-term rates during the year was in contrast to the judgment he had expressed that there was no need for such a rise (*Financial Times* (London), March 30, 1983). In midyear, an administration spokesperson remarked: “We don’t want the discount rate raised.” (*Daily News* (New York), July 8, 1983.)

Friedman, in contrast, was more sanguine about the prospect of a cessation or retracement of the decline seen in interest rates over the June 1982-April 1983 period. In the spring of 1983, he observed (*Boardroom Reports*, May 1, 1983, p. 2): “There’s no basis for those warnings, which business-people are reading all the time, that the recovery will be cut off unless rates fall significantly. You have to ask: *Why* are rates so high? If they are high because there’s a strong demand for credit—and managers can identify when that’s the case—then the high rates are a bullish factor.” (*Boardroom Reports,* May 1, 1983, p. 2.)[236] He would caution that measurement

[234] Benjamin Friedman (1990, p. 165) implied that “public commentary” in 1983 (including, by implication, that of Milton Friedman) failed to recognize the possibility that high monetary growth would mean high real output growth—and so suggested that monetarists only predicted high inflation to result from the higher rate of monetary expansion. However, as we have seen, Friedman *did* predict a strong economic recovery in 1983. He reaffirmed that prediction in May 1983, telling Reuters: “I think we are in a recovery, and I have no doubt it will continue throughout 1983.” (*The Lethbridge Herald* (Lethbridge, Ontario, Canada), May 10, 1983. Also quoted in *Washington Times* (Washington, D.C.), May 10, 1983.)

[235] The course of real interest rates will be discussed further later in this chapter. One reflection of this high real rate was that by the Taylor (1993) rule criterion, the nominal federal funds rate in the years 1982 to 1984 was “too high” (Taylor, 1999a, p. 337), although Taylor was cautious in interpreting this result.

[236] In making his comment that business-people were being exposed to such warnings, Friedman may have had in mind, in particular, the fact that in a recent *U.S. News and World Report* symposium, he had dissented when four

of real interest rates was difficult immediately after a major decline in inflation.[237] And he was, as noted above, skeptical about postulates that specific economic outcomes should be associated with particular values of interest rates. "The argument that U.S. business can't have a long or healthy expansion until interest rates come down sharply may or may not be true. There's no strong theoretical basis, however, for believing it *has* to happen that way." (*Boardroom Reports*, May 1, 1983, p. 2.)

A bad prediction: the 1984 recession that didn't happen

Friedman nevertheless *did* see the new recovery as likely to be cut off. He mistakenly believed that there would be a recession in 1984. This prediction would receive wide publicity over the months in which Friedman made it. The failure of that prediction was also widely noted, especially because Friedman ultimately decided to pinpoint the predicted start of the recession as 1984:Q1—a quarter that would, instead, see one of the most rapid growth rates in postwar U.S. history. This jarringly incorrect forecast of real output behavior in 1984 would largely overshadow Friedman's success in predicting 1983's vigorous expansion.

The starting point for Friedman's recession prediction was the rapid monetary growth that began in late July 1982. As detailed below, that monetary expansion had been in progress for only about three months before Friedman laid out as a likely scenario the occurrence of a 1984 recession.

Friedman did not *altogether* disapprove of the rapid monetary growth in July-December 1982: he recognized that there had been some underlying decline in velocity in 1982 that could justify temporarily higher monetary growth, and he also had stressed the need to move away from the very weak M1 growth in the first half of the year that, he believed, had prolonged the recession. Friedman nevertheless regarded the monetary expansion as having already been overdone by the end of 1982. His judgment regarding the actual annualized M1 growth rate observed in the six months from July 1982, a rate he gave as having been 15 percent, was: "That is excessive." (*U.S. News and World Report,* January 31, 1983, p. 66).[238]

other Nobel laureates in economics had insisted that economic recovery required lower interest rates (January 31, 1983, p. 66).

[237] See the summary of his remarks in late June 1983 recorded in Ando, Eguchi, Farmer, and Suzuki (1985, pp. 4–5), as well as his observation a year later in Heller and others (1984, p. 48).

[238] Friedman, who had previously given the July-November 1982 increase as 16.3 percent (*Newsweek*, December 27, 1982), may have reached the number of 15 percent for July-December 1982 by using weekly data on M1. On monthly-average data as of January 1983, M1 in December 1982 was 6 percent above its July 1982 level, implying a

In the wake of a period of what he considered monetary excess, Friedman did not suggest trying to counter the rapid rise in M1 with a new stretch of below-normal growth. Indeed, as already indicated, he had been critical of the FOMC for having, in effect, followed this practice during the 1979–1982 period, and in doing so magnifying short-run swings in monetary growth. Friedman's preferred course—what he labeled the "most important single action we can take now"—was to start the new year of 1983 with a moderate M1 growth rate target of 5 or 6 percent and for the Federal Reserve to keep monetary growth at that rate over the whole year (*U.S. News and World Report,* January 31, 1983, p. 66).

The possibility of alignment between Friedman's prescription for 1983 and Federal Reserve policy actions was raised by the FOMC's chosen M1 growth target range for 1983. This range—announced in February of that year (Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1983b, p. 16; Hafer, 1985a, p. 18)—of 4 to 8 percent was consistent with the Friedman prescription. But Paul Volcker's public statements during early 1983 repeatedly indicated that he and the FOMC continued to have doubts about the reliability of the M1 aggregate. And before long, in the face of a continuing decline in M1 velocity during the first half of 1983, large overshoots of the target were being permitted. Reflecting this choice, the expansion of M1 in the first half of 1983, as in the second half of 1982, proceeded at double-digit rates: on modern data, 12.2 percent annualized in 1983:Q1 and 13.0 percent in 1983:Q2.

It is clear in retrospect—and was the judgment of many nonmonetarist economists at the time—including James Tobin, who had a negative view of Volcker's first three years as chair, including what Tobin called the "disastrous year" of 1982 (*The Economist* (London), April 27, 1985*b*, p. 24)—that the FOMC made the right decision both in permitting rapid monetary growth in the second half of 1982 and in letting it continue in 1983. That rapid monetary growth offset a permanent decline in M1 velocity while also being of a rate consistent with a strong but noninflationary economic recovery. In Friedman's assessment during 1983, however, the FOMC was compounding the excessively relaxed stance to which it had moved in the second half of 1982. Accordingly, his position hardened that these developments presaged a major monetary restriction that would itself be followed by a new recession.

It would later be reported that of those predicting a new recession, "Professor Milton Friedman has been a leader in this group. In a private memo to President Reagan as long ago as December

14.46 percent annualized rate of increase (Council of Economic Advisers, 1983, p. 233). These values are similar on modern vintages of data: 6 percent and 14.4 percent, respectively.

1982 he warned the Federal Reserve's policies could bring about a recession in 1984." (*Daily Mirror* (Sydney), January 25, 1984.) This memorandum, submitted via the PEPAB forum and already discussed, was actually submitted to that body in January 1983 rather than December 1982.[239] Friedman did indeed give the recession-scenario warning in that memo, referring to the "dismal prospect" that a sharp shift in Federal Reserve policy would give rise to "renewed recession in late 1983 or early 1984."[240] He did so again in an in-person exchange with Paul Volcker when the latter attended the PEPAB meeting of April 20, 1983, held, as usual, at the White House. Although this was a closed-door event, Friedman's indictment of Volcker would be reported in the media (for example, *New York Times,* May 16, 1983).

As was so often the case, however, sentiments expressed by Friedman in internal documents and non-public meetings echoed those he was articulating publicly. During 1982 and 1983, he made many public recession warnings. In the fall of 1982, Friedman had already come to the conclusion that the monetary expansion begun in July 1982 was becoming overdone and that it would likely culminate in steep interest-rate increases and a new economic contraction. He began airing his judgment in commentaries and interviews. In an interview appearing in the second half of October 1982, Friedman observed (*Barron's*, October 25, 1982, p. 7): "Unfortunately, I would say that the odds are better than even that a relatively short [economic] expansion will once again be aborted by the Federal Reserve stepping on the brakes too hard." For the immediate future, he saw recovery ahead, but late in the year, even before economic expansion became apparent, he cast matters as follows (*Commodities*, January 1983, p. 51): "The real question is how long that recovery will last." In particular, he continued, "it is a real possibility that recovery will not last more than 9–15 months and will be followed by another recession. I hope I am wrong but, unfortunately, that's what it looks like now."

In Friedman's *Newsweek* column of December 27, 1982, he spelled out his recession scenario. The outlook he described was one in which the FOMC attempted to slow down the post-July 1982 expansion, setting the scene for an early end to the recovery. "The monetary explosion is then likely to end with a bang and be followed by negligible or negative monetary growth." Consequently, while U.S. economic recovery was imminent, it would likely be quite short-lived. "As in 1981, [the shift to monetary contraction] would abort the recovery and produce a renewed recession, probably early in 1984." (*Newsweek,* December 27, 1982.)

[239] There was, however, an earlier version of the memorandum, which Friedman sent to the Federal Reserve Board's David Lindsey on December 9, 1982. (Letter from Lindsey to Friedman, December 20, 1982, Federal Reserve Board records.)
[240] Friedman (1983f, p. 4).

On output, as already indicated, the prospects that Friedman saw in late 1982 and early 1983 for 1983 as a whole essentially corresponded to what emerged: rapid real economic expansion. On interest rates, too, although excessively ominous in portraying the likely scale of the move, he was right about the direction. At the end of 1982, he predicted interest rates would rise in 1983: as the "recovery gathers steam, interest rates will erupt."[241]

Interest rates did increase, but the rise was more orderly, and more contained, than the "runaway" movement that Friedman had predicted.[242] He viewed a repeat of the 1980–1981 expansion which had been marked by a zooming economy and rising interest rates. He also saw this process as poised to start within a few months of the summer 1982 upward move in monetary growth: "the prospect is that short-term rates will start to rise very soon."[243] In fact, the federal funds rate's trough, although occurring quite early in the economic expansion, was followed by a fairly modest rise at first. Friedman acknowledged in 1983 that interest rates had taken longer to pick up than he had expected.[244]

Cumulatively, the rise in the federal funds rate was quite substantial: a bit over 300 basis points in the 18 months after February 1983 (Figure 4). The real short-term interest rate (Figure 5) went back above 7 percent in July 1983 and reached 7.3 percent in August 1984—lower than many of the values in the May 1981-May 1982 period, but higher than any other postwar readings.

The fact that the rise in interest rates proceeded gradually reflected, in part, the return on the part of the FOMC to the management of interest rates. One of Friedman's reasons for thinking that interest rates would soon rise was that this was the pattern under the New Operating Procedures: monetary growth rose, higher credit demand then pushed up interest rates, and the Federal Reserve let interest rates rise rather than allow high monetary growth to continue. He had remarked (*Commodities*, January 1983, p. 51): "I'm afraid I see nothing that's going to prevent it [variability in monetary growth] from continuing." He soon made a qualification to this judgment in his January 1983 PEPAB memorandum. As already indicated, he concluded in that analysis that a material change in Federal Reserve monetary policy operation had occurred—with the FOMC's return since July to management of the federal funds rate. This, he remarked, would lead to a more "delayed adjustment" of interest rates than had been seen from 1979 to

[241] *Newsweek*, December 27, 1982.
[242] The quotation is from Friedman (1983f, p. 5).
[243] *Barrons*, October 25, 1982 (p. 7).
[244] Friedman (1983e, p. 10).

1982.[245] But in terms of its implications for monetary stability, this was little comfort to Friedman, who viewed the change as moving from one arrangement—the New Operating Procedures—that promoted volatility in monetary growth to another that was also liable to generate major swings in the money supply.

In Friedman's view, the New Operating Procedures had implied a partially automated policy response to deviations of monetary growth from target but a response that, in practice, made for rapid, but exaggerated, changes in course. In particular, he viewed the procedures as giving rise to the overshoots and undershoots of monetary growth of roughly three to six months' duration that were discussed above—with corresponding yo-yo patterns of interest rates and economic activity occurring in the wake of these monetary movements. The return to a federal funds rate targeting by the FOMC after the middle of 1982 took out some of this automaticity, as the FOMC was now more inclined to resist "market pressures affecting interest rates."[246]

It was the reaction function, not operating procedures, that Friedman held responsible for "long sustained" movements in monetary growth of twelve months in length.[247] In this connection, the move back to an interest-rate instrument had the implication that it made overshoots and undershoots in monetary growth likely to be more stretched out. Nor were these deviations necessarily going to be smaller. So Friedman judged that the feast-and-famine pattern of monetary growth seen in 1979–1982 as likely to continue, but with the periods of deficient or excessive monetary growth prevailing for longer periods. This perspective contrasted with the *Financial Times*' characterization of the declining or flat U.S. short-term interest rates seen from mid-1982 to mid-1983 as "the recent stability in monetary policy" (*Financial Times* (London), May 24, 1983). What Friedman called "the jungle of unstable Federal Reserve policy" from 1979 to 1982 (*Commodities*, January 1983, p. 50) was set to continue after the summer of 1982 in the form of wide movements in monetary growth—even if the 1979–1982 pattern of distinct three-month joint swings in monetary growth and interest rates was largely gone.

Friedman's prognosis was that the next stage in the pattern of unstable monetary growth would be a correction of the rapid monetary growth seen since the summer of 1982. "The Fed will be forced to reverse policies sharply, to slam on the brake and so what it has done repeatedly in the past three years: Go from one extreme to another." (*U.S. News and World Report*, January 31, 1983, p. 66.) With the FOMC now back to pursuing interest-rate reaction function, he

[245] Friedman (1983e, p. 10; 1983f, p. 4; 1984b, p. 30). See also Friedman (1985b, p. 54).
[246] Friedman (1983e, p. 10).
[247] Friedman (1984b, pp. 37, 39).

increasingly invoked pre-1979 experience with interest-rate based stabilization policy. The reintroduction of management of the interest rate, he suggested in the summer of 1983, had led in 1982−1983 had to the same phenomenon as that associated with the steering of rates during the 1960s and 1970s: "The result, as earlier, was surrender of control of the monetary aggregates."[248] In particular, he believed that the economic-stabilization calculations governing recent Federal Reserve policy were repeating the stop-go errors seen frequently up to 1979, in providing an excessive amount of stimulation to aggregate demand. A key concern of his was that, as already indicated, the FOMC eschewed Friedman's recommendation of bringing monetary growth down in the first half of 1983. He believed that rapid monetary growth had consequently been allowed to proceed for too long. This extended period of monetary expansion, he believed, stemmed from the fact that the summer 1982 step-up in monetary growth had not been followed by a fourth-quarter recovery in real GNP. This situation, he suggested, had encouraged policymakers to believe into early 1983 that still more stimulus was required (*Newsweek*, September 26, 1983, and January 16, 1984).

Monetary growth had been stable across the second half of 1982 and the first half of 1983. In that sense, monetary policy had been different from much of 1979 to 1982. The catch, as Friedman saw it, lay in the very high growth rates recorded over the year: "In the twelve months from July 1982 to July 1983, M1 rose at a rate of 13.5 percent per year," he noted disapprovingly in an assessment written in late summer 1983.[249] He would give this increase as consisting of 15 percent annualized growth from July 1982 to January 1983, followed by 14 percent rate from January to July 1983 (*Newsweek*, September 26, 1983).[250] The twelve-month rate had first crossed 10 percent in March 1983. This behavior contrasted with even the 1960s and 1970s. Those decades had often certainly seen rapid monetary growth. Yet the twelve-month growth of M1 had never been in double digits at any point in that 20-year period.

The fact that rapid M1 growth had lasted a year, into mid-1983, suggested to Friedman that a subsequent "sharp monetary—and therefore probably economic—contraction" was made more likely.[251] As this rapid 1983 monetary growth accumulated, he accordingly highlighted his recession scenario. Speaking to the Savings Institutions Marketing Society of America during the first quarter of 1983, Friedman remarked: "There is a very real danger that recovery might peak in 1984." (*Savings and Loan News*, April 1983.) And in an interview that he gave during

[248] Friedman (1983e, p. 10).
[249] Friedman (1984b, p. 30; see also page 56). On modern data, this rate is 13.3 percent.
[250] The rapid growth during the first half of 1983 was also even across 1983:Q1 and 1983:Q2, as already indicated.
[251] This formulation (Friedman, 1984b, p. 37) was one Friedman used in the late summer of 1983.

this period, he reaffirmed that "the danger I see is the possibility of a relapse into recession in 1984."[252]

During this spring 1983 period, however, Friedman—although highly critical of policymakers regarding monetary developments in the year to date for having been associated with a "money explosion" that "occurred because the Federal Reserve permitted it" (*Newsweek*, May 2, 1983)—was more openminded than he later became on the possibility of economic expansion continuing. With regard to monetary policy, he suggested, "the least harmful course" would consist of promptly bringing monetary growth within the FOMC's 1983 4 to 8 percent target band. This, he suggested, would imply a rise in short-term interest rates and slow down the economic recovery (*Newsweek*, May 2, 1983). Friedman contrasted an orderly course correction along these lines with what he feared would happen: a "drastic reversal" that pushed monetary growth negative or below 4 percent. The latter course would raise short-term interest rates even more in the short run and lead to a "recession by early 1984." (*Newsweek*, May 2, 1983.)

Friedman was evidently proud of encapsulating the U.S. economy's near-term prospects in these two scenarios—so proud that he reused the paragraphs in the May 2 *Newsweek* column outlining the scenarios in his June speech at the Bank of Japan and even in another *Newsweek* column in mid-summer (July 25, 1983). And there was some justification for his satisfaction with this analysis: it not only brought out an important message of his work on monetary history, including that with Schwartz—that the Federal Reserve historically had overdone moves to firm monetary policy—but also highlighted the feasibility of what would become known as a "soft landing after a period of strong economic expansion. On this theme, Friedman further remarked in an interview during this period: "I think 1983 is going to [continue to] be a year of vigorous expansion. The real question is what's going to happen in 1984." In the same interview, he articulated his alternative hard-landing and soft-landing scenarios: "Will the Fed step on the brake too hard?... Our best hope is [that] the Federal Reserve will do what they have never succeeded doing in the past: slow down without going too far, [as it moves] in the opposite direction [from the previous policy of monetary expansion]." (*Washington Times* (Washington, D.C.), May 2, 1983, pp. 6B, 8B.)

The fault in Friedman's analysis was not in stressing these two scenarios as the two most likely ones—they may well have been the right scenarios to highlight. His fault was in giving too much weight to the likelihood of a hard landing and, starting around August 1983, incorrectly

[252] *Boardroom Reports,* May 1, 1983, p. 1.

concluding that the Federal Reserve was indeed overcorrecting and that a recession would occur in early 1984. Friedman had some bad luck in the form of poor-quality initial M1 data in reaching these two errors of judgment. But they also reflected the fact that he underestimated the extent to which the interest-rate reaction function followed by the Federal Reserve from mid-1982 proved to be more stabilizing for nominal and real spending than either the pre-1979 response or the 1979–1982 monetary policy arrangements.

Friedman's fear of a scenario in which the FOMC followed a practice of "erratically allowing an explosion in [economic and monetary] growth and then cutting way back" (*U.S. News and World Report*, January 31, 1983, p. 66) rested not only on the fact of recent years' volatility but in part also on his judgment that the scenario fit the historical record of the Federal Reserve from Martin to Volcker. So, in his early-1983 column on prospects for the next year, he expressed the matter as a fear of history repeating itself: if, "as has occurred so often in the past, the Fed goes from a monetary explosion to a monetary contraction rather than to steady and moderate monetary growth, the recovery will be cut short and will be succeeded by renewed recession in 1984" (*Newsweek*, February 7, 1983). This remained his concern as the year proceeded. In late April, he restated in *Newsweek* his view that monetary overkill in a restrictive direction was "something that the Fed has often done in the past" (*Newsweek*, May 2, 1983) and, in an interview given shortly afterward, he linked up his concern with Volcker's own record by specifically portraying the scenario he envisioned as "a repeat of 1980–1981," when an economic expansion associated with high monetary growth and then cut of early by a policy tightening (*Washington Times*, May 2, 1983, p. 8B).

Over the second half of 1983, the FOMC went much in the direction that Friedman had stated in April that he wanted to see: a step-down in monetary growth into the target range. In July 1983, in the rebasing of the monetary targets discussed above, the FOMC moved from its previously announced aspiration of a range of 4 to 8 percent growth in 1983 to a 5 to 9 percent range (annualized) for the second half of 1983. It met the revised target: the M1 growth outcome for the two quarters to 1983:Q4 was 5.5 percent on the initial data and, after the 1983 monetary data were revised, the outcome was 7.2 percent, still within the range (see Hafer, 1985a, Table 2, p. 18).[253] In important respects, therefore, the Federal Reserve in the second half of 1983 delivered

[253] The meeting of the rebased 1983 M1 target occurred alongside a rising federal funds rate, as did the meeting of 1984's monetary target. The behavior of M1 and interest rates in the spring of 1983 to the end of 1984 may therefore have provided the basis for a finding that Benjamin Friedman (1997, p. 152) reported but regarded as a puzzle (pp. 159, 163): the strength of M1 growth in estimated reaction functions that included 1982–1986 data.

the monetary growth slowdown of the scale that Friedman had thought most likely to avoid recession.

As so often occurred in the Volcker years, however, Friedman was one of the least impressed with Federal Reserve performance. A complication was that the FOMC's meeting of its second-half-1983 M1 target, on quarterly-average data, was achieved in the context of what, according to the figures available at the time, seemed to be a decided squeezing of the rate of M1 growth rate in the later months of 1983. Specifically, it seemed that the annualized growth rate of M1 was about 4.5 percent in the FOMC's June-September interval and about 3.1 percent in the September-December 1983 interval, when the FOMC had hoped at the start of each interval for growth of around 7 percent, consistent with the announced M1 growth target range (Hafer, 1985a, p. 24). Other calculations at the time underscored the apparently sharp M1 deceleration: data tabulated in early 1984 gave the six-month annualized rate of increase in M1 in the six months to December 1983 as 3.7 percent (see Council of Economic Advisers, 1984, p. 291), and Friedman (*Newsweek*, January 16, 1984) gave the annualized growth rate from July to the end of the year as 2 percent.[254]

Friedman quickly moved, as the second half of 1983 progressed, to a highly critical assessment of the Federal Reserve's management of the monetary slowdown. For one thing, as the Federal Reserve was beginning in earnest in July a tightening that he had been calling for in April, Friedman thought that the policy move was being initiated too late to prevent a winding back in some of the disinflation that had occurred in recent years. Through sometime in August 1983, he had cast a monetary slowdown (irrespective of whether it took a well-managed form or instead induced a hard landing of the economy) as though it would forestall a major upsurge in inflation.[255] By late August, however, Friedman had reached the position that the slowdown had likely been left too late to stop inflation from reviving significantly in 1984: "The present rate of inflation is not likely to be maintained in view of the very sharp monetary explosion that has been occurring since the third quarter of 1982… U.S. inflation rates will rise appreciably in 1984." (*Milwaukee Sentinel* (Wisconsin), August 30, 1983.)[256] From this point on, Friedman became still more categorical in maintaining that the 1982–1983 stretch of double-digit monetary growth was bound to give rise to substantial inflation. His erroneous forecasts of a mid-1980s inflation outbreak will be discussed in detail later.

[254] Friedman may have been using incomplete December data. Full initial December data suggested that the rate was about 2.6 percent (Council of Economic Advisers, 1984, p. 291).
[255] See *Newsweek*, May 2 and July 25, 1983, and Friedman (1984b, p. 37).
[256] See also *Toronto Sun*, August 30, 1983, *Wall Street Journal*, September 1, 1983, and the discussion below.

Also in late summer 1983, Friedman came to another, and likewise erroneous, conclusion regarding the newly emerging monetary slowdown. He quickly found fault with it because he believed it had soon turned into the drastic move that he had warned about in the analyses that he had produced during the previous winter and spring. Having come to this conclusion, Friedman predicted a recession. In the *Wall Street Journal* (September 1, 1983), he declared that unfolding events had confirmed the parallel with 1980−1981. And his conclusion that a recession would occur hardened over September, as he went from offering the possibility of a "decided slowing" in the economy as a possible alternative to recession (*Wall Street Journal* (September 1, 1983) to stating in a column later in the month that the only real uncertainty lay in when recession would begin (*Newsweek*, September 26, 1983).

A string of further warnings, often quite specific in suggested timing, about an early-1984 recession emanated from Friedman during the following four months. He did hedge somewhat, as when he stated that a recession "is a very real possibility—and one you can't rule out" (*Wall Street Journal*, November 17, 1983). But his qualifying remarks were small, as he portrayed the matter as increasingly nearly settled. "If money growth continues at its present rate for another two months, we are almost sure to have a recession in the first half of 1984," Friedman told *Business Week* (December 12, 1983, p. 28).[257]

Things came to a head in what proved, to his surprise, to be Friedman's very last regular *Newsweek* column, that for the edition of January 16, 1984. The column was titled "A Recession Warning." The column still gave it as a possibility that "the economic slowdown now in process" might not "degenerate into a full-fledged recession." But the prediction was nevertheless not very conditional: Friedman wrote that the country would be "lucky indeed" to avoid such a recession—and, indeed, that recession "all too likely to be realized." Nominal GNP growth, he contended, was in a process of slowing that would likely continue in 1984:Q1 at the same time as, he believed, inflation was picking up. This characterization, combined with his more specific indication of a one-quarter lag since 1981 between monetary growth and nominal income growth, seemed to take it as locked in that real GNP growth would be negative in 1984:Q1—the main uncertainty being whether it would be succeeded by further negative readings.

Friedman was, in effect, closing his years as a columnist in a dismal fashion, in multiple ways:

[257] A few weeks later, in the closing days of 1983, he remarked similarly: "If the slow-growth policy continues for two or three months, it will make a recession beginning in the first half of 1984 almost unavoidable." (*New York Times*, December 31, 1983, p. 29.)

they were ending on the dismal note of the recession prediction; he would lose his column in an abrupt way, being removed from *Newsweek* without an opportunity to say farewell; and the prediction in the column proved to be dismal on the dimension of its accuracy.

Friedman's column plotted M1 growth and nominal GNP growth one quarter later since 1981:Q1—a sample period he called "the Reagan years." The reference to Reagan, who was not subsequently mentioned, was something of a distraction from the column's analysis, which focused on Federal Reserve, and not presidential, decisions. But the invocation of Reagan's name underlined a concern that Friedman had voiced in an earlier column (September 26, 1983) that the high inflation-plus-recession scenario he saw emerging in 1984 was "hardly an environment favorable to an incumbent." Indeed, over the fall-winter period of 1983 and well into the spring of 1984, Friedman repeatedly expressed doubt that Reagan would be reelected, owing to unfavorable economic conditions that he saw ahead.[258] "Mr. Reagan may well face a very difficult situation by next fall," Friedman had remarked at the end-of-year American Economic Association meetings. "There is a real threat of a recession in the first half of 1984." (*New York Times*, December 31, 1983, p. 29.) A member of the administration, Beryl Sprinkel, gave credence to Friedman's warnings as late as February 11 when he stated (*Chicago Tribune*, February 12, 1984, p. 1): "This slowdown in money growth subjects the real economy to the risk of an unacceptable slowdown or downturn in the first half of 1984."

After the *Newsweek* column appeared, publicity for Friedman's projection came in a front-page *Wall Street Journal* story on January 19, in which Friedman stated that "a new recession is in the cards, probably starting before the end of the current quarter" (p. 19) and in which Karl Brunner was also reported as seeing a recession starting by summer (pp. 1, 19). The Friedman projection also received coverage in an NBC nightly television news item in the broadcast of February 6, 1984.

A late and prominent appearance of Friedman's recession prediction was a long piece that appeared in *Fortune* magazine (March 19, 1984).[259] *Fortune* was part of the same publishing arm as *Time* magazine, so the extensive interview Friedman granted to *Fortune* amounted to a sign of his newfound liberation from *Time*'s principal rival, *Newsweek*. But the piece served, in effect,

[258] See, notably, Friedman (1984f, p. 30) and *New York Times*, April 3, 1984.
[259] In referencing this interview, Gordon (1984, p. 407) characterized it as a case in which Friedman was making a break with his prior practice in engaging in short-term forecasts. In fact, making real and nominal income predictions for the period ahead on the basis of monetary growth was not out of step with Friedman's practices since his recession prediction in late 1966. Nor was it inconsistent with the Friedman (1968b) analysis Gordon specifically cited in this connection, as that paper concerned both the short run and the long run.

to add to the paper trail of Friedman's forecasting errors. The article's title, "The Dire Warnings of Milton Friedman," along with the text of the article, zeroed in on his first-quarter-1984 recession prediction. The prediction was refuted definitively by the GNP data for the quarter, available starting in April 1984. But, as noted below, it was being heavily rebutted practically as the article was seeing print, with monthly indicators of January and February's economic activity suggesting anything but a recession. Indeed, in testimony in early February, Paul Volcker had mentioned the monetarists' recession concerns but also pointed to the strength of the economy in January.[260]

At the time of the interview, of course, there was more limited evidence against the recession prediction. But the interviewer, Walter Guzzardi, Jr., was highly skeptical about the forecast and seemed exasperated by the "unequivocal" confidence that Friedman expressed in it, remarking: "Shot and shell cannot shake Friedman." The closing quotation from Friedman in the article was a giddy contemplation of a repeat of his success in forecasting growth in 1983: "Now, again, you have a unanimity of conventional wisdom. Personally, I believe the consensus forecast now will prove as wide of the mark for 1984 as it was for 1983—but in the opposite direction." (*Fortune*, March 19, 1984, p. 34.)[261]

This remark was revealing about Friedman's state of mind at the time. Although discouraged by some notable recent setbacks—such as the lack of success of *Monetary Trends* in the economics profession, the FOMC's move away from monetary targets, and his poor 1983 inflation forecast—he entered 1984 seeing vindication ahead, through the expected success of his bold projections with regard to what the year held in store for inflation and real GNP, and through the high attention he anticipated would be given to the book and television versions of *Tyranny of the Status Quo*. Friedman's hubristic state during this period was exemplified by the fact that, in late 1983, he and Rose Friedman commissioned a sculptor to cast a bronze bust of Milton Friedman (*Santa Cruz Sentinel* (Santa Cruz, California), December 29, 1983).

As this interview appeared in print, incoming data were suggesting even more strongly that Friedman's prediction of negative growth would not be realized for that quarter. These patterns prompted Paul McCracken to observe: "It just goes to show that even a brilliant man like Milton Friedman can be wrong." (*Baltimore Sun*, March 9, 1984, p. D9.)

[260] See Volcker's testimony of February 8, 1984, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1984a, pp. 121, 128).
[261] This statement was, other than a change in sign, not very different from a Friedman remark a year earlier (*Newsweek*, February 7, 1983) that forecasters would be confounded by the strength of the 1983 rebound just as they had been surprised by the extent of the weakness of the 1982 economy.

Indeed, real growth continued throughout 1984 and, in particular, was very strong in 1984:Q1, continuing the rapid growth seen over 1983. On modern data, the annualized rates of growth were 12.5 percent for nominal GDP (up from 11.9 percent in 1983:Q4) and 8.1 percent in the case of real GDP growth (compared with 8.6 percent in 1983:Q4). "I have no easy explanation of what went wrong," Friedman said at an Oppenheimer and Company briefing he gave in New York City.[262] In that briefing, Friedman stated that the numbers were released, acknowledging that the 12.8 percent nominal GNP growth was more than double what he had been predicting.[263]

On other occasions, Friedman was willing to elaborate on specifically why his forecast went astray. These statements, together with the evolution of the U.S. economy over the rest of 1984, underlined the fact that Friedman was not as misguided in his recession forecast as he had been in his inflation predictions. There were mitigating factors that made his recession forecast more reasonable and that did not apply to his high-inflation forecasts of the same period.

The first mitigating factor was that his forecast was conditional on a period of very low monetary growth. Beryl Sprinkel had taken Friedman's concerns about the 1983 monetary slowdown seriously and, as indicated above, he shared them to some extent. In December 1983, when M1 data through November were available, Sprinkel also observed of the recession scenario (*Wall Street Journal*, December 14, 1983): "That is a possibility. I don't believe it will happen. Milton is making a prediction that, in the months ahead, we won't get money supply growth. I certainly hope [that] we get some growth in the money supply in the months ahead." The higher monetary growth for which Sprinkel hoped occurred.[264] Friedman himself remarked: "Since I [originally] made those predictions, the Fed has taken its foot off the brake... I no longer predict a recession in the near future." (*USA Today*, April 11, 1984.) More specifically, he remarked: "I don't think we will have a recession in 1984."[265]

Furthermore, thanks to data revisions, the more-rapid rate of monetary growth judged by Friedman and Sprinkel as what would be more consistent with the avoidance of recession occurred not only in the "months ahead" (that is, after November 1983) but retrospectively (in the July-October 1983 M1 data). In what one commentator called the "big 1983 revisions" (*The*

[262] *New York Times*, April 30, 1984. See also his July 1984 remarks in Greider (1987, p. 543).
[263] In a talk in the closing days of June (Heller and others, 1984, p. 45), Friedman specified the nominal GNP growth prediction that would have been implied for 1984:Q1 by its recent relationship with prior M1 growth, using revised data on M1, was 5 to 6 percent.
[264] Friedman later noted that monetary growth turned up in November 1983 (*Wall Street Week*, Maryland Public Television, April 29, 1984, p. 6 of transcript). On modern quarterly data, M1 growth is much the same in 1984:Q1 as in 1983:Q4. It is, however, close to the 7 percent annualized rate that Friedman regarded as appropriate.
[265] *Wall Street Week*, Maryland Public Television, April 27, 1984, p. 6 of transcript.

Australian, March 8, 1984), data on M1 and M2 were retrospectively marked up very substantially. Paul Volcker himself would testify in early 1984 that "we have [had] some fairly significant restrictions [sic; revisions] in the seasonal patterns this year that change the pattern during the year and that we didn't know about until a couple of weeks ago."[266] He added: "I tell you, when I saw the size of those revisions, I began wondering... I was a little surprised we had changes of this magnitude."[267]

Volcker maintained nevertheless that the revisions were "not big enough to be significant in appraising policy."[268] On the same day on which Volcker made these remarks, Anna Schwartz expressed the same sentiment. In particular, she held that the picture of a distinct slowdown in monetary growth over the second half of 1983 remained in the revised series (*Wall Street Journal*, February 8, 1984). It turned out, however, that it was not really accurate to suggest that the arrival of the new numbers had not importantly altered the picture of second-half-1983 monetary developments. Notably, as already indicated, the second-half 1983 annualized growth rate of M1 was raised to about 7 percent, when a figure of 4 percent or lower was what was suggested by the previous data vintages. Subsequent revisions have raised the rate a bit further. In the spring of 1983, Friedman had cited a 6 to 7 percent M1 growth as the appropriate rate to which the Federal Reserve should achieve in the second half of 1983 and as sufficiently high to avert recession.[269] That was not too far from, and certainly not below, the rate that was actually delivered: modern quarterly-average data show annualized M1 growth of about 8.1 percent in the second half of 1983, on average, while, on monthly data, the annualized six-month increase, seasonally adjusted, from June to December is 7.0 percent. What Friedman had initially described as "the most extreme turnaround in monetary policy since the end of World War II" (*New York Times*, December 31, 1983, p. 29) was much less extreme once the second-half-1983 M1 data had been revised.

The revisions to the data on the growth rate of M1 (quarterly average) in the fourth quarter of 1983 were particularly large—and also of special note in view of the fact that Friedman's

[266] Testimony of February 7, 1984, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1984b, p. 76). The transcription error quoted was in the printed volume.
[267] In Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1984b, p. 77).
[268] In Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1984b, p. 77).
[269] In particular, he observed (*Savings and Loan News*, April 1983): "If a miracle happens, the Fed will let money supply growth taper off to 6 percent or 7 percent ..." Clearly referring to Friedman, Jerry Jordan observed in March 1984 (Jordan, 1984, p. 4): "Oddly enough, we also had a number of economists who should know better worrying that the Fed would put us back into recession. Admittedly, the 7.3 percent growth of M1 in the second half of 1983 was slower than the explosive growth of the first half, but since when is 7-plus percent money growth too slow?" What his critique neglected was the fact that the 7-plus percent rate was the revised monetary growth number—and not the much-lower rate on which Friedman conditioned his recession warnings.

recession prediction rested heavily on the behavior of monetary growth during that quarter. The annualized rate of growth in the quarterly average of M1 in 1983:Q4 went from being 2.1 percent on early-1984 vintage data (Council of Economic Advisers, 1984, p. 291) to 4.8 percent on mid-1984 revised data (Simpson, 1984, p. 253) and further large upward revisions would put it at 7.1 percent on the modern data vintage.

The second mitigating factor concerning Friedman's recession warnings lay in the fact that, although he was proven wrong in pointing to 1984:Q1 as a period of likely contraction, Friedman was on the right track when he continued to maintain that "there is not enough monetary fuel going into the economic engine" (*New York Times*, April 30, 1984) and that this had to show up before long in slower nominal income growth and also that "real economic growth will come down fairly dramatically in the next few quarters" after 1984:Q1 (*Wall Street Week*, Maryland Public Television, April 27, 1984, p. 6 of transcript)

As already indicated, the FOMC had, indeed, withdrawn much monetary stimulus in the second half of 1983, and it continued to do so in 1984. The move to restriction was evident not only in M1 data but in monetary base growth and in rising real interest rates. What happened, in line with Friedman's expectation, was a substantial slowdown in real and nominal income growth during 1984. Contrary to his expectation, it commenced in 1984:Q2 rather than 1984:Q2.[270] The one-quarter lag between monetary growth and nominal income growth that he cited in his 1983 analyses proved largely, but certainly not entirely, to be an artefact of the initial money and income data, with later revisions to the data for the 1981–1984 period putting back the more normal two-quarter lag: see the previous chapter.[271]

Beyond this, however, in the revised data patterns, the economic slowdown in 1984 occurred with a longer than average lag in Friedman's estimation: a sustained monetary growth slowdown beginning (though, initially, very mildly) after the fourth quarter of 1982 did not have a counterpart in nominal income growth behavior until mid-1984 (*Wall Street Journal*, August 20,

[270] Friedman had earlier seen the slowing in real GDP growth as already in process during 1983, referring to the "recent slowdown in economic growth" (*Newsweek*, January 16, 1984). This slowdown was essentially eliminated by data revisions, and the 1984:Q1 strength now is comparable to the real growth seen in the second through fourth quarters of 1983.

[271] The one-quarter lag between monetary growth and nominal income growth in early vintages of the data for the first half of the 1980s was present in the case of both the monetary base and M1 (see Friedman, 1984c, p. 399; the statement of March 12, 1984, of the Shadow Open Market Committee, 1984a, p. 6; and Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives 1985, p. 9).

1985).[272] Furthermore, this slowdown, while pronounced for both real and nominal income growth, was not associated with a recession. Rather, the United States did have what Friedman conceded was possible but doubted would happen: a "soft landing," instead of an overreaction to the unsustainably high real growth of 1984:Q1.[273]

On the basis of the prior experience that Friedman had studied, large withdrawals of monetary stimulus implied a recession; but the early years of the United States' 1982–1990 economic expansion produced an example of an orderly transition of the economy from particularly strong real economic growth in 1983 and early 1984 toward lower but still highly robust growth (with four-quarter real GDP growth rate on modern data moving from 8 percent in 1984:Q2 to 3.7 percent in 1985:Q2), with the lower growth in part reflecting the shift to a tighter monetary policy stance.[274]

In fact, the early-to-mid 1984 period was a historical watershed. Friedman had described the four years to mid-1983 as ones in which the country had racked up "four years of unusual instability."[275] This was true even if the monetary-growth numbers were put to one side—though in 1983 some of the "instability" had taken the favorable form of a strong recovery. But after 1984:Q1's large economic advance, a change in economic behavior took place. The period after that quarter is now regarded as the pivotal point at which the U.S. economy transitioned into the "Great Moderation," with the country experiencing much lower-tempo business-cycle fluctuations for nearly twenty-five years until 2008 (see, for example, Bernanke, 2004a; Blanchard and Simon, 2001; McConnell and Perez-Quiros, 2000; and Stock and Watson, 2002).

This change left a marked impression on U.S. economic data even through the end of 1986. The standard deviation of annualized quarterly nominal GDP growth was 5.7 percent in the 1979:Q4–1984:Q1 period but only 2.1 percent in 1984:Q2–1986:Q4. With regard to annualized quarterly real GDP growth, the standard deviation fell from 5.5 percent in 1979:Q4–1984:Q1 to 1.6 percent in 1984:Q2–1986:Q4.

These major declines occurred alongside continuing high variability in M1 growth: annualized

[272] A lag of about five quarters from monetary tightening to the 1984 economic slowdown also emerges if indices of monetary policy other than money, such as nominal interest rates, are used.
[273] James Tobin indeed characterized FOMC performance in the mid-1980s as a protracted process of soft landing, one intended to terminate at the natural rate of unemployment (*The Economist* (London), April 27, 1985*b*, p. 26).
[274] In addition, some of the slowdown in nominal spending was recorded in a fall in the annual inflation rate of about one percentage point from 1984's rate, rather than slower real output growth.
[275] Friedman (1984b, p. 30).

growth in the quarterly average of M1 was similar across the two periods, at 4.9 percent and 4.7 percent. Friedman had observed in the *Journal of Money, Credit and Banking* in August 1983: "Many economists have criticized the Federal Reserve because of the high variability of monetary growth…"[276] The variability continued over the next three-and-a-half years, but criticism of it dwindled.

Monetary volatility received less attention in the years after 1983 because the swings in money started to become less associated with variations in income. Although the M1 velocity trend broke from its previous pattern at the start of 1982, the relationship between M1 growth and nominal and real income growth remained statistically significant, as well as visually apparent, until about late 1984. After that, however, this bivariate relationship essentially disappeared.[277] Friedman, although aware of the velocity break, did not grasp the post-1984 deterioration in the monetary-growth/income-growth relationship until mid-1986.

And by then, further major blemishes, beyond his erroneous recession prediction, had appeared on Friedman's record both as an analyst of monetary policy and as a forecaster. Most prominent of these was on the matter of inflation.

Another bad prediction: The 1984–1985 revival of inflation that didn't happen

As has already been stressed, one of Friedman's erroneous beliefs in the immediate aftermath of the 1982 Federal Reserve policy changes was that it was a straightforward reversion to pre-1979 monetary policy practices. He would incorrectly contend that the Federal Reserve was going back to 1970s inflationary patterns, in which it provided the economy with excessive stimulus late in the recession and early in the recovery period and then found itself with a new inflationary problem. From this belief, together with Friedman's emphasis at the time on the M1 aggregate rather than M2, would stem the spectacularly wrong predictions about inflation that he would make from 1982 to 1984—especially with regard to the rate of price increase in 1984 and 1985.

Certainly, in 1981 and 1982, Friedman's record on inflation forecasting, although not matching his prescience concerning the 1979–1980 double-digit rates, continued to be respectable. In mid-October 1981, President Reagan told the media that inflation was "in single digits now, and I was interested to see that our Nobel economics prize winner, Milton Friedman, has just been

[276] Friedman (1983h, p. 339).
[277] This matter is analyzed further in the later discussion titled "Benjamin Friedman."

quoted as saying that he believes it'll be down to 6 percent next year."[278] This Friedman forecast of declining inflation, which at this time was based primarily on the lower M2 growth recorded after 1977, was borne out in U.S. inflation outcomes in 1982. That said, the 6 percent projection that Reagan cited was on the low end of the 6 to 9 percent inflation range Friedman was projecting in late 1981 for inflation in 1982 (Oppenheimer and Company, 1981, p. 5).

From late 1981, as discussed in the previous chapter, Friedman went to a position of very heavy reliance on M1 growth in his analysis.[279] This eventually put him in the position of greatly overstating the amount of inflation ahead. Initially, however, it steered him toward being correctly predicting a still more disinflationary year in 1982 than he had previously foreshadowed. M1 growth had been low in 1981, and Friedman was also becoming convinced that the lag between monetary growth and inflation of late had only been about one year.[280] Consequently, in February 1982, Friedman forecast 5 to 6 percent CPI inflation by the end of the year.[281]

Some sign that Friedman was going off track in predicting inflation materialized in an August 1982 *Newsweek* column, in which Friedman restated the 6 to 9 percent number in reference to basic inflation prospects (*Newsweek*, August 23, 1982).[282] In the month in which he was writing, twelve-month CPI inflation actually fell below 6 percent. It went on to stay below that rate until 1990. Friedman was wary of the CPI inflation rate, noting that the way in which mortgage rates were included (until 1983) meant that it would exaggerate the decline in inflation in light of the fall in longer-term market interest rates that was in evidence since late 1981. Nevertheless, his estimated range of 6 to 9 percent for the underlying rate of inflation was too high by late 1982. In the fall and winter 1982 period, however, Friedman was oblivious to the extent to which actual U.S. inflation had moved into a lower range on a permanent basis. The most he would grant was that there was "a good chance" that the next cyclical peak in inflation would likely be

[278] Reagan (1981a).

[279] In *Wall Street Journal*, June 28, 1982, he did quote recent growth rates of M1 and M2 but was doing so in light of the item on which he was commenting. This item had referred to M2 growth.

[280] He said so in his PEPAB memo of January 1983, for example (Friedman, 1983f, p. 3), and in Friedman (1983e, p. 9).

[281] In *American Attitudes*, BBC1, February 16, 1982, p. 3 of transcript, also reported in *Business Times* (Singapore), February 19, 1982, p. 5. The top of this range was also the forecast that Alan Greenspan, at that time in the private sector, made: "our latest forecast is for a consumer price increase between December '81 and December '82 of 6 percent. That's really an extraordinary figure… I think that this is not… a short-term aberration. Something fundamental is happening." (*Wall Street Week*, Maryland Public Television, January 29, 1982, pp. 14–15 of transcript.)

[282] This prediction was out of kilter with the fact that M1 growth had recently reached a multi-year low of 5.0 percent in the four quarters to 1982:Q2. In his analysis, Friedman may have been incorporating an expectation of some pickup in monetary growth, as well as an assumption of a rise in M1 velocity during the expansion period.

lower than the 1980–1981 peak (*Commodities*, January 1983, p. 49).[283] This was certainly a significant acknowledgment, but it turned out to be a serious underestimation of the scale of the change in inflation's behavior.

Friedman's newfound belief in a one-year lag from monetary growth to inflation reinforced his view that prospective inflation would be high, as he expected that the more rapid rate of monetary growth seen since July 1982 would show up in 1983's inflation. Friedman's late October 1982 prediction for 1983 had a midpoint of 7.5 percent (*Barron's,* October 25, 1982, p. 7). He affirmed a couple of months later: "There's no sign in the monetary figures of a continued reduction in monetary growth, on average. The inflation rate will be somewhere in the neighborhood of 6 to 9 percent for the next two years." (*Commodities*, January 1983, p. 50).

Indeed, during this period, both in private (in a meeting with Alan Walters in London) and publicly, Friedman, partly on the basis of the rapid growth in monetary growth likely to be recorded for 1982, suggested that U.S. inflation in 1983 would be about 8 percent.[284] "The inflation rate is now running at 6 or 7 percent, Friedman remarked, "[and] my guess is that it will run at about 8 percent for the next couple of years. That's what is built into the monetary system." (*The News and Courier* (Charleston, South Carolina), October 26, 1982, p. 13.) More than a year after the forecasts were made, the full data for 1983 underlined the magnitude of this forecast error. For 1983:Q4, the four-quarter CPI inflation rate was actually 3.2 percent. For the GDP deflator, it is now indicated as having been 3.3 percent.

In early 1983, Friedman had actually shown signs of making some allowance for the possibility that inflation would stay in a considerably lower range. "Inflation may decline somewhat more but is likely at best to fluctuate about its present level for the next year or two," he remarked in his January 1983 PEPAB memorandum, when twelve-month CPI inflation was in the 4 percent area. He added, however: "If anything, it is more likely to rise than to decline."[285] Importantly, at this stage Friedman was leaving open the possibility that the recent decline in inflation could mostly be sustained, provided that the Federal Reserve took prompt efforts "to halt the monetary explosion"—that is, the post-July 1982 rapid increase in M1.[286] In March 1983, he seemed

[283] See also Friedman and Friedman (1984, p. 82; 1985, p. 83).
[284] On the meeting in London, see Walters (1982). During the same visit to Europe, Friedman was quoted saying, "I do not expect a further fall in the U.S. inflation rate in coming years… I think its low point has [already] been reached." (*Business Times* (Singapore), September 18, 1982.) Friedman's public forecast of 8 percent inflation for 1983 was also noted in *Fortune*, March 19, 1984, p. 34.
[285] Friedman (1983f, p. 7).
[286] Friedman (1983f, p. 5).

optimistic on this score, stating that he believed that monetary growth would promptly be moderated and that there would only be a mild rise in inflation in the future.[287] But, as already indicated, the continuation of rapid monetary growth through mid-1983 put a new complexion on matters for Friedman. Monetary growth in 1983, in total, was actually higher than in 1982. He believed that the point of no return had likely been passed and that a new, and very likely major, upsurge in inflation was in the offing.[288]

This judgment underlay the soon-to-be-notorious forecasts of double-digit, or near double-digit, inflation rates that Friedman made during 1983 and 1984.[289]

In late August 1983, he said, "U.S. inflation rates will rise appreciably in 1984, although it's not yet determined where they'll go from there." (*Milwaukee Sentinel* (Wisconsin), *Toronto Sun*, August 30, 1983).[290] Shortly afterwards, he published the article, to which extensive reference was already made above, titled "Why a Surge in Inflation Is Likely Next Year" (*Wall Street Journal*, September 1, 1983). This article stated: "excessive monetary growth over the past year… will almost certainly mean a subsequent acceleration of inflation, probably in middle or late 1984." And in a paper drawn from his remarks at the December 1983 American Economic Association meetings, Friedman wrote, "The increased rate of monetary growth in the 1981–83 biennium suggests that we have passed the trough in inflation and that inflation will be decidedly higher from 1983 to 1985 than it was from 1981 to 1983."[291]

In his interview for *CBS Morning News* taped in mid-February 1984, Friedman said, "I think that by the end of 1984, inflation is likely to be up in the range of 6 to 9 percent. If the [economic] expansion continues, then inflation will be at the higher end of that." (*CBS Morning News*,

[287] *Saturday Briefing*, BBC2, March 12, 1983, p. 11 of transcript.
[288] In House Republican Research Committee (1984, p. 36), Friedman confirmed that the third quarter of 1983 was when he became convinced that the contour of inflation from now on was upward.
[289] Other catalogues of the Milton Friedman warnings about higher inflation have included Rayack (1987, pp. 194–195), D. Smith (1987, p. 137), Benjamin Friedman (1988a, p. 61; 1988b, p. 441), and Nelson (2007, pp. 162–165). The present account includes numerous Friedman statements not in the previous studies and is designed to provide a more systematic picture of his rationale for the forecasts and the subsequent revisions in his thinking.
[290] Also quoted in *Toronto Sun*, August 30, 1983.
[291] Friedman (1984c, p. 400). In a discussion written between these commentaries—in October 1983—Friedman and Friedman (1984, p. 92) were more equivocal, essentially restating Friedman's spring-summer analysis to the effect that a post-June 1983 monetary slowdown could forestall a subsequent reemergence of double-digit inflation, but only at the expense of "renewed recession… in 1984." They immediately added, however, that "we shall have to suffer at least one more upswing of inflation" and that "there is no way to avoid a temporarily higher rate of inflation as a result of the delayed impact of the recent monetary explosion"—with the contribution that a prompt monetary slowdown could make being "to keep inflation from again reaching double-digit levels." (Friedman and Friedman, 1984, p. 93.)

March 1, 1984, p. 23 of transcript.)

At the beginning of April 1984, in a *New York Times* op-ed titled “Inflation Isn’t Beaten,” Friedman wrote, “It looks more and more as if inflation will once again be the No. 1 economic problem… We shall be fortunate indeed if prices are not rising in the 7 to 10 percent range by the fourth quarter of the year and in double digits by 1985.” The following week, in an interview with *USA Today* (April 11, 1984), Friedman predicted a “substantial increase in inflation by the end of this year,” with consumer prices rising in the 8 to 9 percent range.

A string of appearances in Chicago and the East Coast late in the month led Friedman to restate and underline these projections. In Chicago for an appearance on *Donahue* broadcast in that city on April 25, 1984, Friedman said, “Unfortunately, we are heading for a situation where there is a very good chance [that] we will be back in double-digit inflation in 1985 which will mean a very severe recession in late ’85 and early ’86.” When the studio audience reacted unfavorably to his forecast, Friedman, “I don’t like that [projection]. I’m not saying what I like; don’t misunderstand me. I’m just trying to say what I think will happen.” Friedman made an appearance at Oppenheimer and Company in New York City the next day, on April 26, 1984, and his predictions were summarized as “8 to 9 percent inflation on an annualized month-to-month basis appears to be the most likely outcome” for late 1984, and also: “By 1985, inflation will most likely be back into double-digit levels.”[292] Likewise, in Maryland during the same week, when appearing on *Wall Street Week*, Friedman stated, “I believe we are going to be up in the 7 to 10 percent range by the end of 1984” in terms of annualized quarterly rates, while with respect to twelve-month rates, “I think by 1985 we may very well be back in double-digit inflation. I hope I am wrong.”[293]

Furthermore, Friedman purported to see the predicted surge of inflation actually starting to be manifested in recent data. In his April 3, 1984, *New York Times* piece, he cited the 5.5 percent average annualized rate of price increase in the first two months of 1984.[294] Similarly, in his

[292] Oppenheimer and Company (1984, pp. 5, 8). See also the report of these Friedman’s remarks in *Washington Post*, April 27, 1984. As discussed presently, Friedman made only a modestly different projection in midyear, remarking: “I believe [the CPI] will be rising in the neighborhood of 8 to 10 percent in 1985.” (In Heller and others, 1984, p. 46.) Likewise, in a seminar (interview) held with the House Republican Research Committee in Washington, D.C., on July 11, 1984, Friedman said that he saw inflation “going up to something like 10 percent in 1985.” (House Republican Research Committee, 1984, p. 37. The interview was not dated in the publication itself. The date and location of the interview were provided by Gloria Valentine in a personal communication to the author on May 8, 2014.)

[293] April 27, 1984, pp. 7, 8 of transcript.

[294] See also his remarks in *The MacNeil/Lehrer News Hour*, PBS, March 27, 1984, p. 4 of transcript.

April 27 *Wall Street Week* appearance, he suggested that the first quarter's price data did not suggest "a pattern of inflation coming down."[295] Even in the first half of July, Friedman contended with regard to how his prediction had been going: "Not badly, really"—the basis for his contention being the step-up in annualized inflation rates from first-half 1983, to second-half 1983, to the first five months of 1984 by cumulatively over 1.5 percentage points.[296]

In fact, the rise in inflation was brief. Friedman was predicting a longer-term revival that never occurred. The twelve-month CPI inflation rate reached 4.9 percent in March 1984, reflecting in part a sizable price increase in January 1984. The rate promptly started falling again.

As this renewed improvement in inflation was developing, Friedman still expected monetary growth to be manifested in a surge in inflation in 1985 to near double-digit levels, possibly with the U.S. dollar declining at the same time (*Wall Street Journal*, June 13, 1984). Speaking to his colleagues at the July 1984 FOMC meeting, Paul Volcker noted some wavering by Friedman on the issue.[297]

But Volcker overstated matters by portraying Friedman as now having abandoned his high-inflation projection. Instead, the main qualification that Friedman made in midyear—in what turned out to be the first of a sequence of markdowns of this kind that would continue over the next two years—was that he was prepared to regard more of the previous decline in velocity as permanent.[298] This altered the amount of excess monetary growth that was poised to be manifested in inflation. He still saw a 10 percent, or near-10-percent, peak of inflation as being likely.[299] Friedman now assessed there to be a less than 50–50 chance of the 10-percent threshold being crossed in 1985. But he viewed 1985's inflation as poised to be higher than the rate prevailing in 1984:Q4, which he now expected to be 6 to 8 percent annualized, instead of 7 to 10 percent.[300]

[295] *Wall Street Week*, Maryland Public Television, April 27, 1984, p. 7 of transcript.
[296] House Republican Research Committee (1984, p. 36).
[297] On the first day of the July 16-17, 1984, FOMC meeting, Volcker stated: "We can all be encouraged to know that Milton Friedman has decided … [that] all bets are off in the future for inflationary implications of that rapid growth." (Federal Open Market Committee, 1984a, p. 13 of transcript.)
[298] However, one of the specific grounds that Friedman cited as a major reason for a permanent decline in M1 velocity—the introduction of Super NOW accounts (see, for example, House Republican Research Committee, 1984, pp. 36–37)—was a highly questionable one. As discussed below, Super NOW accounts' introduction likely mainly led to transfers within M1.
[299] Heller and others (1984, p. 46).
[300] House Republican Research Committee (1984, p. 36).

Other forecasts at the time

Although Karl Brunner (1988a, p. 49) would later state that, "of course," a key factor in undermining the public's perception of monetarism in recent times had been "Milton Friedman's erroneous forecast of incipient inflations several years ago," other prominent monetarists were also alarmist about inflation in the commentaries that they made during the 1983–1984 period. Indeed, Benjamin Friedman (1990, p. 165) correctly observed that the *Wall Street Journal* op-ed page over 1983 featured a "stream of predictions that rapid money growth was going to generate an acceleration of inflation." Friedman, of course, was one such op-ed contributor, in his September 1 piece. But well before him, Brunner and Allan Meltzer contributed an op-ed to the newspaper warning of the dangers of a resurgence of inflation (*Wall Street Journal*, February 7, 1983). A couple of months later, former Federal Reserve Bank of St. Louis president Lawrence K. Roos had a *Journal* op-ed (April 7, 1983) stating that the FOMC's deemphasis of the M1 target "represented a return to the kind of intuitive fine-tuning that produced the double-digit inflation of the 1970s."

The inflation warnings by monetarists appeared in many outlets as well during 1983 and 1984 and, of course, many of them were by monetarists other than Friedman. For example, in mid-1983 Meltzer was quoted as saying that the period of high monetary growth had proceeded long enough that the likeliest outcome for the U.S. economy in the year ahead was stagflation (*New York Times*, July 25, 1983, p. D1). The Brunner-Meltzer-organized Shadow Open Market Committee meetings also issued a number of public statements over this period warning that inflation was brewing and, in retrospect, Meltzer (1995, p. xxvii) acknowledged that he and his SOMC colleagues "were wrong to predict inflation in 1984."[301] At the time, monetarist-leaning financial journalist Maxwell Newton gave publicity in one of his columns to the SOMC's "grim forecast of accelerating inflation in 1984 and 1985" (*Boston Herald*, March 16, 1984), and Newton himself asserted in another column (*The Australian*, March 2, 1984): "There are many signs of returning inflation."

[301] In 1987, Meltzer testified that "we long ago at the Shadow Open Market Committee gave up on the aggregates for M1 because of the various shifts that have occurred. We have shifted to the monetary base growth rate…" (Testimony of February 18, 1987, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1987, p. 56.) This move did not prevent the SOMC from making predictions in 1983 and 1984 like those Friedman made, for two reasons. First, the SOMC still used M1 heavily. Although Meltzer testified in early 1982 that he would like to see the monetary targets expressed only in terms of the monetary base (see his testimony of February 24, 1982, testimony in Committee of Finance, U.S. Senate, 1982, p. 212), SOMC commentaries in 1983 and 1984 stressed both the base and M1 when discussing economic prospects. Second, as discussed in the next chapter, the monetary base itself underwent a major shift in velocity around 1982, the same time at which M1 did.

Among nonmonetarist economists, too, there were numerous predictions made of a very significant rise in inflation by late 1984 or slightly later. A prominent but unrepresentative example of the proponents of such forecasts was John Kenneth Galbraith, who in mid-1984 was reported as predicting that a collapse in the U.S. dollar would lead to double-digit inflation in a year's time (*Wall Street Journal*, June 13, 1984). Various more traditional economic forecasters were making less dramatic projections that were in the same basic direction. *The Economist*, for example, noted (February 18, 1984): "Even before the dollar started slipping, some forecasts were expecting inflation to be running at 5 to 6 percent by the end of this year." Noted business economist Michael Evans indicated in March 1984 that he expected inflation to be in the 6 to 7 percent range in the second half of the year (*Town Talk* (Alexandria-Pineville, Louisiana), March 28, 1984). In the *American Banker* newspaper, financial columnist Joseph Slevin had reported on June 20, 1983, that many U.S. government economists predicted a significant pickup in inflation would occur in late 1984. In the same newspaper in mid-1984, an investment bank economist was reported seeing inflation in the 6 percent zone in 1985 (*American Banker*, May 24, 1984).

There was also a considerable body of opinion holding that inflation would *not* resurge. Confronted in early 1984 with forecasts, derived from M1 growth, of considerable inflation in the next couple of years, Stanley Fischer declared them "unrealistic."[302] Fischer was much more inclined to be optimistic about inflation prospects, on the basis of the continued amount of the slack in the economy.[303] This echoed Rudiger Dornbusch's position in 1978. On that occasion, Dornbusch had been wrong, and the monetarists correct. But U.S. output-gap estimates had undergone a shakeout since then, and those forecasting inflation using the output gap were on fairly solid ground in 1983 and 1984. This matter will be discussed further in the discussion titled "Otto Eckstein" at the end of this chapter.

Among professional forecasters, too, there was pronounced division. In contrast to Friedman's usually valid generalization that forecasters tended to exhibit herd behavior, this community exhibited a spike during the 1983–1984 period in dispersion in projections of CPI inflation: some, like Friedman, seeing a major revival ahead, and others expecting 1982's lower rate to be maintained or improved on (see Sheen and Wang, 2016).

[302] See the floor discussion summary in Gordon (1986a, p. 392).

[303] Likewise, James Tobin, after having expressed doubt in the 1970s about the scope for slack to make major inroads in inflation, argued (Tobin, 1984, p. 101) that the fact that "recovery has not reversed, or even arrested, gradual progress on inflation to date" should have been "expected given the slack in utilization of labor and capacity still remaining," and he went on to criticize "those" (he did not mention Friedman by name) who had predicted inflation on the basis of the 1982–1983 double-digit monetary growth.

Policy circles at this time were also characterized by less pessimism about inflation than what was implied by the warnings of monetarists. Nevertheless, an upsurge of some significance in inflation was anticipated in the official Federal Reserve Board staff forecasts: their projection saw core CPI inflation in 1985 of nearly 6 percent. This turned out to be an overprediction of about 1.5 percentage points—the largest that the staff ever made in the years from 1984 to 2000 (see Kohn, 2005). Among Federal Reserve policymakers, as opposed to staff forecasters, the projections of inflation for 1984 and 1985 were generally much more optimistic and, as it turned out, this optimism was soundly based. In the summer of 1983, conscious of the monetarist warnings that were intensifying in that period, Paul Volcker testified that not everybody agreed that inflation prospects were actually all that good.[304] But he cautiously disagreed with these assessments. And in their own forecasts, submitted in early 1984, FOMC policymakers' 1984 GNP deflator inflation projections had a central tendency of 4.5 percent to 5 percent (Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1984b, p. 34).

The fact that policymakers were, in effect, parting company with monetarists on this matter did not go unnoticed. In materials prepared for the March 1984 SOMC meeting, Jerry Jordan (1984, p. 42) complained that "the FOMC's inflation projections appear much too low following a two-year growth of M1 of 9.3 percent and monetary base growth of 8.5 percent." But when he was challenged, in a question that specifically mentioned Milton Friedman's pessimism regarding inflation, about his own benign outlook, Paul Volcker was firm: "First of all, let me say that I think there is a relationship between money growth and inflation. I don't think it is quite as mechanically tight as some of the analysts suggest, either in lagging or in amount. The question has been raised as to whether that bulge in the money supply will necessarily give rise to a great acceleration of inflation in 1984 or 1985; I do not believe so." [305] He stuck to his contrary interpretation of 1982–1983's velocity decline: "I think we had some extraordinary things going on in that period, institutional and economic, [producing] a one-time adjustment to a higher desired level of money holdings." Consequently, Volcker suggested, "I don't believe it follows that we are going to get a great inflationary burst two years later... The proof will be a few years from now."[306]

This was a daring statement for Volcker to put on the record. But the subsequent few years'

[304] Testimony of August 3, 1983, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1983b, p. 248).
[305] Testimony of February 7, 1984, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1984b, pp. 78–79).
[306] Testimony of February 7, 1984, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1984b, p. 79).

events must have left him pleased that he had done so. The inflation data in 1984, 1985, and 1986 certainly refuted the notion of an inflationary outbreak. In particular, after early 1984, there was a decided improvement, rather than deterioration, in inflation. The December-to-December CPI inflation rates would be 3.9 percent in 1984, 3.8 percent in 1985, and 1.1 percent in 1986 (see Council of Economic Advisers, 2011, Table B–63, and Figure 2 above).

1985 and 1986

As further readings on low inflation were recorded in 1985 and 1986, monetarists became more isolated. The beginning of 1985, it is true, was still seeing some analysts predicting a revival of inflation on the strength of 1982–1983's high rates of monetary growth. For example, Bear Stearns' economist Robert Sinche cited the historical link between changes in M1 and prices to predict that U.S. inflation would peak at 7 percent at the end of 1985 (*Fortune*, January 7, 1985). And Robert Barro, who had in early 1984 projected that year's inflation rate to be 6.6 percent on the basis of M1 growth (Barro, 1986, p. 386), remained worried in early 1985 about a revival of inflation (see Barro, 1985b, p. 685). In other quarters, however, continued low inflation readings since 1982 had created a mood of skepticism about what monetarists had to say on the matter, with one commentator remarking that "prominent monetarists like Milton Friedman and Karl Brunner have been warning every few weeks for more than two years about a new burst of high inflation" (*Boston Globe*, June 27, 1985).

The dichotomy between high monetary growth readings and low inflation rates received a fresh fillip with the return of very high M1 growth in 1985. Allan Meltzer, for one, remained convinced that an inflation revival was ahead. In a September 1985 television appearance, Meltzer said regarding 1986 that he saw "inflation rising on average during the year... At least to 6 percent by the end of the year… higher inflation is what we're in store for."[307]

Friedman, too, continued to misjudge inflation. In the early-1985 revisions that they made to the text of *Tyranny of the Status Quo* for its U.K. paperback version, the Friedmans acknowledged that "the rise in inflation since [mid-1983] has been extremely modest" but reaffirmed their view that there was more inflation ahead on account of the 1982–1983 monetary expansion.[308] Then, writing in early 1985, Friedman noted that M1 growth had averaged 9.3 percent in the two years to 1982:Q3, and he used this as the basis for contending that "inflation probably bottomed out in mid-1983 and will rise—perhaps modestly, perhaps sharply—in the next year or two."[309] In mid-

[307] *Wall Street Week*, Maryland Public Television, September 27, 1985, p. 6 of transcript.
[308] Friedman and Friedman (1985, p. 92).
[309] Friedman (1985b, p. 60).

1985, Friedman still expected the 1982–1983 monetary growth surge to show up in inflation eventually: he predicted increases in the prime rate and inflation, with the latter reaching "8 or 9 percent in 1987 or 1988." In light of his recent setbacks, however, a jaded air accompanied this declaration, as Friedman added that he had no confidence in the forecast.[310]

Indeed, over 1985, Friedman progressively conceded the permanent character of the fall in inflation seen in the first half of the decade: "accelerating inflation was brought to an end in 1980," he acknowledged (*Wall Street Journal*, August 20, 1985). Later in 1985, Friedman came to accept that the 1982–1983 monetary surge had not left an excess that was still to be reflected in prices: the velocity decline of 1982–1983 and the expansion of output had been permanent.[311] He had even in 1983–1984 suggested that future inflation cycles might have a peak lower than in 1980–1981. He also accepted that the break in M1 velocity had constituted not only the level shift in 1982 but a permanently lower trend. He had believed in the second half of 1983 that an end to a rising trend in M1 velocity "not in sight."[312] But by the end of 1985, his assessment was the velocity of M1 was now roughly trendless.[313] Correspondingly, viewing monetary growth in 1985 as excessive, he still saw a rise in inflation in 1986 but one occurring against the background of "the bumpy disinflationary roller coaster of the 1980s" (*Wall Street Journal*, August 20, 1985).

These changes in position, significant though they were, were not enough to prevent a new round of bad forecasts on Friedman's part. Now taking M1 velocity as trendless, he took the double-digit M1 growth seen in 1985 as portending a period of commensurate nominal income growth. On this basis, he predicted in August 1985 that nominal income growth would pick up by the end of the year: "the substantial rise in monetary growth since the fourth quarter of 1984 will almost surely be followed by a substantial acceleration in the growth of nominal GNP, perhaps already in its early stages" (*Wall Street Journal*, August 20, 1985). Modern data instead show a fall in nominal GDP growth rate over the period in question, with the four-quarter rate of nominal GDP growth declining from somewhat under 7 percent in mid-1985 to a bit over 5 percent at the end

[310] Friedman (1985c, p. 215).

[311] This assessment was conveyed in the report on Friedman's appearance on November 12, 1985, at the Oppenheimer firm event, and in his PEPAB memo of January 10, 1986 (Friedman, 1986f). In the latter, Friedman partially attributed the decline in M1 velocity to a permanent increase in wealth arising from the dollar appreciation. As it is plausible that households and businesses might well regard rises in wealth arising from exchange-rate appreciation as ephemeral, this was a tenuous conjecture—and one that he did not pursue when trying to explain M1 velocity behavior in Friedman (1988a, pp. 241–244).

[312] Friedman (1984b, p. 51).

[313] See *Wall Street Journal*, December 18, 1985, and Friedman (1986f, p. 4).

of 1986.[314]

With regard to the prices picture, in November 1985, Friedman remarked: "Inflation is not dead. It will emerge once again and will be higher next year than it is this year. We almost surely are currently at the bottom of this inflationary episode and are likely to be starting up again." (*Daily News* (New York), November 13, 1985).[315] He reconciled this prediction with his acceptance of a permanent break from the 1970s: "We finally decided as a nation that we were going to do something about it (inflation) so from 1979 on, this nation has been having a disinflation. We have the same roller coaster as before, but now around a declining trend. We are at the bottom of one of these troughs, and I expect that inflation next year will be higher than this year, and still higher the year after that, as the result of monetary pressures that have already been built into the system." This was said by a reporter to be "as close as a senior monetarist has ever come to acknowledging that Paul Volcker's battle against inflation was succeeding." (*Boston Globe*, November 14, 1985.) Friedman had, however, already stated in 1985 that the United States had "succeeded in sharply reducing inflation."[316]

On public television in early 1986, Friedman said that monetary growth had been excessive and that 1986 would likely see higher rates of inflation resuming (*The MacNeil/Lehrer News Hour*, PBS, February 6, 1986, p. 11 of transcript).[317] In contrast to these predictions, twelve-month CPI inflation, as already indicated, fell in 1986, being 1.9 percent on average for the year, while its December twelve-month rate of 1.1 percent was the lowest in nearly 22 years. Friedman had now substantially overpredicted inflation for four years in succession.

Monetarism under siege

The decline in velocity starting in 1982, and the FOMC's downgrading in the same year of the M1 target, produced a reaction in the business press that two economists at the Federal Reserve Bank of St. Louis to note in May 1983 that "there have been numerous reports that monetarism is now virtually dead."[318] One of the most recent examples they cited had appeared only about a

[314] Real GDP growth did pick up during 1986. This was as Friedman had predicted (*Business Times* (Singapore), November 14, 1985; Friedman, 1986f). But the outcome did not really represent a strong vindication, as his higher real output growth projection flowed largely from his incorrect expectation of higher nominal income growth.

[315] Also quoted in *The Detroit News*, November 13, 1985, and *Chicago Tribune*, November 13, 1985.

[316] Friedman (1985e, p. 18).

[317] He had already written in a January 1986 for PEPAB: "The announced commitment of [the U.S.] government to a regime of zero or low inflation is becoming less and less credible… I expect inflation to continue [sic] to accelerate, ending up somewhere around 6 to 8 percent by the end of the year." (Friedman, 1986f, pp. 1, 2.)

[318] Batten and Stone (1983, p. 5).

month earlier: *Business Week* (a magazine that had, since the 1970s, taken a generally critical attitude toward Friedman) published an article, "The Failure of Monetarism" (April 4, 1983), citing the recession, high interest rates, and the decline in M1 velocity as counts against the monetarist position.

In their *Federal Reserve Bank of St. Louis Review* article on this ongoing backlash, Batten and Stone (1983, p. 5) noted wryly that the "alleged death of monetarism could not have come at a more inappropriate time"—as that alleged death seemed to imply that the propositions concerning money documented in Friedman and Schwartz's *Monetary Trends* had become invalid just as their book finally reached print. Batten and Stone noted, however, that even a decade earlier there had been declarations that recent economic developments or policy experiments had discredited monetarism.[319] This was the perspective that Friedman himself took on June 22, 1983, when he opened his keynote speech in Tokyo by observing: "To judge from the financial and popular press, monetarism has been tried in the past ten years in the United States and Great Britain has been found wanting."[320]

Friedman's address, and other writings he produced that year, provided a robust defense of the empirical validity of monetarism in light of recent U.S. experience. But, as has already been indicated, those same writings contained the erroneous inflation predictions that would eventually provide great ammunition for critics of monetarism. Friedman's errors in predicting inflation in the mid-1980s, together with further large declines in M1 velocity, provided the basis for criticism of monetarism in the business press and elsewhere that left a much greater impression than the obituaries of monetarism that had been published in the 1970s.

Despite those earlier critiques, Friedman had entered the mid-1980s with what one newspaper columnist called "impressive credentials as an economic seer" (*San Jose Mercury News* (California), February 13, 1979), especially on the strength of his prediction of resumed double-digit inflation in the late 1970s. Another columnist observed in 1983 with respect to inflation prospects: "Friedman has been right about this topic again and again." (*Pharos-Tribune* (Logansport, Indiana), October 17, 1983.) Friedman's subsequent run of failed inflation predictions meant that his standing as an authority took a major beating.[321] By the time he had, as

[319] See also Nelson (2020b, Chapter 15).
[320] Friedman (1983e, p. 1).
[321] After Friedman's racked up forecast errors concerning inflation in the mid-1980s, a critical article appeared claiming Friedman's *pre-1982* record on forecasting inflation was also poor. But this analysis (Brady, 1986) was flawed by misstatements and material omissions. Most notably, the analysis completely overlooked Friedman's successful prediction of the late-1970s surge in U.S. inflation.

he saw it, put his own monetary analysis back on the rails in 1986, he did not have the same prestige in the media as a monetary policy commentator that he had possessed in 1982.

One of the early items suggesting that Friedman's forecasting skills had deserted him was an Associated Press analysis in July 1984 that bluntly referred to the "terrible blunders… [of] those who were forecasting double-digit inflation by now" and immediately named Friedman (*The Progress* (Moshannon Valley, Pennsylvania), July 19, 1984). *Business Week* (August 13, 1984) again published a piece critical of monetarism, this time concentrating on the Friedman inflation-forecasting error and titled "The Monetarists Scramble to Explain Low Inflation." And although he was himself a monetarist in inclination, prolific newspaper columnist Maxwell Newton took note of the scale of the error, observing in October that Friedman had been projecting "rapidly escalating inflation as recently as April 27 this year… Both Professor Friedman and others forecasting escalating inflation have been grievously wrong." (*Daily Mirror* (Sydney), October 23, 1984.) Similarly, Federal Reserve Board Governor Charles Partee remarked at the FOMC meeting of November 7, 1984: "You may remember in the spring that it wasn't hard to find outliers like Milton Friedman who thought that inflation would be at double digits by the end of this year. That has disappeared entirely."[322] Instead of being the bad year for the economy that he had feared, 1984 had turned out to be a bad year for Milton Friedman.

The years 1985 and, particularly, 1986 then saw a string of articles in the business press suggesting that the money-income relationship had collapsed and that monetarism had met its demise. Friedman's former home of *Newsweek* had one such article headlined "The Monetarists on the Run" (September 23, 1985).[323] Friedman himself would quote a couple of other headlines that appeared in 1986: "Is This the Year Monetarism Vanishes?" in the London *Economist* (January 4, 1986) and "Monetarism Falls from Grace" in the *New York Times* (July 3, 1986).[324] "Some of you may have seen two days ago in the *New York Times* a big article in the business pages about how monetarism has failed or how monetarism has been discarded," Friedman remarked after the appearance of the latter article. "We have [had] a whole spate of articles of

[322] In Federal Open Market Committee (1984b, p. 15).

[323] In *Business Week*, the columnist Robert Kuttner, who was already poorly disposed toward monetarism (as well as toward much standard macroeconomic analysis), wrote a column (August 19, 1985) titled "What's Putting a Stake in the Heart of Monetarism" and declaring: "The theory is [in] a shambles… It is high time to give monetarism the decent burial it so richly deserves." In addition, *Dun's Business Month* (January 1986, p. 78 of reprint) judged that "monetarism [is] now on the ropes."

[324] In the former case, the *Economist* headline appeared on the cover page. A variation of this title was used in the internal editorial. Friedman quoted the headlines in *Wall Street Journal*, September 18, 1986.

this kind."[325]

One sign of the movement in the news media was the disaffection toward monetarism that emanated from economics columnists who had been formerly more sympathetic. In the United Kingdom, Samuel Brittan remained an admirer of Friedman's critique of stabilization policy and of centering aggregate demand on nominal variables. But, as already indicated, he had become disillusioned with monetary aggregates as intermediate targets. In this connection, during the early 1980s Brittan repeatedly defended Volcker's Federal Reserve against U.S. monetarist critics. Furthermore, he now strongly favored nominal income targeting. For his part, Friedman was prepared to grant, as he did in his *New Palgrave* entry that he drafted in late 1985, that advocacy of a nominal income target rather than a monetary-aggregate target could be consistent with a quantity-theory approach.[326] But he continued to defend a constant-monetary-growth rule as an "effective insurance policy" whose adoption in the mid-1960s would have allowed the United States to avoid "the accelerating inflationary roller coaster of the 1960s and 1970s" (*Wall Street Journal*, August 20, 1985). In contrast, Brittan argued that a nominal income target was far preferable, and he was, as already indicated, receptive to judgment-based policymaking, albeit guided by the nominal income target.[327]

Among other economics journalists, U.S. television presenter and newspaper columnist Louis Rukeyser had reacted favorably toward Friedman's critiques of U.S. government (administration and Federal Reserve) policy regarding inflation during the 1970s. The positive attitude toward

[325] From Friedman's remarks of July 5, 1986, at a session of the Western Economic Association meetings in San Francisco, in Darby and others (1987, p. 5). A further addition to this "spate of articles" had appeared in the Bay Area newspaper, *The Tribune*, on the day before Friedman spoke. Pointing to the combination of low inflation, high annualized rates of monetary growth, and real output growth for the first half of the year that was believed to be about 2.5 percent per annum (later found to be about 2.8 percent), the article stated: "Today's economy seems to discredit a basic tenet of monetarism… which has as its dean Nobel laureate Milton Friedman." (*The Tribune* (Oakland, California), July 4, 1986, p. C–1.)

[326] Friedman (1985d, p. 68; 1987d, p. 18). During the same period, Friedman also defended the significance of nominal income stabilization against skepticism expressed by the *Wall Street Journal*. He wrote: "The editorial writer may not believe it 'especially helpful to control nominal GNP,' but that is the most monetary policy can do." Friedman added that, of course, the temporary nature of monetary policy's effect on real variables meant that appropriately restrained monetary policy "is essential to avoid inflation." (*Wall Street Journal,* December 18, 1985.)

[327] Brittan's view that monetary targeting was outmoded likely shaped his reaction to *Monetary Trends*. Brittan had concluded that the money/income relationship had weakened in both the United States and the United Kingdom. When the Hendry-Ericsson (1983) critique of *Monetary Trends* arrived, Brittan defended Friedman and Schwartz to some extent. But, with regard to the debate, he would recall that "I couldn't feel much sympathy for either side on that." (Samuel Brittan, interview, April 18, 2013.) In his column on the debate, Brittan also suggested that the long delay in finishing the book might have reflected the possibility "that Friedman and Schwartz had a great deal of difficulty with the British data." (*Financial Times* (London), January 19, 1984.) The implication Brittan created via this formulation was that data collection had been only one source of delay and that Friedman and Schwartz might have had trouble getting to grips with the empirical relationship between U.K. money and income (which Brittan believed to be particularly loose).

monetarism taken in Rukeyser's 1983 book, *What's Ahead for the Economy* was noted in a review (*San Francisco Examiner*, February 19, 1984). Indeed, the book's discussion indicated that the monetary-growth/inflation relationship was "apparent to anyone" who examined the data offered in the monetarist literature, with Rukeyser stating that, in particular, the postwar relationship between inflation and U.S. M1 growth was the kind of evidence that moved monetarist propositions into "the realm of demonstrable fact" (Rukeyeser, 1983, p. 108). But Ruykeser, too, was one of those who became very disillusioned with monetarism over the 1982–1986 period: one of his *Wall Street Week* television programs (the edition of September 27, 1985) was titled "Dark Days for Monetarism?" and a couple of weeks after its broadcast Rukeyser observed in his syndicated column that "the latest sect of economic priests to incur public ridicule is the monetarists," whom he praised for predicting 1983's economic strength but who had subsequently (and, by implication, Friedman in particular) hurt their credibility through their subsequent warnings of recession and high inflation (*The Olympian* (Washington state), October 6, 1985). Although Rukeyser would continue to bring on Friedman as a guest in a number of broadcasts over the following decade, his disaffection with Friedman's monetary views was permanent.

Another economics journalist who would lose confidence in Friedman's macroeconomics was Warren T. Brookes, who was on the editorial staff of *The Detroit News* and who also had a nationally syndicated economics column. Friedman had expressed admiration for Brookes' journalism in the past and written an endorsement for a 1982 collection of Brookes' pieces. During the Reagan years, however, Brookes sided with what he called the "monetary reform agenda" of the return to some kind of a Gold Standard and against what he called "Volcker's crisis-management high-interest-rate monetarism" (*The Detroit News*, November 14, 1985). In this connection, Brookes published repeated critiques of monetarism, under such titles as "Monetarism's Fatal Flaw" (*The Titusville Herald* (Pennsylvania), March 22, 1985), "Monetarism Adrift" (*The Detroit News*, November 7, 1985), and, in Friedman's home town, "Emperor Monetarism Has No Clothes On" (*San Francisco Examiner*, November 10, 1985). Brookes later reported that, in response to the last piece, Friedman had sent him a lengthy rebuttal (which likely consisted mostly of enclosures of recent writings), taking exception to being associated with the instability seen in the Volcker years (*Boston Herald*, June 26, 1986). But Brookes was unpersuaded, and his analysis continued to draw a distinction between Friedman's work on microeconomic issues—which he saw as the durable contribution of "that great and kindly genius who has done so much for the cause of freedom and free markets" (*The*

Titusville Herald (Pennsylvania), March 22, 1985)—and Friedman's monetary analysis.[328] In opposing the latter, both in its domestic and international dimensions, and doing so from a supply-side and pro-fixed-exchange-rate position, Brookes was taking a stance similar to that long held by the *Wall Street Journal*'s editorial writers.

In the *Wall Street Journal* itself, a long, front-page news-analysis article on monetarism on December 10, 1984, acknowledged that monetarists had changed thinking about monetary policy in the United States and that their views had shaped the U.S. move to disinflation but stressed their recent loss of influence on key aspects of policy, and it judged that Friedman had been "dead wrong" about both the real economy and inflation in 1984 (p. 16). The editorial writers themselves entered the fray a year later with a piece titled "The Trouble With Monetarism" (December 4, 1985), criticizing Friedman's views on domestic and international monetary relationships alike. The *Journal* published Friedman's letter of rebuttal (December 18, 1985*a*): as he often did during this period in his responses to critiques, he took exception to the writers' identification of monetarism with the Volcker Federal Reserve.

An op-ed that Friedman published in the *Journal* nine months later used as its starting point some of the negative coverage of monetarism that had appeared in the international press during 1986. Superficially, the op-ed seemed, like the December 1985 letter to the *Journal* and many of his other writings since 1983, to be a defiant statement that the critics had it wrong. But a closer look at the article revealed the effects of Friedman's midyear reevaluation of monetary events in the 1980s. He acknowledged that there was some substance in the critics' case and, whereas in August 1985 Friedman had been maintaining of the M1 growth/nominal income growth that "if anything, it has been closer in recent years than it was earlier" (*Wall Street Journal*, August 20, 1985), he now granted that this was not true since "about 1984" (*Wall Street Journal*, September 18, 1986). The op-ed was also notable in bringing out a major conclusion that Friedman had reached that year—that M2, rather than M1, was the aggregate on which to focus.

Friedman would subsequently point to this September 1986 *Wall Street Journal* op-ed as the piece in which he registered his revised judgment that "M1, as currently defined, is a less satisfactory aggregate" for monetary analysis than M2. This was a watershed change that, in

[328] Of course, Brookes was making this remark in the course of an analysis that rejected an option that Friedman had advocated on grounds of both monetary analysis and promoting free markets: floating exchange rates. Brookes therefore conformed to Friedman's (1953a, p. 203) generalization that some fixed-exchange-rate proponents would have the price system operating freely everywhere in the economy except the foreign-exchange market.

effect, meant that Friedman was repudiating a good deal of what he had stated on monetary matters in the four-year-plus period from the start of 1982 until the first half of 1986.

M1 versus M2

Friedman's 1986 reassessment settled him on a perspective on 1980s monetary and economic developments that he maintained for the remaining twenty years of his life. It implied many changes in the details of his analysis of recent U.S. monetary policy developments. But it did steer him away from the defiant and M1-focused stance that had led him to make a string of well-publicized forecasting errors.[329]

In this connection, it is worth considering in detail where Friedman went so wrong in his monetary analysis in 1982 to the early part of 1986. One aspect of his error—underestimating the amount of slack in the economy when he made his initial predictions, in 1983 and 1984, of high mid-1980s inflation—will be covered in detail in the later section titled "Otto Eckstein."

Beyond this error, however, a crucial mistake on Friedman's part was his misinterpretation during 1982–1986 of the modern M1 aggregate. He erroneously took this series—which was, essentially, the old M1 series expanded to include thrifts' M1-type deposits—as corresponding to a *broad* measure of money like the M2 series he and Schwartz had used in their work. Interpreting M1 in this way was inappropriate because M1, both in its old (pre-1980) and new versions, lacked the time-deposit series that typically bore better interest rates than checking accounts. The environments of falling interest rates of 1982–1983 and 1985–1986 were those in which a narrow series like M1 was prone to grow quite differently from an M2-type series, because the opportunity cost of holding M1 balances was diminishing much more than that of holding M2 assets. And M1 and M2 growth rates indeed differed over this period: see Figure 6.

Yet Friedman, in the mid-1980s, interpreted M1 as though it was an M2-type aggregate. He resisted viewing the reported M2 series as the successor to the aggregate he had previously studied. And he treated the double-digit M1 growth seen over much of the 1982–1986 period as though it had the same implications for inflation as the double-digit M2 growth of the 1970s.

[329] David Laidler, an admirer and former student of Friedman, nevertheless certainly had Friedman in mind when he referred (Laidler, 1990, p. 59) to "careless monetarist predictions" concerning inflation that were made in the early stages of the economic recovery that began in 1982. Laidler emphasized Friedman's failure to allow for a decline in velocity in response to disinflation. The more notable occasion on which Friedman neglected this decline, however, was in 1985–1986. In 1983–1984, in contrast, as discussed later, Friedman did allow for a major decline in velocity but, in forecasting inflation, he underrated the scope for the U.S. economy to expand without a rise in inflation.

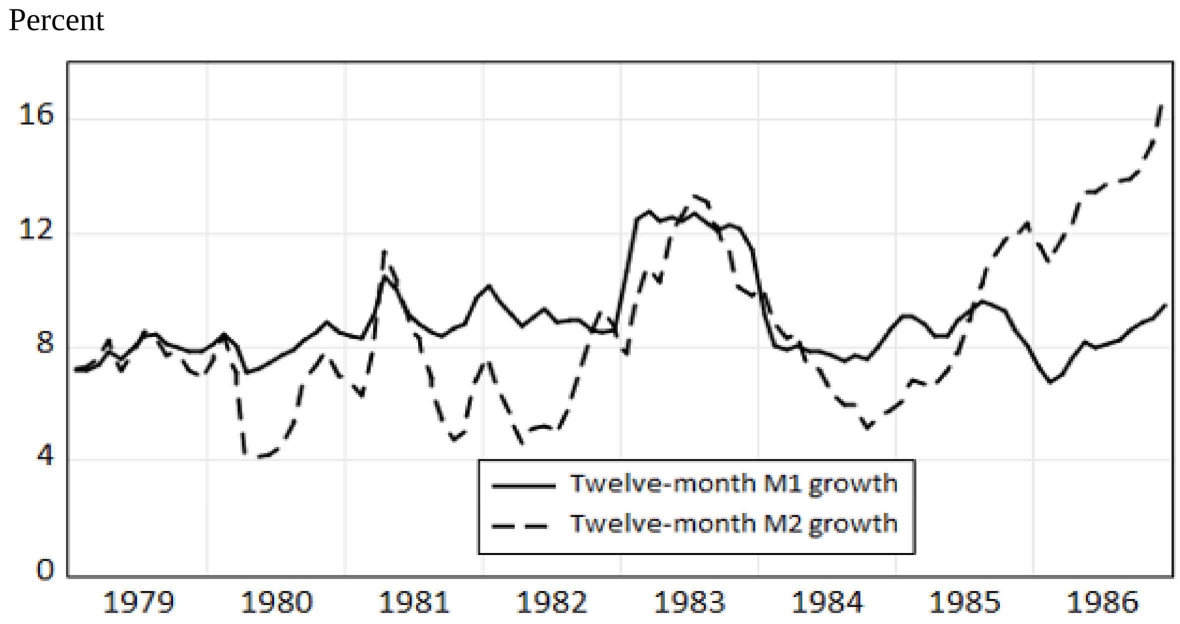

Figure 6. Twelve-month growth rates of M1 and M2, January 1979–December 1986.
Source: Federal Reserve Bank of St. Louis' FRED portal.

Friedman was, of course, long aware of the historical and conceptual differences between the M1 and M2 series. As discussed at length in Nelson (2020a, Chapter 5) as well as in the previous chapter, the longstanding Friedman and Friedman-Schwartz practice was to define money broadly, as M2, even though U.S. monetary analysis was strongly disposed toward an M1 series. As Friedman recalled in 1984: "In the past, I always found M2 to be a more reliable guide to economic events than the earlier M1."[330] After he and Schwartz used M2 in their *Monetary History*, they had defended this choice at length in their 1970 *Monetary Statistics*.[331]

As we have seen, when the Federal Reserve redefined M1 and M2 in 1980, Friedman was initially inclined to rely on the new M2. Indeed, as discussed in the previous chapter, in September 1981 he defended the new M2 series at a time when the Volcker FOMC was strongly stressing M1, Friedman's argument being that M2 was more immune than M1 to distortions arising from the introduction of new financial instruments.

Yet, at the turn of 1981/1982, Friedman—without providing much by way of an explanation—moved to his near-exclusive focus on M1. Because the current M1 and M2 series were behaving

[330] From his remarks in Heller and others (1984, p. 51).
[331] Friedman and Schwartz (1963a, 1970). As already indicated, Friedman and Schwartz (1982a) also used (the old definition of) M2 as the money measure.

quite differently, this switch led to some immediate about-turns on his part when it came to analyzing recent Federal Reserve policy.[332]

Why did he come to rely on M1 from 1982 to 1986?[333] One major reason was the close relationship that M1 growth and nominal income growth enjoyed in the early 1980s. M1 had done quite well in tracking nominal income throughout the New Operating Procedures period. But it did especially well in the period starting in early 1981—a period that Friedman subsequently characterized as a "hot streak" of M1 (*Wall Street Journal*, September 18, 1986) in which the bivariate correlation between M1 growth and nominal income growth was especially strong. In particular, as indicated in the previous chapter, M1 gave a more clear-cut picture of tight money over 1981 and into 1982 than did M2, and so gave an accurate signal both of the severe 1981–1982 recession and the 1982–1983 decline in inflation.

As for M2 growth, it was—as stressed by Abel and Bernanke (1992, p. 635)—generally negative in real terms in 1979–1981, so its behavior was not inconsistent with weakness in real economic activity. In nominal terms, however, its growth was robust and did not foreshadow very well the weakness in nominal income growth seen during the 1981–1982 recession. This weakness was, instead, registered in a fall in M2 velocity in 1981 and 1982—even though, in contrast to M1, M2 velocity did not exhibit the very sharp difference from pre-1981 behavior—a break in trend—that M1 started to display.

A factor on which the Federal Reserve put great emphasis—the potential implications of changes in the institutional environment for the interpretation of monetary aggregates—would, in 1983, firm up Friedman's confidence in M1 and apparently underline his erroneous conviction,

[332] One person who noted this change at the time was Federal Reserve Board Governor Charles Partee, who told an FOMC meeting on March 30, 1982, that "Milton Friedman was [traditionally] for M2—it's only recently that he has changed to M1." (In Federal Open Market Committee, 1982e, p. 49.) Another person who noticed the change was supply-sider Alan Reynolds, who criticized Friedman for his switch from M2 in 1981 to M1 in 1982 and noted that the change had a substantive effect on Friedman's evaluation of the stance of monetary policy (*Wall Street Journal*, June 29, 1982*b*).

[333] Barnett (1997, pp. 1171–1172) suggested that Friedman's analysis, including his erroneous inflation forecasts, during 1983 was based on M2, and Barnett used this interpretation as motivation for advocating the use of Divisia monetary aggregates in monetary analysis in place of so-called simple-sum (that is, the main officially published) money series. But Friedman's 1983 inflation forecasts were based on M1, not M2. And the difference in signals given between M1 and M2 (especially when the latter is adjusted for the introduction of money market deposit accounts in 1982–1983) was likely more important for interpretations of 1980–1984 developments than the distinction between simple-sum and Divisia monetary aggregates that Barnett (1997) emphasized. In particular, the Divisia version of the M1 aggregate actually showed very rapid growth in 1982, much like its simple-sum counterpart, although it did show distinctly slower growth rates than actual M1 in 1983 (see Belongia, 1996, Figure 1, p. 1069). Growth rates in Divisia M1 and actual M1 also had very similar mean growth rates from 1980 to 1992 (see Belongia, 1996, Table 1, p. 1070).

expressed from 1983 to 1985, that the modern M1 series was like the old M2. As background in understanding how he reached this conviction, it is worth considering Friedman's perspective toward the FOMC's 1982 "deemphasizing" decision regarding its targeting of M1.

When the Federal Reserve suspended the M1 target in the fall of 1982, citing distortions to the series that had been or would be produced by a number of financial innovations, Friedman was briefly respectful of the decision but soon became scornful. The latter attitude arose partly, no doubt, from his unhappiness about other steps that the FOMC took in the same period, including its shift back to a federal funds rate instrument. But his negative attitude also reflected Friedman's recollection of past episodes in the postwar period. The fall of 1982 was not the first occasion on which the claim had been made at the official level that financial innovations were disturbing velocity behavior. On the contrary, as Friedman saw it, there was a long tradition of central-bank emphasis on instabilities in money demand. In January 1977, he had remarked: "I have observed over a long period of time that whenever anything goes wrong with monetary policy, the favorite excuse of the monetary authorities is that there has been an exogenous shift in the demand for money."[334] Similarly, Friedman wrote in August 1982 that "the talk about changes in the demand for money is simply a red herring introduced by the Federal Reserve… In each case[,] it has turned out that there has been no change in the demand for money..."[335] Friedman was also chastened by his own lapse in 1972, when he had briefly persuaded himself that the high monetary growth of 1971–1972 had been permanently absorbed by a velocity shift and so did not signal future inflation (see Nelson, 2020b, Chapter 15).

And Friedman was not alone among monetarists in discounting policymakers' appeals to financial innovation and to related shifts in the money demand function. Karl Brunner had referred in 1972 to "many Federal Reserve discussions asserting the fragile, volatile or highly

[334] From Friedman's remarks of January 26, 1977, in Friedman and Modigliani (1977, p. 26). See also his remarks in Friedman (1970g, p. 43) and Instructional Dynamics Economics Cassette Tape 190 (May 1976, Part 1), as well as the discussion in Nelson (2020b, Chapter 15).

[335] From Friedman's letter of August 16, 1982, to Senator Robert W. Jepsen, in Friedman (1982d, p. 73). In the wake of the fall 1982 monetary policy shift, other critics of the Federal Reserve took the same perspective but, when articulating that perspective in their writings, used still more polemical language. For example, Maxwell Newton, the monetarist-leaning financial columnist, wrote very soon after Volcker announced that M1 would be deemphasized in policymaking: "The justification offered to an evidently credulous world for the Fed's decision to allow the money stock M1 to float way above target in the coming weeks is the hoary old lie brought out by the Fed from time to time—namely that 'financial innovations' have made M1 less relevant or less accurate as a measure of 'money.' This lie has been proffered by the Fed for at least 40 years." (*New York Post*, October 12, 1982.) By 1987, Newton had clearly accepted that M1 actually had been distorted by financial innovations, or at least that its velocity had shifted during the 1980s. Like Friedman, he had by then switched to focusing on M2 in his analysis (*The Times* (London), April 21 and July 20, 1987).

unstable nature of the public's money demand."[336] Anna Schwartz's view was that the "routine exoneration of central banks for their failure to achieve the monetary target growth rates is [the claim] that the demand for money function has shifted" (*The Banker* (London), February 1985, p. 101), while Allan Meltzer had written in the early fall of 1982 (Meltzer, 1982, p. 126): "Usually, allegations about changes in the demand for money are a device that the Federal Reserve uses to cover up its errors."

Federal Reserve policymakers made more accurate judgments than their monetarist critics did as monetary and other economic data accrued during 1982–1986. They were, in particular, correct in their early judgment that velocity *did* shift permanently in 1982 and that this shift continued well into 1983. Correspondingly, Volcker and other FOMC members would prove to have been correctly cautious in interpreting M1's behavior in those years. Largely in consequence, their policy actions, and the rates of monetary growth delivered by those actions, were more appropriate than what monetarists were recommending at the time.

An irony, however, is that although the Federal Reserve's actual policy decisions in 1982–1986, and especially in 1982 and 1983, proved deft and typically gave M1 an appropriately diminished weight, policymakers' *diagnosis* of the 1982–1983 M1 velocity pattern—which was to the effect that the velocity of M1 was falling because of the introduction of new types of bank deposit—now appears to have been, in large part, a misguided judgment.

As of the end of 1984, the role of new deposit types in diminishing M1 velocity was not considered a particularly controversial matter: in a news story, for example, the *Wall Street Journal* (December 10, 1984, p. 16) stated: "Financial deregulation severely distorted this relationship [that between M1 and nominal income] in 1982 and 1983."[337] But subsequent research studies of the demand for M1, using the benefit of more 1980s time-series observations than those to which the FOMC had access during 1982 and 1983, have suggested that financial innovations probably were not very important in accounting for M1 velocity behavior in the first six years of the 1980s (although they *did* become very important in the 1990s).[338] Two points are

[336] Brunner (1972, p. 106).

[337] Among researchers, however, the question was treated as a somewhat more open one than it was in policy circles and media discussions. See Federal Reserve Bank of San Francisco (1983).

[338] The financial innovations in the 1990s were more along the lines that policymakers had perceived or stressed up to 1982: those that would *increase* the velocity of M1 (or speed up its rate of increase), rather than be a force lowering it. In late November 1982, after the FOMC had dropped the 1982 M1 target in response to actual and prospective declines in velocity, the Federal Reserve Bank of Boston's president, Frank Morris, correctly anticipated that the principal force destabilizing M1 velocity in the longer term would be the economizing on money balances facilitated by increased "computerization of the financial system" (F.E. Morris, 1982b, p. 13). He had, in a speech

notable here. First, the mainstream Federal Reserve analysis until the late 1980s missed the scale of the influence of interest rates on velocity because it used Goldfeld (1973)-style estimated money demand functions that implied that real money balances depended only weakly on opportunity-cost variables. In contrast, studies of longer-run money demand by Lucas (1988) and Stock and Watson (1993, pp. 799–810) would find that interest-rate behavior accounted for the decline in M1 velocity in the 1980s. Second, the velocity of currency exhibited a break in trend at the start of 1982 that largely paralleled the shift seen in the velocity of the deposit component of M1 at that time. This fact suggested that the shift in M1 velocity's behavior in the 1980s was not to be explained in terms of banking deregulation or in the changing character of M1 deposits (see Rasche, 1987, pp. 15–18, 67).

In this area, notwithstanding numerous erroneous statements about money over these years, Friedman was on the right track in 1982–1986. He downplayed deregulation as a factor driving M1 velocity's decline or making for a distortion of the M1 aggregate. He cited the fact that, despite "all the talk about 'institutional changes' and 'financial innovation,'" monetary growth and nominal income growth had remained closely correlated (*Newsweek,* January 16, 1984). To be sure, he recognized the 1982–1983 velocity decline, as well as the fact that a good part of it was not likely to be reversed. But instead of stressing institutional change, Friedman's interpretation—which proved to be in keeping with the findings of key research on money demand from the late 1980s onward—was that the velocity decline resulted from the declines in inflation and nominal interest rates.[339] He viewed much of the pre-1982 rising trend in velocity in that light and, as early as January 1983, he indicated that he did not expect M1 velocity to resume a 3 percent growth trend.[340]

Friedman also indicated in 1983 and 1984, however, that, even in the absence of any return to upward-trending interest and inflation rates, he still expected a 1 to 2 percent trend in M1 velocity—his belief being that there would be ongoing economizing on money balances even when the opportunity cost of holding real M1 balances had stopped rising.[341] As a description of

given the previous March, accurately assessed that as "deposit-sweeping becomes widespread" due to this increase in computerization, M1 velocity would increase steeply (F.E. Morris, 1982a, p. 85). The principal error that Morris made in early 1982 was in supposing that this factor would be a major force raising M1 velocity in the immediately coming years of 1983 onward.

[339] See, for example, Friedman (1983e, p. 10) and the other Friedman discussions of this vintage mentioned above.

[340] In his January 1983 memorandum, Friedman (1983f, p. 7) indicated that he expected M1 velocity's trend to be positive in the future as it had in the past, but his remark in *U.S. News and World Report* later in the month that "around 2 percent" per year M1 growth was consistent with a long-run inflation rate of zero implied that (consistent with his writings later in the year) that he expected the trend to be about 1 percent per year, provided that inflation and nominal interest rates had ceased to exhibit a longer-term rise.

[341] Friedman (1984b, p. 58).

M1 velocity behavior from the mid-1990s onward, this was not a bad projection. But as a prediction regarding how M1 velocity would behave in the remainder of the 1980s, the postulate of a rising trend fared poorly. By late 1985, as already noted, Friedman had moved to the position that, since 1980, M1 velocity now had no trend in the absence of rising interest rates. This represented a shift to the position already articulated by Judd (1983) and for which formal evidence was later provided by Lucas (1988) and Hoffman and Rasche (1991, 1996): that essentially the whole of the pre-1982 postwar upward drift in M1 velocity had been due to the impetus provided by rising nominal market interest rates.

The basic reason why the behavior of U.S. financial market interest rates provided a plausible explanation for velocity behavior in the 1980s has already been mentioned: the financial changes of the 1980s did *not* have the practical effect of interest rates on M1 deposits highly competitive, *vis a vis* either market rates or interest rates available on other retail accounts at depository institutions. Consequently, while M1 accounts continued to be demanded for transactions purposes, a truly strong motivation for holding them as a savings vehicle was not generated by the financial changes of the early 1980s. Although the payment of interest on M1 accounts (specifically, the “other checkable deposits” category containing either negotiable orders of withdrawal (NOW) or NOW-type accounts) was frequently cited as a variable shifting M1 velocity in the 1980s, this factor was probably less important than commonly thought. The NOW accounts, though interest-bearing and available nationwide from the start of 1981, were subject for the moment to an interest-rate ceiling, whereas deposits outside M1 had a higher, or no, rate ceiling. And although Super NOW accounts, also included in M1 deposits, were introduced by U.S. depository institutions at the start of 1983 and were not subject to an interest-rate ceiling, in practice the rate offered on these accounts was notably below market rates, as discussed presently. So, although *not regulated*, the rates offered on these M1 deposits were not really strongly market-linked.

In contrast, with regard to the non-M1 part of M2 deposits, Lyle Gramley observed in March 1982 that it had “changed materially since 1978: more than 60 percent… consists of assets bearing market-related yields.”[342] Non-M1 M2 deposit interest rates also adjusted more rapidly than before in response to movements in market rates (Judd and Motley, 1984, pp. 60–61).[343] In

[342] Gramley (1982, p. 398) (see also Lindsey, 1983). Admittedly, Gramley was speaking when M2 included certain wholesale items, whose presence was contrary to the spirit of the recommendations in Friedman and Schwartz (1970) and which were later dropped from the aggregate. But Gramley was also speaking before the advent of money market deposit accounts (MMDAs)—the retail item that became a very high-yielding component of M2.
[343] Conversely, the yields on interest-bearing M1 accounts adjusted sluggishly in relation to market rates. One consequence of this feature was likely to intensify somewhat the decline in M1 velocity when interest rates fell in

contrast, upon their introduction, the interest rates offered on Super NOW accounts were reported officially as being 7.5 percent (F.E. Morris, 1985, Chart 1, p. 227 of 1987 reprint). This rate likely overstated the reality facing many bank customers, as reports in some areas suggested lower Super NOW rates, such as 5.95 percent, 6.3 percent, or 6.5 percent: above the 5.25 percent ceiling on NOW accounts, but well below market rates and on certain other retail accounts (*Augusta Chronicle* (Georgia), January 5, 1983; *Seattle Times*, January 6, 1983, p. E1).[344] In this environment, Tatom (1983) correctly contended that Super NOWs' introduction would likely shift deposits *within* M1 (as Super NOWs were undoubtedly an attractive form in which to hold M1 balances) but would do little to attract funds *into* M1.[345]

An appropriate evaluation seems to be that policymakers were correct about M1 velocity's overall behavior in 1982–1983 but that their specific reasoning was faulty. They had precipitously suggested before 1982 that financial innovations would soon greatly distort M1—usually by arguing that it would increase velocity. This feared surge in M1 velocity did not happen in the 1980s (or, at least, had been addressed already by the 1980 redefinition of M1). In 1982–1983, policymakers were largely persuaded that financial innovations *had* indeed distorted M1—albeit by making M1 balances more attractive and so reducing velocity. As it turns out, they were right to stress the importance of a break in M1 velocity's behavior in this period. But they likely underestimated the role that declining interest rates, as opposed to institutional change, had played in generating this outcome.

Although Friedman was correct to downplay financial innovation and deregulation as important

the mid-1980s. See Burger (1988, pp. 51–52) and F.E. Morris (1985, Chart 2, p. 6; p. 227 of 1987 reprint). That said, it should be stressed that Hoffman and Rasche (1991, pp. 668–669) found that, for sample periods ending in the late 1980s, the inclusion of an own rate on M1 balances had little effect on money demand estimates. And Hoffman and Rasche's (1996) subsequent monograph studied empirical M1 demand developments through 1991:Q4 without using an own-rate series, while the study's index had no entries on deregulation or NOW accounts. It should also be stressed that the manner in which the research literature has refined the definition of the Divisia M1 aggregate has essentially implied that U.S. retail bank customers' main means of earning attractive interest rates on their deposits, while also staying liquid enough to meet their transactions needs, has been by allowing their M1 holdings to be routinely transferred into the non-M1 component of M2, and not by seeking interest-bearing accounts in M1: see Belongia and Ireland (2016, p. 1227). That this has proved to be the most fruitful approach to defining Divisia M1 cautions against stressing the importance of the own rate on M1 balances as a factor raising the demand for M1.

[344] It was therefore something of an overstatement to claim that "Super NOW accounts… yield a market return to holders," as the Federal Reserve Board's July 1983 *Monetary Policy Report* did (see Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1983c, p. 49). Another likely overstatement along these lines occurred when the (uncredited) editor of Federal Reserve Bank of San Francisco (1982, p. 77) added to that conference volume a galley note—one that asserted that Super NOWs' introduction had had the effect of "artificially raising" M1 by about 5.5 percentage points.

[345] Correspondingly, Judd (1983, p. 144) noted after the event that "only small amounts of funds in Super NOWs apparently came from non-M1 sources."

as triggers for the declines in M1 velocity seen over 1982–1986, that period saw him overrating the importance of deregulation anyway—because deregulation helped persuade him that M1 was a broad-money-like aggregate.

Friedman arrived at this erroneous position through his misinterpretation of what he referred in June 1983 to as "major changes" in financial institutions in the December 1982-January 1983 period.[346] One of these changes—the introduction of MMDAs—was, as already noted, widely recognized as an important factor bearing on the behavior during 1982–1983 of the modern definition of M2. For the moment, however, Friedman was not very concerned with that aggregate. The other change in this period was the aforementioned advent of Super NOWs, and this development does appear to have led Friedman to a reckless conclusion. The broadening of the definition of M1 to include interest-bearing deposits, and the fact that *legally* some of those deposits (Super NOW accounts) could, from the start of 1983 onward, bear market-linked interest rates, persuaded him that the modern M1 aggregate was the money measure that most resembled the M2 concept that he and Schwartz had used in their joint work.

This judgment apparently arose from an instance in which Friedman, despite his disdainful attitude toward the "talk" about the effect of financial innovations on M1, was somewhat overawed by the hype associated with NOW accounts rather than the reality. High-rate M1 accounts were a hypothetical implication of the existence of interest-bearing deposits appearing in the M1 aggregate. Friedman evidently concluded that this hypothetical possibility was a reality and that Super NOW accounts, in particular, were, in common with some of the deposits in the old M2 aggregate, instruments that offered interest rates that largely kept pace with market rates. This was a conclusion to which Friedman was congenial because it allowed him to reconcile the empirical success of M1 in 1982 and 1983 with his attachment to M2 before the 1980s. But his confounding of new M1 and old M2 proved to be a major mistake in his monetary analysis. It was a mistake to which Friedman adhered during 1983 and 1984 and that marred his commentaries over that period.

Friedman's confident but ill-founded view that M1 had inherited the former M2's properties was evident in the June 1983 talk on U.S. monetary policy that he gave in Tokyo. He stated that "the present M1… is closer to the concept that I have typically used in my research and which the Fed earlier designated M2 than to the concept which the Fed earlier designated M1."[347] And in

[346] Friedman (1983e, p. 10).

[347] Friedman (1983e, p. 4).

Newsweek the following month, he asserted that modern M1 was a better "approximation of the old M2 than the present M2" (*Newsweek*, July 25, 1983). In his *Wall Street Journal* op-ed, he attributed characteristics to M1 that were really only true of the (old and new) M2 definition—claiming, for example, that velocity usually declined in a recession, a property that was true of (old and new) M2 velocity but had not been typical of M1 velocity in the postwar period until 1982.[348]

Elsewhere, he did not altogether claim that M1 was the successor to the old M2, but he came close. Indeed, in his June 1983 remarks, he suggested that the noninflationary M1 growth was 2 to 5 percent—little different from his traditional M2 growth prescription of 3 to 5 percent.[349] However, writing about a month later, he seemingly realized that he had gone too far in suggesting the parallels between new M1 and old M2 and instead gave the noninflationary M1 growth range as 1 to 3 percent, with the likeliest rate now given as 1.5 percent.[350]

So Friedman was not invariably and unequivocally claiming that new M1 and old M2 had identical properties. But in many respects—and, in particular, when analyzing future developments—he treated M1 as a series to which his prior analysis, which had used old M2, could be transferred. His *Wall Street Journal* analysis (September 1, 1983) applied much of the Friedman-Schwartz *Monetary Trends* money demand function estimated through the mid-1970s using the old M2 definition, to the analysis of the recent behavior of M1 velocity.[351]

An article that came out of Friedman's presentation at the American Economic Association meetings in San Francisco in the closing days of December 1983, and which Friedman would write up for the May 1984 proceedings issue of the *American Economic Review*, represented the

[348] See Ando (1985, pp. 2–3) and Friedman's reference (Friedman, 1983e, p. 11) to the "standard cyclical pattern" of velocity.

[349] Friedman (1983e, p. 4).

[350] See Friedman (1984b, p. 37). At a Federal Reserve Bank of San Francisco conference, whose first full day of proceedings (December 5, 1983) Friedman attended, F.E. Morris (1983a, p. 137) noted the conflict between the fact that "Professor Friedman said that the new M1 is really the old M2" and expectations that M1 would have a rising trend much like the old M1 (and unlike the old M2).

[351] It did not, however, apply the Friedman-Schwartz (1982a) estimates directly to the present M1. Instead, it attempted to discern interest-rate-driven and pure-trend aspects of M1 velocity behavior using largely judgmental, episode-based methods. But the analysis did apply to M1 the procedure Friedman and Schwartz (1982a) had used for calculating the own rate on M2 balances. This procedure likely greatly overstated the own rate on M1. Friedman (1988a, p. 241) later accepted that the own rate on M1 balances was better treated as zero than as closely aligned with market rates. However, his general lack of interest in the M1 series from mid-1986 onward likely made him agnostic, or indifferent, regarding arguments about whether deregulation had distorted M1, and he remarked that "I am not arguing that those changes [in connection with NOW accounts' introduction] are irrelevant at all." (In Darby and others, 1987, p. 25.)

high point of his confidence in M1. Friedman spoke at a session “Monetarism: Lessons from the Post-1979 Experiment” that had a standing-room only crowd size (*New York Times*, December 31, 1983). In his presentation, Friedman combined a number of judgments concerning the choice between the modern M1 and M2 series that he would retract within three years.

He stated: “Few if any monetarists ever recommended the use of such broad aggregates as the current M2 or M3 as monetary targets—certainly, this one did not.”[352] This was not an accurate claim: as well as using M2 in his 1981 *Newsweek* analyses, he had advocated M2 targeting in his July 1981 lecture on monetary policy.[353] The element of validity in his statement was that the Federal Reserve’s widening in the definition of M2 in 1979–1980 had not only incorporated thrift accounts in the definition (a change Friedman supported) but also had included some wholesale items thought at the time as being closely associated with nonfinancial economic activity. These wholesale items, however, were not a dominant source of M2 variations and were removed from the aggregate in successive redefinitions starting in 1982.

Yet just as he saw M1 as having some characteristics of the old M1, Friedman was convinced it was like old M2, too. He claimed at the December 1983 presentation: “The current M1 is conceptually… closer to the aggregate we [Friedman and Schwartz] labeled M2 rather than to our M1…”[354] This claim was inaccurate. Empirically, Federal Reserve Board data showed that old M2 and new M2 were more correlated than old M2 and new M1 before 1979.[355] Friedman had himself observed that the old and new M2 series had very similar period-by-period rates of growth (*Newsweek*, June 15, 1981).[356] Conceptually, too, the new M1 had a composition distinct from the old M2. The new M2 was a clearer descendant to the old M2, as will now be detailed.

As a basis for now favoring M1, Friedman claimed that the new M1 was similar to the old M2

[352] Friedman (1984c, p. 398).
[353] Friedman (1982c, p. 117).
[354] Friedman (1984c, p. 398).
[355] Correlations computed using the data for 1973 Q1–1979 Q4 tabulated in Simpson (1980, p. 112) indicated that quarterly growth in the old M2 series had a correlation coefficient of 0.54 with the new M1 growth series and 0.67 with growth in new M2.
[356] Friedman (1984c, p. 398) suggested that the dollar amounts of the levels of old M2 and new M1 might be similar. (He similarly asserted in mid-1984 that “our present M1… is essentially equal to the earlier M2”—see House Republican Research Committee, 1984, p. 44.) This comparison of dollar totals was not meaningful, however: the 1980 redefinitions of money had, in line with the Bach Committee recommendations, brought thrift accounts into M1 and M2—so the new M1 was necessarily going to be larger in dollar amounts than its old counterpart. The fact that the new series may have been roughly the same dollar amount as the old M2 did not establish that the new M1 and old M2 series were conceptually alike. And, of course, it did not imply that their past one-quarter or four-quarter growth rates had been similar.

because the new M1 series included interest-bearing deposits.[357] But this claim did not really hold up. The redefinition of M1 in 1980 had not brought into the aggregate the interest-bearing bank deposits that had been in old M2. It had brought in some low-interest-bearing thrift accounts into M1, and the series also included some bank-issued checking deposits that, as NOW accounts, could bear interest in the early 1980s. But although Paul Volcker stated in mid-1983 that "NOW accounts… perform both a savings and [a] transactions function," this was really just a statement that they were interest-bearing deposits.[358] The most typical situation in the 1980s, and later, was that NOW-ty[e accounts were not paying interest rates that in themselves could bid away funds from market instruments or that matched rates available on the non-M1 balances included in M2.[359] The payment of interest on M1 balances made them more attractive on the margin than otherwise but, unlike some M2 accounts, interest-bearing M1 deposits were not a front-rank competitor with marketable instruments as a savings vehicle.

Friedman nevertheless seems to have had the contrary impression. In particular, in June and July 1984, he indicated that he now attributed a good part of the 1983 velocity decline to the introduction of Super NOW accounts. This attribution was likely unwarranted. For, as stressed above, it continued to be the case that non-M1 M2 funds—and not deposits within M1—really corresponded to market-related interest-bearing deposit instruments. M1 remained dissimilar to M2 (indeed, to both the old and modern versions of M2) on this dimension.

Friedman should have viewed the new M1 as much like the old M1, not like old M2. Because he did not do so, not only in his December 1983 remarks but also for the whole of the period from early 1983 to early 1986 Friedman underestimated the degree to which high M1 growth reflected the process of recovery of real balances after a disinflation. He was a longtime exponent of this process, and, as already indicated, during 1983 he acknowledged it as a key factor driving the rise in M1 velocity through 1981 and in reducing the level of velocity in 1982−1983. But he got the quantitative importance of this factor wrong—greatly underestimating the extent of the increase in real M1 demand.

Friedman's understatement of disinflation's role in M1 velocity's behavior resulted from his

[357] Friedman (1984c, p. 398).

[358] From Volcker's written answer (number 3) in connection with his testimony of July 20, 1983, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1983c, p. 233).

[359] At the end of 1981 (the year in which nationwide NOW accounts were introduced), the own rate on M1 balances averaged only 100 basis points (Hoffman and Rasche, 1991, p. 669). In contrast, in December 1981 the own rate on M2 balances was 7.7 percent (see *https://fred.stlouisfed.org/series/M2OWN*).

viewing the demand for M1 in terms more appropriate for the analysis of M2.[360] In his quantitative assessment of the 1982−1983 decline, he evidently viewed M1, the real demand for which had a high interest elasticity (around 0.50: see Hoffman and Rasche, 1991), as similar to old M2, whose demand function was less interest-inelastic (which he had put at perhaps 0.10 or 0.15, after allowing for the own rate on M2). Consequently, Friedman underestimated the impact that the shift to lower nominal interest rates from 1982 had in reviving the demand for real balances (and probably overemphasized other factors, such as higher uncertainty, as factors driving velocity).

Taking stock, 1985 to 1986

Friedman was still using M1 as his measure of money in the early months of 1986 (*Newsweek*, March 10, 1986).[361] In May 1986, however, Friedman undertook a fundamental review of the monetary data in light of developments in recent years, including his own poor forecasting record on inflation. He settled on M2 as the appropriate money series for U.S. monetary analysis. [362] In doing so, he made a one-time adjustment to the series that recognized the shift up to a new level (a shift taken as, essentially, a rise in the intercept of the money demand function) associated with the introduction of money market deposit accounts (MMDAs).[363] The adjustment implied a lowering of the M2 growth rate in the four months to April 1983, with a conforming reduction in M2 growth in the first two quarters of 1983, as well as slightly higher third-quarter growth.[364]

One of Friedman's first public affirmations of his change in position, in favor of M2 over M1, was in a June 1986 talk in San Francisco in which Friedman stated that M2 "is a better indicator

[360] He was not alone in being under this misapprehension. Notably, Darby, Mascaro, and Marlow (1989, p. 555) incorrectly claimed that "current M1 (or M1B) is defined much like the 'old' M2."
[361] During the period in which he was focused on M1, and notwithstanding his occasional outright dismissals of M2, Friedman did generate statistics on M2 growth variability (as well as M1 growth variability) in correspondence with Michael Bordo and Anna Schwartz (see Bordo and Schwartz, 1983, p. 76) and he also discussed Gordon's (1982) historical work using M2 in Friedman (1984b, pp. 33–34).
[362] See Friedman (1988a, pp. 229, 239–240). Friedman confirmed his move back to M2 in a letter to Oppenheimer and Company's Charles Brunie and Rudolf Hauser dated May 11, 1986.
[363] Again, see Friedman (1988a, pp. 229, 239–240). Friedman was not challenging the legitimacy of including MMDAs in M2. MMDAs were retail deposits, issued by commercial banks and thrift institutions, that offered some checking options and that also involved no restrictions on transferability (see Goldfeld and Sichel, 1990, p. 314). They were, accordingly, a logical item to include in M2. The basis for Friedman's adjustment of the M2 series for the introduction of MMDAs was to allow for the fact noted above that, on being introduced, MMDAs attracted some funds that had previously been outside M2. See Friedman (1988a, p. 239).
[364] This adjustment improved the properties of estimated money demand functions and strengthened the M2-growth/inflation relationship. The same adjustment did not unambiguously improve the correlation between M2 growth and output growth, as it reduced the degree to which a rapid rise in M2 predicted 1983's economic recovery.

of the stance of monetary policy than M1 [is] at the moment."[365] This became the position that he stuck to.

In recalling this change five-and-a-half years later, Milton Friedman observed to the present author (interview, January 22, 1992): "I've gone back to using M2 and only M2. I slid away from the true and narrow path in the 1980s—and was sorry for it!… I think those predictions in the '80s *were* bad, but I think [that], on the whole, my *Newsweek* predictions [dating back to 1966] stand up pretty well."

If, as Friedman thus concluded, monetary policy developments in the early 1980s should have been analyzed using M2 rather than M1, how does the picture of this period change? The basic assessment that monetary policy's variability increased in the early 1980s is supported if series other than M1 are consulted. Friedman's tabulation in a 1984 article showed that the ups and downs of M1 and M2 growth over 1979–1983 were basically similar, even though those for M1 were more dramatic.[366] Consistent with this, Friedman continued to complain about the Volcker record of monetary variability even after his switch back to M2-based analysis.[367]

There are, however, some differences in an account of 1979–1982 that replaces M2 with M1 as the key monetary aggregate. As has been discussed above, M1 growth unambiguously declined in the period from late 1979 to mid-1982. In contrast, growth in M2 was, as Friedman noted in 1981(see the previous chapter), roughly horizontal over 1978–1981 in its annual-average growth rates (see the previous chapter), at about 8.5 percent, and continued at around this rate in 1982. Seen from the perspective of M2 growth, as opposed to M1 growth, the 1979–1982 period takes a somewhat different complexion. Federal Reserve actions in 1979 to 1982 can be viewed as instituting a monetary policy sufficiently tight to maintain the step-down in M2 growth achieved in 1977–1978—frustrating or offsetting some of the strong upward pressure on the nominal quantity of money demanded that was being generated by high rates of nominal spending growth in 1979–1981 and by the increasing attractiveness (thanks to rising own-yields) of assets within M2.[368] Inflation and prior M2 growth remained highly correlated, as emphasized by Leeper and

[365] Friedman (1986b, p. 245). Friedman also indicated his preference for M2, with an adjustment for MMDAs, in *New York Times*, July 3, 1986, p. D7.
[366] See Friedman (1984b, p. 29). This was consistent with the fact that Friedman was emphasizing monetary-growth variability under Volcker even in the period in 1981 that he was using M2 (*Wall Street Journal*, July 30, 1981).
[367] See, for example, Friedman (1986b, p. 246; 1988c, p. 380) and Darby and others (1987, p. 24).
[368] This interpretation is a mirror image of viewing the early-1980s monetary tightness as comprising a squeeze on real M2 balances.

Roush (2003), Nelson (1998, 2003), and Friedman himself (*Wall Street Journal*, July 5, 1989).[369] As shown in Figure 2, however, the decline in inflation during the 1980s was spread over several years (not just the two years associated with Friedman's point estimate of the money-to-inflation lag) after the major decline in M2 growth had been completed.

On the criterion of being positively correlated with fluctuations in real and nominal income, M2 growth performed reasonably well in the 1980s. As already noted repeatedly, it did less well than M1 in registering tightness in the early 1980s—giving a less clear picture of the squeeze on money associated with the 1980 credit-controls episode and of the 1981 policy tightening. Even after moving back to M2, Friedman acknowledged that the short-run relationship between nominal M2 growth and real economic activity was poor from 1980 to 1985 (*Wall Street Journal*, October 23, 1992). As already noted, M2 fared better when these years were considered as a squeeze on real money balances, and so monetary tightness was seen as keeping real M2 growth low. In this vein, Whitesell (1997, Chart 1, p. 5) showed that the match between real GDP growth and a two-year average of real M2 growth was impressive on annual data from 1961 to 1992, and Friedman himself (in Taylor, 2001, Figure 1, p. 103) showed that on quarterly data for the same period the connection between four-quarter changes in real M2 and real GDP.

Nevertheless, M2 growth, both real and nominal, remained significantly correlated with output growth on data that included the whole of the 1980s (Plosser, 1991, Table 5–2, pp. 263–264). Indeed, in a close look at the 1980s data, Feldstein and Elmendorf (1989) documented the explanatory power possessed by M2 growth in reduced-form equations when accounting for the economic recovery in 1983 and 1984. Partly as a result of this research, Martin Feldstein became a major advocate of the M2 aggregate during the late 1980s and through the mid-1990s.

[369] On sample periods through the early twenty-first century, these correlations tended to peak when monetary growth led inflation by one to three years. This was, of course, in keeping with Friedman's earlier generalizations about the bivariate relationship between the two series.

In practice, the long lag also bears importantly on the relationship between M2 growth and inflation in multivariate settings. Notably, Benjamin Friedman (1997, p. 160) contended that "M2 had never shown any relationship to prices anyway"—his basis for stating this being the fact that, in a regression of GDP deflator inflation on four lags of itself, four lags of M2 growth, four lags of real GDP growth, and four lags of the first difference of the federal funds rate, the four lags of M2 growth were never jointly significant in quarterly regressions whose sample period started in the early 1960s (pp. 146, 149, 159). If one estimates this specification for 1961:Q1–1994:Q4 (though with inflation, real GDP growth, and M2 growth expressed in percentage-change form, as they are in the rest of this chapter, rather than in the log-difference form that Benjamin Friedman used), lags 1–4 of M2 growth are indeed jointly insignificant, the *F*-statistic for their exclusion having a *p*-value of 0.70. But things change if one replaces lags 1-4 of M2 growth with lags 5–8 and otherwise keeps the same specification as before. In that case, the *p*-value for excluding the lags of monetary growth is 0.02. (With Milton Friedman's, 1988a, adjustment to the level of the M2 series in 1983 in recognition of the introduction of MMDAs, the *p*-value associated with excluding these lags of M2 growth is 0.008.)

He was impressed by the strength of the relationship between the growth rates of nominal income and M2 (*Wall Street Journal*, June 10, 1991) as well as between the levels of the two series (see Feldstein and Stock, 1994). This focus on monetary relationships was something of a divergence from much of Feldstein's earlier research, but it reflected his service on the Council of Economic Advisers from 1982 to 1984. "One thing that happens if you go to the CEA [is] you get educated about everything. So (*laughs*), before I went there, I didn't know very much about current imbalances, exchange rates, [and] monetary aggregates. [Then] I had breakfast with Paul Volcker every week that I was there. That was a good way of thinking about all of these issues." (Martin Feldstein, interview, November 21, 2013.)

One key embodiment of the durability of the M2/economic activity relationship was that the ratio of the level of nominal income to M2 had been roughly trendless since the mid-1950s and remained so in the 1980s.[370] M2 velocity had nevertheless exhibited sustained movements over shorter periods, rising in the 1970s and falling over 1981 and 1982 (with a further shift down as money market deposit accounts were introduced at the end of 1982). In line with his 1979 discussion of why inflation had risen more steeply than M2 growth, Friedman attributed the high monetary growth, in relation to inflation, to the U.S. private sector's flight back into money once they were confident inflation had peaked (*Wall Street Journal*, February 12, 1987). This was not inconsistent with velocity being stationary—as it was until about 1992—because the opportunity-cost variable mattering for real M2 balances was the market yield minus the yield on M2 deposits. This left open the possibility that substantial declines in market nominal interest rates, such as those in 1982–1983 and 1985–1986, might have a major downward effect on M2 velocity in the short run but much less over longer periods, when the own rate on M2 largely caught up with the market rate.

Studies by Friedman and others found that M2 velocity behavior in the postwar period could be accounted for by opportunity-cost variables—including the own rate—leading Friedman to conclude toward the end of the decade that velocity behavior in the 1980s was explicable "despite all the talk about how the relation between money and other variables has shifted drastically in recent years."[371] As Figure 3*b* shows, M2 velocity settled down after 1983—by which time many of the larger opportunity-cost movements had taken place.[372] Friedman was consequently able to observe in June 1988 that, on average from the first quarter of 1983 to the

[370] See Chapters 16 and 18 for discussions of M2 growth and M2 velocity behavior after 1986.

[371] Friedman (1988a, p. 229).

[372] See Small and Porter (1989, pp. 245–246). MMDAs' introduction was one of the recent sources of large movements in the own rate.

first quarter 1988, nominal GNP had grown 7.4 percent per year and M2, 7.5 percent: "How much closer can you get for a five-year period?," he asked (*Wall Street Journal*, June 22, 1988).

Friedman therefore from mid-1986 onward had the same perspective with which he had started the 1980s: following an M2-type monetary aggregate and emphasizing the resilience of its relationship with nominal income. In the interim, however, his monetary analysis had been shot through with problems, and he had accumulated a public record of statements strewn with inconsistencies and poor, and often alarmist, forecasts. By the time Friedman had settled on an account of developments in the 1980s with which he would stick, the M1-centered dire predictions made by himself and other monetarists from 1983 to 1986 had badly hurt both his reputation and that of monetarism.

Animosity toward the Volcker Federal Reserve

When a U.S. senator referred to Friedman's critique of monetary-growth variability in his "yo-yo economy" *Newsweek* column, and asked whether he wanted to see the column, Volcker responded nonchalantly, "Not really."[373] Volcker, under fire from multiple directions at this point, could afford to be circumspect about Friedman's criticisms. Friedman, however, became more strident in his criticisms of the Federal Reserve chair.

In mid-1982, Friedman contended about his stand on Federal Reserve policy since October 1979, "My criticism is not either hindsight or sour grapes." (*National Revie*w, July 23, 1982.) But over the following year, as Volcker's public standing rose as interest rates fell and the economy recovered, and the course of events was perceived as a rebuff to monetarism, one would be forgiven for thinking that there *was* an element of sour grapes on Friedman's part in his attitude toward Volcker. Although Greenspan (2007, p. 478) may have been right to say that Friedman was less critical of post-1979 than pre-1979 monetary policy, most of Friedman's articulations of a less-critical posture came out only from 1986 onward. As has been documented extensively above, Friedman's running commentary on the Volcker Federal Reserve was typically unfavorable—and perhaps never more so than in the post-New Operating Procedures year of 1983. He viewed the Federal Reserve under Volcker as having repeatedly defied his public advice and as having done so even more after 1982. Yet Friedman often found himself being blamed for a variety of undesirable aspects of the economic and financial picture in the first half of the 1980s.

[373] In Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1982a, p. 17).

Friedman protested that his antipathy toward current monetary policy was not personal: “I don’t criticize Paul Volcker. He’s a very intelligent, able person,” he remarked (*Washington Times* (Washington, D.C.), May 2, 1983, p. 8B). He did indeed refrain on occasion from criticizing Volcker by name. For example, on October 11, 1983, speaking to the American Bankers Association conference in Honolulu on the day after Volcker had spoken at the event, Friedman complained about the fluctuations in monetary growth seen since the late 1970s but did not mention Volcker by name (*Atlanta Journal*, October 12, 1983). He had remarked a week earlier that he did not like “to talk about the Volcker policy,” instead of the Federal Reserve policy.[374] Similarly, Friedman wrote a couple of times in 1983 that “the problem is not the person who happens to be chairman, but the system.”[375] But even these commentaries had direct criticism of Volcker—taking him to task for giving off-the-record briefings to the media, likening his monetary-targeting record to that of a bad baseball player, and suggesting that he was blocking desirable technical improvements to monetary control.[376]

And a factor Friedman gave for allegedly not holding a grudge against Volcker—“The Federal Reserve has behaved very much the same way regardless of who’s been chairman” (*Washington Times* (Washington, D.C.), May 2, 1983)—was itself unflattering, as it carried the implication that Volcker had not really broken away from his 1970s predecessors’ policies.

Friedman took the opportunity to criticize Volcker in person at the PEPAB meeting of April 20, 1983—which, as already noted, was one that the Federal Reserve chair attended at Reagan’s invitation. On this occasion, Friedman stood up and pointed at Volcker, declaring, “Because of the policies of that man, we've had an inflationary surge in the money supply which is going to have to be corrected.” He added—drawing on the recession scenario to which he gave such weight during 1983 and early 1984—that the Federal Reserve’s record suggested that it “probably overcorrect,” in which case, he said of Volcker, “He will cause interest rates to go sky-high, and undermine the recovery in 1984.”[377]

His anger at Volcker stemmed not only from the recent surge in monetary growth but also the whole conduct of monetary policy in the decade to date. Friedman had already expressed

[374] Friedman (1984f, p. 41). See also Friedman’s similar observation, made in New York City in late April 1984, in Friedman (1984j, p. 4).

[375] *Newsweek*, May 2, 1983, and Friedman (1984b, p. 54).

[376] See *Newsweek*, May 2, 1983, and Friedman (1984b, pp. 53, 55–56).

[377] The exchange was quoted in *New York* magazine, May 16, 1983. This Friedman quotation was subsequently also used in *Newsweek,* June 20, 1983 (p. 53), and that article was cited as the source for the quotation in Silber (2012, p. 231) (whose version is, however, a slight misquotation of the Friedman remark).

concern that monetarism has "become discredited" by what he considered the highly nonmonetarist policy approach followed by the authorities in 1979–1982 (*Barron's*, October 25, 1982, p. 7). He confirmed in an interview given in the summer of 1983 that the public reputation of monetarism had been hurt by the experience of U.S. economic policy since 1979 (Heertje, 1984, p. 45). Better economic news since 1982 had not restored monetarism's standing because it had coincided with what the media, and to some extent the authorities, had cast as a move away from reliance on monetarist ideas. The title that Friedman gave to an article he wrote in early 1985—"How To Give Monetarism A Bad Name," a title he originally wanted to use in 1983—summarized his discontent with the Volcker Federal Reserve.[378]

For Friedman, the bad name was unjustified: lower monetary growth had delivered reduced inflation as promised, while aspects of the 1979–1982 experience that were cited as discrediting monetarism either were items he had been open about from the start (such as the need for a temporary decline in output) or were features (volatile monetary growth and interest rates, and the severity of the recession) that he regarded as resulting from the Federal Reserve's deviation from monetarist prescriptions. Friedman's resentment was compounded by the fact that he personally was blamed in some quarters for the early 1980s recession. For example, a *Washington Post* article in 1983 was titled "We Had to Kill the Economy to Save It? That's Not What Milton Friedman Says, But That's What His Policies Did" (*Washington Post*, May 29, 1983)—although, ironically, in the quarter in which this particular article appeared, U.S. real output moved above its pre-recession peak and registered a new historical high.[379]

This background made Friedman hostile to the idea of a second term for Paul Volcker, whose first term was due to expire in late July 1983. Alan Greenspan was mooted in the press as a possible replacement (for example, *Financial Times* (London), March 21, 1983) and he became "the most frequently mentioned candidate" (*Time*, April 25, 1983, p. 97). Friedman had already spoken warmly of Greenspan as a successor to Volcker (*Boston Globe*, April 1, 1981, p. 45).

Friedman was also outspoken in opposing Volcker's reappointment. In this connection, he was quoted giving his standard complaint about the chair's tenure: "There has been more volatility in

[378] This paper appeared in 1985 (see Friedman, 1985b). But Friedman had originally proposed this title when he was delivering a talk at the August 1983 Mont Pelerin meeting in Vancouver and was dissatisfied with the title assigned to the talk, "What Could Reasonably Have Been Expected from Monetarism: The United States." See Friedman (1983i, p. 21). The Mont Pelerin talk (which essentially appeared in print in revised form in various other Friedman writings) was cited by Hafer (1985b, p. 130).

[379] That is, in the final data. In the initial data, real output (real GNP) passed the pre-recession peak only in 1983:Q3. See *The Economist* (London), October 1, 1983 (p. 18).

the money supply in the past three years than in any previous three-year period." (*Time*, June 20, 1983.) A week later, after Reagan had announced his renomination of Volcker, Friedman was quoted as saying (*Time*, June 27, 1983, p. 16) saying, "President Reagan has decided to take the course of least political resistance." In remarks that Friedman made on October 4, he stressed that his own inclination—shaped partly by his continuing high regard for public-choice theory (see the next chapter)—was to discount the importance of the identity of the Federal Reserve's head. He nevertheless reaffirmed his disapproval of the Volcker reappointment, which had been confirmed by a Senate vote in July. "His reappointment is a clear indication that the Reagan Administration approved [of] what I believe to have been a very unfortunate monetary policy."[380]

Reagan's own explanation for the decision, given in his radio address of June 18, 1983, had indeed made it very clear that he did not share Friedman's assessment of Volcker's record: "Paul Volcker… is as dedicated as I am to continuing the fight against inflation, and with him as chairman of the Fed, I know we'll win that fight."[381]

Reagan's judgment amounted to a rejection of the pessimistic inflation forecasts that Friedman was giving by around the time of Volcker's renomination. The president's decision also bucked what he knew were Friedman's wishes. After Friedman had blasted Volcker in person in Reagan's presence at the April PEPAB meeting, it had been remarked: "Certainly, Friedman's attack does not help his [Volcker's] chances [of renomination]." (*New York* magazine, April 16, 1983.) But the reappointment that followed underlined Friedman's diminishing clout.[382]

Even before this diminution, there was no authentic chance of Friedman himself getting the Federal Reserve position. As the nomination decision approached, Friedman laughed when a reporter asked him how he would respond if he was asked to succeed Volcker. "Fortunately, that request will never come, so I don't have to face the issue." (*Washington Times* (Washington, D.C.), May 2, 1983, p. 6B.) After Volcker's renomination was announced, Friedman indicated, in relation to the hypothetical opportunity to be Federal Reserve chair, that he had "no desire whatever to leave San Francisco," but that it continued to be the case that "the job of Federal

[380] Friedman (1984f, p. 41).

[381] Reagan (1983a).

[382] The fact of the reappointment at least meant that Friedman's (1983h, p. 339) reference to Volcker as the current Federal Reserve chair was not out of date when this article appeared in the *Journal of Money, Credit and Banking* in August 1983. In line with the feelings that Friedman harbored toward Volcker by this stage, the article ended with a call for Volcker to issue a public correction of what Friedman considered erroneous public statements that the chair had made regarding various nations' performance with regard to monetary-growth volatility (see Friedman, 1983h, p. 343).

Reserve chairman is the only job in Washington which I would have any interest in accepting." (*Newsweek*, June 27, 1983*b*.)

The April 1983 PEPAB meeting aside, Friedman rarely saw Volcker from 1982 to 1986. Both did attend the American Economic Association meetings held in San Francisco in December 1983. Michael Keran recalled an encounter between them at an event in San Francisco: "One of my images in my head, is one time… there's a six-foot-seven Paul Volcker, and—I don't know—[an about] five-feet Milton Friedman standing. And Milton has got his finger in the air, and his head is up, and he's shaking his finger at Paul Volcker, and Paul Volcker's looking down. And what an image that was, I'll never forget it." (Michael Keran, March 7, 2013.)[383] Volcker, used to being berated by Friedman, was not perturbed. However, other Federal Reserve senior staff who had known Friedman for years noted a sharper edge in their exchanges with him during these years.

Notwithstanding clashes like those in the February 1982 issue of the *JMCB*, Friedman had usually implied that the Federal Reserve economist staff, including those at the Federal Reserve Board, had a much greater affinity with his own views on monetary control and policy rules than the policymakers did.[384] So, for example, although his former Ph.D. student David Lindsey had frequently defended the Federal Reserve against monetarist criticisms, Friedman emphasized the fact that Lindsey had supported the reintroduction of contemporaneous reserve accounting.[385] After CRA was approved by the Board of Governors in the middle of 1982, Friedman wrote Lindsey a letter of congratulations.[386] Indeed, Lindsey was involved in copious correspondence with Friedman in 1982 and 1983 on data and policy matters. They also appeared together in a July 1986 panel, held in San Francisco, on velocity's behavior.[387] But Lindsey, a strong defender

[383] Although this exchange could have been at the 1983 American Economic Association meetings, Keran's recollection was that it was at a Federal Reserve Bank of San Francisco conference. Keran, who left the bank in 1984, may have been remembering the March 1983 opening of the new Federal Reserve Bank of San Francisco headquarters—an event that Volcker attended (see Balles, 1983a, pp. 1–2). On the matter of Friedman's height, Friedman himself gave it as five-feet-two (see Friedman and Friedman, 1998, p. 414).

[384] See, for example, Friedman (1982a, p. 59), *Wall Street Journal*, February 1, 1982, and *Newsweek*, December 27, 1982.

[385] For Lindsey's public support of CRA, in a paper originally drafted before the June 1982 CRA decision (being delivered, as noted above, at an October 1981 conference), see Lindsey (1983, p. 26). Lindsey and his coauthors had also remarked on the destabilization of reserve multipliers in the official Federal Reserve Board staff studies of monetary control (Lindsey and others, 1981), in a passage (p. 52) that would be quoted favorably by both Friedman (1982c, p. 112) and Brunner and Meltzer (1983a, p. 71). (Friedman, 1982c, p. 112, pointed to a later passage as well that referred favorably to "reinstitution of contemporaneous reserve accounting." The sentence Friedman quoted appeared on pages 55–56 of Lindsey and others, 1981, not the page 53 that he referenced for it.)

[386] Letter (of July 22, 1982) from Milton Friedman to David Lindsey, Federal Reserve Board records.

[387] See Darby and others (1987).

of the Volcker record, noticed that Friedman's remarks in their interactions were more cutting than before and were bordering on discourteous. Their correspondence had tailed off well before the 1986 panel. One of their in-person encounters, probably at the 1986 panel, involved Friedman joking at Lindsey's expense. "So that pissed me off and I sort of broke off [contact] with him." This stayed the case almost "until the very end, when [in November 2002] I went to the ceremony at the University of Chicago at which Bernanke gave that talk saying, 'We're sorry, but we won't do it again.'" Lindsey, who was among the former Friedman Ph.D. students invited to attend this ninetieth-birthday celebration, approached Friedman at the event. "And I went up and shook his hand and everything, and I distinctly remember him saying 'Oh, you stopped writing to me,' or 'You haven't written to me for a long time.'" (David Lindsey, interview, May 2, 2013.)

A still more senior Board figure, Stephen Axilrod, who with his wife, Kathy, saw Friedman at the June 1983 Tokyo conference. The Axilrods, who had first met Friedman when they were University of Chicago students over thirty years earlier, were struck by Friedman's more-caustic-than-usual demeanor at the Bank of Japan event. The 1982 change in operating procedures and the 1982–1983 increase in M1 growth, coming on top of Friedman's earlier disappointments with the October 1979 arrangements, had created a highly charged atmosphere in which Friedman felt the Federal Reserve had let him down and in which Federal Reserve officials believed that Friedman's recent analysis was seriously flawed. The Axilrods found Friedman—whose mood at the conference was probably not improved by the fact that Rose Friedman had not joined him on this trip—to be in notably belligerent and acerbic form, including toward them (Kathy Axilrod, interview, April 25, 2013).[388]

The poisonous relations between monetarists and senior Federal Reserve officials at this time were also brought out in an exchange in which Friedman did not participate. A harshly-worded Brunner-Meltzer Carnegie-Rochester conference paper, published in 1983 and titled "Reply to Stephen Axilrod's Comments," referred to "the careless and incompetent treatment of the strategy problem in the studies published by the [Federal Reserve] Board."[389]

And although himself an academic, and not a central bank official, Guillermo Calvo incurred Friedman's ire on the matter of U.S. monetary policy when he was Friedman's discussant at the August 1983 Mont Pelerin Society meeting in Vancouver. Calvo defended the Federal Reserve's

[388] On Friedman's 1983 visit to Japan being one he made on his own, see Friedman and Friedman (1998, p. 632).
[389] Brunner and Meltzer (1983b, p. 115).

conduct against Friedman's critique, which had focused on the harm done in permitting quarter-to-quarter variability in monetary growth. "I was asked to lead the discussion. And I was critical of what he was doing …I was trying to argue that volatility of money supply was not as disruptive as he claimed. And he was furious." (Guillermo Calvo, interview, April 1, 2014.)

MONETARY POLICY CREDIBILITY AND THE TERM STRUCTURE OF INTEREST RATES

Starting with early research on the period like Blanchard (1984) and Benjamin Friedman (1984), and continuing with contributions such as Erceg and Levin (2003) and Goodfriend and King (2005), a large literature has characterized the Volcker Federal Reserve tightening that began in 1979 as one that lacked credibility, in the sense that the private sector's expectations did not instantaneously fall into alignment with the new, lower steady-state inflation rate implied by the post-1979 monetary policy regime.

Friedman's own interpretation of the New Operating Procedures years agreed only partially with the message of the subsequent credibility literature. He shared the view that longer-term inflation expectations were not brought down promptly by the 1979 changes. But he regarded it as unrealistic, in any event, to have expected that U.S. inflation could have been brought down without a recession.

Credibility and the real costs of a disinflation

More specifically, Friedman, too, believed that the 1979−1982 tightening lacked credibility: he himself referred to the "lack of confidence in monetary policy" prevailing during the period.[390] This position lined him up with the later credibility literature.

But he did not subscribe to the position—conveyed by a good deal of the later literature on the Volcker disinflation—that the opportunity was available to the Federal Reserve, if it had possessed credibility from the outset, of bringing inflation down rapidly to price-stability levels and of largely bypassing the real-output costs that were associated in practice with the post-1979 disinflation. Those subscribing to this view tended to argue that the same reduction in inflation achieved by Volcker in the 1980s could have been achieved with much lower, or negligible, short-run output costs under a more-credible policy. In contrast, Friedman, for all his own criticisms of the execution of monetary policy from 1979 to 1982, did *not* take the view that a

[390] Friedman (1985b, p. 59).

disinflation that had minimal repercussions for real variables was ever something that was within the Federal Reserve's grasp.

The expectational Phillips-curve framework that Friedman helped found certainly implied that a speedier disinflation than otherwise, and one involving smaller short-run losses in output and employment, was obtainable if inflation expectations rapidly adjusted to the monetary policy environment. But Friedman thought that it was not realistic to view a costless, or nearly costless, U.S. disinflation as feasible even if the Federal Reserve's policy was widely believed as disinflationary from day one.

Friedman's own view, already discussed above, was that a more orderly and gradual monetary restriction would have been associated with a shallower recession, but that the cost of this more-gradual policy would have been a *slower decline in inflation*. He believed that the output decline associated with the less-gradual policy that was actually followed by Volcker had led to a more rapid disinflation than otherwise.[391] For Friedman, the feasible, and more desirable, alternative policy was not one in which inflation fell as much as, or more quickly than, it did in 1979−1982, with little in the way of a negative output gap. Rather, he saw the alternative policy that had been available, but was forgone, as one involving a slower decline in inflation with a more protracted, but shallower, negative output gap than that observed.

Months ahead of Volcker's move, Friedman indicated that the credibility of a policy of concerted monetary restriction would not be acquired immediately. Rather, it would take time as "people gained confidence that these policies were going to be carried through" (*San Jose Mercury News* (California), February 12, 1979). He further believed that, in October 1979, there had not initially been wide confidence that monetary growth would be reduced.[392] He also made no bones about his feeling that a monetary tightening would have produced a recession even had it been accompanied by considerable credibility.[393]

The economic-research literature has cited various factors as preventing a credible monetary policy restriction from being costless in its real effects, even in a forward-looking model. These have included the presence of lagged expectations in price contracts (Koenig, 1996; Mankiw and Reis, 2002) and of lagged inflation in the Phillips-curve equation (for example, Fuhrer and Moore, 1995a). Friedman's view of price dynamics can be seen as incorporating both of these

[391] See *Newsweek*, July 25, 1983, and the discussion above.
[392] See Friedman (1984c, p. 397).
[393] See especially the discussion in Chapters 10 and 12 above.

elements (see Nelson, 2020a, Chapter 7).

Monetary variability and real interest rates

Friedman did give some weight to rapidly-updated expectations of inflation as a factor driving inflation, alongside slower-adjusting expectations. In light of this, he certainly regarded the fluctuations in U.S. monetary growth observed after 1979 as not helpful in facilitating a rapid adjustment of inflation expectations toward a stable-prices regime.[394] Specifically, the fact that wide movements in monetary growth occurred in 1979–1982 was something that he saw as reducing confidence that the Federal Reserve would achieve the year's monetary target and the associated multi-year reduction in monetary growth.[395] So, largely in line with the later credibility literature, he saw the disinflation as more costly than necessary on the ground it did not manage expectations as well as was feasible.[396]

But that was not his main reason for disliking the fluctuations in monetary growth seen under the New Operating Procedures. As he felt that it was inevitable that monetary policy actions would have short-run real effects, the harm that Friedman saw in short-run movements in monetary growth lay primarily in their implications for output variations. He viewed these monetary fluctuations as giving rise to sharper-than-necessary real output fluctuations: the ups and downs discussed at length above, rather than the extended, but less volatile and more phased-in, period of aggregate demand restriction that he favored.

Friedman also came to the conclusion that volatile monetary growth had made the 1981–1982 downturn worse. One of his justifications for believing that this was the case has already been discussed: his belief that the associated economic volatility led to an increase in the demand for money. Another justification was not really very well established by Friedman or others, but it was one in which Friedman came to believe strongly during the mid-1980s. This was that fluctuations in monetary growth were a factor pushing up the *average level* of real interest rates. "Abnormally high real interest rates are attributable to the volatility of monetary growth," Friedman declared (*Barron's*, October 25, 1982, p. 6).

[394] See Friedman's letter of August 16, 1982, to Senator Robert Jepsen (Friedman, 1982d, p. 74), as well as Friedman (1984c, p. 397).

[395] For example, in *Meet the Press*, NBC, March 21, 1982 (p. 8 of transcript), he remarked that "the real harm which these fluctuation is doing is that it destroys the credibility of the Fed's targets."

[396] One of the statements that Friedman made to this effect was the Federal Reserve "did it [achieved lower monetary growth] in such an erratic and inefficient way that you did not get the assistance of any confidence that you were really doing it—therefore, you went through a much more severe recession than was necessary and desirable." (*Boston Globe*, April 3, 1983, p. 44.)

The notion that swings in monetary growth were building some kind of premium into short-term interest rates had been advanced largely by monetarists other than Friedman until mid-1982.[397] It had been given public endorsement by the administration via Donald Regan's Congressional testimony of January 27, 1982, in which the Secretary of the Treasury remarked: "There is an unusual premium in interest rates right now. That premium is a volatility premium… It has been brought about [in] the last several years by the exact thing we were talking about… the volatility of the money supply."[398]

Friedman was a belated convert to this account. "I wasn't happy about that explanation for a long time, because I didn't have a theoretical explanation of why it should work that way," he noted. Nevertheless, he indicated that he was now more satisfied with the story: "I've thought about it a great deal, and I'm inclined to be a little happier with that explanation." (*Barron's*, October 25, 1982, p. 6.) He accordingly became a proponent of the position that high interest rates were in significant part attributable to "the very high volatility of monetary growth, under what I regard as disgraceful and inexcusable Federal Reserve mismanagement of the money supply" (United Press International, May 16, 1982; also quoted in *Houston Post*, May 17, 1982).

Friedman's own version of this account of early 1980s interest-rate behavior emphasized a considerable degree of market segmentation of U.S. securities markets of different maturities. Short- and long-term interest rates had gone up together in 1981, but, he argued, the conditions driving up rates in each market differed. Uncertainty about future inflation had limited the supply of funds available for long-term lending, pushing rates up in longer-term credit markets. In itself, this development would imply downward pressure on short-term interest rates, arising from the shift of the supply of credit to the short-term market. But this downward pressure, Friedman believed, had been swamped by factors tending to push up short-term interest rates. In particular, he suggested that numerous U.S. corporations had mistakenly taken mid-1980 as seeing the U.S. economy's low point and, on that assumption, they had given the go-ahead to longer-term projects to raise their capital stock. These firms then subsequently engaged in large-scale distress borrowing in the short-term market in order to meet their payment commitments after the monetary tightening of 1981 and associated renewed economic downturn. This distress

[397] For example, Anna Schwartz stated in April 1982: "The Federal Reserve System should move promptly to provide stable, predictable, and credible reductions in money growth along [a] preannounced path. By so doing, the Federal Reserve will lower the prevailing level of uncertainty in financial markets and reduce the risk premium in interest rates." (In Women's Economic Round Table, 1982.) This statement was similar to one that the Shadow Open Market Committee had issued in its March 1982 policy statement (Shadow Open Market Committee, 1982, p. 2).

[398] In Joint Economic Committee, U.S. Congress (1982d, p. 231).

borrowing had helped produce high levels of short-term interest rates.[399]

Friedman's story was certainly qualitatively plausible: James Tobin had noted in October 1981 that the monetary policy tightening that year had seen a major shift from long-term to short-term borrowing.[400] But Friedman's overenthusiasm for the story's quantitative importance was manifested in the outlandishly large estimates of the effect of monetary-growth volatility on real interest rates: in the summer of 1982, following the recent new spike in short-term interest rates, he suggested that this effect was perhaps 3 to 5 percentage points.[401] That is, with real interest rates currently about 6, 7, or 8 percent, he suggested that, absent monetary volatility, they might have been only 2 or 3 percent.[402]

Friedman was, admittedly, not claiming that monetary-growth volatility played a permanent or structural role in the determination of real interest rates.[403] Instead, he was suggesting that, because of surrounding circumstances, monetary variability had played an important part in the recent runup in rates. He posited that, even absent regime change, the effect might prove ephemeral: interest rates should decline as the distress borrowing receded and as the market acclimatized itself to post-1979 conditions (*Newsweek*, June 28, 1982). Indeed, he perceived the effect as already starting to wear off by the fall of 1982 (*Barron's*, October 25, 1982, p. 6).

It was nevertheless a rash conclusion, in light of the absence of a body of established research on the matter, to postulate such a large effect of monetary variability on interest rates. Friedman no doubt was attracted to the conclusion that this effect was important in part because it lined up with his longstanding belief in the harm caused by monetary-growth variability. The postulated effect on volatility on real interest rates also lent support to his advocacy of contemporaneous reserve requirements, as he had stressed the important role played by lagged reserve

[399] See Friedman's expositions of this story in his letter of August 16, 1982, to Senator Roger W. Jepsen (Friedman, 1982d, p. 74), *Barrons*, October 25, 1982 (p. 6), *Newsweek*, August 23, 1982, *Wall Street Week*, Maryland Public Television, April 27, 1984 (p. 7 of transcript), and Friedman (1985b, p. 59).

[400] See Tobin's prepared statement for the hearing of October 6, 1981, in Joint Economic Committee, U.S. Congress (1982a, p. 6).

[401] This estimate was given in *Newsweek*, August 23, 1982, in Friedman (1982d, p. 74; 1985b, p. 59), and in Friedman and Friedman (1984, p. 91; 1985, p. 91).

[402] Friedman may have regarded this large estimate as plausible in part because it was lower than others he had heard about. He had remarked (*The Detroit News*, March 21, 1982): "Some people believe [that] the recent volatility explains as much as 6 percentage points. Call it an 'uncertainty premium.' Six points is probably high, but there may be something to this." He would also likely have drawn comfort from being able, with the story that he converged on in the summer of 1982, to describe the reasons for higher interest rates in terms of demand and supply factors in the credit markets, rather than using the terminology of premiums.

[403] Indeed, Huizinga and Mishkin (1986, pp. 254–256) would find that monetary-growth variability did not help account for the behavior of the U.S. short-term real interest-rate over a long postwar sample period.

requirements in magnifying fluctuations in monetary growth since 1979.[404]

Donald Regan's endorsement notwithstanding, the monetary-volatility-based explanation regarding high real interest rates did not gain much support other than from monetarists. Even Samuel Brittan, frequently supportive of monetarist arguments, was derisive on the matter: "The technical monetarists make very critical remarks about Mr. Volcker, which to say the least are highly exaggerated… To blame the severity of the U.S. recession on quarter-to-quarter monetary deviations or the failure to abolish 'lagged reserve accounting' lacks all sense of proportion..." (*Financial Times* (London), March 21, 1983.) And in retrospect, it would appear that Friedman, despite his view that monetary policy was predominantly tight in 1981 and 1982, likely underestimated the extent to which tight money—that is, low rates of monetary growth—was a factor keeping up real interest rates in these years. So he could have, and probably should have, simply relied more on this standard channel (a liquidity effect) in understanding high real interest rates, rather than resorting to the more exotic and speculative argument that invoked monetary volatility. In addition, nonmonetary factors were likely important, as the long-run average value of the U.S. real short-term interest rate appears to have undergone a shift upward from the mid-1970s to the mid-1980s, separately from monetary policy and other cyclical influences on real interest rates (see Laubach and Williams, 2003, p. 1066).

U-turn risks, inflation expectations, and longer-term rates

With regard to longer-term interest rates, Friedman gave the same lower level—that is, lower by 3 to 5 percentage points—as having been achievable if "the Federal Reserve followed the kind of monetary policy it promised to follow when it made the changes in October 1979."[405] But his reasoning regarding why longer-term rates were elevated in the early to mid-1980s was not closely linked to his distress-borrowing story, which he applied specifically to short-term rates.[406] Instead, in order to explain high longer-term nominal interest rates in the early and mid-

[404] Friedman also stressed that Federal Reserve Board staff analysis supported the notion that lagged reserve accounting was contributing to variability in monetary growth. See Friedman (1982c, p. 112) and *Barrons*, October 25, 1982 (p. 7). This was also the view of numerous researchers in academia (see Friedman, 1982c, p. 111). In addition, Robert Rasche contended that "things such as lagged reserve accounting and so forth add to the interest-rate volatility" when reserves were used as a policy instrument (*Journal of Money, Credit and Banking*, 1982, p. 137)—a position supported by the formal analysis of McCallum and Hoehn (1983), who suggested that lagged reserve requirements tended to raise the variances of monetary growth and interest rates alike.

[405] Letter from Friedman to Senator Roger W. Jepsen, August 16, 1982, in Friedman (1982d, p. 74).

[406] In contrast, other monetarists were trying—more ambitiously, but without presenting a truly persuasive case—to explain *both* high short-term rates and high long-term rates in terms of monetary-growth volatility. For example, in a letter of August 17, 1982, to a Congressional inquiry, Anna Schwartz argued: "The financial markets have reacted to the wide swings in monetary growth by incorporating large risk premia in interest rates *at all maturities*."

1980s, Friedman relied primarily on the much more mundane—but much more well-established—Fisher relationship.

In fact, despite his 1982 embrace of the notion that monetary-growth volatility was raising short-term interest rates, Friedman continued to see some decline in short- and long-term interest rates from the levels prevailing in early 1982 as very likely—with further falls in the latter in prospect provided that markets accepted that inflation was going to be permanently lowered. When he communicated this assessment at the White House during the PEPAB meeting of March 1, 1982, the president's delighted reaction was described in these terms: "Reagan nearly hugged him." (*Time* magazine, March 15, 1982.)

As already indicated, the prevalence in U.S. discourse by 1982 of inflation expectations as a key driver of interest rates was a tribute to Friedman's influence, as he had been propounding the Fisher effect in his public-policy discussions, as well as in much research, for many years.[407] Paul Volcker's innumerable invocations of the Fisher effect over his whole tenure as Federal Reserve chair included his endorsement of it at his April 1982 panel appearance with Anna Schwartz: "Interest rates are not going to go down and stay down—and this is an old story, but I think it's right—… unless people have some degree of confidence in the dollar itself and in the future of prices." In an article published more than four years later—in June 1986, at a time when a number of the more specifically quantity-theory-based positions on monetary policy were under siege—Friedman could still express satisfaction at it remaining the case that "no one any longer disregards Fisher's distinction between nominal and real interest rates."[408]

Even though Friedman expected that it should be a downward force on rates across maturities in 1982, his judgment was that the Fisher mechanism had somewhat different implications for nominal interest rates by maturity in 1982. With regard to short-term interest rates, as Friedman had occasion to note explicitly in mid-decade, the lag in the effect of monetary policy actions on inflation meant that inflation and near-term expected inflation were largely unaffected by the

(Schwartz, 1982a, p. 144; emphasis added). Likewise, Allan Meltzer (*Wall Street Journal*, July 29, 1982) saw both short- and long-term interest rates as having been substantially boosted by persistent increases in risk premiums, induced by the New Operating Procedures regime. And, in the context of considering the 10-year Treasury security yield specifically, Otto Eckstein (1983b, p. 139) himself favored the "effect of the [federal] budget deficits on interest rates" in explaining "the extraordinary interest rate bulge" of 1981–1982 but noted that higher risk premiums arising out of the post-October 1979 arrangements had been proposed as another factor present.

[407] See Nelson (2020a, Chapter 6; 2020b, Chapter 12).

[408] Friedman (1986e, p. 644).

current year's Federal Reserve policy.[409] As a corollary, the lower inflation that he saw as coming in 1982 implied that the expected-inflation term—being largely locked in by past policy actions—should help drive short-term interest rates down. And, as was detailed earlier, Friedman's prediction of a major decline in short-term nominal interest rates during 1982 was largely realized—although, in practice, the unleashing of the downward pressures on rates was facilitated by considerable official prodding, in the form of expansionary operations in securities markets by the Federal Reserve.

With regard to longer-term interest rates, however, current monetary policy did play a major part in shaping the expected-inflation component. But so, too, did expected future monetary policy. And here a major theme articulated by Friedman and others in 1981 and 1982—an interpretation of rate behavior in large part shared by the later research literature on credibility—was the fear of a *U*-turn.

This interpretation of long-term interest rates' behavior was reinforced in the third quarter of 1981. Although 1979–1982 would become known as a period in which short- and long-term interest rates were particularly closely related, the period from July to September 1981 stood out as an exception to this pattern. In this period, the federal funds rate fell, but the long-term Treasury bond rate turned up (see Lindsey, 1983, pp. 8–9).

In the wake of this development, Aan Meltzer in testimony given in October 1981 pithily summarized the interpretation of U.S. long-term interest rates' level: "the principal reason they are high… is the fear that this administration [including the Federal Reserve] will throw in the sponge and reinflate."[410] As Friedman noted around the same time, this was how President Reagan had already interpreted high long-term rates (*Newsweek*, October 19, 1981).

This continued to be Reagan's interpretation—as was evident in remarks the president made at the end of March 1982: "Those interest rates are not staying up because of anything that the Fed is doing or anything that the government is doing. They are staying up because… the money markets just don't believe that we will stay the course, bring down government spending, and hold inflation down." (*Daily News* (New York), April 1, 1982, p. 44.)

[409] Friedman (1985c, p. 215; 1985e. p. 17). Paulus (1982, pp. 74–75) and Lindsey (1983, p. 10) had appropriately used the same reasoning to reject accounts of early-1980s U.S. short-term interest rate behavior that drew on a Fisher effect of current or very recent monetary policy moves. As discussed earlier, Friedman's own account of the relationship between monetary growth and short-term interest rates in 1979–1982 did not rely crucially on a Fisher-effect mechanism.

[410] From Meltzer's testimony of October 6, 1981, in Joint Economic Committee, U.S. Congress (1982a, p. 113).

Although Friedman's own casting of matters put the Federal Reserve in a less favorable light, he basically shared Reagan's *U*-turn interpretation of longer-term interest rates. In fact, while Friedman felt in 1981–1982 that the level of short-term interest rates demanded a special explanation, he defended current longer-term interest rate levels in expected-inflation terms. In March 1982, Friedman suggested that the ten-year bond rate of 15 percent was justifiable in terms of expectations of future inflation of about 12 percent plus a 3 percent real rate.[411] He saw the prospects for future monetary policy, and therefore longer-term inflation, in bimodal terms, with price stability being restored in one scenario and, in the other scenario, the authorities failing to hold firm on anti-inflation policies—in a manner that led inflation to exceed its earlier peak, perhaps generating the 20-percent-plus rates of price rise seen in past years in the United Kingdom.[412] Friedman believed that the market was giving a 50-percent weight to the second scenario—that in which the Federal Reserve would before long abandon monetary restraint precipitously. He thought that the 12 percent longer-term inflation rate embedded in longer-term rates represented the average of the price-stability and super-high-inflation scenarios. "I do not believe that there will be a sustained reduction in the long-term rate until the uncertainty about these two scenarios is resolved," he maintained.[413]

In fact, this was Friedman's greatest degree of overlap with the later "credibility" literature: He saw longer-term interest rates in the early 1980s as embedding an expectation of continued high longer-term inflation rates, and he viewed the New Operating Procedures regime as having failed—particularly, in Friedman's assessment, because of flaws in its setup and execution—to instill a climate that would have generated an early reduction in nominal longer-term rates.[414]

In mid-1983, with U.S. inflation now well down, Friedman noted that longer-term interest rates remained high "by comparison with past levels, primarily because of continued fears of future inflation." (*Newsweek*, June 13, 1983). But, in terms of his own evaluation of the course of monetary policy, matters had moved on. He no longer saw the inflation outlook as dominated by bimodal scenarios. Rather, he emphasized a central case consisting of his forecast of a reversal, in the period immediately ahead, of much of the progress recently made toward price stability: a

[411] Friedman (1982a, p. 61). He continued to defend the market's 1981–1982 evaluation of expected inflation in this period in Friedman (1985b, p. 59).
[412] Friedman (1982a, p. 61), *Newsweek*, June 28, 1982, and *Barrons*, October 25, 1982 (p. 7).
[413] Friedman (1982f, p. 65).
[414] He also implied that these lower longer-term nominal rates would likely have been likely associated with higher levels of real economic activity (*Newsweek*, August 23, 1982). But this did not constitute additional common ground with the later credibility literature, as Friedman was arguing that output would have had a more protracted but higher trough, accompanied by slower disinflation. The credibility literature instead contended that a credible policy would have achieved disinflation more rapidly than that seen in 1979–1982, with the output costs skipped.

surge of inflation in 1984–1985, which, he hoped, could then be followed by a genuinely successful, though economically costly, disinflation.

Interest rates continued to rise during 1984. Furthermore, nominal interest rates remained high in relation to current inflation, including at the long-term end of the maturity spectrum. The account in Goodfriend and King (2005) of long-term interest rate behavior in this period treated bond rates as inexplicable in terms of contemporaneous monetary policy settings and instead appealed to the possibility that the market was constantly expecting a change in monetary policy in an intentionally inflationary direction. Their narrative was, however, ahistorical because it neglected the body of opinion, prevalent not only among monetarists but also many market participants, that *existing* settings of monetary policy in 1983 and 1984 were likely to set off renewed inflation. Indeed, Friedman in mid-1984 defended U.S. longer-term securities as correctly priced and, in particular, as not having an unrealistically high yield.[415] In addition, the notable uptick in longer-term rates in 1983–1984 largely mirrored the rise in short-term rates and likely reflected monetary tightening, not an expectation of easing.[416]

The fact is that, from 1982 through 1984, Friedman's predictions that long-term interest rates would stay high were more correct than they deserved to be—as he and financial markets had similarly excessive forecasts of future inflation.[417] They, like him, had been concerned about

[415] See his remarks in *Wall Street Week*, Maryland Public Television, April 27, 1984 (p. 7 of transcript), and in Heller and others (1984, p. 48).

[416] Ahead of the introduction of the New Operating Procedures, a widespread expectation had been that a disinflation program would see enhanced interest-rate variability on the short end but much more stability in longer-term interest rates (see, for example, Walsh, 1982, pp. 11–12). (This expectation was related, of course, to the hope that longer-term rates' absolute level would quickly move down once the disinflation program started.) Friedman himself had seen this state of affairs as likely to be a feature of a regime of tighter monetary control (see Friedman, 1976c). In the event, in 1979–1982 short- and long-term rates moved unusually closely together (see, for example, Blanchard, 1984)—a phenomenon that has not been satisfactorily explained in terms of monetary policy developments. The comovement was regarded both as a puzzle and a problem, with Governor Henry Wallich expressing the wish that "short-term rates might detach a little from long-term rates" (*American Banker*, June 29, 1982). After 1982, movements in short- and long-term rates continued to be highly correlated, although both were now less variable. It should be stressed that formal analysis of stabilization policy has suggested that it is a desirable feature that the two rates move together to a considerable extent (see, especially, Rotemberg and Woodford, 1999).[417] Long-term interest rates did fall substantially in 1982, as indicated above. The fact that they initially fell in response to the 1982 monetary easing was not really inconsistent with Friedman's view of the liquidity and term-premium effects of monetary expansion. The implication in Samuelson (1984, p. 11) and Benjamin Friedman (1984, pp. 386–387) that the decline in longer-term rates contradicted monetarist propositions was really only applicable if one considered complete-price-flexibility variants of monetarism.

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rapid M1 growth, and they essentially shared his view that the Federal Reserve in 1982–1983 had overestimated the extent to which short-term interest rates could be safely reduced.

Paul Volcker rode out these concerns. He was well aware of fears of a *U*-turn back to inflationary policies and of the need to give reassurance that such a *U*-turn would not occur.[418] But he disagreed strongly with the interpretation of his and other Federal Reserve policymakers' 1982–1983 actions as being that *U*-turn. In testimony given on February 16, 1983, he remarked: "Interest rates have fallen substantially from the high levels of the past couple of years; as confidence builds that inflation can be held in check, further declines should be sustainable."[419]

Volcker did, however, express frustration on occasion at how slowly this confidence was building. He had used the "credibility" terminology in describing his disinflationary aims—for example, when in the summer of 1980 he stated: "The only satisfactory approach must lie in a different direction—a credible effort to reduce inflation further in the period ahead, and policies that hold out the clear prospect of further gains over time."[420]

By late 1982, Volcker seemed to view this credibility as being well established. But he implicitly acknowledged that this process was incomplete, in noting that longer-term inflation expectations had not completely moved down.[421] Similarly, in July 1983 he pointed to continuing concern in the private sector that monetary policy would not deliver results consistent with price stability when he remarked that he had seen survey evidence suggesting inflation was expected to average 6 to 7 percent over the following five to ten years.[422] Indeed, in late 1983 the Federal Reserve chair implied that Federal Reserve attainment of "credibility—in the sense of quickly changing expectations" remained only an aspiration (Volcker, 1983, p. 8).

[418] For example, on February 23, 1982, Volcker testified: "Past failures to 'carry through' have left a legacy of skepticism and uncertainty among workers and businessmen, among consumers and among participants in financial markets… Credibility in dealing with inflation will have to be earned by performance and persistence over time." (In Committee on Finance, U.S. Senate, 1982, p. 329.)

[419] In Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1983b, p. 5).

[420] Volcker (1980b). See also his public use of credibility-related language in 1981 (quoted in the previous chapter).

[421] Specifically, in October 1982, Volcker wrote: "In any event, lasting relief from high interest rates requires that the Federal Reserve maintain a credible posture of anti-inflationary restraint." (In Joint Economic Committee, U.S. Congress, 1982b, p. 36.)

[422] See Volcker's testimony of July 20, 1983, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1983c, p. 221). Volcker also complained during 1983 that nominal wage settlements seemed to be predicated on expectations of inflation much higher than those seen recently: see, in particular, his remarks of April 12, 1983, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1983a, pp. 10, 14), and of October 20, 1983, in Joint Economic Committee, U.S. Congress (1983c, p. 19). In the event—perhaps because of elements of flexibility in national wage adjustment not evident in the reported settlements figures—nominal wage growth moved closely with, rather than lagged, the decline in inflation seen from 1980–1981 to 1986: see DeLong (1997, p. 258). In particular, as Meltzer (2009b, p. 1167) stressed, nominal wage growth did not feature the delayed adjustment to the environment of lower inflation that marked the behavior of longer-term interest rates.

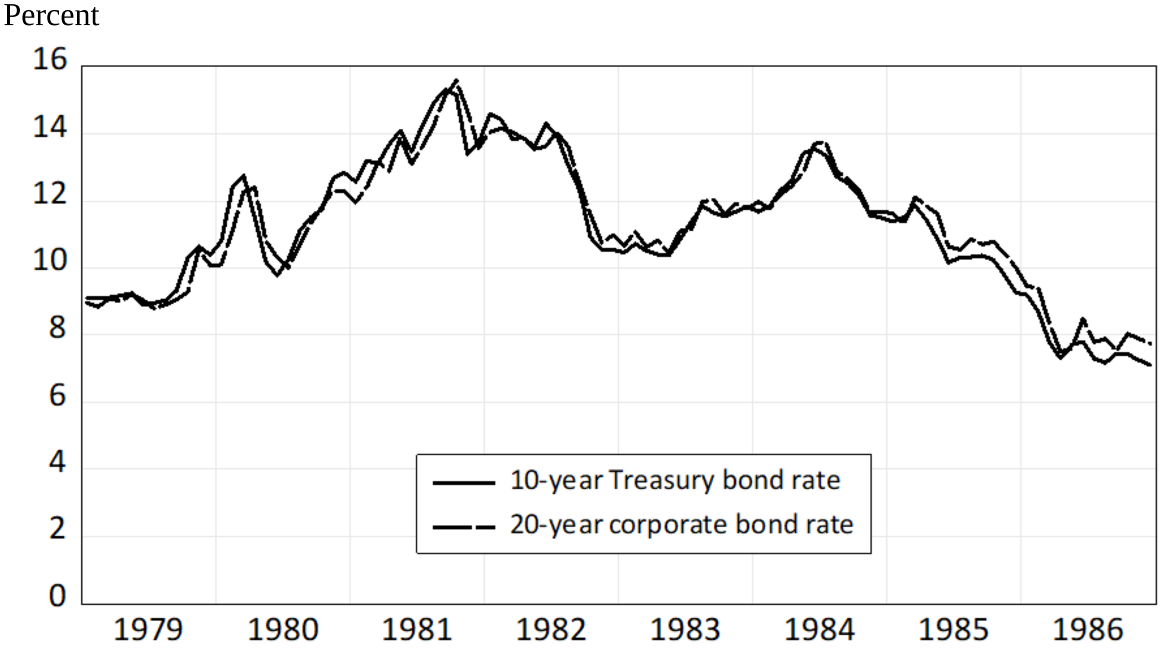

Figure 7. Long-term U.S. Treasury bond rate and long-term U.S. corporate bond rate, January 1979–December 1986.

Source: Federal Reserve Bank of St. Louis FRED portal, and Hess, Jones, and Porter (1998) dataset.

Over the course of 1985, the predictions that Friedman and others had made of a dramatic surge in inflation were refuted even more clearly than they had been in 1984, and Volcker's post-1981 track record of low inflation was consecrated. The move down of longer-term U.S. interest rates into single-digit values, something that Volcker had once hoped would be an early result of a disinflationary policy, finally occurred: see Figure 7.

The 10-year Treasury bond rate averaged 9.76 percent in 1985:Q4, the first single-digit value since 1979:Q3. In the fourth quarter of 1986, it was down to 7.26 percent.[423] Even these rates were above what Volcker thought should emerge from the situation of long-run price stability that he sought: he hoped for a 6 percent rate by 1990 (see Nelson, 2021). But the interest-rate developments in 1985 and 1986 indicated that, after a recession (or two recessions) and a sustained stretch of low inflation, Volcker had now acquired considerable credibility.

Indeed, while U.S. interest rates (especially longer-term rates) retraced a notable amount of their 1982–1983 decline during 1983 and 1984, the decline of interest rates in 1985 and 1986 was more permanent, in large part because, as already indicated, this was the period in which

[423] Similarly, the twenty-year corporate bond rate, also shown in the figure, fell to 9.45 percent in January 1986, its first single-digit value since September 1979, and was 7.74 percent in December 1986.

financial markets came to accept that the large step down in inflation seen after 1981 had proved enduring (see, for example, Kozicki and Tinsley, 2005). A *New York Times* report at the time noted (March 16, 1986, p. F32) with regard to the inflation/interest-rates picture that a “massive change,” begun in the early 1980s, was currently being completed, as “over the past year or so, that sustained improvement [in inflation] has finally persuaded long-term lenders that they can begin to whittle down the inflation premium” built into securities yields.[424]

Managing monetary growth following a disinflation

Longer-term U.S. interest rates were therefore something of a latecomer in really registering the scale of the Volcker disinflation. Their tardiness in adjusting to the new environment contrasted with the behavior of M1 velocity. As already noted, M1 velocity broke from its historical behavior virtually at the time at which the fall in inflation of the early 1980s became dramatic (see Figure 3*a* above). By altering their holding of real money balances in a manner that halted the longer-term upward trend in velocity, U.S. households appear to have been essentially the first set of actors in the American economy to register the notion that there was a permanent change in the trend of key nominal variables around 1982.[425]

Of course, as nominal interest rates, both short-term and long-term, actually peaked in late 1981 and fell sharply in the second half of 1982, the shift in trend of M1 velocity in part simply reflected the private sector’s reaction to actual realized data. But nominal interest rates had fallen by large amounts before 1982, yet 1982 was the first year for which modern M1 velocity growth, fourth quarter to fourth quarter, had been negative. The then-unusual fall in velocity may well have reflected an evaluation by holders of money that the permanent component of interest rates, and inflation, had come down. Such an evaluation would also have been consistent with the view that Friedman held at the time concerning U.S. economic prospects: although more pessimistic than others about inflation and interest rates he was, as discussed above, willing even in 1982–1983 to concede that future peaks of these series would likely be below the early-1980s peaks.

[424] Similarly, in the *Wall Street Journal* the following day, Paul McCracken remarked: “Finally, in 1985–1986, people have decided [that] a better price-level [trend] performance is a reality, and interest rates have adjusted downward.”

[425] Livingston survey data on inflation expectations in the U.S. private sector were consistent with a shift occurring in the behavior of this variable during the 1981–1982 period (see Hoffman and Rasche, 1996, p. 115). Robert Rasche wrote to the present author in a letter of February 21, 1992: “I don’t think that NOWs or greater stock market activity had anything to do with the shift [in M1 velocity] that I believe occurred around 1981. I have come to the conclusion that the best explanation of this event is a change in inflation expectations. This is, of course, an old argument of Milton Friedman[’s].”

In the face of such a disinflation-induced break in monetary velocity, what is the appropriate course for monetary growth? At the time, columnist Samuel Brittan (in *Financial Times* (London), December 9, 1982) described a permanent velocity decline as part of the dynamics of disinflation that was in the "Friedmanite textbooks" but not allowed for in the "Friedmanite policy rules" of constant monetary growth. Brittan did not specify what textbooks he had in mind, but he may have had items like Darby (1976). In addition, the advanced text of Merton Miller and Charles Upton (1974, pp. 244–245, 261, 264), which was discussed in chapter 5 above, and the analysis of Gould, Miller, C.R. Nelson, and Upton (1978, p. 232) had discussed what they called the "Friedman surge"—the scenario Friedman had discussed in which higher velocity growth, induced by rising opportunity costs, reinforced high monetary growth and prompted still higher inflation. In this vein, he noted in 1977 that monetarists had "always argued" that a one-time velocity rise accompanies a move to a higher long-run inflation rate.[426]

But Friedman had also discussed the converse case, which would acquire new relevance in the United States in the 1980s—when a disinflation is accompanied, and potentially reinforced, by slower growth in, or a decline in, velocity. The qualitative point that a shift in velocity behavior could be a consequence of disinflation was one of long standing in Friedman's own monetary analysis, being implicit in his original restatement of the quantity theory in 1956 and something that came out of the celebrated Cagan (1956) study of hyperinflations that Friedman supervised.[427] Friedman himself had laid out in his discussions of the 1969 gradualism program his expectation that, at some point, the planned disinflation would lead to a reduction in velocity, as the Fisher mechanism reduced the opportunity cost of holding money balances (Instructional Dynamics Economics Cassette Tape 20, April 7, 1969; Instructional Dynamics Economics Cassette Tape 35, October 8, 1969), as well as in the late-1972/early-1973 period—when he was under the mistaken impression that the Burns Federal Reserve might have successfully achieved a permanent disinflation.[428]

The notion of managing monetary policy after disinflation can be taken further by considering a situation in which (as in the United States in 1982) slack remains after the decline in inflation has been achieved. James Tobin (1982, p. 302) had been quick off the mark in recognizing the

[426] Friedman (1977f, p. 17). See also pages 45, 67, and 342 to 343 of Friedman and Schwartz (1982a).

[427] See *Newsweek*, October 16, 1972, and Friedman (1973). See also Nelson (2003, p. 1038) for a brief discussion of Friedman's position on this subject.

[428] Friedman recalled in July 1986 that he and others had written on this matter in the research literature (see Darby and others, 1987, p. 21). Dornbusch (1991, p. 459) cited, in addition to Friedman (1983e), Mundell (1971) as another reference that recognized the desirability of letting monetary growth rise for a time after a disinflation. Dornbusch, who helped in the preparation of the Mundell book, was likely referring to the discussion on page 49 of that monograph.

relevance of this situation, and he suggested that under the circumstances described, a period of temporarily high monetary growth was appropriate in order to close the output gap, avoid an unwanted degree of downward pressure on inflation, and accommodate the fall in velocity.

One specific option in these circumstances is to provide monetary growth high enough not only to accommodate higher money demand but *also* to reduce nominal and real interest rates. This can help close the output gap, but if the stimulus is modulated appropriately, a rise in inflation can be avoided. Policymakers can then lock in the lower nominal interest rates by lowering monetary growth again, thereby forestalling a rise in inflationary expectations. A proposal along these lines was made by Fred Bergsten and Lawrence R. Klein in April 1983 (*The Economist* (London), April 23, 1983) when they called for "a one-shot growth in the money supply followed by a return to lower rates of expansion" (p. 20).[429]

In his mid-1983 talk at the Bank of Japan, Friedman addressed the issue of handling the decline in velocity associated with disinflation (although he did not consider the complication considered above of economic slack remaining). Friedman granted that the achievement of a disinflation justified a stretch of time in which the rate of monetary growth could be higher, accommodating the increase in real money demand. He indicated, however, that he disagreed with attempting this in practice. He cited the danger of overdoing the temporary period of elevated monetary growth—and in so doing reigniting inflation.[430]

Friedman had also relayed this message in correspondence with David Lindsey of the Federal Reserve Board. Lindsey had stressed the extent to which the 1982 decline in M1 velocity had struck policymakers and Federal Reserve Board staff as a truly unusual event that called into question the reliability of M1. Paul Volcker himself had communicated the same message to Friedman, telling him in a letter of December 20, 1982: "your comments make no allowance for some obvious institutional problems with M1 that exaggerate the growth [rate]… I have to be aware of the fact that velocity is acting most peculiarly by postwar standards, and for a long enough period and by a large enough amount that it can't be dismissed as a typically cyclical phenomen[on]."[431]

[429] A proposal of this kind was later formalized by Woodford (1994, pp. 371–372), who observed that "[i]n an intertemporal equilibrium model, it is easy for faster money growth to be associated with the initial transition to lower nominal interest rates, while the eventual consequence of keeping interest rates as that level is a low long-run rate of money growth."

[430] Friedman (1983e, pp. 5–6). See also the related discussion in *Wall Street Journal*, September 1, 1983. Dornbusch and Fischer's (1987, p. 655) discussion of post-disinflation policy contained a similar warning about the danger of overdoing the expansion of nominal monetary growth.

[431] Available at *https://fraser.stlouisfed.org/archival-collection/paul-a-volcker-papers-5297*.

Friedman had been unmoved. In the spring of 1983, he had written to Lindsey that "the experience of the past few months is[,] I am afraid[,] reinforcing my view on the desirability of sticking to a rule through thick or thin despite the presumed ability to recognize changes in money demand. The monetary explosion that we are now experiencing cannot, in my opinion, produce anything but trouble for us."[432]

Policy rules and the end of M1 targets

The thick-and-thin imagery that Friedman used in his letter to Lindsey was one that Paul Volcker on occasion also deployed when discussing monetary policy. Indeed, the changing context in which Volcker used that imagery was a revealing indicator of the disaffection with monetary targeting that Paul Volcker developed over the early 1980s.

In early 1981—a time during which he was trying to communicate the FOMC's determination to achieve its monetary targets—Volcker had used the "thick and thin" terminology in a favorable context. Specifically, as discussed in the previous chapter, he suggested that the Federal Reserve would execute multi-year reductions in monetary growth "through thick and thin" in a disinflationary strategy. Things were different nearly three years later, at the end of 1983. Volcker had become far more ambivalent about monetary targets. And in his speech given at the American Economic Association meetings in San Francisco, he used the phrase "thick and thin" in negative terms and specifically in relation to "some simple and fixed rule," whose merit Volcker deprecated by making its advocates sound Utopian: "The appeal [of such a rule] is obvious; if only we could be fully convincing that we have found a certain path to the promised land, and we stick to it through thick and thin, the natural forces of the marketplace will work toward our objectives, speeding their achievement."[433]

Federal Reserve opposition to monetary policy rules was not new in 1983. Indeed, in correspondence with Friedman during the previous spring, David Lindsey took exception to a passage in a draft paper in which Friedman stated that the Federal Reserve had rhetorically accepted the case for monetary policy rules.[434] Lindsey pointed out that, both under Volcker and previously, the Federal Reserve's official descriptions of its approach to monetary targets had stressed the need for flexibility, including keeping open the option of intentionally allowing the

[432] Letter from Milton Friedman to David Lindsey of April 7, 1983 (p. 1), Federal Reserve Board records.
[433] Volcker (1983, pp. 8–9).
[434] David Lindsey, letter to Milton Friedman, April 29, 1983 (p. 2), Federal Reserve Board records.

realized rate of monetary growth to be outside the announced range.[435] Lindsey's comment led Friedman to modify the passage in question, and in the published version of the article it instead read: "The Fed has rhetorically accepted monetary targets but never a firm monetary rule."[436]

It was, nevertheless, true that in 1982 and 1983—alongside the increasing reservations about M1 targets that Volcker had been articulating publicly even during the spring of 1982, and even more so once the M1 targets went through their period of deemphasis in 1982 and 1983—Volcker became more outspoken against monetary policy rules and monetary-growth rules in particular.

In testimony given on July 21, 1982—after the FOMC had decided to keep its monetary targets for the year, but also when it was discreetly going back to interest-rate management—Volcker recalled that he had consistently opposed a definite set of multi-year monetary targets: "I do not think we have enough knowledge to make that kind of a judgment and adhere to it through thick and thin over that length of time."[437] After the deemphasis of M1 targeting in the fourth quarter of 1982, Volcker stepped up his public objections to policy rules. In testimony to the Joint Economic Committee in January 1983, he stated that the appropriate formulation of monetary policy under current conditions did not amount to a "simple mechanical matter of adhering rigidly to a preset guideline for money or credit growth," or indeed able to be represented by any other simple policy rule: "The approach cannot be reduced to an arithmetic or econometric formula."[438] Similarly, in testimony given in mid-February 1983, Volcker referred to "the longing of some" to condense policy decisions into "a simple fixed operating rule," before adding: "The trouble is, right now, in the world in which we live, I know of no such simple rule that will also reliably bring the results we want."[439] He continued to express similar sentiments in a series of statements over the course of 1983, culminating in the end-of-year speech already quoted.[440]

[435] An early example on the public record was in January 1976, when Arthur Burns stated: "Our objective in life is not to hit the target—but [instead] the best possible performance of the economy. We would not be slaves to a number—I should give up my job if we were." (*New York Times*, January 4, 1976.)
[436] Friedman (1984b, p. 42).
[437] Testimony of July 21, 1982, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1982c, p. 51).
[438] From Volcker's testimony of January 27, 1983, in Joint Economic Committee, U.S. Congress (1983a, pp. 68, 75).
[439] Testimony of February 16, 1983, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1983b, p. 15).
[440] For his remarks along these lines in the summer and fall 1983 period, see, for example, his testimony of August 3, 1983, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1983b, pp. 180–182, 185, 250), and of October 20, 1983, in Joint Economic Committee, U.S. Congress (1983c, pp. 19–20).

Testifying in the new year of 1984, Volcker allowed himself to be drawn somewhat into what he referred to as an "old debate in monetary theory and practice: whether to use rules or discretion." He gave his familiar conclusion: "I would have some sympathy for a rule, if I knew a good rule that I thought would give reliable results and that you could enshrine for all time in the Constitution or in legislation. [But] I don't know of any rule that I have that degree of confidence in at the moment."[441] Volcker's occasional discussions of policy rules continued to be negative, and, near the end of the year, he was quoted saying that he believed that it would be "extremely dangerous—and, in fact, practically impossible—to eliminate substantial elements of discretion in the conduct of Federal Reserve policy." (*Wall Street Journal*, December 10, 1984, p. 16.)

The year 1984 had, nevertheless, seen some signs of diminishing hostility between the Federal Reserve and monetarists when it came to the subject of money's role in monetary policy. In particular, things seemed to be settling down on the M1 front, and there were signs of its further rehabilitation of that monetary aggregate in the setting of U.S. monetary policy. As of early 1984, the criterion—in retrospect, a highly misguided one—that the Federal Open Market Committee saw as justifying putting more weight on M1 was evidence of a return to the pre-1982 trend in its velocity: its drift upward of about 3.5 percent per year, on average. The Committee had begun to see evidence of M1 velocity growing closer to this rate over the second half of 1983. The OECD's (1985, p. 36) account of the period observed: "As M1 velocity recovered, the Federal Open Market Committee restored some of the weight to M1."

"Most of the special influences that depressed velocity in late 1982 and early 1983 are behind us," Volcker testified in early 1984, and he evaluated the behavior of M1 velocity in recent quarters to be "more in line with longer-run experience."[442] In February 1984, the FOMC set an M1 growth target range for 4 to 8 percent, conditional on an assumption that the pre-1982 growth of 3 to 4 percent in velocity would occur over 1984.[443] The cautious manner in which this M1 target had been decided on was, however, underlined when, shortly after the 1984 monetary targets were announced publicly, Volcker remarked: "I suppose what I would say is M1 has been restored to probationary status."[444]

[441] Testimony of February 7, 1984, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1984b, p. 61).

[442] From Volcker's testimony of February 9, 1984, in Joint Economic Committee, U.S. Congress (1984a, p. 245).

[443] See Volcker's testimony of February 8, 1984, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1984a, pp. 8, 130).

[444] From Volcker's testimony of February 8, 1984, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1984a, p. 130).

M1 velocity did indeed follow what was perceived to be its normal postwar behavior when it rose at about its pre-1982 growth rate over the course of 1984. This pattern was sufficiently clear by midyear that in July 1984, the FOMC indicated that M1 target was back to the same status as that assigned to the M2 target.[445] Furthermore, the FOMC achieved its target for M1 growth for 1984—with its four-quarter increase being 5.2 percent (Bernanke and Mishkin, 1992, Table 1, p. 1990). Against this background, in setting its 1985 targets in February 1985, the FOMC reaffirmed the restored status of M1 (OECD, 1985, p. 36).

Friedman, however, took a jaundiced view of the meeting of the M1 target in 1984. Although, as discussed earlier in this chapter, he was quite impressed by the M1 growth pace delivered in the first half of 1984, he did not think that monetary policy for the year as a whole was steady and doubted that the outcome for the year really reflected a policymaker concentration on M1 growth: he referred cynically to the FOMC's "full acceptance [of M1] in 1984 when M1 for a change hit its annual target" (*Wall Street Journal*, August 20, 1985). In particular, he felt that, as part of "the continuation of highly volatile monetary growth since mid-1982," monetary policy was allowed to get too tight in the later months of the year, and he was unimpressed by the meeting of the target for the year as a whole.[446] The Friedmans wrote in early 1985 that "[b]y the end of 1984, five months of zero monetary growth had produced a sharp slowdown in the economy and even raised the possibility of a recession early in 1985."[447]

On this particular point, the Friedman perspective happened to be in a reasonable degree of alignment with that of Federal Reserve policymakers—who also started out 1985 worried about the extent of economic slowdown. But their policy actions in response to this concern definitively underlined how dispensable the M1 target had become. The M1 target set for 1985 was 4 to 7 percent (Bernanke and Mishkin, 1992, Table 1, p. 190). But Mussa (1994, p. 121) suggested that, from the very outset, policymakers did little to meet the target. With inflation stepping down, and with real output growth lower than in previous years, the federal funds rate was kept stable in the early months of 1985 then reduced in midyear. And when the Federal Reserve Board reduced the discount rate in May 1985, the Board's press release explicitly motivated this interest-rate reduction in terms of the need to stimulate production (*Glasgow Herald* (Scotland), May 18, 1985).

[445] See Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives, 1985, pp. 10–11; Hafer, 1985a, p. 20). See also Federal Open Market Committee (1984a, especially p. 40).
[446] See Friedman (1985b, p. 60), including for the quotation.
[447] Friedman and Friedman (1985, p. 83). See also Friedman (1985b, p. 60).

These moves certainly helped avoid the recession outcome that had concerned the Friedmans. But the expansionary policy measures also showed that the FOMC's 1984 reemphasis of M1 was now going into complete reverse. In light of rapid M1 growth in the first half of the year, in July 1985 the Committee widened its 1985 target range to 3 to 8 percent and—along lines similar to what had occurred in mid-1983—rebased the reference period for the target, so that the growth referred to in the specified target covered a period that excluded the early part of the year (see Federal Reserve Board, 1986, p. 12). Friedman criticized these adjustments to the 1985 M1 objective and judged that it was another example in a long series of examples of the Federal Reserve's "disregard of targets" by the Federal Reserve (*Wall Street Journal*, August 20, 1985).

In the event, the M1 outcome of 11.9 percent growth for 1985 overshot even the widened and rebased target range.[448] And 1986 saw a repeat of key events of 1985: a modest, if wide, target range for M1 (3 to 8 percent) announced at the start of the year, high actual M1 growth (this time, 15.2 percent), notable cuts in policy rates (the federal funds rate and the discount rate), declining inflation, and a favorable real output growth outcome despite policymaker concerns about the strength of the economy.[449] A difference from 1985 was that, in mid-1986, the FOMC indicated that it would simply accept an outright overshoot of the previously-announced M1 target range. In so doing it, in effect, dropping the year's M1 target.[450] Subsequently, M1 targets never returned.

A motivation for the FOMC's adjustments to and overshoots of the M1 targets in 1985 and 1986 was a new downward movement in velocity (Figure 3*a*). M1 velocity declined again in 1985–1986, as it had in 1982–1983. But the second decline was steeper and spanned a longer period. On modern data (using nominal GDP as the income series), M1 velocity declined by 6.1 percent in the seven quarters through 1983:Q2. Its later fall, in the nine quarters through 1987:Q1, was of 14.8 percent.

In another respect, too, the 1985–1986 decline was more confounding to the use of monetary aggregates for the purpose of evaluating monetary policy stance. Monetary growth and nominal income growth had both been high in 1983. But in 1985 and 1986, nominal income growth was dissimilar to monetary growth (see Figure 1), especially as measured by M1, and slowed down during the monetary expansion. In response, as Robert Rasche would observe, "in recent discussions of monetary policy it is commonly asserted that previously established relationships

[448] See Bernanke and Mishkin (1992, Table 1, p. 190) and Federal Reserve Board (1986, p. 3).
[449] For the numbers, see Bernanke and Mishkin (1992, Table 1, p. 190).
[450] See Lindsey (2003, p. 93).

between monetary aggregates and economic activity measures [have] completely broken down... [I]t is claimed that if there was any life left in the assertion that monetary aggregates are appropriate guides for monetary policy after the experience of the early 1980s, the experience of 1986–87 provides the final nail in the coffin."[451]

In his 1968 "The Role of Monetary Policy," Friedman had acknowledged that "[i]n principle, 'tightness' or 'ease' depends on the rate of change of the quantity of money supplied compared to the rate of change of the quantity demanded," but he had suggested that, in practice, the money demand function was sufficiently stable that monetary growth did not need to be adjusted for demand shifts.[452] Yet 1985 and 1986 had provided a sustained case in which strength in M1 and M2 was not manifested in a speeding up of nominal income.

The episode was not, however, a thoroughgoing refutation of Friedman's monetary views: There was justification in his complaint in July 1986 about commentators who went "around talking about how the terrible movements in velocity of recent periods suddenly have disconfirmed everything that those of us who have been studying money for some thirty years have concluded from the past."[453]

The 1985–1986 period was, without a doubt, damaging to policy analysis that emphasized monetary aggregates. But the rapid rate of monetary growth in this period was not as divorced from policy stance as the disconnection between monetary growth and nominal income that took place in those years might suggest. Indeed, Mussa (1994, p. 121) offered the bold description of 1985 and 1986 as "A Return to Easy Money." The merit in Mussa's description lay in the fact that these were *not* years in which the increase in the money stock simply amounted to the authorities accommodating a large positive money demand shock. Such a scenario requires a constant value for the interest rate being chosen by the authorities. But instead, as already indicated, U.S. nominal interest rates declined substantially in 1985 and 1986. The decline in nominal interest rates, particularly at short maturities, in that period very likely initially reflected monetary stimulus—and so the strength of the monetary aggregates was sending a correctly-signed indication of the direction in which monetary policy was going. Having been initiated by policy actions, the interest-rate declines—and accompanying declines in M1 velocity—were then made, in good part, permanent by the fact that inflation and inflationary expectations fell during

[451] Rasche (1988, p. 43). Rasche was describing conclusions from which he dissented but that were widespread.
[452] Friedman (1968b, p. 7).
[453] Darby and others (1987, p. 11).

1985 and 1986.[454] "We have not had this kind of decline of inflation and this kind of decline in interest rates on the historical record," Anna Schwartz observed in the summer of 1986 (*New York Times*, July 3, 1986, p. D7).

Viewed against this background, the episode may be seen as one of managing monetary growth, including providing a higher rate of money creation in order to provide stimulus, in conditions of disinflation. The policy moves involved can therefore be regarded as lining up somewhat with the scenario laid out by Bergsten and Klein policy in 1983 (see above). It was not a period in which monetary aggregates, including M1, were meaningless or in which their variations were being dominated by institutional change. But neither did the episode bear witness to the case for a constant-monetary-growth rule: the high monetary growth was better attuned to the situation of declining inflation expectations than keeping monetary growth at 1984's quite low rate would have been.

And, even more than before, the events of 1985 and 1986 put a new complexion on the case for monetary policy rules by making critics of rules and many of those sympathetic to rules more convinced than before that the constant-monetary-growth rule could be improved on.

Ahead even of the advent of the new velocity declines of 1985–1986, Friedman was under no illusion that policymakers would evolve into acceptance of his constant-monetary-growth rule: "the Fed never will become monetarist, and that's one forecast you won't prove wrong." (*Wall Street Journal*, December 10, 1984, p. 16.)[455] He nevertheless, as of early 1985, was suggesting that "a strict monetarist policy" was the alternative that might be adopted in the future in the event of a major new wave of dissatisfaction with U.S. economic performance.[456]

[454] The interpretation of the years 1985 and 1986 as seeing (on net) an easing of monetary policy was by no means confined to monetarists. When financial-market economist Edward E. Yardeni appeared on *Wall Street Week* in 1982, host Louis Rukeyser's preamble included a long exposition of recent commentary by "Milton Friedman, the high priest of U.S. monetarism," followed by an indication that viewers would now hear the contrary view held by "the opposing school of economists" in the form of Yardeni, who "studied at Yale under that arch-foe of Friedman-style monetarism, Professor James Tobin." (*Wall Street Week*, Maryland Public Television, August 27, 1982, pp. 6 [first quotation], 7 of transcript.) When Yardeni reappeared on the program in July 1985, he disagreed with monetarist forecasts of a 1986 rise in inflation and predicted, fairly accurately, that "inflation is going to be near zero over the next twelve months." But he agreed that monetary policy was currently expansionary, Yardeni's assessment being that "most of this monetary stimulation is really going into real economic activity instead of inflation." (*Wall Street Week*, Maryland Public Television, July 12, 1985, pp. 7, 8 of transcript.)

[455] Friedman was, of course, alluding here to his well-publicized erroneous forecast, made about a year earlier, of a 1984 U.S. recession.

[456] Friedman (1985b, p. 61). In a memo prepared for the January 1986 presidential advisory board, Friedman (1986f, p. 3) was less specific: "I am persuaded that sooner or later there will be a major monetary reform to provide an anchor for the long-term price level… But the precise character of the reform, let alone its timing, is shrouded in uncertainty."

This possibility became still more remote, however, as the 1985–1986 experience hardened the case against Friedman’s rule. In central-banking circles and in much U.S. economic research since the 1970s, the long-run stabilizing properties of a constant-monetary-growth rule had been accepted, with criticism being centered on other aspects of the rule. In particular, since the introduction of monetary targeting, the case against the application of stricter control of monetary growth, via management of the aggregate level of commercial bank reserves, had been on the short-run variability—especially interest-rate variability—that it would create rather than the average economic performance that it would deliver. “I never became an enthusiast for federal funds rate targeting,” Ann-Marie Meulendyke observed in this connection (interview, April 29, 2013), “but I also saw that hitting a reserves target week to week would not have been an easy thing to do. And it would have introduced an awful lot of noisy volatility into the funds rate. And I always felt that Friedman underestimated that problem.”

The events of 1985 and 1986 in the United States, however, brought the long-run properties of Friedman’s rule into greater question, especially when that rule was interpreted as a prescription of constant growth in M1. These years were not a straightforward case of securing stability in interest rates in exchange for instability in monetary growth. On the contrary, interest rates had not been stable over these years. There had been a major movement in rates, albeit a benign one—a lasting decline—and one that occurred in a gradual fashion over two years, in good part because it was smoothed by the FOMC’s management of the federal funds rate. The objection to a constant-monetary-growth rule in these circumstances was not primarily that it would have destabilized interest rates. Rather, it was that it would have implied an inappropriate *average* stance of monetary policy—as the rule would have implied a low or very moderate rate of monetary growth during a time when the continuation of the United States’ noninflationary expansion required a policy stance that implied a rapid rise in M1.

Those who had been Friedman’s longtime critics in policy circles took note of this implication. Lyle Gramley, who had still been a Federal Reserve Board governor during the beginning of the new decline in velocity and had since become a private-sector economist, remarked: “What has happened is that the relationship between growth of the economy and the growth of the money supply is just no longer there.” (*New York Times*, July 4, 1986, p. D7.) And Frank Morris, who had been a reserve bank president since 1968 and a critic of Friedman’s over all of the intervening years, used his September 1985 speech in New York City to highlight a fresh aspect of the case against Friedman’s rule. “Milton Friedman has argued that monetary policy should ideally be conducted by a few clerks,” Morris remarked. Morris offered a conjecture about “what would have happened if Milton’s clerks were running monetary policy” since mid-1982.

"The rate of inflation would probably have been even lower than it is today, but the costs in terms of employment and output would have been prohibitive."[457] Morris suggested that events of the past three years had provided the "most critical recent demonstration of the need for discretion in monetary policy."[458]

Morris' speech did, however, provide something of an olive branch to advocates of policy rules (though less so Friedman's rule specifically) by having a title that referred to "Rules Plus Discretion" instead of "Rules Versus Discretion." And although Friedman remarked in July 1986 that the Volcker regime seemed to him to have devolved into a highly discretionary arrangement that employed forms of "fine-tuning," that regime came, as already discussed, to be regarded as rule-like on certain dimensions.[459]

Indeed, immediately after criticizing simple policy rules, Volcker himself had (in testimony given on February 16, 1983) described his approach of encouraging "a sense of price and financial stability" not only as an "objective" but also as the "basic rule we must observe."[460] And, as this and the preceding chapters have stressed, his record of statements was replete with affirmations of the need to instill favorable longer-run expectations through encouraging confidence in future monetary policy. One example of this was his remark in April 1982: "We have a major role in sticking to it—in the sense of giving people some sense of confidence that they can plan on a disinflationary world, rather than the reverse."[461]

As of 1986, however, interpretations of Federal Reserve policy under Volcker concentrated on the respects in which it had dropped its interest in ideas associated with Friedman. Federal Reserve Board Governor Charles Partee stressed this point, when referring to Friedman in the specific context of 1985–1986 developments. "I feel sorry for him. He's an old man now. He spent his life on this theory. Now it's destroyed. The decision to let money go and try to have enough stimulus to keep the economy going pretty much finishes the monetary aggregates. This is the second time we've had to do that… [We were] proved right both times. The aggregates can never again be a discipline on monetary policy."[462]

[457] F.E. Morris (1985, p. 4; p. 225 of 1987 reprint).
[458] F.E. Morris (1985, p. 4; p. 225 of 1987 reprint).
[459] The quotation is from Darby and others (1987, p. 12).
[460] In Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1983b, p. 15).
[461] Volcker (1982a, p. 22).
[462] In Greider (1987, pp. 684–685).

Partee therefore gave a comprehensive obituary regarding Friedman's work and influence. But damaging though recent years had been for Friedman, Partee had overplayed his hand. Monetary aggregates would, in fact, make a prominent, if ill-fated, comeback in Federal Reserve policymaking within a few years in the form of the concern with M2's behavior that Alan Greenspan showed in the early years of his tenure.

In the even nearer-term future, Partee's statement would prove to be hubristic. During 1986, having moved back to relying on M2, Friedman expressed some retrospective approval of the policymakers' having allowed monetary growth to be on the high side since 1982 but expressed the firm view that the Federal Reserve was likely to overdo things by providing more monetary growth than the permanent decline in velocity justified: "We are sooner or later going to overstay this period of expanding money supply and we are going to have a resumption of inflation."[463] Similarly, Anna Schwartz argued that "that money is going to show up somewhere" (*New York Times*, July 4, 1986, p. D7).

In contrast to Friedman's warnings about how inflation would behave in 1984–1986, these fears about the post-1986 inflation rate proved justified. The FOMC likely did overestimate, in 1986, the degree to which interest rates could be reduced without losing ground on inflation. Consistent with this possibility, after 1986 output overshot estimates of potential output and inflation showed a distinct pickup in 1987–1988, according to both the CPI and GNP deflator measures (see Judd and Rudebusch, 1998, Figure 1, p. 5; Council of Economic Advisers, 2011, Table B–63, p. 263).

Falls in oil prices had materially influenced the headline CPI readings in 1985 and 1986. But, perhaps reflecting the mid-decade monetary stimulus, even the reduction in the GDP deflator inflation seen in those years had been fully wound back by the end of the 1980s: the GDP deflator inflation rate was 3.8 percent in 1989, the same as in 1984 (Council of Economic Advisers, 2011, Table B–7, p. 199). The second half of Volcker's second term had seen a partial reversal in establishing monetary policy settings consistent with progress toward price stability.[464]

[463] Darby and others (1987, p. 21). See also Friedman's remarks in *Idea Channel*, 1987.

[464] Benjamin Friedman's (1997, p. 159) categorical remark that "these years of rapid money growth… did *not* result in a renewed increase of inflation rates" was, therefore, accurate with regard to three of the four high-M1-growth years he cited (1982, 1983, and 1985) but more questionable in the case of 1986.

III. PERSONALITIES IN DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1982–1986

BENJAMIN FRIEDMAN

Among veteran Keynesians, the very public battering that Milton Friedman took on monetary matters took in the mid-1980s provided an entertaining spectacle. As one financial columnist summarized the situation, Friedman had "ridden high over the last 20 years, and his discomfiture is widely enjoyed in some quarters." (*Boston Globe,* September 24, 1986.)

Prominent in those quarters was one of the leading U.S. Keynesians, Paul Samuelson. In a talk given in March 1984, Samuelson applauded the "Fed's departure from monetarism," which he dated to the summer of 1982.[465] In addition, although Samuelson had relinquished his *Newsweek* column in 1981, he had been a regular reader of Friedman's—often ill-judged—post-1981 columns for that magazine. Samuelson's March 1984 speech noted the 1983−1984 Friedman columns' failed prediction of a recession—a failure that Samuelson attributed to not taking "into proper account the vast bulk of the relevant non-*M* data."[466] Another lengthy Samuelson piece, which appeared as an op-ed in the U.S. press in mid-1984, focused on Friedman's warning of high inflation starting late in the year—a warning that Samuelson considered off-base (*The Plain Dealer* (Cleveland, Ohio), June 2, 1984).

Before long, of course, Samuelson could take satisfaction in having prevailed in the disagreement with Friedman regarding 1984's inflation prospects. He was nevertheless unhappy that Friedman was not making a more fundamental concession of defeat beyond acknowledgment of his recent forecasting errors. In March 1985, after Friedman's heart attack, Samuelson was magnanimous, remarking that "without a doubt" Friedman was the most important influence on current U.S. economic policy.[467] But the flailing predictive performance of monetarists in the mid-1980s and the Federal Reserve's renewed moves during 1985 and 1986 away from M1 targets led Samuelson to tell the *New York Times* that monetarism's flaws had been highlighted by recent events (*New York Times*, July 3, 1986, p. D7). He realized, however, that Friedman did not see himself as having been defeated, and on the day on which Friedman published his *Wall Street Journal* article (September 18, 1986) laying out his preference for M2

[465] Samuelson (1984, p. 10).
[466] Samuelson (1984, p. 11).
[467] Quoted in *San Francisco Examiner*, March 18, 1985. A year earlier, Paul McCracken had expressed the same judgment—but went further, by saying that the breadth of Friedman's influence made him the century's most influential economist (*Baltimore Sun*, March 9, 1984, p. D9).

over M1, but reaffirming his belief in the "failure of fine-tuning," Samuelson spoke out on monetarism again. He reaffirmed his approval for the Federal Reserve's deviation from monetarist prescriptions, while expressing puzzlement that Friedman could interpret events since 1982 as supporting, rather than repudiating, his and other monetarists' views. "Actually, the 1980s have been hard on monetarism… [Yet] today, in the *Wall Street Journa*l, there is a piece by Milton Friedman in which he manages to interpret the most successful fine-tuning incident of our times… into an indictment of fine-tuning and a vindication of the wisdom of monetarism's inflexible rule concerning [a] constant growth rate of some *M* or another."[468]

These interventions on Samuelson's part were occasional commentaries on the monetary scene. He had not been contributing regularly to research conferences or journals on the subject of monetary policy for many years, and he was not among those going into detail on recent developments in the debates regarding money.[469]

As for another longtime opposite number of Friedman's, James Tobin, he, too, displayed in the mid-1980s a wish to step away from the Keynesian-monetarist debates—even the rational expectations aspect of it in which he had been most engaged of late. In February 1985, Tobin served as a discussant of a paper by Bennett McCallum at a conference on monetary targeting. The conference marked the *n*th event held on the topic, Tobin said, and the *m*th occasion that he had participated as the requisite spokesman for the anti-monetarist position. "Both *n* and *m* are large numbers," Tobin went on, "*m* fortunately a bit the smaller. I cannot control *n*, but I think *m* has reached its bound."[470]

Tobin largely adhered to this promise: although he contributed a piece to a series of articles on the status of monetarism that *The Economist* published in the spring of 1985, the Bank of Japan conference of May 1985 for which both he and Milton Friedman served as keynote speakers was not an occasion for the revival of Keynesian/monetarist tensions. Instead, each of them provided a talk that, in large part, could have been delivered by the other. On the one hand, in contrast to

[468] Samuelson (1987, p. 110).

[469] It is open to doubt also whether Samuelson read the public-policy and research debate related to monetarism as avidly as he once did. In Samuelson (1984, p. 11), he implied that it was an idiosyncrasy on Friedman's part to believe that the response of inflation to monetary policy actions involved a longer lag (on average) than the response of output to those actions. In fact, this position—although it was certainly one for which Friedman provided much evidence (see Nelson, 2020b, Chapter 15)—had become a standard one by the 1980s, shared by many researchers in practitioners in the fields of monetary economics and macroeconomics. For example, Otto Eckstein, in testimony of April 30, 1979, observed: "First, in the short run, the inflation rate does respond to the level of aggregate demand… But… [it] take[s] as much as a year before the demand pressure is fully converted into price pressure." (In Joint Economic Committee, U.S. Congress, 1979, p. 24.)

[470] Tobin (1985b, p. 605).

his June 1983 contribution to the same conference series, in which he had been specifically concerned with defending monetarism, Friedman—belligerent at the 1983 conference, but on his best behavior at this one—wrote about evolving monetary policy regimes and made minimal reference to monetary growth or to the Federal Reserve. On the other hand, Tobin's (1985a) speech contained the combination of positions that others had found puzzling in Friedman: a discussion of the benefits of financial deregulation, alongside sympathetic words regarding a regime of 100 percent reserve requirements. Thereafter, Tobin remained active in making comments on current economic issues, but, after over twenty years, his debate with Milton Friedman was very largely concluded.

As Tobin was withdrawing from the field, the main driver of the anti-monetarist position in U.S. academia was emerging as Benjamin M. Friedman.

Benjamin Friedman had been a member of Harvard University's economics department since 1972. Even before then, he had been a student at the university. "I arrived as a freshman in the fall of '62, and I graduated in June of '66. And then I went to do the first part of my graduate work at the other Cambridge [that is, Cambridge University in the United Kingdom], and I was there from the fall of '66 until June of '68. And then I returned and did the latter part of my graduate work at Harvard, and finished, and got my Ph.D. in June of '71." As far as Milton Friedman was concerned, "I certainly knew of him as an undergraduate, and even more so as a graduate student. And then I guess the other thing to say is that not when I was an undergraduate, but all through my years as a graduate student, I had a lot of involvement at the Federal Reserve. And, of course, Milton and his ideas were very much under discussion at the Fed in those days." (Benjamin Friedman, interview, May 10, 2013.)

The specific reason why Benjamin Friedman had occasion to know well the Federal Reserve discussions of Friedman was that, well ahead of Ph.D. graduation, he had had notable employment at the Federal Reserve in multiple capacities. These included an affiliation with the Federal Reserve Bank of Boston, headed from 1968 to 1988 by that adamant opponent of monetarism, Frank Morris. After initial hiring as a research assistant, Benjamin Friedman served for many years, beginning even in his time as a graduate student, as a consultant to the president of the Federal Reserve Bank of Boston.[471] As for his other early affiliations with the Federal Reserve, he recalled: "Almost immediately after when I returned from Cambridge [U.K.] in the

[471] Benjamin Friedman (interview, May 10, 2013) noted that "at some stage during my first year as a graduate student at Harvard, I took on an assignment as consultant to the President of the Federal Reserve Bank of Boston."

summer of 1968, I took a job as a research assistant in the research department of the Federal Reserve Bank of New York… And then I had a summer position—not doing research, but working on a particular project for the head of the research department—at the [Federal Reserve] Board in the summer of '69." (Benjamin Friedman, interview, May 10, 2013.)[472]

In his Board position, Benjamin Friedman was soon assigned a high-classification task at the heart of monetary policy: working on possible options for the development of the FOMC directive—the instruction issued after each FOMC meeting to the Federal Reserve Bank of New York regarding open market operations in the period ahead. Instead of doing research, therefore, Benjamin Friedman had found that his Federal Reserve role involved being "a quasi-staff member of this committee to redo the directive, the so-called Maisel committee, which consisted of three FOMC members—[Board governor Sherman] Maisel, Frank Morris from Boston, and Mr. Swan, who was the head of the San Francisco bank—plus very senior people from the Board and elsewhere in the System: there was Dick Davis from the New York Federal Reserve, there was Steve Axilrod from the Board, and then some people right down to me as junior members. So I learned a lot there." (Benjamin Friedman, interview, May 10, 2013.) With some breaks, Benjamin Friedman was on the Board staff for over a year in the 1969–1970 period.[473]

This period was part of the long stretch of time in which Milton Friedman was calling for reform of Federal Reserve operating procedures—and was doing this not only in public commentary, but also in correspondence with and meetings with the Board hierarchy. Over this period, Benjamin Friedman discussed Milton Friedman's views with senior Board officials, and he also watched their reactions to Milton Friedman's appearances at the consultants' meetings. "They found his role nettlesome, I would say, in a way that academics who disagreed with him had no reason to." (Benjamin Friedman, interview, May 10, 2013.)

Benjamin Friedman therefore completed his student years personally familiar both with the academic opposition to monetarism, which had a major Cambridge, Massachusetts, base, and with central bankers' objections to the monetarist movement. Indeed, "by the time I graduated with my Ph.D. in '71, I felt I had learned about what the Federal Reserve did. I mean, I had attended FOMC meetings, for example. And I had been at the Board, I had been at the Boston bank, I had been at the New York bank—I basically knew everybody." (Benjamin Friedman, interview, May 10, 2013.)

[472] See also *https://scholar.harvard.edu/files/bfriedman/files/benjamin_m._friedman_c.v._-_december_2019.pdf.*
[473] This included going "back to the Board in the summer of 1970." (Benjamin Friedman, interview, May 10, 2013.)

With regard to Federal Reserve's perspective on monetary-growth rules, Benjamin Friedman found it similar to that prevailing at Harvard University during the 1960s and early 1970s. "Especially in those days, Milton was very much identified in the public discussion and within the Federal Reserve, and also in the academic community, with the idea of fixed money growth rates as an operating principle for monetary policy. The people who were my teachers at Harvard at that time didn't accept that idea, and they didn't later. And I didn't later, and I don't now—none of that should come as any surprise." Benjamin Friedman remarked that Federal Reserve officials' attitude to Milton Friedman was shaped not only by irritation at his interventions, including the tone of those interventions, but also by the fact that "at the substantive level, I think, they rejected his policy proposal. They thought that having fixed money growth rates was not useful." (Benjamin Friedman, interview, May 10, 2013.)

Although he would later himself become a prominent critic of Milton Friedman and monetarism, after Ph.D. graduation Benjamin Friedman would have occasion to be in agreement with his near-namesake on a leading economic-policy issue. In 1971–1972, Benjamin Friedman had a position at the financial institution Morgan Stanley. Benjamin Friedman recalled that the Nixon wage/price freeze of August 1971 was imposed soon after he started working for Morgan Stanley, and he was asked to predict the repercussions of the freeze for the bond market. "And I said, 'Well, it seems pretty clear to me that that the usual pattern is that people enact wage and price controls not as a supplement to anti-inflationary macroeconomic policy, but as a substitute for it, and therefore, we should expect that over time, the inflation rate is going to go up rather than down, and therefore long-term interest rates should go up.'… On that one, I was absolutely on Milton's side." Benjamin Friedman added that, in the initial period after the freeze, longer-term interest-rate behavior did not validate his analysis of bond-rate prospects, but that "over the period of the '70s, that [prediction] became absolutely right." (Benjamin Friedman, interview, May 10, 2013.)

An emerging critic of monetarism

Benjamin Friedman was still employed at Morgan Stanley when, on September 8, 1972, he was a discussant in the final session of a conference organized by the Federal Reserve Bank of Boston.[474] The topics with which the event was concerned were money, monetary policy rules, and the Federal Reserve. The event foreshadowed the new direction in which his career would

[474] This was a two-day (September 7 and 8) conference held in Melvin Village, New Hampshire (*Boston Globe*, September 8, 1972).

take when, shortly thereafter, he returned to academia to take his Harvard University position. The conference, titled *Controlling Monetary Aggregates II: The Implementation*, was not one that Milton Friedman attended. But Karl Brunner and Anna Schwartz did do so, while Benjamin Friedman's discussion referred to "the fixed monetary policy regime as advocated by Milton Friedman."[475]

The published volume of the conference also pointed up an issue that would recur perennially in discussions of Benjamin Friedman: the shared surname with Milton Friedman. The Federal Reserve Bank of Boston Conference volumes traditionally listed all contributors on their cover, in surname only. So, for the 1969 volume *The International Adjustment Mechanism*, the list read, in part, "CAVES–FRIEDMAN–GILBERT," while for the 1972 volume of the *Controlling Monetary Aggregates II* conference, the list included "DUESENBERRY—FRIEDMAN—GUTTENTAG."[476] In the first of these lists, the Friedman was Milton Friedman. In the second, it was Benjamin Friedman.

In light of the coincidence of names, it was inevitable that some people would believe that Benjamin Friedman and Milton Friedman were related. They were not. Benjamin Friedman recalled that "his son David was a year ahead of me at Harvard [in their undergraduate years]… and so even people who knew Milton well enough to know that he had a son, and to know something about the age of the son, and to know that the son was a Harvard undergraduate—a lot of those folks assumed that I must be the son." He added: "Milton and I once talked about it, and we concluded that there was no family relationship that we were able to establish." Indeed, Milton Friedman was emphatic on the point that he had hardly any relatives named Friedman, because his own family had adopted that name only in the previous generation.[477]

It was also inevitable that, over the years, the two would occasionally simply be mistaken for another. "I don't think he and I ever had any professional-level correspondence," Benjamin Friedman remarked of the totality of his interactions with Milton Friedman. "We never had much correspondence at all, and our correspondence was mostly over things like: he received a piece of mail that was obviously meant for me, and so he was forwarding it to me; or I was

[475] B.M. Friedman (1972a, p. 179). For the list of conference attendees, see Federal Reserve Bank of Boston (1972, pp. 185–186). It should be mentioned that the discussion that follows has little on Benjamin Friedman's very considerable interaction in the 1970s and 1980s with Karl Brunner and Allan Meltzer, or on the books and articles he published in those decades in defense of the importance of traditional, Keynesian channels of effect of fiscal policy against challenges to those channels that had been made by monetarists and by proponents of Ricardian equivalence.
[476] See Federal Reserve Bank of Boston (1969, 1972).
[477] Friedman and Friedman (1998, p. 19).

forwarding to him. Or some such thing like that." (Benjamin Friedman, interview, May 10, 2013.)

The research literature would also include instances in which Benjamin Friedman papers would be cited as publications by Milton Friedman. For example, a 1979 textbook cited a celebrated 1975 paper by Benjamin Friedman as "Milton Friedman, 'Targets, Instruments and Indicators of Monetary Policy,' *Journal of Monetary Economics*" (Boreham and others, 1979, p. 682). Many other commentators, particularly in the 1980s, were well aware that the two Friedmans were distinct monetary economists but jokingly referred to them as though they were close family relatives. An example of this occurred at a Congressional hearing on the subject of "Alternative Targets for Monetary Policy," held on July 12, 1982. Milton Friedman had no involvement with the hearing, but Benjamin Friedman was one of those testifying at the session. A panelist appearing alongside Benjamin Friedman referred to "Mr. Friedman's cousin, Milton."[478] No doubt, the many jokes made along these lines over the years were taken by some listeners to be factual statements.

A decade before this hearing, one of Benjamin Friedman's early papers on monetary policy saw print (Benjamin Friedman, 1972b). It was also one of the final research papers to give Benjamin Friedman as having a Morgan Stanley, rather than Harvard University, affiliation. In what would become a poignant coincidence, the article appeared in the same September/October 1972 issue of the *Journal of Political Economy* that would contain the symposium in which Milton Friedman's monetary views were debated. Over the fifteen years after this issue appeared, it would become apparent that Benjamin Friedman was, to a large degree, the modern successor to the critics Milton Friedman had faced in his own generation— including James Tobin (who was part of the 1972 *JPE* symposium) and Franco Modigliani (who was not). "I think it's fair to say that [I identified with] people like Tobin and Modigliani—neither of whom was at Harvard, incidentally. Tobin had gone from Harvard [long] before I got here, and Modigliani, except for one visiting year, was never at Harvard. But, to be honest with you, I always regarded myself, and I think was seen, as [in essence] a student of Tobin and Modigliani. I learned a lot from them." (Benjamin Friedman, interview, May 10, 2013.)

Informed followers of monetary policy debates in the research literature knew from an early stage that Benjamin Friedman was far from in sympathy with Milton Friedman's views on money. This fact had emerged via early Benjamin Friedman papers such as his celebrated 1975

[478] In Committee on Banking, Finance, and Urban Affairs, U.S. House of Representatives (1982d, p. 66).

article, already mentioned, that cast doubt on singling out monetary aggregates as a target for monetary policy. And, upon joining the Brookings Institution's panel of economists in 1977, he would write for *Brookings Papers* what he would remember as "the 'anti-monetary aggregates' paper in 1977" (see Benjamin Friedman, 1977).[479]

Benjamin Friedman was not, however, invited to talk at the University of Chicago's money workshop in the years in which Milton Friedman ran it (or subsequently). And Benjamin Friedman's clashes in print on monetary matters during the 1970s tended primarily to be with the rational-expectations school that used flexible-price models (see especially Benjamin Friedman, 1978) rather than with the core monetarists.[480]

The decade of the 1980s would, however, see Benjamin Friedman become one of the most prominent critics of the reliance on monetary aggregates in the formulation of U.S. monetary policy.

Filling a vacuum in monetary research

It is worthwhile putting the work that Benjamin Friedman produced during the 1982–1986 period on monetary aggregates in the context of overall U.S. macroeconomic-research activity in those years. The key point is that, over that period, the deteriorating money/income relationship was topical in public debate on monetary policy and was the subject of considerable study in the internal and published written output of the Federal Reserve System—but a notable amount of interest in this topic was not forthcoming from macroeconomic researchers at leading U.S. universities. Indeed, with the exception of research published by central bank economists and papers solicited for research conferences hosted by the Federal Reserve banks, the U.S. economic-research literature, although predominantly viewing M1 as the definition of money, was generally slow to pick up on, and concentrate on, the weakening of the relationship between

[479] Benjamin Friedman would later write (1979, p. 91): "For purposes of this memorandum[,] I take as given the use of explicit monetary growth targets in planning and executing monetary policy, although I have criticized this approach elsewhere." He then cited his 1977 paper.

[480] Contributions by the two Friedmans appeared back-to-back as the contents of a section on interest rates in the monetary-economics readings of Havrilesky and Boorman (1976). A reprint of Friedman's (1968d) article on the links between monetary growth and interest rates (with an emphasis on short-term interest rates) was placed alongside Benjamin Friedman's (1976) article on the determination of long-term interest rates. The editors of the readings volume took Benjamin Friedman's piece as complementary to, rather than offering a contrasting perspective from, the "seminal article by Milton Friedman" that preceded it (Havrilesky and Boorman, 1976, p. 361).

M1 and the economy.[481]

In fact, much of the monetary-economics work of the time followed a contrary approach. It took the correlation between money and real output as a stylized fact—with which flexible-price theories, especially the newly emerging real business cycle (RBC) theory, had to be confronted.[482] For example, the econometric studies of Bernanke (1986) and Eichenbaum and Singleton (1986) presented empirical evidence—primarily obtained using vector-autoregression approaches—on the connections between money and output, as part of a discussion of the RBC challenge. These studies used sample periods that, although they included the 1980s, were long enough that the relationship between M1 and real economic activity was still significant. Indeed, one paper concerned with the RBC view included a figure plotting detrended M1 and real output that was titled "The Growth Rates of M1 and of Real GNP Follow Each Other Closely."[483]

And all this discussion took place in an environment in which research in monetary economics was a less bustling activity than it had been from 1962 to 1981. Indeed, McCallum (1999b) and Taylor (2007) would classify the period from 1982 to 1992 as a dark age for monetary economics.

Furthermore, in 1982–1986 specifically, much of the research that was taking place in monetary analysis in the specific period was theoretical—as this period saw considerable development of New Keynesian models and cash-in-advance models. Such theoretical studies typically took a positive money/output correlation as a given empirical fact. For example, in a symposium on monetary theory, held in Hong Kong in January 1986, Bruce Greenwald and Joseph Stiglitz motivated their paper by stating that it "has long been recognized that there is an important connection between money and the level of economic activity." They affirmed, without qualification, that "recent econometric studies have confirmed the existence of a relationship between money and economic activity."[484]

[481] One exception was Thornton and Batten (1985) who, in studying money/income relations, stopped their sample period in 1982:Q3 in order to exclude some of the period of pronounced velocity decline (p. 167). This research, although published in an academic journal, was by Federal Reserve Bank of St. Louis economists, whose policy work included keeping tabs on the evolving monetary data (in part for the maintenance of the Federal Reserve Bank of St. Louis nominal-income equation), although in practice their research also involved monetary aggregates.
[482] Milton Friedman's reactions to the RBC literature are discussed in Chapter 16.
[483] See Walsh (1986a, Figure 2, p. 7). The sample period for the figure ended in 1984. In retrospect, it is clear that this was the final year for which the statement was accurate.
[484] Both quotations are from page 337 of the record of the conference in Institute of Economics (1986). They also appeared in the version of the paper in the same event's more widely-circulated conference volume (Greenwald and Stiglitz, 1988, p. 141). Stiglitz was a visiting scholar at the Hoover Institution when the paper was written

Evolving developments in monetary policy and monetary relationships were therefore largely being sidestepped by leading academic researchers. Indeed, at the January 1986 symposium, Sir John Hicks expressed disappointment that the conference discussion had not given him a window into current economic-policy developments: "I was hoping that at this conference someone would tell me what Mr. Reagan and Mr. Volcker have really been up to."[485]

Benjamin Friedman during these years was a rare instance of a high-profile researcher doing empirical work specifically on monetary aggregates, and on the implications of evolving money/income relationships for Federal Reserve policy. Departing from the common practice of 1980s monetary economics research, he put recent money/income patterns, and the implications for monetary policy, high on his research agenda during the 1980s.

The money/income relationship

In the 1970s, Benjamin Friedman's interest in looking at monetary policy using variables other than monetary aggregates was not accompanied by a denial of the existence of significant money/income relations: in 1979, for example, he acknowledged the historical stability of M2 velocity and the existence of a good relationship between variations in M2 and the economy.[486] This stand of his would change. As data for the 1980s accumulated, what Benjamin Friedman perceived as a severe weakening in the money/income relationship became one of his major and best-known areas of research.

Benjamin Friedman regarded the relationship between monetary aggregate and economic activity as having become unreliable even in 1979–1982—a proposition he advanced even during the New Operating Procedures period (see B.M. Friedman, 1982a). Then, as M1 exhibited more clear-cut discrepancies *vis a vis* nominal GNP, Benjamin Friedman was one of the leading researchers documenting the loosening—or, as Benjamin Friedman saw it, the breakdown—of relationships between money and income.

Benjamin Friedman's series of papers focusing on the subject of an empirical breakdown in the money/income relationship began with a face-to-face confrontation with Milton Friedman in a

(Greenwald and Stiglitz, 1988, p. 141), so Milton Friedman may have had some exposure to the work underlying the paper.

[485] Institute of Economics (1986, p. 607).

[486] See Benjamin Friedman (1979, pp. 93–94) and his testimony of November 27, 1979, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1980a, p. 57).

session, already mentioned, that was part of the December 1983 American Economic Association meetings in San Francisco and that was titled "Monetarism: Lessons from the Post-1979 Experiment." Milton Friedman was the assigned discussant of Benjamin Friedman's paper.[487]

In his comments, Milton Friedman seemed to provide a direct rebuttal to Benjamin Friedman's paper: whereas Benjamin Friedman had said that the 1979–1982 period refuted monetarist propositions, Milton Friedman argued that the experience in those years had confirmed them, pointing in particular to the recent strength of the relationship between M1 growth and nominal income growth. So confident was Milton Friedman on this matter that he wrote up his comments and had them published, in the May 1984 proceedings issue of the *American Economic Review*. The Milton Friedman article appeared directly after Benjamin Friedman's paper, with the same title as Benjamin Friedman's article but no other acknowledgment of it, and not identified as a comment.[488]

What Milton Friedman likely saw at the time as a devastating refutation of the Benjamin Friedman paper became instead a gift to Benjamin Friedman. Milton Friedman's article not only had, as discussed earlier, misstatements of his own prior positions. It also had hostages to fortune in the form of an emphasis on the M1/nominal income correlation and Milton Friedman's statement, also discussed above, that inflation would, because of the surge in M1 growth, likely be higher in 1983–1985 than in 1981–1983.[489] Benjamin Friedman would return to Milton

[487] Milton Friedman had earlier, at the very start of the 1980s, served as a discussant-cum-commentator regarding B.M. Friedman (1980). But the focus on that occasion was not recent monetary policy (although there was certainly some discussion of that topic). In addition, Benjamin Friedman noted: "I wouldn't call that [Milton Friedman's role] a 'discussant.' That was right when Marty [Feldstein] had first taken over the NBER, and Marty commissioned a bunch of us to write what were called 'background papers.' And then, for each area—I was for financial markets—there were two really senior folks to be the speakers, and Milton was one… What Marty wanted to do was have this come out as a book, and he wanted there to be academic substance to the book—and that was what we more junior paper writers were for. But, then, the book was supposed to be interesting because of senior people [contributing] like Milton. And I remember Pete Peterson was there, George Shultz was there, Paul Samuelson was there, Arthur Burns was there, so there were folks like that… And my recollection is that I said nothing. My view was I had written my background paper, I had said what I had to say, and then Milton spoke, and this banker spoke, and there was a discussion in which I didn't even take part. So I wouldn't describe it [in terms indicating] that he was a 'discussant' of my paper." (Benjamin Friedman, interview, May 10, 2013.)

[488] See Friedman (1984c).

[489] One element of self-restraint on Milton Friedman's part was that he did not let himself be drawn into the discussion of interest-rate behavior in 1979–1982 in Benjamin Friedman's (1984) paper. Benjamin Friedman's work on this matter was, in any event, more favorable to monetary aggregates. Notably, in a paper presented at the same economics meeting, Richard Clarida and B.M. Friedman (1984) found that short-term interest rate behavior was largely explicable in terms of the behavior of M1. They also underlined the key Milton Friedman point that liquidity effects of monetary expansion depended on variations in nominal monetary growth being translated in the short run into movements (in the same direction) of real monetary growth.

Friedman's 1984 piece many times to point out how badly events after 1983 had confounded the positions taken by Milton Friedman at the AEA session.[490]

"In the end, I was very pleased," Benjamin Friedman noted of Milton Friedman's contribution seeing print (Benjamin Friedman, interview, May 10, 2013.) "…And over the years, once that broke down, I have repeatedly taken great delight in showing people that doing the correlation in the way that Milton Friedman thought was robust and recommended leads to no correlation—or, over some sample periods, negative correlation. So I wound up being very pleased that, in effect, this led him to make [an error]. I think of Milton as an excellent and very elusive debater, but I think this led him to make a debater's error—that he put on paper, and in print, a lag structure he was willing to stand by. And, of course, it collapsed."[491]

Benjamin Friedman certainly prevailed over Milton Friedman on the issue of the strength of the M1/nominal income and M1-growth/inflation relationships over the 1980s. As has been clear from the discussion earlier in the chapter, Milton Friedman's analysis from 1982 to 1986 was often flawed and was exacerbated by the confidence that Milton Friedman attached to it and by his dismissal of alternative interpretations of the evolving data. The Milton Friedman paper published in May 1984 represented an act of hubris that put him exactly where Benjamin Friedman wanted him, and Benjamin Friedman—in a series of papers that he embarked on in earnest in 1987—quite properly subsequently seized on the erroneous predictions Milton Friedman had made, and the fragile relationships he had advanced, in the 1984 piece.

All that said, Benjamin Friedman's critique of the 1984 Milton Friedman analysis was not, on the dimension of the money/income relationship, quite as comprehensive a refutation as it might appear. This point will now be elaborated upon.[492]

Benjamin Friedman (1988a, p. 61) critically considered Milton Friedman's statement in the 1984 article that the relationship between nominal GNP growth and prior M1 growth was "unusually close" in the early Volcker years and, on the basis of 1987-vintage data, stated: "The GNP-to-lagged-M1 correlation was not 'unusually close' during 1979 Q4–1983 Q4 compared to the past… The correlation of 0.45… is essentially identical to that for the previous 79 quarters."

[490] See Benjamin Friedman (1988a, pp. 60–62; 1988b; 1990, p. 165; 1993, p. 170).

[491] As discussed previously, Milton Friedman's willingness to give a concrete empirical representation of the quarterly or monthly money/income relationship was not as uncommon as many seemed to believe, and he had done so since the 1960s.

[492] On the issues raised here, see also the related discussion in Nelson (2007, pp. 166–167).

What Benjamin Friedman overlooked was that Milton Friedman had stated that the unusually-close relationship prevailed to the period “[f]rom 1981 on.”[493] Once this aspect of Milton Friedman’s statement is taken into account, Benjamin Friedman’s challenge to Milton Friedman’s contention regarding the M1/nominal income relation under the new operating procedures is undermined.

In particular, it was shown in the previous chapter that the correlation between nominal income growth and M1 growth correlation in quarterly U.S. data over the early 1980s seems to be strongest when a simple average of the first and second lag of M1 growth is used. This is true of the 1981:Q1–1984:Q4 period, too: the correlation of nominal GDP growth and the (one-period) lagged two-quarter moving average of M1 growth is a very high 0.785. The peak M1/future-income correlation was, indeed, unusually high from 1981 onward. In contrast, the corresponding correlation was 0.531 in the eighty quarters through 1979:Q3.

But, on a sample starting in 1981:Q1, the same correlation just described falls from 0.785 to 0.339 when observations for 1985:Q1–1986:Q4 are included. And when 1985:Q1–1986:Q4 is considered in isolation, the correlation is *minus* 0.549. Therefore, Benjamin Friedman (1988a, p. 61) was on solid ground in suggesting that the bivariate M1 growth/nominal income correlation had collapsed in the 1980s. He also demonstrated (1988a, p. 62) that the 1984 Milton Friedman tabulation of a close link between averages of M1 growth and of future inflation was not resilient when new data were added, the connection having clearly collapsed in the recently-elapsed years of 1983 to 1987.

In the course of 1988–1997, Benjamin Friedman published many articles documenting the weakening of the relationship between monetary growth—especially M1 growth—and major economic series like nominal income growth, real income growth, and inflation. These included more solo-authored work (such as Benjamin Friedman, 1993). It also included joint work. Benjamin Friedman and Kenneth Kuttner (1992) became probably the best-known single piece of research on the deterioration of money’s leading-indicator role. This paper showed that the predictive power, with regard to income (nominal and real) and prices, that M1 had once enjoyed had vanished, when vector autoregressions included 1980s data in their estimation sample. The study also found that other monetary aggregates, like M2, had not weathered the past decade especially well either.[494]

[493] Friedman (1984c, p. 399).

[494] The article also suggested that real M1, real output, and short-term interest rates did not cointegrate into a long-run money demand relationship. This aspect of the paper was probably the one that was most successfully

Viewed as a critique of monetary aggregates' relationship with key macroeconomic totals, this body of research was highly effective. Viewed as a response to Milton Friedman's monetary economics, however, Benjamin Friedman's 1988−1997 work was somewhat anticlimactic after the direct debate between the two Friedmans at the 1983 AEA meetings, for two reasons. First, in much of Benjamin Friedman's work, he focused on M1, and so, frequently, he was not presenting results that directly challenged Milton Friedman's current monetary analysis, as from 1986 onward that analysis focused on M2. Indeed, having abandoning M1 as his favored aggregate in May 1986, Milton Friedman then went in the other direction. After Beryl Sprinkel had made remarks in San Francisco saying, "No one believes M1 is a good measure of what's going on," Friedman endorsed the remarks, observing: "I was particularly glad to see Sprinkel argue against fine-tuning. The relationship between the money supply and the state of the economy has always been a loose one." (*San Francisco Chronicle*, July 4, 1986, p. 33.)

Second, although aware of Benjamin Friedman's series of critiques, Milton Friedman refrained from replying to them. His sparing research on money after 1982 did not include any responses to Benjamin Friedman's writings.

Credit and the economy

Benjamin Friedman's work on post-1982 developments would harden the negative perspective he already had about monetarism. But another dimension of post-1982 financial behavior would undermine one of his own existing positions.

Benjamin Friedman was strongly oriented toward an interest in, and belief in, the importance of the economy's credit process: "the way I was taught economics at Harvard" was that "what was interesting about banks was not that they passively accepted deposits, but that they actively made decisions on whether to give people loans... So the idea of focusing on credit markets was just there from 'minute one' in my thinking." (Benjamin Friedman, interview, May 10, 2013.)

Against this background, in the late 1970s and the first half of the 1980s, Benjamin Friedman had believed that credit had a stable relationship with economic activity. Initially, he had emphasized the closeness of the relationship between commercial bank-issued credit and U.S.

challenged—see Hoffman and Rasche (1996, pp. 102−110)—as the finding may well not have been valid for the sample period (one going through the early 1990s) with which Benjamin Friedman and Kenneth Kuttner were concerned. Subsequently, however, the intensified usage, from the early 1990s onward, by commercial banks of deposit-sweeping arrangements certainly put paid to the long-run M1 demand relationship.

nominal national income. So, in Congressional testimony that he gave in November 1979, it was bank credit that Benjamin Friedman emphasized. He was willing to accept that money, specifically (old) M2, was related to the economy, but he suggested that the apparently reliable relationship between M2 and nominal GNP, instead of reflecting stable money demand relationships, might actually be a by-product of the connections between bank credit and the economy, with the balance-sheet relations between bank credit and M2 conveying to the latter series apparent informativeness about the economy.[495] As Bernanke (1986, p. 59) observed, the Benjamin Friedman position suggested that the money/income correlation "is 'really' a correlation between income and credit."

Bernanke (1986, p. 59) also noted that Benjamin Friedman had adopted an empirical approach in which "credit is defined very broadly." In particular, in the early 1980s, Benjamin Friedman turned to credit aggregates that included nonbank credit, including such items as securities issued by the U.S. government and nonfinancial corporations. The direction in which his thinking proceeded was evident in a paper he wrote for the April 1982 Carnegie-Rochester conference in which he favored the use of a broad credit aggregate in monetary policy (B.M. Friedman, 1983a).

Benjamin Friedman's position was, however, different from Henry Kaufman, who, as discussed in the previous chapter, was also an advocate of a broad credit series. Even at the start of the 1980s, Kaufman wanted the monetary aggregates consigned to oblivion in policymaking and, in an October 1980 discussion of a paper by Karl Brunner, suggested that it was futile to "try to establish targets for money" and that, instead, "We have to think about managing credit."[496] In contrast, Benjamin Friedman, although by 1982 he assessed that the money/income relationship had become unsettled, stated in his July 1982 testimony that credit should be targeted alongside money, rather than displacing monetary targets: "it would be an error at this time to switch entirely from a money target to a credit target—even if the evidence were clear that credit was better, which it is not."[497] He made the same case in a March/April issue of the Federal Reserve Bank of Boston's economic review: Benjamin Friedman's (1982a) article in the issue, although titled "Time To Re-Examine the Monetary Targets Framework," argued for augmenting

[495] See Benjamin Friedman's testimony of November 27, 1979, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1980a, p. 57). This was also the position taken by Modigliani and Papademos (1980, p. 142).

[496] Kaufman (1980, pp. 66, 68).

[497] Testimony of July 14, 1982, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1982d, p. 22). Benjamin Friedman defended having intermediate targets in the form of financial variables, as opposed to a sole focus on ultimate goals, on the grounds that the intermediate targets made monetary policy more accountable to Congress (p. 20).

monetary targets with a credit target rather than discarding monetary targets altogether.[498] But when completing a subsequent paper the following August—by which time the break in M1 velocity's trend was emerging strongly—he argued for a credit target "either together with or instead of on one of the monetary aggregates."[499]

Both in its empirical findings and its policy prescriptions, the Benjamin Friedman research on credit was clearly a challenge to Milton Friedman's past body of work on money. But the Benjamin Friedman papers varied in the extent to which they made this challenge explicit by referring to Milton Friedman by name. For example, the more policy-oriented Benjamin Friedman (1982a) article on credit targeting referred to him, but the more empirical Benjamin Friedman (1983a) Carnegie-Rochester conference contribution on the same subject did not include any reference to Milton Friedman at all.

At an earlier point of his advancing the case for credit, Benjamin Friedman had made the challenge more explicit. A paper that he produced as a draft in April 1980 and put into the NBER Working Paper series in March 1981 had as its main title "The Relative Stability of Money and Credit 'Velocities' in the United States."[500] This title evoked that of the 1963 Friedman-Meiselman paper on the relative stability of velocity and the fiscal multiplier.[501] In the

[498] Benjamin Friedman (1979) had also given this advice when he was focusing on a bank-based measure of credit. The difference between Benjamin Friedman and Henry Kaufman on the matter of retaining monetary targets was noted by F.E. Morris (1982a, p. 86).

[499] Benjamin Friedman (1983a, p. 144). This concluding passage also seemed quite receptive to leaving an intermediate-targets framework altogether.

[500] See B.M. Friedman (1981). For the April 1980 date of the earlier draft, see Modigliani and Papademos (1980, p. 154).

[501] In contrast to Milton Friedman and David Meiselman, Benjamin Friedman put "velocity" in quotation marks. This choice reflected not only the unfamiliarity of the "credit velocity" concept but also his own dislike of the term "velocity" in reference to monetary aggregates. "I think the word 'velocity' should simply be banned from the literature. Because people think it's a real thing… [and] say all sorts of things that they would never say if you forced them to use the phrase 'income-to-money ratio' instead of 'velocity.'… I don't kid you—I once was on a panel discussion with a very, very prominent monetary economist, not Milton. And the issue was why income had risen but money had not. Or maybe the opposite. But with an absolutely straight face, the guy explained to the audience that the reason income had risen and money had not was that velocity had increased, as if he was describing some kind of physical thing. And I looked at him in disbelief… People are misled by the word 'velocity' into believing that they're talking about some kind of physical process that has a life of its own, when all it is a ratio of two variables." (Benjamin Friedman, interview, July 23, 2013.) See also Benjamin Friedman (1988a, p. 58; 1990, p. 162).

Milton Friedman and Anna Schwartz associated the denial of the usefulness of velocity concepts with the views of extreme Keynesians of their generation and the previous one (see Friedman and Schwartz, 1982a, pp. 207–208). Anna Schwartz accordingly reacted sharply at the March 1984 NBER conference on business cycles when Benjamin Friedman described velocity as "only a ratio" (see Gordon, 1986a, p. 455). Friedman and Schwartz (1982a, pp. 3, 144, 208), however, also considered the ratio in terms of the "weeks of income" that were held in the form of money balances, and Benjamin Friedman (interview, July 23, 2013) indicated that he did not object to that formulation: "the money-income ratio is an aspect of people's demand for money. Well, that's very interesting, and, of course, people's demand for money should depend on their income, and so putting it in terms of 'How many weeks of event, Benjamin Friedman did not use the title in the numerous subsequent publications that he published on the properties of credit aggregates. But the underlying argument that the velocity of this credit aggregate (the inverse of its ratio to nominal income) was more stable than that of monetary aggregates was one he did press in print in the first half of the 1980s, including in a paper "Money, Credit, and Interest Rates in the Business Cycle" that he produced for the NBER's major conference on business cycles held in Puerto Rico on March 22–25, 1984.[502]

By the time of that conference, Benjamin Friedman's advocacy of credit had gained considerable traction, and a breakthrough in its use in U.S. monetary policy seemed to have occurred when, in February 1983, the FOMC added a monitoring range for total nonfinancial debt alongside its monetary targets for the year. Indeed, the *New York Times* (February 27, 1983) gave the heading "The Federal Reserve's New Monetary Policy" to an op-ed that it invited Benjamin Friedman to write on this move.

Benjamin Friedman's op-ed was subtitled "Applause for a Credit Guideline." The title notwithstanding, the use of a credit monitoring range was not altogether new in 1983. Alongside its monetary-aggregate targets, the FOMC had, up until this point, published annual ranges for growth in commercial bank credit. But these bank-credit numbers had tended to have a low profile. The use of monitoring ranges for the broader debt total that began in 1983 replaced this practice. The 1983 switch, in turn, raised the prospect of credit-like aggregates having a more prominent role in U.S. monetary policy—especially as it was occurring while M1 behavior was still being deemphasized by the FOMC.

Within the Federal Reserve, there were enthusiasts for the debt (and so *de facto* credit) aggregate like Federal Reserve Bank of Boston president Frank Morris. But at the Federal Reserve Board in particular, the attitude of governors and senior staff members tended to be more circumspect. Stephen Axilrod, as already discussed, represented the Federal Reserve at the June 1983 Bank of Japan conference at which Milton Friedman was also a speaker. A high official in Japanese economic policy asked Axilrod, in light of the FOMC's adoption of the credit aggregate in its list

income does somebody want to hold in the form of money balances?,' defined however—that's an interesting economic phenomenon, and one that lots of us have worked on, including me… [and], in the first instance, the variable on the left[-hand side of the equation] that we're trying to explain is the money holdings—whereas, when people use the phrase 'velocity,' with [nominal income] on the top, the mechanical relationship that they implicitly have in mind is that money is the driving variable, and it's the income [variable] that's responding in some mechanical fashion. And that's what I'm resisting. Phrasing it as [weeks of income], so that we're now talking about the demand for money, where money is an endogenous variable, strikes me as a very—not just legitimate, but important, part of macroeconomic analysis. I've written a fair number of papers on the demand for money."

[502] See B.M. Friedman (1986).

of targets, "'Is Ben Friedman the most important economist in the U.S. at this point?'… I told him no, he wasn't the most important economist." Axilrod viewed the credit target as a token concession to Frank Morris and outside observers: "Why did we include it? It was just Paul's way of 'defusing' people [who were] thinking about bank [and other] credit. [He could say to them:] 'We're not ignoring it. We pay decent attention to it.' Some people might have, but those of us who were assigned the problem of trying to figure out policy paid no attention whatsoever to it. That might have been right, that might have been wrong, [but] we paid it no attention." (Stephen Axilrod, interview, April 24, 2013.)

And even publicly, Paul Volcker indicated that the credit aggregate would have to earn its place: "the credit range during this experimental period does not have the status of a target," he stated, while adding that "the Committee does intend to monitor developments with respect to credit closely for what assistance it can provide in judging appropriate responses to developments in the other aggregates [that is, the targeted money series]."[503] Volcker also noted that broad monetary policy tools did not "influence closely total flows of credit." As discussed in Nelson (2020a, Chapter 6) and in the previous chapter, the point that monetary policy could affect money more dependably than aggregate credit was one Milton Friedman repeatedly advanced over the years.[504]

For his part, Benjamin Friedman applauded the adoption of a credit (or debt) guideline, but he urged that the new series be interpreted pragmatically in policymaking, and he indicated that he would not necessarily object in the event that the stated growth range for 1983 was missed (*New York Times*, February 27, 1983).

As it turned out, caution and pragmatism about the credit series were amply justified, because the credit/national income relationship loosened drastically, and very rapidly, starting in 1981–1982—and did so on a scale that eclipsed the deterioration in the relationship between monetary aggregates and income. The sharp decline in the "velocity" of credit was in process even in 1981, preceding M1 velocity's decline, so there was no window of time in which the monetary aggregates' velocities were breaking with historical patterns and credit velocity was not. As indicated above, Benjamin Friedman had once suggested that M2 velocity's historical

[503] From Volcker's testimony of February 16, 1983, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1983b, p. 16).
[504] The Federal Reserve Bank of New York's veteran monetary specialist, Richard Davis, was also one who doubted whether broad credit aggregates were reliably related to the central bank's traditional open-market and reserve-managing tools (see Davis, 1979, pp. 21–22).

stability might have been a property inherited from the stability of credit velocity. But in the 1980s M2 velocity was considerably more stable than the velocity of credit or debt. For example, Rasche (1990, Table 1, p. 119) found on monthly data that M2 velocity showed a decline in its mean annual growth rate of 2.1 percent between 1953–1981 and 1982–1985, whereas a debt aggregate's velocity showed a decline of 4.5 percent from 1956–1981 to 1982–1985.

The growth in the debt aggregate actually fell within the FOMC's monitoring range for 1983, which was the only year after 1981 in which the ratio of credit or debt to income was fairly flat.[505] Indeed, Benjamin Friedman (1983b, p. 88) contended that "data [on the debt/income ratio] for the first half of 1983 already show the beginning of a return to the historical norm." But, in fact, the ratio never returned to its prior historical norm. Correspondingly, debt growth subsequently overshot the authorities' monitoring range in 1984, 1985, and 1986—with the realized rate exceeding the top of the FOMC's range each year, by 1.4 to 3.3 percentage points.[506] Testifying in February 1986, Paul Volcker named M1 and debt as the "policy guideposts" that had behaved most differently from ultimate economic totals.[507]

Benjamin Friedman himself noted the "manifest failure of the credit aggregate to perform satisfactorily" (B.M. Friedman, 1988a, p. 63) and, in an article that had already documented various breakdowns in the M1/income relationship, plotted the very sharp break in the ratio of credit to income (p. 64). His disillusionment with credit, on top of his distrust of monetary aggregates, was reflected in a title that he chose for another paper that appeared in 1988: "Monetary Policy Without Quantity Variables."[508]

As already discussed, during the mid-1980s Federal Reserve Bank of Boston's president, Frank Morris, publicly pointed to the decade's empirical problems with M1 as an indication of the flaws in Milton Friedman's policy prescriptions. Over the same period, however, he also went out of his way to note that Benjamin Friedman's work on credit, too, had been upended by recent

[505] With regard to debt's monitoring range and outcome for 1983, see, for example, Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1984a, p. 34), as well as Argy, Brennan, and Stevens (1990, Table 1, p. 54). See F.E. Morris (1985, Chart 3, p. 7; p. 229 of 1987 reprint) and B.M. Friedman (1988a, Figure 4, p. 64) for plots of the debt-to-income series.
[506] See Argy, Brennan, and Stevens (1990, Table 1, p. 54).
[507] Volcker (1986, p. 235).
[508] See B.M. Friedman (1988b). This, of course, would be a theme developed in the New Keynesian literature in the 1990s and 2000s, including by Woodford (2003). (F.E. Morris, 1983b, had given a speech under the prescient title "Monetarism Without Money." But the substance of the speech had been a call for targeting credit. The speech did not come out in favor of a monetary policy framework that eschewed reference to financial quantities.)

events. "No sooner had the FOMC adopted a monitoring range for debt that it became apparent that the very stable relationship which Benjamin Friedman of Harvard had found… had gone off the track," Morris noted in his September 1985 speech on monetary rules.[509] Morris' recriminations on the matter were heightened by his having had a public stake in the issue. In March 1982, Morris had made a speech calling for the FOMC to adopt a credit target (see F.E. Morris, 1982a), and the 1983 FOMC inclusion of a debt monitoring range had been seen as a win for him. Speaking to his Committee colleagues on July 9, 1986, Morris remarked: "Ben Friedman sold the debt/income relationship to me; and then it immediately went [off track]."[510]

Milton Friedman had been surprised by Benjamin Friedman's favorable findings about the stability of credit velocity. Should such a finding endure, Milton Friedman conceded, it was important: "some years back, when Benjamin Friedman first raised this, my reaction was: 'Well, if a credit aggregate works better—let's see if it does work better. We've got evidence for a money aggregate for a long time—we don't have evidence for a credit aggregate.'… This is science, it's not religion, and every hypothesis must be regarded as tentative. You mustn't be afraid to put it to the test. And if Benjamin had been able to come up with a credit aggregate that had a more reliable relationship to nominal income than the money aggregate[s], then that would raise real questions about the validity of the theoretical basis for believing that the monetary aggregates were [central]." He suggested that this might mean moving to an investigation of demand functions pertaining to credit. (Milton Friedman, interview, January 22, 1992.)[511] Such an investigation would itself have amounted to a departure from Milton Friedman's thinking, in part because he had tended to view the demand for credit as unstable.

But the post-1981 behavior of credit velocity, and other breakdowns in the credit/income link, had subsequently cast doubt on the resilience of the relationships that Benjamin Friedman had been emphasizing. "I believe—I'm not sure, I haven't followed it closely, but my impression is that the credit aggregate has [in recent years] proved to be utterly useless from the point of view of predicting nominal income." (Milton Friedman, interview, January 22, 1992.)

Much of Benjamin Friedman's research was on the problems of monetary policy in the 1980s in the form of the deteriorating performance of monetary and credit aggregates. In terms of actual economic outcomes, however, he shared the view expressed by Paul Samuelson above that the

[509] F.E. Morris (1985, p. 5; p. 226 of 1987 reprint).
[510] In Federal Open Market Committee (1986, p. 54). The bracketed words were supplied as part of the official transcription.
[511] As Offenbacher and Porter (1983, p. 2) stressed, the U.S. demand function for credit was far less well studied than that for money.

Volcker Federal Reserve had clearly produced good results in recent years, and he opened a 1988 article by noting that “monetary policy was a distinct success” in 1982−1987 (B.M. Friedman, 1988a, p 51).[512] But Benjamin Friedman did not share Samuelson’s enthusiasm in casting the period as an example of highly discretionary policymaking that could be successfully repeated in the future. Instead, he closed the 1988 article by criticizing the absence in the FOMC’s public statements of an outline of the framework linking the “information base to either the policy instrument or the policy objectives.”[513] As it happened, important aspects of the Committee’s basic economic framework could be discerned by consultation with the public record—by consulting the numerous public statements made by Volcker and other Federal Reserve officials as well as the FOMC’s regularly published policy records.[514] Benjamin Friedman’s suggestion that policymakers had not made a clear-cut articulation of their framework was nevertheless unquestionably valid: indeed, the FOMC was still not even acknowledging publicly in an authoritative document that it was managing the federal funds rate.

At a conference held in November 1988, Bennett McCallum likewise lamented the inadequacy of public information provided by the Federal Reserve on its monetary policy strategy. He noted that, in light of the clearly diminished role given to monetary aggregates by the FOMC since 1982, “most would agree that a need exists for a more explicit and coherent strategy for the conduct of monetary policy. Many would even subscribe to the recent contention by Friedman that ‘there is now a conceptual vacuum at the center of the U.S. monetary policymaking process.’ The foregoing quote is unusual, incidentally, as it seemingly could have as easily come from Milton Friedman as its actual author [that is, Benjamin Friedman].’”[515]

OTTO ECKSTEIN

“Dr. Eckstein… is a young, bubbling, enthusiastic, able fellow,” Paul Samuelson remarked when, in 1973, he provided the subscribers to his audiotaped commentaries with a word-picture of Otto Eckstein (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 142,

[512] In contrast, and as discussed in previous sections, Milton Friedman’s praise was far more grudging and qualified. Much later, in 2002, he acknowledged that, on the criterion of economic outcomes, there was “no doubt [that] the major central banks have performed much better since about the middle of the 1980s than they did before.” (In Pringle, 2002, p. 18.) In keeping with Milton Friedman’s reluctance to give much credit to Paul Volcker, this formulation permitted an interpretation under which the period since the mid-1980s could be taken as the years since 1987—and so, possibly, referring only to the Greenspan years.

[513] B.M. Friedman (1988a, p. 70).

[514] Benjamin Friedman was writing before the appearance of the study by Romer and Romer (1989) of Federal Reserve statements, including those made in the 1980s.

[515] McCallum (1989b, p. 1). The cover page of this paper gave the date of the conference for which it was commissioned as November 14–15, 1988.

November 29, 1973). Samuelson's description of Eckstein as "young" was, from Samuelson's perspective, no doubt justified: Eckstein was over a decade younger than Samuelson (and almost exactly fifteen years younger than Milton Friedman).[516]

It was, nevertheless, a jarring, if flattering, label to give to such a well-established economist. As of 1973, Eckstein was a veteran member of Harvard University—one of Benjamin Friedman's most senior colleagues in the economics department. He had been an undergraduate student in economics at Princeton University in the same cohort as Gary Becker. That situation led to Friedman hearing about him for the first time, as recommendation letters sent to the University of Chicago and Harvard University had indicated, Friedman remembered in 1989, that "these were [by] head and shoulders the two best students in the past twenty years or something like that." Consequently, he explained, each of the universities had offered graduate fellowships to Becker and Eckstein alike. "Becker chose to come to Chicago and Eckstein chose to go to Harvard."[517] Friedman added that he had always wondered what would have been the outcome if each had made the opposite decision.[518] In the event, Eckstein became a leading Keynesian as a member of Harvard University's economics department as soon as he received his Ph.D. from the same department, in 1955.[519]

But Samuelson's feeling that Eckstein was youthful may have been heightened by the other attributes that he associated with him—"bubbling" and "enthusiastic." Eckstein's prolific activities amply attested to Samuelson's labels. By the 1970s, he was managing multiple roles as a teacher, researcher, and business and policy adviser.

In a biographical summary that Eckstein wrote in the early 1980s, Eckstein gave his research areas as: "Econometric models, forecasting, policy analysis."[520] The interest in economic policy implied by this list had been prominently manifested by Eckstein even in 1958 when—like Milton Friedman—he published a chapter in a volume of invited contributions in the congressional volume *The Relationship of Prices to Economic Stability and Growth*.[521] After

[516] Eckstein's date of birth was given as 1926 in Blaug and Sturges (1983, p. 103) but as 1927 in many other sources, including American Economic Association (1981, p. 131). The *New York Times* (March 23, 1984) gave Eckstein's date of birth as August 1, 1927—fifteen years and one day after Friedman's birthdate.

[517] Hammond (1989, p. 42).

[518] Hammond (1989, p. 42).

[519] See Blaug and Sturges (1983, pp. 103–104). Eckstein's activities early in his period as a member of the department included co-teaching, with Gottfried Haberler, a graduate course on business cycles. See Lovell (1986, p. 105).

[520] American Economic Association (1981, p. 131).

[521] See Eckstein (1958).

much further engagement in analysis and commentary regarding current macroeconomic issues, Eckstein served as a member of the Council of Economic Advisers in the Johnson Administration, from 1964 to 1966.[522] Later, a telling slip that revealed the synergies of his research and policy experiences—and the close association that the Kennedy and Johnson Administrations had enjoyed with the economists of the main Cambridge, Massachusetts, universities—occurred when Eckstein gave his current affiliation as: "member, council of econ. advisers, Harvard U." (American Economic Association, 1970, p. 119.)

By 1970, Eckstein had begun engaging in the third, and ultimately most prominent, aspect of his career in economics. Eckstein was what Samuelson later described as "the founder and sparkplug" of what became a leading economic forecasting-and-advice unit (*Financial Times* (London), December 30, 1978). Specifically, in 1969, Eckstein established a macroeconometric modeling firm, Data Resources Incorporated, based in Lexington, Massachusetts, taking the role of firm president (American Economic Association, 1981, p. 131). In 1978, a *Wall Street Journal* profile of Friedman discussed the high income that he was receiving from speaking fees—quoting him as saying, "The free market has worked very well for me"—and pointed to the financial benefits ahead if the project that became *Free To Choose* proved to be a success. In listing other academic economists who had also found it lucrative to branch out, the article cited John Kenneth Galbraith's success as a popular author and broadcaster and the fact that Paul Samuelson's textbook had made him a millionaire, while also noting that "Harvard's Otto Eckstein has prospered" through the economic-consulting services of DRI (p. 27).

"I worked with Otto for 12 years at DRI," Allen Sinai recalled (interview, May 7, 2015). "I came there in the fall of 1971, very early in the DRI history—the only person from outside the East Coast, by the way. I always wondered why Otto and DRI hired me. I think they just saw somebody who didn't mind working 20 hours out of 24 hours for them. They got a real patsy there, and I indeed did that. But Otto—I was so lucky to have worked with him and watched him operate."

The centerpiece of DRI's consulting was the DRI model. "The Data Resource Economic Information System has been [my] main tool," Eckstein observed (Blaug and Sturges, 1983, p. 104), "and is also used by various public, private[,] and academic groups for macroeconomic analysis." Sinai remarked (interview, May 7, 2015) that Eckstein embedded the model with his own worldview: "he *was*—Otto—the DRI model, [although] I was [also] a major contributor to

[522] Blaug and Sturges (1983, p. 104).

and author of parts of that. It was really very structural, large-scale. It grew out of the Klein-Goldberger structural approach. And that was Otto's [approach], which is very much not Chicago."

"And, of course, the company did very well financially," Sinai noted (interview, May 7, 2015). This fact was highlighted spectacularly in July 1979 when Eckstein agreed to an offer to sell the firm, whose shares were traded in the stock market. "Since he built the country's largest economic forecasting and consulting firm, Data Resources, and sold it to McGraw Hill, his personal share of the profits [has] made him perhaps the world's richest economist," Louis Rukeyser observed on one of the occasions when he had Eckstein as a studio guest (*Wall Street Week*, Maryland Public Television, July 23, 1982, p. 9 of transcript). McGraw-Hill purchased the shares for an amount initially given as $101 million (*Fort Myers News-Press* (Florida), July 14, 1979) but that ended up being $103 million (*New York Times*, March 23, 1984). "Professor Eckstein hadn't put up any of his own money [in setting up DRI]," Allen Sinai observed (interview, May 7, 2015), "and his return, and his family's return, was $20 million of the sale price—on an investment of next to nothing, other than the sweat that he put in. That marks Otto down as one of the great entrepreneurs of the time, if you measured things in terms of return on the capital."[523]

Yet, as Louis Rukeyser noted in his 1982 introduction, Eckstein had stayed on as DRI's head.[524] He also kept his Harvard University position and his longstanding teaching role there: "He ran a principles course at Harvard, and [as the course text] they used Samuelson's book," Allen Sinai observed (interview, May 7, 2015.) "Otto Eckstein doesn't have to work anymore," Rukeyser remarked in his 1982 introduction. "So he must get some pleasure out of it."[525] Confirmation of this assessment, beyond Eckstein's continuation of his DRI and teaching responsibilities, lay in the fact that he continued to publish in journals and other research outlets throughout the years in which his consulting and forecasting activities thrived.

But, despite his status as a leading U.S. Keynesian in academia and policy circles, Eckstein did not have a direct debate in print with Friedman. Allen Sinai remarked regarding Eckstein, "He

[523] See also *The Bellingham Herald* (Washington state), April 2, 1984.

[524] "Chairman" was the title that Rukeyser gave for Eckstein's DRI role in 1982. As already noted, Eckstein as of 1981 named his position as president (see also Eckstein, 1979, p. 75). Sinai (1986, p. 35) laid out Eckstein's DRI title as "chief executive officer, president[,] and chief economist." Eckstein relinquished the role of chief executive officer in 1981 (*New York Times*, March 23, 1984). Subsequently, he gave his title as chairman (see Eckstein, 1983b, p. 138).

[525] *Wall Street Week*, Maryland Public Television, July 23, 1982, p. 9 of transcript.

was *not* involved in those debates [in the sense that] he did not go head-to-head." Sinai attributed this both to Eckstein's personality and his other commitments: "he stayed out of arguments, so it wasn't Otto's thing to get into an academic debate. He published a lot, [but] it wasn't the kind of stuff that people went back and forth on. It just wasn't him. He had a lot of other things to do: He was at Harvard, but he was [also] very active in Washington, and then along came the DRI thing, which became a huge enterprise, doing science and applications of science, macro science in the real world. We think we pioneered that, pioneered, you know, applied macro—large-scale macroeconomic modeling, building on others' principles, and made a huge impact on the real world." (Allen Sinai, interview, May 7, 2015.)[526]

Sinai stressed that, in some respects, the DRI model itself amounted to a rejection of the monetarist approach: "Look, when we did macroeconomics, there was nothing like the way Chicago—University of Chicago looked at the macro world, so he was not simpatico with the Friedman approach. But he just didn't fight about it." (Allen Sinai, interview, May 7, 2015.) This was also the kind of challenge to monetarism that was least likely to prompt a detailed response from Friedman, who eschewed large models. In 1984, at a gathering that included Friedman, Robert Heller referred to "the Data Resources Incorporated (DRI) model that virtually every one of us in this room nowadays uses to do our economic forecasting."[527]

Robert Heller's generalization certainly did not apply to Friedman. In line with his longstanding preference for smaller-scale analytical approaches, Friedman had observed in 1981 that the argument against the Reagan tax cut then being made publicly by leading Keynesians—that the tax cut would generate inflation—could be expounded on "simple Keynesian grounds" without a complicated apparatus, but that he suspected it was one that the Keynesians in question had reached on the basis of policy experiments "no doubt expressed in the form of a 50- or 60-equation econometric model" (*Newsweek*, July 27, 1981).

Despite the absence of a debate between them in research journals, Eckstein's and Friedman's mutual interest in the practical policy implications of economic analysis did lead them to be occasional opposite numbers in policy forums. For example, both were among those present as panelists at the Federal Reserve Board's economic-consultants meetings of June 22, 1973, and December 12, 1974.

[526] Eckstein and Sinai (1986, p. 59) did briefly cite Friedman and Schwartz (1963a) in the context of explanations for the Great Depression. Elsewhere in the article, however, a reluctance to delve into the chapter and verse of the Keynesian-monetarist debate was evident, with no references provided for the statement (p. 75) that "there has been controversy over whether monetary policy has stabilized or destabilized the business cycle."
[527] In Heller and others (1984, p. 16).

Moving to the natural rate hypothesis

In addition, as a leading macroeconometric modeler in the 1970s, Eckstein could not fail to react to, and take a stand on, the natural rate hypothesis. He started out as part of the Keynesian resistance to that challenge to the 1960s vintage of the Phillips curve. But, as now will be discussed, and consistent with Allen Sinai's characterization of the DRI model's ongoing development as a process of, in part, "building on others' principles," Eckstein became a convert to the vertical-Phillips-curve notion by the end of the 1970s.

Eckstein's name would be forever associated with the topic of "The Econometrics of Price Determination," on account of his being the editor of a 1972 book, composed of contributions to a 1970 Federal Reserve Board research conference.[528] The book received immortality, thanks to the inclusion in it of one of Robert Lucas' papers on the natural rate hypothesis. Indeed, it was in the context of a citation of Lucas' paper that Eckstein received his only mention in Friedman and Schwartz's *Monetary Trends* volume in 1982.[529]

The econometrics of price determination was, however, also a longstanding theme of Eckstein's own work. As he put it, when citing areas common to his early career and his DRI years: "Inflation analysis has also been a continuing topic, beginning with econometric papers on wages, productivity and prices."[530] Apart from the wage/price linkage that Eckstein mentioned, he was involved also in investigations of the Phillips curve relationship between inflation or nominal wage growth and the unemployment rate or output.

Eckstein was an important representative in the 1960s and the early 1970s of a position that would be superseded in modern inflation analysis. He was a major proponent of a permanently nonvertical Phillips curve. In the early 1980s, Eckstein was still listing among his major articles a study with Gary Fromm called "The Price Equation."[531] This article had appeared in the *American Economic Review* in December 1968. It therefore was one of the final articles for the year in that journal, which had begun the year by publishing Friedman's "The Role of Monetary Policy." Unlike Friedman's article—which it did not cite—the Eckstein-Fromm analysis

[528] See Eckstein (1972). Friedman did not attend the conference.

[529] See the citation of Lucas (1972b) in Friedman and Schwartz (1982a, p. 446). For his part, Eckstein had cited much Friedman work over the years, beginning with his citation of the Friedman-Savage (1948) microeconomic study in Eckstein (1957).

[530] Blaug and Sturges (1983, p. 104).

[531] See his selected list of articles in Blaug and Sturges (1983, p. 104).

concluded that there was a “long-term average Phillips curve of the economy” under which full employment would require some inflation (Eckstein and Fromm, 1968, p. 1182).

Two-and-a-half years on, at a conference in Germany in late June 1971, Eckstein now acknowledged the existence of the natural rate hypothesis but reaffirmed his belief that, although “the monetarists would deny” it, the appropriate statement to make about the long-run tradeoff was: “Even if we had perfect monetary and fiscal policies, it would still be true in the United States… that we would not succeed in having both price stability and full employment.”[532]

This attitude was manifested in what Eckstein at the same conference jokingly called his “rosy” attitude to incomes policy (Giersch, 1972, p. 138). As a CEA member, Eckstein had been an advocate of the Johnson Administration’s use of wage-price guideposts. Such a policy, U.S. Keynesians of the time believed, could help achieve price stability and full employment simultaneously. Their contention was that, absent an incomes policy, a tradeoff between the goals would have to be made, in light of the positive-mean cost-push pressures in the U.S. economy.[533] Eckstein continued to make this case after he left the CEA—as was clear when he and Fromm described a major source of the cost-push forces as “the inflationary bias in our industrial price-setting mechanism” (Eckstein and Fromm, 1968, p. 1182).[534] Indeed, in a report produced for Congress, Eckstein and Brinner (1972, p. 2) attributed the stagflation seen under the Nixon Administration in the early 1970s to the lack of an incomes policy until August 1971.

Eckstein never really departed from his view that much of U.S. inflation reflected autonomous forces. But he had also consistently suggested that another portion of inflation was sensitive to the output gap or unemployment gap. And, with respect to the gap/inflation connection, Eckstein after 1972 dropped his traditional position that there was a permanently nonvertical Phillips curve. His exposure to the accumulating data led to changes in the DRI model’s

[532] Giersch (1972, p. 17). This conference volume did not give the specific date on which the event had been held. This omission was likely motivated by a desire to make the book appear current even though it recorded proceedings that predated the momentous Nixon changes in economic policy in August 1971. The conference can, nevertheless, be pinned down as having occurred on June 20, 1971—the day before a wider conference featuring the same participants (Bergedorfer Gesprächskreis, 1971).

[533] For further discussion of their position, see Nelson (2020a, Chapter 10). Eckstein’s belief in a long-run inflation/output tradeoff was, of course, consistent with a belief in an absence of a long-run tradeoff between inflation and *economic growth*. He was long on record as believing that “as a long run phenomenon, there is no historical association between growth and inflation.” (Eckstein, 1958, p. 361.) He reaffirmed in June 1971 (Giersch, 1972, p. 135): “We find in the long run a result [that]… the system heads for a long-rate growth of potential” in which monetary and fiscal policies initially affect real variables but “then gradually the real effect converts into nothing but price.”

[534] Similarly, on the side of wages, Eckstein (1958, p. 367) referred to “the steady upward thrust in wages, due to the institutional nature of collective bargaining.”

specification over time—part of a broader pattern among modelers over the 1970s that Friedman described in 1978, and repeated in his major 1987 *New Palgrave* entry, as one in which "every major econometric model is always being sent back to the drawing board as experience confounds it."[535] In the case of Eckstein, the change in his thinking was registered in his 1979 remark that "it is impossible to run the economy in the excess demand territory in the long run. The public's inflation expectations gradually adjust to the correct levels."[536]

Core inflation and the output costs of disinflation

In 1974, with Eckstein's model seemingly evolving in a direction closer to his own perspective, Friedman seemed agreeable to conditioning on the estimates of the unemployment cost of a hypothetical disinflation as presented by Eckstein at the White House economists' conference.[537] Yet in July 1980, when a recent Eckstein/DRI study commissioned by Congress was mentioned to him, Friedman dismissed its estimates of the real cost of disinflation, as well as the similar numbers that had been offered by Arthur Okun.[538]

What had happened? Essentially, over the second half of the 1970s, Eckstein's thinking on the inflation/unemployment relationship had paralleled, albeit with differences in both terminology and substance, those that Arthur Okun made over the same period: Both economists had moved toward a long-run-vertical Phillips curve—but each had also downgraded the response of inflation to slack, compared with the response estimated as having prevailed in the 1960s.[539] In Eckstein's case, his Phillips-curve specification had long-run vertical properties. But—in contrast to the view he had once held that nominal wage growth, and with it inflation, responded "quite drastically" to the same year's labor market conditions (Eckstein, 1958, p. 367)—the Eckstein of the late 1970s assessed the tradeoff over time spans of five years or less to be very bleak, with a large amount of lost output and employment required to get inflation down even by a small amount: "It takes forever, and the unemployment that goes with it is very high."[540] Therefore, Eckstein, although he remained more definite than Okun was (by 1980) that inflation *did* respond to a negative output gap, considered that response to be very small over periods even of five years, although it cumulated over that period and beyond.

[535] Friedman (1978d, p. R–184; 1987d, p. 15).
[536] From Eckstein's testimony of April 30, 1979, in Joint Economic Committee, U.S. Congress (1979, p. 24).
[537] See the discussion in the session of September 5, 1974, in Council of Economic Advisers (1974, p. 308). See also the discussion in Chapter 5 above.
[538] See the previous chapter. The Eckstein/DRI study was Eckstein (1980),
[539] On Okun, see Chapter 5 above.
[540] From Eckstein's remarks on *Wall Street Week*, Maryland Public Television, July 25, 1980, p. 14 of transcript.

Another important factor was that, by 1980, Eckstein had formalized the notion that a significant portion of inflation was not susceptible to the influence of aggregate demand policies at all. As he put it, "more recently core inflation" had been the variable on which he had focused (Blaug and Sturges, 1983, p. 104). This focus was brought out in the back-to-back books of Eckstein (1980, 1981).

In reviewing Eckstein's (1981) book on the subject, Parkin (1984a, p 362) complained that "the theory of core inflation [as expounded by Eckstein] is nothing other than the conventional expectations-augmented Phillips curve under a new name." There was substantial validity in this contention—but it would be wrong to conclude that, quantitatively or qualitatively, Eckstein's model had the same implications as Friedman's exposition of the expectations-augmented Phillips curve. On the quantitative dimension, as already indicated, the output-gap slope of the expectations-augmented Phillips curve was, in Eckstein's assessment as of 1980–1981, much smaller than what Friedman considered realistic. And on the qualitative dimension, by applying Phillips-curve analysis to core inflation and not total inflation (that is, core plus non-core rates), Eckstein was, in effect, consigning a sizable portion of prices to a category largely or wholly resistant to the level of aggregate demand—although, like Okun, Eckstein did imply that, among non-core prices, commodity price changes were sensitive to the *growth rate* of aggregate demand.[541]

The notion of a core inflation rate that abstracted from short-run influences was not anathema to Friedman, who had himself used the term "underlying hardcore inflation" (Instructional Dynamics Economics Cassette Tape 159, December 1974).[542] But the Eckstein notion that the non-core rate invariably simply added to the mean overall inflation rate, for given settings of aggregate demand policy, was at variance with Friedman's framework, on two counts. First, with regard to such items as food and energy, Friedman did not believe that price changes were immune to downward pressure due to economic slack. Second, he believed that insofar as energy prices, for example, *did* rise autonomously, monetary restraint could ultimately still steer the longer-term mean inflation rate and deliver a long-run combination of full employment and price stability. This was because higher energy prices drained purchasing power and so meant an intensified restriction on demand, and hence greater downward pressure on price increases, in the non-energy sector. This position, relying on traditional arguments against cost-push inflation,

[541] See Eckstein's testimony of April 30, 1979, in Joint Economic Committee, U.S. Congress (1979, p. 24).

[542] See Wynne (2008, pp. 207–209) on the development of the "core inflation" concept in the economic literature. As that discussion makes clear, Eckstein did not coin the term "core inflation," as was sometimes claimed (for example, in *Washington Post*, March 23, 1984).

was one to which Friedman had appealed after the first oil shock (see Chapter 3), and which the Friedmans restated in lucid fashion in their 1984 *Tyranny of the Status Quo* book.[543] It was also one that Friedman underlined when, catching up, post-heart attack, on reading material, he found that an article in the August/September *Federal Reserve Bank of St. Louis Review* (Hafer, 1984) seemed to treat the mean rise in a price index that excluded food and energy as independent of the increase in the prices of food and energy. Friedman wrote to Hafer on November 21, 1984, to indicate that he considered this approach to be in error.

The Volcker Federal Reserve, too, showed signs of believing that it was the total inflation rate, not the core rate, that was the responsibility of monetary policy. For example, on raising the discount rate by 100 basis points in February 1980, the Federal Reserve Board issued a statement indicating that it was "particularly concerned that recent economic developments, including the large increase in the price of imported oil, are adding to inflationary pressures" (*The State* (Columbia, South Carolina), February 16, 1980).[544] And in July 1981, Volcker rebuffed the interpretation that the major contribution of slower growth in food and energy prices to the recent step-down in inflation meant that monetary policy had not played an important role. He insisted that these prices were sensitive to monetary policy actions.[545]

In seeing reductions in aggregate demand as giving little benefit in terms of lower inflation for long stretches of years, Eckstein as of 1980 had common ground with other Keynesians closely associated with 1960s economic policy, like Arthur Okun and James Tobin. But he had parted ways with them in another respect, as over the 1970s Eckstein had become jaded about the practice of demand management. As of June 1971, his main criticism of *past* economic policy was the standard Keynesian position that taxes should have been raised in 1966. For *present* policy (again, as of June 1971), he urged the Nixon Administration to adopt fiscal stimulation (see Giersch, 1972, p. 12). By 1980, in contrast, his position was: "We have learned the bitter lesson that aggressive policies against recession have been among the principal causes of the present economic impasse."[546]

Eckstein had also, by 1980, upgraded the importance of expanding the supply side of the

[543] See Friedman and Friedman (1984, pp. 83, 89; 1985, pp. 83–84, 90).
[544] This was an example not only of the Volcker Federal Reserve's concern with headline inflation series, but also of its practice, even before 1982, of basing its policy responses partly on the behavior of the ultimate objectives of monetary policy, even while pursuing announced monetary targets.
[545] See Volcker's testimony of July 22, 1981, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1981b, p. 96).
[546] From Eckstein's testimony of May 21, 1980, in Joint Economic Committee, U.S. Congress (1980d, p. 24).

economy and doing so by improving incentives. "The supply-side viewpoint really now enjoys an enormous range of support," Eckstein remarked in the middle of the year. "People understand that you cannot keep on beating on business through regulation and rising taxes and still get that productivity advancing."[547] Both during the 1980 election campaign and later, Eckstein was critical of many parts of the Reagan economic program, including its choice of tax cuts.[548] But his endorsement of the need to boost supply by increasing incentives was so prominent that, in 1983, a fervently pro-supply-side-economics book, *The Supply-Side Solution*, reprinted his 1980 testimony on tax policy.[549]

Eckstein put together his belief in supply-side effects and his low estimate of the response of inflation to economic slack by recommending, in 1980, that the authorities adopt a policy that raised the level of potential output but that did not let actual output move up to this higher level (Eckstein, 1980, p. 49). Allowing this constant output gap to last five years would reduce core inflation ultimately by 2.3 percent, he suggested, although he judged that "even under that approach, core inflation remains far higher for most of the 1980s than it was just a few years ago." After a fashion, therefore, Eckstein was recommending an output-gap solution to inflation, but one implemented by letting aggregate demand grow along a path parallel to, but below, the potential-output path, rather than by actually reducing aggregate demand.

The disinflation and monetarism

Instead, of course, the U.S. authorities relied on considerable restriction of aggregate demand. In a paper prepared as the opening contribution to the March 1984 NBER conference on business cycle conference, Eckstein and Sinai (1986, pp. 46–47) remarked: "The recessions of 1980 and 1981–82 were worsened by the policy decision to accomplish a massive disinflation by maintaining a condition of severe credit restraint deep into the recession." As written, this did not sound like much of an endorsement of Paul Volcker's record in his first three years in office. But, in a sense, it was, as the authors' reference to a "massive disinflation" amounted to an acknowledgment that aggregate demand restriction could generate a fall in inflation of a much greater magnitude, more promptly, and at less short-term cost than Eckstein had previously assessed. Inflation had fallen steeply: at one end, the total 12-month CPI rate of increase fell

[547] From Eckstein's remarks in *Wall Street Week*, Maryland Public Television, July 25, 1980, p. 13 of transcript.
[548] See, for example, *Wall Street Week*, Maryland Public Television, July 25, 1980, pp. 11–13 of transcript.
[549] See Bartlett and Roth (1983, pp. 195–206), excerpting testimony given on May 21, 1980 (printed in Joint Economic Committee, U.S. Congress, 1980d). Early work by Eckstein in the area had been Eckstein and Tanzi (1964).

more than 11 points from its 1980 peak to its initial 1983 trough; at the other end, according to the data printed in the same volume in which Eckstein and Sinai (1986) appeared, the GNP deflator inflation rate had declined from an annual-average rate of 9.6 percent in 1981 to 3.8 percent in 1983 (Balke and Gordon, 1986, p. 783).

This picture was emerging by July 1982, when Eckstein gave the decline in U.S. inflation since its peak as about six percentage points (*Wall Street Week*, Maryland Public Television, July 23, 1982, p. 13 of transcript). He went on to foreshadow, in line with the logic of the expectations-augmented Phillips curve, that a noninflationary expansion would follow the recession: "most responsible economists feel that to get out of a fifteen-year inflation binge is going to be painful—and we're now going through the pain. And on the other side of it, there is a reward: We really have taken a dangerous part of the inflation out, got it to more reasonable amounts… [and] we really ought to get back to some kind of normalcy, in which real living standards rise once more, unemployment fades away, and inflation is not so disastrous."[550]

In the same interview, Eckstein criticized fiscal policy and called for some of the personal tax cuts legislated in 1981 to be rescinded.[551] Correspondingly, and notwithstanding his sympathy with some kinds of supply-oriented tax cuts, Eckstein was known as a Keynesian critic of the Reagan Administration's economic policy, with the vehemently pro-supply-side columnist Warren Brookes describing him as among "Reagan's strongest critics" (*The Titusville Herald* (Pennsylvania), December 30, 1982). By the second half of 1982, however, Eckstein saw very good prospects for the U.S. economy, which he identified with the course that the Volcker Federal Reserve had taken: "the triumph that Volcker has really scored over the double-digit inflation is going to stick with us," he remarked in the July 1982 interview.[552] Correspondingly, Eckstein would be an enthusiastic advocate of Volcker's reappointment. In mid-1983, won over by the past year's results, Eckstein remarked: "Volcker will go down as a giant in history." (*Time* magazine, June 20, 1983.)

At a further remove, these laudatory words were praise for Milton Friedman, as Eckstein and Sinai (1986, pp. 75, 96) classified Volcker's first three or four years in office as featuring the "monetarist policy after 6 October 1979" or "a near-monetarist approach."[553] Of course, as noted above, Friedman resisted such blanket characterizations. Indeed, Eckstein noted in July 1982

[550] *Wall Street Week*, Maryland Public Television, July 23, 1982, p. 14 of transcript.
[551] *Wall Street Week*, Maryland Public Television, July 23, 1982, p. 15 of transcript.
[552] *Wall Street Week*, Maryland Public Television, July 23, 1982, p. 15 of transcript.
[553] Likewise, Eckstein (1983b, p. 139) referred to the post-1979 "monetarist regime."

that he had "lately had an exchange of ideas with Milton Friedman. He complained that we [DRI] called the current policy 'monetarist.'" Eckstein explained that, in his reply, "I wrote Milton that if we had not had the monetarist philosophy, the Federal Reserve never would have been able to raise interest rates to [these] levels." More broadly, Eckstein noted that "the toughness of the policy is really an outgrowth of the monetarism," while making the observation, correct at the time, that on a longer horizon, "they [the Volcker FOMC] really haven't missed the [M1] target by much."[554]

Although Friedman disliked the identification of the Volcker period with monetarism, the Volcker episode had borne Friedman out a couple of key propositions from which Eckstein had previously dissented: that lower monetary growth would see inflation well down within a couple of years, and that this reduction would be secured by a substantial short-run response of inflation to economic slack. Eckstein had not grasped the size of this response ahead of time, and so he had been caught unawares by the degree of disinflation that occurred from 1980 to 1982.

The Phillips curve and the short-run output/prices split, 1983–1984

But if Eckstein had not used expectations-augmented Phillips-curve ideas to the full in analyzing inflation in 1980–1982, Friedman, too, could be faulted for not using those ideas sufficiently in 1983 and 1984, when he made his initial mistaken predictions of a surge in inflation.

Surprisingly enough, the dismal record on projecting inflation that Friedman had racked up by the end of 1984, documented in previous sections, did not reflect a major error on his part in predicting nominal aggregate demand. In contrast to his overpredictions of inflation in 1985 and 1986, Friedman's overprediction of 1984's inflation rate did not involve a major misjudgment about the rate of nominal income growth in store for 1984.[555] His forecast of high inflation for 1984 had, appropriately, taken the 1982–1983 M1 velocity as having been largely permanent (see the discussion earlier in this chapter as well as *Wall Street Journal,* September 1, 1983) and, in making the same forecast, he correctly foresaw—though perhaps for the wrong reasons—a 1984 rise in M1 velocity.

Friedman nevertheless greatly misjudged the amount of inflation in store for the economy in

[554] *Wall Street Week*, Maryland Public Television, July 23, 1982, p. 12 of transcript.

[555] This was a point correctly noted by D. Smith (1987, p. 138) and underlay Friedman's own unpublished retrospective on the 1983–1984 period in his January 1986 PEPAB memorandum (Friedman, 1986f, pp. 2, 10). See also D.F. Siegel (1986, p. 12).

1983 and 1984 by seriously overestimating how much of the nominal income growth would be taken up in inflation. This, in turn, likely reflected a misjudgment on Friedman's part on how much slack was available in the economy during 1983 and 1984. The presence of this slack in the economy's initial conditions in 1982 meant that a period of rapid nominal income growth could proceed in the context of stable (or only mildly rising) inflation.

The Phillips-curve approach was, of course, the manner in which factors such as rapid monetary growth were mapped into inflationary pressure in a structural model. Friedman, along with Edmund Phelps, had achieved undoubted success by 1982–1986, with the expectations-augmented Phillips curve and natural rate hypothesis now a fixture in discussions of inflation (see the next chapter). But Friedman was not himself relying on a quantitative version of it in making his 1983–1984 forecasts. Others were—and did much better than he did in predicting inflation in that period.

In a column appearing in September 1983—just after Friedman had made his high-inflation projections more categorical—Samuel Brittan noted the irony in the fact that new research—Englander and Los (1983)—produced by the economic staff of the Federal Reserve Bank of New York was providing far more optimistic U.S. inflation forecasts using the "concept developed by Friedman himself, the NAIRU." Brittan observed accurately that Friedman actually eschewed using the output gap in his inflation forecasts—favoring, instead, reduced-form projections using monetary growth (*Financial Times* (London), September 15, 1983.)[556]

The reduced-form approach had been an effective technique in 1976–1978, when output-gap estimates were badly biased. By 1983, they had been substantially corrected, in light of better awareness of the changes in supply conditions in the first half of the 1970s, including the post-1973 slowdown in long-term economic growth.[557] Eckstein had observed in 1980 that "we all

[556] In a remark not likely to be regarded as the most glowing assessment made by a former student of their one-time dissertation adviser, David Lindsey observed (personal communication, April 2, 2012): "For some odd reason, the co-originator of the expectations-augmented Phillips curve never himself accepted its predictions for near-term inflation, unlike the correct Board staff. Maybe, like Orphanides [later contended], he thought the natural rate couldn't be estimated closely enough for practical policy purposes. Instead, he adopted the ridiculous view that over a varying period of one to three years inflation will follow money growth." The Federal Reserve Board staff projections to which Lindsey referred will be discussed presently.

[557] Analysts who had become aware of the permanence of the post-1973 slowdown included Otto Eckstein (1983a, p. 144), who, writing in October 1979, stated that with regard to growth in factor inputs, "There is no prospect of returning to the higher rates of growth of output and capital that prevailed in the exceptional 25 years from the end of World War II to the early 1970s." The post-1973 decline in productivity growth, as well as its quantitative magnitude, was also well understood by 1982 (see Orphanides, 2003) Chapter 10 as well as such studies as A. Morgan, 1982), as was the rise in the natural rate of unemployment (see Orphanides and Williams, 2005). See also the discussion in Chapter 10 above.

ultimately have to learn from the facts" and argued that data on productivity in "these [past] seven years, and worsening inflation" pointed to a post-1973 permanent productivity slowdown.[558] The same conclusion about the recent trend of potential output had been made by modelers inside government. So, by 1983, the process in which—in the words of Orphanides and others (2000, p. 120)—"fundamental misconceptions about potential GDP were discarded" had already largely taken place by 1983. And so now, in contrast to the 1970s, it was Friedman who was at a disadvantage in not consulting output-gap or unemployment-gap estimates when assessing the U.S. economy's inflation outlook.

In a briefing circulated to the Federal Open Market Committee in November 1983, the Federal Reserve Board's staff highlighted the different answers regarding the likely future U.S. inflation rate that were coming from modern Phillips-curve analysis and a reduced-form monetarist approach.[559] The briefing noted that "[t]he natural rate hypothesis is consistent with a Phillips-cure view of inflation that includes a role for price [sic; inflation] expectations… This view… generally is used by the staff."[560] The briefing observed that a reduced-form, monetary-growth-based growth forecasting equation implied GNP deflator inflation of 7.5 to 8.75 percent in the four quarters ending 1984:Q4.[561] In contrast, the Phillips-curve-guided staff forecast implied a four-quarter inflation rate of 5 percent for that data point.[562]

The Federal Reserve Board staff analysis of inflation, in common with that of many forecasters like Eckstein, in practice embedded the Phillips-curve approach in frameworks that decomposed the inflation rate into sectoral components as well as tracing price movements to production costs. Friedman had, of course, been suspicious of such components-based approaches because of their tendency to be used to justify cost-push-based perspectives on inflation. But, in principle, a components-based approach could be consistent with making inflation an endogenous variable that depended on the aggregate demand/supply balance and ultimately on monetary policy, while also providing for improvement in forecasts by recognizing very short-run influences on prices.

In 1983–1984, those forecasters who used this Phillips-curve-plus-components approach, either

[558] *Wall Street Week*, Maryland Public Television, July 25, 1980, p. 9 of transcript.
[559] The briefing gave no indication that Friedman was, via his research, closely associated with both of these ways of analyzing inflation. Indeed, it did not mention him at all.
[560] Federal Reserve Board (1983b, p. 4).
[561] Federal Reserve Board (1983b, p. 17).
[562] Federal Reserve Board (1983b, Chart 15, p. 36). Although well below Friedman's public forecast, this internal forecast also proved to be too pessimistic regarding U.S. inflation in 1984, overpredicting GNP deflator inflation by around a percentage point.

in place of, or as a cross-check on, inflation forecasts drawn from monetary-growth behavior were generally on a sounder footing in their forecasting than Friedman was at the time. So Alan Greenspan, likely relying in large part on a components-based approach, observed in mid-1983 that inflation "will stay down" and that "inflation has almost no chance to reaccelerate in any significant way in the next year."[563] And William Nordhaus observed in the fall of the year that U.S. "inflation is likely to continue in the 4 to 6 percent range as far [ahead] as the forecaster's eye can see." (*The Economist* (London), November 12, 1983.) These projections proved accurate.

The Reagan Administration also eschewed high-inflation forecasts. In February 1983, Secretary of the Treasury Regan contended, "we have about the right amount of money in the system to continue recovering without overly inflating." Regan elaborated: "Look at all of the things that go into prices—labor cost, energy, food prices, and productivity—they all point to inflation staying down." (*Financial Times* (London), March 30, 1983.) Regan's perspective can be regarded as amounting to saying that the behavior of costs did not confirm the scenario of excess-demand pressure that would be materializing if, as the monetarists contended, monetary expansion was excessive.[564]

Friedman's own position was that wages might not be providing a good signal of the emerging inflationary pressure because they typically lagged inflation. He made public remarks in 1983 and 1984 to this effect.[565] Friedman was therefore inclined during 1983 to see analysis of the price index using its components or using costs as liable to give an overly comforting picture regarding inflation prospects. However, as of 1983, this approach to analyzing inflation gave an accurate signal, one dissimilar to forecasts using monetary growth but consistent with that coming from Phillips-curve equations.

The fact that the expectations-augmented Phillips curve was part of Friedman's framework raises the question of how he reconciled his high-inflation predictions with that framework. It appears that the reconciliation came in a high implied estimate of the natural rate of unemployment, so that Friedman saw overheating as likely to occur as early as 1984 (*Wall Street Journal*,

[563] *Meet the Press*, NBC, July 17, 1983, p. 4 of transcript. See also *The Sun* (Baltimore) July 18, 1983.
[564] The compatibility of markup-style models of price determination and the monetarist account of inflation, provided costs were made appropriately endogenous, was stressed during these years in the aforementioned book review by Parkin (1984a, p. 261) and by Artis and Lewis (1985, p. 213) in a paper prepared for a conference on inflation and unemployment held at Macquarie University (Australia) in September 1983.
[565] See Friedman (1984f, p. 44) and *Bottom Line Personal*, May 15, 1984 (p. 2).

September 1, 1983), as well as a belief in forward-looking elements of inflation behavior, so that inflation might rise ahead of the output gap closing (*New York Times*, April 3, 1984).

Eckstein weighs in

Eckstein and Friedman's views on the economy were juxtaposed in a wire news report in May 1983. But although they did express contrasting views on the urgency of reducing the federal budget deficit and about how to reduce it, both were optimistic about recovery proceeding for the rest of the year, Eckstein describing that eventuality as "money in the bank" (*Washington Times* (Washington, D.C.), May 10, 1983). Several months later, on another of his appearances on *Wall Street Week* (September 30, 1983), Eckstein was asked on television about monetarists' success in forecasting the year's economic expansion. After Eckstein had remarked that the economy "really turned out much better than anybody had reason to believe," Rukeyser raised the subject of "your long-time intellectual adversary and also friend, Milton Friedman."[566] Rukeyser asked, "Before we discard the monetarists, isn't it fair to say that they were the only ones who saw the strength of this recovery, this year?" In response, Eckstein did not disagree: "You've got to give them credit for that. The one thing they are always good for is an emphasis on the monetary factor. And that was key."[567]

Eckstein parted company with Friedman, however, when it came to the prospects for the U.S. economy in 1984. This was brought out when Eckstein was asked to comment on Friedman's predictions as part of *Fortune*'s long profile of Friedman in its edition of March 19, 1984. Eckstein correctly rejected Friedman's recession prediction, declaring: "This is a happy moment for business." Eckstein was only half-right, however, in rejecting Friedman's diagnosis of recent quarters: "We don't believe the slowdown late last year was produced by earlier reduced money supply with a lag." (*Fortune*, March 19, 1984, p. 34.) As already discussed, it would turn out after data revisions that there was not much of a slowdown at all in late 1983. But the economic slowdown that did start in the second quarter of 1984 was quite plausibly related to the monetary policy tightening in the second half of 1983 on which Friedman had put so much emphasis.

[566] *Wall Street Week*, Maryland Public Television, September 30, 1983, pp. 5, 6 of transcript.

[567] *Wall Street Week*, Maryland Public Television, September 30, 1983, p. 6 of transcript. Although emphatic about the importance of Keynesian effects of fiscal policy, Eckstein had been receptive, even during the 1960s, to the notion that monetary policy had sizable effects. For example, Eckstein (1965, p. 17) endorsed the view, by no means uniformly held by major Keynesians at the time, that a non-accommodative monetary policy would have "nullified" the aggregate-demand effects of the 1964 tax cut. He was also open to analyzing monetary policy via the use of monetary aggregates. In Eckstein (1979, p. 75), for example, he named the monetary base, nonborrowed reserves, and total reserves as series that could measure monetary policy, and he would later use the monetary base when modeling U.S. bond-rate behavior (see Eckstein, 1983b, p. 139).

Eckstein was on much more solid ground on inflation: he told *Fortune* that, at the end of the year, the rate would be 4.5 percent.[568] This prediction was roughly half what Friedman was predicting, and it would be close to the CPI inflation outcome for 1984 (which was about 4 percent in the case of the December twelve-month rate, 4.1 percent in the case of the fourth-quarter-to-fourth-quarter rate).

Eckstein would never see this vindication, however, nor would there be any more sparring with Friedman. He was in deep health trouble. For several years, Eckstein's continuing high level of professional activity had been in the context of being an out-patient after being diagnosed with cancer. Eckstein succumbed to cancer at Boston's Massachusetts General Hospital on March 22, 1984, at age 56 (*New York Times*, March 23, 1984). When, the following day, the proceedings of the NBER's business-cycles conference began in Puerto Rico, Allen Sinai told participants at the event: "Otto Eckstein passed away yesterday morning," while also indicating that his paper with Eckstein—the study commissioned to open the conference—would immediately "be presented as scheduled" at the event, roughly at the same time as Eckstein's funeral was taking place in Lexington, Massachusetts.[569]

The distress that the Friedmans felt at Eckstein's passing was still evident more than five years later, in July 1989, when Rose Friedman instant reaction to Friedman's reference to "the fellow who founded IDI" was to interrupt by saying, "Oh, no. He died." Milton Friedman continued her thought. "He died, unfortunately. Cancer."[570]

By 1984, Sinai had left his position of senior vice president as DRI but, as the paper ultimately published as Eckstein and Sinai (1986) attested, he had continued to collaborate with Eckstein. Their work together would be underscored when, in mid-1984, Robert Heller referred to Eckstein and Sinai's joint endeavors on the DRI model in remarks introducing Sinai as part of a panel that included Friedman.[571] More than thirty years later, Sinai recalled of Eckstein: "he was a joy to work with." (Allen Sinai, interview, May 7, 2015.)

[568] *Fortune*, March 19, 1984 (p. 34). Eckstein had already criticized Friedman's inflation warnings when he had been asked about them during his television appearance the previous fall: "Well, he [Friedman] hangs it all, of course, on the rapid increase of the money supply over the last 12 months." Of this increase, Eckstein discounted the part of it that had occurred in early 1983 as being due to the introduction of Super NOW accounts and judged that, subject to that, monetary growth was "nor excessive. I mean, it would accommodate a good [real] growth rate and an inflation rate of 4 or 5 percent." (*Wall Street Week*, Maryland Public Television, September 30, 1983, p. 6 of transcript.)

[569] Sinai (1986, p. 35).

[570] In Hammond (1989, p. 42).

[571] See Heller and others (1984, p. 16).

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 14: Debates on Fiscal Policy, Regulation, and Aggregate Supply, 1982 to 1986

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I. EVENTS AND ACTIVITIES RELATED TO DEBATES ON FISCAL POLICY, REGULATION, AND AGGREGATE SUPPLY, 1982–1986

As discussed in the previous chapter, over the five years from 1982 through 1986 Milton Friedman had a series of setbacks on the monetary scene—with the Federal Reserve decisively parting ways with him on the matter of the choice between a reserves total and interest rates as a policy instrument and on the interpretation and weight to be given to the behavior of U.S. monetary aggregates. It was also a period in which unfolding developments—and some faulty interpretations that Friedman made of those developments—had the effect of generating far more widespread skepticism among economic commentators about the validity of his monetary analysis.

Over these same years, however, Friedman had occasion to be pleased with a number of important developments in other areas of domestic economic policy. One such area—that of regulation—will be the focus of the second section of this chapter, which will consider the course of events associated with the aftermath of oil price decontrol and with the implementation of financial deregulation, especially deposit interest-rate deregulation, in the United States. Ahead of that discussion, another source of considerable encouragement from 1982 to 1986 to Friedman is now considered: the evolution of the national debate on fiscal policy.

Friedman's approval of the actual course of U.S. fiscal policy developments in this period came with very heavy qualifications: in particular, contrary to his hopes in 1981, things did not unambiguously move in the direction of a diminishing public-sector role in the economy. In particular, the Reagan years never saw the undoing of the 1970s' rise in the share in U.S.

[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 regrets to note that, since the research underlying this chapter began, eight individuals whose interviews with the author are quoted below—William R. Allen, Kenneth Arrow, Francis Bator, Martin Feldstein, Allan Meltzer, George Shultz, Donald Winch, and Leland Yeager—have passed away.

government spending to output—a reversal that Friedman had seen as a realistic goal. Indeed, as detailed below, during Reagan's first term the federal government's share reached new peacetime peaks.

There was, nevertheless, a material underlying change—consolidating the pattern associated with Reagan's election and his first year in office—in the direction of a slowing in the momentum for a larger U.S. public sector. In this connection, a *Wall Street Journal* analysis of the era noted of federal spending (October 21, 1985, p. 16), "Some big budget items grew rapidly" from 1981 to 1985, but that the two largest items (national defense and interest on the national debt) were not of the kind associated with expansions of the federal government's role in the domestic economy, while "only a few [new] programs were created."

This development paralleled a change in the default position taken by many of those on the opposite side of Friedman and Reagan in economic debate—from urging fiscal expansion on many occasions in the 1970s to, in the 1980s, agitating for some form of fiscal tightening, one usually partly entailing restraint in domestic federal spending. In light of this change, Friedman had little reason over the subsequent five years to alter his judgment, stated in the course of a conference talk given at the Atlanta Hilton Hotel on March 17, 1982, that "all of the talk about deficits, all of the concentration on compromise and so on, is a sign of the absolute triumph of his [the president's] policy… The triumph, in my opinion, of the policy that President Reagan has been following is that he has changed the whole basis of the discussion."[2]

No major anti-recession fiscal stimulus

In 1982's recessionary conditions, Friedman gleaned a shift in tone in public discussions of countercyclical fiscal policy. The budget deficit was poised to, and did, expand due to the recession and the phased-in Reagan tax cuts, as well as the realization of the defense buildup. But in the past, recessions had given rise to specific new public-spending programs, and major domestic federal government expenditure programs had been launched in the 1960s and 1970s in periods of expansion, too. Although, as discussed further below, after Reagan's first year there was not great support for Congress for reductions in government spending, it was also the case that, during the 1981–1982 recession, it proved much more difficult than in previous comparable episodes to obtain wide support for a specific and major anti-recession public-spending program, so the Reagan Administration prevailed on this matter by rebuffing or mitigating pressure for

[2] Friedman (1982a, pp. 60, 61).

programs of this type. Similarly, during the economic expansion that started in late 1982, fiscal restraint of some kind, and not expansion, was the usual prescription of Reagan's opponents.

Friedman was particularly struck by the fact that the 1981–1982 recession featured far fewer calls for large-scale fiscal-expansion measures than had recessions in previous decades. [3] Instead, he noted, even during this recession period it was the case that calls for deficit reduction were being made by prominent critics of the administration. Walter Heller, for example, had stated: "We need to cut back the defense budget and possibly rescind some of the tax cut." (*Savings and Loan News*, October 1981, p. 67.) Another example, one occurring when the recession was more well established, was cited by Friedman in in March 1982: Charles Schultze, the Carter Administration's Chairman of the Council of Economic Advisers (CEA), had at the start of the year given a television interview in which he called for a tax increase.[4] Friedman expressed the wish that he had also been a guest alongside Schultze on the television panel, in order to confront the former CEA head with the fact that the early period of the Carter Administration had seen official proposals for a tax rebate—not a tax increase—in response to the 1976 economic pause. "Where do these born-again budget balancers come from?," Friedman asked regarding Schultze and other longtime sparring partners of his.[5]

On the public-spending side, too, as of March 1982 Friedman perceived a contrast between prior recessions and the current one in the volume of calls for "increased government spending to get people to work" (*Milwaukee Journal*, March 18, 1982.)[6] "When before in recent times have you had a recession when people haven't been saying, 'Oh, my goodness, we have got to have some job-creating programs, we've got to have new job expansion programs, training, etc.?,'" Friedman remarked on March 23, 1982. Yet, he added, in the current U.S. situation, "Almost nobody is saying that."[7]

His evaluation did change somewhat and, when in mid-decade the Friedmans looked back over

[3] This would be the judgment not only of Friedman but also of early retrospectives such as Boskin (1987, p. 69).

[4] In an appearance on *The MacNeil/Lehrer Report* (PBS, January 1, 1982), Schultze had remarked: "The tax cut was too big; we need to raise taxes again at the outer end [and] postpone some of those [other] tax cuts. Two, we've got to at least pare some off defense spending. We can certainly get a few percentage points out of it."

[5] Friedman (1982a, p. 61).

[6] See also *Omaha World-Herald* (Nebraska), March 18, 1982, and *San Diego Union* (California), March 18, 1982.

[7] Friedman (1982f, p. 17), recording remarks Friedman made in Vancouver. Blinder (2004, p. 12; p. 34 of printed version) dated the point at which "fiscal stabilization fell deeply out of favor" to an earlier period, associated with the 1977 tax-rebate proposal (a proposal that Blinder stated was rejected by Congress but was, in fact, preemptively withdrawn by President Carter, as discussed in Chapter 8 above). It should be stressed, however, that President Carter did succeed in having stimulus measures passed in 1978 and even in the early fall of 1980 was formally submitting anti-recession fiscal measures to Congress.

the period through early 1983, they acknowledged that there had been quite an abundance of suggestions that the federal government provide an anti-recession expenditure program. They were, however, impressed by how little steam these suggestions picked up. The Friedmans observed: "Not until late 1982, when the recession had just about run its course, did a so-called jobs bill finally get through Congress." In fact, although there had been legislative moves in relation to a jobs bill in late 1982, the bill in question was not passed until March 1983. By then, the administration itself had offered a $4.3 billion jobs bill as a counterproposal (*Dallas Morning News,* February 11, 1983). Building on this, the bill signed into law by Reagan on March 24, 1983, involved $4.6 billion in spending, spread over the next couple of years (*Seattle Times*, March 31, 1983). The Friedmans took comfort in the modest nature of the appropriation, in contrast to the experience of the previous fifteen years: "that bill was far smaller in size than similar measures passed during earlier recessions."[8] This, they believed, attested to the "vast difference in the general atmosphere in which policy was being conducted."[9]

On account of the small nature of this March 1983 concession by Reagan, it did not represent a break from the stand that Friedman a year earlier had stated the president should take: one in which "he stays firm and sticks to his guns… holding down tax rates, holding down government spending."[10] He remarked on television during that March 1982 period: "This economy is basically a strong, healthy economy. It is going through a temporary period of adjustment and shake-up, primarily as a result of one of the great successes—which is that inflation is coming down." The recession reflected the fact that "we're going through those adjustment problems"—magnified, he believed, by the erratic manner in which the Federal Reserve had carried out the disinflationary policy.[11] Against this background, he suggested, Reagan could put the conditions

[8] Friedman and Friedman (1984, p. 3; 1985, p. 10). Here, the Friedmans were likely taking a number of the expansionary fiscal actions taken by the Nixon Administration during 1971 and 1972 as anti-recession spending measures, although they were, in fact, enacted in the *post*-recession period—in the early stages of what was believed at the time to be an unduly slow recovery. Milton and Rose Friedman also likely took the Ford Administration's anti-recession rebate of 1975 as a spending measure. Such a practice would be in line with Milton Friedman's view that transfer spending should be counted in government expenditures rather than as negative taxes (see Nelson, 2020a, Chapter 4). And as discussed in Chapter 5 above, Friedman was further disinclined to view the 1975 rebate as a tax cut because it was unrelated to current income and work effort. This criticism of the 1975 rebate was held by others who were typically less critical of Keynesian economics than he was. Most notably, Paul Volcker, during a stint in the private sector, was a member a panel of witnesses at a Congressional hearing held on January 27, 1975, and in the course of his contribution he remarked: "I question, however, the form in which the president would provide this relief. At least part of the purpose of a tax cut should be to sharpen work incentives… Indeed, it [the rebate proposal] seems to me hardly to justify the name 'tax cut'—it is rather a kind of grant program loosely related to past circumstances… That is why we say: Don't do it [cut taxes] by rebate." (In Committee on Ways and Means, U.S. House of Representatives, 1975, pp. 525, 526, 553.)

[9] Friedman and Friedman (1984, p. 2; 1985, p. 10).

[10] Friedman (1982a, p. 61).

[11] *Meet the Press,* NBC, March 21, 1982, p. 7 of transcript.

in place that would best “restore business activity” post-recession: “stick to his policy… continue to remove the barriers to the appropriate use of our capital—that is, to reduce regulatory controls of various kinds, as he has been doing, to reduce the marginal tax rates.”[12]

Another item that Friedman nominated in March 1982 as a necessary component of holding firm—the condition that “the Federal Reserve can stick to” the disinflationary monetary policy carried out, on average, since 1979—was one that during 1983 and 1984 he believed had been lost.[13] This judgment was reflected in many Friedman commentaries in those years, as discussed in the previous chapter, as well as his observation with regard to the Federal Reserve authorities, “In mid-1982… they stepped hard on the accelerator.” (*Newsweek*, January 16, 1984.)

Monetary policy did, according to numerous metrics, indeed ease during 1982–1983 and, despite his denials at the time of a change in policy stance, the period was described in retrospect by Volcker himself as seeing an easing. He would remark of the summer of 1982: “we had to ease... So I jumped when the opening came.”[14]

But despite Friedman’s fears at the time, the policy change, while initially certainly an easing in the FOMC’s stance, was not a *U*-turn, because it did not change monetary settings so much that the disinflation of the previous years was actually lost. As discussed in the previous chapter, the policy changes of the period did involve sharp changes in the role of monetary-quantity variables (reserves and monetary aggregates) in the setting of policy, as well as a surge in monetary growth. But it proved not to be a move that led to an excessive expansion of aggregate demand. In part, this result was achieved because the 1982–1983 easing had the effect of substantially closing a negative output gap, not of generating a positive one. The downward pressure on inflation associated with the restraint of aggregate demand in the early part of the decade was not undone.

Likewise, on the financial front, Friedman’s observation on television in March 1982 was—with some interruption in 1983–1984—borne out over the 1980s: “the reason why I believe interest rates will come down is because inflation is coming down. Interest rates largely reflect inflationary expectations... If he [Reagan] sticks to his policy, if we get his policies through, then interest rates will be coming down.”[15]

[12] *Meet the Press*, NBC, March 21, 1982, p. 4 of transcript.
[13] Friedman (1982a, p. 61).
[14] Quoted in Cannon (2000, p. 233).
[15] *Meet the Press*, NBC, March 21, 1982, p. 9 of transcript.

Even President Reagan, who was often reluctant to articulate his economic policy in terms that implied that a recession was likely a necessary part of the adjustment process and who rarely made a virtue of policy-driven restriction of spending in the economy except in the public sector, was on occasion, notably in early 1982, willing to describe the recession publicly as a necessary part of the temporary pain that the U.S. economy needed to go through (Cannon, 2000, p. 231). And as stressed in the previous two chapters, the president was extremely well disposed both toward describing his administration's policy (implicitly or explicitly encompassing that of the Federal Reserve) as one that would persevere with an anti-inflation policy and toward contrasting this with previous disinflation attempts in the 1960s and 1970s. The Reagan and Republican party slogan of "Stay the Course" in the leadup to the 1982 Congressional elections alluded to the administration's no-*U*-turn attitude toward macroeconomic policy.

Deficits and Keynesian calls for fiscal restriction

In a *Newsweek* column in mid-1983, Friedman cited the "*U*-turn [of] longtime New Dealers who have always praised deficits as a way to prime the pump and stimulate the economy but are now preaching the virtues of balancing the budget." (*Newsweek*, July 4, 1983.) The use of the term "New Dealers," rather than Keynesians, partly reflected the fact that Friedman was here largely talking about the economic debate among politicians rather than among economists. Consistent with this, the column also referred to those who had changed their position as the "Democratic leaders," while the following spring Friedman named Tip O'Neil (speaker of the U.S. House of Representatives) and Walter Mondale (former vice president and 1984 presidential contender) as key examples of who he was talking about: they had "turned around" from their longstanding position "that we've got to prime the pump, that we shouldn't worry about deficits."[16]

Nevertheless, the absence of an explicit reference to Keynesians in the 1983 *Newsweek* column may have been an effort on Friedman's part to allow for the fact that leading U.S. Keynesians had not made such a clear-cut break in position on this matter. This was so even though their policy prescriptions during the 1980s largely paralleled Mondale's in urging a winding-back of the Reagan tax cuts as a means of reducing the budget deficit. There was a logical consistency across the 1970 and 1980s in the position taken on fiscal policy by Friedman's leading Keynesian opponents.

[16] *The MacNeil/Lehrer Report*, PBS, March 27, 1984, p. 6 of transcript. See also Friedman and Friedman (1984, pp. 88–89; 1985, p. 89) and *Newsweek*, November 23, 1981. Friedman referred to O'Neil as one of "the old New Dealers" in Friedman (1982f, p. 17), although he did also call him a "Keynesian" in *Sunday Advocate* (Baton Rouge, Louisiana), June 5, 1983, p. 8G.

To be sure, there had certainly been a notable change in U.S. Keynesians' policy prescriptions: 1971, 1975−1977, and 1982−1984 were all periods of a perceived large negative output gap, yet, in the third period, individuals like Walter Heller, Paul Samuelson, Schultze, and James Tobin were proposing fiscal tightening after having frequently called for fiscal stimulus in the course of the previous two episodes. Nevertheless, as already implied, there was a basic constancy over time in their analytical framework. U.S. Keynesians' 1980s prescriptions were mainly in the context of urging a change in the policy mix: rather than advocating restrictive policies overall in times of economic slack, they saw it as appropriate for monetary policy to deliver most of the stimulus to aggregate demand. This was a change from much early 1960s-vintage U.S. Keynesianism that made fiscal policy the central tool.[17] But it still reflected a belief in the merits of very activist countercyclical demand management.[18]

Furthermore, as an analytical matter, there had not been a great change in recent decades in U.S. Keynesians' position on the effects of fiscal policy: both the New Economics of the 1960s and the widespread Keynesian objections to the Reagan fiscal policy were based on a belief in the power of "pure"—that is, unmonetized—fiscal policy actions.

This dialogue's focus on "pure" fiscal policy reflected the common ground among many commentators, including both supporters and critics of Reagan fiscal policy, that, for the immediate years ahead at least, deficits were unlikely to be accommodated by the Federal Reserve. There were some dissents from this position, to be sure. Nevertheless, Friedman and leading Keynesians both primarily treated the situation facing the United States as consisting of a large step-up in government debt issuance without that debt being financed, directly or indirectly, by money creation. As indicated in Chapter 11, in the early 1980s Friedman again affirmed his belief that noninflationary monetary growth and large budget deficits were compatible for long stretches of years. As discussed in the previous chapter, Friedman did, from 1983 until May 1986, erroneously expect a major and imminent revival of inflation. Importantly, however, this prediction was not based on the emergence in recent years of the

[17] In the case of some of these Keynesians, there may also have been a break in their attitudes to the idea of having the budget balanced at full employment. Although Friedman himself was, during the 1980s, far less likely to discuss in detail the structural budget balance than he had been in prior decades, the notion that the structural deficit should be zero in most circumstances had more traction in U.S. policy discussions (especially among those urging rapid action on the deficit) than it did in the decades when Friedman was a leading advocate of such a policy.

[18] In addition, during the 1982−1984 period of economic slack, the recommendations of fiscal restriction were sometimes given as primarily pertaining to out-years rather than immediately. Charles Schultze, for example, stated that a $100 billion budget deficit in 1982 was not a problem but that he worried about it persisting in the anticipated recovery years of 1983 and 1984 (*The MacNeil/Lehrer Report*, PBS, January 1, 1982). In the event, the deficit was $128 billion in fiscal-year 1982 and was higher subsequently, exceeding $200 billion in fiscal years 1983, 1985, and 1986 (Council of Economic Advisers, 2011, p. 283, Table B−78).

super-high budget deficits. So, although, as late as April 1986, he was still claiming that the "view that people have that inflation has been licked is nonsense—[instead] you [only] have a temporar[il]y low inflation," he added with regard to this judgment, "The federal deficit has almost nothing to do with it…. The deficit is important only insofar as it induces the Federal Reserve to monetize the deficit by printing money. That has happened on the average, but there is no need for it to happen." (*American Banker,* April 30, 1986, p. 20.)

Consequently, against the background of a consensus that U.S. monetary policy would be nonaccommodative through the late 1980s at least, the debate on budget deficits' importance centered on beliefs about the effects of pure fiscal policy.

In particular, the position stressed by leading U.S. Keynesians in the 1960s that fiscal policy generated powerful multiplier effects on nominal aggregate demand for given monetary growth reflected the same basic belief in pure fiscal policy as that underpinning Keynesians' warnings in the 1980s that U.S. fiscal deficits, occurring in a situation in which monetary policy restrained aggregate nominal demand, would tend to push up interest rates and reduce private investment. In both periods, the underlying position was that fiscal deficits tended to raise credit demand, push up interest rates, and raise velocity. The difference in the 1980s was that monetary policy was seen as likely to offset the demand-stimulating effects of fiscal expansion, in order to keep total spending in the economy on a planned path. Consequently, the higher velocity associated with fiscal expansion would not imply higher total nominal spending.

That this—higher nominal interest rate raising velocity—was the mechanism by which Keynesian effects of pure fiscal policy were manifested in nominal aggregate demand was not how Keynesians typically described matters in their own expositions. But Friedman had cast the fiscal-multiplier mechanism in such terms on numerous occasions (see Nelson, 2020b, Chapter 12) and did so again in the context of early discussions of the Reagan tax cut proposal (*Newsweek*, July 27, 1981). And in the 1980s, with the large Reagan deficits leading Keynesians to stress, increasingly, the interest-rate-raising effects of deficits, James Tobin made a point of stating that pure fiscal policy actions worked by raising interest rates. Referring to the monetary/fiscal policy debate of the 1960s and early 1970s—a debate to which the discussion will return below—Tobin remarked (*The Economist* (London), April 27, 1985*a*, p. 23): "In theory, this debate turned on the question [of] whether fiscal stimulus can systematically raise the velocity of money, by raising interest rates by inducing businesses and households to manage their transactions with smaller holdings of cash."

This Tobin discussion underlined the fact that the interest-rate-to-velocity mechanism was common ground between Keynesians and monetarists in their views on the behavior of nominal income. This mechanism had featured in numerous analytical and empirical portions of Friedman and Schwartz's *Monetary Trends*, including a table, "Regressions of Nominal Income and Nominal Quantity of Money and Other Variables," in which interest rates—appropriately—entered positively in nominal-income equations that were conditional on the money stock.[19] In their initial critique of *Monetary Trends*, Hendry and Ericsson (1983, p. 63) pointed to the U.K. nominal-income equation in this table and to an equation for the price level elsewhere in the book and remarked: "We are at a loss for an interpretation of the *positive* 'long-run' effect of increasing interest rates on prices and income." This criticism was certainly not valid. With regard to income, Hendry and Ericsson had, in effect, invalidly transported IS-equation-type intuition (that income and interest rates should be negatively related when the *bivariate* relationship between the *real* values is considered) to the *trivariate* relationship between nominal income, money, and nominal interest rates (in which it is indeed the case that interest rates should have a positive coefficient in an income equation when money is another right-hand-side variable).

The positive link between interest rates and prices, conditional on money, was—like that with nominal income—also a well-established part of monetary analysis. Indeed, during 1982, not only *Monetary Trends* but also two notable research articles—one by Michael Darby in the *American Economic Review* and one by Robert Barro in an NBER conference volume on inflation—stressed this positive relationship.[20] Barro (1982, p. 101), in particular, pointed out that "higher nominal interest rates… reduce the demand for money and thereby push up the price level" and suggested that, when M1 was used as the definition of money, this mechanism was found to have added about 1.1 percent to the average U.S. inflation rate in 1948–1979. Insofar as fiscal deficits were an important influence on nominal interest rates, calculations such as those in Barro's 1982 paper lent support to the importance of pure fiscal policy actions as an influence on nominal income and prices. But, of course, a separate area of economic research in

[19] Friedman and Schwartz (1982a, p. 349, Table 8.1). As Friedman and Schwartz argued that longer-run movements in nominal interest rates were, in large part, traceable to monetary developments—principally via the Fisher effect—this equation was, in principle, consistent with other equations later in the chapter (as well as in other research of Friedman's) that made nominal income a function solely of a distributed lag of money. Other studies, however, found that changes in nominal interest rates entered positively in equations for the change in nominal income, alongside a distributed lag of money-supply change: see Gordon (1971, pp. 537–538) and Batten and Thornton (1986, pp. 13–14).

[20] Darby (1982, p. 747) observed that when price-level behavior was viewed in money supply/money demand terms, it was the case that increases in real income tended to make the price level lower than otherwise. He went on: "Increases in M^S [money supply] or in [the nominal interest rate] R_t, on the other hand, tend by themselves to raise the price level."

which Barro was prominent—that on Ricardian equivalence—cast doubt on the deficit-to-interest-rate chain. If debt neutrality held, then one could believe both that interest rates mattered for velocity behavior and that fiscal deficits did not raise interest rates and velocity.

The debt-neutrality position associated with Ricardian equivalence was gaining ground in the economics profession in the early 1980s. Correspondingly, the consensus that budget deficits had an important bearing on the behavior of real interest rates was undermined. The division of opinion on this matter was evident also in the Reagan Administration's economic-policy team. Donald Regan, Secretary of the Treasury through 1985, greatly downplayed the deficits/rate linkage. In contrast, Martin Feldstein, CEA chair over 1982–1984, emphasized the link frequently, including at a Congressional hearing held on March 29, 1984, in which he stated that the federal budget deficit raised overall demand, creating "an increase in the real return on debt and equity investments" in order to clear the credit market and draw funds from abroad.[21]

As for Friedman himself, Chapter 8 above discussed the fact that although he was once a champion of the importance of the crowding-out mechanism, during the late 1970s and early 1980s he moved much closer to the Ricardian debt-neutrality position. He was now far less inclined than previously to see public debt as a key factor driving real interest rates (holding constant monetary growth). Paul Samuelson was consequently out of date when, in a talk given on March 8, 1984, he stated: "Even Milton Friedman expects large deficits to affect the mixture of aggregate investment and aggregate consumption at the natural rate of unemployment." Friedman's revised position was instead reflected in remarks quoted in the *New York Times* earlier in the same week: "The ills that are attributed to deficits do not derive from deficits as such, but from the high level of government spending." (*New York Times*, March 5, 1984, p. D14.)[22] Correspondingly, Friedman explicitly endorsed Ricardian equivalence in print late in the following month.[23]

Friedman was not a participant in the research debate that took place in the 1980s on Ricardian

[21] In Committee on Ways and Means, U.S. House of Representatives (1984, p. 59).

[22] Samuelson (1984, pp. 7–8) referred to Friedman (1968c) before making his statement of Friedman's position. But, as indicated, Friedman's view on the implications of government borrowing for the split between consumption and investment was quite different starting in about 1977 from what it had been previously.

[23] See *Wall Street Journal*, April 26, 1984. See also Nelson (2020b, Chapter 13) for further discussion. Proponents of debt neutrality did contend that changes in government purchases mattered for interest rates, including short-term rates (see, for example, Barro, 1981, and Plosser, 1982). Friedman's remark, "There is nothing Congress can do that will have an effect on short-term interest rates" (United Press International, May 16, 1982; also quoted in *Dallas Morning News*, May 17, 1982, and in *Houston Post*, May 17, 1982) was not necessarily inconsistent with this position, as he may have been referring to the likelihood that Congressional legislation on appropriations affected outlays only with a delay.

equivalence. An allusion to him came, however, when James Tobin discussed a paper by Charles Plosser on the empirical relationship between deficits and interest rates. Tobin described the paper—subsequently published in the *Journal of Monetary Economics* (Plosser, 1982)—as the most pernicious example of the methodology of positive economics that he had ever seen (Charles Plosser, personal communication, October 29, 2018).[24] But Plosser's finding—that deficits and interest rates had little relation, for given monetary growth—would be supported by many other studies, and by mid-decade it was becoming commonly accepted among researchers.[25]

Restraining public spending: stalled progress

One area of fiscal policy in which Friedman was definitely disappointed with the Reagan record from 1981 to 1986 was that of reining in government spending.

Following the announcement of the administration's initial economic program, Friedman had indicated with regard to the "very ambitious, very courageous, very far-reaching proposals made by President Reagan" that in his judgment they did go far enough because they did not imply an absolute reduction in real government spending (*Houston Post*, May 20, 1981).[26] But he already had, in effect, recognized that ongoing real cuts in total federal government spending did not seem to be a particularly realistic policy proposal. Indeed, in advancing his own balanced-budget amendment, Friedman had previously described its aim as "a very gradual downward pressure on the federal government's share of the GNP" (*Newsweek*, February 12, 1979, p. 34) and so being a more modest objective, in the short term, than a reduction in real federal spending.

Reagan's 1981 program involved such a reduction in the federal spending share, less gradual and terminating in mid-decade. Through 1986, however, Reagan did not achieve this step-down in the share. On the contrary, as his former CEA head Murray Weidenbaum noted in the middle of that year, "federal spending became a higher percentage of the GNP than when Jimmy Carter left office."[27]

During the president's first year or so in office, the auguries for achieving the hoped-for

[24] See also the coverage of Plosser (1982) in Haliassos and Tobin (1990, pp. 928–929).
[25] For further discussion, see the next chapter.
[26] See also the April 1981 Friedman remarks quoted in the discussion below titled "David Stockman."
[27] *Wall Street Week*, Maryland Public Television, May 23, 1986, pp. 7–8 of transcript.

medium-term reduction in the spending share had seemed reasonable. In May 1981, Martin Feldstein (who was at this stage not part of the administration but—as discussed in Chapter 11—was generally supportive of its economic policy) remarked that "it seems to me that the mood in these two houses [of Congress] is clearly against increasing spending."[28] The following January, another Reagan-supporting economist, Alan Greenspan, continued to see the momentum on the issue as being in the president's favor: "He's been extraordinarily successful so far on the budget."[29] Greenspan linked the budgetary success with popular support for disinflation: "the political changes in this country are nothing short of phenomenal. The American people are fed up with inflation, and I think they are clearly supporting those actions which are going to be required to bring it under control." He predicted that this mood would likely help secure the passage of more domestic government spending cuts during Reagan's second year in office.[30]

By this time, however, Friedman had already raised the possibility that Reagan's political opponents were calculating that the popular opinion favoring tax cuts and spending restraint might fade after the president's first year or so in office. Although Friedman clearly hoped that what he called "the current public mood" favoring spending restraint and tax reduction would hold, he did concede that "pressure for increased spending" could return in earnest in the near future (*Newsweek*, November 23, 1981). As noted, over the course of 1982 he would be pleased to see the lack of traction that proposed large-scale anti-recession public-spending projects obtained. It was also true, however, that the momentum for actual reductions in domestic spending items (and so for slower growth overall in domestic federal spending) stalled. Friedman had remarked in April 1981 that "all the indications are that the Congress is going to adopt a very large fraction, if not all, of the proposed cuts" for the coming year.[31] Friedman proved to be not too far off in his prediction for that year (see Chapter 11). But the administration would not enjoy this success again. Instead, President Reagan noted in an address to the nation on August 16, 1982: "When I submitted the 1983 budget to the Congress in February, it contained very significant spending cuts on top of those we obtained last year. This time, however, we couldn't get the support we had last year."[32]

The stalling of progress was reflected in the behavior of the share of public spending in output. At the March 1982 conference in Atlanta at which Friedman also spoke, Council of Economic Affairs chairman Martin Weidenbaum stated that fiscal-year 1982 would see an increase in the

[28] Testimony of May 14, 1981, in Committee on Finance, U.S. Senate (1981b, p. 244).
[29] *Wall Street Week*, Maryland Public Television, January 29, 1982, p. 14 of transcript.
[30] *Wall Street Week*, Maryland Public Television, January 29, 1982, p. 11 of transcript.
[31] Friedman (1981e, p. 9).
[32] Reagan (1982b).

government-spending share, instead of the reduction that the administration had hoped for at the beginning, but he added: "The decline in the share of GNP devoted to federal spending is now expected to start next year."[33] This expectation was not met. Indeed, when, early in fiscal-year 1983, Friedman declared federal spending to be "already far too high" (*Newsweek*, December 6, 1982), spending was actually well on its way not only to rise in real terms during the fiscal year but also to reach a new peacetime high as a fraction of output.

On the basis of budgetary documents issued in January 1983, the Friedmans would make their own calculations of federal spending and aggregate U.S. income and concurred with the official publications on the points that total federal expenditures rose as a share of national income in fiscal-years 1981 and 1982 and that the share would rise considerably further in the 1983 fiscal year.[34] Despite the stronger-than-expected economic recovery in 1983—a development that would tend to restrain the rise in the spending share—the expected increase in the share largely occurred, and Paul Volcker testified on March 1, 1984, that "expenditures are much higher relative to the GNP than they have been historically."[35] Modern data on the share of federal outlays in GDP record that it rose to 23.5 percent in fiscal-year 1983, higher than any other previous value since 1946 and a value not exceeded until 2009 (Council of Economic Advisers, 2011, p. 284, Table B–79).

In their diagnosis regarding this behavior of the spending share, the Friedmans rejected the 1981–1982 recession as the primary explanation: "Government spending continued to rise as a fraction of income *even after allowing for the expenditures associated with recession*."[36] Rather, they pointed to inadequate curbing of in-built growth in outlays. "I think he should have asked for larger cuts," Friedman remarked of the president (*Wall Street Week*, Maryland Public Television, April 25, 1984, p. 9 of transcript). The point at which Reagan should have done so was soon after his inauguration, when he had a new mandate and maximum goodwill: "I wish that [on taking office] he had been even bolder than he was and had urged much larger cuts than he did." (*The MacNeil/Lehrer News Hour*, PBS, March 27, 1984, p. 6 of transcript.) The Friedmans suggested that if the president in 1981 had asked for much bigger reductions in outlays, he would not have got everything he asked for but would have achieved larger budgetary restraint than those actually seen.[37]

[33] Weidenbaum (1982, p. 10).

[34] Friedman and Friedman (1984, p. 30;1985, p. 36). On their source documents, see Friedman and Friedman (1984, pp. 19–20; 1985, pp. 25–26).

[35] In Committee on the Budget, U.S. House of Representatives (1984, pp. 55-56).

[36] Friedman and Friedman (1984, p. 2; 1985, p. 10). Emphasis in original.

[37] Friedman and Friedman (1984, p. 9; 1985, p. 16). The same judgment was expressed in Friedman (1984f, p. 30).

Friedman contrasted the situation in the president's first year and since then: "In 1981 the president got a lot of what he asked for… But, since then, any relation between the budgets he's submitted and what Congress has come up with has been purely coincidental."[38] He expressed qualifications about cutting defense spending (see the next chapter). This spending item aside, however, Friedman suggested that "budget cuts ought to be across the board" and added with regard to the president: "I hope if he's reelected, one of the things he will do in 1985 is to urge really big cuts in government spending."[39]

But Reagan's new term that started in 1985 was not marked either by a revival of the 1981 priority on reducing domestic spending or by a return to 1981-style success in securing Congressional approval of a high share of those cuts he did propose. The period since 1981 had actually seen some of the original spending cuts rescinded, while Reagan's expenditure-reduction proposals after 1981 tended to be much less ambitious, and a proposal he made in February 1985 to eliminate 20 domestic programs was rebuffed in the Congressional negotiations (*Wall Street Journal*, October 21, 1985). When, in early 1986 Reagan's proposed budget for fiscal-year 1987 contained proposed domestic spending cuts, Friedman advocated these reductions in an op-ed, while also stressing their modest character and that implementing them fully would still imply a real increase in federal spending (*San Francisco Chronicl*e, March 5, 1986). Furthermore, once the pattern of Congress rejecting Reagan's domestic-spending proposals had become established, some of the proposed Reagan cuts assumed a *pro forma* character: for example, Friedman would later refer to the government "agencies that, year after year, President Reagan unsuccessfully tried to abolish" (*San Francisco Chronicle*, July 2, 1990, p. C6).[40]

Nevertheless, judgments that appeared around the middle of Reagan's first term declaring his smaller-government campaign completely defeated went too far. An example of those writing such obituaries was Walter Heller, who, in a *Wall Street Journal* op-ed (March 23, 1983), referred to Reagan's "conversion," citing "the expanding federal sector under his presidency" as part of "a return to good old Keynesian economics." Quantitatively, Heller's analysis was enduring in some respects—notably, he correctly assessed that the spending share was unlikely to fall back to (or below) its 1979 value in the near future, and indeed it did not do so until fiscal-year 1997. In other respects, however, it would turn out that Heller had overstated the rise in the share under Reagan: he expected it to be a full 4 percentage points above its 1979 level in 1983.

[38] *The MacNeil/Lehrer News Hour*, PBS, March 27, 1984, p. 5 of transcript.
[39] *The MacNeil/Lehrer News Hour*, PBS, March 27, 1984, pp. 5, 6–7 of transcript.
[40] A list of various such agencies appeared in *Wall Street Journal*, October 21, 1985 (p. 16).

In contrast, in modern data, this rise is reckoned to have been only 3.4 percent and to have been followed by a fall beginning in fiscal-year 1984.[41]

In what was, in effect, an overstatement of matters similar to that Heller had made, *Fortune* magazine, in looking at Reagan's longer-term budgetary projections, declared: "Reagan Steps Back from Reaganomics" (February 21, 1983). It further stated (p. 68) that, in contrast to 1981, when Reagan had hoped to deliver a spending share of 19 percent by 1984, "Now he's saying that it could take until 1989 or later just to get back down to the 22 percent of GNP that he inherited from Jimmy Carter." By late in Reagan's second term, however, things were turning out differently: having peaked in fiscal-year 1983, the federal share of outlays in GDP was 21.6 percent in fiscal 1987, just below the value in fiscal 1980. And the share continued to decline in fiscal years 1988 and 1989, while still remaining above most of the values recorded during the 1970s (Council of Economic Advisers, 2011, p. 284, Table B–79).[42]

Friedman would later remark: "From 1985 on, government spending fell as a fraction of income… Not a single new major civilian spending bill was passed in the Reagan years." (*New York Times*, February 2, 1992.)[43]

So although Reagan fell short of his aim to reduce the role of government, his overall tenure did see a reduction in this role, using the criterion of the federal government's spending share. On this dimension, the Friedman were accurate in stressing that moves to expand government spending fell out of favor during the Reagan years and that this shift in mood could have material effects on fiscal policy outcomes.

As much of the progress in reducing the spending share was concentrated in the later Reagan years, it is evident that the declining share reflected in considerable measure developments in the second half of the decade rather than in Reagan's first term. That later period is covered in Chapter 16 below.

Even in Reagan's first term, however, there were, beneath the surface, movements in the

[41] In common with Heller's estimate and the modern data, Friedman and Friedman (1984, 1985) suggested that fiscal-year 1983 saw a new peacetime peak in the outlays share. But the Friedman and Friedman (1984, 1985) assessments of this share used a smaller denominator (national income) and a start date of 1980—and so was hard to compare with Heller's numbers. As in the case of Heller's calculations, however, their numbers likely overstated the rise in the share through fiscal-year 1983.

[42] In addition, as discussed below, Martin Feldstein was also proud of Reagan's 1983 budget for embedding a five-year plan (not implemented) to eliminate the budget deficit.

[43] As indicated above, modern data suggest that the share actually peaked in (fiscal-year) 1984.

direction of a less steep government spending trend, beyond the initial round of spending cuts secured in 1981. Factors making for domestic federal spending restraint included, as Friedman stressed so heavily, the lack of a new major domestic federal spending program in Reagan's years in office. In addition, however, there was a change in the upward slope of the path of actual domestic government spending. Late in Reagan's first term, Martin Feldstein, by then CEA chairman, suggested that there had been "a remarkable revolution" in public expenditure: he contended that "all domestic spending other than Social Security and Medicare has already declined significantly as a share of GNP and is expected to decline further in the coming years." (*New York Times,* March 5, 1984, p. D14.) This judgment was borne out eighteen months later, in a *Wall Street Journal* analysis of the Reagan Administration's spending record. Although the article stressed the setbacks that Reagan had faced in curbing the U.S. public sector, it acknowledged: "Domestic spending growth has slowed sharply." (October 21, 1985, p. 1.)

Starve-the-beast arguments and the Laffer curve

The same *Wall Street Journal* article noted of members of the administration, "they deny charges that they actually planned the huge deficits as a way to force social spending down."[44] The administration's policy had, indeed, not been one of deliberately entering a period of very large budget deficits. Certainly, the administration prioritized maintenance of tax cuts over short-term reductions in the budget deficit. But, once faced with multi-year deficits, it did not leave it to Congress to find a way to bring spending down. On the contrary, as indicated, Reagan proposed a full budget each year, in which some spending-restraint proposals were provided, albeit usually not on a scale that would close the deficits.[45] And in his speech at the Atlanta conference, CEA chairman Weidenbaum declared in defiance of the starve-the-beast argument: "Cutting taxes provides no assurance that comparable spending offsets will be forthcoming."[46]

Nevertheless, President Reagan gave credence to the notion that starve-the-beast considerations underlay the administration's policy through his occasional articulations of the starve-the-beast argument. The *New York Times* (March 5, 1984, p. D14) did not provide a source when it referred to the "contention… of President Reagan… that the most important reason for reducing

[44] October 21, 1985, p. 16.

[45] Martin Feldstein pointed to a notable exception: "the first budget that I worked on was the 1983 budget, and we did put enough in there on spending cuts and tax increases that we got to a balance [over a five-year period], at least on paper, and the president signed off on it. But the political people—When it came to dealing with it on the Hill, their heart was certainly not in it, and so they didn't push the president's own budget." (Martin Feldstein, interview, November 21, 2013.)

[46] Weidenbaum (1982, p. 10).

taxes is to stop the growth of government spending." But Reagan had indeed made public statements as president in support of the notion that tax cuts would promote spending restraint (although perhaps not the specific claim that it was the "most important reason" for doing so). An early case in point was ahead of the tax cuts' passage when, in an address to the nation on February 5, 1981, Reagan suggested that "simply cutting their allowance" would limit the spending propensities of policymakers in Washington (quoted in Eisner, 1986, p. 158, and in Romer and Romer, 2009, p. 139).

Friedman, in contrast to the official administration policy, made the possibility that deficits would encourage spending restraint a central argument for cutting taxes. Indeed, he had cited that argument when endorsing the Kemp-Roth bill in 1978 (see Chapter 9 above). For him, the induced downward pressure on public spending was a key attraction of tax cuts—and was something to be highlighted rather than made a secondary or covert argument for such cuts.

Friedman forcefully restated the starve-the-beast argument in his *Newsweek* column of February 23, 1981. He recalled that, "years ago," he had shifted to being essentially unconditionally in favor of cutting taxes on account of the implications for government-spending control, and he enthusiastically interpreted the Reagan program in that light: "If the tax cut threatens bigger deficits, the political appeal of balancing the budget is harnessed to reducing government spending rather than to raising taxes." Evidently buoyed by Reagan's recent allusion to the same argument, Friedman suggested that this was "the way that President Reagan proposes to follow."

The argument applied symmetrically to tax increases, and Friedman was pleased when, in his State of the Union address of January 26, 1982, President Reagan invoked the argument in that connection. "Higher taxes would not mean lower deficits," the president observed. "If they did, how would we explain that tax revenues more than doubled just since 1976; yet in that same six-year period we ran the largest series of deficits in our history… Raising taxes won't balance the budget; it will encourage more government spending and less private investment."[47] In his March talk in Atlanta, Friedman remarked: "I agree fully with what President Reagan said in his State of the Union message: increasing taxes now would not reduce the deficit, it would simply increase spending. The Congress will spend whatever the tax system will raise, plus some more."[48]

[47] Reagan (1982c).

[48] Friedman (1982a, p. 60). Friedman also recalled this Reagan statement favorably in *Newsweek* (May 31, 1982) and in *U.S. News and World Report* (January 31, 1983, p. 67) (reprinted in McClelland, 1983, p. 59).

Friedman remarked on television several days later that "the fundamental problem is to hold down government spending, and there is only one way to hold down government spending, and that's to hold down the amount of income that Congress has to spend. It's no different with the Congress than it is with kids who are on allowances. If they're spending too much, there's only one way to curb them… Cut their allowance."[49]

As new highs of budget deficits emerged during 1982, Friedman maintained his stand on the taxes/spending relation. "The only way to hold down spending is to cut the amount of revenue," he remarked early in the following year.[50] Alan Greenspan continued to be largely on Friedman's side of the argument, remarking in mid-1983 that "a tax increase wholly by itself probably would end up financing higher expenditures" (*Meet the Press*, NBC, July 17, 1983, p. 3 of transcript). The person whom Greenspan would eventually succeed as Federal Reserve Chair—Paul Volcker—was, however, not persuaded. In testimony given in early 1984, Volcker—an outspoken proponent of tax increases as a means of helping to reduce the federal budget deficit—took issue with the generalization that tax increases had not reduced deficits in the past. In contrast to Friedman's position that it took an "utter inability to learn from experience" to suggest that rises in tax revenue did not drive increases in outlays (*Newsweek,* November 23, 1981), Volcker suggested: "That is not the way I read the record… Whether any increase in revenues will necessarily be followed by a still further increase in spending—I don't think the historical record bears that out."[51]

Around the same time, Paul Samuelson was ready to declare the starve-the-beast hypothesis as having been refuted by recent years' events. As the deficits of Reagan's first term had not given rise so far to "the desired Reagan- Friedman expenditure cuts," Samuelson argued that the conclusion to be drawn was: "The tax cuts meant huge structural deficits ahead."[52] But Friedman refused to see the validity of the taxes-drive-spending position in these terms. In contrast to Samuelson's imagery of a game of Russian roulette, and the imagery invoked by Eisner (1986, p. 158) and others of a game of chicken, Friedman did not see the pressure for lower spending associated with a budget deficit as associated with a terminal point at which legislators responded to the pressure and acquiesced in spending cuts. Rather than seeing things in terms of a threshold being reached, he viewed the starve-the-beast pressures as being *continually* in operation: "at the moment, the deficit is serving a useful function. It's only the deficit that's

[49] *Meet the Press*, NBC, March 21, 1982, p. 5 of transcript.
[50] *U.S. News and World Report*, January 31, 1983, p. 67 (reprinted in McClelland, 1983, p. 59).
[51] From Volcker's testimony of March 1, 1984, in Committee on the Budget, U.S. House of Representatives (1984, pp. 55, 56).
[52] Samuelson (1984, p. 7).

keeping Congress from letting its spending propensities go mad." (*Wall Street Week,* Maryland Public Television, November 15, 1985, p. 7 of transcript.)

Friedman therefore saw revenue restraint as an important factor bearing down on the deficit over time. But he did not engage in predicting the course of the deficit. And he had little time for others' attempts to do so: "The figures written on what the deficit or interest rates will be five years from now are not worth the paper they're printed on." (*San Francisco Chronicle,* August 21, 1984.)

Furthermore, Friedman did not see the taxes-drive-spending argument as being adhered to only by those on his side of the debate on the size of government. Instead, he regarded some leading Democratic politicians as subscribing to the same position and this viewpoint as being the source of their opposition to the Reagan tax cut.[53] Not long after the tax reduction was signed into law in 1981, Friedman noted that a "drive to rescind some of the tax cuts" was already starting and stated of those in Congress who had opposed passage in the first place: "They professed to be voting against 'irresponsible' tax cuts. They really were voting for future spending increases."[54] Similarly, three years later, in a discussion of the presidential election campaign for the *San Francisco Chronicle* (September 14, 1984), Friedman contended that, the increase in government spending under Reagan notwithstanding, "there is a real difference between the candidates," as Democratic presidential candidate Walter Mondale would create an environment in which the pressure to restrain spending would be dissipated. Friedman lashed out at Mondale's proposal to increase taxes, arguing that a rise in tax rates "would make it easier to increase government spending—or at least to resist cuts."

Another oft-cited rationale for the Reagan tax cuts—and again one that was not embedded in the official U.S. government position—was the Laffer-curve possibility that tax-rate cuts would generate higher tax revenues. In his March 1982 address, then-CEA chair Weidenbaum made clear that "especially in the short term, general tax-rate reductions will increase the budget deficit," and reminded the audience that "from the outset the Reagan Administration had proceeded on that basis."[55] He acknowledged that the contrary position was associated with the administration—in part, Weidenbaum noted, because "friends" of the administration had indeed

[53] The tax cut had received heavy bipartisan support in 1981 in the Senate but had had greater Democratic party opposition in the U.S. House of Representatives and outside Congress.

[54] *Newsweek*, November 23, 1981. Friedman and Friedman (1984, pp. 88–89; 1985, p. 89) would suggest that the subsequent drive to rescind the planned 1985 introduction of income tax indexation was similarly based on an agenda of raising government expenditure.

[55] Weidenbaum (1982, pp. 10, 9).

suggested, in contrast to the official government projections, that tax cuts would be self-financing or better.[56]

As with starve-the-beast arguments, however, the Laffer-curve position did emanate from the administration on occasion, despite the fact that official economic policy was not premised on it. In particular, Ronald Reagan, who had repeatedly invoked the Laffer-curve argument during the 1970s (see Chapter 9), continued to appeal to it in remarks made during the 1980s, especially in the context of likening the 1964 tax cut to his own proposals. In his GOP convention speech of July 17, 1980, when accepting the Republican presidential nomination, Reagan had stated that every major tax cut had been a source of new revenues.[57] Three months earlier, *Time* magazine (April 14, 1980) had quoted him as saying of the Kennedy tax cut: "In the very first year, the government got $5.8 billion more in total revenue.'"[58] And early in his second term as president, Reagan remarked to a member of Congress: "You can't show me a time in history when a major tax cut did not result in greater revenue. (*New York Times,* July 11, 1985.)[59]

Friedman, however, continued to associate this idea mainly with economists outside the administration who were in large part opponents of his, even though they, like him, were supportive of Reagan. "'Supply side' has gotten a bad name, because it has been linked with some foolish predictions by some people that cutting taxes, no matter how it was done, would inevitably raise government receipts and eliminate deficits. And that of course is not right," Friedman remarked in mid-1984.[60] He did not name names on that occasion. Indeed, Friedman mostly stopped short of attributing to Arthur Laffer the empirical contention that the tax cut legislated in 1981 would promptly pay for itself (or more than pay for itself). In January 1982, he had attributed the position that it would be self-financing to Kemp rather than Laffer and predicted that it would soon be refuted: "it just isn't going to yield high enough revenues to beat the deficit." (*Daily News* (New York), January 19, 1982.) Speaking on October 4, 1983, he did name Kemp, Laffer, and Jude Wanniski as having "overpromised on what the tax cuts would do."[61] But this likely was in part a reference to the fact that this group, which often spoke of

[56] Weidenbaum (1982, p. 9).

[57] See Reagan (1980b). In addition, Feldstein (1994, p. 21) provided an example of a July 1981 statement in which Reagan stated that both he and President Kennedy believed that their tax-cut proposals would be revenue-boosting.

[58] Reagan also cited the Kennedy-Johnson tax cut as a measure that generated revenues in an interview in the February 1982 issue of *Readers Digest* (*Boston Herald American*, January 18, 1982).

[59] Similarly, in a portion of a January 1986 cabinet meeting for which television cameras were permitted, Reagan quoted and endorsed a statement by President Kennedy that "an economy stifled by restrictive tax rates will never produce enough revenue to balance the budget, just as it will never produce enough jobs and enough profits." (Excerpted in CSPAN, December 14, 2005.)

[60] In R.H. Heller and others (1984, p. 46).

[61] In Friedman (1984f, p. 44).

monetary policy's role in controlling inflation but rarely discussed the mechanisms through which that control operated, had denied the need for a temporary period of economic weakness in the process of achieving disinflation.

For his part, Friedman not only anticipated that the tax cuts would reduce tax revenue, but also insisted, on starve-the-beast grounds, that it should be a design principle of the tax cuts. A tax cut that raised tax revenue would be undesirable in Friedman's view because it would be followed in due course by higher government spending—so a revenue-neutral or revenue-enhancing tax package was *not* what he wanted out of the Reagan program. At the start of the Reagan years, he wrote that "the aim should be not higher revenue but lower revenue… If a legislated rate cut did mean higher revenue, that would simply demonstrate that the rates had not been cut enough." (*Newsweek*, February 23, 1981.) Accordingly, he observed in his March 1982 talk, "[i]f the Lafferites were correct in the most extreme form," and the tax cut moved the United States to a higher point on the Laffer curve rather than a lower one, tax rates would have to be reduced still further, so that the United States reached a point on the Laffer curve sufficiently far to the left in order to generate lower tax revenues than those prevailing before the tax cut.[62]

In response to the backlash in public debate against the tax cuts, occurring after their passage but prior to their main implementation, Arthur Laffer suggested that the tax reductions should go ahead but that some of the Reagan Administration's domestic spending cuts should be rescinded: "He should restore some of the funds that have been cut for social programs." (*U.S. News and World Report*, January 18, 1982, p. 36.) In the same interview, however, Laffer indicated that he would be amenable to reducing domestic spending once the economy was well into recovery. Roughly in line with his suggestion, nearly four years later (and three years into renewed economic expansion), Laffer endorsed spending cuts—and indeed cast things in starve-the-beast terms, declaring that a tax increase "will remove the pressure on Congress to cut spending." He acknowledged that the 1981 tax cut had so far "cost revenue," but he suggested that this was because "a tax cut takes time to pay for itself." (*San Francisco Chronicle*, December 26, 1985.) Laffer would subsequently argue that the tax cut was roughly self-financing by the time Reagan left office (*Investor's Business Daily*, April 9, 1996). As for why large deficits emerged in the 1980s, he remarked: "Tax revenues under Reagan went through the ceiling. So did spending, however." (*The Business*, Australian Broadcasting Commission, March 17, 2015.)

[62] Friedman (1982a, p.60).

The Reagan CEA

The relationship between monetarists and supply-siders was at a low point in 1982. This situation was evidenced in Friedman's disparaging remarks early in the year about their views on tax revenues and on the Gold Standard (*Daily News* (New York), January 19, 1982). Other prominent monetarists also had poor relations with the supply-side movement, with Allan Meltzer joking in testimony in March that an image that pleased him was that of a busload of supply-siders sinking into a valley.[63]

Supply-siders and monetarists tended, however, to be on the same side in PEPAB meetings with President Reagan on the issue of deficit control—with both opposing tax increases and offering more general support to a change in direction on the president's part. As discussed in Chapter 12, the makeup of the PEPAB, like that of other economic groups in the administration, attested to the Reagan Administration's establishment of a coalition of supply-siders and monetarists in support of its economic policy.[64] In terms of personalities, the presence of Friedman and Laffer on the PEPAB, together with figures such as Martin Anderson and (in 1981–1982) George Shultz—both of whom had closer ties to Friedman and Laffer individually than Friedman and Laffer had with one another—underlined the Reagan Administration's development of a sometimes shaky alliance between these economic movements.[65]

"I think the function [of PEPAB] was a very useful one for the president," Allan Meltzer remarked (interview, April 21, 2013). "He gets scores of critics all the time telling him: You're doing this wrong, you're doing that wrong, you should be doing this, you should be doing that. And this was a group that, with a few exceptions, mostly told him: Keep doing what you're doing, it's going to work out. And in the tough days of 1982, when the economy was headed toward 10.8 percent unemployment, that was tough advice to follow and he needed some help,

[63] See Committee on Finance, U.S. Senate (1982, p. 180).

[64] It also in practice mixed free-market microeconomists or public-finance theorists who had continuing close links with the research and academic community with supply-side figures who were, or had become, primarily active un media, financial, or policy circles. A member of the former group, Martin Weidenbaum, referred to the situation he faced in the early period of the administration of finding "a compromise between my view of reality and the really way-out estimates of the supply-siders." (*Wall Street Week*, Maryland Public Television, May 23, 1986, p. 7 of transcript.)

[65] Laffer described the position he took in the 1980s with regard to the ideal assignments as follows: "the best thing in the world—Nirvana, Valhalla—is George Shultz at the head of fiscal policy, and Paul Volcker at the head of monetary policy." In contrast, he remarked, his "modern-day conception of hell would be Paul Volcker at the head of fiscal policy, and George Shultz at the head of monetary policy." Laffer's basis for the latter position was that he considered Volcker to be too much in favor of tax increases and Shultz to be too much in favor of monetarism (Arthur Laffer, interview, August 11, 2014).

[though] it was [already] his inclination to follow it... I mean, a president needs something which tells him: Look, get a long-term objective and follow it, it may be bloody hell to get there but that's where you want to go."

The coalition nature of the Reagan Administration's economic team was evident not only in the PEPAB membership but also in the makeup of the advisory body that was a permanent part of the administration was the three-member Council of Economic Advisers. By tradition—albeit with notable exceptions, including Alan Greenspan during the Ford years—the CEA was composed of economists that had backgrounds as professional academics. The lack of standing of the supply-side economics movement in formal economic-research circles, including in most universities, tended to mean that the Reagan Administration CEA did not feature the most well-known figures associated with supply-side economics. Like the PEPAB, however, the CEA over the Reagan years tended to comprise a combination of monetarists and of market-oriented economists who were not associated with monetarism. A member of the latter group was Murray Weidenbaum, who, as already noted, was the Reagan Administration's first CEA head.

Weidenbaum was not a monetary specialist and would explicitly describe "monetarists" as a separate group from himself.[66] Friedman nevertheless applauded the Weidenbaum appointment. Pointing to Weidenbaum's pro-market and research credentials, Friedman—in remarks made on January 22, 1981, soon after the administration took office—described him as a specialist in the economics of the firm and as having set the standard in quantifying the economic cost of government regulation. Friedman contrasted this background with that of past CEA heads (such as Walter Heller and his successors in the 1960s) who had been steeped in demand management and indicated that this was a hopeful indication that U.S. economic policy would become less oriented toward fine-tuning and less closely keyed to reliance on forecasts.[67]

In fact, Weidenbaum had a substantial background in macroeconomic-management issues in the fiscal area. Since 1960, he had primarily had an academic career, located mainly at the Washington University-St. Louis, and produced the regulation-related research Friedman had described. But he had worked as an economist at the U.S. Bureau of the Budget from 1949 to 1957 (*St. Louis Globe-Democrat*, May 3, 1969) and then been Assistant Secretary of the Treasury in the early years of the Nixon Administration. Weidenbaum's experience in U.S. fiscal policy had nevertheless been in periods during which Keynesian fiscal-management ideas

[66] See Weidenbaum (1986). See also Weidenbaum's (1970, pp. 14–15) contention that the 1968 income tax surcharge had restrained aggregate demand.
[67] Oppenheimer and Company (1981a, p. 3).

had been less prevalent or when there had been pressure to restrain the government budget, particularly its domestic component.[68] He had become disillusioned with U.S. government policy in the decade after the Nixon Administration broke from its gradualist policy in 1971. Reflecting this judgment, when speaking at the conference on supply-side economics that he and Friedman attended in Atlanta in March 1982, Weidenbaum (1982, p. 12) referred to the "stop-and-go economic policy in the1970s."

In the same January 1981 remarks in which he praised Weidenbaum's appointment, Friedman noted that it was still unknown who would be appointed to the CEA as a monetary policy specialist (Oppenheimer and Company, 1981a, p. 3). Eventually, Jerry Jordan became such a CEA member, serving on the Reagan Council of Economic Advisers for a year from July 1981. During his tenure, Jordan noted in Congressional testimony on September 23, 1981: "For one semester, Milton Friedman was a teacher of mine at UCLA… I have very great respect for Professor Friedman. As professional economists, we sometimes will disagree on some fine point. But, for the most part, I share his views."[69]

The Council during the Weidenbaum-Jordan period produced a document that probably reflected Friedman's influence on the Reagan Administration more than any other publication. Tobin (1982, p. 297) noted the parallel between the *Economic Report of the President* for 1982—the first issued by the Reagan Administration—and the corresponding report for 1962—the first of the Kennedy Administration (Council of Economic Advisers, 1962)—as both reports had articulated a changed intellectual framework for the conduct of economic policy. In terms of its content, however, the 1982 *Economic Report* paralleled less the Kennedy Administration's first Report but another item that saw print in 1962—*Capitalism and Freedom*. Rayack (1987, pp. 9–13) documented the fact that some passages of the 1982 *Economic Report* on market economics, including those dealing with the relationship between economic and political freedom, had close analogues in *Capitalism and Freedom*. The monetarist literature, likely largely via Jerry Jordan, also left a clear stamp on the discussion of monetary policy and fiscal policy in the 1982 *Report*, with Tobin (1982, p. 304) expressing irritation at the *Report*'s stress on the point that the interest rate is the price of credit rather than the price of money—an old Friedman refrain.[70]

[68] See in particular Weidenbaum's (1970, p. 17) support for Nixon's goal at the time to attain full-employment budget balance.

[69] In Committee on Small Business, U.S. Senate (1982, p. 50).

[70] The *Report*'s discussion of the compatibility of monetary policy and supply-side economics (see Council of Economic Advisers, 1982, p. 21, and the discussion in Tobin, 1982, pp. 300–301) lined up with the position that

The *Economic Report* was released in February 1982. And, in effect underscoring the links between the Reagan Administration's outlook on economic policy and that of Friedman, the president quoted (or, at least, attributed a statement to) Friedman in a press conference in the same month.[71]

On October 1, 1982, Reagan nominated William Poole to be the newest CEA member (see White House, 1982). Poole had received his Ph.D. in 1966 from the University of Chicago's business school and had been in Friedman's money-and-banking Workshop during his time as a graduate student. Poole had already been an acting CEA member for some time before his formal nomination. During this period, he was asked by the *Washington Post* about the philosophy of the Reagan CEA and its staff of economists, Poole stated that what "most clearly unites" the CEA members and staff was "our market orientation"—"All of us feel government should have a less pervasive economic role in our society." (*Washington Post*, October 3, 1982, p. M3.) This was a bold generalization indeed, as the economist staff of the CEA at that time included Paul Krugman.[72] Poole remained on the CEA until 1985.

The CEA chair when Poole joined the council was Martin Feldstein, who had succeeded to the role in October 1982, Weidenbaum having resigned in August. Feldstein, in turn, left in July 1984. The position of CEA head was then vacant until April 1985. Beryl Sprinkel, already a monetarist voice in the administration due to his position at the U.S. Treasury, moved over to the CEA to become its new head in April 1985. He stayed in the position for the rest of Reagan's tenure as president.[73]

TEFRA, 1982

The CEA chairs in Reagan's first term—Weidenbaum and Feldstein—had similar positions on deficit reduction and differed strongly from Friedman on this matter. Neither Weidenbaum nor Feldstein was persuaded of the applicability to the U.S. fiscal situation of the starve-the-beast argument. Furthermore, both CEA chairs assigned a high priority to securing substantial

Jordan had taken on the matter in his Congressional testimony, already mentioned, of September 23, 1981 (see Committee on Small Business, U.S. Senate, 1982, p. 50).

[71] In his press conference held on February 18, 1982, in the context of a discussion of welfare programs, Reagan remarked, "it's what Milton Friedman once said, that if you start paying people to be poor, you're going to have a lot of poor people. And we want to help people that are poor but help them get to the place that they can take care of themselves." (Reagan, 1982d.)

[72] In a Twitter contribution of June 11, 2019, Krugman said that he accepted the CEA staff position despite disagreeing with Reagan's policies and that the technical character of his work made this possible.

[73] See *http://www.whitehouse.gov/administration/eop/cea/about/former-chairs*.

reductions in the U.S. federal budget deficit and, in their respective tenures, each of them was willing to endorse tax increases as a means of achieving major and prompt reductions in the deficit.

In neither case did Weidenbaum's and Feldstein's concerns about deficits arise from a nonmonetarist belief that the deficit was *per se* inflationary. On the contrary, each of them had a monetary view of inflation: Weidenbaum noted in March 1982 that "my colleagues and I have pointed out on innumerable occasions [that] the inflationary consequences that have so often been attributed to past budget deficits have actually been the result of inappropriate monetary policy, which monetized those deficits."[74] Similarly, Martin Feldstein, testifying on March 29, 1984, noted: "We are beginning to hear more comment and concern about inflation these days. Certainly, any return to the inflation experience of the late 1970s would be terrible."[75] But he indicated that this was not a reason to be concerned about deficits: "A sound monetary policy is the key to sustained progress on inflation."[76]

Nevertheless, both Weidenbaum and Feldstein were concerned about adverse effects of large budget deficits. Weidenbaum was used to reining in budget deficits when in government, and had, shortly before joining the Nixon Administration, referred to "the reality of a massive budget deficit" when referring to the years from 1965 to 1968 (*St. Louis Globe-Democrat*, May 3, 1969). In 1982, as CEA chair, he was instead seeing the emergence of a far larger budget deficit. Among his fears were that the new open-ended period of large deficits might lead to a situation in which the deficits would be monetized (Weidenbaum, 1986, p. 22).[77] For his part, Feldstein, being a longstanding critic of Ricardian equivalence, was concerned primarily about serious crowding-out effects of the deficits.

In their respective tenures, Weidenbaum and Feldstein became proponents of tax increases to help close the deficit. Weidenbaum would recall saying to the president in early 1982: "Mr. President, none of us wants to raise taxes. The problem is, we just haven't cut spending enough." (*Wall Street Journal*, October 21, 1985, p. 16.) During Reagan's first term, the main

[74] Weidenbaum (1982, p. 10). He added: "I would give the primary credit for less inflation [in 1982] to the Federal Reserve and its monetary policy." Weidenbaum did, however, become wary about monetary aggregates in the mid-1980s and was critical of "monetarists and other inflation alarmists," as "the alarmists who have been forecasting an early return to escalating if not double-digit inflation were wrong." (Weidenbaum, 1986, p. 22.)

[75] In Committee on Ways and Means, U.S. House of Representatives (1984, p. 56). On the commentary and concern to which Feldstein referred, see the previous chapter.

[76] In Committee on Ways and Means, U.S. House of Representatives (1984, p. 56).

[77] Weidenbaum was also, however, a longstanding subscriber to the view that unmonetized deficits tended to raise real interest rates. See, for example, Weidenbaum (1970, pp. 15–16).

item that arose from the agitation, in U.S. public discourse, to raise taxes was a law passed during the interregnum between Weidenbaum's and Feldstein's tenures. Reagan signed the 1982 Tax Equity and Fiscal Responsibility Act (TEFRA) in September, shortly after Weidenbaum's departure and before Feldstein had joined the administration. Reagan had, however, already agreed in principle to the measure in July (see Weber and Weber, 2010, pp. 71–72) and discussed his acquiescence to TEFRA in an address to the nation on August 16, 1982 (see Reagan, 1982b).

The act was assessed to have canceled or withdrawn about a quarter of the tax cuts legislated in 1981 (Boskin, 1987, p. 66)—although such calculations were rendered difficult by the fact that the scale of the disinflation after 1981 increased the real amount of some of the legislated 1981 cuts while decreasing that of others.

In his controversial biography of Ronald Reagan, Edmund Morris (1999, p. 798) cited an interview that he had with Friedman on August 11, 1989, as the basis for Morris' statement that Reagan "was seduced into his 1982 tax increase by a promise of three-dollar-to-one" provision of spending cuts for every revenue increase agreed upon. Friedman was an odd source to give for such a well-known aspect of Reagan's attitude to TEFRA. The three-dollar-to-one spending-to-taxes ratio had been explicitly referred to by Reagan in his August 1982 national address foreshadowing and endorsing TEFRA.[78]

Friedman was opposed to TEFRA. He opposed the president's initial agreement to consider a package that would include tax increases (*Newsweek*, May 31, 1982). And when the tax-increase agreement was under legislative consideration, he condemned it as being "a monstrosity that should not be passed" (*Newsweek*, August 23, 1982). The Friedmans affirmed their disappointment with the tax increase after it was passed.[79] Likewise, in a 1985 article, Friedman expressed concern that the severity of the recession during 1982, though due, in his view, to monetary policy, had created political momentum for TEFRA: "the occasion never would have arisen for the introduction of a tax increase bill in 1982. Bad monetary policy does not alter the need to lower tax rates rather than to raise them."[80] After TEFRA's passage, Friedman's position was there should be no recurrence of such a measure: "The president absolutely should not raise taxes to help close the deficit." (*U.S. News and World Report*, January 31, 1983, p. 67.)[81]

[78] Reagan (1982b) stated in the address: "Now, as you can see, that figures out to about a 3-to-1 ratio: $3 less in spending outlays for each $1 of increased revenue."
[79] Friedman and Friedman (1984, p. 91; 1985, p. 91).
[80] Friedman (1985b, p. 59).
[81] Reprinted in McClelland (1983, p. 59).

Reagan himself became disillusioned by the TEFRA experience. He regretted having adopted his 1982 posture of agreeing to a tax increase, especially as the federal government spending cuts that he believed were a *quid pro quo* of TEFRA (though not actually embedded in the legislation that he signed) did not prove to be forthcoming. Reagan summed up his recriminations in early 1984: "we were going to get three dollars in spending cuts for every dollar of increased revenue. We never got the three dollars in spending cuts."[82] The president's subsequent harder line was reflected in his January 1986 remark: "the only right way to reduce the federal deficit is to stop the explosion of domestic spending.'"[83]

In some accounts, TEFRA was portrayed as having gone a long way to canceling the original Reagan tax cut legislated in his first year in office. For example, financial columnist Maxwell Newton (*The Australian*, July 4, 1984) said of Reagan: "His famous 1981 tax cut was largely emasculated by later tax increases."[84]

The notion that TEFRA, in quantitative terms, canceled out the imprint that the 1981 legislation had on overall U.S. tax rates or the share of the economy taken by federal taxes appears, however, to be incorrect. Indeed, in his August 1982 address defending his support for TEFRA, Reagan (1982b) indicated that, even with this tax increase taken into account, the ratio of federal revenues to U.S. output would show about as great a decline from 1980's levels as that envisioned in his original plan. In the event, that ratio certainly continued to decline in 1983 and 1984—reaching, on modern data, 17.3 percent of GDP in fiscal-year-1984, 2.3 percentage points lower than in fiscal-year 1981.[85]

TEFRA increased corporations' tax liability in numerous respects and removed additional acceleration of investment deceleration that the 1981 tax legislation had directed be introduced in 1985–1986 (see Boskin, 1987, p. 56). So as Friedman had warned (*Newsweek*, August 23, 1982), TEFRA did raise some tax rates. Nevertheless, although tax rates on certain types of business investment were increased considerably by TEFRA, most of them were still lowered, on net, by the 1981 and 1982 tax legislation (see Feldstein and Jun, 1987, p. 102, and Boskin, 1988, p. 79). Accordingly, post-TEFRA, Modigliani (1988, p. 418) could still write of "the large investment incentives offered by the Reagan tax reform," and Bernanke (1987, p. 37) would note

[82] In Bartley and Hunt (1984).

[83] See CSPAN, December 14, 2005.

[84] Likewise, in his *New York Times* column of July 15, 2011, Paul Krugman wrote, "Reagan himself enacted significant tax increases, offsetting to a considerable extent his initial cuts."

[85] See Council of Economic Advisers (2011, p. 284, Table B–79). See also the discussion in Poterba (1994, p. 252) of the modest extent to which TEFRA wound back the 1981 act's effect on tax revenues.

that the "Kemp-Roth-Reagan cut… offered significant incentives to capital formation."

From Friedman's perspective, TEFRA was undesirable *per se* because it raised tax revenues, and—believing, as already noted, that the level of revenues was a continuous influence on the level of government outlays—he was immediately skeptical about whether the extra revenue had actually secured a lowering of the budget deficit (*U.S. News and World Report*, January 31, 1983, p. 67).[86] Other proponents of tax cuts, however, expressed relief that the 1982 tax increase had not been directed at moving up marginal rates. Arthur Laffer, for example, was critical of TEFRA, later suggesting that Reagan's acquiescence to the measure reflected a temporary loss of self-confidence on the president's part during the eighteen months after his shooting in March 1981 (*American Banker*, November 1, 1983). But he would also stress with regard to TEFRA: "It was not a marginal[-rate] tax increase." (Arthur Laffer, interview, August 11, 2014.)[87]

Correspondingly, although Murray Weidenbaum would suggest that it might have been desirable to cancel the final phase of the personal income tax cuts (*New York Times*, March 5, 1984, p. D14), this did not occur. Indeed, in a letter he wrote on July 30, 1982, to a disgruntled supporter, the defense that President Reagan offered of his agreement to the package included the fact that he had rebuffed those who "were determined to cancel the remaining tax cuts."[88] The ultimate phase of the personal income tax cut indeed went ahead on July 1, 1983, and Barro and Redlick's (2009) measures of the marginal tax rate continue to decline over the 1982−1984 period.

The tax cuts' implementation is completed, 1983

The final phase (other than indexation, which began in 1985) of the personal income tax cuts legislated in 1981 therefore proceeded near the end of Reagan's second year in office. Arthur Laffer had criticized the phased-in character of the taxes, arguing that "the incentives to postpone [payment of] taxes" until rates were lower would lower output by postponing production decisions until 1983 (*U.S. News and World Report*, January 18, 1982, p. 36). He affirmed in April 1982 that "they're all postponing… And what you're going to see, I think, in late '83, '84, an enormous expansion of the U.S. economy. 1983 will be the beginning of a big expansion." (*Wall Street Week*, Maryland Public Television, April 9, 1982, p. 7 of transcript.) Laffer subsequently perceived vindication in the strength of the recovery that began at the turn of the

[86] Reprinted in McClelland (1983, p. 59).

[87] Supply-sider Marc Miles expressed this view of TEFRA (personal communication, February 24, 2014). Reagan (1982b) himself expressed matters publicly as follows: "We did, however, agree to limited revenue increases so long as they didn't harm the incentive features of our economic recovery program."

[88] See Weber and Weber (2010, p. 72).

1982/1983 year, “precisely as should have been expected” (*Family Weekly*, March 18, 1984). He would further remark: “tax cuts don’t work until they take effect. Now when did the recovery begin? It literally began on January 1, 1983.” (Arthur Laffer, interview, August 11, 2014.)[89]

Friedman—being far more inclined to view output as demand-determined in the short run and to see monetary policy as the driver of much of U.S. output’s fluctuations—instead attributed the timing and scale of the 1983 recovery to the change in monetary policy. As discussed in the previous chapter, this has become the standard interpretation of output’s behavior in that year.[90]

Friedman did, however, see the tax cuts as having the effect of raising U.S. *potential* output and so, at some point, actual output, too. Symmetrically, with regard to the effect of tax increases, he endorsed President Reagan’s January 1982 statement, “Raising taxes will slow economic growth” (*Newsweek*, May 31, 1982) and himself added in his column that “higher taxes would reduce the incentive for individuals to work, to save, to invest” (*Newsweek*, December 6, 1982). Furthermore, he regarded these tax/output linkages as capable of materializing at the business-cycle frequency. The Friedmans suggested that the yo-yo nature of economic fluctuations, associated with erratic monetary policy, seen in 1981 and 1982 had prevented the tax cuts in those years from having their full supply-side effect on actual output.[91] By the same reasoning, the strong rate of monetary expansion in the year from July 1982—although regarded at the time by Friedman as being excessive—provided a better background against which the stimulative effects of tax cuts on work and production decisions could take place.

The Social Security commission

After a backlash against the administration’s proposals to scale back Social Security benefits, Reagan turned the whole matter over to a bipartisan commission headed by Alan Greenspan. This had the effect of largely defusing the topic in political debate for the immediate future, as the commission’s report (National Commission on Social Security Reform, 1983), which was released on January 20, 1983, enjoyed wide support and its recommendations were legislated,

[89] Laffer later elaborated that this date was that which the 1982 part of the tax cut (enacted the previous July) applied to a full calendar year, while adding, “The full effect of the 1983 cut [implemented on July 1] would come in 1984.” (Laffer, Domitrovic, and Sinquefield, 2022, p. 365.)

[90] The postponement mechanism stressed in Laffer’s discussion of developments in the early 1980s has, however, received some attention in the economic-research literature, with Nakamura and Steinsson (2018, p. 73) citing Mertens and Ravn (2012) as the basis for their statement: “Anticipation effects associated with the phased-in tax cuts of the Reagan Administration may also have played a role in the 1981–1982 recession.”

[91] Friedman and Friedman (1984, p. 91; 1985, p. 91). See also Friedman’s remarks in *Wall Street Journal*, June 28, 1983, and in Heertje (1984, p. 41).

with] Reagan signing it into law on April 20, 1983 (Svahn and Ross, 1983, p. 3).

Among the changes resulting from the 1983 package was that the retirement age relevant for Social Security was specified as to be gradually raised—so that it would reach, in the twenty-first century, age 66 and then age 67 (Boskin, 1987, p. 69). The 1983 reforms also included actual or *de facto* tax increases, including the introduction of a permanent six-month delay in cost-of-living adjustments of Social Security payments and making Social Security benefits partially taxable (Boskin, 1987, p. 69; Viard, 2012, p. 1118). Furthermore, previously legislated increases in payroll tax increases were expedited (Boskin, 1987, p. 69). These accelerated measures included an increase in the payroll tax rate facing both employers and employees—from 6.7 percent to 7 percent, with this increase now taking place in January 1984—although in the case of employees, an accompanying tax credit meant that this increase did not initially apply, on net. Further increases were scheduled to 7.05 percent in 1985 and 7.15 percent in 1986.[92]

Friedman was not one of those applauding this outcome. The characterization that he repeatedly used was that the commission had provided "Band-Aids."[93] Most graphically, in the *Tyranny of the Status Quo* book, he and Rose Friedman mocked the media coverage of the matter for having claimed that the commission had saved Social Security. They argued that the "Band-Aids" applied in the wake of the commission report served merely as a temporary means to "hide the bleeding" of Social Security.[94] They also continued to criticize Social Security on equity grounds, arguing that it "can accurately be described as middle-class, or, indeed, upper-class welfare."[95]

Friedman had earlier remarked on April 15, 1983, shortly before Reagan signed the post-commission legislative package: "After working long hours, the commission came up with a collection of Band-Aids. There was no real attack on the fundamental problems of the Social Security system, no attack on the fundamental question raised by a system under which you tax the current working people to provide benefits to current retired people and in which the number of working people per beneficiary is going down."[96]

[92] For these dates and rates see, for example, Railroad Retirement Board (1984, p. 6).
[93] See, for example, *Newsweek*, February 28, 1983, and Friedman's remarks about a year later (in an interview given at Stanford University in mid-February 1984): "The Social Security commission came up with Band-Aids." (*CBS Morning News*, March 1, 1984, p. 23 of transcript.)
[94] Friedman and Friedman (1984, p. 29; 1985, p. 34).
[95] Friedman and Friedman (1984, p. 30; 1985, p. 37).
[96] Friedman (1983b, pp. 121, 124).

He added, however, that even in his ideal, "much more radical reform," he would have the government redeeming its existing Social Security commitments.[97] In *Newsweek* (February 28, 1983), Friedman referred readers to the Friedmans' *Free To Choose* book for his preferred sweeping reform.[98] But, conditioning on the continuation of Social Security, he criticized the commission for not pursuing greater savings, with Friedman suggesting that the increase in the retirement age should start immediately, that indexation of benefits be discounted by about 2 percentage points, and that the "strong incentive" to retire early be removed.

When Friedman was making these critical commentaries over the first half of 1983, Alan Greenspan was still in contention to replace Paul Volcker as Federal Reserve chair when the latter's first term expired. Friedman's public discussions of the commission usually avoided taking Greenspan to task by name.

Friedman's own package of recommended changes reflected the priority he gave to preventing "the Social Security System [from] continuing to be a source of the rising line of total government spending."[99] He was disinclined to exclude Social Security-related outlays and revenues from aggregates of government spending and taxes.[100] His wish to count them as part of the federal budget led to a clash with Franco Modigliani when the two appeared together on television on February 6, 1986. Modigliani's basis for rejecting Friedman's contention that federal taxes had declined little in relation to U.S. national income since 1980 was that Social Security taxes should be excluded from this calculation.[101] Even with Social Security included, Modigliani was on solid ground in suggesting that aggregate federal tax revenues had fallen in relation to measures of U.S. aggregate income.[102] But as far as the marginal rate of federal income tax was concerned, the aggregate measure of the rate was virtually the same in 1985 as in

[97] Friedman (1983b, p. 124).

[98] This was also Friedman's reaction when, several weeks earlier, he had been asked at a press conference about his Social Security recommendations: "If you want to know, read the chapter in my book." (*Chicago Sun-Times*, January 19, 1983.)

[99] Friedman (1983b, p. 124).

[100] With regard to taxes, Poterba (1994, p. 255) noted the importance of Social Security tax increases in the 1980s: "Although often neglected in federal tax policy discussions, the payroll tax was a critical source of federal revenue growth… [It] rose from 6.1 percent of GNP in fiscal 1981 to 6.7 percent in fiscal 1985 to 7.0 percent in fiscal 1990."

[101] *The MacNeil/Lehrer News Report*, PBS, February 6, 1986, p. 12 of transcript.

[102] Data in Council of Economic Advisers (2011, p. 284, Table B–79) suggest that receipts as a share of GDP were 19.0 percent in fiscal-year 1980 and 17.7 percent in fiscal-year 1985. Poterba (1994, p. 237), using Office of Management and Budget tabulations, suggested a less steep decline—from 19.4 percent of GNP in fiscal-year 1980 to 18.6 percent in fiscal-year 1985. Revenues were also much higher in nominal and real dollars in 1985 than in 1980 (being about 42 percent higher in nominal terms in fiscal-year 1985 than in fiscal-year 1980, according to Council of Economic Advisers, 2011, p. 283, Table B–78). On that ground, there was a basis for Friedman's statement: "Excessive government spending and nothing else was responsible for the deficit." (*Corpus Christi Times* (Texas), November 6, 1986.)

1980: 39.9 percent versus 40 percent. Over this period, a fall of about 2½ cents in the federal income marginal tax rate had been offset by a rise of over two cents in employees' overall marginal rate of payroll tax as well, as by a slight rise in states' marginal income tax rates (Barro and Redlick, 2009, p. 42, Table 1).[103]

Tax reform

The fact that the overall marginal income tax rate was, near the start of Reagan's second term, roughly back to its 1980 level likely helps explain the direction that the fiscal policy agenda took during that term. In particular, there was renewed momentum during the mid-1980s to reduce tax rates. In considerable contrast to 1981, this time the rate-cutting campaign was presented as part of a major restructuring of the federal tax system. Or, as Reagan's original CEA head disapprovingly put it: "for most of 1985 politicians in both parties have been busy diverting attention from the difficult question of cutting the deficit by focusing on tax reform."[104]

Friedman, in contrast, did believe that it was a timely occasion for tax reform and, as discussed below, he believed that such reform could be an important part of deficit reduction. Indeed, Friedman's view was that the administration's interest in the subject had been too long in coming. He contended that the Reagan Administration in its early years had squandered the opportunity for tax reform—that is, for major alterations in the overall *structure* of federal taxation, as distinct from changes in tax rates in the context of a basically unchanged structure. "I do wish he had been much stronger from the very beginning, in terms of cutting spending and reforming the tax system," Friedman remarked of the president (*Wall Street Week*, Maryland Public Television, November 15, 1985, p. 9 of transcript).

On a matter pertaining to the intersection of personal and corporate income taxes, Friedman had been briefly encouraged by a statement that the president had made midway through his first term. In early 1983, Reagan publicly spoke of integrating corporate and personal income tax arrangements and so having profits taxed at the shareholder rather than the firm level. This lined up with Friedman's longstanding prescription—made in public statements going back to the early 1950s, and prominently featured in *Capitalism and Freedom* as well as in many subsequent Friedman discussions of public policy—to abolish the separate corporate income tax and have

[103] See also Miles (1988, p. 562).
[104] Weidenbaum (1986, p. 23).

undistributed profits categorized as income of the owners of the firm's stock.[105] "No part of our tax system… makes less economic sense than the corporate income tax," Friedman proclaimed in 1983, arguing that a "sensible system" would place the tax liability on corporations' income with "the stockholders and let it go at that." (*Wall Street Journal*, October 25, 1983.)

Proposals along these lines, which did not originate with Friedman but had his repeated support, did have considerable backing among other members of the economics profession. An illustration of this point was seen in January 1975, when Paul Volcker, appearing before a Congressional committee while he was out of public office, remarked: "integration with personal income tax—in the long run, I want to support that very much."[106] Nevertheless, support among economists for this kind of proposal was by no means uniform. In particular, Alan Auerbach, who was emerging in the early 1980s as one of the major researchers on the U.S. taxation system and its macroeconomic implications, was highly critical of such a reform. Indeed, Auerbach (1981) offered what he called a "1980s view" of tax integration and concluded that "traditional tax integration proposals may be poorly designed to accomplish their objective" of making the tax system more neutral (p. 21).[107] Auerbach later summarized matters by noting that "some of the work I've done suggests that taxes on dividends don't have the double-taxation disincentive that people have sometimes thought, so that integration would tend to deliver windfalls to owners of assets, corporate assets, more than eliminating distortions." (Alan Auerbach, interview, May 18, 2015.)

Reagan's January 1983 suggestion that this change had merit was reported to have been to have been an off-the-cuff remark, one made "almost as an afterthought" (*Casper Star-Tribune* (Wyoming), January 27, 1983, p. A1). Nine months later, Friedman complained about the lack of follow-up, citing Reagan's corporate tax statement when complaining that the president "has shown a knack lately for saying exactly the right thing and then letting it drop." Of the corporate-income tax reform idea, Friedman observed: "when Mr. Reagan said something of the

[105] See Friedman (1962a, pp. 132, 135, 174). See also NBC (1951, pp. 7, 8), Friedman (1958a, p. 26), Friedman's testimony of October 30, 1959, in Joint Economic Committee, U.S. Congress (1959, p. 3026), his remarks in *Business and Society Review* (Spring 1972), p. 8 (also in Friedman, 1975a p. 243), and the discussion in Nelson (2020b, pp. 60, 410). Richard Goode, who had been a colleague of Friedman's in the latter's early years as a department member at the University of Chicago, was a longtime proponent of this so-called integration proposal (see, for example, Goode, 1947).

[106] Testimony of January 27, 1975, in Committee on Ways and Means, U.S. House of Representatives (1975, p. 561).

[107] See also Auerbach (1983, p. 497). Auerbach's critical view regarding corporate/personal income-tax integration received heavy coverage in the chapter on corporate income tax in the Hoover Institution book on domestic economic policy, *To Promote Prosperity*, which was published in the 1984 election year and to which Friedman contributed a chapter (on monetary policy). See McLure (1984, pp. 308–310).

sort, his aides were horrified—and nothing has been heard of the idea since." (*Wall Street Journal*, October 25, 1983.)[108]

The fleeting spotlight that Reagan put on how to tax corporate income occurred in an environment in which discussion of tax changes in the U.S. policy agenda continued to be, even after the 1981 passage of the tax cut, mainly in the area of taxation of households rather than corporations. Friedman, too, continued to concentrate on what he in October 1983 called "our present monstrous income tax," and on individual (or household) income taxation in particular.[109]

Notably, in the area of personal income taxes, the famous 1981 tax act, even after its implementation, had not addressed, in the case of labor income, what Friedman and Schwartz in *Monetary Trends* called the "present high marginal rates of tax."[110] The 1981 legislation still left the top federal tax rate on labor income at 50 percent.[111] Against this background, Friedman was a strong advocate of further income-tax-rate reductions beyond those embedded in the 1981–1983 income tax cuts (*Newsweek*, August 23, 1982; *Wall Street Journal*, August 31, 1984).[112]

Notably, roughly six months after the 1981 bill had passed, Friedman renewed his call for further rate reductions, in the form of a swap of large-scale elimination of deductions for cuts in rates: A "tax change I would be strongly in favor of," he remarked on television, would be to cut the top rate from 50 percent to 25 percent, in exchange for the elimination of tax deductions.[113]

The idea of flattening the rate schedule so that rates above 25 percent were simply replaced by a 25 percent rate was one Friedman had advanced on numerous previous occasions—including in *Newsweek* columns published in 1968, 1976, and 1980.[114] The tax reform that eventually took

[108] During the spring of 1981, a Treasury undersecretary, Norman Ture, had stated that the integration of the corporate and personal income tax arrangements was being studied and suggested that the administration was sympathetic to such a move (see Davies, 1986, p. 148). Ture, however, left the administration in mid-1982.
[109] Friedman (1984f, p. 35).
[110] Friedman and Schwartz (1982a, p. 495).
[111] Friedman confirmed that he considered 50 percent to be a high marginal rate in *Newsweek*, April 12, 1976.
[112] See also Friedman and Friedman (1984, p. 91; 1985, p. 91).
[113] *Meet the Press*, NBC, March 21, 1982, p. 4 of transcript.
[114] *Newsweek*, April 22, 1968, April 12, 1976, and August 18, 1980. Friedman also endorsed the idea on his cassette-commentary series, including in his remark that "it would be highly desirable to eliminate the special tax preferences, deductions, and so on of the income tax, provided that were accompanied by a lowering of the marginal rates, so that you got roughly the same tax revenue, or lower tax revenue, with a much lower marginal rate." (Instructional Dynamics Economics Cassette Tape 103, July 12, 1972.) See also his 1976 talk in London published as Friedman (1977i, p. 50) (reprinted in Friedman, 1978b, p. 75; 1991a, p. 156).

place in the second term of the Reagan Administration—the 1986 measure discussed below—was really not too different from this, as it set the top federal income tax rate at about 28 percent while eliminating many tax deductions. The outcome was in line with what Friedman had stated nearly a quarter-century earlier, in *Capitalism and Freedom*: "The appropriate solution is the drastic scaling down of the higher rates, combined with an elimination of the avoidance devices that have been incorporated into the law."[115]

If Friedman had left his public contributions on deductions-for-rates swaps to those he had made through early 1982, he would likely have become regarded as a key inspiration for the 1986 tax reform. Furthermore, as discussed below, he did participate directly, albeit to a small extent, in the course that led to the 1986 reform. This was because the administration's participation in the reform originated with a discussion that George Shultz had with President Reagan in late 1982. Shultz would credit discussions with Friedman as part of the catalyst for the tax-reform suggestions that he made to the president.

In the event, however, Friedman would *not* be closely associated with the 1986 tax reform, despite his discussion with Shultz and his considerable body of prior public writings that foreshadowed the reform.

This lack of association was, in fact, understandable, for two reasons discussed in detail below. First, it was another economist, Joseph Pechman of the Brookings Institution, and not Friedman, who had done much of the detailed economic research on the feasibility of a major joint pruning of tax rates and tax deductions. Pechman would come to be closely associated with the ideas underlying the 1986 tax reform.[116] And, second, Friedman's public writings from 1982 to 1985 had the effect of making him largely a critic, rather than a clear supporter, of the process that led to the 1986 reform. He deprecated the major Congressional and administration reform proposals in this direction that were made from 1982 to 1985. Once the tax reform became a reality in 1986 when it was signed into law, he did praise it strongly. Virtually right up to that point, however, he had been, in large part, a critical bystander—and, in particular, he had been highly skeptical about the prospects for true tax reform.

As noted, notwithstanding Friedman's longstanding in the matter, the basic economic research

[115] Friedman (1962a, p. 133).

[116] For example, with regard to providing the analytical inspiration for the 1986 tax reform, Alan Auerbach stated: "I would have said Joe Pechman—Friedman not so much… I associate that [idea] very much with Joe Pechman." (Alan Auerbach, interview, May 18, 2015.)

underlying a deduction-elimination/rate-reduction move had been carried out by others, most notably Pechman. Pechman testified in September 1982 that he had first published an article on the matter in 1955.[117]

This remark was given in one of many Pechman appearances before Congressional committees over the years. In these appearances, he repeatedly offered tax-reform proposals stemming from his research. For example, when testifying in mid-1977 on tax reform, Pechman had stated: "It would be much better if most of the exclusions, deductions, and preferences were eliminated from the tax law, and the revenue used to reduce the tax rates both on individual and corporate income very substantially."[118] But the tax-rate schedule that Pechman had given on that occasion as the aim of such a reform—rates ranging from 10 percent to 50 percent—involved no decrease in the top rate. Furthermore, by the early 1980s, such a schedule seemed unambitious as, once the Reagan tax cut was passed, the rate range that Pechman had proposed in 1977 was essentially scheduled to come into force anyway: 11 percent to 50 percent. In 1982, Pechman instead suggested more drastic cuts: reducing the rates so that the range covered was 9 percent to 28 percent.[119] Reflecting 1982's climate, in which further tax measures that increased the budget deficit were out of favor, Pechman stressed that the change that he proposed would, because it eliminated many deductions, be revenue-neutral: "we could reduce the top bracket rate from 50 to 30 or 28 percent and still get the same revenue." [120] "I think the American people would find that very attractive—a tax rate schedule that doesn't exceed 28 percent," Pechman added. "That's the case under my plan, and [also] under Senator Bradley's plan."[121]

"Senator Bradley's plan" was actually a proposal jointly advanced by Representative Richard Gephardt and Senator Bill Bradley, under which the income tax rates (above a threshold) would consist of 14, 20, 25, and 28 percent (*Detroit Free Press,* May 28, 1982; Committee on Finance, U.S. Senate, 1983a, p. 45).

That Pechman was now proposing a top 28 percent rate, rather than the 50 percent maximum rate that he had favored in the 1970s, and that prominent Democratic members of Congress—Gephardt and Bradley—were also proposing a reform that would also produce a maximum rate of 28 percent, bore out Friedman's contention in 1980 that a reform that would bring the top rate

[117] See Pechman's testimony of September 30, 1982, in Committee on Finance, U.S. Senate (1983b, p. 107). He was referring to Pechman (1955).
[118] From Pechman's testimony of July 19, 1977, in Joint Economic Committee, U.S. Congress (1978, p. 257).
[119] See Pechman's testimony of September 30, 1982, in Committee on Finance, U.S. Senate (1983b, p. 2).
[120] In Committee on Finance, U.S. Senate (1983b, p. 107).
[121] In Committee on Finance, U.S. Senate (1983b, p. 108).

to near this level had become “both highly practical and politically feasible.” (*Newsweek*, August 18, 1980.) And that by the second half of 1982 some key members of the other side of politics were seeking to go further than Reagan had in 1981 in reducing tax rates contrasted with what Friedman assessed was politically feasible, at least for Republicans, even when speaking at the start of 1982: “what President Reagan should have done is what for political reasons he couldn’t do—which is bring down a top rate of 25 percent… But Reagan couldn’t come out for that because his critics would say he was saving the rich.” (*Daily News* (New York), January 19, 1982.)

But as this proposal gained momentum in the later months of 1982, Friedman became more critical of it and more wedded to his preferred option: a flat tax. He had wished for a flat tax all along, as he had indicated in *Capitalism and Freedom*.[122] In 1981, he had reaffirmed: “I think that a flat-rate tax, with no deductions except straight-out occupational expenses, would be both fairer and less destructive of incentives.” (*Sydney Morning Herald*, April 7, 1981.)[123] But Friedman had regarded the flattening-graduated-rates proposal as more politically feasible, in light of public support for progressive income tax. “Personally, I would prefer a flat rate,” he remarked in an interview published in 1973, but “to achieve consensus” he would settle for a reductions/rate swap that would flatten the rate schedule and, in particular, would eliminate the higher rates.[124]

Until the early 1980s, however, Friedman had regarded both proposals as politically unattainable, owing to the attraction to politicians—for campaigning purposes—of creating or maintaining specific tax deductions. With regard to the flat tax, he had argued in 1972: “Of course, you’re not going to have it because the major purpose of the present tax structure is not to raise revenue, not to be equitable: it’s to provide political plums.”[125] Along similar lines, in 1978 Friedman remarked, with regard to the alternative of setting a low maximum to the top rate, “this reform is impossible… You put a 25 percent top rate in the tax system, and what do Congressmen have to sell in order to raise campaign funds?”[126]

Once the political climate became more favorable to rate-flattening proposals, however, Friedman—instead of restating the attractions of a flatter structure—was largely critical of such a

[122] Friedman (1962a, pp. 174-176).
[123] Friedman had also called for a flat tax at the outset of the Carter Administration, while noting with regard to current arrangements: “The personal income tax… is a maze of special measures.” (Friedman, 1977h, p. 91.)
[124] *Playboy*, February 1973 (p. 66), reprinted in Friedman (1975a, p. 31; 1983a, p. 49).
[125] *Business and Society Review*, Spring 1972 (p. 8) (also in Friedman, 1975a, p. 244).
[126] *Milton Friedman Speaks*, Episode 7, “Is Tax Reform Possible?,” taped February 6, 1978, pp. 12, 13 of transcript.

move on the grounds that it did not go far enough. Friedman was becoming more ambitious when it came to what tax reform that he wanted instituted in the United States. He evidently saw his longtime preferred option, the flat-rate federal income tax, as now being attainable. Against the background of this assessment of the current political climate, Friedman saw the Bradley-Gephardt flattening proposal—despite its similarity to schemes he had himself put forward—as a rival measure that needed to be beaten. So whereas, in *Playboy* in 1973, he had cited—as a desirable and, possibly, politically attainable step—a requirement that the top income tax rate should not be more than double the minimum (positive) rate, he now condemned the fact that the Bradley-Gephardt proposal had rates ranging from 14 to 28 percent as being a "far cry from a flat tax" (*Newsweek*, August 7, 1982).[127]

Friedman's *Newsweek* column also voiced a concern that would recur in his discussions of tax-reform plans over the next four years: that the reform would not stick. He believed that, post-reform, new barnacles would be added to the tax system—in the form of fresh, or reintroduced, tax deductions—and that the new rate structure would be undermined by pressure to reinstate higher maximum tax rates. This criticism—which would certainly receive considerable vindication via developments in the post-Reagan era—echoed Friedman's oft-stated 1970s argument that tax reform of the kind advocated by Pechman and himself would, if implemented, be reversed over subsequent years by the working of the political process.[128] Although his late August 1982 column primarily applied this point to the Bradley-Gephardt plan, the column seemed to acknowledge, as Friedman had before, that rate-flattening and flat-tax proposals alike were susceptible to being undercut by subsequent tax changes of the kind indicated. In this column, as a means of locking in a flat tax, Friedman pointed to what had become his usual, if unrealistic, recommendation when advancing fiscal policy arrangements: constitutional change—in this case, instituting a flat tax by constitutional amendment.

Before the Bradley-Gephardt proposal made him strongly distinguish between the two sets of types of tax reform, Friedman had produced a column (April 5, 1982) covering both the flat tax and rate-flattening measures and presenting each as attractive alternatives to the *status quo*. The title of the *Newsweek* piece, "Painless Revenue," reflected his view that a reductions-for-rates swap would raise tax collections rather than be revenue-neutral. This was his position irrespective of whether the change took the form of the introduction of a flat tax or if it simply

[127] For his earlier discussion, see *Playboy*, February 1973 (p. 66), reprinted in Friedman (1975a, p. 31; 1983a, p. 49).
[128] See *Playboy*, February 1973 (p. 66) (reprinted in Friedman [1975a, p. 31; 1983a, p. 49]), *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978 (pp. 12–13 of transcript), and *San Francisco Chronicle*, February 6, 1979.

involved abolishing rates above 25 percent. "No doubt about it: Beginning immediately, federal revenues would rise," he had already remarked of the latter proposal (*Daily News* (New York), January 19, 1982). In what was, in effect, an acknowledgment of 1982's clamor for more tax revenue and lower deficits, Friedman had also argued that this move "would both raise more revenue and cost the taxpayer less" because it would make tax shelters uneconomical.[129] Either type of tax reform (specified in the April column as a flat rate of 17 percent or, alternatively, a schedule featuring a 25 percent maximum rate), he believed, would render it not worthwhile to devote resources to create legal tax shelters: "At a top rate of 25 percent, most of the current activity undertaken to avoid taxes would become unprofitable." (*Newsweek*, April 5, 1982.)

Friedman's April *Newsweek* column on the revenue-boosting aspects of a flat-rate or 25-percent-maximum-rate system was written just a week after his speech in Atlanta saying that a tax cut should be big enough to reduce the federal government's revenue. This contrast recurred in Friedman's writings in the early 1980s: although he himself wanted to reduce tax revenue, he was also aware of the pressure for more immediate closure of budget deficits, and his practical policy recommendations often made allowances in response to that pressure. So, for example, he made a concession to political realism in early 1983 by indicating that measures to reduce the budget deficit would involve actions "on the revenue side," even though he insisted that spending restraint should remain "the primary method" of deficit reduction (*U.S. News and World Report*, January 31, 1983, p. 66).[130]

In this vein, Friedman was not averse to emphasizing the revenue-boosting aspects of some of his preferred reforms. Arthur Laffer's memory of Friedman's attitude to the tax/revenues relationship (interview, August 11, 2014) was: "His argument was almost always starve the beast, that 'revenues would go down, but who cares? They don't need more money.'" This characterization captures the spirit of many of Friedman's discussions accurately, and it does line up with the attitude he consistently took toward the Kemp-Roth tax cut.[131] But it neglects the many occasions on which Friedman discussed how a streamlined tax system—swapping lower tax rates for fewer deductions—would boost revenue. This was true of his August 1980 *Newsweek* analysis and of his renewed activity on the matter two years later and was reflected in a letter to Jepsen of August 2, 1982, in which he stated: "Our present income tax… is counterproductive, yielding less revenue than would a flat-rate tax at a moderate rate."[132]

[129] *Meet the Press*, NBC, March 21, 1982, p. 4 of transcript.
[130] Reprinted in McClelland (1983, p. 58).
[131] For further discussion, see Chapter 9 above.
[132] Friedman (1982g, p. 187).

That letter was in response to an invitation to appear at a Joint Economic Committee hearing on flat-tax proposals. Friedman declined the invitation but was encouraged by the surge of political interest in the flat-tax proposal, which as recently as late March 1982 he had judged "too radical to be a practical proposition at the moment" (*Newsweek*, April 5, 1982). Writing in early 1983, he implied that the Gephardt-Bradley proposal was not an acceptable alternative to what he recommended President Reagan should adopt: "the thing to do is to replace the present personal-income tax with an honest-to-God, real flat rate tax. If you eliminated the present income tax completely and instead taxed all income above present personal exemptions at a flat rate of 17 percent, you'd raise more revenue." (*U.S. News and World Report,* January 31, 1983, p. 67.)[133]

Friedman was buoyed further by what he perceived as continuing momentum in favor of a flat tax, and his *Newsweek* column of February 28, 1983, referred readers to a short book that Robert Hall had coauthored with a mutual Hoover Institution colleague, Alvin Rabushka, propounding a flat tax.[134]

But an appearance that Hall made at another Congressional hearing on the flat tax—this one a session of a U.S. Senate Committee held on September 30, 1982—underscored the fact that, to many, a flat-rate tax was not the obviously attractive measure that Friedman claimed it to be. Hall appeared alongside Pechman. In his submission for the hearing, Pechman concurred with the number that Friedman had given on the rate of flat tax that would generate as much revenue as the existing tax system: 17 percent.[135] But Pechman strongly opposed the flat tax on grounds of equity and, as already indicated, advocated instead a graduated rate schedule. In the hearing, Pechman and questioner Senator Bill Bradley put Hall on the defensive when they indicated that a flat-rate tax would, in contrast to their own proposals, raise the tax liability of a household whose annual income was $30,000.[136]

Although this exchange involved Hall, not Friedman, as the proponent of a flat tax, it exemplified Friedman's own position *vis a vis* the proposals coming out of Washington in favor of a much flatter, but still progressive, income tax-rate system: because they were not a flat tax, he was mainly positioned toward those proposals in an adversarial way.

Milton and Rose Friedman largely repeated Friedman's 1982 critique of the Bradley-Gephardt

[133] Reprinted in McClelland (1983, p. 59).
[134] See Hall and Rabushka (1983).
[135] See Pechman's testimony of September 30, 1982, in Committee on Finance, U.S. Senate (1983b, p. 3).
[136] See the exchange recorded in Committee on Finance, U.S. Senate (1983b, pp. 104–105).

proposal when discussing it in the book version of *Tyranny of the Status Quo*.[137] That presentation continued to push for a flat rate tax accompanied by the elimination of all but strictly defined work-related expenses as deductions—a proposal Friedman also advanced in a *Donahue* appearance (NBC, April 25, 1984). As Friedman had in 1982, the Friedmans declared the flat tax to be "a splendid idea," while leaving the impression that they thought that the Bradley-Gephardt schedule-flattening proposal was not.

In the meantime, Friedman had already made what became his most material contribution to what became the 1986 tax reform. George Shultz (1995, p. 8) would recall that when he and Joseph Pechman were CEA staff members during the Eisenhower years, Pechman explained to him his work on the idea of reducing marginal rates in exchange for the large-scale elimination of tax deductions. In December 1982, Shultz, who had become Reagan's Secretary of State in June, took up a suggestion from Reagan to give the president advice on domestic economic policy when he saw fit. Shultz seized on tax reform "after talking it over with Milton Friedman-a source of wise and practical counsel over many years—I suggested the [reductions-for-rates] idea on a more radical basis to Ronald Reagan" when he and the president were in California.[138]

Shultz elaborated: "I was aware of the work that Joe Pechman had done. You know, Joe was forever saying, 'If you get rid of this or that preference [i.e., tax concession], you could reduce the [tax] rates by this or that much. You want to do that.' And Milton was very much aware of that and had talked about it… At Christmastime [1982], I called Milton and talked to him about it, just to try it out, and he said: 'Yes, yes.'" (George Shultz, interview, May 22, 2013.)

This input notwithstanding, Friedman's continued his new public tone of being fairly downbeat about proposed rate/reduction swaps other than those that also entailed a straight flat tax. This was the posture he took in 1985, when reacting to the Reagan Administration's own variants of the idea of exchanging elimination of tax reductions for lower (but graduated) rates. In off-the-cuff comments in midyear, Friedman remarked, "I'm not optimistic about any meaningful tax reform. 'Treasury I' [the reform outline circulated by the U.S. Treasury after the 1984 election] was pretty good, and Reagan's [subsequent] proposal was still pretty good. But the Senate and House are [now] making it worse and worse." (*San Francisco Chronicle*, June 29, 1985.)

An op-ed by Friedman on Reagan's tax reform agenda appeared in the *San Francisco Chronicle*

[137] Friedman and Friedman (1984, p. 64–65; 1985, pp. 66–67).

[138] Shultz (1995, p. 8). See also Shultz's remarks in *The Right All Along: The Rise, Fall, and Future of American Conservatism*, Fox News Channel, Episode 4, November 28, 2010.

on October 16, 1985 and, later, in several other newspapers.[139] In this op-ed, Friedman stated that the proposal advanced by the president "contains many desirable changes" but was not ambitious enough, still leaving the tax system "complex, inequitable and very far from neutral."[140] Furthermore, in line with his June remarks, Friedman predicted that Congress would gut the proposal by putting further compromises into the reform package. He reaffirmed his view that "there is no chance of enacting true tax reform through Congressional action" and went back to his familiar proposal of a constitutionally-imposed flat tax (*San Francisco Chronicle*, October 16, 1985, p. 12).

In contrast to Friedman's expectation, a major rate-reduction/deduction-elimination did see its way through Congress—with the Tax Reform Act of 1986 being what Poterba (1994, p. 255) deemed "the single most important change in the U.S. tax code in decades." The law, taking effect over 1987 and 1988, officially instituted two positive federal income tax rates: 15 percent and 28 percent (Boskin, 1987, pp. 141, 158)—although, due to the way in which the new structure was implemented, a different top rate, of 33 percent, essentially applied over a sizable income range (Hausman and Poterba, 1987, p. 102; Fullerton, 1994, p. 168).[141] Even with this complication, the reform was, as Hausman and Poterba (1987, p. 101) observed, "hailed as the most far-reaching change in the personal income tax since the Revenue Act of 1942" (an act produced during Friedman's period as a junior Treasury official).

As the 1986 reform bill proceeded toward passage in the Senate, longtime *Wall Street Journal* writer and former University of Chicago student Lindley H. Clark, Jr. highlighted the connection with Friedman's longstanding proposals and suggested that the bill's good chances meant that "Maybe Milton Friedman's Time Has Come" (*Wall Street Journal*, June 17, 1986).[142] In fact, however, Friedman had set such a negative tone over the previous three years regarding tax-

[139] See, for example, *Gazette Telegraph* (Colorado Springs, Colorado), October 20, 1985, *Houston Post*, October 21, 1985, and *Dallas Morning News*, October 27, 1985.

[140] Friedman noted, in particular, the plan's maintenance of the corporate income tax and the likelihood that the plan, if implemented, would increase the amount of tax charged to corporations. Martin Feldstein likewise felt that the plan, as it proceeded to Congressional deliberation in 1986, was going in the direction of increasing taxes on businesses—something that he felt was unjustified in light of his contention that TEFRA in 1982 and further measures in 1984 had canceled a good deal of the reduction in corporate taxes legislated in 1981. (See *Wall Street Journal*, February 14, 1986, and Feldstein and Jun, 1987, pp. 102, 115, 136.) In any event, Feldstein felt, as his fellow former CEA head Weidenbaum did, that the fiscal policy agenda should concentrate on deficit reduction at this time: "It would be best if the Senate could drop the whole idea of major tax reform in 1986. The nation would be better served if Congress concentrated on deficit reduction…" (*Wall Street Journal*, February 14, 1986.)

[141] Friedman treated 33 percent as the top rate in the new system in *Financial Times* (London), December 4, 1987.

[142] The title and timing of the article were incongruous in view of the fact that Clark's column appeared in a period in which Friedman was being blasted in many business press commentaries on the basis of these commentaries judgment that monetarism had failed empirically. See the previous chapter.

reform plans other than his own favored flat tax that it was difficult to associate him closely with the Tax Reform Act. As passage came closer, he acknowledged that a genuine rates-for-deductions reform was in the offing, while still affirming his preference for a constitutionally-imposed income-tax setup (*Wall Street Journal*, July 7, 1986).

Reagan signed the Tax Reform Act on October 22, 1986 (see Reagan, 1986). Friedman would describe the large marginal income tax rate cut that had been obtained by the act as a "major achievement" (*The Business*, Fourth Quarter 1987, p. 12). Indeed, Barro and Redlick's (2009) estimates indicate that the overall marginal rate in 1988 was at its lowest value since 1976. Alan Walters (1993, p. 30) would recall: "In 1986, Milton Friedman told me that although he welcomed the great tax reform he was quite certain it would not last. He argued that since members of Congress depended on raising funds from interested parties, they would simply have to pass laws which opened up those loopholes which the tax reform had abolished. We would soon be back with the old system of high rates and a myriad of loopholes." Indeed, with respect to loopholes, Friedman regarded lobbyists and members of Congress as potentially seeing attraction in a *bona fide* 1986 tax reform, because it "wipes the slate clean," providing room for a raft of new loopholes to be added after 1986 (*Wall Street Journal*, July 7, 1986).[143]

In July 1988, after the reform had been instituted, Friedman stated: "The chief monument to the Reagan administration will be tax reform. I doubt that anyone would have predicted in 1980 that by 1988 the top personal income tax rate would be in the neighborhood of 28 percent. The most I had hoped for was 50 percent."[144] Two years later, he indicated that he was pleased with the operation of the new system: "In the three years since the 1986 Tax Reform Act, tax receipts went up by more than $200 billion. They will continue to go up, without any change in the law, by more than enough to permit any additional necessary expenditures and to reduce the deficit." (*San Francisco Chronicle*, July 2, 1990, p. C8.)[145]

Soon after Friedman made these comments, however, his concern that the consensus underlying the 1986 act was very fragile was borne out by the fact that Congress and the Bush Administration agreed on a tax increase. The retreat from the 1986 act's new tax-rate schedule had was occurring only a couple of years after that schedule had come fully into force.

[143] A similar assessment was made by James Buchanan (1987).

[144] Friedman (1988c, p. 380). Friedman also referred to the lowering of the top tax rate since 1981 as Reagan's "major domestic achievement" (*Financial Times* (London), December 4, 1987).

[145] Of course, as discussed earlier in this chapter, a rise in overall tax revenues in the wake of tax reform or a tax cut was something about which Friedman had mixed feelings, and in Friedman (1992c, p. 211) he remarked negatively on the fact that the ratio of federal tax revenues to national income was higher in 1989 than in 1978.

In the 30 years after 1990, the top marginal federal tax rate fluctuated but was consistently above 30 percent and often very close to 40 percent. Roughly half of the decrease in the top rate legislated in 1986 was therefore wound back, although there has never been a return to the top rate of 50 percent.

In addition to the addition of new and higher rates, many new deductions were put into the federal income tax system after 1986.[146] The year in which Friedman died, 2006, was also the twentieth anniversary year for the Tax Reform Act of 1986. The verdict rendered in that year by longtime tax researcher Vito Tanzi was that “the reduction in the number of rates and brackets that came with the 1986 Reagan tax reform did not prevent the income tax from becoming much more complex in later years.”[147]

Friedman regarded this development as predictable: “How are they [candidates for Congress] going to raise their campaign funds if they don’t have loopholes they can put in and take out from year to year?” (CSPAN, May 7, 1993.) As these comments indicated, he saw the Congressional election process as a source not only of tax loopholes but also of the frequency of changes in the precise structure of the tax system.[148]

Capital gains tax

One area in which Friedman and Reagan alike underestimated the momentum for tax reduction was in the area of capital gains taxes. The 1986 reform moved to treat longer-term capital gains income symmetrically with labor market income, rather than taxing it at a discounted rate. This move, advanced at the time as a desirable reform, was subsequently sharply reversed.

One of the few tax measures that occurred under Jimmy Carter that President Reagan had spoken

[146] William Poole (“Policy Seminar With William Poole: ‘A Market-Based Taylor Rule for Monetary Policy,’” Hoover Working Group on Economic Policy, Hoover Institution, October 5, 2022) conjectured that Vice President Bush was not persuaded of the merits of the 1986 tax reform. This conjecture is consistent with the fact that a significant winding back of the measures embedded in the Tax Reform Act of 1986 occurred during President George H.W. Bush’s period in office. Bush also proposed a further winding back by advancing a plan (not subsequently implemented by Congress) to reintroduce, albeit apparently on a temporary basis, the investment tax credit (*The Times-Picayune* (New Orleans), January 30, 1992)—an idea that Friedman, a longstanding critic of the investment tax credit, labeled “bad” (*New York Times*, February 2, 1992).

[147] Tanzi (2006, p. 20).

[148] On the latter point, Friedman contended (*Wall Street Journal* (March 30, 1995), that “only when there are new tax laws do the members of Congress have something to sell. Only then is there something that lobbyists can buy to earn their incomes.” He added in remarks made the following year (CSPAN, December 1, 1996): “Our tax system is terrible. But, even so, it would be an enormous benefit if it could be kept the same every five or ten years. Why do you [instead] have a change every second year?... Because that's when the Congressmen have to raise campaign funds. And they sell changes in the income taxes or [commitments] not [to make] changes.”

highly of was the lowering, enacted in November 1978, of the maximum tax rate on capital-gains income.[149] Before the 1978 move, the top rate on longer-term capital gains was somewhat above 45 percent (Feldstein, 1994, p. 23). In 1977, it stood at 49 percent—below the 70 percent marginal rate applying to other investment income, but virtually the same as the top rate on labor income (M.K. Evans, 1983, p. 175). The 1978 tax law that Carter signed cut the top rate on capital gains to 28 percent, and the subsequent increase in capital gains tax receipts was cited by Reagan and others as one of the most clear-cut examples of a tax-rate reduction boosting tax revenues.[150]

The 1981 tax cut reduced the top capital gains tax rate further, to 20 percent (Feldstein, 1994, p. 30). The Tax Reform Act of 1986, however, raised the top rate to 28 percent—equal to the main new top rate on labor income. At the time, some economists praised this harmonization of the maximum rates on labor and capital gains income: Walsh (1986b, p. 2), for example, suggested that "the current law's preferential treatment of such gains has generated much of the industry devoted to tax avoidance… [and] taxing all realized capital gains as ordinary income… is likely to simplify the tax system."[151] And in Australia, Treasurer Paul Keating had recently introduced a capital gains tax whose rates were the same as those pertaining to labor income.[152] Keating suggested in mid-1986 that the impending U.S. tax reform amounted to a recognition that setting the capital gains tax rate at a different value from the labor income tax rate hurt national economic efficiency.[153]

There was, however, a countercurrent of opinion among U.S. economists in academia and public policy that opposed the 1986 harmonization. Martin Feldstein, for example, felt that the lower tax rate on capital gains income should have been maintained (Feldstein, 1991, p. 30). Similarly,

[149] See, for example, Reagan's remarks in interviews given in December 1981 (Reagan, 1981b) and January 1982 (*Boston Herald American*, January 18, 1982).
[150] See the Reagan statements cited in the previous footnote, and for similar assessments by others, see M.K. Evans (1983, pp. 175, 181–182), Boskin (1987, p. 162), and Feldstein (1994, pp. 23–25).
[151] Similarly, William Poole, who was involved in the administration's development of its tax-reform proposals through his departure from the CEA in 1985, saw the symmetrical treatment of capital gains income and labor income as a virtue of the 1986 reform. He later observed: "I always like to think back to the Tax Reform Act of 1986—which I thought was [implementing] a great idea: tax all income at the same rate, [with] no special deals for capital gains or anything." ("Policy Seminar With William Poole: 'A Market-Based Taylor Rule for Monetary Policy,'" Hoover Working Group on Economic Policy, Hoover Institution, October 5, 2022.)
[152] See, for example, Tanzi (1987, p. 16).
[153] Keating (1986, p. 3903) remarked: "It is very interesting, is it not, to see the United States Government's tax reform proposals. The United States Congress is now seeking to increase the United States capital gains tax rate to the same level as that of the income tax. Why would [the U.S.] government seek to increase the level of its capital gains tax rate to the same level as that of the income tax rate? It is because that government knows that it is open to avoidance and that it distorts the level of allocative efficiency in the economy."

as the tax-reform bill crystalized, Feldstein's predecessor as CEA head, Martin Weidenbaum, named "raising the tax on capital gains" as one of "the most serious elements in terms of weakening capital formation and weakening economic growth."[154]

This perspective was shared by many in the non-academic supply-side movement. Lawrence Kudlow, for example, stated in early 1992: "I felt that the 1986 tax bill had a lot of bad elements in it... I think we should provide relief on the capital gains tax... [and] my preference is to bring the rate back to 20 percent—where it was before the 1986 bill—or even 15 percent."[155]

In contrast, Friedman was largely indifferent to the calls for a lower capital gains tax rate. In 1977, he had suggested that provided that the base for computing capital gains was properly indexed, it would likely be worthwhile treating capital gains symmetrically with other types of income for tax purposes.[156] And when the harmonization of capital gains and other income was poised to take place through the 1986 reform bill, Friedman was unperturbed—and even indicated that he would himself be satisfied with putting into the U.S. Constitution a requirement that "the same tax rate is applied to all income," as long as the amendment in question had other features that he considered desirable (*Wall Street Journal*, July 7, 1986).

In subsequent years, Friedman was aware of the agitation to reduce capital gains tax rates but seemed bemused by it. He indicated that while he himself would be receptive to a lowering, or even abolition, of taxes on capital gains (*The Salt Lake Tribune*, January 14, 1992; *New York Times*, February 2, 1992), he believed that the objective of bringing down the capital-gains tax rate should not be assigned the high priority that the business community was giving it and that such a proposal was not, in any event, salable politically (*Dallas Morning News*, September 27, 1989). In contrast to this assessment, however, the top federal capital gains tax rate would be cut to 20 percent in 1997 by President Clinton and a Republican-party-controlled Congress (with a further reduction to 15 percent, in 2003, occurring under President George W. Bush).

Balanced budget amendment proposals

As the preceding discussion implied, Friedman's lack of enthusiasm for tax-reform efforts in the

[154] Wall Street Week, Maryland Public Television, May 23, 1986, p. 9 of transcript. The other item that Weidenbaum suggested made the reform one that "really hits investment" was its "elimination of the investment tax credit" (pp. 8, 9). The investment tax credit would, indeed, be abolished by the 1986 act (see Boskin, 1987, pp. 103, 158).

[155] Testimony of January 9, 1992, in Joint Economic Committee, U.S. Congress (1992 pp. 30, 31).

[156] *Milton Friedman Speaks*, Episode 6, "Money and Inflation," taped November 7, 1977, p. 36 of transcript.

1980s in part reflected his preoccupation with constitutional amendment as the means to implement lasting changes to fiscal policy. During 1982−1986, Friedman not only invoked this route when suggesting how to proceed with reform of the federal tax structure, but he also continued to press it as the best means by which to impose rules on the spending-and-borrowing aspects of the federal budget.

Specifically, continuing the push he started in the late 1970s, Friedman called during 1982−1986 for a balanced-budget constitutional amendment. By the second half of 1982, the federal budget had moved into deficits whose scale was much greater than those that had prompted the budget-balancing campaigns of the 1970s. In bringing budgetary balance into the forefront of policy discussion, the new background of very high deficits bolstered support in Congress for the proposed addition to the U.S. Constitution. The U.S. Senate approved a proposed balanced-budget amendment whose wording Friedman favored. It voted, 69 to 31, in favor of this amendment on August 4, 1982, acquiring the needed Senate super-majority.[157] The U.S. House of Representatives leadership—which, unlike that of the Senate in 1982, consisted of Democrats—was not well disposed toward considering the amendment, but it did eventually vote on it (on October 1, 1982). The amendment was defeated in Congress, as it received a simple majority, but not a super-majority, in the House.[158]

Friedman professed to being relieved that the amendment had been defeated in the House because he believed the House's rewording had watered down the amendment.[159] It was the Senate draft of the amendment that the Friedmans published, and offered as a model, in their *Tyranny of the Status Quo* book in 1984.[160]

Friedman's complaint about the House amendment was: "It did not seek any limit on government spending, it simply declared that the government shall balance its books."[161] In contrast, the draft amendment that the Friedmans published restricted tax revenue to grow at or less than national income and planned spending to be set in light of that revenue (with provisions for Congress to vote higher spending and tax levels explicitly).[162]

[157] See Friedman and Friedman (1984, p. 55; 1985, p. 58).
[158] See Friedman's discussion in *The Atlantic*, February 1983, p. 18. See also Friedman and Friedman (1984, pp. 55–56; 1985, pp. 58–59).
[159] Friedman (1986b, p. 242).
[160] Friedman and Friedman (1984, pp. 56–57). See also Friedman and Friedman (1985, p. 59) and *The Atlantic*, February 1983 (p. 20).
[161] Friedman (1986b, p. 242).
[162] Friedman and Friedman (1984, p. 56). See also *The Atlantic*, February 1983, p. 20.

Therefore, just as was the case when he launched the balanced-budget campaign in 1978–1979, balancing the budget was a secondary, or even lower, consideration in Friedman's conception during the 1980s. After the large Reagan deficits had been well established, he reaffirmed: "The federal deficit is not a serious problem. The level of federal spending is a very serious problem."[163] He capsulized his position much later as follows: "A balanced-budget amendment… is a means to an end. The end is holding down the growth in (or better, sharply reducing) government spending." (*Wall Street Journal*, January 4, 1995.)

In the same vein, when in March 1984 Friedman was reminded that he was an initiator of the balanced-budget amendment, and was asked why he was not "outraged" by the Reagan budget deficits, he replied: "But I am outraged… not by the deficits but by the high level of spending."[164]

Friedman's advocacy of this amendment put him at odds with much of the economics profession. One aspect of the difference between himself and many other economists was with regard to his actual rationale for the amendment. Friedman was consistent that what he sought was lower spending. That being the case, however, focusing on a "balanced budget amendment" could be criticized as not being forthright. Why seek spending control only indirectly by describing the proposed change as "a constitutional amendment to balance the budget and limit the growth of taxes," as Friedman did in October 1983?[165]

In defending this indirect approach, Friedman acknowledged the marketing or political appeal of proposals that focused on restricting taxes and deficits. But he also rationalized it by appealing to his now-familiar argument (which became a key theme in *Tyranny of the Status Quo* book) that putting restrictions on spending and taxes jointly made voters and legislators face up to the aggregate budgetary consequences of their spending decisions. The default situation in fiscal policy was one in which legislators considered spending proposals piecemeal and had limited opportunities to decide on the aggregate budget items jointly. Nor at the federal government level did tax receipts set a firm ceiling on government spending. "Deficits are bad because they enable our representatives to vote for spending without having to vote for taxes to pay for the spending," Friedman contended when facing up to the issue of "Why Deficits Are Bad" in one of his final *Newsweek* columns (January 2, 1984). In the face of this situation, Friedman suggested,

[163] *CBS Evening News*, March 1, 1984, p. 22 of transcript.
[164] *The MacNeil/Lehrer News Hour,* PBS, March 27, 1984, p. 6 of transcript.
[165] Friedman (1984f, p. 35).

"a constitutional amendment is a way in which we can arrange a package deal."[166]

Some economists who were Friedman's opponents on other issues saw some merit in this argument. Notably, Otto Eckstein remarked (*Washington Post*, March 6, 1979): "If the political process must levy the taxes to pay for the expenditures, there is likely to be a more careful scrutiny than if the expenditures can be clothed in the virtue of deficit-creating stimulus packages." But they parted company with Friedman's position that, in order to address the problem, there was "only one way that offers any hope… the constitutional route" (*Newsweek*, January 2, 1984). Eckstein, for one, eschewed a balanced-budget amendment, in part because it made this scrutiny too indirect and so were "partially dishonest. Every device, Kemp-Roth or a balanced-budget amendment—they are all an attempt to do anything other than face up to the [spending] programs one at a time." (*Washington Post*, January 14, 1979, p. G5.)

The main objection by economists to the amendment remained, however, that by making an annually balanced budget into the Constitution, it put embedded retrograde economic analysis into that document. This objection was voiced by Arthur Okun, when he stated in 1979 that the proposed amendment was "a throwback to the days of [President] McKinley, if not to the cave man." (*Washington Post*, January 14, 1979, p. G5.) It was also articulated during the 1980s in one-to-one public debates that Kenneth Arrow and Alan Blinder each had with Friedman.

In the Arrow/Friedman debate, held at Stanford University, Arrow contrasted Friedman's advocacy of the balanced-budget amendment with the enthusiasm Friedman had shown for countercyclical budget deficits in his 1948 monetary/fiscal policy proposal. "I finally said, you know, 'Well, this contradicts the paper you wrote in 1949,' or whenever it was… If you let tax rates remain constant, tax rates fall, and increase transfer expenditures… maybe you could have a cyclically balanced budget, but you can't have an annually balanced budget. And I don't know—I didn't think he gave much of a reply." (Kenneth Arrow, interview, December 7, 2013.)

Friedman may not have felt inclined to defend the proposal he had advanced in print in the late 1940s because his views—particularly on the importance (as an influence on aggregate demand) of fiscal policy—had changed so much since then. Additionally, his proposed amendment did allow for automatic stabilizers to operate to a limited degree by making planned budget balance the prescription. "A deficit that emerged because a recession produced a reduction in tax receipts would not be in violation of the amendment," Friedman noted in an article on the

[166] Friedman (1984f, p. 35).

amendment. "… I have never been willing to support an amendment calling for an annually balanced budget." In the same passage of this article, however, Friedman indicated that *outlays* were restricted by the amendment, even in times of recession—so, in the event of transfer payments going up during a recession, the resulting upward pressure on aggregate outlays might have to be offset by spending cuts elsewhere (*The Atlantic*, February 1983, p. 23).

The debate with Alan Blinder was held at the University of California, Davis, on October 5, 1984. Blinder observed: "I remember the preparation, and I remember the following little incident that, after it was all over, and the press left, and there were only a few people lingering in the auditorium. I went over to Milton—or he went over to me; I don't remember. Anyway, we were sort of chatting, and our wives were on the other side of the room, looking at the two of us talk, and Rose said to Madeline, my wife: 'Who do you think is teaching whom?' (*laughter*)." (Alan Blinder, interview, December 6, 2013.)

One of the items that Blinder read in preparation for the debate (and which he would later print in excerpted form in Baumol and Blinder, 1985, p. 301) was Friedman's February 1983 article in the magazine *The Atlantic* advocating the amendment. The *Atlantic* article was written several months after the Congressional defeat of the amendment. But, in the article, Friedman remained optimistic: "In view of its near passage and the widespread public support for it, the amendment is sure to be reintroduced in the current session of Congress. Hence it remains a very live issue." (*The Atlantic*, February 1983, p. 18.)

In October 1983, Friedman noted that thirty-two states had supported a federal balanced-budget amendment and predicted confidently that Congress, in order to avoid being preempted by the states, would before long itself take the initiative in advancing the amendment.[167] But a bit over two years later, he did not have any more progress to report, merely repeating the figure of 32 states, two short of what was required for Congress to act formally on the matter (*San Francisco Chronicle*, October 16, 1985, p. 12).

Friedman had not realized that 1982 had been the high-water mark for the proposed amendment. His own continuing activism on the matter notwithstanding, the balanced-budget constitutional drive had lost much of its momentum after its defeat in Congress in 1982. Friedman admitted later: "As a founding member in 1975 of the National Tax Limitation Committee, I naïvely thought it was a simple matter to draft an effective balanced-budget amendment. Long

[167] Friedman (1984f, p. 35).

experience has taught me better." (*Wall Street Journal*, January 4, 1995.)

To some extent, however, a process of automatic restriction of government spending as a means of controlling the budget deficit—a process that Friedman wanted to implement through the balanced-budget amendment—did make it into the U.S. fiscal system during this period in another way. The Gramm-Rudman legislation (later the Gramm-Rudman-Hollings legislation), originally passed in 1985, provided for across-the-board cuts in nondefense federal spending to be imposed in the event that Congress could not achieve a certain target amount of deficit reduction through other agreements. Friedman, who had been a longtime advocate of across-the-board cuts (see Chapter 11), remarked on television: "Gramm-Rudman, it seems to me, is a very flawed measure which is likely to have very good results." (*The MacNeil/Lehrer News Hour*, PBS, February 6, 1986, p. 11 of transcript.)[168]

Reflecting the priority that he and numerous other leading Keynesians were giving to deficit reduction during this period, Franco Modigliani—who was appearing with Friedman on the television program—partly concurred: "The Gramm-Rudman amendment—I think I agree with Milton on the basic idea that it is a pretty bad law whose only virtue is that every other proposal considered seriously was even worse."[169] Modigliani's objection to the law echoed what some had said about Friedman's constitutional proposals: "instead of relying on good judgment to do the things that should be done, one relies on tying one's hands in silly ways to be sure that the final outcome comes about." (*The MacNeil/Lehrer News Hour*, PBS, February 6, 1986, p. 12 of transcript.)

Reagan's second term and economic policy

The tax reform and Gramm-Rudman legislation contained certain policy prescriptions that Friedman had advocated. In each case, however, his status with regard to them was that of an outside observer and public commentator—with skeptical remarks on the road to passage being followed by favorable comment on them once they had become law. This pattern exemplified the more distant relationship with the economic-policy process in Reagan's second term.

[168] Friedman criticized the law's essential exclusion of defense spending, and he cited particularly the possibility that defense spending could be cut by reducing waste and by eliminating "unnecessary military bases and posts." (*The MacNeil/Lehrer News Hour*, PBS, February 6, 1986, p. 12 of transcript.) The next chapter considers in detail Friedman's views on defense spending and on U.S. military commitments.

[169] *The MacNeil/Lehrer News Hour*, PBS, February 6, 1986, p. 12 of transcript. In a similar vein, Walter Heller had remarked of the Gramm-Rudman measure: "All the way through, you can criticize it mechanically, technically, and so on. And, yet, in a curious way, I think it might work." (*Wall Street Week*, Maryland Public Television, December 13, 1985, p. 7 of transcript.)

The October 1984 Associated Press report on Friedman's heart attack stated that he had "advised President Reagan and other world leaders on economic policy."[170] Likewise, the back cover blurb for *Bright Promises, Dismal Performance* (the final collection, published in 1983 and discussed below, of Friedman's *Newsweek* columns) had described Friedman as "a trusted advisor to our nation's leaders." But in Reagan's second term Friedman had a distinctly more remote relationship with the president than previously and—notwithstanding the presence on the CEA of former students Sprinkel and, in the first half of the second term, William Poole—he had only sporadic interactions with the economic-policy team.

A sign of the new arrangements was brought out by what seemed on the surface to be an auspicious development: an invitation that Friedman received on October 22, 1985, to attend a meeting with the president on the subject of international trade. Friedman was suspicious about the fact that it did not appear to be a PEPAB event. In light of this, he contacted PEPAB member and one of its original coordinators, Martin Anderson. Anderson ascertained that Donald Regan—who was now the White House chief of staff and who, as Reagan's first Secretary of the Treasury, had been unhappy with the PEPAB apparatus—had allowed PEPAB to be dissolved. The planned trade-focused meeting was not going to be a PEPAB event and had among its invitees critics of the administration, including Walter Heller. Partly through pressure exerted by Anderson, this trade meeting was canceled and PEPAB was restored (see M. Anderson, 1990, pp. 269–271).

The 1985 retention of PEPAB, and the cancellation of this meeting, amounted to a Pyrrhic victory. The areas of international trade and exchange-rate arrangements were ones on which Friedman would wish he had had a closer dialogue with the administration during its second term, as his assessment was that its policies here were veering off course.[171] Being able to talk to Reagan in late 1985 about trade was an opportunity that Friedman should have seized, even if he had had to share the spotlight with Walter Heller. And as for PEPAB, its meetings in the event tailed off during Reagan's second term: Friedman's own retrospective account stated that the administration had its time taken up "by noneconomic issues, notably Iran-Contra. As a result, PEPAB met less frequently."[172]

The final PEPAB meeting was held on November 20, 1986, and Reagan seemed disinterested, writing in his diary, "They are all top nationally known economists & it's surprising how many

[170] *Toronto Star*, October 17, 1984.
[171] See the next chapter and Chapter 17.
[172] Friedman and Friedman (1998, p. 395).

things they can disagree on."[173] The reference in the same entry to what Reagan called "the Iran affair" (which would become known as the Iran-Contra scandal) confirmed that Reagan had other things on his mind than what was, in effect, his last-ever meeting with Friedman on economic policy.

During 1984, Friedman continued to speak of his admiration for Reagan, describing him as "a man of principle" (*CBS Morning News,* March 1, 1984, p. 22 of transcript), even adding later, "I know somewhat how his mind works." (*California* magazine, October 1984, p. 163). This was the sort of affinity that Friedman reserved for heroes like Irving Fisher.[174] Friedman would also give Reagan marks for what he believed was the president's own intervention that overturned the dissolution of PEPAB in October 1985.[175]

To a large degree, Friedman separated his disappointments with aspects of the Reagan Administration's economic record from his assessment of Reagan himself. Michael Boskin observed of Friedman: "The thing he was concerned about was the inability to really get a lot of control over spending. And I don't think he blamed Reagan personally for that—I think he thought it was more Congress. But, then, he was not surprised. He always thought that was a very hard thing to do. But if you asked him what was kind of a disappointment in that era, it would be that spending didn't get controlled." (Michael Boskin, interview, July 3, 2013.) Consistent with this recollection, Friedman remarked late in the president's first term that Reagan was "certainly dedicated to cutting down the size of government. And yet, what happened? Government spending has risen, [and you] do have both sides of the political spectrum in a bind where it's very difficult to keep spending down."[176]

The fiscal policy/monetary policy debate

Although Friedman still had much to complain about with regard to the curve of U.S. government spending in the mid-1980s, the same period saw his side of the analytical debate on fiscal policy's role consolidate the gains that it had already made. In particular, when it came to the positive economics of what movements in government spending implied for aggregate

[173] From the entry of November 20, 1986, in Reagan (2009, p. 452).

[174] Although he usually disliked analyses that presumed to know what someone was thinking, Friedman made an exception in his discussions of Irving Fisher: see Nelson (2020a, p. 542, fn. 102).

[175] Friedman and Friedman (1998, p. 395).

[176] *CBS Morning News*, March 1, 1984, p. 22 of transcript. Friedman was not, however, giving Reagan special treatment in making this evaluation. He had remarked early in the Carter years with regard to fiscal policy: "The active players … are the Congress, which determines the level of spending and taxing … The president does not really control either." (Friedman, 1977g, p. 6.)

demand, the mid-1980s confirmed the increased acceptance, in research and public-policy discussions alike, of Friedman's position.

To be sure, as discussed above, leading Keynesians continued to insist that "pure" fiscal policy, such as bond-financed budget deficits, mattered for nominal aggregate demand, other things equal and that, even if total nominal spending was held down by monetary policy, that pure fiscal policy measures affected real interest rates. But there was, as already indicated, considerable professional opposition to this argument. Perhaps still more importantly, it was predominantly the case that, even among those who accepted that fiscal policy in principle figured in aggregate demand determination, quantitative judgments of the importance of monetary policy and fiscal policy had shifted decisively in favor of monetary policy.

"It's not so much that monetarism has conquered—has achieved a great victory—as that simple-minded Keynesianism has suffered a great defeat," Friedman suggested in late 1982 (*Barron's,* October 25, 1982, p. 7). This qualified assessment—emphasizing the defeat of older Keynesianism, rather than a victory of monetarism—would prove durable in the following years. As discussed in the previous chapter, monetarism certainly received heavy criticism, and had serious setbacks, during 1982–1986, due to widespread disaffection with monetary aggregates. But the criticism of monetarism did not lead to assessments of monetary policy's importance to be downgraded or to a rehabilitation of older Keynesian views concerning fiscal policy.

With regard to public discussion, an economist at a Chicago-based financial institution observed in mid-1985: "In a way, I suppose it is the ultimate victory of Milton Friedman. The public and politicians understand better than ever before the importance of the Fed and monetary policy." (*New York Times*, May 13, 1985.)[177] The change in perceptions was evident in the remarks of newspaper columnist Joseph Kraft. Kraft declared: "To an extraordinary degree, the Federal Reserve Board dominated the recent ups and downs of economic activity." (*The Plain Dealer* (Cleveland), May 19, 1983.)

Similarly, in research discussions stabilization policy, monetary policy had also very largely prevailed over fiscal policy, with Guillermo Calvo (1985, p. 95) noting that the theoretical debate

[177] Similarly, a syndicated op-ed piece that appeared in the late summer of 1985 observed that "no one doubts any longer Milton Friedman's general proposition that changes in the money supply have important effects on the real economy." (*The Sun* (Baltimore), August 28, 1985.) Although this was presented as the settled opinion prevailing among academic economists, it was more accurate as a characterization of the consensus prevailing in *public debate*, rather than in economic research, by 1985. In the research world, the real business cycle literature in the 1980s was challenging, albeit with limited success, the notion that monetary policy importantly affected real output. For further discussion, see Chapter 16 below.

on "'monetarism vs. fiscalism' issues" should now be regarded as part of an "earlier literature." In the same vein, Karl Brunner observed in January 1984 that the profession was "quite a bit away from the old issues that agitated us twenty years ago."[178]

This conclusion was borne out when, on October 11–12, 1984, the Federal Reserve Bank of St. Louis held a conference specifically concerned with those "old issues." The event took stock of the previous two decades of the monetary/fiscal policy debate. In the paper that led off the conference, Bennett McCallum (1986, p. 6) judged, on the issue of the quantification of the relative importance of monetary and fiscal policy for the behavior of aggregate demand, that macroeconomists in the mid-1980s were two standard deviations away from their 1965 mean—and that they had moved in the monetarist direction on this issue.

Although he did not contribute a research paper to the conference, Friedman spoke at it, giving a lunch talk. Other contributions to the conference, most notably Karl Brunner (1986b), paid tribute to and emphasized the enduring aspects of the Friedman-Meiselman work in the 1960s on monetary versus fiscal policy. In his own speech, however, Friedman concentrated on Homer Jones and the Federal Reserve Bank of St. Louis. R.W. Hafer recalled (personal communication, February 17, 2012) of the talk that "it was a nice history of how he knew Homer, got to Chicago, etc., but also [covering] the traditions that Homer took to St. Louis, where some flourished (especially data collection)."

Friedman's heart attack in the week after the conference meant that he never wrote up the talk (see also Nelson, 2020a, Chapter 2). But his contribution proved poignant nonetheless. He had used it to pay tribute once again to his former teacher Jones—who was long retired but, being located in the city of St. Louis, attended the event. It was also the final chance that Friedman had to salute Jones in person. On March 11, 1986, Jones fell while walking in snowy conditions in St. Louis and, not being able to stand up, died of exposure, his body being found by his son a few days later (*St. Louis Post-Dispatch*, March 15, 1986). The following weekend, the *New York Times* recognized Jones' contribution by publishing a shortened version of Friedman's 1976 *Journal of Monetary Economics* article on his one-time Rutgers University instructor.[179]

Allan Meltzer would recall (interview, April 21, 2013): "Homer Jones was a courageous man and full of questions—very probing mind—and he got the St. Louis Fed onto the monetarist

[178] Brunner (1986a, p. 61).

[179] See Friedman (1976d) and *New York Times*, March 23, 1986, as well as the discussion in Nelson (2020a, Chapter 9).

track. And so he used to have me and others out there frequently to talk about it, and he was as tough in his criticisms of us as he was of anybody else, and of course [our work was] better for it… [though] he practically never [himself] wrote anything, as far as I know… He was a very different kind of person—I mean, never pushing himself forward, always being in the background, questioning what you said, with probing questions and so on. And so he was a very good leader of young economists who went to the St. Louis Fed, because he would pepper them with questions regularly and get them to improve the way they thought about these things."

The strong ranking of monetary policy over fiscal policy, once a renegade position associated with the Federal Reserve Bank of St. Louis and individual monetarists like Friedman, Brunner, and Meltzer, had become a consensus position by the 1980s. The manner in which Friedman approached the 1983 centenary of Keynes' birth likely reflected the emergence of this consensus. An Friedman article on Keynes in *The Economist* in mid-1983, as well as comments that he gave at a Mont Pelerin Society meeting of September 1984—held at Cambridge University (United Kingdom), in belated recognition of the Keynes centenary—largely stayed away from the monetary-versus-fiscal-policy aspects of the Keynesian revolution.[180]

But Friedman had no illusions that the Keynesian-monetarist debate was over or that economists as a whole had converged on a common position. "In or out of season, I'm not a Keynesian," he remarked in the spring of 1982 (*The Detroit News*, March 21, 1982) and, four years later, he added: "I have much more criticism for my fellow economists who blindly accepted Keynes and continue to do so. That isn't over by any means." (*American Banker*, April 30, 1986, p. 20.)

That some fairly "unreconstructed" Keynesian analysis could still be found in the United States was evident in some of the commentaries made on the strong recovery years of 1983 and 1984. A number of longtime Keynesians saw in the fact of simultaneous economic expansion and large U.S. budget deficits evidence of fiscal stimulus at work. "In 1982, fiscal policy turned very sharply from restraint to expansion," Friedman's longtime opponent in live debates, Robert Eisner, would contend late in the winter of 1983/1984. "… It is hence no wonder that the economy recovered sharply in 1983 and that that recovery is apparently still continuing."[181]

[180] See *The Economist* (London), June 4, 1983, and Friedman (1985h).

[181] From Eisner's written submission for his testimony of February 21, 1984, in Joint Economic Committee, U.S. Congress (1984b, p. 11). Partly on account of his living in the Greater Chicago area, Eisner had been Friedman's debating opponents in many live events from the 1950s to the 1970s. Something of a passing of the torch occurred in February 1985, when it was David Friedman who engaged in a public debate with Eisner on fiscal policy, held at David Friedman's base at the time of Tulane University. During their debate, David Friedman labeled Eisner the "last surviving unreconstructed Keynesian" (*The Times-Picayune* (New Orleans), February 16, 1985).

Likewise, Walter Heller had remarked in mid-1983: "What's bringing on this recovery is the Keynesian formula: a strong fiscal thrust, a tax cut, and a big deficit. That's a pure Keynesian prescription." (*Sunday Advocate* (Baton Rouge, Louisiana), June 5, 1983, p. 8–G.)

But even these strong proponents of fiscal policy made some important concessions regarding the importance of monetary policy. Heller acknowledged that monetary policy easing had contributed to the 1983 expansion to some degree (*Sunday Advocate* (Baton Rouge, Louisiana), June 5, 1983, p. 8–G).[182] And Eisner, having previously presented econometric work that traced real output growth to movements in a fiscal variable alone (see Eisner and Pieper, 1985), added measures of monetary policy stance to the regressions in further research (Eisner and Pieper, 1988) that was presented at a conference on the economics of fiscal policy organized by Kenneth Arrow and Michael Boskin and held at Stanford University on June 24–26, 1986.[183] The principal monetary policy variable considered by the authors was the monetary base. They found that, alongside their budgetary variable, "the real change in monetary base also proves a good predictor of subsequent increases in GNP" (Eisner and Pieper, 1988, p. 8).[184]

John Taylor, who joined Stanford University in 1984 and interacted regularly with Friedman from that point on, was a participant at this Stanford University conference that Arrow and Boskin organized. He considered the Eisner-Pieper analysis that launched the conference to be too much of a throwback. Taylor was recorded remarking that "the paper could be interpreted as a rejuvenation of the Ando-Modigliani/Friedman-Meiselman debate on the relative strength of monetary and fiscal policy," and judged "this 'contest' to be unsatisfactory." His reasons included the fact that, in the 1980s, a "vector autoregression would be more reasonable as a reduced form" than the distributed-lag specification that Eisner and Pieper (and much of the 1960s literature on the subject) had used.[185]
Indeed, the VAR approach had become, since 1980, the standard basis for the econometric

[182] In a similar vein, when Allen Sinai appeared on a panel with Friedman in June 1984, Friedman noted accurately that Sinai "attributes the extraordinarily strong economy or the extraordinarily strong recovery to fiscal stimulus" through fiscal-multiplier channels (R.H. Heller and others, 1984, p. 46). Sinai also suggested, however, that monetary policy exerted important effects and (accurately) foreshadowed that monetary tightening might soon produce a "major slowdown of economic activity" (p. 37).
[183] The date was given in Eisner (1998, p. 495).
[184] The authors used as the monetary regressor the real change in monetary base as share of GNP (Eisner and Pieper, 1988, p. 9). A disadvantage of this approach, compared with simply using the percentage change in the real monetary base, was that the variable was equivalent (up to a change in sign) to the percentage change in velocity. For the purposes of short-run analysis, however, the regressor might still accurately relay changes in the quantity, rather than the velocity, of the monetary base. This would be the case especially insofar as changes in the base were not accompanied by a same-period movement in aggregate real and nominal income.
[185] See Arrow and Boskin (1988, p. 39).

analysis of macroeconomic time series. The empirical macroeconomic work that had appeared using VARs through the mid-1980s actually further attested to the fact that monetary policy had prevailed over fiscal policy in much U.S. macroeconomic analysis. For, as McCallum (1986, p. 10) noted, VARs almost invariably incorporated at least one monetary policy variable among the time series considered but—at this point—VAR systems included fiscal variables only infrequently.

Friedman's approach to aggregate demand analysis also permeated the theoretical and empirical research in the 1980s that became known as early New Keynesian economics. John Taylor was one of the members of this group to be interviewed for a January 1986 media piece on New Keynesian economists. Taylor was quoted as saying: "Fiscal policy has been shown to be a very unwieldy instrument… [and] to think of it as an instrument that can be used to stabilize the economy is implausible." The article, although titled "Comeback for Keynes?," consequently had occasion to observe that many New Keynesians "are suspicious of fiscal policy… [T]hey overwhelmingly reject the view of the 1960s that taxation and spending can even out the economy's minor fluctuations."[186]

The same article nevertheless accurately noted that "most of Taylor's colleagues [in the New Keynesian group] are unwilling to rule out fiscal policy so absolutely" as Taylor did.[187] Indeed, among New Keynesians, some research in the 1980s was concerned with establishing possible deviations from the fully forward-looking versions of the permanent income hypothesis (and the corollary of Ricardian equivalence) and thereby uncovering possible scope for more traditionally Keynesian channels of effects of pure fiscal policy. Notably, John Campbell and Gregory Mankiw (1990) considered the possibility that a portion of households might gear their spending to current rather than permanent income, while, in a paper presented at a conference held at Rochester University in October 1986, James Poterba and Lawrence Summers (1987) suggested that the behavior of U.S. national saving in the 1980s was characterized by considerable deviations from Ricardian equivalence.[188]

[186] *Dun's Business Review*, January 1986 (reprinted in McClelland, 1986, p. 80).

[187] *Dun's Business Review*, January 1986 (reprinted in McClelland, 1986, p. 80).

[188] In the case of Mankiw, however, there was notable convergence, over the course of two decades, toward the fiscal policy prescriptions that Friedman had expressed during the 1980s. In a letter published in the *Wall Street Journal* in 1983, Mankiw reacted to Friedman's analysis of the demand for money by suggesting that his own research on money demand suggested that an increase in taxes on household income was desirable (*Wall Street Journal*, September 29, 1983). Mankiw, who was completing graduate studies at MIT at the time of the letter, later changed his mind and indicated that he believed that 1984's presidential candidate Walter Mondale had erred in proposing to raise personal income taxes (*Wall Street Journal*, March 3, 2005).

But even this approach accepted the permanent income hypothesis as a baseline. Therefore, in this area of the specification of aggregate demand in the specification of structural models, the period 1982 to 1986 saw a strengthening of Friedman's views as the prevalent ones in the profession. "Surely, permanent income is now generally accepted as the variable to which private consumption responds," Anna Schwartz remarked in 1986.[189]

In the same connection, Abel and Bernanke (1992, p. 172) viewed the 1981−1982 recession in the United States as being a "particularly clear" example of a match with the prediction of the permanent income hypothesis: they cited the relative stability of U.S. private consumption in the face of declining total income. In research journals, Hall's (1978) optimization-based version of the permanent income hypothesis had become widely accepted as a baseline specification. This development (which complemented the trend of research concerned specifically with Ricardian equivalence) cemented the earlier break from current-income-based models of consumption. True, it also amounted to a rejection of Friedman's particular empirical implementation of the permanent income hypothesis in his time-series work—in which permanent income was made a fixed function of current and past income. But Friedman had not regarded that empirical measure as inherent to the permanent income hypothesis and had long entertained approaches to the hypothesis that imposed rational expectations.[190] By 1987, Thomas Sargent was characterizing Friedman's permanent income hypothesis, as developed by Hall, as having decisively prevailed over more mechanical or static characterizations of consumer behavior.[191]

Permanent-income-centered models were quite compatible with the central bank playing an important role in the short-run behavior of output, and the benchmark New Keynesian models in this period focused on monetary policy. Consequently, Blanchard and Fischer (1989a, p. 418) observed that "new Keynesian economists often focus on the effects of nominal money." A representative example of the literature during this time was Blinder and Mankiw (1984, p. 68), who simply postulated a representation of nominal income in terms of the money stock.

[189] Schwartz (1986a, p. 671).

[190] See Nelson (2020a, Chapter 5; 2020b, Chapter 15) for further discussion. The old, backward-looking definition of permanent income was very occasionally still used in empirical work in the 1980s. For example, Darby (1982, p. 750) used permanent income as a regressor (albeit without being explicit that he was, in fact, using the old backward-looking formula in creating the regressor), while McAleer, Fisher, and Volker (1982, p. 580) explicitly used the backward-looking formula in generating a permanent-income series. (In addition, Modigliani and Sterling, 1986, p. 1178) took it as being well established that there were "relatively long lags required to approximate permanent income," although they did not use Friedman's specific formula.) Predominantly, however, the backward-looking calculation was regarded as having been superseded by the rational expectations literature on the permanent income hypothesis.

[191] See Sargent (1987).

The abstraction from fiscal policy in these analyses partly reflected a desire to focus on the price-setting side of macroeconomic models. But it also attested to the shift that had occurred over the preceding three decades in the Keynesian position on aggregate demand behavior—from one in which Keynesianism was associated with effects on output and employment of fiscal policy actions, to one in which Keynesianism entailed an emphasis on the short-run effects of monetary policy on real variables. In this vein, Michael Woodford, speaking at a conference in Hong Kong held at the start of 1986, associated models in which short-run monetary nonneutrality arose from nominal rigidity with "a very Keynesian view of the role of money in the business cycle."[192]

The natural rate hypothesis—under challenge but prevalent

The effect of monetary policy on output was felt for "years, not months," Friedman remarked at the Bank of Japan's June 1983 research conference. As was detailed in Chapter 13's discussion of that conference, Friedman's talk was given during a time when he was updating the 1963-vintage Friedman-Schwartz research on the link between monetary-growth variability and instability in real output. The results of this update helped him stress the point that the U.S. experience accumulated during Paul Volcker's first term as Federal Reserve chair added to the evidence that "monetary instability is so destructive of economic stability."[193]

In the article that he wrote on Keynes for *The Economist* shortly before this talk, Friedman put the analysis of output variability into perspective. He noted that the fact that he had spent much of his career on the analysis of the cyclical effects of monetary policy demonstrated his belief in the importance of such analysis. But he cautioned against letting such a focus slip into a "dismissal of the 'long run'" (*The Economist* (London), June 4, 1983, p. 36). Correspondingly, when, later in the year, the *New York Times* asked Friedman to give his views of aggregate economic behavior, the summation he offered was: "Changes in the quantity of money, as such, in the long run have a negligible effect on real income, so that nonmonetary forces are 'all that matters' for changes in the real income over decades and money 'does not matter.'"[194]

The notion that the real effects of monetary policy wear off and other factors come to dominate the behavior of output was further underscored by Friedman in a memorandum he prepared for

[192] In Institute of Economics (1986, p. 373).
[193] Friedman (1983e, p. 2).
[194] *New York Times*, November 16, 1983.

the January 1986 meeting of President Reagan's economic advisory board: "The long-term outlook for real income depends largely on nonmonetary factors."[195]

Friedman's postulate of the long-run independence of real output and monetary factors rested heavily on the natural rate hypothesis that he and Edmund Phelps had advanced. That hypothesis, promulgated by them primarily during the 1960s, was receiving considerable further empirical support in light of the U.S. disinflation experience of the 1980s. The U.S. data for the first half of the 1980s seemed to fit neatly in line with the Friedman-Phelps story neatly. The years 1980 to 1982 had seen divergent, and large, movements of unemployment and inflation and so made for a clearly downward-sloping Phillips-curve scatter—consistent with Friedman's position that such a curve described short-run fluctuations.[196] Conversely, the fact that the unemployment rate was much the same (a bit above 7 percent) at the end of Reagan's first term as at the beginning, even though inflation was much lower, added more evidence to the lack of a *long-run link* between the two series.[197]

In addition, the years preceding the early-1980s disinflation were coming to be viewed in the way Friedman had characterized them at the time: as predominantly featuring excess demand, not slack. That this assessment had prevailed was brought out in Bernanke (1982a, p. 219) reference to the "apparent growing consensus that aggregate demand was overstimulated in the late 1970s."[198]

Not only did the short-run nonvertical long-run vertical, expectational Phillips curve receive further empirical support being provided for it by new data points in the years from 1982 to 1986, it also in those years became still more widely accepted in economic analysis—including in policy discussions, teaching, and research. Friedman could observe on this matter in 1986 (*American Banker*, April 30, 1986, p. 20): "There is no doubt that the opinions of people in the profession changed drastically."

[195] Friedman (1986f, p. 3).

[196] G7 countries' aggregate unemployment and inflation data also formed a smoothly downward-sloping Phillips curve from 1980 to 1982. In contrast to the case of the United States, in which unemployment and inflation fell together in 1983, the G7 data showed a continuing inverse unemployment/inflation relation into 1983—the further fall in G7 inflation in that year being associated with unemployment moving up still more (see Buiter and Miller, 1983, p. 310, Figure 2).

[197] The unemployment rate was 7.2 percent in November 1984 and in February 1985 and was 7.3 percent in the intervening months. The rate of 7.2 percent was also the rate prevailing in December 1980.

[198] In addition, in a contribution that he made to the Federal Reserve Bank of New York's May 1982 conference on monetary targeting, vice president and monetary adviser referred to the FOMC's "recurring underestimation of the strength of aggregate demand" in 1977–1979 (Meek, 1983, p. 70).

In the world of textbooks, the increasingly successful William Baumol/Alan Blinder guide to economics principles exhibited a notable change on this subject. Having stuck with the long-run nonvertical Phillips curve in the first edition, the second and third editions—Baumol and Blinder (1982, pp. 302–303; 1985, pp. 316–317)—cast the relationship as being vertical in the long run, and the "natural rate of unemployment" terminology was also used in these subsequent editions. The natural-rate terminology had, by this point, come to be such a mainstay in economic language that neither Baumol and Blinder, nor the more "new classical" macroeconomics-sympathetic textbook of Barro (1984) actually credited Friedman with the term.

Likewise, the term had permeated newspaper discussions of economic matters: a *New York Times* analysis titled "When Is Unemployment 'Natural?'" (October 25, 1982) discussed the concept (albeit without mentioning Friedman).[199] And—in keeping with the notion that the natural rate was time-varying—the article took for granted that the full-employment goal embodied in the Humphrey-Hawkins Act (or at least in practitioners' interpretation of that legislation) was one that shifted as the natural rate of unemployment itself changed. Further into the 1980s, longtime financial columnist Sylvia Porter deployed the terminology, observing in a discussion of developments since the 1960s: "The natural rate of unemployment has gone up." (*Daily News* (New York), November 4, 1985.)

The summer of 1986 saw the launch of the NBER's annual macroeconomics conference proceedings. The paper and Olivier Blanchard and Lawrence Summers wrote for the conference put forward the concept of "hysteresis" in the unemployment rate. As a theoretical matter, the article was offering a potentially powerful challenge to the natural rate hypothesis. The paper did not dispute that the demand restriction had produced disinflation in the 1980s but argued that the job loss associated with such a disinflation might evolve from cyclical to structural unemployment. In such a case, and contrary to the natural rate hypothesis, the long-run levels of output and unemployment were not independent of monetary policy, and the weakness of the real economy associated with the restriction of nominal aggregate demand could become permanent.

The hysteresis postulate of Blanchard and Summers (1986) did not restore an older-generation Phillips curve, however, because it did not imply that high unemployment could necessarily be reduced by expansionary or inflationary demand policies. "I don't think the hysteresis work is a fundamental challenge to what Friedman said, [although] I think it's a very important

[199] See also *Miami News*, October 26, 1982.

emendation," Lawrence Summers remarked (interview November 22, 2013). "You know, the core of Friedman's proposition was: You don't get permanently higher benefits by running a permanently higher inflation rate. And that is a very important truth."

Despite receiving considerable attention, the hysteresis article in the event did little to move the professional consensus away from being in favor of the natural rate hypothesis. Even the authors were not offering it as an empirical description of the U.S. situation, and indeed Blanchard (1997, p. 347) later showed that a scatter plot inspired by expectations-augmented Phillips-curve ideas described 1970–1994 U.S. data well. The authors did suggest that hysteresis might describe unemployment in Western Europe in the 1980s. But many subsequent studies found that real factors—in particular, labor market institutions—could account well for the emergence of a high natural rate of unemployment in Europe.

As for the trajectory of the U.S. unemployment rate, it continued to fall from 1983 to 1985. One of the many Keynesian queries raised about the quality of the economic expansion that occurred under Reagan concerned the extent to which it had brought down the unemployment rate after three years. "I submit that we can and should do better than this," Alan Blinder argued in his *Business Week* column (February 3, 1986). "The average unemployment rate in 1985 was 7.2 percent. Of all the years between the Depression and 1980 only two—1975 and 1976—had an annual unemployment rate that high." In light of this, Blinder called for further monetary policy easing to take place in 1986.

Unemployment indeed did fall further in 1986, ending the year at 6.6 percent. But the rate that Blinder's 1986 analysis recommending policy easing—1979's average of 5.8 percent—had been achieved during a period that, as noted above, was now increasingly recognized as having been one characterized by economic overheating. In retrospect, policymakers recognized that the natural rate of unemployment was about 6 percent in the late 1970s (see Orphanides and Williams, 2005). Consistent with this, Paul Volcker told the Joint Economic Committee on February 9, 1984, that "the natural rate of unemployment…. has been much debated, and there is a feeling it may be higher now than it was 10 years ago or 15 years ago."[200] A few months later, he indicated that the Federal Reserve had been assuming a 6 percent full-employment unemployment rate for some years.[201]

[200] In Joint Economic Committee, U.S. Congress (1984a, p. 266).

[201] See Volcker's testimony of June 14, 1984, in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1984c, p. 203).

The likelihood that this value, or slightly higher, corresponded to the natural rate in the 1980s was underscored when unemployment fell below 6 percent in 1987. As discussed in the previous chapter, policymakers in 1986 likely overestimated the extent to which monetary policy could be eased without reviving inflationary pressure in the subsequent year, and the U.S. economy seemed to overshoot the level of potential output.

Aggregate supply, employment, and investment

The possibility that aggregate U.S. output would exceed its potential value had been discussed by Friedman and his longtime friend Francis Bator in January 1986, on one of the occasions on which they made their annual skiing visits to Alta, Utah, at the same time. Friedman disagreed with Bator's prescription of canceling the 1981 Reagan tax cuts. But, aside from this predictable area of disagreement, Bator was struck by what he considered Friedman's "shocking" further reply that, with regard to U.S. economic policy, he was vastly less interested in the course of cyclical macroeconomic fluctuations than in reducing the role of government in the economy. "He was baiting me to some extent," Bator, an MIT-trained Keynesian, noted, "because I don't think it's true that he didn't care about the short-run macro effect of policy. But that's the most vivid conversation in my head." (Francis Bator, interview, January 6, 2015.)

Friedman, of course, was still devoting a large amount of time to each of these sets of issues. But the prominence of his concern with the longer-run behavior of the U.S. economy and the nonmonetary factors driving that behavior was evident in the large volume of writings and public-policy activities he devoted to the subject during the Reagan years.

This concern was also evident in the PEPAB memorandum that Friedman submitted a few days after the January 1986 Utah trip. Although it was mainly concerned with the possible implications of recent monetary developments for output and inflation, the memorandum also referred to the variables that "determine the long-term trend around which monetary and other disturbances will continue to produce fluctuations" in real output.[202] These variables, Friedman suggested, were "the level of government spending and taxes, the extent of government intervention into the economy, and such basic factors as the enterprise, ingenuity, and saving and investing propensities of the population."[203]

[202] Friedman (1986f, p. 3).
[203] Friedman (1986f, p. 3).

This concern with the real factors driving potential output primarily involved the areas of microeconomics and longer-term economic growth—and so it was largely outside the monetarist body of thought with which Friedman was most associated. As Friedman had put it in a television appearance: "Monetarism is not economics in general. It's a narrow subject."[204] The natural rate hypothesis could be encompassed in a definition of monetarism. But that hypothesis conditioned on the behavior of the natural rate of output or unemployment and did not seek to explain either the trend of, or fluctuations in, those series. In contrast, when he made policy prescriptions intended to improve the behavior of the real economy, Friedman was, in essence, making postulates about the determination of the natural output level and its growth rate.

Friedman's policy prescriptions in this area, in contrast to those regarding monetary policy, involved considerable common ground with supply-side economists. Consequently, at the March 1982 conference on supply-side economics that was discussed at the start of this chapter, Friedman was able to portray himself, the administration, and outside supply-siders as having a united front. "[F]or a long time we forgot about good economics and practiced bad economics," Friedman suggested, in referring to the comparative lack of emphasis on incentives and market solutions in the setting of U.S. economic policy before the 1980s.[205]

At the end of 1982, a year during which criticism of supply-side economics had intensified but also in which supply-siders' criticisms of monetarist policy prescriptions had also risen in pitch, Friedman again blasted the "exaggerated claims" of supply-side proponents. "They said the federal government would see immediate revenue increases from across-the-board tax reductions. Obviously, that was exaggerated." Predictions of this kind, he suggested, were "dead and buried." But Friedman added that the supply-side movement's essential emphasis on price-sensitive private sector decision-making, including upward-sloping supply curves, was justified. "The willingness of people to work and save and invest depends upon the return they'll get. That is a basic economic truth and a basic supply-side claim. That has always been the case and always will be." Consequently, supply-side "principles are alive and well." (*San Diego Union* (California), December 20, 1982, p. A–11.)

In the year and a half after the initial Reagan tax cuts were fully phased in, however, much of Friedman's public commentary was gloomy about the economy, on account of his fears of recession and a major revival of inflation. During 1983 and 1984, sympathizers with supply-side

[204] *Meet the Press*, NBC, March 21, 1982, p. 9 of transcript.
[205] Friedman (1982a, p. 54).

economics in the business and conservative press criticized Friedman for spreading pessimism and, they believed, underestimating the significance of the tax cuts for prospective U.S. economic performance (*The Detroit News*, October 19, 1983; *National Review*, June 1, 1984). The Friedmans' book *Tyranny of the Status Quo*, with its implication that the Reagan revolution had run out of steam after its initial months—and its focus on the administration's setbacks in controlling government spending—had, on the whole, reinforced the impression that Friedman did not see a major change in policy direction as having been achieved.

Friedman's disagreements with supply-siders in substance and in emphasis remained. But, starting from around the spring of 1984, after he withdrew his prediction of an early-1984 recession, he put a greater spotlight on the favorable aspects of the post-1982 economic expansion. In part because he believed that a monetary excess had been built up, Friedman continued to believe, until the May 1986 rethink discussed in the previous chapter, that a corrective recession was likely. His January 1986 PEPAB memorandum had contained one of his final warnings along these lines.[206] In contrast, however, to his 1983–1984 analyses—which had seen the post-1982 expansion as on track to be an ephemeral economic revival along the lines of 1980–1981—his 1984–1986 warnings of recession occurred alongside a brighter characterization of the features of the expansion seen to date and about longer-term U.S. prospects.

In particular, from mid-1984 onward, Friedman increasingly discerned favorable supply-side features of the current expansion. Speaking in late June 1984, he suggested that, in terms of the composition of real growth, "the actual shape of the expansion is more nearly consistent with the supply-side interpretation than it is with the Keynesian interpretation," because "this is not a consumer-led expansion" but instead was seeing firms' purchases rising strongly (Heller and others, 1984, p. 46). With these comments, Friedman put himself on one side of what became a perennial dispute about the Reagan-era economic expansion: whether it had been a strong one for business fixed investment. Especially in view of their emphasis in the 1980s on crowding out, Keynesians tended to take a position that was nearly the polar opposite of Friedman's strong-investment interpretation. Benjamin Friedman, for example, published an op-ed in mid-1985 titled "The Vaunted Investment 'Boom' Is a Bust," in which he stressed that the strong increase in investment during 1984 had had its starting point "the recession-depressed level of 1983" and also highlighted—as did many critics of the Reagan-era economic expansion—the considerably

[206] That memorandum stated: "The only question is when the recession will come—in late 1986 or in 1987." Friedman, 1986f, p. 3.)

weaker performance of net investment than of gross investment.[207] As Benjamin Friedman's remarks implied, the evidence of the Reagan years on investment was ambiguous. And the mixed picture persisted in the later Reagan period: the change in real private nonresidential investment was strong (6 percent) in the year of Benjamin Friedman's op-ed, but then was essentially zero in 1986 and 1987 before picking up strongly in Reagan's final year or so in office.[208]

During the 1980s, the peak change in real private nonresidential investment was the 1984 rise that Benjamin Friedman scrutinized: a 16.7 percent increase. It was in the second half of 1984 that Milton Friedman highlighted some of the brighter features of the ongoing U.S. economic recovery. During his trips to the United Kingdom and Continental Europe in the first half of fall 1984, he continued to maintain that Reagan was lucky to have avoided an election-year recession (*Naples Daily News* (Florida), October 1, 1984) but remarked that he was impressed by the "largely unprecedented" dynamism being shown by the U.S. real economy and hinted that this behavior might reflect structural improvements (*The Times-Picayune* (New Orleans), October 2, 1984), including with regard to long-term economic growth (*Schenectady Gazette* (New York), September 21, 1984). Friedman further underlined the positive aspects of the Reagan recovery during 1985 when he compared labor market performance in the United States and the European Community countries (particularly France and Germany): the latter group had had stagnant or even declining levels of total employment since the end of 1982, he stressed, while jobs in the U.S. economy had grown in the same period (*San Francisco Chronicle*, April 18, 1985; *Wall Street Week*, Maryland Public Television, November 15, 1985, p. 8 of transcript).[209] Friedman had become so positive about the U.S. real picture that an anti-Reagan letter to the editor complained about "the blissful assessment of the U.S. economy by Milton Friedman" (*Boston Globe*, June 18, 1985).

In October 1985, Friedman spotlighted the improvement in longer-term real economic performance under Reagan and contrasted this with his continuing mostly negative assessment of monetary policy under Paul Volcker. "You have a very healthy real productive economy side by

[207] *New York Times,* July 7, 1985. Similarly, Walter Heller (*Wall Street Journal,* February 28, 1986) referred to "investment performing so poorly."

[208] See the annual data available at *https://fred.stlouisfed.org/series/PNFICA.*

[209] In the book *Tyranny of the Status Quo,* Friedman and Friedman (1984, 1985) had previously contrasted the United States favorably with Europe, but they had mainly focused on greater political stability and more market-friendly institutions rather than on a comparison of recent labor market performance. (For further discussion, see the next chapter.) The original writing of *Tyranny of the Status Quo* occurred when the economic recovery was considerably less than a year old, and the authors emphasized with regard to Reagan's economic record that the "sharp rise in unemployment was clearly his greatest failure." (Friedman and Friedman, 1984, p. 105.)

side with a very unhealthy system that is generating it," he remarked. At this point, he believed that although supply-side prospects had improved, monetary policy had provided demand stimulus out of proportion to the improvement, so the economy was heading for "a big surge" that he expected to be followed by a sharp Federal Reserve tightening (*San Francisco Chronicle*, October 5, 1985). Over the following months and years, Friedman would revise his assessment notably, thereby bringing his assessment of the real and nominal aspects of the U.S. economic expansion of the 1980s into better alignment.

The productivity slowdown continues

One aspect of real performance in the 1980s on which Friedman and others proved to be disappointed was that of long-run productivity and economic growth. The Friedmans noted in 1984 that the slower average rate of increase in aggregate U.S. output recorded in recent times reflected "the marked slowdown during the past decade in economic productivity."[210] Early in that same year, Paul Volcker had pointed to the rebound in productivity growth seen in the 1983 recovery year as a hopeful sign that the 1970s' slow rate of productivity increase might have been replaced by a higher trend rate.[211] In the event, however, growth in U.S. productivity (output per hour of all persons in the nonfarm business sector), having been 4.4 percent in 1983, would be only 2 percent in 1984 and 1.6 percent in 1985 (Council of Economic Advisers, 2011, p. 250, Table B–50).

At the start of 1986, Friedman was nevertheless upbeat: "The long-term outlook for real income has been improved greatly in the past five years by reductions in marginal tax rates and government regulation, and [by] the slowing, though not halting, of the growth of government spending as a fraction of income."[212]

In the event, however, the period after 1981 would not be regarded as seeing an uptick in longer-term productivity growth. In terms of the behavior of levels, the economy was doing better than the stagnant or declining productivity values of 1979 and 1980—consistent with the Friedmans' conjecture that the high inflation rate of those years had lowered productivity.[213] But an improvement in the slope of the longer-term productivity line was not forthcoming in the Reagan years. Instead, the period would be categorized as part of the 1973–1995 "slow growth" era of

[210] Friedman and Friedman (1984, p. 110; 1985, p. 107).
[211] See Volcker's testimony of March 1, 1984, in Committee on the Budget, U.S. House of Representatives (1984, pp. 4, 59).
[212] Friedman (1986f, p. 3).
[213] Friedman and Friedman (1984, p. 110; 1985, p. 107).

1.3 percent average increase per year in output per hour (see Fernald, 2016, pp. 6, 8). In terms of productivity growth and the accompanying aggregate growth in real output, it would turn out that things were not as bad as Walter Heller had suggested in December 1985 when he stated that "we have the slowest growth that we've had in the past 25 years: we were growing at 4.1 percent in the '60s; 3.1 percent in the '70s; so far, 2.1 percent in the 1980s."[214] Once the full decade's data had accrued, the inference that output growth had slowed further in the 1980s did not hold so strongly. Average real GDP growth rates in the 1970s and 1980s would be similar: 3.26 percent in the 1970s, 3.05 percent in the 1980s.[215]

Friedman's own diagnosis of the productivity slowdown had stressed the increased role of the public sector in the U.S. economy. Conversely, he saw a reduction in the role as the means of securing a productivity revival. In this vein, Friedman stated in early 1982: "we have the opportunity in the next decade or two of having enormous improvement… This is a powerful country. We have all the makings for a potential explosion in output and productivity, if we will only take the shackles off the enterprise and initiative of the American people."[216]

And when it came to the fiscal aspect of the economic role played by government—and so one of the roles that, in his view, most needed redressing—Friedman's analysis had given public expenditure a hierarchical position over both taxes and deficits. Over the frequent objections of Keynesians and supply-siders alike, he maintained: "The crucial thing about fiscal policy is how much government spends."[217]

Having taken this position, Friedman could reasonably cite—as a reason for the absence of a productivity rebound—the absence during the Reagan years of a winding-back of the 1970s rise in the government spending's share of output. "It's the only way to release the fundamental drive and initiative that this country has," he had remarked with regard to reductions in public expenditure (*The MacNeil/Lehrer News Hour*, PBS, March 27, 1984, p. 7 of transcript).

Stock prices: the turnaround starting in 1982

The economic expansion that began in the later months of 1982 had a further feature, one that tentatively pointed to the possibility that an improvement in the supply side of the economy: the

[214] *Wall Street Week*, Maryland Public Television, December 13, 1985, p. 8 of transcript

[215] Computed from Council of Economic Advisers (2011, p. 194, Table B–5).

[216] *Meet the Press*, NBC, March 21, 1982, p. 10 of transcript.

[217] *Meet the Press*, NBC, March 21, 1982, p. 3 of transcript.

behavior of the stock market.

The steep rise in equity prices that occurred during the 1982−1990 U.S. economic expansion—which would be undone only temporarily and partially by the stock market crash of October 1987—set that expansion apart from those seen in the 1970s, when equity price indexes had been largely stagnant even against the background of considerable growth in real output.

The U.S. stock market's rally starting in 1982 was registered not only in equity prices but in a major revival in the issuance of equity claims by American firms. In September 1974, Otto Eckstein had remarked at a White House conference on the current state of the U.S. financial system: "Our equity markets are already gone as a practical source of capital."[218] The longstanding perception that the U.S. equity market was moribund as a basis for financing business investment was hard to dispel even after the bull market had begun in 1982. For example, at a conference held at the Federal Reserve Bank of Boston in October 1983, Benjamin Friedman had treated it as implausible that firms would move to reliance on equity finance on a large scale in obtaining funds for their capital formation. His basis for this judgment was that the volume of new share issues was stagnant in the 1970s and early 1980s—though he noted that an upsurge in stock market activity had occurred in the first half of 1983 (B.M. Friedman, 1983b, p. 93). Despite this pessimism, large-scale equity financing by firms did indeed enjoy a major comeback in the 1980s.[219]

The bull market of U.S. equity prices began in August 1982, somewhat ahead of the start of the economic recovery. An important impetus driving the timing of this shift was a revised assessment of the U.S. economic and financial outlook by Henry Kaufman, who, in part in response to the FOMC-engineered declines in short-term interest rates, abandoned his famous prediction (which he had reaffirmed as recently as July) of a late-in-the-year surge in U.S. longer-term interest rates.[220]

This proposed explanation for the timing of the stock market revival does not, however, explain

[218] From Eckstein's remarks of September 20, 1974, in U.S. Treasury (1974, p. 132). Subsequently, in 1978, Eckstein supported the proposed cut in capital gains tax as a measure designed to support the stock market. See Laffer and Seymour (1979, pp. 86–94).
[219] There was also a revival in corporate bond issuance. Cagan (1984, p. 31) judged that long-term bond issuance by U.S. firms had been "largely abandoned during the 1970s" along with equity issuance. But the corporate bond market has a renaissance in the 1980s. See, for example, Miles (1988, p. 561) and Bernanke (1989).
[220] With regard to Kaufman's July 1982 reaffirmation of his interest-rate prediction and his August 1982 rethink, see *The Times-Picayune* (New Orleans), July 20, 1982, and *Corpus Christi Times* (Texas), August 18, 1982*a*, *b*. See also the retrospective by Kaufman and Sicilia (2021).

why the upward trend in stock prices proved lasting. In view of the numerous changes taking place in economic policy in the early 1980s, it is not surprising that quite divergent explanations for this development have gained ground. One class of explanations centered on the Reagan tax cuts. Arthur Laffer identified the start of the bull market with the growing acceptance among observers that the budget negotiations that led to TEFRA would not lead to cancellation or curbing of the personal income tax cuts (interview, August 11, 2014). In contrast, Blanchard and Summers (1984, p. 275) stressed the business tax cuts, through the boost that this provided to the profitability of investment, as a key factor driving the rise in stock prices.[221]

A different class of explanation focused on change in monetary policy regime—and, in particular, the substantial restoration of price stability starting in 1982. Notably, in a contribution made to a conference held in May 1983, Alan Greenspan (1984, p. 11) suggested that Paul Volcker's tenure as Federal Reserve chair had seen reflects "sharp and very credible decline in the inflation rate in the last year or two" and argued (p. 13) that the recent revival of the stock market reflected investors' assessments that inflation was permanently down. And, 40 months into the bull market, television host Louis Rukeyser, discussing what "pushed the market so high," observed: "Paul Volcker has been hailed as the dragon-slayer: the man who slayed the dragon of inflation."[222]

Milton Friedman himself had suggested in 1978 that a successful end to inflation in the United States might generate a stock market boom (see Chapter 11 above). But he was reticent about applying this analysis to the combination of a market rally and much lower inflation that emerged starting in 1982. For one thing, as stressed in the previous chapter, it took Friedman a long time to accept that the disinflation would last. In contrast to Greenspan, he believed during 1983 that a rebound in inflation was in the making. So in October 1983, asked about the rise in stock prices, Friedman replied that it might just reflect the upward pressure on asset prices, especially prices of claims on U.S. companies, that was common in the early stages of an inflationary process.[223] For another thing, as he stated in the same remarks, "I don't predict

[221] It is worth quoting Obstfeld and Rogoff's (1996, p. 35) account of how the "influential paper by Blanchard and Summers (1984)" primarily supported their position: "they offered econometric equations for the main industrial countries showing that investment in 1983 and early 1984 was higher than one would have predicted on the basis of factors other than the expected future productivity of capital." That is, the authors regressed variable Y on a regressor set X and evaluated whether the regression residuals were likely related to the non-included variable Z (see Blanchard and Summers, 1984, pp. 304–305). A quarter-century earlier, some of Friedman's (1959) money demand work had taken this approach, and his doing so had given rise to considerable criticism.
[222] *Wall Street Week,* Maryland Public Television, December 27, 1985, pp. 7, 8 of transcript.
[223] Friedman (1984f, p. 44).

anything about the stock market."[224]

Especially since the late 1960s, this disinclination on Friedman's part to make market predictions was accompanied by a conviction that stock prices did not feed back much into economic activity—and, in particular, that equity price variables mattered little for aggregate demand.[225]

Nevertheless, the constant commentary during the mid-1980s regarding the impressive performance of U.S. stocks evidently had the effect of making Friedman inquisitive about the repercussions of the bull market. In 1986, he started a research project in which he investigated whether stock market variables mattered for demand for money. This project will be discussed in Chapter 16 below.

Other writings and research activity, 1982–1986

Friedman's launch in mid-1986 of his project on the stock market was an example of his continuing forays into economic research during the 1982–1986 period, even when much of his time was spent on public policy.

Friedman's activities during a succession of days early in this period demonstrated how his research participation and policy activism jostled with one another. On March 17, 1982, he attended the conference in Atlanta described above. Although Friedman's talk at the symposium was almost entirely about policy matters, the overall occasion was essentially a research conference. Three days later, Friedman attended a *bona fide* research conference—an NBER event on the historical experience associated with the Gold Standard—organized by Michael Bordo and Anna Schwartz held on Hilton Head island in South Carolina.[226] The following day, on March 21, he gave a live interview in New York City's NBC studios for *Meet the Press*. Friedman then spent several days in Canada for a number of public-policy engagements, including a talk on March 23 at the Fraser Institute in Vancouver.[227]

The NBER conference involved Friedman defending the *Monetary History*'s account of the 1933–1937 U.S. recovery from the Great Contraction. It was a defense of the Friedman-

[224] Friedman (1984f, p. 44).
[225] See Nelson (2020a, Chapter 5).
[226] See Bordo and Schwartz (1984) and Friedman (1984i). The conference was held on March 19–21, 1982, although Friedman left about halfway through it.
[227] See Friedman (1982f).

Schwartz account of the Great Contraction itself that formed the basis for another Friedman research contribution, coauthored with Schwartz, that appeared in the journal *Explorations in Economic History* in April 1986.[228] Six months earlier, the journal had published an article by Joseph L. Lucia on the 1930 failure of the Bank of United States. Friedman and Schwartz had argued that the bank had been financially sound on important dimensions—and that financial and macroeconomic stability considerations also suggested that the bank should have been given an official rescue. They argued that neglect of the monetary repercussions of a prominent bank failure, along with dissension (magnified, they suggested, by prejudice) among the officialdom of the U.S. banking system concerning whether a rescue should proceed, had led to the bank's failure. Lucia (1985) argued that the bank was unsound and made this part of a broader challenge to Friedman and Schwartz's indictment of U.S. monetary management in the 1930s.

The Friedman-Schwartz monetary project had essentially ended with Friedman's departure from the NBER in 1981 and *Monetary Trends*' publication in 1982. "I sort of went on, on my own," Anna Schwartz recalled (interview, April 22, 2013). "The project essentially came to an end." The lack of attention generated by *Monetary Trends* reinforced Friedman's inclination from 1983 onward to leave the Friedman-Schwartz work where it was.

Friedman also had a longstanding aversion to going into print in journals with replies to challenges to his work. "I asked him at one point in time—there was somebody who sent in a manuscript to my journal [the *Journal of Post-Keynesian Economics*] about something Milton wrote—and I asked him would he like to [reply]: I was going to publish it, would he like to write a rejoinder," Paul Davidson recalled. "And he said: No, he treats his publications like his children… [With publication,] they're on their own from then on. So that was his response. He obviously didn't want to respond to these things." (Paul Davidson, interview, May 3, 2013.) In the letter to Davidson (of March 7, 1984), Friedman also suggested that it would be a full-time job for him to write rebuttals to criticisms of his research.[229]

In the case of the Lucia paper, Friedman was better disposed toward writing a reply, in part because he felt that Lucia had given little weight to a letter of response Friedman had sent Lucia in October 1982, after Lucia sent him his manuscript. Friedman was therefore receptive to writing a reply to Lucia, and he and Schwartz did so. In their reply, they took exception to how

[228] See Friedman and Schwartz (1986b).
[229] Letter in Paul Davidson papers, Duke University library.

the Lucia account had rendered their narrative of the Bank of United States' demise.[230] More importantly, they contended that he had misstated their critique of the Bank of United States' fate and the Federal Reserve's response to the Depression.[231]

The reply was notable for its shooting-from-the-hip tone—with Lucia's paper seemingly having drawn out the bad-tempered side of Friedman and Schwartz alike. The reply went beyond the subject matter of the original Lucia article to the matter of how it reached print, as the closing words of the reply blasted the journal editor for permitting the publication of Lucia (1985) without, apparently, having asked Friedman and Schwartz for their refereeing input.

Friedman and Schwartz also regarded Lucia as having had too ambitious an aim. In particular, they suggested that Lucia's archival work might have been the basis for "an interesting and informative article" revisiting the issue of the Bank of United States' soundness. Instead, Lucia had challenged the Friedman-Schwartz account of overall 1929–1933 monetary policy conduct: "apparently he could not resist the temptation to make a bigger splash in a bigger pond."

The last sentiment was one that did not reach print very often but was far from an uncommon reaction that economics professors—or, as in Friedman's case, former professors—at "top-20" U.S. universities had when other researchers, particularly those new in the profession, were challenging, or claiming that there were errors in, the established economists' prominent work.

[230] O'Brien (1992, p. 376) stated: "Friedman and Schwartz do not claim that Lucia has misrepresented their views on this issue [the Bank of United States]." In fact, they had complained that he provided a "caricature" of their views on the matter (Friedman and Schwartz, 1986b, p. 201).

[231] As discussed in Nelson (2020b, Chapter 12), one matter on which Friedman and Schwartz strongly criticized Lucia (1985) pertained to what they saw as the key failure of the Federal Reserve during the banking crisis. They took him as implying that they believed that discount-window lending to commercial banks was inadequate, while they maintained that their position was, instead, that enough reserves had not been created and that they had made no stipulation that the reserves necessarily had to be furnished via lending to banks. Despite Friedman and Schwartz's (1986b, p. 201) strongly reproachful tone on this matter, it is not surprising that many commentators interpreted the *Monetary History*'s account as having contended that discount-window lending should have been made in 1930–1931 on a much larger scale than it was. Friedman and Schwartz certainly were critical of the Federal Reserve's lending policy over the period, and Friedman's numerous capsule summaries of the central bank's policy approach over the early 1930s were certainly capable of being interpreted as focusing on the inadequacy of lending to commercial banks. For example, he stated that the Federal Reserve through 1931 failed "to exercise its responsibilities to provide liquidity" (*Boston Globe*, October 28, 1979, p. G2) and that the "rediscount mechanism... was not properly used in the early 1930s" (letter to Senator William Proxmire of September 13, 1968, in Joint Economic Committee, U.S. Congress, 1968b, p. 32).

Shortly before the appearance of Friedman and Schwartz (1986b), Michael Mussa (1986, p. 107) defined lending of last resort in a nonstandard way so that it included open market purchases, ahead of stating with regard to last-resort lending that "Milton Friedman and Anna Schwartz argue persuasively in [*A*] *Monetary History of the United States, 1867-1960* (1963) that the failure of the Federal Reserve to perform this vital function permitted the massive decline in money and credit between 1929 and 1933 that contributed importantly to the severity of the Great Depression." Mussa may have seen the in-press Friedman-Schwartz (1986b) reply.

Friedman was not the first or last high-profile economic researcher to suspect, when a new challenge to their work appeared, that the challenger saw the possibility of benefits accruing to their own career.

Joseph Lucia was, however, not a good example of a junior researcher seeking fame. He had had a low-key career, teaching at Villanova University in Pennsylvania since 1957. As it turned out, in the mid-1980s Lucia was near the end of his career. He died on April 8, 1988, age 55 (*Philadelphia Inquirer*, April 9, 1988).[232]

Anthony Patrick O'Brien of LeHigh University—believing strongly that Lucia's critique had merit, and dismayed by the tone of the Friedman-Schwartz reply—published a "Defense of Joseph Lucia" in the August 1992 issue of the *Journal of Money, Credit and Banking*.[233] A riposte to O'Brien's expansion of Lucia's 1985 critique appeared in the same *JMCB* issue. This rejoinder was written not by Friedman and Schwartz but by Paul Trescott of Southern Illinois University. Trescott, whose piece thanked Friedman and Schwartz for comments, had little good to say about the Friedman-Schwartz 1986 reply but nevertheless came out strongly in favor of the *Monetary History*'s account and against the Lucia critique, including its judgments about the Bank of United States.

In the opposite area of Friedman's work—his popular writings—the year 1983 had seen the final collection of his *Newsweek* columns, *Bright Promises, Dismal Performance: An Economist's Protest*.[234] In putting together this paperback compilation, William R. Allen included a few previously-reprinted columns that appeared in *Newsweek* in 1968 and 1972. But the collection consisted predominantly of Friedman columns that had not been in the previous collections and that appeared in *Newsweek* over the period from 1975 to 1982. Allen also included a couple of *Milton Friedman Speaks* talks given in the late 1970s. In doing so, Allen chose to draw upon shortened versions of the talks that were used as syndicated newspaper pieces (in, for example,

[232] Trescott (1992, p. 395) stated that the argument consisted of "an acrimonious exchange between Lucia and Friedman and Schwartz." Such wording might imply that there was a published reply by Lucia to Friedman and Schwartz's (1986b) trenchant rebuttal. In fact, as O'Brien's (1992) article stressed, Lucia's death foreclosed the possibility of such a reply.

[233] One of O'Brien's (1992, p. 376) most serious criticisms was that, in quoting a vintage passage that appeared in the newspaper *Commercial and Financial Chronicle*, Friedman and Schwartz (1963a, p. 310) had omitted—without providing ellipses—a paragraph of the passage that was unfavorable to their own interpretation of events. What O'Brien did not mention was that Friedman and Schwartz actually omitted two consecutive paragraphs—and one of the omitted paragraphs actually supported their account of events. The quotation in question was a late insertion into the *Monetary History*, not being in the 1961 draft, and the lack of ellipses may have reflected the last-minute nature of the addition.

[234] See Friedman (1983a).

Belleville Telescope (Kansas), November 30, 1978), rather than on the original lecture transcripts. This was a judicious choice, especially as it meant that, in its version of one lecture, *Bright Promises, Dismal Performance* omitted a gratuitous and tasteless reference that Friedman had made to Hitler.[235]

Allen remarked of the creation of the book from these materials: "Milton did not at all participate in that endeavor." (William R. Allen, personal communication, March 14, 2014.) Rather, "I had a free hand to put it together as I wanted. And he expressed gratification later that he thought it worked out very well." (William R. Allen, interview, March 14, 2014.) Although Paul Samuelson's *Newsweek* column had ended in 1981, the publisher of the *Bright Promises, Dismal Performance,* released a companion paperback, *Economics from the Heart*, made up of various Samuelson columns that had appeared in *Newswee*k from 1975 to 1981.[236] "I later heard that Mrs. Friedman was very pleased to learn that the Friedman book outsold a Samuelson collection put out by the same publisher," Allen recalled (personal communication, March 14, 2014).

Rose Friedman had, for some time, had her own solo-authored book project. A profile of her that appeared shortly before the Friedmans' move to California stated: "She is planning a book on the development of Milton Friedman's economic theories." (*Sunday Sun-Times* (Chicago), October 31, 1976.) In September 1982, Rose Friedman indicated that the book was still in the works, with Harcourt Brace Jovanovich the interested publisher. She now suggested that the book would be concerned more with the Friedmans' collaborations and personal experiences and would use her 1976–1977 series of articles on a base on which to build. Rose Friedman also indicated that a theme of the book would be Milton Friedman's battles in economic debates, in order to address the question: "Why does it take so long to persuade fellow economists?" (*The Times* (London), September 28, 1982.) These public discussions of the project notwithstanding, the planned book failed to appear.

Demise of incomes-policy prescriptions

One matter on which Friedman had very largely been able to persuade fellow economists concerned whether to rely on incomes policy, instead of monetary policy, as a device against inflation. As late as June 1981, Congress' Joint Economic Committee was devoting a hearing to the subject "Austrian Incomes Policy: Lesson for the United States." In contrast, by the end of 1986, calls for incomes policy—such as governmental national wage-price guidelines—had

[235] See Nelson (2020b, Chapter 14) for further discussion.
[236] See Samuelson (1983).

moved to the fringe of policy recommendations in the United States.[237]

As of early 1982, incomes policy was still a prominent part of the anti-inflation package advanced by veteran Keynesians. Charles Schultze stated at the start of the year (*The MacNeil/Lehrer News Hour*, PBS, January 1, 1982): "I would think we need to do something more directly to deal with business and labor, trying to get prices down directly through what's called an incomes policy, rather than doing it only through beating them over the head with tight money." Appearing before the Washington, D.C. press a few months later, Walter Heller urged that President Reagan should "stop hugging Milton Friedman" and embrace incomes policy—"a modest program of wage-price restraint" (*Washington Post*, March 25, 1982). A week earlier, at the Federal Reserve Bank of Atlanta conference also attended by Friedman, Lawrence R. Klein called for the introduction of an incomes policy, possibly of a tax-based form (*Atlanta Journal*, March 18, 1982, p. 14D).

Such calls did not altogether disappear as lower inflation rates prevailed during 1982 and 1983. "Every American administration from Kennedy to Carter inclusive, possibly excluding Ford, has felt the need to have some kind of wage-price policy," James Tobin remarked in October 1983, while implying that the need would soon be felt again (*New York Times*, October 12, 1983). It was increasingly the case, however, that such calls for a return to the incomes-policy approach of the 1960s and 1970s were against the tide of economists' opinion both in the United States and elsewhere. The anti-incomes policy majority opinion prevailing among economists was manifested in the ever more routine acceptance of monetary policy as the appropriate, and paramount, anti-inflation weapon. For example, in a guest *New York Times* op-ed (July 23, 1982)—one designed to explain the need to improve monetary control—Federal Reserve Bank of Cleveland economist John M. Davis began with the words: "Today, few deny that controlling the money supply could end inflation."

The change in the center of professional views was also poignantly brought out in the shift in position of one of Lawrence Klein's prominent students and collaborators, U.K. economist R.J. (James) Ball. Ball, head of the London Business School, was commissioned by the *Scandinavian Journal of Economics* to write an account of Klein's research and career after Klein won the economics Nobel in 1980 (see R.J. Ball, 1981). Ball was well suited to write the article, and he highlighted Klein's considerable achievements. Ball had, nevertheless parted

[237] The fading away of advocacy of incomes policy by Democratic politicians was noted in *The Plain Dealer* (Cleveland), May 19, 1983.

company since the 1960s with Klein on macroeconomic matters. As his other writings confirmed, Ball had undergone an intellectual change in favor of the monetarist position. In May 1962, Friedman, having been presented with suggestions that Ball write a book on inflation or on employment for the Cambridge economic handbooks that Friedman coedited, vetoed both suggestions. Friedman implied that, on the basis of his encounters with Ball when the latter was affiliated with Klein and the University of Pennsylvania, he would not like the economics of books that Ball produced on the subject.[238] In contrast, twenty years later, Ball produced a monograph for Macmillan titled *Money and Employment*. The book strongly reflected Ball's conversion to monetarism, and it included a rejection of cost-push views—with Ball disputing "the idea that trades unions can cause permanent shifts in the inflation rate."[239]

The decline in fortunes, among economic-research circles, experienced by incomes-policy ideas was also underlined by the events associated with a proposed prestige volume on the subject. In 1982, under president Victor Urquidi, the International Economic Association made tentative plans to have incomes policy as the subject of one of its periodic roundtable conferences, on the grounds that it "appeared timely" (Urquidi, 1989, p. xi). But little progress had been made in firming up the conference planning by the time Urquidi stepped down as the association's president in 1983. Attempts to schedule the conference in 1985 or 1986 then failed, and the idea of holding a conference was abandoned. The International Economic Association nevertheless agreed to publish a pseudo-conference volume on the topic. Urquidi, however, continued to have considerable problems obtaining papers for the volume, which finally appeared (under the title *Incomes Policies*) in 1989 (Urquidi, 1989, p. xii).

In policymaking, too, incomes policy was further losing support. Paul Volcker and the Reagan Administration were already on record by 1982 with adverse judgments on incomes policy as an option. But Anthony Solomon, president of the Federal Reserve Bank of New York and vice chair of the Federal Open Market Committee, had been part of the Carter Administration economic team through the early spring of 1980.[240] Although supportive of Paul Volcker in policy decisions, Solomon resisted giving up the cost-push views of inflation that he had espoused in his previous post. Reflecting this posture, the Federal Reserve Bank of New York's annual report for 1981 (issued in January 1982) stated that the FOMC's monetary targeting "makes no commitment to any particular theory of the underlying causes of inflation [or] to the

[238] Letter of Milton Friedman to C.W. Guillebaud, May 16, 1962, Milton Friedman papers, Hoover Institution, Box 27, Folder 30. Ball did, nonetheless, press ahead with producing a monograph, which—titled *Inflation and the Theory of Money*—would be issued by a different publisher (see R.J. Ball, 1964).
[239] R.J. Ball (1982, p. 69).
[240] See *https://www.newyorkfed.org/aboutthefed/ASolomonbio.html*.

possible need for other anti-inflation policies."[241] This passage was sufficiently supportive of nonmonetary perspectives on inflation that the Reserve Bank of New Zealand favorably quoted it in a publication in 1983—a year during which New Zealand had strict wage/price controls.[242]

It would turn out, however, that both Solomon and the Reserve Bank of New Zealand were not accurately characterizing the emerging consensus: thinking had moved much more in favor of directing the control of inflation exclusively to monetary policy than their statements suggested. Reinforcing these trends, Solomon left his Federal Reserve position at the end of 1984, and New Zealand abolished wage and price controls around the middle of the same year.

At the end of 1984, despite setbacks regarding other aspects of monetarism, Friedman was correspondingly able to record success by putting things into a longer perspective, telling the *Wall Street Journal* (December 10, 1984, p. 1): "It's widely accepted that you can't hold inflation down unless you hold down the money supply. Thirty years ago, people didn't agree with that."

Having—to Tobin's (1982) disapproval—criticized incomes policy in its first *Economic Report of the President* in 1982, the Reagan Administration continued to do so in the 1983 report under the CEA's Martin Feldstein. "The popular axiom that attributes inflation to 'too much money chasing too few goods' reflects a basic truth: it is difficult to imagine a sustained inflation that is not supported by excessive money growth," the report stated (Council of Economic Advisers, 1983, p. 20).

About a year later, Beryl Sprinkel of the Treasury noted in Congressional testimony: "A growing number of economists—and I think the public in general to an increasing extent—now recognize that inflation is fundamentally a monetary phenomenon. A necessary prerequisite for price stability is that money growth be constrained to a noninflationary pace over the long run."[243] Sprinkel contrasted this position with cost-push traditions. The appeal of these traditions lay partly in the fact that "[w]hen we focus on month-to-month movements in price indices, it is always possible to identify the particular items that contributed to a given rise in a composite price index."[244] But this approach had led to a misguided longer-run approach to anti-inflation

[241] Federal Reserve Bank of New York (1982, p. 4).
[242] See Deane, Nicholl, and Smith (1983, p. 234).
[243] Testimony of February 9, 1984, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1984a, p. 145).
[244] Testimony of February 9, 1984, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1984a, p. 146).

policy: “During the 1970s, the tendency to confuse relative price changes with inflation provided a series of anecdotal explanations or justifications for a rising inflation rate. Each short-run increase in the price indices was attributed to increased energy costs, increased food prices, rising wage rates or some other developments; such analysis implied, directly or indirectly, that the general rise in inflation was somehow beyond our control.”[245] In contrast, Sprinkel observed: “History has repeatedly demonstrated that inflation is not something that is imposed on us by mysterious and uncontrollable forces.”[246]

And with regard to the policy prescription that flowed from cost-push accounts of inflation, Paul Volcker had himself reaffirmed in mid-1983: “We and other industrialized countries have had little success in dealing with that threat [adverse wage-price behavior] through so-called ‘incomes policies.’”[247]

By the time of these 1983–1984 statements emanating from Volcker and the administration, 1982’s experience had provided very influential evidence on the cure for inflation. U.S. inflation had fallen very considerably in the wake of a protracted period of restrictive monetary policy. This outcome contrasted with the United States’ experience in the 1970s using various incomes-policy devices against inflation. And even as, from 1982 onward, monetary aggregates were cast into greater doubt as guides to U.S. monetary policy, the position that monetary policy could and should control inflation remained a widespread conviction in policy and research circles. The crystallization of the consensus that inflation was a monetary phenomenon helped drive incomes-policy approaches off the agenda in the United States, with even critics of the Reagan Administration unlikely to prescribe a revival of such approaches.

II. ISSUES IN DEBATES ON FISCAL POLICY, REGULATION, AND AGGREGATE SUPPLY, 1982–1986

FINANCIAL DEREGULATION

The term “deregulation” had become a standard one in U.S. policy discussions in the second half of the 1970s. But its most well-known applications through 1979 had been to nonfinancial

[245] Testimony of February 9, 1984, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1984a, pp. 146–147).
[246] Testimony of February 9, 1984, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1984a, p. 147).
[247] From Volcker’s testimony of July 20, 1983, in Committee on Banking, Finance and Urban Affairs, U.S. House of Representatives (1983c, p. 133).

economic activity, including air travel and trucking.[248] In the 1980s, this situation would change. “Financial deregulation” became a standard phrase in discussions of developments in the United States and abroad.[249] In the United States, the term particularly pertained to relaxation of controls on deposit interest rates, but it was also used in countries whose regulatory changes in the 1980s largely took other forms (see Germany and Morton, 1985). The term permeated economic research in the 1980s on banking, monetary policy, and their intersection, and it would be used by Robert Hall in his 1982 review of Friedman and Schwartz’s *Monetary Trends*.

That book review—although concerned with a study whose coverage ended in the mid-1970s—highlighted the “recent moves toward financial deregulation” (p. 1554) that had occurred so far in the 1980s. It did so in the context of offering a perspective that would recur in many subsequent discussions of Friedman and monetarism. Hall’s review implied that financial deregulation was something that Friedman favored—but was also something that was bad news for monetarism, as deregulation would, Hall contended, undermine the historical link between monetary aggregates and the economy.

In the period from 1982 to 1986, Friedman spent considerable effort resisting implications of the kind that Hall had made. Friedman argued that one could be—and make clear that he was—in favor of both financial deregulation and analyses centered on monetary aggregates. With regard to U.S. financial policy, he lamented the “slow progress of deregulation” once it was in motion (*Newsweek*, June 13, 1983). And with regard to monetary aggregates, he acknowledged problems in interpreting U.S. money series during the 1980s but suggested that they could have been avoided if financial deregulation had been implemented earlier.[250]

Friedman also contended that deregulation “came too late” to prevent a serious loss of market share by the country’s traditional depository institutions—which were the entities subject to the regulations but, he believed, had for a long time been parties interested in preserving the regulations.[251]

The end of Regulation Q’s time-deposit rate ceilings

By the time of the appearance of Hall’s (1982) review, financial deregulation had come to be

[248] For further discussion, see Chapter 9 above.

[249] It had already been used in a syndicated news column that appeared in 1978, but in that case had been used to describe financial innovation, rather than deregulation. (*Indiana Gazette* (Pennsylvania), October 27, 1978.)

[250] See especially Friedman (1982c, p. 108; 1985b, p. 59).

[251] See the discussion below. The quotation is from Friedman (1985e, p. 13).

closely associated in the United States with commercial bank deposit-rate deregulation. In particular, when an article in the *ABA Banking Journal* (May 1980) stated, "Far-reaching financial deregulation legislation is now a fact," it was referring to the passage of the Depository Institutions Deregulation and Monetary Control Act of 1980, which authorized deregulation of interest rates on many key deposits offered by commercial banks, by abolishing the ceilings on these rates embedded in the Federal Reserve Board's Regulation Q.[252]

The interest rates offered by commercial banks on their *wholesale* liabilities had either not been subject to a ceiling in the first place or had had limitations removed already by the first half of the 1970s. Most notably, the large time deposit (negotiable certificates of deposit) interest-rate ceiling had long been revoked in, beginning with the suspension of the limitation on short-term CD rates in June 1970 and with all CD rate ceilings removed in 1973 (see, for example, Huertas, 1983, p. 24; Burns, 1988, p. 22). Consequently, the deposit-rate deregulation allowed by the 1980 law—and the focus of the discussions in the 1980s of the implications of financial deregulation for U.S. monetary policy—referred primarily to commercial banks' *retail* deposit liabilities. In the 1970s, these had consisted of demand deposits and interest-bearing nontransactions bank deposits—the latter categorized either as savings deposits or small time deposits.

Although a great deal of discussion of financial deregulation in the early 1980s concerned banks' checkable deposits—specifically, the newly emerging "other checkable deposits" category—developments concerning checking deposits in the 1980s were largely separate from those relevant to the deposits whose rates were deregulated via the removal of much of Regulation Q. Rates on the checking deposits traditionally offered by banks—their demand-deposit liabilities—were not deregulated in the 1980s. Rather, the prohibition of interest on demand-deposit accounts continued. This prohibition had been legislated in 1933–1935 when Regulation Q was first set up.[253] It was, in Friedman's estimation, the only part of Regulation Q to have exerted a binding constraint on banks in the first twenty years or so after Regulation Q was introduced.[254] This part of Regulation Q remained after the deregulatory moves of the 1980s. Interest payments on households' demand deposits remained illegal until Regulation Q was fully abolished in 2011.

[252] The act would often be referred to simply as the Monetary Control Act, or MCA (see, for example, Feinman, 1993). Friedman and Friedman (1988, p. 465) erroneously stated that Regulation Q was abolished in the 1970s. In fact, it was being deliberated during 1979 but, by the end of the year, only the Senate had passed a bill that would authorize the phasing-out of most of Regulation Q (*The Sun* (Biloxi, Mississippi), December 24, 1979).
[253] See Friedman and Schwartz (1963a, p. 444), Cagan (1965, p. 123), and Ruebling (1970, pp. 30–31).
[254] Friedman (1970f, p. 16). See also Friedman and Schwartz (1963a, pp. 444–445).

"Other checkable deposits"—bank checking accounts, essentially emerging only in the 1980s, other than demand deposits, and largely comprising bank-issued NOW accounts—*were* permitted to pay interest. But this deposit category very likely had less importance in the story of monetary policy and financial deregulation in the 1980s than that ascribed to it at the time. The behavior of other checkable deposits was repeatedly invoked by commentators during the 1980s as a possible source of distortion to M1 and its velocity. As discussed in the previous chapter, however, although these accounts were interest-bearing, their presence in the modern definition of M1 did not revolutionize the character of M1 deposits. Banks nationwide were allowed, starting in 1981, to offer NOW accounts. But standard NOWs were not deregulated-rate accounts. They were subject to a low (5 percent) interest-rate ceiling. And the deregulated-rate Super NOW accounts issued from the start of 1983 likely offered interest rates that were probably much less competitive (*vis a vis* market interest rates) than was thought at the time.[255]

That left—as the retail bank deposits whose rates were free to move from the 1980s onward—what Friedman and most commentators had most often been referring to when discussing Regulation Q: ceilings on offered to U.S. customers on commercial bank retail non-transactions deposits (bank-issued time and savings deposits). The removal of the longstanding ceilings on these rates was a key piece of the 1980 legislation.

For these deposits, the 1980 legislation permitted elimination of rate ceilings on a phased basis, with ceilings eventually fully removed as of March 1986 (Gilbert, 1986, p. 22). It was this elimination of ceilings on the time and savings deposits—the series that, along with the money market funds (MMFs) discussed below, essentially made up the non-M1 component of the modern M2 aggregate—that was what Friedman highlighted in 1983 when referring to "the essential elimination of the Reg Q regulations."[256]

Policymakers' changed position on Regulation Q

A 1965 study of Regulation Q had stated: "The complete abolition of Regulation Q has not attracted many adherents."[257] Among the few advocates of abolition, Friedman was the most

[255] The flipside of this is that although Friedman was likely correct that the pre-1982 upward trend in M1 velocity reflected the noninterest-bearing feature of M1 balances, this feature was (contrary to what he had suggested in Ketchum and Strunk, 1965, pp. 119–120, and Friedman, 1980d, p. 82) not very importantly due to Regulation Q.

[256] From page 1 of his letter to David E. Lindsey, May 14, 1983 (Federal Reserve Board records).

[257] Ritter (1965, p. 33; p. 207 of 1968 reprint).

prominent.[258] His opposition to Regulation Q ceilings on rates offered on demand deposits and non-demand deposits alike was long on the record.[259] In May 1959, he had testified: "This is a species of price fixing and I think it has no place in our system—and that this power of the Federal Reserve ought to be abolished."[260] So when, twenty years later, Congress was negotiating legislation that would phase out the ceilings on rates on accounts other than demand deposits, Friedman urged that the demand-deposit rate ceiling of zero be abolished too, while with regard to the savings-and-time-deposit rate ceilings, he declared that they not be phased out, but instead be removed "flat out, overnight—that's the only way to do it." (*American Banker*, June 12, 1979, p. 3.)

As indicated above, the law that was passed instead had a six-year phase-in of the elimination of rate ceilings on nontransactions deposits. Friedman urged that this abolition be expedited (*Wall Street Journal*, January 30, 1981). In the event, although the full expiration of the positive rate ceilings in Regulation Q did not occur until 1986, much of the rate deregulation was indeed fast-tracked over the course of 1982 (Benston, 1983, pp. 38–39; *Newsweek*, June 13, 1983). This

[258] A list of analyses that appeared in the 1960s that criticized Regulation Q—not all of which, however, recommended its repeal—would be given in Cox (1969, p. 152) (although he on this occasion omitted any references to Friedman, whose 1959–1960 statements predated all those of those given in Cox's list). Anna Schwartz (1993, p. 209) also later remarked: "Another success for Milton's views was the abolition of interest rate ceilings in the 1980 Monetary Decontrol [sic] Act, although others including Tobin could equally claim to be its fathers." Schwartz's assessment was generous to Tobin, who actually had been one of the proponents of the view (later out of vogue) that interest-rate ceilings could be a powerful monetary policy tool. Indeed, Brunner (1970b, p. 2) had appropriately characterized Friedman (1970f) and Tobin (1970b) as reaching opposite policy conclusions on this matter.

Among monetarists other than Friedman, Meltzer (1967a, p. 499)—in the course of a discussion that mainly concerned other forms of regulation—suggested that the repeal of Regulation Q was desirable, while Clark Warburton (1964) actually *endorsed* Regulation Q's prohibition of interest payments on demand deposits. Warburton's support for Regulation Q, as far as its coverage of demand deposits was concerned, was one example of the judgment, made by a number of economists, that that there was a clearer case for applying rate ceilings to demand deposits than to time and savings deposits. This mixed position contrasted with, for example, Cox's (1969) stance that all the ceilings should be subject to the same analysis and to Friedman's recommendation that they all be repealed.

Relatedly, and as already indicated, much of the debate on Regulation Q centered on the prohibition of the payment of interest on demand deposits, and Friedman's own references to Regulation Q often focused on this subset of its prohibitions. Nevertheless, when he stated of the Federal Reserve's position about Regulation Q, "they administer [it] and have never considered repealing [it]" (*The News-Courier* (Charleston, South Carolina), October 26, 1982, p. 10), he was referring the whole of Regulation Q, as the Federal Reserve under Miller and Volcker had endorsed repealing the considerable part of Regulation Q that did not pertain to demand deposits.

[259] Ritter (1965, p. 32; p. 207 of 1968 reprint) did suggest that nongovernmental inquiries like the Commission on Money and Credit (1961, p. 168) had suggested that Regulation Q be invoked only in emergencies. But the Commission had, in fact, come out in favor of continuing the ban on the payment of interest on demand deposits: see Friedman (1962c, p. 295). And with regard to controls on rates on time and savings deposits, commercial banks, as discussed below, became more comfortable again with a permanently-in-force Regulation Q once it was turned into a control applying to retail deposits issued by both banks and thrifts, rather than one encompassing banks' wholesale deposit issuance and excluding thrifts' deposits.

[260] Testimony of May 30, 1959, in Joint Economic Committee, U.S. Congress (1959a, p. 633).

boosted the own-rate on M2 balances. The own-rate (the average nominal interest rate accruing to the holding of M2) had already moved much closer to market rates over the 1980s. It was made even closer to short-term open-market rates at the end of 1982 when, as noted in the previous chapter, commercial banks started issuing money market deposit accounts—highly competitive-rate instruments that were also, to a limited degree, checkable.

The Federal Reserve and deregulation

When signing the deregulation bill into law on March 31, 1980, President Carter emphasized that it repealed "a wide range of outdated, unfair, and unworkable regulations," in particular "interest-rate ceilings that prohibit small savers from receiving a fair market return on their deposits."[261] The president noted that his Secretary of the Treasury, G. William Miller, had been aiding the drafting of the interest-rate deregulation law, both in his current post and in his previous position of head of the Federal Reserve.

This was accurate: the Federal Reserve had supported the dismantling of the Q ceilings on small savings deposits. This represented a change from the William McChesney Martin era when the Federal Reserve had come to treat variations in rate ceilings as a monetary policy tool. Against this background, Friedman, just after Martin left office, published an extensive critique of Regulation Q, "Controls on Interest Rates Paid By Banks," that, among other things, deprecated the contribution that ceilings could make as a monetary-control tool.[262] He had, with Anna Schwartz, in the *Monetary History*, criticized as "largely erroneous" the original rationale for ceilings that had attributed the late-1920s boom to excessive competition among banks for funds and that had interpreted the early-1930s banking problems as stemming from earlier financial excess.[263]

In the early 1970s, the Federal Reserve had come around to the view that ceilings were not an effective control device. As the decade progressed, it had also come to share Friedman's position that the financial community, as well as its customers, would be better served by there being no Regulation Q ceilings on nontransactions deposits.

[261] See Carter (1980e).

[262] See Friedman (1970f).

[263] See Friedman and Schwartz (1963a, p. 444). By the 1970s, their doubt about this verdict on developments in the 1920s and 1930s was widely shared. See, for example, J.J. Holland (1975), as well as Mayer's (1982b, p. 122) observation that "the prohibition of interest payments on demand deposits was based on the mistaken notion that excess[ive] interest payments caused the bank failures of the 1930s."

Financial innovation and banks' interest in deregulation

Speaking in July 1981, Friedman took note of the belated character of the Federal Reserve's conversion to deregulation. "In recent years, but only in recent years, the Fed has favored eliminating the ceilings, at least for time and savings deposits," he observed. Friedman immediately added, however, that U.S. commercial bankers' support for these ceilings' elimination had likewise been a latter-day phenomenon.[264]

It was true that, once the commercial banks had been forced to compete on a major scale with open-market sources of credit, they had tried to work around Regulation Q's effects on their ability to offer attractive instruments. But before the wholesale/retail deposit distinction came to the fore in the 1960s, the banking community had, Friedman believed, taken a favorable view of the Regulation Q ceilings. Indeed, he had put a great deal of blame on the banking system for the existence of the ceilings and had, even in the 1970s, grave doubts about their consistency over time in supporting deposit-rate deregulation on the retail side. Friedman was a speaker at the Honolulu American Bankers Association conference in Honolulu on October 12, 1983, and found that the leading commercial banker who had introduced him had done so by praising the end of Regulation Q. "When I started my talk, I couldn't resist saying, 'Where do you think those restrictions came from? Who do you suppose was responsible for putting them into effect?' The answer, of course, is the commercial banking industry."[265]

With regard specifically to the ban on the payment of interest on demand deposits, Tullock (1983, p. 201) remarked: "I once heard Milton Friedman use it as an example of how business men who were in favor of free competition tend to look to government regulations to protect themselves, but this is the only public criticism I ever heard." Friedman had, in fact, made this point on the record on a number of other public occasions. He remarked in a magazine interview (*Dun's Review*, February 1968, p. 94) that the fact that demand deposits were a major item that they purchased for lending had led banks to "want" the prohibition of interest payments on those deposits. And he had earlier criticized 1962's private-sector-produced Report of the Commission on Money and Credit for coming out in favor of continuing the prohibition.[266]

Friedman granted that some leading commercial bankers were in favor of allowing interest payments on demand deposits. But he criticized the banking system for not being "really

[264] Friedman (1982c, p. 108).
[265] Friedman (1984a, p. 9).
[266] Friedman (1962c, p. 295).

uniformly opposed" to the removal of the zero-rate ceiling (*American Banker*, April 14, 1978).[267] As for the original introduction of the ban in the mid-1930s, he remarked that banks had "lobbied" for the prohibition of interest payments on demand deposits for years before the prohibition became law.[268]

Likewise, with regard to the nontransactions deposits that were the center of much of the debate on Regulation Q, Friedman and Schwartz noted in the *Monetary History*: "The limitation of rates of interest paid on time deposits… [was] initially welcomed by commercial banks."[269] Indeed, although President Carter described banks as "hampered" by the Q ceilings, the banking system had generally not seen things in this way for much of the period after ceilings on time and savings deposit rates were first instituted in 1933–1935.[270] A couple of months before Carter's remarks, Friedman had observed that the ceilings "were adopted only because the circumstances enabled commercial banks and other financial institutions to persuade Congress to grant them special privileges they had long sought."[271] He later remarked that, had he been in contention to be Federal Reserve chair, "I would be opposed by the banking industry of this country," on account of his being frequently opposed to commercial banks on policy matters (*Washington Times* (Washington, D.C.), May 2, 1983, p. 6B). The Regulation Q ceilings were a prominent example of the frictions between himself and bankers on policy issues.

Did the ceilings have a material effect? In 1982's *Monetary Trends*, Friedman and Schwartz treated, for the purpose of their econometric work, U.S. commercial banks as paying interest in kind in a manner that offset the effects of Regulation Q. This approach ran counter to Friedman's usual complaint about Regulation Q—that is, that it advantaged banks and disadvantaged bank customers—as well as his frequent charge about price controls that they did harm by sending the wrong price signals. It was clear, however, from Friedman's practical discussions that *Trends*' treatment of the Regulation Q ceilings as having been costly evaded was merely an approximation. Elsewhere, Friedman noted that, although there was considerable indirect payment of interest when the Regulation Q ceilings were reached, it remained the case

[267] This *American Banker* piece was quoting from a speech that Friedman had given (see *Milton Friedman Speaks*, Episode 15, "The Future of Our Free Society," taped February 21, 1978, p. 11 of transcript).

[268] Friedman (1984a, p. 13). Friedman had previously discussed the longstanding agitation of U.S. commercial banks for the prohibition of interest payments on demand deposits in the course of Congressional testimony that he gave in January 1976. See Committee on Banking, Currency and Housing, U.S. House of Representatives (1976a, p. 2176).

[269] Friedman and Schwartz (1963a, p. 445).

[270] For the quotation, see Carter (1980d). The timing of Carter's remarks was not auspicious, as the president and the Federal Reserve Board had actually recently imposed new constraints on banks and their customers, via the credit-control program (discussed in Chapter 12 above).

[271] Friedman (1980d, p. 82).

that the ceilings were "to some extent successful" in cartelizing the pricing of deposits: banks paid less, and customers received less, for the deposits than would otherwise have been the case (*Newsweek*, October 30, 1978). He acknowledged that *Trends*' treatment, in assuming the complete bypassing of Regulation Q, had overstated the own rate on commercial bank deposits.[272]

Banks, too, viewed Regulation Q as a material constraint on their freedom of action, and the reality of the constraint led them to turn against Regulation Q. The need to compete with the open-market bidders for funds in wholesale markets during the 1960s had then made banks resent the impediment that Regulation Q created to their large-scale deposit raising. Then, in the 1970s, the appearance on a large scale of money market funds (MMFs), also called money market mutual funds, as a competitor with depository institutions, made commercial banks seriously disadvantaged by Regulation Q when it came to competing for retail customers. The new round of disintermediation in the 1970s that resulted from MMFs taking market share, as Regulation Q made bank deposits a comparatively unattractive vehicle, led Friedman in early 1980 to provide a summary of the previous fifteen years: "In the 1960s and thereafter, the banks were hoisted on their own petard."[273]

Thrift institutions and Regulation Q

True, even before the advent of MMFs, banks had had a modicum of competition for their funds due to the presence of thrift institutions. It was this competition that Friedman and Schwartz—writing at a time when thrifts were not subject to Regulation Q—referred to when they noted that Regulation Q had "more recently been a hindrance," having commercial banks lose customers to savings banks and savings-and-loan (S&L) associations—which, initially, faced no ceilings on the rates that they could offer.[274]

Friedman dated the time at which Regulation Q started to lead banks to lose time deposit funds to 1954.[275] But this loss of market share was relieved by successive upward moves in the ceiling rates after 1956, so periods of losses to thrifts were limited stretches of time.[276] The banks' competition with thrifts was further restricted by the fact that U.S. regulators had informal rate

[272] Friedman letter to David E. Lindsey, May 14, 1983 (Federal Reserve Board records), p. 1.
[273] Friedman (1980d, p. 82).
[274] Friedman and Schwartz (1963a, p. 445). See also Friedman (1960, pp. 28–29).
[275] Friedman (1970f, p. 16).
[276] See Friedman (1970f, p. 16) and Ritter (1966, p. 13; p. 189 of 1968 reprint). The latter author, as well as Friedman and Schwartz (1970, p. 169), noted that maximum rates were first raised on January 1, 1957.

ceilings on thrift institutions in force through the mid-1960s (Tobin, 1970b, p. 4). Then in 1966, the thrift institutions were formally brought by legislation into the Regulation Q apparatus, being given higher rate ceilings than the banks. This formalization of the small spread of thrift deposit rates over commercial bank deposit rates contained the competition between the two types of depository institutions.[277]

Friedman would lament the fact that the thrift institutions, like the banks, supported Regulation Q, including after it was broadened to cover thrifts. Indeed, he cited this support as a historical contributor to the highly precarious condition that the thrift industry had reached at the end of the 1980s (*Dallas Morning News*, September 27, 1989). He could trace their problems far back in time because the thrift institutions had already reached a poor condition at the start of the 1980s. After a notable respite during the 1983–1986 period—when a major recovery in the U.S. housing sector and (generally) lower interest rates brought relief to the thrift institutions—their problems would culminate, during the late 1980s and early 1990s, in the S&L crisis, which is discussed in Chapter 16 below.

Friedman traced the S&L crisis to the financial and economic experience of the 1970s. One key aspect of this had to do not with Regulation Q, but with the assets side of their balance sheet and the inflation of the decade. The situation as of the early 1980s was summarized by Kane (1983, p. 173): "Holdings of low-interest mortgage instruments are the apparent source of ongoing weakness in S&L and MSB [mutual savings bank] balance sheets." The "low" interest instruments were fixed-rate mortgages that actually bore rates that, in many cases, seemed high in nominal terms when negotiated but that had proved to be low in relation to the 1970s' average rate of inflation. In the event, Friedman noted, the period had seen an inflation-driven bonanza for both governments and home-owners, both sets of actors paying negative real long-term interest rates.[278] In contrast, the thrift industry—being heavy housing lenders—had suffered from the real transfer of resources to their mortgage customers. In order to curb this process, Friedman wanted the thrift institutions to end their reliance on nominal-fixed-rate mortgages, his favored alternative being indexed loans (*Pittsburgh Post-Gazette*, October 28, 1980). He pointed blame at government regulation as an obstacle to this change (*Newsweek*, May 26, 1980). In the more deregulated environment of the 1980s, however, Friedman increasingly indicted the thrift industry for not treating a change in its business model as urgent once the housing market moved

[277] See Huertas, 1983, p. 25), as well as the discussion in Nelson (2020b, Chapter 12) of this episode.

[278] Friedman (1977g, p. 7). With regard to home-owners, see also *Milton Friedman Speaks*, Episode 6, "Money and Inflation," taped November 7, 1977 (p. 16 of transcript), Friedman and Friedman (1980, p. 272), and Friedman (1982a, p. 57; 1992c, pp. 216–217).

into a cyclical recovery (*Newsweek*, June 13, 1983).

On the liabilities side, by early 1976 Friedman had concluded that "Regulation Q was conceived as a measure that would favor thrift institutions but, in fact, has not had that effect." In particular, the intention "of protecting the thrift institutions against competition" was not realized in the 1970s because of the competition that sprang up from the nonbank sector.[279] In particular, the inroads that MMFs made into the customer base of both banks and thrifts intensified considerably during the new runup in short-term interest rates in 1977–1979—with MMFs providing a convenient means by which retail investors could access rates close to those prevailing on Treasury bills.[280]

In Friedman's evaluation, it was the appearance on the scene of MMFs—a development itself stemming from the underlying monetary regime—that created the decisive pressure for deregulation. "It took the severe inflation of the 1970s and accompanying double-digit interest rates—combined with the enforcement of Regulation Q—to produce money market mutual funds and thereby force a considerable measure of deregulation of banking," he observed.[281] The pressure that MMFs put on depository institutions had, he noted, already prompted the authorities to raise the Regulation Q ceilings during the 1970s.[282] Even so, in the October 1979 monetary policy tightening, short-term market interest rates rose to levels that far exceeded the ceilings (*American Banker*, November 13, 1979; Huertas, 1983, p. 25).

Money market funds and financial innovation

As indicated, the MMFs invested primarily in short-term Treasury securities and, as liability counterparts to these asset holdings, issued claims in the form of small-denomination "shares" that could be purchased by U.S. households. MMFs—being subject to neither reserve requirements nor Regulation Q—offered their customers yields much closer to market rates than what banks typically provided to their retail depositors. In principle, households could not redeem MMF shares without incurring any change in the market value of their investment.[283] In practice, however, households' MMF assets were perceived as highly redeemable at face value

[279] From Friedman's testimony of January 22, 1976, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1976a, p. 2176).
[280] One sign of the strain that the MMF challenge was posing on thrifts' ability to finance their lending was that they emulated the banks in tapping the wholesale market, issuing large CDs and nondeposit liabilities. See Friedman (1980d, p. 82) and Kane (1983, p. 180).
[281] Friedman (1984b, p. 52).
[282] Friedman (1985e, p. 13).
[283] See, for example, Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1980c, pp. 337, 390).

(see R.G. Anderson and K.A. Kavajecz, 1994, p. 3).[284] The perception of them as being money-like on this dimension led to retail MMF shares being included alongside banks' time deposits when M2's official definition was broadened in 1980.

Indeed, U.S. regulators during the 1970s had made it more difficult for bank customers to redeem time deposits ahead of maturity without occurring a penalty. This reduced the distinction between MMFs and time deposits in terms of their comparability with transactions deposits. It also made U.S. households more inclined to judge between the two assets in terms of their yield—a comparison that favored MMFs over banks. The outflow of funds from depository institutions to MMFs was, in Friedman's estimation and that of Barro (1984, p. 410), what made the banks and thrifts change their tune about the merit of keeping Regulation Q's limitations on time and savings deposit rates.

In terms of the technical case for including MMF liabilities in a revised M2 definition, Friedman backhandedly supported it: "There wouldn't have been any money market funds to mess up the definition of money if it had not been for Regulation Q." (*The News and Courier* (Charleston, South Carolina), October 26, 1982, p. 10.)[285]

In terms of their merits as a financial instrument, Friedman saw money market funds as providing a valuable service: he welcomed them as constituting an advance that "enable[d] small savers to benefit from high market interest rates."[286] In their *Tyranny of the Status Quo* book, the Friedmans applauded the fact that MMFs had served as a bridge—in effect, allowing the general public to move *en masse* into the holding of interest-bearing securities that were routinely traded on a daily basis in organized U.S. financial markets. MMF shares were a better deal for savers than nonmarketable savings bonds, whose low interest rates Friedman had also lamented, and the Friedmans also suggested that MMFs' prevalence would "reduce government's incentive to resort to inflation."[287] They evidently anticipated that the U.S. government would now mostly be

[284] This constituted, of course, a difference from the actual short-term securities themselves. Friedman had regarded the non-redeemability of securities at their face value before the maturity date as one reason for excluding them from monetary aggregates (see, for example, Friedman and Schwartz, 1970, p. 148, and the discussion in Nelson, 2020a, Chapter 6).

[285] These remarks were made at a point—the fall of 1982—when Friedman gave notable weight to M2 in his analysis but less than that he assigned M1. They preceded the roughly two-and-a-half-year period from late 1983 onward when he was temporarily dismissive of the modern M2 series. See the previous chapter for details.

[286] Friedman (1985e, p. 13). He had expressed a similar sentiment in Friedman (1982a, p. 56).

[287] Similarly, in *Financial Times* (London), February 23, 1987, Friedman remarked that institutional changes had "taken the profit out of inflation." As has been stressed in other chapters of this book, remarks like this, while technically accurate, tended to imply a conscious desire to inflate for revenue-raising purposes that did not really characterize actual past U.S. economic policy.

seeking funds in the short-term securities market—as MMFs would shift the supply of funds to that market—and that the government would also expect that *ex post* negative real rates were now unlikely to recur and so was even more likely to gravitate toward short-term borrowing in order to avoid paying a term premium.[288] In the event, although real interest rates were indeed positive in the 1980s across the maturity spectrum, and long-term real rates exceeded short-term rates, the market for long-term U.S. government securities thrived after 1982. In particular, contrary to Friedman's anticipation in the mid-1980s, the large increase in federal debt in the Reagan years was associated with a considerable expansion of longer-term U.S. government borrowing and a lengthening of the maturity of public debt.[289]

Friedman was nevertheless highly ambivalent about money market funds, owing to the manner in which they had arisen as major savings vehicles in the United States. Speaking of them at the start of the decade, he observed: "Benjamin Friedman [whose paper had led off the discussion] emphasizes the extent to which these and other financial innovations contributed to market efficiency. My own reaction is quite different: what a waste of capital and human ingenuity simply to get around restrictions and regulations that should never have been imposed!"[290] Milton Friedman liked the services that MMFs had provided to customers but not the reason—Regulation Q, along with inflation—why MMFs thrived in the first place.

This evaluation of MMFs—saying customers were better off having them as a vehicle in which to place their funds, but that the institutional structure should not have created a need for them—had parallels with Friedman's earlier reaction to U.S. financial innovations. It was much like how he had looked upon commercial banks' late-1960s innovations in the wholesale deposit market, including the expansion of Eurodollar issuance, in response to Regulation Q. It also paralleled his reaction to innovations associated with avoidance of direct credit controls.

Friedman's attitude contrasted, however, with what might have been expected from such an enthusiast for free-market innovation. The matter boiled down to the fact that Friedman was enthusiastic about innovation when it filled a gap that would exist in a free market for credit. His enthusiasm for futures markets in domestic securities and foreign exchange was a prime example of this perspective. But Friedman viewed financial innovation as being wasteful when it simply arose as a means of bypassing regulations like Regulation Q that should not have been instituted in the first place. Consequently, he thought that much of the major financial innovation that

[288] Friedman and Friedman (1984, p. 101; 1985, p. 99).
[289] For further discussion, see Chapter 17 below.
[290] Friedman (1980d, p. 82).

occurred in the United States during the 1960s and 1970s involved waste, as so much of it was undertaken with the aim of evading government controls—in particular, monetary and financial controls like reserve requirements and rate ceilings.[291] These were controls that were unnecessary in Friedman's view, because monetary control could and should be delivered by the more market-compatible device of open market operations.

In this connection, Friedman had testified in 1976: "Innovation is highly desirable when it is a constructive response to basic economic needs and opportunities. But it is not desirable when it is simply a way to get around unnecessary and undesirable government controls."[292]

With regard to MMFs specifically, Friedman and Schwartz—in a 1986 *Journal of Monetary Economics* piece that is considered further below—observed: "The money market funds performed a valuable social function. Yet, from a broader perspective, their invention constituted social waste."[293] Friedman believed that, absent Regulation Q, innovations like the development of the services that money market funds provided would have taken place within the banking system.[294] He further believed that commercial banks could, had they been allowed to do so, have provided these services more efficiently.[295] This was part of his overall judgment that "the financial structure would have developed in a more rational and efficient way" in the absence of Regulation Q (*Newsweek*, October 30, 1978).[296]

Friedman's concerns about resources having been wasted in the course of regulation-avoiding financial innovation extended beyond the financial sector, as he felt that other sectors could have benefited from the know-how that instead went to regulatory avoidance in the financial sector. "A lot of resources, a lot of entrepreneurial capacity, was devoted to developing money market mutuals," he observed in October 1983. "We've got a lot of entrepreneurial ability and intelligence in this country. The more of it that is devoted to finding ways around governmental regulations and controls, the less of it is available for the really effective purpose of promoting the well-being of the country—of improving our standard of life."[297]

[291] See Instructional Dynamics Economics Cassette Tape 38, November 10, 1969), Friedman (1971c, p. 24), and the discussion in Nelson (2020b, Chapter 14).
[292] From Friedman's testimony of January 22, 1976, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1976a, p. 2153).
[293] Friedman and Schwartz (1986a, p. 39). Friedman (1984a, p. 9) had earlier expressed the same judgment more pithily: "for the country as a whole[,] the development of money market mutuals was a complete waste."
[294] Friedman and Tobin (1990, p. 76; p. 7 of reprint).
[295] See Friedman's testimony of January 22, 1976, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1976a, p. 2153).
[296] See also the similar statement (Friedman, 1978e, p. 282) in his Congressional submission of August 21, 1978.
[297] Friedman (1984a, p. 13).

Friedman reaffirmed in the same remarks that “given those government regulations, it’s desirable” that innovations developed that reduced the cost of regulation to the consumer.[298] That is, conditional on the existence of regulation, it was a good thing that the innovation occurred. And similarly, he indicated that conditional on the fact that the regulation existed, it was desirable that financial innovation and, in particular, MMFs emerged to force financial deregulation, including the removal of much of Regulation Q.[299] Deregulation represented a change for the better, he explained in late July 1986: “Until recently, banking was a highly monopolistic industry in which banks were sheltered from competition by government regulation.” (*Los Angeles Times*, July 27, 1986, Part 1, p. 20.) “Deregulation is good,” Friedman had declared earlier in the month. “That is the one good thing—the only good thing, in my opinion—that has come out of this [the inflation-disinflation process].”[300]

Deregulation and monetary control

One of the likely reasons why Friedman felt he had to spell out his approval of deregulation so starkly was that, by 1986, the perception was widespread that financial deregulation had made it far more difficult for U.S. monetary policy to be focused on monetary aggregates.

This perception had two facets. One of them concerned whether monetary aggregates and the economy would have a reliable relationship in a deregulated environment. The other facet centered on whether central banks’ scope to control monetary aggregates was undermined by deregulation.

On both counts, Friedman was confident. He believed strongly that both the money/income relationship and central-bank monetary control would be resilient under deregulation.

The first facet—pertaining to the money/income relationship in the 1980s—was already discussed at length in the previous chapter and so can be covered briefly here. Friedman in the 1970s had supported, via his participation in the Bach Committee, the broadening of the monetary-aggregate definitions that recognized the emergence of M1-type and M2-type assets outside the existing measures of those series. His subsequent commentaries indicated that he felt that the process of producing new definitions of money had been largely successful in appropriately broadening the series.

[298] Friedman (1984a, p. 13).
[299] Friedman (1985b, p. 59).
[300] In Darby and others (1987, p. 10).

There was some opposition to this view expressed in policy circles. One view, widely held, was that deregulation had permanently blurred the definition of money. One variant of this view was that trying to obtain a better monetary definition of money was largely pointless because it was regulation that had made monetary aggregates a clear-cut concept in the first place. For example, at a conference that Friedman attended, Federal Reserve Bank of San Francisco president John Balles suggested: "Regulation Q ceilings meant that assets in M2 were clearly differentiated from other financial assets. As a result, there was a well-defined demand for M2…"[301]

In opposition to this interpretation, Friedman had contended for years that deposit interest-rate ceilings had, on balance, *worsened* money/income relations by encouraging lurches between different categories of bank deposit and promoting the creation of deposit-like instruments, issued by banks and their competitors, that were not included in the standard monetary aggregates.[302] He had often noted that the money/income relationship prevailed under a variety of financial arrangements. Some support for Friedman's position that the link between money and income could persevere in a deregulated-deposit-rate environment came in the fact that M2 velocity was stable, on net, in the decade after the effective deregulation of deposit rates after 1982.

On the matter of monetary control, the central question raised was whether M1 and M2 remained reliably related to open market operations in a deregulated environment. Federal Reserve Bank of Boston president Frank Morris (1982a, p. 82), for example, claimed that M1 was "the one thing we are positioned to control through bank reserves" but implied that this is because of the existence of reserve requirements on M1 deposits. The 1980 act discussed above had reflected the view that reserve requirements played an important role in monetary control, as it extended reserve requirements to cover thrift institutions as well as commercial banks that were not linked

[301] Balles (1983b p. 19).

[302] See Friedman (1970f p. 21, 1982c, p. 107), Friedman and Schwartz (1970, p. 145), and *Newsweek*, October 30, 1978. Not all of Friedman's contentions in this area panned out. For example, he was confident that the M1 and M2 aggregates would move together (that is, have very highly correlated growth rates) if Regulation Q ceilings on demand and nontransactions deposit rates were removed. See, for example, his statement in *Newsweek*, March 1, 1971: "Abolition of Regulation Q would make *either* M1 or M2 a satisfactory measure of the ease or tightness of money[,] and *both* would tell the same story." See also Friedman's testimony of March 3, 1964, in Committee on Banking and Currency, U.S. House of Representatives (1964, pp. 1141), his later related submission (Friedman, 1964c, p. 1220), and his remarks in Friedman and Heller (1969, pp. 76–77). A late assertion that he made in this vein was Friedman (1984b, p. 36). Instead, however, M1 growth and M2 growth had recurring differences: see Nelson (2020b, Chapter 14) for a discussion. As the discussion in the previous chapter implied, part of Friedman's error here lay in supposing that M1 deposits would bear near-market rates in a deregulated environment.

into the Federal Reserve apparatus.[303]

In contrast, Friedman's longstanding position was that the Federal Reserve's ability to control monetary aggregates did not rest on the existence of reserve requirements. His contention was that open market operations were the key control instrument. And he insisted that the money-multiplier mechanism did not rest on the existence of reserve requirements or on other regulations.[304] If reserve requirements were to be imposed, he was in favor of them being uniform across the deposits in the aggregate targeted.[305] But—short of a fundamental change to a 100 percent reserves system—he did not think that they should be imposed. And he believed that monetary control could be secured in a zero-reserve-requirement world, thanks to the emergence of a voluntary demand for reserves on the part of commercial banks—this demand arising from the need to engage in interbank transactions and related prudential considerations. Consequently, he argued that the "control of monetary aggregates…. does not depend on required reserves but is maintained equally by the prudential reserves that banks feel it desirable to keep."[306]

This position made Friedman confident about the feasibility of monetary control in a deregulated environment, including one in which depository institutions were not subject to reserve requirements. His stand was at odds with a number of views that were prevalent when the era of financial deregulation began. Representative of those views was a 1975 statement by then-Federal Reserve Board governor Robert Holland. R. Holland (1975, p. 170) had criticized "academic literature that argues that monetary reserve requirements are inefficient and unnecessary," as Holland doubted "that banks and other financial intermediaries will always want to hold some kind of central bank liability." Holland concluded: "Explicit monetary reserve requirements seem to be the most dependable means of providing that essential

[303] The act, however, did legislate substantial reductions in *average* reserve requirements imposed by the Federal Reserve on depository institutions. These reductions began almost immediately (see Levin and Meulendyke, 1980, p. 402).

[304] This contention did not amount to a denial that some regulatory measures (such as those concerning bank capital) could affect the money stock when they were varied (see Nelson, 2020a, Chapter 2, for a discussion). It instead amounted to a belief in what Friedman (1960) called the sufficiency of open market operations as a monetary policy tool.

Friedman's association with another position advocated in Friedman (1960) and elsewhere—100 percent reserves—likely encouraged the view that he was predisposed toward favoring quantitative or other direct monetary controls. In an encounter with Friedman at the May 1985 Bank of Japan conference, Hong Kong's venerable monetary economist S.C. Tsiang expected to find Friedman in agreement with him on the need for direct limits—that is, beyond open market operations—on bank deposit expansion and was surprised to find Friedman denying the need for such controls (Institute of Economics, 1986, p. 249).

[305] See, for example, Friedman's testimony of October 30, 1959, in Joint Economic Committee, U.S. Congress (1959b, p. 3053), his written statement of August 21, 1978 (Friedman, 1978e, p. 281), and Friedman (1982c, p. 117).

[306] From his statement of August 21, 1978 (Friedman, 1978e, p. 280).

ingredient for monetary control." At the time, this was a standard position in policy circles in the United States and many other countries.[307] The same reasoning had been articulated by Friedman's former student Phillip Cagan, who wanted reserve requirements imposed on nonbank issuers of transactions deposits (see Cagan, 1979).[308]

Although Cagan was on the alarmist end of the spectrum with regard to the implications of financial deregulation and innovation for monetary control, the judgment that reserve requirements were needed for monetary control was still quite prevalent at the start of the 1980s. In time, the Federal Reserve itself would indeed come around to Friedman's position that reserve requirements were not essential to monetary control and started to welcome a zero-reserve-requirement regime.[309] But a decade earlier, the contrary position was still being endorsed by U.S. officialdom, including through the 1980 act.[310]

Friedman, in contrast, had pressed the contrary view to Federal Reserve officials for decades. In early 1961, he wrote to his former student Stephen Axilrod, who by then had been a Federal Reserve Board economist for nearly a decade, that banks would have a precautionary demand for reserves even if there were no required reserves.[311] And in 1983, in a letter to another former student turned Board official, David Lindsey, Friedman disputed Lindsey's implication that, because M1 were subject to higher reserve requirements than non-M1 balances, monetary policy actions were necessarily more closely related to M1 than to M2. Friedman argued instead that M2 might have a tighter link to Federal Reserve actions than M1, both under the current reserve-

[307] For example (in a paper that did not mention Friedman, but whose title and contents indicated that it was written partly in response to Friedman, 1971c), Crockett (1976, p. 380) stated that it was "the existence of reserve requirements that makes [commercial banks'] credit creation conform to a multiplier framework."
[308] Woodford (1997, p. 6) stated: "The quantity-theoretic approach to price-level determination is sometimes used to argue that unchecked financial innovation and deregulation are likely to result in macroeconomic instability, or a loss of control over aggregate spending and hence inflation on the part of the central bank." Woodford accurately named Phillip Cagan as a proponent of this position. But it should be emphasized that, as stressed here and in Nelson (2020a, Chapter 2; 2020b, Chapter 14), Friedman explicitly took the opposite position and did not believe that regulation was needed for the central bank to influence monetary conditions, aggregate spending, and inflation.
[309] The "monetary control" that the Federal Reserve often focused on in this later period was the ability to generate a desired value of short-term market interest rates. But the same arguments that implied interest-rate control in a low-reserve-requirement world also implied influence on monetary aggregates. And the Federal Reserve had implicitly granted that monetary-aggregate control did not require reserve requirements in 1991, when it simultaneously pursued M2 growth targets while ending reserve requirements on non-M1 M2 deposits.
[310] In addition to viewing ordinary reserve requirements as necessary for the control of growth of M1 and M2, the Federal Reserve in the early Volcker years attached importance to marginal reserve requirements as a means of controlling wholesale deposits and bank credit. This matter was discussed in Chapters 10 and 12 above.
[311] Letter of Milton Friedman to Stephen H. Axilrod, January 10, 1961 (p. 2), Milton Friedman Papers, Hoover Institution, Box 19, Folder 32.

requirement setup and under zero required reserves.[312] At the time of this 1983 correspondence, Friedman actually favored M1 over M2. But his view that M2 was sensitive to monetary policy actions stood him in good stead when, in 1986, he returned to concentrating on M2.

As it happened, although M2 growth and monetary base growth often showed quite different growth rates during the 1980s, it was still the case that the M2 multiplier was reasonably statistically predictable and also that M2 movements were still largely understandable in terms of movements in the monetary base (see Rasche and Johannes, 1987, p. 187; Burger, 1988, pp. 52–53, 58; Pecchenino and Rasche, 1990; and the discussion in the previous chapter). And the Federal Reserve would later itself be converted to the view that reserve requirements were not necessary for monetary control.

Friedman's view that monetary control was feasible in a deregulated environment reflected his position that the working of monetary policy rested not on the presence of regulation but on voluntary private-sector behavior, associated with the desire by private-sector financial market institutions desire to hold reserves and the changes in incentives (regarding portfolio choice) induced by central bank actions in the market for reserves. Near the end of the Volcker era, he summed up the situation as follows: "The government determines the quantity of its high-powered money and thereby the raw material available to the entire financial market." (*Wall Street Journal*, July 2, 1987.)[313]

Monetary rules in a deregulated financial environment

Friedman's position that monetary control in a deregulated environment was feasible, including under no reserve requirements, was something that would win the day, despite the opposite view being prevalent in policy circles, and among monetary economists, well into the 1980s. The same was not true of his constant-monetary-growth rule—which would lose support over the 1980s even as Friedman continued to recommend it. The rule already had considerable

[312] Letter from Milton Friedman to David E. Lindsey, October 29, 1982 (p. 1), Federal Reserve Board records. Consistent with this position, Friedman acknowledged many times that reserve requirements were higher in the United States on demand deposits than time deposits (see, for example, Friedman and Schwartz, 1970, p. 121) but on occasion implied that Federal Reserve actions might have a closer link to M2 behavior than to M1 behavior (see, for example, his testimony of January 22, 1976, in Committee on Banking, Currency and Housing, U.S. House of Representatives, 1976, p. 2276).

[313] Tobin (1986) also expressed optimism about the scope for the central bank to influence monetary conditions in a deregulated environment, as he felt that the associated financial system would still meet the "reserve test" (p. 190) that institutions cleared their payments through the central bank. As a precaution, however, Tobin (1986, pp. 193–195) recommended placing a small reserve requirement on the liabilities of all depository institutions.

opposition among researchers and had been rejected by policymakers, but it would further lose adherents over the 1980s as part of the general disaffection with monetarism and monetary aggregates.

By May 1985, Friedman was starting to accept that the high inflation of the 1970s had been lastingly ended. But he felt that the inflation experience had had the effect of "reinforcing and giving greater credence to the conclusions about prior policy that various scholars had reached, including Anna Schwartz and myself in our *Monetary History*."[314] When it came, however, to Friedman and Schwartz's conclusions specifically about *monetary aggregates*, these 1985 remarks overstated the profession's feeling on whether recent years, particularly those since 1982, had given "greater credence" to the *Monetary History*.

In particular, the policy that was only lightly touched on in the *Monetary History* but that was strongly prescribed by Friedman in his other writings—constant monetary growth—would come under intensified criticism. Friedman felt in mid-1985 that there was still considerable "interest in monetary reform" as a reaction to the 1970s experience.[315] But, as discussed in the previous chapter, even he acknowledged that monetarist rules were off the national policy agenda for the foreseeable future. Events after mid-1985 would reinforce this situation, as the behavior of M1 in 1985 and 1986 gave round to a new spate of obituaries for monetarism in the media.[316] There were some holdouts in the press, most notably the *St. Louis Globe-Democrat*, which published an editorial (August 24, 1985) titled "Why Not Try Friedman's Theory?"—"Friedman's theory" here meaning his constant-monetary-growth rule.

Such shows of support, however, ran counter to the overwhelming tendency among media observers at the time—which was to interpret events as having highlighted the shortcomings of money-oriented policy prescriptions and to take actual monetary policy as having produced much better outcomes than what would have been resulted from monetarist prescriptions. Indeed, roughly three years into the U.S. economic expansion, the London *Financial Times* (November 8, 1985) editorialized that Paul Volcker had pursued "old-fashioned discretionary monetary policy" since September 1982. This way of describing Volcker's policy overstated the extent to which he had moved back to prior practice: as discussed in the previous chapter, much economic research has judged that the Volcker Federal Reserve, even after its abandonment of the New Operating Procedures in 1982, was much more oriented toward inflation control than

[314] Friedman (1985e, p. 12).
[315] Friedman (1985e, p. 12).
[316] For examples, again see the previous chapter.

was Federal Reserve interest-rate policy had been in the 1970s. But Volcker himself was outspoken during his second term about his rejection of monetary-growth rules.[317]

The rule remained a mainstay in research in monetary economics, with a celebrated paper by Rogoff (1985), a theoretical study but practically oriented, referring to the "*k* percent rule" (pp. 1184, 1188). But discussions among monetary economists oriented toward current policy were less favorable about the rule. Robert Gordon had always been on the Keynesian side of the Keynesian-monetarist debate. Nevertheless. he had also been seen as a bridge-builder between the groups. In Congressional testimony delivered in late 1982, however, Gordon denounced monetarism, which he blamed for the severity of the recession. In this testimony—which was taking place a decade after he had served in an arbitrating role in a debate between Friedman and his critics—Gordon was more adversarial toward Friedman than anyone in the 1972 debate had been. He spoke caustically about monetarism and its influence on policy, describing the Federal Reserve as having a "slavish pursuit of monetarist dogma" and contending: "The doctrine known as monetarism is directly responsible for the unemployment of at least 4 million individuals in the Western world."[318] The following year, Gordon was quoted saying, "I think monetarism has been decimated by the collapse of velocity in 1982" (*Business Week*, April 4, 1983, p. 64) and, in another 1983 remark, he referred to recent events as having seen "the velocity recession and the demise of monetarism" (quoted in T.E. Hall and N.R. Noble, 1987, p. 112).

In the presence of the heightened doubts during the 1980s about the money/income relationship, some monetarists highlighted the fact that the constant-monetary-growth rule had been advanced in part in recognition of uncertainty about the relationship. After his departure from the CEA, William Poole decried the downgrading of monetary targets, observing (*American Banker*, August 16, 1985): "The system of monetary targeting arose in the first place through the recognition that a policy of fine-tuning money growth had so often gone astray." Karl Brunner and Allan Meltzer, who had focused on the model-uncertainty argument in many of their writings in favor of a constant-monetary-growth rule, likewise suggested that the recent instability of velocity "strengthens the case for stable, predictable policies." (*Wall Street*

[317] See the previous chapter for examples of the relevant Volcker quotations.

[318] Testimony of December 1, 1982, in Committee on Banking, Finance, and Urban Affairs, U.S. House of Representatives (1983d, p. 76). Having received from Gordon a copy of the written submission that Gordon included with his testimony, Friedman threw out the copy after reading the first paragraph and wrote indignantly to Gordon. Predictably, Friedman's letter particularly objected to Gordon's association of Federal Reserve policy, as executed in recent years, with monetarism (letter from Friedman to Robert J. Gordon, December 27, 1982, Milton Friedman Papers, Hoover Institution, Box 149, Folder 8).

Journal, December 18, 1985*b*.)[319] Friedman himself observed: "It's more important to keep the authorities from doing harm than to try to get them to do good, because they simply do not know enough to do good." (*San Francisco Chronicle*, November 12, 1986).

But the instability in velocity in the 1980s did not, in practice, boost support in the United States for a policy of constant monetary growth. There were a couple of particularly important reasons for this result. First, although velocity was unstable, its direction (generally declining, especially in relation to past trends) in 1982–1983 and 1985–1986 was known. Furthermore, it was regarded by many as an obvious fact that a monetary-growth overshoot was the appropriate outcome in these circumstances.[320] And policymakers, for their part, were confident that they could do better than a strategy of trying to keep monetary growth constant.

A second reason why instability in velocity did not boost support for stability in monetary growth was that the definition of constancy in monetary growth was rendered ambiguous by the continuing tendency for different monetary aggregates to send conflicting signals. In particular, the prescription of constant monetary growth was complicated in the 1980s by the ongoing sharp discrepancies between M1 and M2 growth.

This problem was compounded by different prior views across economists about the appropriate definition of money. Many U.S. monetary economists remained strongly predisposed toward a medium-of-exchange concept of money and continued to prefer an M1-type aggregate whose deposit component was unambiguously checkable. For a good number of these economists, the message of the 1980s was not—as Friedman had believed from May 1986 onward—that M2 should supersede M1 as the definition of money, but that monetary velocity had become highly variable.[321] For some of them, this meant discarding monetary aggregates.

[319] Other work of Brunner and Meltzer that appeared over these years emphasizing the model-uncertainty argument for a constant-monetary-growth rule included Brunner (1983) and Brunner and Meltzer (1983a). As indicated in the previous chapter, Laidler (1983) likewise suggested that monetary targeting might remain the best policy in the face of uncertainty about velocity. So did Mishkin (1983, pp. 131–132).

[320] For example, Alan Blinder stated: "If the Fed returns to *M*-fetishism, look out. For as long as velocity keeps declining, seemingly high money growth rates are not only appropriate, but actually essential if recession is to be avoided." (*Washington Post*, August 10, 1983.)

[321] In the policy arena, of course, M1 had had pride of place in Federal Reserve monetary targeting through 1982, despite M2 also having its own announced target range. As indicated in the previous chapter, the FOMC put increased emphasis on M2 briefly in 1982, in response to financial changes affecting M1, but it soon let M2 recede in policy decisions, and this state of affairs remained for the rest of the Volcker tenure.

Within the Federal Reserve System, the Federal Reserve Bank of St. Louis also tended to be an advocate of M1. Its interest in the monetary base was mainly with a view to it being an instrument for controlling M1. Particularly until the mid-1980s, the bank also tended to be very critical of M2. For example, the bank's president, Lawrence K. Roos, stated at the March 1982 FOMC meeting that "M2 is a much inferior predictor of economic activity than M1" (Federal Open Market Committee, 1982e, p. 48). After stepping down as president, Roos called for the FOMC to

A number of prominent monetarists likewise did not follow Friedman in embracing the modern M2 series. Phillip Cagan, despite using an M2 series in his work at the NBER with Friedman and Schwartz, had become a strong partisan for M1. "Monetary policy cannot sensibly avoid attention to transactions balances," Cagan (1984, p. 48) contended. Cagan remained focused on M1 after Friedman became enamored with the modern M2 aggregate.[322]

A possible means of achieving greater common ground among monetarists was to focus on the monetary base. And monetarists' interest in the base as a target in its own right did increase in this period. From the mid-1980s onward, Brunner and Meltzer—who, although generally favoring M1 in defining money, had also always given prominence to the monetary base—became proponents of the base as the preferred monetary aggregate from the mid-1980s onward. In this connection, they, Anna Schwartz, and other signatories to the statement of the Shadow Open Market Committee (1986) recommended targeting base growth. A key element of the Brunner-Meltzer argument was that the monetary base was less subject to the influence of bank-deposit-related financial deregulation of the 1980s.[323]

achieve its 1983 M1 target and to end the practice of having targets for aggregates other than M1 *(American Banker,* June 2, 1983).

[322] Another monetarist, Robert Rasche, who would do a considerable amount of empirical work on M2 but focused mainly on narrower aggregates, remarked that his own "priors are strongly biased against broader money concepts and towards aggregates that are unambiguously medium of exchange." (Letter from Robert H. Rasche to the present author, February 21, 1992.)

Leland Yeager was another monetarist-sympathetic economist who opposed M2-type series in this period. Yeager (1983, p. 308) strongly defended monetarism against charges that it was outdated: "The financial innovations that [the critics] emphasize largely represent attempts to wriggle around interest ceilings and reserve requirements made particularly costly by inflation-boosted nominal interest rates, the inflation being due in turn to disregard of monetarist advice." But, contrary to Friedman's attitude toward defining money, Yeager also vigorously defended sticking to a medium-of-exchange-based monetary aggregate, rather than M2 (see Yeager, 1968, 1978, as well as the discussion of Yeager's views in Friedman and Schwartz, 1970, pp. 121–122). Later, in view of later financial developments that made non-M2 deposits more transferable, Yeager saw the situation as having changed. "Institutionally, things are different nowadays than they were back then. But when there was a pretty clear distinction between the media of exchange and near monies, I think that there was a good reason to give attention to point out the distinction between the actual stuff that's spent, [including] checking accounts, and near monies. If the distinction has become very blurred, as it has in subsequent decades, well, the kind of thing I was insisting on is not so relevant anymore." (Leland Yeager, interview, August 8, 2013.)

[323] See Nelson (2019) on Brunner and Meltzer's employment of this argument. Friedman and Schwartz (1970) had appealed to similar reasoning, when suggesting that currency might, under some circumstances, be a good proxy for the underlying money stock (see Chapter 7 above). The same basic argument was found in what the SOMC had been saying on the eve of deregulation, with Tobin (1979, p. 320) remarking: "I notice that M0 [the term increasingly being used in reference to the monetary base] is now the favorite of the monetarists of the Shadow Open Market Committee. They want to control M0 and let unregulated markets determine the supplies and yields of inside monetary assets. While I have considerable sympathy for their advocacy of deregulation of deposit interest rates, I do not share their optimism that such reform would make steadiness of M0 growth a sure road to macroeconomic stability."

Robert Hall—who, as indicated, suggested in his review of *Monetary Trends* that M2/income relations would not endure in the era of financial deregulation—was also briefly an enthusiastic proponent of monetary base growth

Although his own reasons centered on Federal Reserve accountability, rather than the existence of deregulation, Friedman, too, had in the 1980s moved to the position that direct targeting of the monetary base was a desirable policy.

Specifying constant monetary growth as a freeze on the monetary base

On Friedman's part, this recommendation went back to the start of the decade. As indicated in Chapter 12, the Friedmans in the *Free To Choose* book in 1980 specified a constant growth rate of the monetary base as the rule that the Federal Reserve should follow. That essentially remained Friedman's prescription—sometimes offered as his single preference, sometimes given as one of a set of possible alternatives that he favored over current monetary policy arrangements—for the remaining quarter-century of his life.

The fact that constant growth in the monetary base was recommended by Friedman in 1980 makes clear that it is a major misconception to regard Friedman's advancing a zero-base-growth rule prescription in 1984 (erroneously dated as 1987 by Ebenstein, 2007, p. 278, on the basis of what was actually a truncated reprint) as a break, a "final policy recommendation with respect to money" (Ebenstein, 2007, p. 233), from Friedman's previous advocacy of a constant-monetary-growth rule. Friedman had always incorporated a velocity trend assumption into his monetary-growth prescriptions, and, as discussed below, by 1984 he had become persuaded that base velocity's trend in the era of deregulation implied that a zero growth rate in the monetary base would be consistent with price stability—and so was the appropriate number to assign when applying his traditional rule to the monetary base aggregate.

Subject to his continuing support for constant monetary growth, Friedman by the end of the 1970s was increasingly favoring the monetary base as the series to be targeted (and not just be used as an instrument with the aim of attaining a target for a broader total). He certainly believed strongly that the control of M1 or M2 was well within the Federal Reserve's power, and a great deal of his commentary continued to be focused on one or the other of these deposit-inclusive monetary aggregates, while his policy recommendations often entailed the targeting of

targeting. He made this recommendation in a paper given at the May 1985 Bank of Japan conference on financial deregulation and monetary policy at which Friedman was also a speaker. Hall (1986, p. 234) contented that the rule would be economically stabilizing in conditions of a "fully deregulated economy," in which interest rates were free to adjust and reserve requirements had been brought down to low levels (Hall, 1986, p. 234). His preconditions for instituting this rule were that interest should be paid on reserves and that the resulting increase in banks' demand for real reserves (assumed to be one-time in nature) should be accommodated. "With an economy saturated in reserves," Hall (1986, p. 227) asserted, "a policy of constant growth of the monetary base could be expected to provide a reasonably smooth evolution of nominal GNP."

one of the two series (and specifically, from 1986, M2). Friedman was nevertheless frustrated at the extent to which target misses were explained by policymakers in terms of problems in controlling money.

Friedman accordingly became more interested in targets expressed in terms of the monetary base—grounded as it was in the Federal Reserve's balance sheet, and hence directly manipulable by open market operations. In *Fortune* (October 6, 1980, p. 46), Friedman remarked: "If the Fed won't stick to other targets, the base is a lot better than nothing." As indicated, by that point he and Rose Friedman had, in their *Free To Choose* book, when suggesting how a monetary-growth rule might be enshrined in a constitutional amendment, already expressed the rule as one for the monetary base. But the fact that the assigned growth rate (and implied velocity assumption) in the proposed amendment was more applicable to M2 than to the base was one clear indication that, at that stage, the idea had not been worked out by them in detail.

Friedman again offered the base as a possible target variable in his 1981 Money, Credit and Banking lecture.[324] As already discussed (see Chapter 12), in that article he also affirmed that his preferred method of implementing a monetary rule or pursuing monetary targets would consist of a preannounced series of open market purchases by the Federal Reserve. The following year, Friedman stated that he would like to see such purchases carried out in pursuit of a base target, with the authorities making "a simple statement that, every Monday, the government agent will buy a certain specified amount of bonds—$^{1}/_{17}$ of one percent of outstanding high-powered money—and close up shop and go home." (*Barron's*, October 25, 1982, p. 7.) It was this remark that underlay Robert Gordon's reference, in a paper delivered at the June 1983 Bank of Japan conference, to "Friedman's recent proposal for the Fed to make '$*X*' hundred million of open market purchases every Monday."[325]

In his article "Monetary Policy for the 1980s," drafted and redrafted over the spring and summer of 1983, and published in 1984, Friedman laid out a new variant—specifically, a variant using a different number in the rule—of his base-targeting proposal. The new variant was actually announced by James Tobin, who had seen a pre-publication copy of Friedman's article (Tobin, 1983c, p. 507): "Friedman's (1984[b]) latest preference, by the way, is to hold the base constant as an ultimate goal."

[324] Friedman (1982c, p. 117).
[325] Gordon (1985, p. 75).

The 1984 Friedman article was much reprinted—or, to put it more accurately, it was much abridged and then printed in the abridged form. As discussed in the previous chapter, a cut-down version of the article appeared, under a new title, in *Challenge* magazine in 1985. Then, in 1987, a different abridgment of the 1984 article appeared under still another title in Dorn and Schwartz (1987, pp. 361–382), while yet another abridgement, this one using the original article title, appeared in the collection *The Essence of Friedman* (see Leube, 1987, 404–428). After Friedman's death, a cruder, five-page version—with most of the text, along with all footnotes, of the original article omitted and the title of the abridgment not being one of those previously used in Friedman's lifetime but, instead, a new title imposed by the editor—appeared in Ebenstein (2012, pp. 207–211).

Much has been made of Friedman's sporadic advocacy starting in 1983 of a monetary-base freeze. This freeze proposal has often been proposed as a break from Friedman's *k* percent monetary-growth rule. Even Tobin's (1983c) description of it gave that impression in Tobin's reference to a constant base as Friedman's "ultimate goal" rather than as an intermediate target intended to deliver price stability. In contrast, the position taken here—supported by Friedman's statements—is that the recommendation to freeze the base was just a restatement of the constant-monetary-growth rule. It was the *k* percent rule applied to the base. As Friedman put it: "In effect, a monetary rule of zero growth in high-powered money would be adopted."[326] The freeze—the prescribed *k* value of zero—reflected Friedman's view at the time that a zero rate of base growth would be consistent with long-run general price stability.

The interpretation that the base-freeze proposal represented Friedman's abandonment of the *k* percent rule, although earlier created by writers such as Tobin, was reinforced by Ebenstein's (2007) aforementioned discussion of the base-freeze proposal. But Ebenstein does not relate the proposal to Friedman's discussions of monetary policy during the 1980s. Indeed, because Ebenstein relies on one of the later abridgments of the article, he does not even put the base-freeze in the context of the 1984 article in which the freeze proposal appeared. That 1984 article actually reaffirmed Friedman's view that the ultimate objective of monetary policy should be zero inflation.[327]

The fact that Friedman still sought price stability under the base-freeze proposal brings out the point that his advocacy of it was not a break from his constant-monetary-growth rule.

[326] Friedman (1984b, p. 49).

[327] See Friedman (1984b, p. 37).

Friedman's selection of a zero rate of base growth reflected not a repudiation of his earlier views on the right rate of nominal income growth or inflation but, instead, a belief that the future trend rate of increase in base velocity was about 2.5 or 3 percent. This conclusion in turn flowed from his view at the time that M1 velocity in the future would have a 1.5 or 2 percent positive trend, and that the M1 multiplier trend was also positive, at around 1 to 3 percent a year.[328] He also granted that, as it happened, a value of zero for monetary base growth had a "special appeal" not possessed by a positive rate in terms of communicating the rule and adhering to it.[329] As indicated, however, he accompanied this observation with a statement of his judgment that the rule would be consistent with zero inflation on average.

As the abridged reprints of the 1984 article took, to varying degrees, Friedman's remarks away from their original context, the existence of shortened versions has doubtless boosted the tendency to regard the freeze proposal as a break from Friedman's prior *k* percent growth proposals. For example, Hammond (2011) cites only the abridged and retitled version of the paper that appeared in the Dorn and Schwartz (1987) volume, and he takes the proposal as a repudiation of the *k* percent rule. In contrast, as indicated above, the base freeze—the zero percent recommended rate of growth in the targeted aggregate—reflected a constant-monetary-growth rule prescription, applied to the monetary base and using a base velocity growth assumption, and with price stability as the ultimate goal. Friedman also remained a gradualist and so, in the move to the price-stability goal, he wanted to start at 6 percent base growth in the mid-1980s and move down by 1 percentage point each year.[330]

The continuity between Friedman's 1980 and 1984 writings advocating monetary base targeting—with the change in the recommended rate to zero in the 1984 piece reflecting a modification of the velocity assumption, rather than a basic alteration in the rule—was brought out in 1983–1984 statements by Friedman in which he, in effect, treated these recommendations as the same as one another. Speaking in October 1983, he stated of the monetary base that he favored "fixing it once and for all or allowing it to grow at a specified rate"—thereby implying

[328] On Friedman's view, as of 1983–1984, that the M1 multiplier had a roughly 1 to 3 percent trend after 1982, see House Republican Research Committee (1984, p. 34, 44) as well as Friedman (1984b, pp. 50–51). The latter discussion, together with the surrounding text, implied an assumed trend in the M1 of about 1 percent. He did, however, see a flat M1 multiplier as possibly characterizing the very long run (Friedman, 1984b, p. 51) and also granted that he might be overestimating the trend in M1 velocity or the M1 multiplier by a percentage point or so (House Republican Research Committee, 1984, p. 44).

[329] Friedman (1984b, p. 50).

[330] House Republican Research Committee (1984, p.42). As Friedman wanted this phase-in to be accompanied by reductions in reserve requirements, the prescribed growth rate presumably referred to the base adjusted for changes in reserve requirements.

that a zero rate was not a fundamental element of his policy proposal.[331] And in the Friedmans' *Tyranny of the Status Quo* book, they recalled their proposal in *Free To Choose* of a constitutional amendment to fix the growth rate of the monetary base and endorsed that proposal again. In the 1980 book, it had been specified as a 3 to 5 percent rate of monetary growth. This time, in their 1984 book, they referred to the prescription as being for monetary growth "at a fixed rate"—thereby implicitly allowing for Friedman's recent assessment that the appropriate long-run growth rate of the base was zero.[332]

Friedman, conceded, however, that his stress on the base *had* increased during the 1980s. Events, he remarked in July 1986, had "led me to move, more or less in desperation, to a zero-base rule."[333] Why did Friedman shift in the mid-1980s to even stronger advocacy of targeting the base instead of targeting M1 or M2? The answer lies in multiple factors. As a preliminary matter, it should be stressed again that he did not completely shift on this score. As indicated above, in 1983–1986, he continued to advocate M1 targeting, and for the rest of the decade he advocated M2 targeting.

Subject to that caveat, there are three reasons, two of which have already been discussed to some extent, for Friedman's growing interest in base targeting. The first of these was its proximity to Federal Reserve actions. Friedman believed a base target might provide greater accountability. "If the Federal Reserve is to be held accountable, then we ought to look at the base. Period!," he stated in his July 1986 remarks.[334] Five years later, Friedman added: "I do believe that current M2 can be successfully targeted and that such a policy is preferable to M1 targeting at the moment, though perhaps not to targeting the base. The reason for possibly preferring the base is… because it is the one thing over which the Federal Reserve has complete control and therefore it cannot avoid accountability."[335]

A second reason was Friedman's aforementioned belief that base control was sufficient for general monetary control in a deregulated financial environment. "It would be a fixed point which would give us a long-run stability, that is, a long-run anchor to the price level," he remarked in 1986.[336] He indicated that he disagreed with those who believed that reserve requirements were necessary for base control to deliver monetary-aggregate control but, as a

[331] Friedman (1984f, p. 42).
[332] Friedman and Friedman (1984, p. 101; 1985, p. 99).
[333] In Darby and others (1987, p. 28).
[334] In Darby and others, 1987, p. 11). See also House Republican Research Committee (1984, p. 41).
[335] Milton Friedman, letter to the present author, July 16, 1991 (p. 1).
[336] In Darby and others (1987, p. 28).

cautionary measure, he suggested that the phasing-out of reserve requirements should proceed gradually, taking place over a protracted period as the base growth rule was being instituted.[337]

A third reason was that Friedman's confidence in the base growth-nominal income growth relationship was buoyed during the first half of the 1980s by his own studies. In 1983 and 1984, he was convinced that it was a strong relationship but not as good as the M1/nominal-GNP relationship.[338] The subsequent problems with M1 and Friedman's reassessment of M2 led him to view the base/nominal GNP connection as weaker than the link between M2 and income, but respectable and better than that between M1 and income.[339]

The 1980s also saw a number of empirical studies by others take the monetary base seriously as a monetary aggregate in its own right. As discussed above, the Eisner and Pieper (1988) article used the monetary base in regressions intended to ascertain the effects of monetary policy on economic activity.[340] Later, McCallum (1988, pp. 179–192) would illustrate the significance of monetary base growth in a number of small-scale estimated models of the U.S. economy.

Once Friedman's monetary-base-freeze proposal appeared, it took a pounding, drawing criticism from Hotson (1985) and Frankel (1990, p. 138) on the grounds that it was inconsistent with Friedman and Schwartz's account of the Great Depression.[341] In particular, that account had implied that a situation of a domestic currency drain made low or moderate growth in the monetary base inappropriate and that stabilization of monetary growth then required large-scale expansion of the monetary base. The base-freeze proposal seemed to leave the economy exposed to situations in which a rise in the private sector's currency-deposit ratio was not accommodated and instead was allowed to produce a monetary contraction.[342] Benjamin

[337] Friedman (1984b, p. 50).

[338] See Friedman (1984c, p. 399) and House Republican Research Committee (1984, p. 40).

[339] See *Wall Street Journal*, September 18, 1986, Darby and others (1987, pp. 9, 11), and Friedman (1988a, p. 224).

[340] Another paper of the same vintage, Lupoletti and Webb (1986), used the monetary base in a vector autoregression. Its inclusion was justified as follows (p. 268): "The monetary base was substituted for M1 because of our suspicion that financial deregulation distorted the demand for MI in 1981–82." As discussed in the previous chapter, financial deregulation likely had only a small effect on M1 in this period. The decline in the velocity of M1 in 1982 was nevertheless greater than that of the velocity of the base, on account of the greater interest-rate-responsiveness of the real demand for checking deposits than of the real demand for currency.

[341] Frankel took Friedman as having "in recent decades" recommended "a firm target for the monetary base." This was literally true only if one took "in recent decades" as meaning since 1979 or 1980.

[342] The currency component of the base was a frequent feature cited as a drawback of targeting the base. The Federal Reserve Bank of New York's Richard Davis was expressing the view of many central bankers and nonmonetarist academics when he argued (possibly including a deliberate pun in doing so) that the "chief liability" of monetary base control or targeting was the large currency component of the base (Davis, 1982, p. 59). Objections of this kind can be represented by the concern described in the text about the need to accommodate changes in the currency/deposit ratio.

Friedman objected to monetary base as an instrument more generally on this ground, suggesting that a base-focused policy might lead to undesirable cyclical movements in the money stock: base control, he suggested, would "reduce the stock of reserves, and thereby force the banking system to shrink deposits" when the nonbank sector's increase in currency demand increased.[343] But Milton Friedman's proposals regarding base targeting did not altogether neglect the Friedman-Schwartz findings. The *Monetary History* and its companion work had argued that the currency-deposit ratio was prone to large shifts during banking panics. In the case of more normal conditions, however, Friedman and Schwartz's empirical judgment was that the currency-deposit ratio behaved smoothly.[344] Indeed, when discussing policy instruments, as opposed to targets, Friedman often moved between advocacy of control of reserves and control of the base because he believed the two proposals would be similar in normal conditions.

It is clear, however, that Friedman was flexible with regard to following the base-freeze rule in the face of one-time shifts in the currency-to-deposit ratio. Alan Walters (in *The Economist* (London), May 4, 1985, p. 22) noted in this connection: "I suspect that virtually all MBC [monetary base control] advocates would require a 'crisis override' of the MBC rule [in order] to maintain M1." Friedman fell into this category. When pressed about his freeze proposal, he acknowledged that it would have to be overridden in the event of major changes in the currency-deposit ratio.

For the United States, currency drains associated with domestic banking panics were the most familiar source of these shifts, thanks to the experience of the 1930s and earlier. But another case—one more typical of the modern era—was raised by Jerry Jordan with Friedman when they discussed the latter's base-freeze proposal in about 1986. "Milton came out with the paper arguing that the best that could be done was constant M0. And I was later talking with him about that—and asked him: Well, what would you do in an event like—something like what actually did [later] happen, starting in '89, or '90, after the [Berlin] Wall came down—where we had this huge increase in foreign demand for U.S. currency? And Milton said, 'Well, of course, you have to accommodate that. That would be a mistake—to fail to increase the monetary base, because, if you fail to do that, then you would get a contraction of bank reserves and therefore a multiple contraction of the domestic money supply.' And so Milton said: 'Well, OK, you have to accommodate it.' [I also asked:] "But then what do you do if foreigners gain confidence in their own currency, or some other foreign currency, and it starts to flow back in?' And he said, 'Well,

[343] B.M. Friedman (1988c, p. 211).
[344] Friedman and Schwartz (1963b, p. 46).

then, of course, you need to shrink the monetary base because failure to do that'—and so on. So he was very pragmatic—even in urging a constant monetary base—in that you had to use judgment, and judgment meant discretion, and you can have a committee sit around and vote. So he wasn't at all hostile to the idea of, in a sense, violating rules, though I don't know that he would have expressed it that way." (Jerry Jordan, interview, June 5, 2013.)

A different criticism of the base-freeze proposal pertained not to its workability in emergency conditions but to its operation, on average, in more normal times. As noted, embedded in Friedman's base-freeze proposal was an empirical judgment that the velocity of base money had an upward drift. Like that in M1 velocity, however, this upward drift disappeared after 1981, and the velocity of the monetary base held by U.S. residents became well approximated as trendless (Rasche, 1987, p. 21, Table 1.5; Anderson and Rasche, 1999). Indeed, the presumption of a rising velocity in Friedman's prescription was the basis on which Michener (1989, p. 152) stated that the "proposal now looks ill-advised."[345]

Friedman was somewhat slow to take this change in trend on board in his own analysis, referring in a 1988 article to the "rising ratio of national income to high-powered money" as a continuing regularity.[346] By 2002, however, a recognition of the change in trend in velocity was reflected in his statement that he wanted a steady increase in the monetary base of between zero and five percent.[347]

Earlier, in a 1999 interview, Friedman indicated that the zero rate was not vital to his proposal when he stated he wanted the base "essentially unchanged, or if you want, growing at three percent a year." (*Uncommon Knowledge*, Hoover Institution, episode titled "Libertarianism," February 10, 1999.) In a later interview, given in the final year of his life, he again indicated that the base-freeze proposal entailed an assumption that the reserves-deposit ratio was rising.[348]

[345] Strictly speaking, the years (1984 to 1986) whose velocity patterns provided Michener's grounds on which to evaluate Friedman's freeze proposal was a period over which, had his prescription been followed, base growth would have been positive rather than zero, as Friedman envisioned a multi-year phasing-in of the rule.

[346] Friedman (1988d, p. xxiii). If one takes Friedman as referring literally to high-powered money, or to the Federal Reserve's aggregate balance-sheet size, and not a base series adjusted for reserve-requirement changes, the conclusion one reaches is that his 1988 assessment was less far off than is suggested by the behavior of the velocity of the adjusted monetary base. In part reflecting reductions in reserve requirements over the period, the ratio of central bank assets to nominal GDP kept falling in the United States through 1984. See the annual series, "Central Bank Assets to GDP for United States," in the Federal Reserve Bank of St. Louis' FRED portal (*https://fred.stlouisfed.org/series/DDDI06USA156NWDB*).

[347] See Pringle (2002, p. 19).

[348] See R. Roberts (2006).

This flexibility—in accommodating one-time shifts in the currency-deposit ratio—was not obviously present in Friedman's proposal to enshrine monetary base targeting in a constitutional amendment. But it should be stressed that the *Free To Choose* monetary-growth amendment did not arise from the heavy deliberation with legal and other experts that the spending-restriction or balanced-budget amendments did. These fiscal-policy amendments had provisions to override the rule in specific conditions. Had the base-rule amendment been as seriously advanced by Friedman and others as the fiscal-policy-related amendments, it, too, would inevitably have included provisions under which the application of the rule would be modified, including in conditions in which the currency-deposit ratio shifted.

As already indicated, the value of such an amendment was reaffirmed in the Friedmans' book *Tyranny of the Status Quo*. In addition, in October 1983, Friedman endorsed a constitutionally imposed base-growth rule.[349] He remarked in this connection: "The solution is to replace the Federal Reserve by a computer directed by a constitutional amendment."[350]

A continuing skeptic regarding private money

It was noted above that the 1984 Friedman article in which he advocated a base freeze appeared in multiple abridged forms, and one of these abridgements was the basis for a bold claim by Hirsch and de Marchi (1990, p. 296): "Very recently Friedman (in Dorn and Schwartz, 1987) has moved closer towards the Austrian position." The "Austrian position," in this context, meant the position that outside money creation should be transferred from being a public-sector responsibility to a private-sector responsibility, as recommended by Hayek (1976).

Contrary to Hirsch and de Marchi's statement, Friedman had not changed his position.[351] He saw the provision of outside money by the state, and the private sector's demand for that money for use in its transactions, as inevitable institutional facts.

For one thing, in propounding a base freeze in his 1984 article, Friedman was not moving to free banking. Instead, he was advancing a fixed-growth rule for the government's provision of outside money. And he saw this provision of base money as the means of governing inside money creation. "The proposal would be consistent with, indeed require, the continued existence

[349] Friedman (1984f, p. 42).
[350] Friedman (1984f, p. 40). On Friedman's discussions concerning replacing the Federal Reserve with a computer, see Chapter 12 above.
[351] Again, however, the fact that Friedman's 1984 article was abridged appears to have made it more liable to be misinterpreted.

of private institutions issuing claims to government currency," Friedman remarked in the 1984 article.[352]

"Require" in this context did not connote methods of compulsion—such as reserve requirements or laws that required that bank deposits be redeemable in currency. Rather, Friedman saw private-sector behavior as likely, through voluntary arrangements, to produce a banking system that used base money as its medium for interbank transactions and that made conversion (at face value) into currency a characteristic of commercial banks' deposit liabilities.

What is more, Friedman did not believe that a private-sector-issued currency, should such a project be launched on a major scale, would supersede the government's dominance in the area of base money provision. He certainly had no objection to the private sector trying to move into the currency-provision area. A 1982 interview with Friedman indicated that, as well as wanting a monetary base target, "He also favors elimination of legal obstacles to the issuance of money by private organizations." (*Barron's*, October 25, 1982, p. 7.) But he did not see such a move as leading to the situation Hayek envisioned—one of a new private-sector currency superseding the official money. Indeed, the very fact that Friedman saw base control as a means of delivering price stability even when the private sector was legally permitted to issue its variants of currency was revealing about what he saw as the likely outcome of the advent of private-sector-issued alternatives to base money. He believed that such instruments, although intended to compete with the monetary base, would, in practice, only catch on if they were redeemable for base money—and so would end up being much like inside money.

Friedman made this point clear in his "Monetary Policy for the 1980s" article in 1984. He reaffirmed his approval "of Professor Hayek's proposal to remove restrictions on the issuance of private monies."[353] But he differed with Hayek's "belief about the outcome" because Friedman felt that, even in a deregulated world, the government-issued money would be the preferred one, and that privately-issued monies' acceptance would arise from their being reliably transferrable into government money.[354] Friedman made essentially the same argument in an article specifically on the subject of private money—a piece that was also published in 1984 but was of an earlier origin, as it drew on remarks that Friedman had prepared for Mont Pelerin Society events in 1977 and 1981.[355] When it saw print as a chapter in a mostly pro-free-banking book,

[352] Friedman (1984b, p. 49).

[353] Friedman (1984b, p. 47). See also Friedman (1984k, pp. 43, 45–46).

[354] Friedman (1984b, p. 47).

[355] See Friedman (1984k, p. 42).

the editor, Pascal Salin, made it clear that he saw Friedman as adversarial to the Hayek position by giving Friedman's article a title that labeled him a skeptic about currency competition.

That was a fair characterization. Indeed, after the *National Review* published an article (July 27, 1984) criticizing the Friedmans for taking for granted that government should have responsibility for monetary management, Friedman's reply (October 5, 1984) did little to dispel this characterization, as he quoted from his two recent critical pieces on Hayek. Through such interventions, Friedman gave ample fuel for the London *Times*' generalization in mid-1985: "The greatest opponents to 'market money' are, paradoxically, monetarists."[356]

Indeed, in a *Journal of Monetary Economics* review of the Salin book as a whole, Stanley Fischer titled the review "Friedman Versus Hayek on Private Money." In contrast to the Keynesian-monetarist debate, in which Friedman was leading a counterrevolution, this debate was one in which he was representing the mainstream monetary-economics position, which was shared by Keynesians and monetarists. Fischer interpreted Friedman's brief contribution to the book in this light and praised it for its questioning of the private-money argument.[357]

Friedman had remarked in 1978: "For control of monetary aggregates the crucial requirement is simply that the Federal Reserve has a monopoly over the issuance of high-powered money."[358] He saw this requirement as a condition that would very likely prevail for the foreseeable future, even if not imposed by law. Because his position was that, in order to attract widespread usage, private-sector-issued currency would be backed by the government-issued money, Friedman did not see such new instruments as truly ending or threatening the government monopoly. Asked in October 1983 about legal-tender laws, he replied: "It doesn't make a bit of difference if they are abolished or not."[359]

In this connection, Friedman's remarks in the 1984 Salin book cited the precedent of American Express travelers checks.[360] Even the *Monetary History* had described those checks as a case of

[356] *The Times* (London), July 5, 1985.
[357] Fischer (1986, p. 435). Fischer's article, notwithstanding its title, considered Friedman's (1984k) contribution only briefly. It focused on Friedman's position that private-sector-generated money might not be able to deliver constant purchasing power. In contrast, this chapter's discussion focuses on Friedman's other point—that there were strong reasons for believing that the government would not be able to be edged out as the leading currency.
[358] From his Congressional submission of August 21, 1978 (Friedman, 1978e, p. 280).
[359] Friedman (1984f, p. 45).
[360] See Friedman (1984k, p. 44). See also Friedman (1995, p. 172).

"private institutions in the U.S. issuing currency."[361] Similarly, in 1986, Friedman stated in motivating his view that there was "no reason why banks couldn't issue substitutes for currency" the fact that already we have travelers checks—which are the equivalent of currency."[362] But, he noted, these travelers checks were claims on a specific amount of the official U.S. currency.[363]

Consequently, in Friedman's view, a successful private-sector-issued currency would be of a piece with issuance of inside money: in both cases, and even in a deregulated financial system, they would be linked to government-issued base money. Friedman saw targeting of the (official) monetary base as well suited to such an environment: "we would gradually be moving to a free-market system for determining the total quantity of money, pinned down by the fixed nominal amount of base."[364]

Six months earlier, Friedman and Anna Schwartz had published an article in the *Journal of Monetary Economics* that faced the matter of free banking squarely. This was another of their one-off joint writings during this period. "It really didn't grow out of anything we were doing at the time… Just an editorial [request]," Schwartz recalled (personal communication, May 29, 2009). Robert King had contacted Schwartz to see if she and Friedman might be agreeable to contributing to a special issue that he and coeditor Charles Plosser were preparing in honor of Karl Brunner, who had stepped down as *JME* editor at the end of 1984. Friedman and Schwartz were indeed agreeable, and their article was titled "Has Government Any Role in Money?" Although the article did not explicitly say "Yes," that was the answer that clearly flowed from their analysis.[365] The authors expressed continued "skepticism" about the Hayek position on

[361] Friedman and Schwartz (1963a, p. 777). As American Express was a nondepository institution, the travelers checks were nonbank money and so, in Friedman and Schwartz's categorization scheme, a form of currency. The notion that travelers checks should be considered money at all was contested by Yeager (1968, p. 57), who considered them "payment in nonmoney." But the Federal Reserve Board would adopt a position close to that taken by Friedman and Schwartz when, starting in May 1981, it included nonbank-issued travelers checks in the currency component of its definition of M1. See Rasche (1987, p. 71) and Anderson and Kavajecz (1994, p. 10).

[362] In Darby and others (1987, p. 28).

[363] Friedman (1984k, p. 44).

[364] In Darby and others (1987, p. 28). This position could be regarded as implying that a variety of depository institutions would not only demand reserves but also would ultimately have quite a stable demand function for reserves. Cagan (1986, p. 199) shunned monetary base targeting on the grounds that the reserve-to-deposit ratio would change as the composition of commercial banks' deposits shifted between items associated with different reserve requirements. Friedman's proposal was not directly subject to this criticism, as he was applying it to a case in which there were no compulsory reserve requirements.

[365] Friedman and Schwartz (1986a, p. 46). The article also recalled and reviewed Friedman's endorsements of government's role in monetary management in his earlier work, especially Friedman (1960, pp. 4–8). Some of this material in *A Program on Monetary Stability* regarding the appropriate role of government in the monetary system overlapped in content with that in other Friedman writings, including *Capitalism and Freedom*. The latter source was used when the *National Review* piece of July 27, 1984, critically quoted Friedman's statement, "There is widespread agreement that government must have some responsibility for monetary matters." This statement had

private-sector money, in part by favorably citing the writings of Friedman's former student Benjamin Klein, who had critiqued pre-1976 work in the area.[366]
Friedman's position remained different from the free-banking one on the matter of inside money, too. As repeatedly stressed above, he hoped that the default situation would be that a base rule would provide a tolerable degree of stability in the growth in broader monetary aggregates and that depository institutions' business could proceed without much regulation. He continued, however, to support government measures to forestall banking panics and to fortify commercial bank deposits in a financial emergency. Most notably, in 1984 Friedman supported the Continental Illinois bank rescue (see the next chapter), and the Friedman-Schwartz *JME* paper cited the Continental incident as showing the continued need for the authorities to work against tendencies for monetary contraction in panic conditions.[367] In addition, during 1986, Friedman praised deposit insurance, judging that it continued to work well (see Chapter 16).[368]

One final point about Friedman's position in this period is that the fact that he urged the Federal Reserve's abolition did not make him an advocate of ending the existence of U.S. monetary policy. It is not correct, as Hammond (2011, p. 7) claims (on the basis of one of the 1987 abridgements of Friedman's article) that he had reached a new conclusion "that fundamental reform was needed" and now urged, when he had not before, that the Federal Reserve be abolished.

On the contrary, as discussed in Nelson (2020a, Chapter 9), Friedman had, in fact, articulated the case for the abolition of the Federal Reserve in print since 1951. Indeed, Congressional testimony that he gave in 1952 in favor of his monetization rule had highlighted the feature of his

appeared in Friedman (1962a, p. 39; 1962d, p. 219 [p. 174 of 1968 reprint]). Friedman (1960, p. 8) had likewise stated, after Friedman had himself endorsed government having a role, "The appropriateness of governmental responsibility for the monetary system has of course been long and widely recognized." On Friedman's longstanding assignment to the public sector of the role of the control of money, see also McCallum (1985, p. 19) and Nelson (2020a, Chapter 8).

[366] See Klein (1976). Klein's confidence that the public sector's base money provided services that the private sector would demand without any regulation was one Friedman shared. Friedman (1984k, p. 44) could not see U.S. inflation becoming so high that the demand for government-issued money would vanish. In contrast to Friedman, who did not see a serious private-sector competitor emerging in the provision of currency or bank reserves, other authors have suggested how the central bank might actively encourage a demand for base money. Phillips and Jacobs (1983, pp. 262–263) and Woodford (2000), for example, suggested paying interest on reserves as a means of doing so. This, of course, was a change that Friedman (1960) urged on different grounds.

[367] Friedman and Schwartz (1986a, pp. 52–54). Conversely, in early 1985 Friedman criticized Ohio's state government for allowing some thrift institutions to close, describing it as "a replay on a small scale of episodes that occurred" in the United States in the early 1930s (*Dallas Times Herald*, March 21, 1985). See also Friedman and Schwartz (1986a, p. 52).

[368] As discussed in that chapter, the subsequent S&L crisis would make him less enamored of deposit insurance, at least if not accompanied by specific prudential requirements applying to the depository institutions as a *quid pro quo*.

proposal that the Federal Reserve would cease to be a separate government agency.[369] This recommendation produced press attention at the time (for example, *Richmond Times-Dispatch* (Virginia), March 26, 1952), and that attention, in turn, would lead to a *precis* of Friedman's monetary work that appeared in a University of Chicago research newsletter being titled "Abolish the Federal Reserve?" (*Research Reports* (University of Chicago), April 1952.) This controversy would be remembered in retrospect, too, with the *Chicago Sun-Times* (October 27, 1974, p. 85) recalling: "In 1952, he urged Congress to abolish the Federal Reserve."

Friedman's position that the Federal Reserve should be abolished—and that the implementation of his monetary policy rule should be a U.S. Treasury function—continued after he switched to advocacy of the constant-monetary-growth rule. One of the major newspaper profiles of Friedman noted that, rather than have the Federal Reserve voluntarily pursue stable monetary growth, "if he *really* had his way, he says, he'd abolish it [the Federal Reserve] altogether" and have the rule legislated (*New York Times*, January 25, 1970, p. 83). This characterization of Friedman's position was, of course, in line with the elaboration that he had written in 1962 of his view that the operation of monetary policy should be assigned to the Treasury.[370]

Friedman continued to urge in the 1980s that monetary policy be placed in the Treasury's hands. For example, a report on remarks that Friedman made in June 1982 in the city of Chicago stated with regard to the Federal Reserve: "He called for the agency's abolition, saying that monetary policy should fall under the auspices of the Treasury Department so it would better follow administration directives in reaching goals." (*Commodities Magazine*, July 1982, p. 12.) This recommendation, already familiar, was one that Friedman made again in the next couple of years—being more specific on these later occasions that he also wanted a policy rule imposed as part of the transfer of monetary responsibilities.[371]

With these remarks taken into account, it becomes clear that when, in October 1983, Friedman stated that he wanted to create conditions in which one could "simply dismantle the Federal Reserve," it was in the context of imposing a monetary-base-growth rule and making that rule a routine Treasury function—not in the context of shifting to a free-banking regime.[372] Similarly,

[369] See his testimony of March 25, 1952, in Joint Committee on the Economic Report, U.S. Congress (1952b, p. 691). Friedman gave this testimony alongside Paul Samuelson, who would correspondingly later attribute the view that the Federal Reserve should be abolished to Friedman (Samuelson, 1956, p. 19).

[370] See Friedman (1962d). See also *Reason* (June 1975, p. 92).

[371] See Friedman and Friedman (1984, p. 101; 1985, p. 99) and House Republican Research Committee (1984, pp. 34–35).

[372] The quotation is from Friedman (1984f, p. 42).

when, in 1984's "Monetary Policy for the 1980s," Friedman remarked that his proposals would end the Federal Reserve's role in "determining the quantity of money" and would "abolish the money-creating powers of the Federal Reserve," these statements were not references to taking responsibility for monetary management out of the government's hands but, instead, to automating that responsibility via a base rule—and thereby, Friedman believed, ending the need for a separate central bank within the U.S. government.[373]

It is therefore seen that it is incorrect to infer, as Hammond (2011, p. 7) does, that, during this period, "Friedman proposed… to take monetary policy away from the Fed… and not give it to anyone else." As Friedman recommended a monetary base rule, and the provision of the monetary base is central to monetary policy, in this proposal he was clearly giving monetary policy to the U.S. Treasury—albeit with the stipulation, in conjunction with this transfer, that the Treasury then follow a monetary policy rule. The dictum that Friedman had articulated a quarter-century earlier, "There is no such thing as no monetary policy," therefore still applied.[374] A monetary policy subject to a rule was still a monetary policy. And a policy with respect to the monetary base of "fixing it once and for all or allowing it to grow at a specified rate" (as Friedman recommended in the mid-1980s, as quoted above) was certainly a monetary policy rule: in his view, this policy fixed a path for government-provided money—which, he believed, would continue to be demanded in its own right—while also, through the economic connections between inside and outside money, setting limits on the amount of money issued by private-sector institutions.[375]

THE OIL PRICE CRASH

During the summer of 1975, the *Washington Post*'s longtime economics writer Hobart Rowen had remarked: "My colleague in the columning business, Prof. Milton Friedman, says that decontrol of [U.S. domestic] oil 'will not produce an immediate rise in the price of petroleum' and 'ultimately it will lower prices as the free market works its magic.'" (*Washington Post*, August 24, 1975, p. G1.) Rowen was highly critical of Friedman's conjecture, contending instead that "all energy prices will go up as oil goes up" (p. G2).

Six and a half years later, and twelve months after full petroleum price decontrol had been

[373] The quotations are from Friedman (1984b, pp. 49, 53). As this article also recommended a freeze in the monetary base, the reference to abolishing money-creating powers was also a reference to the implied zero growth in the base.
[374] From Friedman's testimony of May 25, 1959, in Joint Economic Committee, U.S. Congress (1959a, p. 617).
[375] The quotation is, again, from Friedman (1984f, p. 42).

instituted in the United States, Friedman felt that the proponents of decontrol had been vindicated. In his talk at the Atlanta Hilton hotel in March 1982, he remarked: "Those of us who have for years argued against price control on oil and against the allocation and entitlement[s] system said that if you eliminated price control, oil prices and gasoline prices would come down and not up. That, of course, is exactly what happened."[376]

Oil and gasoline prices had, indeed, turned around. Their upward trend had ended in 1980–1981. During the subsequent change in trend, what was most marked in the period from 1981 to 1984 was the contour—the end of a rise—rather than the magnitude of the decline. But even the latter was notable, especially in real terms. With regard to U.S. retail gasoline prices, 1981's nominal average value was $1.31. 1981's gasoline price was the highest in real terms in modern times in the United States up to that point. The 1981 price would not be exceeded in real terms until 2006 and it would not be surpassed even in nominal terms until 2000. Following the 1981 peak, U.S. retail gasoline prices averaged $1.22 in 1982. This decline in the nominal price of about 6¾ percent occurred during a period in which U.S. consumer prices generally, while rising more slowly thanks to the end of the Great Inflation, still clearly had an upward trend.[377]

At the time of Friedman's March 1982 talk, a change had also taken place in world oil prices. In that month, the West Texas Intermediate (WTI) price of oil averaged $28.48 per barrel. This was down from the $38 WTI price prevailing for the first five months of 1981—a price that was not far from the highest values seen historically up to that point (with the peak monthly average having been $39.50, in July 1980).[378]

Longtime Friedman opponent Walter Heller was, as indicated above, still conceding very little when it came to the Keynesian-monetarist debates on macroeconomic relationships. But, in the wake of the post-decontrol behavior of oil and gasoline prices, he was willing to admit errors regarding the position that he had taken during the country's previous decade of debate on energy policy. Appearing on television in February 1983, Heller was reminded that, as recently as 1980, he had been advocating that gasoline be meted out to U.S. customers via coupon-based

[376] Friedman (1982a, p. 60).

[377] 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*. This source also provides the 1983 value cited later.

[378] See the monthly data on the WTI oil price given in the Federal Reserve Bank of St. Louis' FRED portal: *https://fred.stlouisfed.org/series/WTISPLC*. (This is also the source for the monthly-average WTI values given below.) Some other, higher-frequency, measures of the spot price gave the peak as occurring in early 1981.

rationing.[379] He was asked if he now disagreed with the judgment that "the market hasn't done a bad job on energy." Heller replied that "indeed it hasn't. And, on that, I was just plain wrong." He also took the view that decontrol had "had a lot to do with bringing OPEC to bay."[380]

In the mid-1970s, Friedman's views on the pricing of petroleum had been disputed by Rowen and Heller not only with regard to the U.S. market—because of his support for price decontrol—but in the international area, too—because of his repeated statements that OPEC was near to a collapse and that the first oil shock would be undone by a supply glut and a resulting price decline. A few days after his March 1982 talk in Atlanta, Friedman—being interviewed by a panel of questioners that included Rowen—felt emboldened to give an updated vintage of this prediction. Of the members of the oil cartel, he affirmed: "Yes, they are disintegrating. Whether they will continue to disintegrate depends primarily on things… I'm not very good at predicting—and nobody is—namely, political events. If there were to be further political disturbances in the Middle East, if Saudi Arabia were to go the way of Iran or something like that, then all bets would be off… But if the situation in that part of the world does not deteriorate, then I think the OPEC cartel is definitely on its way toward disintegration."[381]

Six months later, speaking during a visit to a key oil exporter (but not OPEC member) Norway, Friedman suggested that a dramatic step-down in oil prices was in prospect. He did so in a speech on September 14, 1982, at the executive's club in Oslo (*Business Times* (Singapore), September 16, 1982). Friedman again indicated that his predictions were conditional—doing so this time by noting that so too were others' forecasts: "Betting on continued high oil prices is equivalent to betting on a disruption in the Middle East." Barring such further disruption, he told his audience, oil prices would decline: "I am not talking about a reduction from $32 to $30 a barrel, but a drop to somewhere around $15 to $20." In qualifying these remarks, Friedman indicated that some hedging was required with regard to the time frame: "The past has shown that I do better in indicating the direction of future developments than in predicting the more specific timing of events." (*South China Morning Post* (Hong Kong), September 17, 1982.)

In his remarks on this occasion and in other statements given during this European trip, Friedman

[379] Of course, as discussed in previous chapters, there certainly was rationing of oil and gasoline in the United States until 1981. But most of it occurred in the distribution process rather than in ways that were regularly visible to U.S. motorists.

[380] *Wall Street Week*, Maryland Public Television, February 25, 1983, p. 10 of transcript. Heller, who in 1975 had criticized President Ford for attempting oil-price decontrol, praised Ford's successor for the same policy: "Jimmy Carter had the guts, in the face of a lot of Democratic and liberal opposition, to decontrol oil prices. Now it's true that Reagan [in 1981] sped up the last seven months of it. But he [Carter] was the one that decontrolled."

[381] *Meet the Press*, NBC, March 21, 1982, p. 10 of transcript.

suggested that a $10 price was also possible (*Business Times* (Singapore), September 16, 1982). Asked if his statement might disturb oil markets, he suggested that he didn't mind if traders did react: "they are crazy and they deserve what they get." (*The Albuquerque Tribune*, September 15, 1982.)

Early in the following month, Friedman told an audience of 1000 executives in Dallas that "there is no possibility [that] the price of oil will go anywhere but down" in the next few years. The "wave of rising energy prices" had ended, he suggested. "Now, all the market forces are pushing prices down. Supplies are up, while the recession has kept demand from rising."[382]

By the following spring, the recession was clearly over, but Friedman was emboldened to predict in his *Newsweek* column (March 21, 1983) that, absent another event like the Iranian revolution, an even lower range than that of $7 to $8 (the range that was the approximate real equivalent of the pre-1973 price) was possible. The grounds he gave were that Saudi Arabia was unlikely to agree to the production cuts that would keep the oil price in recent ranges, that a breakdown of the OPEC cartel was likely, and that developments in demand and supply since 1973 had made post-1973 prices unsustainable.

In suggesting, however, that the resulting oil price might be below the pre-1973 price in real terms—that is, be less valuable than it had been even *before* the first oil shock—Friedman was seemingly overlooking a point that he had himself endorsed in the late 1970s—that even had OPEC not taken its drastic steps, there were forces present in the early 1970s that likely justified a substantial and lasting rise in the real price of oil.[383]

Even without a return to the pre-1973 oil price, however, there was plenty of scope in early 1983 for the oil price to fall further, and Friedman reaffirmed in an interview given about a month after his column appeared (*Boardroom Reports* May 1, 1983, p. 1): "The drop in oil prices is still very far from its end."

Projections of substantial price declines by this stage had become quite widespread. At a Congressional hearing on July 2, 1983, one of those testifying mentioned Friedman's prediction and added that the predicted direction of movement was widely shared: "There is a broad and

[382] *Dallas Times Herald*, October 6, 1982, p. D3. Reflecting the connection of the region to the oil industry, this was reported in the Texas media as being a "bearish" prediction and not a bullish one (p. D1).
[383] See Chapter 3 above.

general consensus that the price of petroleum products will continue to fall around the globe."[384]

At the retail level, U.S. gasoline prices fell a bit further, to average $1.16 per gallon in 1983. But the world price of oil was not registering a clear-cut movement in that period. Friedman's remarks in March 1982 about recent declines in oil prices had taken place near that year's low point in the world price, and, as his talk in Norway the following September indicated, the price rebounded over much of 1982. Oil prices were about $36 in October 1982. Against this background, Bernanke (1982b, p. 153) could refer in a talk given during the following month to "still-high energy prices." By March 1983, the time of Friedman's *Newsweek* column on the subject of oil prices, they had experienced a new decline and were back around to the levels of a year earlier. But, then, oil prices registered mostly higher levels—in the $29 to $32 range—for the following eighteen months. The lower end of this range was about double the price of oil prevailing at the end of 1978. The second oil shock in 1979–1980 had added about $25 to the price, and developments through the early fall of 1984—while certainly seeing a definite decline in oil prices, on net—had seen only about $7.50 to $10.50 of this increase wound back. Writing in the second half of 1983, the Friedmans acknowledged that the decline in oil prices had been "thus far, rather minor," a statement they kept when revising their this discussion in early 1985.[385]

The commodity price collapse and the Chilean economy, 1982–1983

The decline in world oil prices during the 1981–1982 recession had been decidedly smaller than that of non-oil commodity prices. Starting from an index level of 100 for the average of 1980, the International Monetary Fund's (1986, p. 173) index of non-fuel world commodity prices fell to 89.4 in 1981 and 80 in 1982. In Chile, the decline in commodity prices produced a terms-of-trade collapse. In conjunction with other economic arrangements in Chile, including trying until 1982 to maintain a fixed exchange rate against the dollar, this collapse was associated with a massive fall in national output.

As of the mid-1980s, Chile's real GDP was reckoned to have declined by amounts of 14.5 percent (Edwards, 1986, p. 243, Table 1) or 16.4 percent (International Monetary Fund,1986, p. 273), and in 1983 by 0.7 percent (International Monetary Fund, 1986, p. 273) or 2.6 percent (Edwards, 1986, p. 243, Table 10.1). Modern data put the annual declines at 13.5 percent in

[384] Norman deVall, testimony of July 2, 1983, in Committee on Interior and Insular Affairs, U.S House of Representatives (1984, p. 32).
[385] Friedman and Friedman (1984, p. 83; 1985, p. 84).

1982 and 2.8 percent in 1983.[386] In addition, in 1982, Chile's official unemployment rate peaked reached nearly 24 percent (Edwards, 1986, p. 274).

This collapse came after a period in which Chile's recent economic-growth record had been cited as strong, averaging 8.5 percent in 1977–1980 (Edwards, 1986, p. 241). The catastrophic events of 1982–1983 led to unfavorable comparisons of Chile's economic performance with those in the region but also to a renewed international spotlight on the country's economic policy under the Pinochet junta. This resurgence in discussions of Chile's economy often featured intensified criticism of Friedman—with many commentators referring to him in the context of Chile while making adverse judgments about monetarist and free-market policies.

When the economic collapse was in its early stages, the Chile case had been highlighted by Friedman himself, when he used his column in early 1982 to talk favorably about the country's economic record (*Newsweek*, January 25, 1982). His piece argued that the "adoption of free-market policies by Chile" under Pinochet had been unusual for a military government in the region, as Latin American dictatorships tended usually to be "as authoritarian in the economic sphere as they have been in politics." Although positive on economic performance since 1975, the column recognized, but underestimated, recent economic problems by noting that "Chile is currently having serious difficulties."

By choosing the venue of his column to discuss Chile, Friedman—despite his unhappiness at being characterized as orchestrating Chile's economic policy—showed that he wanted to put clearly on the record his judgment that Chile's economic record in recent years had supported the position that free-market policies and monetary restraint against inflation improved economic performance.[387] He continued to stress, however, his disapproval of the dictatorship. "I do not and never have approved of the political suppression in Chile. I am in favor of a free society both politically and economically," he remarked in Canada in March 1982.[388] In a talk taped in the following September for U.K. television, Friedman alluded to the case of Chile when he stated that many countries that had had economic freedom lacked political freedom.[389] His

[386] See the Federal Reserve Bank of St. Louis' FRED portal series, "Real GDP at Constant National Prices for Chile, Millions of 2017 U.S. Dollars, Annual, Not Seasonally Adjusted" (*https://fred.stlouisfed.org/series/RGDPNACLA666NRUG*). The numbers given in Friedman and Friedman (1998, p. 406) were declines of 13 percent in 1982 and of 3.5 percent in 1983.

[387] Friedman's column did not cover the controversy about his own 1975 visit to Chile. He had, however, focused on that controversy in a column a year earlier (*Newsweek*, January 12, 1981). This controversy was discussed in detail in Chapter 6 above.

[388] Friedman (1982f, p. 61).

[389] See Friedman (1983d, p. 16; 1983g, p. 55).

January 25 column had suggested that free-market policies would not be sustained if the junta did not relinquish office: "sooner or later—and probably sooner or later—economic freedom will succumb to the authoritarian character of the military." (*Newsweek*, January 25, 1982.). "Milton's argument during that episode was that, if you do not have political reforms, you will not maintain economic reforms," Jerry Jordan observed (interview, June 5, 2013).

The economic difficulties that Friedman had acknowledged in his January column, but whose scale he had not appreciated then, became manifest over 1982. And, to many commentators, once the economic and financial crash occurred, it was treated as a decisive adverse verdict on monetarism, free-market policies, and Milton Friedman. One internationally syndicated news article was printed under such headlines as "Monetarism Has Left Chile in Shambles" and suggested that the ultimate effects of the "monetarist policies fathered by Professor Milton Friedman" were now apparent: "After years of conspicuous economic success in Chile… [m]assive unemployment has arrived with a vengeance." (*The Advocate* (Red Deer, Alberta, Canada), October 6, 1982.)

As it happened, by general agreement, a major contributor to the collapse was a policy that was not a free-market policy, and one of which Friedman was a longstanding critic, and that was not part of his monetarism—although it was a policy favored by many among those trained at the University of Chicago. The Chilean currency had had a fixed exchange rate against the U.S. dollar since 1979, and the parity was not ended until the second quarter of 1982 (Dornbusch, 1986, p. 4; Edwards, 1986, p. 156).

Friedman would later note that in his own analyses of Chile he had recommended a floating exchange rate.[390] But, after the decision to fix exchange rates had been made in Chile, Friedman generally took those in charge of economic policy as better qualified than he was to judge the suitability of that exchange-rate policy for Chile's conditions. A passing remark that he made about the policy in his July 1981 Money, Credit, and Banking lecture reflected both his inclination to defer to others on such judgments and his underestimation at the time of the severe implications of the policy. Friedman observed that Chile "has now pegged its exchange rate to the U.S. exchange rate, having decided that, bad as U.S. monetary policy is, it is likely to be more successful than their own."[391] In fact, in the conditions facing Chile's economy in the early 1980s—a declining terms of trade and an internationally appreciating U.S. dollar—the pegging

[390] Friedman and Friedman (1998, p. 405).

[391] Friedman (1982c, p. 102).

policy led not to a replication of U.S. monetary conditions in Chile, but to drastically severe domestic conditions: for example, its real interest rates exceeded 40 percent in late 1981 (Edwards, 1986, p. 273).

Likewise, in a visit to Chile in November 1981 that Friedman made in order to attend a Mont Pelerin Society regional meeting, Friedman reaffirmed his belief in floating rates but again did not take issue with Chile's exchange-rate policy—partly likely in order not to be seen as trying to upset its position in foreign-exchange markets (see Edwards and Montes, 2020). In his talk at the Mont Pelerin Society event on November 19, he stated that Chile's fixed exchange rate policy had been a success so far.[392]

When he later wrote on Chile's monetary experience in 1992's *Money Mischief,* Friedman stressed that the 1979–1982 fixed-exchange-rate policy was a key policy mistake. Similarly, in 1983, when discussing the Chilean economy's poor state, Friedman had emphasized the pegging of the exchange rate as having been a major problem. For example, in a telephone interview that he gave on Chile's situation, he remarked, "Chile is in very deep trouble," and traced it to the previous policy of pegging the currency (*Christian Science Monitor,* September 27, 1983).[393] In a letter published around the same time, Friedman hinted that the fact that Chile had a military government may have led to the policy of a fixed exchange rate not being repealed sooner in response to the strains that the arrangement had caused: "It is a shame things went as they did, but it is hard to believe they could have gone otherwise given the combination of the mistake in pegging the Chilean currency to the U.S. dollar plus the existence of a military government." (*Inquiry* (Cato Institution), October 1983.)

Earlier in 1983, Friedman had tried to give a perspective on Chile's economic record over the last stretch of years. "In the early stages, when inflation was brought down from something like four or five hundred percent a year, back down to a figure of 5 or 10 percent, the decline in the

[392] See Friedman (1995, p. 176). As Edwards and Montes (2020) note, it would seem from the remarks that he made during his November 1981 visit to Chile, and certainly including this later-published statement, that Friedman was under the misapprehension that Chile's arrangements consisted of a currency-board setup—which he considered to be a harder and more sustainable means of joining currencies together than a pegged-rate arrangement. He did not seem to have this misconception prior to his trip and was disabused of it later: In his July 1981 comments, in contrast, he accepted that Chile had a pegged-rate system (Friedman, 1982c, p. 102), and later he cited as one weakness of the 1979–1982 system the fact that it was a peg and not a hard-currency link (*Christian Science Monitor*, September 27, 1983). It may be that, as of November 1981, Friedman believed that Chile was preparing for a harder link to the dollar.

[393] Similarly, in *California* magazine (October 1984, p. 77), Friedman observed that "the government made a great mistake. The government pegged their currency to the U.S. dollar. As a result, Chile is going through a very major depression."

monetary growth [rate] worked very well. Again, for about a year and a half, you did have a very severe recession, and then for about three years you had a considerable expansion. More recently, Chile has fallen onto very hard times, it's going through a severe depression, as a result of the collapse of copper [prices], on the one hand, and the enormous appreciation in the U.S. dollar [on the other]. They made a great mistake of pegging their exchange rate to the U.S., [to] the currency of the U.S. dollar. So Chile is having a very serious time now."[394]

In the newspaper interview that he gave on Chile six months later, Friedman indicated that although Chile under Pinochet had retreated from some free-market policies after the 1982 economic collapse, including by increasing protection, his judgment was: "The structural reforms remain almost untouched." (*Christian Science Monitor*, September 27, 1983.) Indeed, the London *Economist* (August 10, 1985) stated, much like Friedman's *Newsweek* column had three and a half years earlier, that, unlike other states in the area, including other military dictatorships, Chile had continued to follow largely free-market policies.

With regard to the behavior of aggregate real economic activity, Friedman in retrospectives continued to stress that in the early to mid-1980s Chile's economy saw "disaster—serious economic decline, indeed depression" (*National Review*, June 11, 1990*a*, p. 30). But he added that the Chilean economy experienced rapid growth after its 1982−1983 slump.[395] Real GDP in Chile did not, however, exceed its 1981 average until 1987, and during the latter year a book-length critique of Friedman's views took for granted that the free-market policies pursued in Chile would be seen in retrospect as having been a failure (see Rayack, 1987, pp. 64, 69). In the same vein, in the immediate wake of the 1982−1983 deep downturn, numerous studies had appeared on the subject of whether free-market-based economic policies had been shown, by Chile's experience, not to work. Friedman himself provided a list of some such articles in his memoirs.[396]

In the areas of economic research and policy analysis, the World Bank, in a conference titled "Economic Liberalization: Adjustments During the Transition Period" on October 13−14, 1983, had as its concluding session" a discussion of "The Economic Liberalization of Chile: Did It Fail?," with five articles produced for the session.[397] But with the accumulation of further years

[394] *Saturday Briefing*, BBC2, March 12, 1983, pp. 12−13 of transcript.
[395] Friedman and Friedman (1998, p. 406).
[396] Friedman and Friedman (1998, p. 406).
[397] Three of the session's papers were published in the conference volume: Hanson (1986), Harberger (1986), and Edwards (1986). The two other papers presented were Balassa (1983) and Corbo and de Melo (1983) (see also Corbo, 1985).

of data through the mid-1990s, Chile gradually came to be seen again as having generated a record of strong economic growth against a background of structural reforms (see, for example, Dornbusch, Goldfajn, and Valdés, 1995, p. 253).

Chile's 1982–1983 economic collapse occurred as part of a broader event—the start of the Latin American debt crisis—and Chile was one of the countries that made loan arrangements with the International Monetary Fund in the wake of the crisis, with its loan agreement signed in January 1983 (Edwards, 1986, p. 268). Friedman's analysis and commentary concerning the debt crisis are discussed in the next chapter. As detailed there, much of what he said concerned the implications for U.S. financial system, and a good deal of his comments on the specific Latin American countries concerned nations other than Chile, like Mexico.

With regard to IMF policies toward the nations that borrowed from the Fund in the course of the debt crisis, Friedman's commentary was notable. Although Friedman himself encouraged free-market policies and restraint in government spending for all countries in the world, he disapproved of IMF packages that advanced loans to national governments as a *quid pro quo* for specific policy changes—its procedure of "conditionality"—even when those packages involved instituting policies of which he generally approved.[398]

This opposition to conditionality was tempered by the fact that Friedman believed that, in practice, governments often took a conditional IMF loan as a means of facilitating the acceptance at home of policies that it itself accepted were necessary (see Nelson, 2009, p. 490). But with regard to the debt-crisis response, in some cases Friedman believed that the IMF-recommended policies were not a good match for the country's problems. In particular, he was critical of IMF requirements that the borrowing governments restrict aggregate demand when, Friedman believed, this was a one-size-fits-all policy that was not suitable in some instances: "I'm worried," he was reported saying, "about the [IMF] austerity programs" being applied to Third World nations. He explained: "Austerity may make sense for one or two developing countries, but not for such countries as a whole." (*Wall Street Journal,* October 12, 1984, p. 25.) Although demand restriction or austerity was frequently labeled "monetarist" policy in commentary at the time, Friedman was opposed to a policy of uniform application of demand restriction—as distinct from instituting a policy consistent with the achievement of monetary and price stability.

[398] See Kuehn (2020) for examples of Friedman's articulation of this point. Kuehn, however, takes Friedman as having dropped this point "[b]y the 1980s" (p. 1). As the example given presently indicates, however, this conclusion is not warranted.

Developments in 1985 and 1986 regarding oil prices

In late July 1984, Paul Volcker testified: "In fact, a number of sensitive commodity prices have dropped recently, following sizable cyclical increases."[399] It would turn out that the development that Volcker was describing was the start of a process that would accelerate in the later part of 1985 and that would see global commodity prices slump during 1986. During this slump, oil prices would experience a very large, if largely ephemeral, decline.

Interviewed in mid-1983 (*Texas Monthly*, July 1983), Friedman had remarked: "I don't know what the price of oil will be in July 1984. If economic considerations were permitted full play, if there was a really free market, then in a year prices would fall to seven to ten dollars a barrel." On this occasion, he was speaking of a hypothetical situation and not making a prediction. But the downward pattern of the kind that Friedman sketched did get started around July 1984, as Volcker's testimony quoted above attested. The oil price was showing renewed weakness starting around the middle of 1984. After May 1984, there was only one month—November 1985—in the rest of the decade in which its monthly average was above $30. And the free-market conditions that Friedman had described in 1983 came much closer to being realized in 1985–1986 when the OPEC cartel was in disarray. Within OPEC, Michael Canes remarked (interview, November 7, 2013), "the Saudis suddenly found themselves in the unenviable position of being the swing producer… [F]or a while, they went along with it. And then, finally they said: 'No—no more.'"

As indicated, the oil price was quite strong during November 1985. The WTI price reached a local peak of $31.01 on November 25, 1985—but a steep decline followed, and its price at the tailend of March 1986's trading was $10.42, the lowest level recorded since 1978 (*Asbury Park Press* (New Jersey), April 1, 1986; *Evening Post* (Wellington, New Zealand), April 1, 1986). The price then briefly went briefly below $10 per barrel in early April (*Australian Financial Review*, April 14, 1986). The slide that started in late 1985 was sometimes called "The Third Oil Shock" in the business press (*Financial Times* (London), December 14, 1985; *Australian Financial Review,* April 14, 1986)—although that term did not catch on as a permanent label, perhaps because of the subsequent rapid rebound in oil prices.

The *Financial Times* editorialized in the early weeks of the major price decline that Friedman,

[399] From Volcker's written submission for the hearing of July 25, 1984, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1984b, p. 5).

having long predicted a decline associated with dissension in OPEC, could now claim a belated vindication (*Financial Times* (London), December 14, 1985). Friedman did just that, writing a guest column (March 10, 1986) for his former home of *Newsweek* magazine about the recent collapse of the oil price. As was often the case in his publications (especially by this point), Friedman's piece contained a considerable amount of quotation of his past writings on the subject matter.[400]

Further along in the year, a research article on "the 1986 oil collapse" was produced for the Brookings Institution economics panel. In this study, which covered developments through midyear, Gately (1986, p. 244) included the nearly obligatory reference to Friedman's incorrect public predictions during the 1973–1974 first oil shock of an imminent winding-back of the OPEC price increase. But Gately went on to suggest that formal economic research, represented in "the analytic work of 1974–75" (p. 245), had agreed with Friedman to some extent: although it had suggested that the first oil shock in part produced a price increase that was justified by underlying market forces, it had also concluded that the price rise had been overdone. Gately nevertheless noted that the large-scale decline in 1985–1986 had caught many commentators off guard: "Now that it has happened, it is possible to understand the 1986 price collapse. But it certainly came as a surprise to most observers at the time." A footnote then added, "A few analysts may not have been so surprised," and cited four articles, dating from 1981 through early 1985, that had indicated that a substantial further decline, associated with a break in OPEC, was in the offing. Gately did not mention Friedman here, thereby neglecting the March 1983 *Newsweek* column as well as the internationally reported remarks that Friedman had made in the fall of 1982.[401]

There was renewed weakness in oil prices in the summer of 1986. Against this background, Friedman said in June 1986 that he expected the oil price to move to the range of $8 to $9 per barrel, corresponding to the value of "the long-term equilibrium price of oil" at current prices. "I believe that OPEC has no future," he added, as he did not believe that a consensus in the cartel could be restored (*Houston Post*, June 14, 1986.) Friedman observed on the same occasion: "I do not share the view that oil prices have reached their low and will go back up. On the contrary… prices are likely to go even lower."[402]

This again turned out to be a case of Friedman overdoing his oil-price predictions. The oil price

[400] See also Friedman and Friedman (1998, p. 364) on this column.
[401] Gately (1986, p. 260).
[402] Friedman (1986b, p. 246).

fell briefly again in July 1986 to below $10 a barrel, but this proved to be a low point, and it had moved up to $18 by January 1987 (National Energy Information Center, 1987, p. 49). This meant that oil price developments in the 1980s had largely indeed undone the effect of the second oil shock on the real oil price but left in the price a very great deal of the effect of the first oil shock.[403] Furthermore, OPEC, having rallied after mid-1986, would continue as a price-setting organization. In the numerous subsequent episodes of very major increases in oil prices, however, OPEC's role in generating those increases was, from now on, typically less crucial than had been the case during the 1970s. For example, in 1990, OPEC did initiate a sizable midyear oil price increase, but the major runup in oil prices that followed was associated with Iraq's invasion of Kuwait, not with concerted actions on the part of OPEC.

II. PERSONALITIES IN DEBATES ON FISCAL POLICY, REGULATION, AND AGGREGATE SUPPLY, 1982–1986

GEORGE STIGLER

A mid-1978 piece by the *Financial Times*' columnist Samuel Brittan highlighted the contrast between the image and the reality of Milton Friedman's position *vis a vis* the University of Chicago. "One of the amusing aspects of a term at the University of Chicago (spent in fact at the law school)," Brittan wrote, "is to discover for oneself just how ludicrous are some of the popular ideas held in the U.K. about Chicago economics… [It appears that] some Britons think that Professor Milton Friedman is personally in charge of all economic teaching and research [and] vets all newcomers for doctrinal purity." Brittan stressed not only that this was a serious misconception about Friedman's years at the University of Chicago, but also that those years were over: "Impossible though it is to get people to believe this, Professor Friedman retired from his Chicago chair last year… and operates professionally from the Hoover Institution in Stanford." (*Financial Times* (London), June 8, 1978.)

Indeed, although he was fully entitled to list himself as affiliated with the University of Chicago—something that he, indeed, continued to do—Friedman had not truly been a member of the university's economics department since he wrapped up his teaching and workshop activities

[403] This point is brought out by comparing the oil price in July 1987 (by which time the monthly average WTI price per barrel was back above $20) with that in January 1974 (by which point the 1973–1974 oil price rise had been fully registered in the monthly average of the WTI price). Using the seasonally adjusted monthly CPI, the implied real price of oil in July 1987 was about 13.1 percent below its January 1974 level—an appreciable decline, certainly, but small in relation to the fourfold increase that had produced the 1974 price. See also the plot of the real oil price (from 1975 onward) in Baumeister and Kilian (2016, p. 141, Figure 1).

in late 1976. He had not lived in the city of Chicago since the earliest days of 1977. And Friedman's visits to the city during the first half of the 1980s were infrequent—roughly comprising a couple of days in each calendar year.[404]

Friedman's lack of presence after 1976 on the University of Chicago scene was underlined when George Stigler's receipt of the Nobel award in economics was announced on October 20, 1982 (*Muncie Evening Press* (Indiana), October 20, 1982). In contrast to Friedman, his contemporary Stigler had remained a physical presence on the university's campus after 1976. Correspondingly, Stigler's affiliation with the university was an ongoing, full-fledged, and genuine one—in contrast to the courtesy character of the title of professor of economics that Friedman had retained.

Although Stigler's routine from the late 1970s onward did typically involve spending winters at the Hoover Institution (*Chicago Tribune,* October 24, 1982*a*), he had remained a major fixture of the University of Chicago's economics scene. He had formally acquired emeritus status in 1981, the year in which he turned 70 (A&C Black, 1987, p. 1677), but he continued over the late 1970s and during the 1980s to be an active senior professor in both the economics department and the business school. Also unlike Friedman, who had not been a named editor of the *Journal of Political Economy* even when working at the University of Chicago, Stigler was one of the principal *JPE* coeditors during Friedman's closing years at the university and for all the years beyond in Stigler's lifetime. Reflecting this fact, Stigler was listed in the *JPE*'s internal cover pages as one of the journal's four coeditors in the first issue of 1975, and he was still being listed in this way in the first issue of 1983 and the first issue of 1992 (one largely prepared before Stigler's death on December 1, 1991).

Stigler's activity at the University of Chicago, both before and after Friedman's departure, also attested to another point that Samuel Brittan's 1978 column had underlined: there was much important University of Chicago economics in which Friedman was not involved as a researcher, supervisor, or catalyst. Brittan had noted, in particular, that much University of Chicago economics had nothing to do with the money supply or monetary economics. Stigler's research was a leading case in point. His body of work, most notably that in price theory, took place in areas that, after the early 1950s, were outside Friedman's own main field of specialty.

[404] In A&C Black (1987, p. 621), however, Friedman indicated that he had kept up his membership of the city's Quadrangle Club. It was outside this club's location that, during their overlapping years at the university, the well-known photograph of Friedman and George Stigler walking side by side had been taken. See *Chicago Tribune,* October 24, 1982*a*.

During his years there, Friedman knew about a good deal of the economic research outside his own field taking place at the university. He read a considerable amount of this work and offered substantial, if sporadic, comments on it over the years to its authors. This was particularly so in the case of Stigler's research. Nevertheless, from the 1950s to the 1970s, Friedman's exposure to Stigler's work on price theory and other matters was as a consumer-reader, occasional workshop attendee, and interlocutor, but not as a fellow researcher in the field or as someone who kept closely in touch with the evolution and details of the associated research literature.

Another difference between the two longtime University of Chicago figures lay in Friedman's celebrity. Stigler, despite being well known in the economics profession, was not a household name like Friedman. Stigler certainly provided a large amount of commentary on economic news in media interviews and had had forays into public policy. The statement by Assar Lindbeck at the time of the Nobel award that Stigler "has not figured in the public debate" (*Chicago Tribune*, October 24, 1982*b*) was certainly not defensible as being literally accurate.[405] Lindbeck's remark nevertheless brought out an important difference between Stigler and Friedman, as Stigler had not established himself as an economist in the public square in the Friedman mold. Indeed, he had an ambivalent—if, on balance, encouraging—attitude toward Friedman's devotion of considerable energy to public-policy activity (see Nelson, 2020b, Chapter 15). Stigler brought this attitude to the fore again soon after receiving his award, when he contrasted his interest in understanding the reasons why economic arrangements—particularly the part played by government—had arrived at their current state with the fact that, as he put it, "Milton thinks the world ought to be saved, and he'd love to play a part in it." (*Chicago Tribu*ne, October 24, 1982*a*.)

Certainly, Stigler was a better-known economist to the public than the bulk of his post-1976 University of Chicago colleagues. It was true, nevertheless, that—as a 1986 newspaper piece on the current economics scene at the university put it—the University of "Chicago really hasn't had a publicly recognizable name for 10 years since full-time economist, part-time media star Milton Friedman left for the West Coast." (*Newsday* (Long Island, New York), October 19, 1986.)

In light of Friedman's greater celebrity, Stigler's friendship with the previous Nobel winner was inevitably played up in media coverage of Stigler's award. Stigler himself mentioned it in an

[405] In the same remarks Lindbeck stated that Stigler "has never been an economic government adviser." This, too, was of very questionable literal accuracy, in light of Stigler's participation in government commissions or committees on price statistics and regulatory policy during the 1960s.

interview that he gave on the day of the prize announcement. "I'm sitting in an apartment that used to belong to Milton Friedman," Stigler, who had taken up the Friedmans' residence of 5825 Dorchester Avenue in Chicago when they gave it up in 1976/1977. "He's a very good friend of mine." (*Muncie Evening Press* (Indiana), October 20, 1982.)[406] Friedman bolstered the public association of the two when he flew from the West Coast to attend a dinner that the University of Chicago's business school held in Stigler's honor on January 18, 1983.[407]

Price theory and antitrust policy

But the fact was that the Stigler award was received for research in areas of work separate from those for which Friedman had won his. Recognizing this, a news article that appeared in the days after Stigler's prize was announced, and that amounted to a broad overview of their association as colleagues and friends, noted that in the previous three decades "the two economists have worked mainly in entirely different fields." (*Chicago Tribune*, October 24, 1982*a*.)

The formal citation for Stigler's 1982 Nobel award reflected the separation of Friedman's and Stigler's area of specialty: "his seminal studies of industrial structures, functioning of markets[,] and causes and effects of public regulations." (*Fort Lauderdale News* (Florida), October 20, 1982.)

It was true, of course, that Friedman had been a price theorist before he became a specialist in the behavior of the aggregate economy. Nevertheless, Friedman's Nobel had been exclusively for his work in the latter field. Friedman's last major pieces of research in price theory reached print in the mid-1950s. And this work focused on household utility and on agricultural prices—areas far from the economics of the firm with which Stigler became most associated.

Correspondingly, when discussing his years from 1958 onward as a professor at the University of Chicago—a period in which "Friedman was an ascendant figure in world economics" with his already-published consumption-function research and his ongoing "work on monetarism"—Stigler (1986, p. 100) noted: "My main work was on industrial organization."

[406] By later in the decade, however, Stigler had moved to Flossmoor, a suburb in the Greater Chicago area but outside the city proper. See A&C Black (1987, p. 1678).

[407] Information on the event was provided to the author by Gloria Valentine (personal communication, May 13, 2014). The event was held at Chicago's Ritz Carlton Hotel. Prior to the dinner, Friedman, Stigler, and another University of Chicago Nobel economics winner, Theodore Schultze, gave a joint press conference (*Chicago Sun-Times*, January 19, 1983).

After the mid-1950s, even Friedman's most prominent written product in price theory in the quarter century—his *Price Theory* text, published in 1962 and issued in a somewhat revised and expanded form in 1976—further confirmed the paucity of his collaborative work with Stigler. For, instead of writing a price-theory text together, they each wrote their own guide to price theory. And, reflecting his ongoing activity in price theory, Stigler was much more active than Friedman in refining his textbook. Stigler would note that he had produced "numerous revised editions" of his 1946 *Theory of Price* (A&C Black, 1987, p. 1677). Indeed, its latest, fourth, edition would appear more than forty years after the original: see Stigler (1946, 1987).

The Nobel citation of Stigler quoted above referred to Stigler's research on regulation. Part of this work covered the subject of antitrust, and Stigler was well known for being—as he later put it—among the economists who over time had "lost… our enthusiasm for antitrust policy" (Stigler, 1993, p. 401). Although, in discussing Stigler's research, Schmalansee (1983, p. 82) made only passing reference to "an interesting and provocative attempt to measure the actual effects of U.S. antitrust policy," it was this work (Stigler, 1966) and related writings that identified Stigler, from the 1960s onward, as being primarily critical of antitrust law.[408]

Antitrust, too, has been an area in which Stigler's views and those of Milton Friedman have tended to be unduly confounded. In fact, as Nelson (2020a, Chapter 4) and Chapter 4 of the present book have discussed, Friedman's shift to a stance that was primarily critical of antitrust law was a process that only really reached its completion in 1973–1974—well after Stigler's own move had occurred. And Friedman's criticism of antitrust laws remained more cautious and less thoroughgoing than Stigler's. Friedman's very limited amount of public statements—particularly in his public writings, as opposed to remarks solicited via interviews or correspondence—brought out this fact, while also reflecting the reality that antitrust was not his field of research.

And alongside those criticisms of antitrust policy that he did voice, Friedman retained support for some basic antitrust laws—in particular, laws that made contracts to restrain trade or competition not legally enforceable. In this connection, he had remarked in 1977: "The real value of free enterprise, in my opinion, is not that it leaves anybody free to do what they want

[408] As it happened, Stigler (1966) appeared in the same issue of the *Journal of Law and Economics* as an article by Friedman (1966) on a totally different subject (interest rates and the demand for money).

Stigler had previously been an advocate of breaking up firms and had made interventions to this effect in public debate (see, for example, *The Jersey Journal* (Jersey City), June 5, 1950). "But that was Stigler in his youth," Claire Friedland observed (interview, October 27, 2014). "And he took that back." See also Stigler (1988, pp. 97–100).

but that it leaves anybody free to set up an enterprise." (*New Guard*, April 1977, p. 9.) He elaborated on this point in a speech delivered in May 1983 that was subsequently the basis for an op-ed by Friedman that received wide newspaper syndication in late November of the same year.[409] In this piece, he urged "distinguishing between a correct and a false meaning of free enterprise."[410] The correct meaning was "that anybody shall be free to set up an enterprise," while the incorrect meaning was "that business enterprises shall be free to do what they want." This was a distinction that Friedman had stressed thirty years earlier, when he was much more enthusiastic about antitrust law.[411] The 1983 op-ed implied that, in the specific area of restrictions on trade, Friedman's views about antitrust law had not changed *too* much over the years. "Free enterprise does not mean the freedom of an enterprise to do what it wants… It means that they are not free to join in collusive agreements with others."

Public choice theory—and applying it to monetary policy

Notable in Stigler's official Nobel citation was what was *not* included: his application of the theory of public choice to U.S. politics. At the start of the 1980s, Friedman had suggested that Stigler would win the Nobel in economics on the basis of his work on *both* firms and elected officials. The grounds for the award, he forecast, would be "Industrial Organization; Economic Analysis of Politics."[412] Similarly, when Stigler's Nobel was announced, Friedman remarked in a telephone interview with the Associated Press: "He has been one of the pioneers in the application of economic analysis to problems of political science—his work has stimulated a whole series of studies by many people around the world." (*The Times-Picayune* (New Orleans, Louisiana), October 21, 1982.) Likewise, a few days later, the *Chicago Tribune* piece (October 24, 1982*b*) on Stigler, in making the case that he had been more involved in public-policy discussions than some accounts had suggested, noted that "he is widely regarded as a pioneer of the idea that a political marketplace operates in the United States as well as an economic marketplace."

Yet the Nobel citation quoted above had narrowed Stigler's contributions to the analysis of regulatory policy. Even the (non-official) elaboration of the citation released by the Nobel

[409] The speech was given in Los Angeles on May 21, 1983, to the Americanism Educational League. Friedman was speaking at an event centered on an essay-writing competition that the organization had run and had named after him. See Valentine (1987, p. 547).
[410] *The Plain Dealer* (Cleveland), November 25, 1983. Some years earlier, he had expounded the same basic argument in Friedman (1977d, p. 7).
[411] See Friedman (1952a, pp. 12–13).
[412] In Wallechinsky, D. Wallace, and I. Wallace (1981, p. 417).

officials explicated this part of the award by reference to regulatory capture rather than to the analysis of politicians' or regulators' motives (see Royal Swedish Academy of Sciences, 1983, p. 63).[413]

In contrast, the explicit application of the theory of public choice to policymakers' motives was an approach that energized Friedman's thinking in the 1970s and 1980s (see the preceding chapters as well as Nelson, 2020b, Chapter 9). In view of his move away from research, his interest in this approach was manifested more in his writings on current policy and his related public statements than in formal research.

As for Friedman's applications of public-choice arguments, through the mid-1980s this was to a large degree in the nonmonetary areas of policymaking. The *Free To Choose* book had appealed to public choice mainly in discussing the perseverance of the bureaucracy and in considering reasons why pressure might increase for expanding government services. This perspective had buttressed Friedman's previous concerns about inefficiencies in the provision of transfer spending. Whereas his longstanding interest in the negative income tax mainly concerned inefficiencies in the supply of transfer spending and its effect on incentives, Friedman by the early 1980s was voicing increased concern that "rent-seeking" behavior was creating a demand for transfer spending. At a conference in Los Angeles in March 1981, he credited the public-choice literature with bringing this phenomenon into focus (see Kitch, 1983, p. 210). Similarly, in the *Tyranny of the Status Quo*, public-choice arguments underlay some of the Friedmans' explanation for the difficulty in implementing a mandate to reduce the size of the public sector. They also linked this process to the U.S. productivity slowdown, with the book referring to the "roadblocks to productivity and growth that are generated by the political market."[414]

These applications had some overlap with the analysis of regulation and public policy that had been part of George Stigler's research. During the 1980s, however, Friedman's high regard for public choice theory was also felt, to a much intensified degree, in his discussions of an area that was Friedman's specialty and was not one of Stigler's: monetary policy.[415]

[413] Although it did refer to Stigler's efforts in "extending the sphere of application for the basic assumption of economic theory," this discussion limited itself to referring to "legislation," rather than politics or politicians, as the variable now being brought itself into the analysis (see Royal Swedish Academy of Sciences, 1983, p. 63). Schmalansee (1983, p. 84) stated more explicitly that "Stigler [1971] extended the economist's fundamental assumption of self-interested, rational behavior to the political arena."

[414] Friedman and Friedman (1984, p. 104; 1985, p. 102).

[415] Another area that Friedman commented on frequently in the 1970s had been energy policy. Friedman's usual stance had been to express puzzlement and exasperation at why regulators had responded to the first oil shock. He had, however, also pointed to some domestic firms' benefits from existing energy policy regulations (see Chapters 9

One such indication of this focus was Friedman's July 1981 Money, Credit and Banking lecture, in which he named Stigler among those who had been "analyzing bureaucratic behavior, not in terms of stated objectives, but in terms of the self-interest of the bureaucrats."[416] Friedman indicated that he was supportive of viewing monetary policy in these terms, even though the *Monetary History* had not done so. He noted that that book had, in fact, been criticized by public choice theorists for attributing to the Federal Reserve the standard, non-self-interested, policymaker objective of macroeconomic stabilization.[417] Friedman was now apologetic regarding this feature of the *Monetary History* (although, as discussed below, the need for such contrition was highly questionable, and the *Monetary History*'s focus on conceptual mistakes by policymakers seems warranted).

These 1981 remarks in favor of a public-choice approach were published in the *Journal of Money, Credit and Banking* in 1982—and so was an example of Friedman applying public-choice ideas to monetary policy in a research paper, albeit not one conducting a formal analysis. His increased orientation toward looking at monetary policy in a public-choice context was also felt in a number of public-policy writings and commentaries that Friedman made from 1982 to 1986, reflecting what Friedman described to his former student David Lindsey in 1984 his "increasing conversion to the view that the operations of bureaucracies are best understood in terms not of a public service perspective but of a self-interest perspective—the public-choice approach."[418]

Lindsey, being a senior Federal Reserve Board staff member, was one of those whose monetary analysis was often under challenge from Friedman. Friedman's focus on public choice theory, in a sense, increased the targeting of Federal Reserve personnel like Lindsey, as Friedman's *JMCB* article had criticized "the persistence and importance of bureaucratic inertia in the Federal Reserve System."[419]

That said, and as discussed in the previous chapter, Friedman's criticism of the Federal Reserve hierarchy apparently did not apply primarily to its economist staff. He clearly regarded the personnel employed at the Federal Reserve as, on the whole, having over time discouraged

and 10 above) and stressed that the U.S. oil industry had encouraged government-imposed restrictions on market forces in the past (see Chapter 3). Yang (1977, p. 8) had cited Stigler (1971) as having provided a theory of regulation, one emphasizing regulatory capture, that Yang argued fit developments in the oil industry well.

[416] Friedman (1982c, p. 115).

[417] Friedman (1982c, pp. 114–115).

[418] Letter from Milton Friedman to David E. Lindsey, November 2, 1984 (Federal Reserve Board records), p. 1.

[419] Friedman (1982c, p. 114).

changes in monetary policy arrangements and, in particular, had persuaded new Federal Reserve chairs not to make them (see Nelson, 2020a, Chapter 8). In applying these criticisms during the 1980s, however, he regarded—probably overestimating the convergence by Federal Reserve economists toward his own views—much of the economist staff as being on the same side as himself on monetary issues (particularly in the area of monetary control). In that light, Friedman directed his criticism at those officials most closely involved in the implementation of monetary policy and monetary-policy-regulated regulations, while also citing the Federal Reserve governors themselves as a source of inertia. His zeroing in on these senior figures was evident in his remark (*Newsweek*, December 27 1982): "An administrative job in the Federal Reserve System is the closest thing to secure employment this country offers." Friedman also believed, however, that Federal Reserve economist staff were also a source of opposition to his monetary-growth rule, in part because its adoption would reduce the need for their services.[420]

In applying his public-choice-based criticism to the Federal Reserve, Friedman seized on themes related to accountability. One was what he perceived as "the absence of a bottom line" for the U.S. central bank.[421] As stressed in earlier chapters, Friedman at an early stage came to the conclusion that the Federal Reserve had assimilated monetary targeting into its processes without changing its policy choices very much. He consequently did not consider monetary targets to be much of a constraint on Federal Reserve behavior. Nor did he perceive there to be a concrete financial constraint on the country's main monetary authority. In contrast to the case of Congress, for which he had long stressed tax revenues as providing something of a constraint on its desire to spend, Friedman suggested that the Federal Reserve "is not subject to an effective budget constraint" because it printed its own budget.[422] This feature of being able to generate its own financial resources at will was essentially always true of central banks. What was new was Friedman's highlighting of this feature as one motivation for understanding the course of U.S. monetary policy.[423] By this stage, he evidently believed that the point that the Federal Reserve generated the resources it needed by printing money was something worth emphasizing—as he repeated the point during 1982–1985 across a variety of formats, including in *Newsweek* and in

[420] For example, he clearly had Federal Reserve economists in mind when he stated (*The News Courier* (Charleston, South Carolina), October 26, 1982, p. 13): "Employment at the Federal Reserve is the nearest thing we have in the United States to guaranteed lifetime employment. If they did what I suggest, there would be fewer jobs for them, both in the System and out of the System."
[421] Friedman (1982c, p. 114).
[422] Friedman (1982c, p. 114).
[423] His previous emphasis on the central bank's ability to acquire real resources by creating money had primarily been in reference to the manner in which this potentially provided revenues (both formal tax revenues and effective revenues in the form of monetization of debt) to the central government.

interviews.[424] Another item that Friedman argued reduced accountability was the lengthy (14-year) character of the terms of Federal Reserve Board governors.[425]

It was, however, still another aspect of the Board governors' role, and particularly that of the Chair, that Friedman stressed in pressing his public-choice-based criticisms. In 1984, Friedman stated that he was impressed with the "continuity of Fed policy" and so now gave different personalities less weight than he had once done in his assessments.[426] When explaining the continuity, however, he did rely on an argument that rested on what he perceived a common element across the personalities of Federal Reserve governors—namely, what Friedman unflatteringly characterized as their wish to have a high public profile.

In advancing this argument, Friedman suggested that Federal Reserve policymakers resisted monetary policy rules and, in particular, his constant-monetary growth rule because it would make the U.S. central bank's position in public life too routine and heading it less prestigious. In 1985, he suggested that, if the officials carrying out monetary policy were simply required to follow his rule, "certainly they would not be the subject of daily speculation in the financial press, of regular attention on the daily TV news shows."[427] Its adoption was unattractive, he suggested, because it would deprive the head of the Federal Reserve of being regarded "as the chairman of the Fed now is, 'the second most important man in the country.'"[428] On that basis, he concluded that it was "not in the self-interest of the Federal Reserve hierarchy to follow the hypothetical policy" of constant monetary growth.[429] And, in line with his ambivalent attitude toward Wall Street and financial markets, Friedman suggested that imposition of a policy rule would be resisted on self-interest grounds by many financial community participants, too, as it would reduce the value of Fed-watchers: "Have you any idea of the number of people who are paid six-figure salaries because they were formerly employed by the Fed and therefore are presumed to have an insight into the way [it] operates?" (*The News-Courier* (Charleston, South Carolina, October 26, 1982, p. 13.)

[424] See *The News Courier* (Charleston, South Carolina), October 26, 1982 (p. 13); *Newsweek*, December 27, 1982; and Friedman and Friedman (1984, p. 94; 1985, p. 94).

[425] Friedman (1982c, p. 114).

[426] Friedman (1984b, p. 45).

[427] Friedman (1985b, pp. 60–61).

[428] Friedman (1985b, p. 61). Friedman made similar statement in his Mont Pelerin Society comments of September 1984 (Friedman, 1985h, p. 16) and in his talk on July 2, 1985, to the Western Economic Association (Friedman, 1986d, p. 3). Although Bennett McCallum was far less enamored of the public-choice approach to monetary policy than Friedman was, McCallum (1989a, p. 48) also invoked the prestige of being the second-most-important figure in the country as a reason he believed that Federal Reserve chairs resisted the introduction of a policy rule.

[429] Friedman (1986d, p. 3).

In its application to central bankers, this line of thinking was not one that had originated solely from Friedman's immersion in the public-choice literature. Even in the Bretton Woods era, he had suggested that central bankers (as well as leaders of treasury departments) enjoyed the national attention they received when they were perceived as helping to resolve international monetary crises.[430] Indeed, Friedman's invocation of this argument during the 1960s had irritated Paul Volcker, despite Volcker's own inclination at the time to agree that a more flexible exchange-rate regime was necessary (see Silber, 2012, pp. 57–58). Friedman's position regarding central-banker prestige in the 1980s was, however, different in important respects from that he had invoked under fixed exchange rates. In the 1960s, he had been discussing it the international monetary arena and had been asking why policymakers might resist a market mechanism (floating exchange rates). In the 1980s, in contrast, he was considering domestic monetary management and purporting to have found the reason why, against a background of floating exchange rates, policymakers were opposed to the introduction of a domestic monetary rule.

It is not hard to see that Friedman was carrying the public-choice argument too far. His 1984 remark on the continuity of Federal Reserve policy—a motivation for his belief in the validity of public-choice arguments—appears jarring in retrospect, in light of the permanent change in monetary policy regime now regarded as having been achieved by Paul Volcker. In the later months of 1984, Friedman also stated that he was now convinced that opposition to his rule reflected not "the wrongness of that advice" but "the simple fact that the self-interest of the Federal Reserve System would not have been served by adopting that advice."[431] Yet the particular version of the rule he was advocating during that period—a constant M1 growth rule—would be shown to be inappropriate for the circumstances of 1985 and 1986, and, in September 1985, in light of the M1 velocity instability that was already evident, Federal Reserve Bank of Boston president Frank Morris was able to pour scorn on the fact that Friedman "argues that Federal Reserve officials reject this advice [to make M1 growth constant] only because it would eliminate their power."[432]

Friedman's repudiation of explanations of past monetary policy mistakes in terms of "ignorance of the powers that be" also went against the grain of his previous work. And that work—especially the *Monetary History* with Schwartz—in emphasizing analytical mistakes on the part

[430] See his remarks in Friedman and Roosa (1967, pp. 15–16). See also Instructional Dynamics Economics Cassette Tape 5 (November 1968).
[431] Friedman (1985h p. 16).
[432] Morris (1985, p. 4; p. 225 of 1987 reprint). Morris was speaking on September 11, 1985.

of policymakers, would stand up better than the more impressionistic and sweeping public-choice-based evaluations of monetary policy performance that Friedman would make during the 1980s.[433]

When he put things through a public-choice lens, the picture that Friedman relayed of Federal Reserve policymakers was one of ranking prestige and attention-seeking over economic-stabilization goals was something that also struck numerous other economists as a caricature. Probably referring to Brunner and Meltzer's work as well as Friedman's, Franco Modigliani (1986, p. 223) lamented the fact that the mid-1980s were an "era in which monetarists and libertarians have been making so much of the notion that policymakers are concerned with their own rather than the general welfare."[434]

Athanasios Orphanides undertook a reexamination of Friedman's writings on monetary matters, as part of his work on the staff of the Federal Reserve Board in the mid-1990s. In the course of this examination, as well as being struck by Friedman's work on model uncertainty and the limitations of stabilization policy, Orphanides noted the presence of

> the other element that came through in some of his writings, which was the incentive structure of policymakers. Unfairly, in my view, he believed that policymakers care mostly about not being blamed for things; and, as a result, they are driven to take decisions not because they're the best for the objectives they have to meet, but to avoid personal blame and advance personal agendas. [But] I think that when it comes to the Board of Governors, he was very unfair on this. … [I]n the United States, it's clear that many so-called "technocratic" decision-makers [do] operate like that, but … I think that the [Federal Reserve] Board, or the Federal Reserve System, has a structure that keeps the Fed much more honest in this regard than virtually any other institution I'm aware of. So I think on that one, he was a little bit unfair. (Athanasios Orphanides, interview, June 27, 2014.)

It is appropriate to conclude that U.S. monetary policy was not the ideal subject of public-choice theory that Friedman's later inclinations implied.[435]

[433] The quotation is from Friedman (1985h, p. 16).

[434] As his surrounding comments implied, Modigliani was also unimpressed with the application of this argument to the makers of U.S. fiscal policy. He did not shift from this position when James Buchanan won the Nobel award in economics in October 1986 for his contributions to public-choice theory. Charles Steindel (interview, December 3, 2015) noted that "when Buchanan won the Nobel Prize, Franco was quoted as saying, 'Oh. It looks like they're expanding into a general social science award.'"

[435] George Stigler would also be taken to task on the ground that he had deployed public-choice approach excessively. Donald Winch (interview, September 22, 2014) stated, "I was deeply critical of Stigler," on account of Stigler's portraying Adam Smith's economics as strongly aligned with the public-choice approach. Winch argued

Stigler's distance from macroeconomics

Notwithstanding the heavy credit that Friedman gave to Stigler for the development of public-choice theory, Stigler was not a prime mover in that theory's applications to macroeconomics—and, in particular, to economic-stabilization policy.

Stigler's longstanding distance from macroeconomics and his very limited familiarity with even the work in that field of his close friend Friedman were underlined after he received the Nobel award. Inevitably, in interviews Stigler was asked about the current U.S. economy and national economic policy. In great contrast to his friend's views, and more in line with impressions about the economy's behavior that he may have acquired in business-school circles, Stigler linked the prospects for economic recovery not to monetary policy but to the stock market: "I think we're at the pit—and it [employment] will begin to rise. But it won't be rapid unless the stock market continues to rise. If the stock market goes up, employment will go up, too." (*Muncie Evening News* (Indiana), October 20, 1982.)

Stigler's use of the more graphic terminology "pit" instead of the standard term of "trough" had an echo in an appearance Stigler made before reporters when he was invited to the White House a week after receiving his Nobel. He again affirmed: "I believe in things like the stock market… as a predictor of the business revival." But—in a more newsworthy comment—Stigler described the current recession, though likely near its end, as being a "depression," and he suggested that it was merely "linguistics" to call it by that label rather than "recession" (*Atlanta Journal*, December 28, 1982).

In his memoirs, Stigler (1988, p. 137) would emphasize the discomfort that his "depression" label caused for the administration officials who hosted him but did not really consider the issue of whether the label was, in fact, accurate. Similarly, Leeson's (2000, p. 136) reference to the White House incident stated that Stigler's remark was embarrassing for the administration and indiscreet, but Leeson made no allowance for the possibility that Stigler's diagnosis that the U.S. economy was in a depression in 1982 was inaccurate.

In fact, of course, mainstream macroeconomics strongly points to Stigler being seriously in error

that Stigler in particular neglected Smith's (1759) *Theory of Moral Sentiments* and argued more broadly with regard to Smith's thinking, including that expounded in Smith (1776), that "Smith is a good deal more complex than these modern theories of public behavior would allow. There's a lot more going on, because it's a lot more ambitious by way of an intellectual enterprise than most modern social scientific efforts are in this respect."

in applying the “depression” terminology in the context of the U.S. economy’s state in 1982. About twelve months after Stigler’s remark, James Tobin did describe the preceding years as having seen “the Volcker depression” (*New York Times*, October 12, 1983). But this language was seen as jarring by numerous others. Even many Keynesians highly critical of aggregate-demand policy settings in the early 1980s were very disinclined to describe 1981–1982’s economic downturn as a depression. Instead, they, and posterity, classified it as the severest of the postwar recessions to date.

In this connection, Walter Heller, speaking at around the time of the end-1982 trough of the recession, described U.S. real GNP as being about 10.5 percent below potential GNP.[436] This was actually significantly shallower than what, at the time, the 1975 trough in economic activity was believed to have generated. Furthermore, subsequent revisions put the trough at the output gap in late 1982 at about minus 7 percent (see Orphanides, 2003, p. 642, Table 1).

In terms of unemployment, the 1981–1982 generated a rate above 10 percent from September 1982 through June 1983, with a peak of 10.8 percent in November and December 1982. In 1975, Paul Samuelson had tentatively suggested that a 10-percent-or-higher unemployment rate might qualify as a depression (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 158, late February/early March 1975). But that statement had preceded widespread acceptance that the natural rate of unemployment had risen by about 2 percentage points in the 1970s—and so any given unemployment rate implied fewer spare resources than previously.[437] Friedman stressed during 1982 that, although there was slack in the economy, the excess of the unemployment rate over its average in previous decades overstated the amount of slack. “Although recorded unemployment is at a 40-year high, that number by itself is very, very misleading… [because] we have changed the terms under which people are unemployed.” (*The News-Courier* (Charleston, South Carolina), October 26, 1982, p. 10.)

Samuelson was, at around the trough of the 1981–1982 recession, clearly deeply worried about the prospect of an extended period of economic weakness.[438] But his commentary during the

[436] In his appearance on *Wall Street Week*, Maryland Public Television, February 25, 1983 (p. 9 of transcript), Heller gave U.S. real GNP as being 300 to 350 billion dollars below potential—implying a midpoint estimate of the output gap of about negative 10.5 percent (using the nominal GNP value of $3,101.3 billion for 1982:Q4 reported in Council of Economic Advisers, 1983, p. 170, Table B-6).

[437] On the consensus that there was a rise in the U.S. natural rate of unemployment by about 2 percentage points from the 1960s to the 1970s, see Orphanides and Williams (2005) as well as Chapter 5 above.

[438] He was quoted as saying, in January 1983, that he was pessimistic about the prospects for economic recovery and that in midyear the country would be “reflecting on the lack of our courage in the winter of our discontent” (*Atlanta Journal and Constitution*, February 12, 1984, p. 1E).

subsequent recovery made clear that he, too, did not believe that the postwar period had, in fact, seen any depression in the United States (for example, *Wall Street Journal*, December 12, 1984, p. 1).

For his part, Friedman stressed in early 1982 that he continued to believe that the U.S. economy was depression-proof (*The Vancouver Sun* (British Columbia, Canada), March 24, 1982). He similarly told an interviewer the following October that, since his "Why the American Economy Is Depression-Proof" talk of nearly thirty years earlier, "I have been right so far, and I don't see any reason to change the line now." He added: "We cannot have a major depression, in my opinion, without a sharp decline in the quantity of money—and we're not going to have a decline in the quantity of money." Asked in the same round of interviews, another investigator of the money/depression relationship—28-year-old Ben Bernanke, at that time spending a year at the Hoover Institution—remarked that "the popular idea that the current situation is a few inches away from the Great Depression is erroneous." (*State-Times* (Baton Rouge, Louisiana), October 26, 1982, p. 1A.)

The Bernanke and Friedman remarks, appearing publicly virtually on the eve of the commentary that Stigler gave at the White House, would underline how little awareness the new Nobel laureate had of modern work on business cycles and of the terminology and classification schemes prevalent in that work. Nevertheless, Stigler's less strict use of the word "depression" in reference to recessions had some limited justification, in somewhat older research on the business cycle—including writings by Milton Friedman.

In order to bring out this point, it is worth considering an organization that, by 1982, both Friedman and Stigler had departed but with which they had once been closely affiliated. During the 1980s, Stigler would list the NBER as the first of a number of outlets to which he had contributed writings over the years (see A&C Black, 1987, p. 1677). His movement in NBER circles during the 1940s—when his work was less further away from macroeconomics than it later became—may have been a factor in Stigler's readiness to use the term "depression" in 1982. In the older NBER work of the 1930s through the 1950s, and much other macroeconomic work of that era, the term "depression" was used much more freely than it later was. Although Stigler's (1947) own work for the NBER on business cycles mostly refrained from using the term "depressions" except in the context of the interwar period, and Burns and Mitchell (1946, p. 455) opted for the term "contraction" to cover both recessions and depressions, other work, both NBER and non-NBER, had used the term "depression" more routinely to cover a wide class of recessions. Indeed, at just the time when Friedman published his work on the U.S. economy being depression-proof, and distinguished between depressions (which he did not foresee occurring again) and smaller economic contractions (which he expected to recur), the NBER's Geoffrey Moore (1954, p. 18) seemed open to using the term "depressions" for recessions generally, provided that one distinguished between "mild depressions" and such episodes as "the severe depressions of 1929–1932, 1937–1938, and 1920–1921."

Even at the time when Moore was writing, and to an even greater extent subsequently, the term "mild depressions" had become jarring. As Friedman's distinction between contractions and depressions underlined, the term "depression" was increasingly being reserved for deep contractions in output. The practice of labeling shallower contractions "depressions" was becoming far less acceptable—especially in light of the fact that "mild depressions" was, to many, a contradiction in terms.

Nevertheless, Friedman himself was not consistent on this point, even in the decade or so after his 1954 talk. Haberler (1984, p. 123), in fact, suggested that the sharp recession/depression distinction was a modern one and that Friedman himself had come up with the "mild depression"/"deep depression" categorization and had put recessions in the former category.

Haberler's characterization attributed the terminology too strongly to Friedman: in actuality, the term "mild depressions" was already in use when Friedman came onto the economic-research scene in the 1930s.[439]

Nevertheless, it took Friedman a long time to stop using the term "depression" quite liberally. In his 1945 *Income* monograph with Simon Kuznets, the authors had referred to economic contractions as "cyclical depressions."[440] In his 1950 public statement with Paul Samuelson and others, the contributors had made reference to "mild depressions."[441] More surprisingly, in the decade after his "Depression Proof" talk of 1954, Friedman continued occasionally to use the "mild depressions" term as a label for recessions. For example, in his May 1959 Congressional testimony, Friedman remarked: "I think that one can in fact distinguish historically among two different classes of depressions or of recessions, relatively mild depressions and relatively deep depressions. The kinds of depressions or recessions we have had since the end of World War II have been relatively mild depressions."[442] He also used the "mild depressions" terminology in

[439] See, for example, Slichter (1937).
[440] Friedman and Kuznets (1945, p. 297).
[441] Despres and others (1950, p. 525).
[442] From Friedman's testimony of May 25, 1959, in Joint Economic Committee, U.S. Congress (1959a, p. 619).

his NBER/*JPE* study of monetary relations that appeared in the same year.[443] And in their "Money and Business Cycles" study of 1963, Friedman and Schwartz referred to "the minor U.S. economic fluctuations that we have classified as mild depression cycles."[444] In their contribution to the same year's NBER annual report, they likewise stated: "Minor economic fluctuations are mild depression cycles."[445]

Friedman was not alone in clinging to the term "mild depressions" to refer to recessions. For example, James Duesenberry (1963, p. 21) referred to the "mild depressions such as we have had in the postwar period," while a 1965 undergraduate economics textbook stated (Guthrie and Wallace, 1965, p. 227) stated that "[l]ess devastating [economic] declines [are] often described as recessions or mild depressions." Nevertheless, by the 1960s, such terminology was obsolescent in economic discussions. Reflecting this fact, Phillip Cagan's (1965) study, while sticking to the NBER's "mild cycle"/"severe cycle" classification scheme, referred to "mild contractions" (pp. 90, 103, 311) rather than to mild depressions.[446] From the mid-1960s onward, Friedman, too, was much more circumspect in using the "depression" label in his commentaries.

George Stigler had himself used the term "depressions" as a catch-all term that included recessions in a 1951 discussion.[447] Being decidedly out of touch with the course of macroeconomic discussions over the subsequent three decades, he was likely largely oblivious to how provocative it had become to describe postwar recessions, including 1981–1982's, as a depression, until he experienced the reaction to his use of the term in the White House in October 1982.

It should be emphasized that, although Stigler's "depression" label likely was, taken in isolation, something that could give encouragement to advocates of highly expansionary demand policies, his surrounding commentary in 1982 and 1983 would have given such advocates little comfort. In the hours after receiving his Nobel award, Stigler stated that "the rise [in unemployment] was the price we had to pay to fight a persistent inflation" (*Muncie Evening News* (Indiana), October 20, 1982). And he later added, "The natural rate of unemployment in this country may be between 5 and 7 percent," while stating that 3 to 4 percent was "now [an] unnatural rate" because

[443] See Friedman (1959, pp. 342, 343).
[444] See Friedman and Schwartz (1963b, p. 55).
[445] Friedman and Schwartz (1963c, p. 63).
[446] Cagan's (1965) use of the NBER's traditional mild-cycle/severe-cycle dichotomy was stressed by Meltzer (1967b, p. 171). The review of Cagan's book in the London *Economist* (April 2, 1966), however, used an NBER practice that Cagan (1965) did not actually employ by referring to recessions as "mild depressions."
[447] Stigler (1951, p. 127).

the accumulation of laws and regulations had raised the full-employment rate so much.[448]

DAVID STOCKMAN

President Reagan turned seventy a couple of weeks after taking office in January 1981. Consequently, even before he was seriously hurt in an assassination attempt on March 30, 1981, there was speculation that the president would not run for a second term. Asked about this possibility—in an interview published just after, but likely conducted just before, the assassination attempt—Friedman gave his own preference for a substitute Republican presidential candidate in 1984: David Stockman, a former Republican member of Congress who was director of the Office of Management and Budget (OMB) in the new administration. Friedman characterized Stockman as brilliant and a charismatic leader on economic issues (*Boston Globe*, April 1, 1981, p. 45).

A couple of weeks later, when speaking in Sydney on April 14 about the new U.S. administration's spending-reduction proposals, Friedman remarked: "In my opinion, they are far less than we need but, nonetheless, they are a major proposal—and the largest budget cuts that any president has proposed for many years. They are a move in the right direction, and they are intended to be only the first step in cutting back government spending. There are proposals for equally deep further cuts in successive fiscal years… I think with respect to [this] first element of President Reagan's program, he and his extraordinarily brilliant director of the Office of Management and the Budget, Mr. Stockman, have done a remarkable job in getting together a detailed proposal of this magnitude and this scope within the brief span of two months."[449]

Two years later, however, *Newsday* (April 11, 1983) took as a well-known fact "David Stockman's fall from grace." Although Stockman had remained head of the OMB over the intervening two years and would continue in that position until 1985, he had become badly out of favor among many Reagan supporters, after having established a reputation of being a critic of the administration's approach to fiscal policy—and, in particular, of its wish to maintain the 1981 personal income tax cuts and its rejection of winding them back for the sake of near-term deficit reduction.

Correspondingly, Stockman had come to be seen in a new and favorable light by critics of the

[448] *U.S. News and World Report*, January 31, 1983, p. 69 (reprinted in McClelland, 1983, p. 61).
[449] Friedman (1981e, p. 9).

administration. So even before Stockman's (1986) book *The Triumph of Politics* provided ammunition for critics of the administration, and portrayed economic policymaking in Reagan's first term in a mostly unflattering light, Paul Samuelson (1984, p. 6) could refer to "the old David Stockman" as someone he disagreed with and contrast that state of affairs with his more recent commonality of views with Stockman's.

Stockman's disaffection with the Reagan tax cuts and his publicly-known support for introducing substantial tax increases put him at odds with supply-siders and Friedman alike. But, in the 1980s, only the former group made repeated public attacks on Stockman. Friedman ceased his giddy praise of Stockman, but he stayed mostly above the fray in the period from late 1981 onward when Stockman was a target of other supporters of Reaganomics. This reticence may have been partly a result of the fact that Stockman was generally sparing in his criticism of Friedman—a situation that would change drastically after Friedman's death, as discussed below. It may also have reflected the likelihood that, although Friedman disagreed with Stockman on some of the matters that led to Stockman's break with the supply-siders—particularly on the question of whether a tax increase was warranted in response to the emergence from 1982 of larger budget deficits—he agreed with Stockman on other points, including the need for a concerted spending-reduction program, the likelihood that a general income tax cut would reduce, rather than augment, federal revenues, and the imperative—due to high inflation—to start the Reagan years off with a recession rather than with immediate growth in output.

Stockman becomes an internal critic

Responding to the observation that Friedman had a favorable impression of Stockman during the early stages of the Reagan Administration, Arthur Laffer stated (interview, June 10, 2013): "We all did." Indeed, as of early 1981, Stockman was a member of Reagan's economic team whose views struck a chord with Laffer and Friedman alike.

Stockman had established a reputation as "an ardent, longtime supply-sider" (as he was described in *Financial Times* (London), February 18, 1981). Unlike numerous other supply-siders, however, he also espoused some longstanding monetarist positions. Notably, on inflation, Stockman's prescriptions seemed well aligned with the monetarist prescription: in his confirmation hearings, Stockman stressed the importance of restraining monetary growth while adding, "I don't see any real need for an incomes policy at all."[450] And whereas Friedman was

[450] In Committee on Governmental Affairs, U.S. Senate (1981, pp. 29, 68), the latter page being the source of the quotation. In keeping with his more general opposition to price controls, Stockman had been a strong advocate of

critical of many supply-siders for emphasizing tax cuts as an aim in itself and not also stressing the need for cutting public spending, Stockman made it a point to call for reform of and reduction in domestic federal expenditures (*The Sentinel-Record* (Hot Springs, Arkansas), January 13, 1981).

But a watershed in perceptions of Stockman would occur with an interview that he gave to journalist William Greider—one that Greider wrote up as a long article in the December 1981 issue of the magazine *The Atlantic*. Stockman was frank about his misgivings about the validity of the economic projections that the Reagan Administration had advanced in its early months. Although, as indicated above, Friedman praised the launch of a new fiscal program that Reagan and Stockman provided, Stockman likely had common ground with Friedman in having doubts about the early forecasts that the administration produced. One of the misgivings that Stockman's relayed—that the projections exuded too much confidence about restraining public expenditure—likely did not cut much ice with Friedman, who believed that spending cuts could be implemented on a largely across-the-board basis and so did not share Stockman's strong concern about the need to identify specific program-by-program cuts at the outset. But Stockman's reservations about the administration's *economy-wide* forecasts were likely substantially shared by Friedman. From a monetarist perspective, there were legitimate grounds for skepticism about those projections, as they largely sidestepped the recession that Friedman believed was necessary in order to get inflation down.

The most publicized Stockman remark in the *Atlantic* interview, long remembered afterward (for example, in *New York Times*, July 11, 1985) was to the effect that the administration had not proposed a cut in the top rate on (investment) income from 70 percent to 50 percent but wished the Congress to decide on such a reduction nevertheless. In retrospect, the cut from a 70 percent to 50 percent marginal rate was one of the least controversial aspects of the 1981 tax cut: there was genuine bipartisan support for such a cut, as evidenced by the advocacy of such a cut by figures like Michael Blumenthal and Joseph Pechman in the late 1970s; it was one of the rate reductions least likely to lower tax revenue overall; and, as earlier noted, marginal income tax rates above 50 percent have never been restored in the United States in the period since 1981. Stockman's description of the administration's wish to have the rate lowered as a "Trojan horse" move made it sound like a more clandestine and parochial tactic than it actually was.

oil price decontrol when he served as a member of Congress. See, for example, his various remarks in Committee on Interstate and Foreign Commerce, U.S. House of Representatives (1979).

Milton and Rose Friedmans were in Chile at the Mont Pelerin Society meeting when the news of the Stockman *Atlantic* interview broke in November 1981. At the meetings, Rose Friedman expressed incredulity that Stockman would have endorsed the notion that the Reagan domestic program was not really designed to raise national economic performance but instead was covertly aimed at attempting to help the wealthy. She suggested that the published interview had given the wrong impression of Stockman's views, including on the cut in the top rate from 70 percent to 50 percent (*State-Times* (Baton Rouge, Louisiana), December 11, 1981). The administration similarly gave Stockman the benefit of the doubt, and he was retained as OMB director. For his part, Milton Friedman both before and after the Stockman interview rejected the position that the Reagan economic policies were oriented toward supporting the wealthy. He regarded this as a caricature and pointed to aspects of the Reagan program that would protect or help low-income earners (see *Newsweek*, March 2, 1981, p. 33, and April 19, 1982).[451]

Arthur Laffer, in contrast to Rose Friedman and the administration hierarchy, took the tone of the *Atlantic* interview as accurately reflecting a change in Stockman's views. "I haven't been enthusiastic about David Stockman's role," Laffer remarked on television in April 1982. "In fact, I was a great admirer of David Stockman for a long time, and I was frankly quite disappointed in his interview in *The Atlantic*." He found fault also with Stockman's influence on economic policy since 1981, including for having been part of the decisionmaking that "delayed the tax cuts" by phasing in the main tax-rate reductions over 1981 to 1983.[452] Indeed, as the signs of renewed recession became clearer in early 1982, Laffer labeled the episode a "Stockman recession" (*St. Petersburg Times* (Florida), January 11, 1982, p. 3A). Friedman, of course, did not sheet home blame for the recession to Stockman: Friedman believed that the recession was due to monetary policy, not fiscal policy, and, in contrast to Laffer, Friedman believed that a recession was necessary in any event, for the purposes of disinflation.

Proponent of tax increases

As budget deficits emerged, Stockman quickly became an advocate of a tax increase and remained so for the remainder of his tenure at the OMB. This change of opinion put him at odds with Friedman. A temporary alignment of their policy prescriptions occurred, however, in August 1982 when Stockman wrote a *New York Times* op-ed backing the balanced-budget

[451] This remained his assessment in 1984, when Friedman said that over Reagan's tenure to date, "[t]he poor did not suffer because of spending cuts; the total amount of money being spent on programs for the poor has stayed as a relatively constant percentage of national income." (*USA Today*, April 11, 1984.)

[452] *Wall Street Week*, Maryland Public Television, April 9, 1982, p. 11 of transcript.

amendment before Congress that Friedman supported (*New York Times*, August 8, 1982).[453] Where Stockman and Friedman differed was on how to proceed in the event that, as was likely, the proposed amendment, and its automatic ceilings on the growth in government spending went nowhere. Friedman wanted the Reagan Administration to continue to incur deficits and to wait things out until greater support, or pressure, for spending restraint emerged, and he believed that too much of the blame for the increase in deficits had been placed on the tax cuts. Stockman, in contrast, wanted more immediate action on the deficit and saw tax increases as a means of achieving this goal. Stockman would conclude: "We overdid it in 1981 and cut taxes too much." (*This Week*, ABC, May 1, 2011.)

Stockman supported and promoted the 1982 tax increase—TEFRA—discussed earlier in this chapter. And whereas President Reagan reverted after TEFRA to opposition to tax increases, Stockman continued to negotiate with members of Congress after 1982 on proposed deficit-reduction packages that had a substantial tax-increase component. During 1983, the conservative syndicated columnists Rowland Evans and Robert Novak, who had become proponents of the supply-side position, expressed exasperation at what they saw as an instance of Reagan having "passively submitted" to negotiations centered on reducing the deficit through tax increases. As proponents within the administration of raising taxes, the two columnists named James Baker (White House chief of staff), Martin Feldstein (often a target of supply-siders until his departure as CEA chair in 1984), and Stockman (*San Diego Union* (California), January 21, 1983). In a later column, "Stockman Shaping U.S. Budget," Evans and Novak contrasted the "orgy of anti-tax rhetoric" that had sprung up among Reagan, Friedman, and Laffer at a recent PEPAB meeting with the reality that Stockman was representing the administration in talks on raising taxes (*Tyler Morning Telegraph* (Texas), April 28, 1983).

The Triumph of Politics *appears*

After leaving office, Stockman wrote an account of his time in the Reagan Administration titled *The Triumph of Politics*. Just ahead of its publication, one newspaper titled its editorial "Look for Stockman's Book on Discount Table." "Now and then, a political book will do well. *RN* [the Nixon memoirs] sold 300,000 copies in hardcover. But books by other ex-presidents and

[453] Like the Friedmans, who would emphasize that the balanced budget amendment was a limitation on spending, one that would "ultimately reduce" the public sector's role via a "gradual ratcheting down" in the government spending share of national income (Friedman and Friedman, 1984, pp. 55, 58; 1985, pp. 58, 60), Stockman's 1982 op-ed emphasized the intention of the amendment to reduce the public-spending share over time. Also like Friedman, Stockman's piece traced the productivity slowdown in the United States and other major economies to recent decades' increases in the government-spending and tax-revenue shares of output.

other recent political figures have fared poorly."[454] The editorial concluded: "There's little reason to expect Stockman's book to enjoy a kinder fate." (*Argus Leader* (Sioux Falls, South Dakota), April 11, 1986.) In fact, *The Triumph of Politics,* although it reportedly sold substantially less than its publisher had hoped (*Detroit Free Press*, April 6, 1987), entered the bestseller lists and gained an enormous amount of attention.

By relaying Stockman's assessment of the Reagan economic program as disorganized and lacking in principles, the book indicated that the 1981 *Atlantic* interview had largely conveyed his views accurately. The book was, in particular, highly critical of supply-siders, including those in the administration and those, like Arthur Laffer, who were basically outside the administration. Laffer's responded to the book sharply: "The Stockman book has a lot of interesting anecdotes, but the economic message is all wrong." As for Stockman's own record, Laffer remarked: "The Reagan revolution has not succeeded in bringing down government spending—and that's the one area David Stockman was in charge of." (*Tulare Advance Register* (California), April 26, 1986.)[455]

Columnist William F. Buckley Jr., in defending the administration against Stockman's criticisms, criticized the book for not addressing Friedman's opposition to higher taxes and instead "suggesting that anyone who opposed [increasing] taxes knew nothing about economics" (*Daily News* (New York), April 30, 1986). In fact, *The Triumph of Politics* not said much at all about Friedman, the references to him being primarily cursory and neutral. In terms of the monetarist/supply-side disputes within the administration, Stockman's book was supportive of the monetarist group and had criticized the supply-siders to task for the stand of opposition to monetary restraint that they had taken during the first eighteen months or so of Reagan's first term. So Stockman's posture at this time was not overtly anti-Friedman. Indeed, a biography grandiosely titled Stockman: *The Man, the Myth, the Future*, which appeared during the

[454] The editorial evidently categorized Stockman as a political figure, on account of his important role in policymaking, although he had not been an elected politician after 1981 and not a member of Cabinet as OMB director in 1981–1985. The editorial also evidently classified *Free To Choose* as an economics book, rather than a contribution by an active figure in U.S. policy.

[455] Broadcaster Louis Rukeyser implied that he had a similar view of Stockman. Conducting an interview on *Wall Street Wee*k after The Triumph of Politics appeared, he remarked: "Let's just talk about Mr. Stockman for a moment. As I understood his original assignment as the director of the Office of Management and Budget, his job was to get the federal government's [outlays] share of gross national product down from around 23 percent, which it reached under Jimmy Carter, to 19 percent. How good a job did he do?" (*Wall Street Week*, Maryland Public Television, May 23, 1986, p. 7 of transcript.) Replying to this question, Murray Weidenbaum declined to single out Stockman: "I wouldn't blame any one person. There's a lot of blame to go around. The Congress—both sides of the aisle, the president, Dave, the Cabinet officers who were fighting for their spending departments. And basically, the public, [who are] very ambivalent: the public that wants a balanced budget but doesn't want to see any of their pet programs cut." (*Wall Street Week*, Maryland Public Television, May 23, 1986, p. 9 of transcript.)

1986–1987 surge of public interest in Stockman, listed among the "tracts that had a major impact on his thinking… Milton and Rose Friedman's *Free To Choose*"—although the author was likely actually meaning to refer instead to the Friedmans' earlier collaboration, *Capitalism and Freedom*.[456]

The Friedmans' most recent collaboration—their book *Tyranny of the Status Quo*—actually had a degree of compatibility with the content of *The Triumph of Politics*. As one commentary on the latter noted (*The Gazette* (Montreal), August 12, 1986), both books were heavily concerned with the Reagan Administration's failure to reduce the size of the public sector in its first term. And both books emphasized the obstacles to achieving such a reduction—including the difficulty in creating and sustaining a strong political consensus toward that end.

It would turn out to be only a matter of time before Stockman set his sights on Friedman, although he did so only well after Friedman's death in 2006. In his book *The Great Deformation: The Corruption of Capitalism in America* (2013), Stockman would go so far as to argue the Federal Reserve's record during the 1929–1933 Great Contraction was entirely appropriate—and that the Friedman-Schwartz critique had been misplaced. As in *The Triumph of Politics*, Stockman's new book was very critical of Laffer. But the striking change since 1986 was the virulence and extent of Stockman's antipathy toward Friedman and monetarism. Things had come a long way since the time, over thirty years earlier, when Friedman indicated that he would be happy to see Stockman succeed Ronald Reagan as U.S. president.

Agreement on disinflation

It is worthwhile, however, to point to an area of important and lasting agreement between Friedman and Stockman. The April 1983 Evans-Novak column quoted above complained that President Reagan was "detached on the sidelines as a non-participant" as economic policy was being formed in his administration. Friedman did not himself altogether challenge this characterization, and he took the president to task on numerous occasions in public for not being more active in carrying out his agenda. Nevertheless, a recurring Friedman judgment was that Reagan, while neither brilliant nor a master of detail, was literate and principled on economic matters and had generally set the tone for administration policy.

In most respects, *The Triumph of Politics* had given the opposite impression: it portrayed Ronald

[456] See Ullmann (1987, p. 70).

Reagan as pliable and as having very limited comprehension of the course of his administration's economic policy, with Cannon (2000, p. 202) arguing that Stockman (1986) "gave Reagan almost no credit for his own program." On one matter, however, Stockman had strong praise for the president, and similar praise featured prominently, if somewhat belatedly, in Friedman's own evaluations of the Reagan record. This pertained to Reagan's support for disinflation and monetary restriction in 1981 and 1982.

Stockman (1986, p. 378) wrote, after praising the Volcker disinflation: "There is also little doubt that Volcker's feat would not have been possible without Ronald Reagan's unwavering support during the dark days of 1982. The president stands almost alone among Washington's current politicians in his instinctive comprehension that inflation is a profoundly destructive phenomenon. He has often been misled by the mumbo-jumbo of his advisers. But when it counted, the president gave Volcker the political latitude to do what had to be done. It was a genuine achievement."

This evaluation was in line with some expressed during Reagan's first term, such as a *Newsweek* report (August 27, 1984) that stated: "Perhaps Reagan's greatest contribution has been his support of Federal Reserve Board chairman Paul Volcker. Faced with an 11.2 percent inflation rate when he entered office, Reagan permitted Volcker to engineer a relentless monetary squeeze that threw the nation into recession. Few other presidents would have tolerated such policies for long, but the results were profound." Friedman, too, praised Reagan for his resistance to calls for an economic-policy *U*-turn in 1981–1982, and, as discussed in this and the previous chapter, Friedman pointed to the fall in inflation during those years as a major policy success. From 1983 to 1985, however, he was reluctant to include a permanent disinflation among Reagan's list of achievements because Friedman was concerned—unduly, as it turned out—about a major rebound in inflation being in prospect.

Over 1985 and into 1986, as Friedman became more confident about the permanent character of the U.S. disinflation, he became more inclined to name Reagan's position on inflation as one of his administration's lasting successes. In June 1986, Friedman told the Commonwealth Club in San Francisco: "When Mr. Reagan came into office, he encouraged a policy of stability and restraint. The remarkable thing about Reagan as a president is that he has been consistent and has stuck to those goals."[457] Similarly, at another Commonwealth Club appearance two years later, he remarked that Reagan "was willing to take the heat in 1981 and 1982" and that the

[457] Friedman (1986b, p. 246).

president [gets] a positive mark for being willing to stand… a very severe recession to bring inflation down."[458]

In the 1986 Commonwealth Club appearance, the question that was posed to Friedman practically invited him to give the lion's share of the credit for disinflation to Reagan rather than Volcker. Friedman did not resist this temptation—so, in contrast to the 1984 *Newsweek* and Stockman (1986) accounts, Friedman dismissed as "wrong" those who gave most credit to Volcker and suggested that, in pursuing disinflation, the Volcker Federal Reserve had adhered to the "broad outlines of policy" laid out by the Reagan Administration.[459]

On other occasions, Friedman was somewhat more generous to Volcker—such as in the discussion in *Money Mischief,* which did not refer to Volcker by name but dated the move to restrictive monetary policy to 1979–1980, before Reagan took office.[460] He would, however, repeatedly stress Reagan's role, as discussed further in Chapter 16. Friedman's retrospectives therefore offered a narrative of a Reagan-backed Federal Reserve disinflation that lined up fairly closely with the words of praise that David Stockman had given to the president on this issue.

[458] Friedman (1988c, pp. 380, 381).
[459] Friedman (1986b, p. 246).
[460] Friedman (1992c, p. 237).

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 15: Debates on International Economic Policy and Geopolitical Developments, 1982 to 1986

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I. EVENTS AND ACTIVITIES RELATED TO DEBATES ON INTERNATIONAL ECONOMIC POLICY AND GEOPOLITICAL DEVELOPMENTS, 1982–1986

On October 15, 1976, the *Milwaukee Sentinel* newspaper published a photograph of Milton Friedman alongside an abbreviated version of the Associated Press report on his Nobel award, which had been announced on the previous day (*Milwaukee Sentinel* (Wisconsin), October 15, 1976*a*). Appearing adjacent to the AP report on this page of news roundups was a much longer piece by United Press International titled "Soviet Missile Perils N-Balance in Europe." This article summarized remarks made by General Alexander Haig, the NATO supreme commander in Europe, in which Haig had expressed concern about the Soviet Union's ongoing transition to the new SS-20 models as the basis for its fleet of intermediate-range ballistic missiles (IRBMs). These "theater" nuclear weapons, targeted at Western Europe, were being put into service by the USSR to replace its previous generation of IRBMs. "I look at it as a matter that we have to address in the context of modernization of our [own] theater capability," Haig observed (*Milwaukee Sentinel* (Wisconsin), October 15, 1976*b*).

The newspaper's juxtaposition of the two reports was just one reflection of the fact that, in the years when Friedman was making waves on the world stage by promulgating his views regarding how Western countries should pursue economic policy, those same nations continued to be involved in geopolitical developments that proceeded in parallel with, but largely separately from, the debates on domestic economic management and on international monetary and trade arrangements.

Many of the same national politicians to whom Friedman had occasion to refer when discussing

[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 and George Tavlas for help and generosity in providing information for this chapter. The author regrets to note that, since the research underlying this chapter began, five individuals—William R. Allen, Ronald McKinnon, Allan Meltzer, George Shultz, and Paul Volcker—whose interviews with the author are quoted below have passed away.

issues such as monetary control, fiscal policy reform, exchange rates, foreign trade, oil prices, or the Phillips curve were also immersed in these debates about international affairs. Among them were successive U.S. presidents, figures like U.K. prime minister James Callaghan (whom Friedman had mentioned in his Nobel lecture and in numerous other writings and speaking appearances), Callaghan's successor Margaret Thatcher, and prominent, and roving-brief, U.S. senators like William Proxmire.

For the bulk of the time, Friedman was not himself among those who were a constant presence in both sets of dialogues. On the contrary, the discourse on the matters of the international power balance and national defense constituted a different area of debate from that in which Friedman was steeped and into which he was accustomed to making interventions. Nevertheless, as discussed below, he was not wholly absent from public exchanges on global-affairs and national-security matters. In fact, during the 1980s, Friedman made quite a number of public statements concerning geopolitical relations and U.S. national defense. Furthermore, a striking feature of developments from the late 1970s through the later 1980s was the extent to which various economists with whom Friedman had, in previous decades, interacted on macroeconomic matters—and had come to know well—emerged as prominent participants in the shaping and execution of U.S. foreign and defense policy.

The Euromissiles debate

General Haig's October 1976 remarks, referred to above, foreshadowed what became a major area of geopolitical discussion. A key matter of concern to policymakers in the United States and its Western European allies in the late 1970s and beyond would be the Soviet Union's installation of the fleet of SS-20 IRBMs. Being theater in character—and, in particular, being targeted by the USSR at Western European sites, rather than at the United States—these weapons were not covered by the superpowers' ongoing strategic arms limitation talks (SALT), which pertained only to homeland-to-homeland nuclear vehicles (such as intercontinental ballistic missiles, or ICBMs) of the two major opposing nations—that is, those that could reach the USA from the USSR, or vice versa. The UPI report on Haig's comments correspondingly noted that "U.S. officials have considered the SS-20 to be a gray area" because it did not fall under the coverage of SALT.

This was a theme pursued in an op-ed by neoconservative writer Kenneth Adelman in the *Wall Street Journal* almost two years later. The *Journal* was a periodical whose opinion and editorial pages often alternated between coverage of the economic issues in which Friedman was deeply involved and consideration of national defense and geopolitics. Reflecting this mixture of interests, the *Journal*'s edition of August 28, 1978, had on its letters page a long contribution by Friedman defending floating exchange rates—one of the bones of contention between himself and the editorial position of the *Journal*—while Adelman's article on nuclear arms appeared on the facing page (*Wall Street Journal*, August 28, 1978*a*, *b*). In his piece, Adelman named the "SS-20 mobile missile" as among the "'gray area' systems" that needed to be brought into the remit of broadened nuclear-arms negotiations (*Wall Street Journal*, August 28, 1978*b*).

Meanwhile, the Western alliance was proceeding with further moves to upgrade its own theater-weapon capability in Europe, largely in response to the advent of the SS-20 missiles, which the USSR had started deploying and making operational. A leaders' summit in January 1979—concerned with both economic and defense matters, and attended by President Jimmy Carter, Prime Minister Callaghan, and others—took tentative steps toward an agreement that the United States would make a large-scale placement of intermediate-range nuclear missiles of its own in Western Europe, as a counter to the SS-20 deployment (Morgan, 1996, p. 619).

Later in the year, Leonid Brezhnev, president of the USSR, stepped in, as part of what Garthoff (1980, p. 82) called "a heavy Soviet diplomatic and propaganda campaign from October to December [1979]"—one designed to sway Western public opinion against the development of a counterweight to the Soviet Union's new IRBM fleet. Brezhnev's principal contribution on this score was a speech given on Saturday, October 6, 1979—the same Saturday on which, in Washington, D.C., Paul Volcker launched the Federal Reserve's New Operating Procedures. International news coverage of Brezhnev's speech in East Berlin on the world military balance rivaled that of Volcker's announcement in Washington about U.S. monetary policy.

In the speech, as well as announcing some reductions in conventional forces, Brezhnev raised the prospect of a cut in the number of SS-20 missiles deployed, provided that the West did not introduce any new theater weapons on its side. He also played down the significance of the SS20 deployment: "As chairman of the Defense Council of the USSR, I categorically state that the number of medium-range nuclear delivery weapons on the territory of the European portion of the Soviet Union has not been increased by even one missile, or one airplane, over the past ten years. On the contrary, the number of launchers of medium-range missiles, and also the yield of the nuclear charges of these missiles, have even been somewhat reduced." Garthoff (1983, p. 115) remarked of these assertions regarding the numbers and properties of the newly installed weapons: "One can argue over their significance, but his claims were valid." This amounted to a different approach from that taken by Brezhnev's USSR during the early 1970s, when the Soviet

Union entered the SALT I agreement. As Foy Kohler, previously U.S. ambassador in Moscow, noted after the SALT I treaty was signed, the agreement's missile numbers "have never been published or commented on in the Soviet Union."[2] In contrast, in 1979, Brezhnev was himself giving out publicly pieces of numerical information about the USSR's nuclear-missile program—albeit in a self-serving manner designed to dampen the momentum for a Western response.

Such pressure notwithstanding, the United States and its allies decided to proceed with deployment of their own theater nuclear missile forces in Western Europe. The *San Francisco Chronicle* for the morning of December 13, 1979—a point at which Milton Friedman was unusually concentrated on military matters because he was to deliver a paper later that day of the Hoover Institution's conference on conscription—reported (p. 1): "The North Atlantic Treaty Organization gave unanimous backing yesterday to a $5 billion program designed to produce 572 new nuclear weapons for Western Europe, coupled with a detailed offer to the Soviet Union to begin arms control negotiations as soon as possible." The specifics of the NATO plan later took shape as 464 ground-launched cruise missiles to be sited in the United Kingdom and 108 Pershing missiles in the Federal Republic of Germany—with both sets of missiles scheduled to be installed in 1983, unless agreement between the superpowers on restraint in theater nuclear weapons had been reached before then.

The stage was therefore set for each of the world powers to have arrayed against one another in the greater European theater their own fleet of intermediate-range nuclear weapons—or what commentary at the time often dubbed "Euromissiles."[3] Inheriting the NATO plan when it came into office in 1981, and proceeding with its implementation, was the Reagan Administration, with Alexander Haig, as Secretary of State in Reagan's first cabinet, in one of the most relevant posts. Reporting to Haig and Reagan, and also figuring heavily in the work of the administration on Euromissile deployment and the associated diplomatic and public communications, was someone who was now returning to public service—having departed the position of Federal Reserve chair in 1978, after eight years in office that had not only featured not only much economic trouble but also considerable strain in his friendship with his former student, Milton Friedman. Arthur Burns became the Reagan Administration's ambassador to the Federal

[2] From Kohler's written submission for his testimony of May 15, 1974, in Committee on Foreign Affairs, U.S. House of Representatives (1974, p. 76).

[3] The two sides' missile forces could not be collectively called IRBMs because the theater nuclear forces being introduced on the NATO side, although intermediate in range, were not ballistic (that is, they lacked the property of leaving the Earth's atmosphere during flight).

Republic of Germany in the middle of 1981.[4]

Arthur Burns: From Federal Reserve chair to ambassador

On December 1, 1981, the new ambassador opened a speech given in Bonn by remarking: “As an economist who has studied and lived history, I am both humble and proud to represent my country in the Federal Republic of Germany.”[5] With these words, the 77-year-old Burns was, in effect, acknowledging his career shift from economics to foreign policy.[6]

Burns turned to the international situation and the role it had played in the Western allies’ decision regarding theater-missile deployment in Germany. As background, Burns referred to the “massive Soviet effort to gain a unilateral nuclear advantage.”[7] This, he contended, was “the critical destabilizing element in the current international situation, and it has forced the Atlantic Alliance to undertake modernization of its nuclear arsenal.”[8] With regard to recent developments on the matter, Burns referred to the proposal regarding theater nuclear forces that Reagan had advanced publicly the previous month. The president, Burns observed, “has indicated that the United States is committed to the goal of virtual elimination of intermediate-range missiles in Europe and that it will therefore not deploy the modernized nuclear weapons scheduled for 1983 if the Soviet Union will agree to dismantle its own intermediate-range nuclear weapons.” [9]

As ambassador to one of the countries planned as the site of the new intermediate-range missiles, Burns was articulating the U.S. “dual track” policy of deployment and negotiations: his role was both to articulate the reasons for the deployment and the willingness to do without them if there was a breakthrough in negotiations with the USSR. The United States’ negotiating position had now been made more concrete—as the president’s “zero-zero” proposal, under which both superpowers would eschew having theater nuclear weapons deployed in Europe.

Friedman on foreign policy

It was against the backdrop of his old teacher and sparring partner Burns now being firmly

[4] See White House (1981b), *Boston Globe,* May 21, 1981, and *San Francisco Chronicle,* June 26, 1981.

[5] Burns (1982, p. 2).

[6] Early in Burns’ tenure at the Federal Reserve, London’s Institute for Strategic Studies had issued a monograph credited to an Arthur Burns, on the subject of nuclear deterrence. But the author was Arthur Lee Burns (1970), not Chairman Arthur F. Burns.

[7] Burns (1982, p. 4).

[8] Burns (1982, p. 4).

[9] Burns (1982, p. 5).

ensconced in the area of foreign policy that, the following month, Friedman agreed to be interviewed about his own perspective on foreign policy for William Buckley's syndicated column. The interview was conducted during Buckley's regular January skiing visit to Utah with Friedman and another friend, Lawry Chickering.

Friedman cautioned to Buckley that, in his public statements, "I don't like to talk about foreign policy, because it is not my field." (*Daily News* (New York), January 19, 1982.) The kind of caution embedded in this remark had already been evident in comments that Friedman gave five years earlier, shortly after he became a laureate in economics, when he expressed misgivings that any Nobel "award converts every recipient into an instant expert in every field," with the public tending to attribute to the opinions of the recipients "an importance that is utterly unjustified."[10]

Even by the time of his Nobel, however, Friedman had broken the implied injunction to stick to economics topics (or to focus on the economic angle when considering areas ostensibly outside his area of expertise), as he had used a *Newsweek* column in the spring of 1976 to discuss U.S. relations toward southern African states and the alleged implications for the superpower balance. He had built on these remarks in a further *Newsweek* column in late 1977.[11] No doubt in part because of his extensive travels, Friedman had developed a considerable, albeit a nonspecialist's, interest in foreign policy. Indeed, this was one of his main policy interests outside economics (Anna J. Schwartz, personal communication, September 25, 2008). But his cautious statement in 1982 perhaps reflected misgivings about that earlier commentary, and he had abjured from writing foreign-policy-focused columns since 1977.

Notably, the 1976 and 1977 Friedman columns on foreign policy were omitted when William R. Allen, serving as editor-selector, put together *Bright Promises, Dismal Performance*, the 1983 Friedman book collection that largely consisted of a compendium of his *Newsweek* columns from 1975 to mid-1982.[12] Allen recalled with regard to the material available for the collection that "there was a considerable pile of stuff, and I had to be somewhat selective and certainly organize it in units, and prepare introductions and summaries and commentaries to go along with it." As far as the 1976–1977 foreign-policy columns were concerned, "the fact that they're not included I guess has to speak for itself. You know, I must have had that line of thought that, however interesting they might be, and useful to some readers, it just didn't fit into the format that at least I had in mind." (William R. Allen, interview, March 14, 2014.)

[10] Friedman (1977a, pp. 5, 6).

[11] See *Newsweek*, May 3 and 24, 1976, and November 28, 1977, as well as Chapter 6 above.

[12] See Friedman (1983a).

Friedman later remarked in 1984 with regard to foreign policy: “I am not an expert, but I have a great deal of enthusiasm for the dialogue—and many strong views that I’m not entitled to.” (*Donahue*, NBC, April 25, 1984.)

Among these many strong views was one he outlined in his 1982 interview with Buckley: a dislike for stationing U.S. troops in Allied countries that were advanced economies. “As for [Western] Europe, what is the reason for our continuing to accept responsibility for its defense? I’d say to them: In exactly five years, there won’t be one American soldier in Europe—not one. We’ll help in any other way. Our navy will be out there—[and] our defense factories at your disposal. But the local muscle will be European. After all, aren’t they as wealthy as we, as numerous as we, and don’t we have more direct responsibilities elsewhere?” (*Daily News* (New York), January 19, 1982.)

These suggestions were in sync with aspects of his views on military commitments that Friedman had articulated publicly for over a decade. He had expressed a preference for the utilization of capital over ground troops in the use of U.S. military force during the Vietnam War. As of the early 1970s, Friedman, although not an opponent of the war, was critical of the extent to which U.S. military personnel had been committed and favored greater use of military hardware over the current reliance on heavy troop deployment. He remarked on television: “For a country [the United States] whose characteristic feature is that it has a large amount of capital per person, to try to fight a war primarily through infantry, people on the ground, seems to me to be an incredible act of stupidity that I don’t understand.” (*NET Journal Presents Conservative Viewpoint*, WTTW Chicago, May 4, 1970.)

And with regard to facing more directly the military threat posed by the Soviet Union, Friedman had, also in the early 1970s, already indicated a strong preference that Western Europe and other allies draw more on their own resources, particularly in the area of personnel. In mid-1971, Friedman had remarked (Instructional Dynamics Economics Cassette Tape 74, May 20, 1971): “We have, in fact, been providing a shield, a military shield for Japan and for Western Europe which has enabled them to spend a very much lower fraction of their annual income on defense than we spend... From that point of view, I think there’s a great deal to be said for the fact that we ought to reduce the extent to which we underwrite the military expenses of other countries… It seems to me highly desirable that other countries should assume a larger part of the defense of the Free World.” A year later, he observed (Instructional Dynamics Economics Cassette Tape 100, May 31, 1972): “Now, I believe [that] one should be an internationalist, and I am a very strong believer in internationalism. But I believe it has to be a mutual internationalism—and not a paternalistic internationalism." What he advocated was "moving toward a position of a strong nation in mutual partnership with other strong nations." He also referred in 1977 to "George Washington's excellent advice to steer clear of foreign entanglements."[13]

These views—of long standing on Friedman's part by the 1980s—were at variance with those of successive U.S. governments, including the Reagan Administration. In particular, his 1982 suggestion of limiting American military commitments to Europe to technology, navy, and consultation went against the grain of the United States' continued undertaking, under Reagan, to large troop deployments in Western Europe. Friedman's position contrasted notably with the portrayal Arthur Burns gave of the *status quo* in his December 1981 speech: "The people of the United States have been sacrificing materially and personally to maintain some 350,000 American troops in Europe. They are stationed here to ensure the maintenance of peace, so that the democratic way of life in our Western societies can be preserved."[14]

In sum, Friedman even in the 1970s and 1980s held an unorthodox view of the appropriate means by which the United States should contribute to the defense of Western Europe. There have been erroneous accounts that have suggested that he only later became disaffected with U.S. military commitments—or that he lined up wholly with the Reagan Administration's approach to deterring the Soviet Union. In this vein, Ebenstein (2007, p. 232) contended incorrectly that Friedman had become less supportive of international military ventures only in recent years, while Ruger's (2011, p. 142) suggestion that Friedman's inclination toward a less activist foreign policy became evident only around 1990 proves to be unsupportable once Friedman's pre-1990 public statements on foreign policy are examined systematically.

In April 1984, Friedman elaborated that, although he supported the U.S. defense buildup and military cooperation with U.S. allies—"We ought to cooperate with them; I'm in favor of alliances"—his views on European engagement stood: "I personally believe that it's time we considered seriously withdrawing troops from Europe. I believe that we went in and provided an umbrella over Europe at a time when Europe was unable to provide one for itself. It's now grown up. It's now perfectly capable of providing for itself." (*Donahue*, NBC, April 25, 1984.)

The Euromissile debate continues

From the perspective on military commitments abroad that Friedman had articulated, the placement of intermediate nuclear weapons in Western Europe represented the kind of

[13] *Milton Friedman Speaks*, Episode 1, "What Is America?," taped October 3, 1977, p. 4 of transcript.

[14] Burns (1982, p. 4).

development that he favored, as it represented assignment of U.S. defense capital equipment rather than infantry.[15] Furthermore, one rationale for the deployment of the new missiles, over and above the Soviet Union's modernization of its own intermediate missile fleet, was as a counter to the USSR's advantage in numbers of armed forces—and so, on that score, the introduction of Euromissiles in Western Europe represented the kind of emphasis on military capital rather than labor that Friedman had long emphasized. Nevertheless, the missiles had not been introduced directly as a substitute for troop deployment—which, as noted above, remained very substantial under Reagan—or as a response to USSR troop numbers, but, instead, as a counter to the Soviet Union's own nuclear modernization.[16]

As indicated above, the position advanced by Reagan in 1981 and communicated in Bonn by Burns was that the cancellation or removal of the Western Euromissiles depended not on reductions in Soviet force numbers but on decommissioning of the corresponding Soviet missiles. This continued to be the U.S. position as the controversy regarding the Euromissiles proceeded over the 1980s. As negotiations with the Soviet Union failed to reach a breakthrough, the issue became more prominent in the countries in which deployment was planned: major protest movements against deployment developed in the United Kingdom and Germany, along with related political disputation—something Friedman was able to see to some extent for himself when, in September 1982, he visited both the Federal Republic of Germany (for a Mont Pelerin Society meeting in West Berlin) and the United Kingdom (for a speaking engagement, meetings, and a television taping, all in London).

The protests did not prevent the deployment of the theater nuclear weapons in both the Federal

[15] Amplifying Friedman's inclination to support IRBM deployment was that, by 1981, its proponents included figures with whom he largely saw eye-to-eye on economic matters, like Reagan and Thatcher. Opponents of theater missile deployment in the United Kingdom included, in the United Kingdom, longtime critics of Friedman and monetarism such as the Labour party's leader Michael Foot, and his deputy, Denis Healey. But there were exceptions to the pattern. In the United Kingdom, for example, Francis Pym (who served in positions in defense and foreign policy in Thatcher's government through mid-1983) was a critic of monetarism but a strong proponent of Euromissile deployment and had criticized Leonid Brezhnev's October 1979 defense of the Soviet Union's SS–20 program (*Yorkshire Post* (Leeds, England, UK), October 10, 1979). Similarly, Sir Ian Gilmour, a minister in the Thatcher Government through 1981, was highly critical of Friedman, but also advocated a defense buildup, and as a Conservative member of parliament in the late 1970s had stated: "The threat to the West is as great as ever it was… despite all the talk of détente." (Gilmour, 1978, p. vii.)

[16] This rationale for the NATO theater-weapon deployment as a response to the Soviet Union's own missile deployment would be criticized by opponents of the eventual treaty to remove intermediate-range missiles on both sides. For example, General Bernard W. Rogers, a former commander of U.S. forces in Europe, contended that Reagan's "zero-zero" proposal "was a magnificent political ploy" when advanced in 1981 but was a "proposal we did not expect the Soviets to accept." Rogers contended that the United States should not have seriously sought to achieve a zero-zero outcome, as he contended that NATO needed theater missiles to counter the Warsaw Pact's advantage in conventional-force numbers. (The quotations are from Rogers' testimony of January 29, 1988, in Committee on Armed Services, U.S. Senate, 1988, pp. 105, 106.)

Republic of Germany and the United Kingdom. The missiles were made operational in both countries in 1983. On October 25, 1983, Friedman observed that "the problem about the deployment of the missiles in Europe… is very high on everybody's agenda"—meaning the agenda of both the United States and its Western European allies.[17] The ongoing public debate over Euromissiles proceeded against this background—and it included, in Spain, the release in 1985 of a monograph on the matter by Jordi Galí—who was subsequently to become a major figure in monetary economics.[18]

Eventually, under Mikhail Gorbachev (who became the Soviet leader in March 1985, after the deaths of two previous leaders following Brezhnev's own death in November 1982), the USSR reached an agreement with the United States patterned after Reagan's 1981 "zero-zero" proposal. The Intermediate Nuclear Forces (INF) treaty, arranging for the Euromissiles on both sides to be withdrawn, was signed by Reagan and Gorbachev in November 1987 and ratified by the U.S. Senate in 1988. Arthur Burns, who had returned to the United States after leaving his post of ambassador in 1985, did not live to see the culmination of these developments, having passed away in August 1987.[19]

Defense spending and the federal budget

Although new weapons systems, such as the European theater missiles and modernized strategic nuclear vehicles, did play a prominent role in the process, much of the U.S. defense buildup that had begun under President Carter and continued under President Reagan in practice took the form of an increase in conventional forces, and in particular in U.S. military personnel. The aggregate membership of the U.S. armed services rose from about 2.027 million in 1979 to a decade peak of 2.174 million in 1987.[20]

Friedman was in basic agreement with this move: He believed that the U.S. military tended to assign too many resources to personnel but by the late 1970s had concluded that the expansion of USSR military power justified an increase (albeit in the context of a continuing all-volunteer force posture) in the number of Americans in uniform (*Newsweek*, April 16, 1979).

[17] Friedman (1984a, p. 15).

[18] See Jordi Galí i Garreta (1985). See also *https://www.worldcat.org/title/armes-nuclears-i-seguretat-a-europa/oclc/433900349&referer=brief_results*.

[19] Burns was still alive, however, when Gorbachev agreed in principle to the zero-zero proposal. This occurred in February 1987. (See, for example, the testimony of Bernard W. Rogers of January 29, 1988, in Committee on Armed Services, U.S. Senate, 1988, p. 105, and Garthoff, 1994b, p. 305.)

[20] See "U.S. Military Personnel 1954–2014," *https://historyinpieces.com/research/us-military-personnel-1954-2014*.

And once the overall U.S. defense buildup was underway, together with further increases in military spending scheduled for coming years, Friedman defended its quantitative contours. In particular, during a television appearance in the first quarter of 1982, he stated that the planned expansion in defense expenditures was comfortably within the country's feasible economic limits: "The nation can afford to have a strong defense. Military spending is scheduled to be something like 5 or 6 percent of our GNP. Fifteen years ago, it was 8 or 9 percent."[21] Writing in early 1983, Friedman endorsed a statement President Reagan had made to the effect that choices concerning the amount of funds that the country spent on national defense should not be dictated by efforts to control the federal budget deficit (*U.S. News and World Report*, January 31, 1983, p. 66). Friedman elaborated on this point in a television interview the following year: "Look, the first obligation of a country is to defend the people against foreign enemies. We should not decide how much we spend for defense on the basis of whether we have a deficit or not. We should decide it on the basis of how much we need for our national security." (*The MacNeil/Lehrer News Hour*, PBS, March 27, 1984, p. 6 of transcript.)

Friedman, in any event, was unimpressed by claims that defense spending was a primary reason for the federal budget deficit. He noted in early 1983 that military spending "accounts for less than a quarter of the budget" (*U.S. News and World Report*, January 31, 1983, p. 66). As for increases in federal expenditure (as distinct from levels), he argued the following spring that government "spending has gone up, and our deficits have been zooming, primarily because of the steady increase in nondefense spending."[22] Friedman did acknowledge in 1984 that the expansion of the federal deficit seen in recent years "partly reflects increased spending on defense" (*San Francisco Chronicle*, September 14, 1984). He maintained, nevertheless, that defense spending was neither the "source of our deficit," nor the "major source" among the items on the outlays side of the budget making for a deficit.[23]

Invoking Parkinson again

In the course of his discussions during 1983 and 1984 of defense spending in the United States, Friedman returned to an idea that he had credited to Cyril Northcote Parkinson's book *The Law*

[21] *Meet the Press*, NBC, March 21, 1982, p. 1 of transcript. A more apposite comparison may have been with twenty years earlier—the early 1960s, before the United States' major Vietnam War engagement. This change in the basis for comparison would, however, have left Friedman's remark still valid. Defense spending was 9.4 percent of GDP both in fiscal year 1961 and in fiscal year 1968 (Council of Economic Advisers, 2011, Table B–79, p. 284). Friedman did, in fact, compare 1982's share of defense spending with that at the start of the 1960s in *San Francisco Chronicle*, December 2, 1982 (p. 29).

[22] Friedman (1983b, p. 121). See also *Newsweek*, April 18, 1983.

[23] Friedman (1985a, p. 6).

and the Profits (Parkinson, 1960). This idea brought Friedman's coverage of defense matters into an intersection with a subject on which he felt more comfortable speaking: the control of domestic government expenditure.

As Friedman had remarked in a 1976 *Newsweek* column, Parkinson (1960, pp. 91–93) had conjectured that, if a large public sector emerged because of expanding domestic spending programs, the government was, in effect, already calling heavily on the nation's resources in peacetime and was committed to social expenditures that might not easily be cut back at short notice. This situation, Parkinson had suggested, left the government disposed toward reductions in defense spending, as well as likely to impose higher taxation that reduced the strength of the economy. The upshot would be a loss for the nation of "influence on world affairs."[24] In endorsing this argument, Friedman stressed also that the scenario under contemplation left a country poorly positioned to move to sharply higher defense spending in the event of a sudden increase in international tensions or its entry into a war. Friedman applied this argument in the postwar U.S. context by pointing to the growth of transfer programs in particular: to "continue down the income-transfer road," he had suggested, would have "the inevitable effect [of] pressure to reduce the military budget" (*Newsweek*, February 9, 1976).

Friedman had made the remark just quoted while cuts in defense spending were occurring in the initial years of the Ford Administration. But he had already articulated the same point when President Nixon had abandoned fiscal restraint in the course of his 1971 economic-policy *U*-turn. That development had prompted Friedman to observe: "The real obstacle to improving our defense is going to be… the preemption of the taxable capacity of America." (*Firing Line*, PBS, January 5, 1972, p. 9 of transcript.) The large increase in Social Security benefits authorized in 1972 proved to be another major example of the trend toward domestic federal spending programs—particularly in the transfers category—and in 1977 Friedman expressed concern that "one of the greatest costs we may be paying for the unbridled growth of transfer expenditures is that it has produced excessive pressures to weaken our defenses. We are in danger of following the British example of sacrificing military power and worldwide political influence to feed the insatiable demands of the welfare state." (*The Australian*, February 8, 1977.)

In Reagan's first term, Friedman expanded upon the Parkinson argument, which he perceived to be highly applicable to existing conditions in the United States. Defense spending had turned up markedly in recent years, but international tensions, particularly in the area of U.S.-Soviet

[24] Parkinson (1960, p. 93). See also Parkinson (1983, p. 61).

relations, had also increased, and Friedman saw little prospect of a declining trend in American military spending until the international situation improved.[25] What worried him was what would happen, under the current U.S. fiscal policy configuration, if the world situation underwent a further, and drastic, deterioration—of a kind that raised the prospect of a major military conflict involving the United States as a combatant.

In a *Newsweek* column titled "High Taxes, Low Security" (April 18, 1983) and a speech given at San Francisco's Commonwealth Club a few days after the column appeared in print, Friedman argued for the pertinence of Parkinson's argument. Although he made clear in his column that "I am no defense expert," Friedman ventured to suggest that "Parkinson has more to contribute to the debate about how to ensure the future security of the nation than either the Kremlinologists or the military experts." Nondefense federal government spending was, Friedman observed, at a historical high, of nearly a quarter of national income (*Newsweek*, April 18, 1983). Of those outlays, "entitlement programs… account for half the budget," he had noted (*U.S. News and World Report*, January 31, 1983, p. 66). Under these circumstances, Friedman argued, the leeway available to the nation to ramp up defense spending to the double-digit percentage of national income that would be associated with wartime conditions—especially a major mobilized war against another world power—was compromised. In his speech, he further observed that the fact that Congress, including Senate Republicans, had established a record of revising Reagan's spending proposals in favor of a higher weighting to domestic programs, itself already illustrated, in the present environment, "the difficulty of cutting nondefense expenditures to make room for defense expenditures."[26]

In their book *Tyranny of the Status Quo*, appearing the following year, Milton and Rose Friedman had a chapter (albeit one of only twelve pages) on national defense. They again offered the now-familiar caveat: "We are not ourselves defense experts."[27] In line with this acknowledged lack of specialized knowledge of the geopolitical scene, they avoided devoting much discussion in their chapter to world affairs or defense strategy.[28] Instead, as Friedman so often did when discussing the military, they concentrated on the issue or the contours of defense

[25] Friedman remarked: "The only way we're ever going to get a lower military expenditure is by an improvement in the world situation." (*Donahue*, NBC, April 25, 1984.)

[26] Friedman (1983b, p. 124).

[27] Friedman and Friedman (1984, p. 70; 1985, p. 71).

[28] In a separate chapter, however, they referred to the "increasing instability and insecurity of property throughout the world," with one example of this state of affairs being "concern in Europe about Russia's behavior and intentions" (Friedman and Friedman, 1984, p. 124; 1985, p. 121). This argument was in the context of explaining U.S. capital inflows in terms of safe-haven demand by non-U.S. residents. On this matter, see the discussion of "The Dollar and the Current Account" in Section II below.

spending *vis a vis* nondefense spending—once again stressing the facts that military expenditures under Reagan were not historically high as a share of national income and that domestic spending had been the major driver of growth of federal outlays—as well as recapitulating the Parkinson argument hypothesizing adverse effects of increased social spending on national security and on a country's scope to have international influence.

The Friedmans did, however, close their chapter with a discussion of "Waste in Defense."[29] Friedman had brought this subject up in his 1982 *Meet the Press* interview: "I have no doubt that there is waste in the military area. [And] I have no doubt that we ought to be able to get the same amount of strength for less." He had added in 1983 that "I have no doubt that there is a great deal of spending in the military that is not achieving effective results… I'm reconciled to paying twice as much for the military as I have to, but I don't want to pay 2½ times as much." (*U.S. News and World Report*, January 31, 1983, pp. 66–67.)

The Friedmans' 1984 discussion of reducing waste in defense spending implied that this might be better achieved by internal reform of the defense agencies rather than by lowering aggregate defense spending. In particular, they pointed to parochial empire-building within different parts of the U.S. defense establishment as a source of duplicated effort in the military and suggested that a rationalization of the organization of the military bureaucracy might improve matters.

Their discussion and proposals concerning this matter of reform of defense were tentative, however, and in late 1991 (by which time—with international tensions having eased and the collapse of the Soviet Union very much in train—significant cutbacks in aggregate defense spending were on the horizon in the United States), Friedman clearly felt that little headway had been achieved in curbing waste in the defense establishment, as he observed that "our socialized defense industry provide[s] clear evidence that we are no better at socialism than countries that have gone all the way." (*Wall Street Journal*, November 12, 1991.)

Notwithstanding its discussion of waste in expenditure on the military, *Tyranny of the Status Quo,* like numerous other Friedman discussions of the matter since the late 1970s, accepted the need for higher U.S. defense spending. Indeed, the back cover of the 1985 paperback edition used a 1980 Reagan reference to the need to "restore our defenses" and so implied that a defense buildup was one criterion on which to judge the Reagan record.[30] Friedman's April 1983 speech

[29] Friedman and Friedman (1984, pp. 75–79; 1985, pp. 76–80).
[30] See Friedman and Friedman (1985).

had likewise taken this point largely for granted, referring to the "widespread recognition around the country of the urgent needs to strengthen our defenses."[31]

At the conclusion of his *Newsweek* column of the same vintage, however, Friedman sounded a more ambivalent note by saying that he not only did not know whether current U.S. defense spending was "deficient" (as foreign policy and military specialists at the Hoover Institution often tended to argue, even after allowing for the ongoing buildup) but that he also was unsure whether it was, on the contrary, "shamefully excessive" (*Newsweek*, April 18, 1983). In allowing for the possibility that defense expenditure might be too high, Friedman may have been alluding to the aforementioned waste component. Or he may have had in mind his long-standing preference for a shift in defense resources toward machinery, with the associated possibility of savings, and for less U.S. spending on the defense of wealthy allies. It is also possible that he also was making a concession to the maverick views of those like Senator William Proxmire, who had contended for many years that military spending could be reduced very substantially by a rationalization that reduced redundancies in the U.S. deterrent posture.[32]

Mostly, however, Friedman's caution in entering a definitive judgment on defense spending likely reflected his discomfort with moving so far outside economics. Perhaps chastened by his experiences, noted above, in the 1970s when his column ventured into geopolitical matters, Friedman remarked in 1984: "I've tried very hard over the years to avoid shooting my mouth off on subjects on which I have no competence. It seems that destroys your credibility in an area where you really *have* something to say." (*California* magazine, October 1984, p. 73.)

Tellingly, on September 26, 1984, in an address to a prestigious military audience—the U.S. Military Academy at West Point, New York state—Friedman mainly avoided foreign policy issues.[33] He confined most of his coverage of defense matters to a discussion of his activity against the draft and to identify nondefense spending as the source of the U.S. budget deficit.

The Strategic Defense Initiative

But Friedman's preexisting attraction to greater concentration on military technology as the direction in which the defense establishment could proceed—possibly as a stepping stone toward

[31] Friedman (1983b, p. 121).
[32] On Proxmire views on this matter, see page 425 of Walter P. Reuther's submission for the hearing of June 9, 1969, in Joint Economic Committee (1969), as well as the comments made by Proxmire himself on pages 2 (June 3) and 410 (June 9) of the same hearings volume.
[33] See Friedman (1985a).

providing national security at lower overall cost—was evident in Friedman's moderately favorable comments about the Strategic Defense Initiative (SDI), which was announced by President Reagan in March 1983 and which aimed at the development of an antiballistic missile (ABM) system.

Rather than dwell on his own views on the desirability or feasibility of an ABM system like that pursued in the SDI, Friedman used the debate that SDI had generated in the scientific community to demonstrate a proposition that had opened his Nobel lecture.[34] This was that dissent among economists was not grounds for denying economics the status of a science. "Here is physics," Friedman observed in 1991. "People think of it as a pure, hard, scientific discipline. But when it comes to issues where values enter in very much, such as, 'Do you want to have SDI?,' for example, or not, you don't have the physicists speaking with a single voice. They are divided into camps and into groups. It's not different [from economics]." (Academy of Achievement interview, January 30, 1991.)

Here, Friedman was in effect expanding on remarks he had made at a much earlier in point in the SDI debate when, in Texas in early 1985, he had cited "the argument that is now raging about Star Wars, the Strategic Defense Initiative."[35] Proceeding along the same line of thinking as in his Nobel lecture, he was implying that the SDI controversy indicated that both value judgments and assessments of political reactions to the availability of policy tools entered into the public statements made by natural scientists in public debate, just as they did into those of economists in public debate. "Some physicists issue manifestos opposing Star Wars; other physicists issue manifestos supporting Star Wars," he noted.[36] The manifestos in question had these contrasting features, Friedman suggested. because the competing sides had rested their cases in part on judgments such as whether a SDI program would increase the likelihood of war (by, for example, encouraging one or both sides into believing that conventional war would not escalate into nuclear war, or that a nuclear war would not leave both sides ruined).

Friedman did, however, acknowledge that the dispute among Western scientists concerning SDI did include a positive-analysis component, in the form of disagreement over the implications of "agreed scientific knowledge."[37] In the SDI debate, some scientists—prominently including nuclear physicist Edward Teller, who was a senior fellow at the Hoover Institution alongside

[34] See Friedman (1977b, pp. 451–453).
[35] Friedman (1986a, p. 89; p. 91 of 1988 paperback version).
[36] Friedman (1986a, p. 89; p. 91 of 1988 paperback version).
[37] Again, see Friedman (1986a, p. 89; p. 91 of 1988 paperback version).

Friedman (though the two were not close)—made the case that antiballistic missile technology was already a technologically feasible option (or nearly so) for the United States. Many other scientists, who were represented in one side of the series of manifestos to which Friedman referred, disagreed with Teller on whether—even after allowance was made for the more advanced level of technology in the United States than in the Soviet Union—an ABM system was truly feasible on the basis of the 1980s' state of knowledge. This latter, skeptical, camp of scientists was probably the larger one. Nevertheless, it has been hypothesized that the Soviet Union's giving weight to the possibility that the SDI project might, in the foreseeable future, provide the USA with a rudimentary ABM capability was important in leading Mikhail Gorbachev to provide what Garthoff (1994, p. 525) to make "far-reaching Soviet concessions" in arms-control agreements. This included Gorbachev's acceptance of the "zero-zero" INF proposal—after essentially the same idea had been rejected by the Soviet Union under Brezhnev when, in 1981, it was originally advanced in the United States by Reagan and then advocated to U.S. allies by the administration's Western European representatives such as Arthur Burns.

George Shultz arrives at the State Department

Although Friedman's own areas of specialty implied that his commentary largely stuck to the domestic side of policy, and to trade and exchange rates when considering the international dimension, it would turn out that the Reagan Administration personnel with whom he had the longest ties tended to be on the foreign-policy side. The most senior example of this in the administration was George Shultz, who had replaced Alexander Haig as U.S. Secretary of State in June 1982, and who had been a close colleague of Friedman at the University of Chicago in the late 1950s and throughout most of the 1960s.

Shultz quickly moved to beef up the economic credentials of the State Department, and as part of this process he hired Martin Bailey—who had overlapped with Friedman as a member of the University of Chicago's Department of Economics for many years—as the department's *de facto* chief economist.[38] In the course of Bailey's tenure, Bailey found opportunities for research spillovers from his work at the department on developments overseas. For example, with George Tavlas, an economist then at the State Department, Bailey produced in 1985 an article on inflation and money demand in Israel. Those thanked in the paper's acknowledgments for comments on an earlier draft were Friedman and Phillip Cagan (who had, in the mid-1970s, been

[38] Unlike Friedman, Bailey also had considerable research credentials in the areas of national security and defense alongside his contributions to monetary economics.

one of Tavlas' teachers at Columbia University).[39] Another University of Chicago connection at the State Department during the Shultz years was monetarist William Dewald—who had been a junior colleague of Friedman's in the university's economics department in the early 1960s, and who joined the State Department in 1985.[40]

Other than Shultz, the most senior of those at the State Department in this period who had a University of Chicago background was Allen Wallis, Shultz's predecessor as dean of the University of Chicago, who became undersecretary of state for economic affairs in 1982. Wallis had worked with Friedman on microeconomic and statistical issues in the 1940s but had not emulated Friedman's move into research on monetary economics. In his interventions on public policy, however, Wallis did frequently comment on macroeconomic matters, and this continued to be the case during his tenure at the State Department.

A striking example of this came in 1983 when Wallis was in New Zealand. Friedman had visited New Zealand in April 1981 and had been critical of the policies of Robert Muldoon's government—which adhered far more to the 1970s pattern of Western conservative governments being highly economic interventionist than to the different direction being taken in more recent times by Reagan and Thatcher. In mid-1982, Muldoon's government became even more interventionist when it imposed an indefinite national wage-price freeze. This approach was so different from that of the Reagan Administration, and so affronted Wallis' assessment of appropriate economic policy, that during his New Zealand trip he took the step, unusual for a State Department official visiting an allied country, to make a speech criticizing the wage-push view underlying New Zealand's domestic economic policy. "Wage demands have never had anything to do with inflation," Wallis remarked in a speech in Wellington, while also reiterating a favorite point of Friedman's that wages, if anything, lagged the inflation rate. Wallis deplored "quick fixes" in addressing inflation and suggested that price stability required a sustained policy of restraint of aggregate demand (*Evening Post* (Wellington, New Zealand), June 23, 1983).

The Labour Government led by David Lange that took office in New Zealand in July 1984 abolished wage and price controls, moved toward monetary restraint, and introduced a battery of deregulatory measures that led Friedman to regard the government's record very highly.[41]

[39] See Bailey and Tavlas (1985, p. 339). Bailey served at the Department of State from 1983 to 1989 (American Economic Association, 1993, p. 42).

[40] Dewald served at the Department of State from 1985 to 1992 (American Economic Association, 1993, p. 127).

[41] See, for example, Friedman and Friedman (1988, p. 466). Friedman told the present author that it was "really rather distressing" that the free-market Labour government had been "kicked out" by New Zealand's voters in late

Reflecting the fact that he had moved from economics to foreign policy, however, George Shultz could take less heart than Friedman did in the policy changes enacted by New Zealand's new government. For, along with its liberalization of economic policy, the Lange made major changes to the country's foreign and defense policies—implementing an anti-nuclear stance that strained its alliance with the United States and that put it at odds with Shultz's State Department.

Eugene and Walt Rostow

Even before Shultz built up the economics credentials of the State Department, the Reagan Administration's appointments in the area of defense and foreign policy already included a couple of prominent personnel who had a formidable background in economic research. The first was Arthur Burns, dispatched to the Federal Republic of Germany in 1981, as noted above. The second was Eugene Rostow. Although primarily a legal scholar, Eugene Rostow had been heavily involved in economics subjects, especially during the 1940s and 1950s. Like Friedman, he had published in the *American Economic Review* in 1943 and th*e Journal of Political Economy* in 1949.[42] During the 1950s, Eugene Rostow had dwelled on such issues as the appropriate policy against inflation (a matter on which he and Friedman were both speakers at a conference at the University of Chicago in the spring of 1951) and the social responsibility of business (on which his position was close to Friedman's, at a time when Friedman was starting to articulate it prominently). [43] Rostow also taught at Yale University in economics-related fields.

But by 1976, when a Congressional interlocutor recalled to Rostow at a hearing of the period "29 years ago when you taught me corporation finance at the Yale Law School," Eugene Rostow was very heavily concentrated on geopolitical matters.[44] He had served in the Johnson Administration in a foreign-policy role.[45] And after leaving office, he parted company with a great many fellow Democrats, first in continuing to support the United States' engagement in the Vietnam War (see E.V. Rostow, 1972) and then as a critic of successive administrations' policies in the 1970s of detente with the Soviet Union. As one of the leading members of the neoconservative

1990 but applauded the continuing free-market orientation of its successor government (Milton Friedman, interview, January 22, 1992).

[42] See E.V. Rostow (1943, 1949). Rostow wrote prolifically in this period on the subjects of antitrust policy and the oil industry. Although neither of these exactly became research topics for Friedman, they both were matters on which he nevertheless ended up writing a considerable amount.

[43] See Nelson (2020a, Chapter 5; 2020b, Chapter 14). Rostow had earlier penned an article specifically on inflation control (E.V. Rostow, 1948).

[44] The quotation is from the hearing of February 11, 1976, in Committee on Armed Services, U.S. House of Representatives (1976, p. 1615). In the same hearing, however, Rostow described himself as "an economist" (p. 1595).

[45] Rostow was Undersecretary of State for Political Affairs from 1966 to 1969 (see E.V. Rostow, 1972).

movement, Rostow had, during the defense spending cutbacks of the 1970s, called for a U.S. military buildup and had been highly disturbed about the Soviet Union's military posture in the decade. This was so much the case that, after he had remarked in a preamble to Hoover Institution monograph titled *Soviet Strategy for Nuclear War* that the Soviet Union was determined "to outflank, envelop, and neutralize the United States" and to impose "Soviet dominion" (E.V. Rostow, 1979, p. x), one reviewer had remarked: "Nor is the study helped by the rather extreme and largely irrelevant foreword by Eugene Rostow."[46]

Eugene Rostow was brought back into government in 1981 as head of the Arms Control and Disarmament Agency in the Reagan Administration. By the middle of Reagan's first term, however, Rostow had, in Shultz's (1993, p. 161) assessment, lost the "confidence of the president," as Rostow had been critical of the administration for not pursuing arms-control opportunities more expeditiously. Eugene Rostow was dismissed in early 1983 and returned to writing on public policy. In doing so, he stuck largely to world affairs rather than economic issues.

The same was not true of his brother, Walt Rostow. Like Eugene Rostow, Walt Rostow had been a senior official in the Johnson administration on matters of foreign policy, as well as subsequently a 1970s neoconservative and supporter of the Reagan-era military buildup—a buildup he implied, in a post-Cold War retrospective, had helped put the Eastern bloc economies to breaking point by making them spend 12 percent of GNP on defense in 1982 and 9 percent later in the decade (W.W. Rostow, 1995). Unlike Eugene Rostow, however, Walt Rostow stayed outside the Reagan Administration.

Also unlike his brother, Walt Rostow stayed in the field of economics in the 1970s and 1980s. Walt Rostow, a professor of economics, was in awe of his brother: "I never met Eugene Rostow," Anna Schwartz observed (personal communication, May 7, 2008). "I know that Walt worshipped him as a superior human being." But Walt Rostow was better established in economic-research circles than Eugene Rostow, and, prior to his government service, was well known in particular for his contributions to the field of long-term economic growth.[47] In the area of business cycles and inflation, however, Walt Rostow was less well known but very outspoken.

[46] Baylis (1980, p. 379).

[47] His most well-known work was *The Stages of Economic Growth* (W.W. Rostow, 1961). Although this study did not give the free operation of market forces the weight Friedman believed it deserved in accounting for successful growth performance, the work was explicitly advanced as a challenge to Marxist accounts. The Soviet press, in condemning the work, grouped Walt Rostow among the "noted imperialist ideologists" (commentary by G. Shliapnikov in *Kommunist*, March 1970, quoted in translation in Gouré, Kohler, Soll, and Stiefbold, 1973, p. 67).

He was an ardent advocate of Keynesian macroeconomics of a largely unreconstructed kind.[48] And this would put him at loggerheads with both Anna Schwartz and Milton Friedman.

Walt Rostow had actually been a co-researcher with Anna Schwartz well before Milton Friedman ever was. He and Schwartz were engaged from the early 1940s in a National Bureau of Economic Research study of U.K. historical economic performance. They were, in effect, doing the bulk of the research on the project even before the nominal head of the project, Arthur Gayer, died prematurely. The resulting book came out belatedly as two volumes in 1953 (Gayer, Rostow, and Schwartz, 1953a, 1953b). By the time of its publication, Anna Schwartz was several years into her monetary project with Friedman—and no longer adhered to the nonmonetary account of business cycles and price behavior expounded in her work with Walt Rostow on the United Kingdom. Her repudiation of the study's posture on money's role became explicit when she and Walt Rostow wrote a foreword to a 1975 reissue of their study.[49]

Although the foreword was nominally coauthored, it contained several pages that, essentially, were solely attributable to Schwartz: these pages previewed the *Monetary Trends* study, rejected the cost-push view of inflation she and Walt Rostow had once articulated together, outlined the postwar revival of interest in monetary policy, and elaborated on the monetarist view of interest-rate determination and the transmission mechanism (Rostow and Schwartz, 1975, pp. ix–xiii). The foreword then, in effect, turned to being in Walt Rostow's voice, with a reaffirmation of cost-push views: "The other coauthor (Rostow) would hold to the portrait [in their 1953 work]… of the relation between price movements and the monetary system… [H]e would not accept the distinction between individual and general price analysis. He believes still that the British monetary system played a limited and essentially passive, responsive role in determining the outcome for the economy as a whole." Rostow, indeed, stated that since 1953 he had been "strengthened" in his adherence to this position.[50]

Walt Rostow's continuing strong Keynesianism in the 1980s was manifested in a book he issued in 1983 called *The Barbaric Counter-Revolution*, which made plain that a major part of the counter-revolution in question was the emphasis on monetary policy, particularly for the control

[48] Walt Rostow's conventional, and sometimes hardline, Keynesian positions contrasted the disagreement he expressed with some U.S. Keynesians regarding the analysis of longer-term economic growth. In particular, he was critical of Robert Solow, and later Paul Krugman, for using what he called the "poor instrument for analyzing economic growth" of growth accounting (W.W. Rostow, 1995, p. 183).

[49] See Rostow and Schwartz (1975). See also Capie and Wood (1989, p. 81).

[50] Rostow and Schwartz (1975, p. xiii).

of inflation, advanced by Friedman and followed by U.S. policymakers in the early 1980s.[51] Rostow's critique clearly indicated that his support of the Reagan Administration in the area of defense policy did not extend to economic policy. Later in the decade, Walt Rostow remained a holdout in favor of a nonmonetary strategy against inflation. In an article written in 1989 and nominally concerned with the topic of U.S. foreign policy in the 1990s, Rostow suggested that "paying our way in the world may require that we contain inflation by means that go beyond fiscal and monetary policy. We may well have to devise, in terms of our institutions, an income policy…"[52]

Friedman and neoconservatives on economic policy

The ongoing schism between Friedman's views and those of a neoconservative like Walt Rostow showed the limits of a generalization Friedman liked to make about neoconservatives. They had once combined their foreign-policy views with an interventionist stance on domestic economic matters. But their economic positions—so Friedman contended—had dissipated as neoconservatives had increasingly been "mugged by reality" and converged toward views on economics much closer to his own.[53] This shift in neoconservatives' economic positions was much more limited than Friedman suggested. Not only did some neoconservatives maintain strongly Keynesian views on macroeconomics, but also Friedman found himself in disagreement with many neoconservatives when it came to their enthusiasm for subordinating U.S. economic policy decisions to foreign-policy aims. In particular, as he had during the 1970s, Friedman remained opposed to trade and loan arrangements with the Eastern bloc when these involved a government subsidy on the part of the Western nations. But Friedman remained opposed to a general economic blockade of the Soviet Union and its satellites.

It was in connection with these matters that in late July 1982 Friedman recalled, in a letter to George Shultz, a recent party at Shultz's Bay Area residence. At the party, Friedman had talked to Helmut Schmidt, Chancellor of the Federal Republic of Germany at the time. In his letter, Friedman noted that he regretted not mentioning to Schmidt the costs of loans that Germany provided to the USSR on concessional terms, including the fact that the extension of these loans had the effect of reducing the pool of saving available for capital formation in market economies.[54] Here, Friedman was not taking a position with which neoconservatives could take

[51] See W.W. Rostow (1983), as well as Glissman (1985).
[52] W.W. Rostow (1991, p. 49).
[53] See Friedman and Friedman (1988, p. 464).
[54] Milton Friedman letter to George Shultz, July 30, 1982, Box 179, Milton Friedman Papers, Hoover Institution. See also Service (2015, p. 37).

issue. However, he was *also* critical of attempts to restrict commercial transactions with the USSR that were on market terms, unless restrictions of this kind could be rigorously justified on national-security grounds (such as when they involved export of weapons or high-technology products). This posture contrasted with that typically taken by neoconservatives with regard to economic relations with the USSR: they held that, in trade as in other areas, the United States and its allies should be concerned with intensifying the strains on the Soviet economy.

Although some retrospective accounts (such as Schweizer, 1994) have characterized the Reagan Administration as having made the imposition of limits on access to foreign goods and capital, the creation of adverse movements in the Soviet Union's terms of trade, and restriction on the USSR's exporting opportunities key parts of its geopolitical strategy, the Reagan era, in fact, saw only limited use of economic weaponry of this kind. Indeed, Allen Wallis testified in March 1984: "Our policy is not one of economic warfare against the Soviets."[55]The economic strains on the USSR that certainly intensified over the period were in large part driven by its centrally planned internal arrangements rather than by a concerted effort by the U.S. government to weaken either the Soviet Union's external trade position or its scope to borrow from abroad. Trade between the USA and the USSR increased substantially over the period (see Garthoff, 1994a, p. 1173), and the Reagan Administration in its first year lifted a major trade sanction (the grain embargo) against the Soviet Union that it inherited, with other inherited sanctions on energy exports then relaxed in 1983 (Garthoff, 1994b, p. 109).

Furthermore, a move toward intensified economic sanctions made by the Reagan administration in 1982—its efforts to stop its Western European allies from constructing a trans-Siberian pipeline that would supply natural gas in the Soviet Union—was essentially abandoned late in the year (Garthoff, 1994b, pp. 52, 156). Friedman had initially tentatively supported the U.S. policy to block the pipeline—his own grounds being not the existence of the project itself but the fact that it entailed a subsidy to the USSR (as the Soviet government's payment was not required to take the form of the prompt provision of hard currency). But he abandoned this support once it became clear that the Reagan Administration was willing to subsidize some of its own exports to the Soviet Union (*Newsweek*, November 15, 1982). When the administration largely ended its efforts to block Western Europe from proceeding with the pipeline, Friedman applauded this development: he told Shultz that he considered America's attempt to stop the pipeline to have been a fiasco.[56]

[55] Statement of March 29, 1984, quoted in Garthoff (1994b, p. 155).
[56] See Friedman's letter to George Shultz, November 16, 1982, Box 179, Milton Friedman Papers, Hoover Institution.

With regard to the capital-account side of economic relations with the Eastern bloc, neoconservatives such as Podhoretz (1984, p. 456) found fault with the Reagan Administration's response to the declaration of martial law in Poland in December 1981. This development, they argued, should have led the administration to pressure U.S. commercial banks to call in their loans to the Polish government. Such a move, Podhoretz suggested, would have forced Poland's government into default and thereby intensified the economic burden on the USSR. Friedman was openly skeptical about the likely value of such proposals in terms of their effect on the Soviet Union: "There isn't anything we can do to the Russians over Poland that would really hurt them." (*Daily News* (New York), January 19, 1982.)

One of the reasons why the Reagan Administration was not attracted to the prospect of straining the Polish government's debt situation was the concern that, when non-advanced economies were declared unable to repay their loans to U.S. commercial banks, this would hurt the U.S. economy and not just the debtors. In mid-1982, this consideration became an urgent one beyond just the case of Poland.

II. ISSUES RELATED TO DEBATES ON INTERNATIONAL ECONOMIC POLICY AND GEOPOLITICAL DEVELOPMENTS, 1982–1986

THE LATIN AMERICAN DEBT CRISIS

The late summer of 1982 saw the onset of a crisis for the U.S. financial system, initiated when the government of Mexico discontinued its repayment of loans to the U.S. commercial banks. The prospect that that event might set off a series of defaults by the governments of what were, at the time, often called the less developed countries (LDC), many of them located in Latin America, prompted the crisis beginning in 1982 to be labeled "The LDC Debt Crisis" (for example, Sachs 1985; Federal Deposit Insurance Corporation, 1997, Chapter 5) or the Latin American debt crisis.

This episode does not count as a specific financial crisis in the chronology of U.S. financial crises constructed by Reinhart and Rogoff (2009). Rather, the Reinhart-Rogoff chronology classifies the years 1982 and 1983 as crisis-free years for the United States. In addition, according to the Reinhart-Rogoff classification, the U.S. financial crisis that they date as beginning in 1984 was the savings-and-loan crisis, rather than the Latin American debt crisis. But these classifications have great tensions with the accounts given by many who were closely watching events at the time. To them, the Latin American debt crisis was indeed a financial crisis as far as the United States was concerned. In this vein, Jeffrey Sachs observed in 1985, "For three years, the world economy has been threatened by the most severe financial crisis since the Great Depression… Commercial banks, the principal creditors of the developing countries, have seen a significant erosion in their market value that could be multiplied severalfold if the prospects for the LDC debt worsen significantly."[57]

Paul Volcker, too, recalled the seriousness of the this crisis: "I have learned in my old age that anything that happened more than thirty years ago, nobody remembers. They remember [that is, know about] the Peloponnesian Wars better than they remember [events of] 35 years ago. I mean, as the memories go back, they do always cite the banking crisis in the early '90s [and] the savings-and-loan crisis just shortly before that. And they were important crises, but there's no doubt in my mind the biggest crisis of the postwar period *before* this last one [in 2007–2009] was the Latin American debt crisis. Now, it was easier to manage [than that in 2007–2009]. It wasn't all that easy to manage. But it was confined to banks, to the big American banks, the big international banks. If that crisis had gotten out of hand, they all would have been bankrupt. But, still, it was within the banking system: the so-called shadow banking system wasn't very important in those days. And so it was easier to handle. But it was a very substantial threat to the banking system, no doubt about that." (Paul Volcker, interview, October 16, 2013.)

Friedman's attitude to the crisis

Friedman entered this episode with a record of skepticism about the prospect of a crisis involving U.S. commercial bank obligations to the Third World. The period 1976–1977 had seen the widespread airing of concerns about the possibility of an imminent wide-scale LDC default that would imperil the solvency of the large U.S. banks. Paul Samuelson, for example, expressed such concern in his commentaries at the time, including Instructional Dynamics Economics Cassette (Paul Samuelson series) Tapes 203 (second-half-July 1976) and 214 (early February 1977). Friedman, however, remarked in June 1977 that he had "much greater confidence in the business judgment of the banks than I do in the judgment of most of the people who write about the [possibility of] coming defaults. Therefore, as a matter of practice, I do not believe there will be any significant defaults. I don' believe that the great banks are going to lose any large amount of money on these debts. Most of them are perfectly good banking debts."[58] Indeed, earlier in 1977, an angry Friedman successfully sought a retraction when a financial magazine incorrectly claimed that he had remarked privately that billions of loans made by Citibank to the Third

[57] Sachs (1985, p. 15).

[58] Friedman (1977d, p. 29).

World were in a precarious state.[59] The fears of a debt crisis were, indeed, not realized in 1977.

Once, then, a crisis indeed proceeded to break out in August 1982, Friedman initially did not appear to appreciate its gravity. In a talk in London in September 1982, he mocked discussions of the recent developments that had given the impression that the normal scenario was one in which sovereign debt was paid off. He emphasized that, instead, refinancing, such as rolling over the existing aggregate amount of government debt via the issuance of new securities or the renewal of loans, was the routine procedure (*Financial Times* (London), September 23, 1982).[60]

Underlying this perspective was Friedman's position that the borrowing governments and the lending commercial banks should converge on a rescheduling arrangement for the loans, without third-party involvement by the U.S. government or the International Monetary Fund. That was a position that he would maintain in the years to come—such as in his explicit advocacy of privately-arranged debt rescheduling in his *Newsweek* column of November 14, 1983. In his initial reactions to the problems of Third World debt to banks, Friedman was, however, altogether too sanguine about the threat to the U.S. banking system posed by a further breakdown in the process of loan repayments. He may have owed the formation of this sanguine view in part to one of his closest friends in the U.S. banking system, Citibank's Walter Wriston. Wriston, who served alongside Friedman on the Reagan Administration's outside panel of economic advisers, became widely criticized for the dismissive attitude he took during the early 1980s to the prospect, and perhaps even to the concept, of a widespread default on the part of sovereign borrowers (see, for example, Sjaastad, 1989, p. 21; Zweig, 1995, pp. 705, 765).[61]

[59] *Financial World* (New York) stated (February 1, 1977): "We sincerely regret an attribution… that appeared in [our] Jan. 1 issue… [I]t was stated that Nobel Laureate Milton Friedman claimed that Citicorp may now have as much as $9 billion in doubtful overseas loans. Dr. Friedman informs us that he never made such a statement or said anything to anyone that could be construed in that manner. Our sources, and there were indeed more than one, had been quite reliable in the past and there was no reason to doubt them. Attempts to verify the statement with Dr. Friedman were impossible because, as he himself told us, he was out of the country. Obviously, some massive miscommunication occurred[,] and we apologize to both Dr. Friedman and to Citicorp."

[60] More generally, he later affirmed that "most [large] loans are never paid off, they're just turned over and renegotiated." (*California* magazine, October 1984, p. 164). In the same vein, Sebastian Edwards drew a distinction between households, which paid off debt, and governments and corporations, which typically did not: "I sit on boards, on corporate boards… [and] we never talk paying off our debt. We talk about different forms of debt and keeping it within certain limits—but never [of] paying it off." (Sebastian Edwards commentary during "Policy Seminar with John Cochrane: $r < g$," Hoover Working Group on Economic Policy, Hoover Institution, May 26, 2021.)

[61] Wriston may have been influenced by an analysis his economist staff conducted and put out in public form in mid-1976 (*Citibank Monthly Economic Letter,* June 1976) in an article titled "Why the Debt Time-Bomb Won't Go Off." This analysis took the position (p. 14) that it was a generic feature of sovereign borrowers that they did not default, and, in this light, it had to be remembered that "the overwhelming preponderance of bank credits to LDCs are those extended to governments and quasi-governmental agencies. So long as governments utilize their sovereign power to levy taxes, debts can be serviced and retired." In retrospect, the flaw in the article was less its position that

By late 1983, however, Friedman was acknowledging the gravity of the problem. "Whatever the reasons for the international debt crisis, it is real and serious," he now acknowledged (*Newsweek*, November 14, 1983).[62] He was outspoken during the fall of 1983 in continuing to press for a solution in which the U.S. public sector did not play the role of actively facilitating or financially guaranteeing new arrangements. The step that should be taken was to "require the people who make the loans the collect them," he had insisted at an appearance in San Francisco on October 4, 1983. "If they can, fine, and if they can't, that's their problem."[63]

A week later, in an October 11, 1983, speech in Honolulu to the American Bankers Association, Friedman reiterated his view that it was commercial banks that "should be given the responsibility of restoring the loans" (quoted in *Journal of Commerce* (New York), October 13, 1983). Later on, in July 1984, Friedman affirmed (House Republican Research Committee, 1984, p. 44): "The government should stay out of it and let the banks make their own deals with those countries any way they want to. That's their business."

In light of such remarks, Rudiger Dornbusch (1986a, p. 85) accurately characterized one perspective on the Latin American debt crisis as that manifested in the "recommendation by Milton Friedman, that the government should get out of the process altogether, letting the banks try to collect their debts if they can."[64] Friedman had neatly encapsulated the financial dimension of this philosophy in his *Newsweek* column (November 14, 1983): "If any loans go sour, the banks (i.e., their stockholders) should bear the loss, not the taxpayers."

Friedman's comments muddy the waters

In pressing his case that shareholders should bear the losses arising from bad loans, however, Friedman during this period made statements so clumsy and sweeping that they were at cross-purposes with a major message of his own past research. Notably, these included a written statement, in the form of a letter, that appeared in a Congressional volume. "National central banks that have sought to stave off financial collapse by shoring up borrowers who face default

"outright debt repudiations by governments" were rare (although this too proved to be a shaky proposition) than its claim that those foreign nations borrowing from U.S. banks would be anxious to "avoid even the appearance of debt rescheduling" and so would meet previously-contracted repayment schedules (p. 14).

[62] In addition, he wrote an endorsement for Makin (1984), a book that emphasized the gravity of the Latin American debt crisis. After 1983, Friedman continued to accept the "crisis" terminology—for example, in his statement (Friedman, 1985c, p. 215): "The debt crisis should be solved by negotiations between lenders and borrowers."

[63] Friedman (1984f, p. 38).

[64] Makin (1984, p. 257) had also named Friedman (as well as the *Wall Street Journal* editorial writers) as a prominent advocate of this position.

in order to help lenders have not succeeded," he wrote to Representative Clarence Long in September 1983. "Instead, the result has often been the postponement of relatively minor adjustments until a major collapse cannot be avoided. That was certainly true in the United States in the 1930s."[65]

No elaboration of the final sentence just quoted was provided. And that sentence was the most problematic one in Friedman's statement. For, in invoking the dramatic experience of the Great Depression, Friedman seemed perilously close to endorsing an interpretation of that epoch that was greatly at odds with that given in the *Monetary History*. Friedman and Schwartz had emphasized the maintenance by the Federal Reserve during the early 1930s of an excessively restrictive posture—with regard to its monetary policy tools as well as its banking-support and supervisory policies—as the factor responsible for having converted the private-sector credit-market disturbances of that period into a general monetary and financial collapse. And indeed, within a year of this 1983 letter, Friedman, as we shall see, was citing 1930s experience when endorsing a major federal intervention to protect banks. In so doing, he was taking a perspective recognizably aligned with that of the *Monetary History*.

A possible reconciliation of Friedman's 1983 letter with his other writings is that he was criticizing the notion that credit-market disturbances should lead to specific policy interventions to protect borrowers and would have preferred the authorities to concentrate on avoiding any repercussions of the crisis for total bank deposits. In past discussions of the 1930s, he had welcomed deposit insurance (established after the crisis) in this connection. The support for lenders in the 1930s that Friedman may have had in mind when expressing disapproval in 1983 may have been the authorities' focus on the existence of low short-term nominal interest rates—a state of affairs erroneously thought during the Depression years as, by itself, connoting easy credit. With regard to the 1930s, the "relatively minor adjustments" to which he made reference in 1983 may have been fortifying the money stock via central bank operations (as well as, on the support and supervisory side, a more generous approach when assessing the value of banks' assets and collateral). In the 1980s, the adjustments he regarded as desirable may have been writing off some of the banks' capital at an early stage, and he may have viewed the absence of this adjustment as leaving open the likelihood of a larger, and more disorderly, adjustment of banks' assets and liabilities later on. In contrast to such an approach, as Friedman emphasized in another—and better—September 1983 statement, the contractionary Federal Reserve posture before 1933 had turned good loans into bad loans (see Hulbert and Meltzer, 1983, p. 74).

[65] Friedman (1983c, p. 191a).

Still, the reading between the lines required to square Friedman's September 1983 letter with the Friedman-Schwartz account of the Depression is considerable. Friedman's body of public statements would have been more cohesive without the letter to Long, as well as earlier occasional *laissez-faire*-like statements that Friedman made about the separation of the government and banks—such as his statement in 1977 that he would "certainly let the banks collapse" in the face of loan defaults associated with Third World debt.[66]

Friedman statements like these, with their tough-sounding positions invoking the need for banks to accept the consequences of their loans and calling for governments to stay out on the sidelines, concealed more than they revealed about his perspective on the public sector's responsibility for the stability of the banking system. On this issue, his bark was worse than his bite, because of his limited conception of what it meant for a bank to bear losses. Crucially, Friedman's conception of letting a bank fail did *not* correspond to letting the bank close or have its depositors—or for that matter its bondholders—suffer losses. In urging that banks take responsibility for their loans, he was concerned that loan losses be absorbed by a lowering of banks' net worth (and so the incurrence of losses by the banks' equity investors), and not by the public sector. Yet he still remained of the view that major bank failures should be avoided—if necessary by the public sector stepping in. That this remained his view would become very clear during the dramatic developments in U.S banking in the summer of 1984.

The Continental Illinois rescue

Having to outward appearances established himself in 1983 as one of the proponents of a *laissez-faire* solution to the Latin American debt crisis, Friedman would soon become an enthusiastic supporter of a major U.S. public sector intervention that arose partly from the same crisis.

A clue to the transition in his commentary that would come in 1984 was evident in September 1983 when, speaking by phone to an audience in Washington, D.C., Friedman had remarked, "I see no justification for bailing out individual banks," but he added a qualification that the authorities should certainly be active in "preventing a broad financial collapse."[67]

In July 1984, federal authorities, including the Federal Reserve, having already taken measures

[66] Friedman (1977d, p. 28). Similarly, on October 25, 1983, Friedman (1984a, p. 8), after asking whether large banks should be allowed to go broke, remarked, "Why not?"

[67] In Hulbert and Meltzer (1983, p. 74). On Friedman's mode of participation in this event, see *Washington Times* (Washington, D.C.), September 22, 1983 (p. 8B).

to support the Continental Illinois bank during the spring (*American Banker*, May 21, 1984), undertook a full-scale rescue of the bank, whose severe financial problems had arisen in good part because Latin American government debts were on its loan book. Friedman's remarks in San Francisco a few days after the rescue were reported in the following terms (*San Francisco Chronicle*, July 28, 1984):

> Letting the bank fail would undermine confidence in the nation's banking system, Friedman said.
>
> The Continental Illinois rescue was correct, he said, because it protects depositors and creditors but does not assist the bank's shareholders. Friedman said the shareholders probably will lose their investment.

As this account of his remarks indicated, Friedman considered a desirable feature of the Continental rescue to be the fact that equity holders took losses. In that respect, the U.S. authorities' move adhered to his prescription in his November 1983 *Newsweek* column. In other respects, however, events had confounded the scenarios Friedman had envisaged in 1983. First, the Latin American debt crisis had had a severe effect on U.S. financial stability, something that Friedman had regarded as unlikely.[68] Second, even though he had earlier distinguished between rescuing individual banks and maintaining overall financial stability, it transpired that when a bank as large as Continental was threatened, Friedman concurred that its rescue was a necessary part of maintaining overall financial stability. The separation of an individual bank's problems and systemic problems was, therefore, not as clear-cut as he had likely implied in 1983. And third, even though he had painted deposit insurance as sufficient to protect depositors in the event that a bank had gotten into difficulties (*Newsweek*, November 14, 1983), the large wholesale-deposit component of the liabilities side of Continental bank's balance sheet meant that federal guarantees going beyond ordinary, legislated deposit insurance would be needed if Continental's debtors were to be protected (as Friedman felt they should be, on grounds of systemic stability).[69] Continental's large wholesale liabilities had also played an important role in making it a vulnerable bank, and Friedman would acknowledge that "banking crises such as the

[68] For example, in his October 1983 talk in Honolulu, Friedman had cast doubt on the notion that banks would be in trouble in a way that required federal intervention (*Journal of Commerce* (New York), October 13, 1983).

[69] Friedman's support for the protection of wholesale liability holders in the Continental rescue contrasts may seem jarring in light of the fact that he emphasized maintenance of the quantity of money in a financial crisis and the fact that he excluded those liabilities from his preferred money definitions. It may be that Friedman was concerned that wholesale deposit market disruption could spill over into a loss of confidence in retail deposits, through counterparty risks and other channels. See Nelson (2013) for a further discussion. Schwartz (1992, p. 64) likewise conjectured that "fear of contagion" was the "overriding concern" driving the rescue.

Continental Illinois" case had in part reflected the influence of financial innovations (in this case, the advent of the large, and highly news-sensitive, wholesale market for commercial banks).[70]

Friedman's 1983 statements dismissing the idea that official intervention would be warranted had, like many others' criticisms of federal intervention to stabilize commercial banks, been predicated on the notion that the equity buffer of a troubled bank was large enough to insulate deposits from loan losses.[71] When that had proven not to be the case, he was well disposed toward overt federal intervention to support the banking system.

Anna Schwartz, in contrast, was critical of the Continental bank rescue. "MF was mistaken about Continental Illinois," she insisted to the present author nearly a quarter-century later (Anna Schwartz, personal communication, April 2, 2008). Schwartz preferred that the federal authorities had declared a formal bank failure and concern themselves with regular deposit insurance and with maintenance of the overall money stock.[72] But Friedman saw the interconnectedness of the banking system as rendering this option impractical and making a more extensive federal intervention desirable. "I believe that the Continental Bank problem has been handled very well," Friedman said in the course of his July 27 remarks in San Francisco. "… If Continental Illinois had been allowed to fail, it would have set off a run on all major banks, and we would have been faced with another major financial collapse. I think the authorities were wise to avoid that."[73]

Basic to Friedman's support for the Continental Illinois rescue was the overriding importance in his mind of the need to avoid a repeat of the banking collapse of the 1930s and the monetary contraction that accompanied it. "There's no doubt in my mind that Continental Illinois was saved because [in 1930] the Bank of the United States failed," Friedman said (*California* magazine, October 1984, p. 77).[74] Paul Volcker's own account at the time of the rescue, in testimony on July 25, 1984, likewise stated that "we were founded so that there should be that

[70] Friedman (1986d, p. 645).

[71] In particular, Friedman's sanguine remarks of October 1983, appearing in Friedman (1984a, p. 8), seemed to take for granted that banks' problems would reduce their capital but would not threaten their volume of deposits.

[72] See Shadow Open Market Committee (1984b) and Schwartz (1992, p. 64). In Friedman and Schwartz (1986a, pp. 52–53), as discussed further below, the authors spoke favorably about the Continental Illinois rescue, using language that predominantly reflected Friedman's view but that, evidently, both authors could live with. These jointly written remarks took a considerably more restrained form than Friedman's solo statement in 1984 that the 1930s had led "to the recognition that you should not allow a major bank to fail" (*California* magazine, October 1984, p. 77).

[73] Friedman (1984g, p. 269). Mishkin (1989, p. 377) voiced support for the authorities' actions regarding Continental Illinois Bank for reasons similar to those expressed by Friedman.

[74] As many did, Friedman here misstated the name of Bank of United States as "Bank of *the* United States."

elasticity in the system. That's what a central bank is all about, to provide liquidity in those circumstances. We are just carrying out the most classic function of a central bank."[75] Volcker was, in effect, acknowledging that the Federal Reserve had heeded a lesson laid out in the Friedman-Schwartz *Monetary History*.

After the fact, Friedman would applaud the handling of the crisis by Volcker and the other authorities. He would cite the fact of "no significant spread to other institutions" as indicating that the central bank had been able to "cure" the crisis that Continental's troubles produced.[76]

The crisis atmosphere recedes

The Latin American debt crisis can be regarded as having peaked with Continental's troubles and its subsequent rescue (see López-Salido and Nelson, 2010). Dornbusch (1985) discussed the improvements in the debtor countries' economic condition during 1984, and Sachs (1985, p. 15) observed that "the dramatic crisis atmosphere of 1982–1983 has abated." The policy measures taken in 1984 to address the Continental Illinois bank's troubles led to further relief of the situation. Interviewed in 1985, Friedman said that answered that what prevailed was not "a major problem of a kind that calls for great bailouts by the government"—a formulation that both recognized the reduced feeling of crisis and left open the possibility that, in principle, a banking problem *can* be large enough to justify official capital injections.[77]

It is clear that although Friedman initially seemed sanguine about the situation and aggressively *laissez faire* in his approach to the resolution of the debt crisis, his attitude changed materially once a more-material threat to the U.S. banking system emerged. His reaction in 1984 confirmed what had not been clear in some of his more dogmatic-sounding 1983 statements: his fidelity to the Friedman-Schwartz analysis of the 1930s, including its emphasis on preventing bank runs, maintaining the banking system, and the recognition of the need for official recapitalization of the banking system if a systemic solvency problem did emerge.

As noted, however, after 1984 the fear of such a systemic problem faded as the debt crisis receded. And although over the second half of the 1980s the U.S. government and IMF continued to take a far heavier part in deliberations concerning the loans that Friedman would

[75] In Committee on Banking, Housing and Urban Affairs, U.S. Senate (1984b, p. 108). See also Nelson (2020b, Chapter 11).
[76] Friedman and Schwartz (1986a, pp. 52, 53).
[77] From Friedman's appearance on *Wall Street Week*, Maryland Public Television, November 15, 1985, p. 10 of transcript.

have liked, the low-profile resolution of the crisis involved an outcome much like that Friedman had envisioned in 1982 and 1983: rescheduling and renegotiation of the loans. Friedman's remarks in previous years that the international debt crisis would in large part be resolved by "refinancing" and "stretch-outs" of the loans were borne out.[78] Reflecting this result, Saunders and Subrahmanyam (1988, p. 14) observed: "In practice, LDCs and banks have dealt with these repayment problems by rescheduling outstanding loans into the future."

THE DOLLAR AND THE CURRENT ACCOUNT

In economic commentary during the first half of the 1980s, the behavior of the U.S. dollar exchange rate (see Figure 1) was frequently cited as a reason for concern. In particular, the combination of a strong and (until early 1985) rising dollar and high U.S. real interest rates was seen by many as a symptom of the crowding-out forces induced by the Reagan Administration's fiscal policy, especially over the economic-recovery period that began at the end of 1982. See, for example, Dornbusch and Fischer (1987, p. 665).

The Federal Reserve had, on this interpretation, been sufficiently tight over the period to insulate nominal aggregate demand growth and the inflation rate from being unduly high in the face of the fiscal expansion, but this posture had meant that the economy's adjustment to high federal borrowing had taken the form of high real interest rates, substantial capital inflows in the direction of the United States, and an extremely high U.S. dollar exchange rate. To those subscribing to this interpretation, the exchange rate's behavior might not justify an abandonment of floating exchange rates, but it was a sign of an unbalanced U.S. monetary policy/fiscal policy mix—with government borrowing reducing the private sector's ability both to raise credit and to spend on goods and services.

It was also suggested that the increase in the U.S. external imbalance arose from this policy mix. Part of the alleged effect of the U.S. budget deficit was to require U.S. businesses' investment expenditures to be financed by foreign saving—leading to saving being below investment and the related emergence of a sustained deficit in the current account of the balance of payments. The advent of current account deficits in the Reagan years is shown in Figure 2.[79]

[78] The quotations are from Friedman (1983d, p. 191) and Friedman (1984a, p. 8), respectively.

[79] The share of GDP shown in Figure 2 closely matches the table of annual figures for the series shown in McCallum (1996, p. 7).

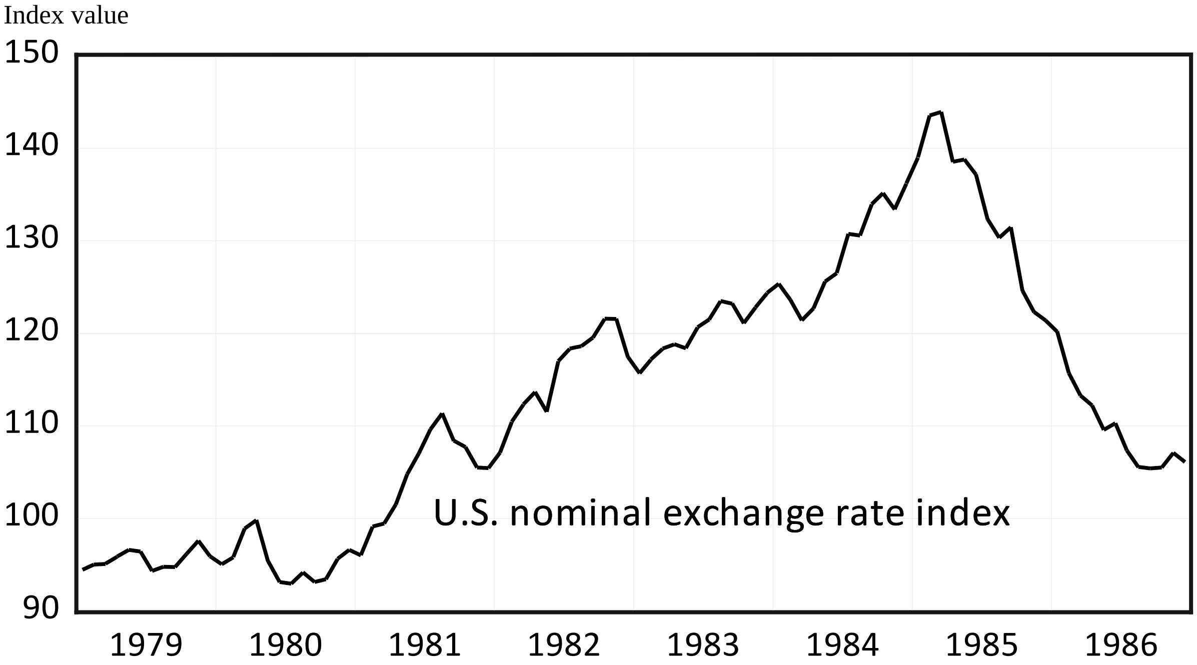

Figure 1. U.S. dollar exchange rate index, January 1979–December 1986.
Source: Federal Reserve Bank of St. Louis FRED portal, Nominal Major Currencies, U.S. Dollar Index (Goods Only) Index March 1973=100, *https://fred.stlouisfed.org/series/TWEXMMTH*.

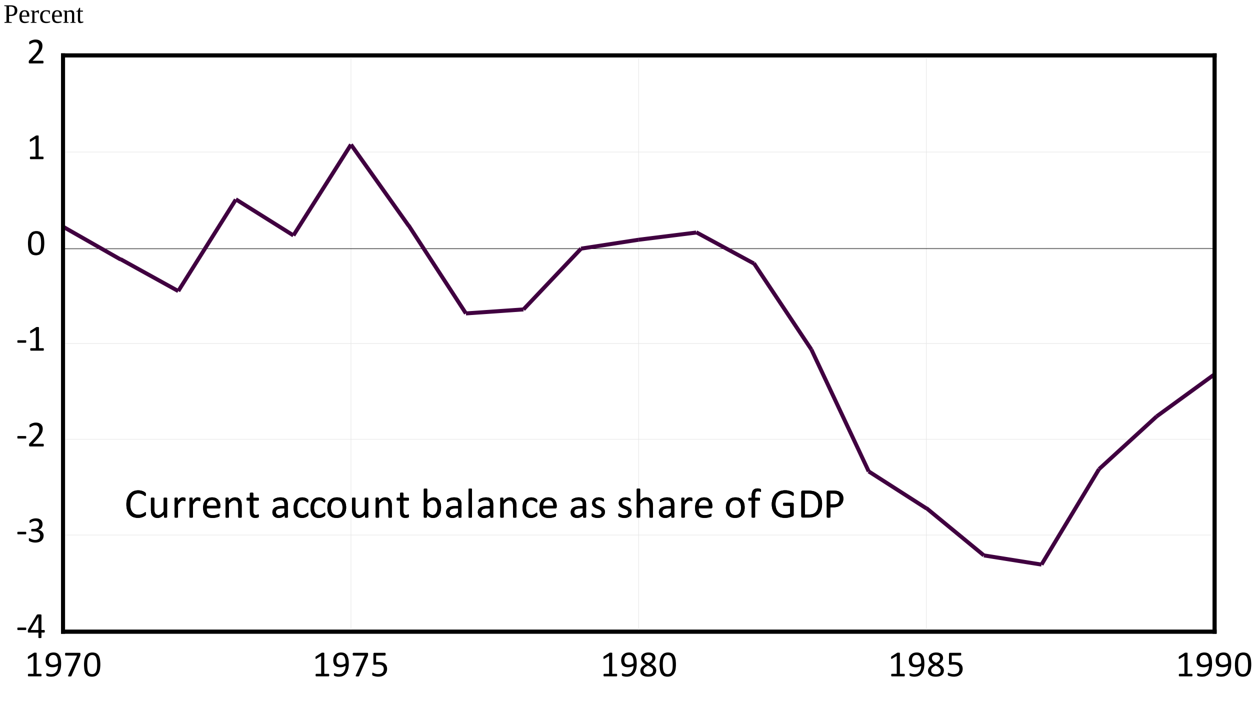

Figure 2. United States current account balance as share of GDP, annual data, 1970–1989.
Source: Council of Economic Advisers (2011, Table B–103, p. 308), divided by annual-data series on nominal GDP, Federal Reserve Bank of St. Louis' FRED portal (*https://fred.stlouisfed.org/series/GDPA*).

Up until the late 1970s, Friedman would likely have largely agreed with much of the basic analysis that had underlined these conclusions—particularly with regard to the crowding-out aspect of it. By the early 1980s, however, he could no longer accept it.

In particular, as discussed below, Friedman certainly concurred that high real returns in the United States were a source of strength in the U.S. dollar exchange rate.[80] But he did not regard it as the most important factor responsible for the high dollar. And he had become very doubtful about the importance of budget deficits as a factor driving real interest rates. The last of these developments is considered first.

Abandoning conventional crowding out

As discussed in Nelson (2020b, Chapter 13) and Chapter 8 above, Friedman had been a strong proponent during the 1960s and 1970s of the empirical importance of the crowding-out effects of fiscal stimulus measures—including tax cuts—and he had viewed this crowding out as operating via the generation of higher interest rates.[81] He did so frequently in the monetary policy/fiscal policy debate. By the early 1980s, however, although monetary policy seemed to have largely prevailed over fiscal policy in that debate, empirical evidence suggested that the emphasis that monetarists had put in that debate on crowding out had been misplaced.

In this connection, Meltzer (1995, p. xxvi) observed: "By 1984, we had recognized that the connection between budget deficits and interest rates was far from close." Friedman, too, had reached this conclusion firmly by 1984. By this point, he had been exposed to research such as Evans (1985). In this study, Evans had found no significant historical relationship between interest rates and the U.S. budget deficit.[82]

[80] The administration, including the president himself (see Reagan, 1983b), also acknowledged this.

[81] In the discussion that follows, "crowding out" refers to this interest-rate-raising channel rather than any other channel that might muddy the relationship between budget deficits and aggregate spending.

[82] This article was one of numerous studies Paul Evans made of Ricardian equivalence in the 1980s and early 1990s. Evans noted of Friedman that "he would take the view that what matters as far as government finance is concerned is what the government spends not what the government taxes. From my Walrasian perspective, for that [proposition] to be something that hung together, you would have to have something like Ricardian equivalence to be true, or at least an approximation to what the world was like. So Friedman might have bought into something like an approximation—that Ricardian equivalence was approximately true, [in which case] it could also be true that spending is what the burden of government is not the [amount of] taxes. I imagine he was quite aware that you would have to have something like Ricardian equivalence in order for it to be true for taxes themselves, except for their distortions, not be the burden that government imposes—but, rather, what it spends." (Paul Evans, interview, February 26, 2013.) See Chapter 8 for a discussion of Friedman's view that the cost of government was what it spent. Of his statements to this effect, one closest to aligning with Ricardian equivalence was the remark: "Ultimately, the tax that people pay is the amount the government spends." (*Daily Express* (London), November 30, 1976.)

Friedman had emphasized that it was the stock of public-sector debt, rather than budget deficits *per se*, that should on *a priori* grounds be expected to be related to interest rates (*Fortune*, March 19, 1984, p. 24; H.R. Heller and others, 1984, p. 47).[83] But even when using the stock of government debt in his analysis, Friedman, in empirical work he did around 1981, could not uncover a relationship.[84]

The matter of the link between interest rates and the federal budget deficit became an issue of controversy between the Reagan Administration and its critics. In her televised debate with Vice President George Bush in October 1984, Democratic vice presidential candidate Geraldine Ferraro maintained that cutting the deficit would reduce interest rates, "and I know the president [i.e., Reagan] doesn't believe that—but it's so." This subject also gave rise to intramural debate within the administration. As indicated, Reagan downplayed the deficits-interest-rate relationship, as did other figures like Donald Regan (Secretary of the Treasury until early 1985). But the Council of Economic Advisers under Martin Feldstein pressed the case for a link between deficits and interest rates, with the *Economic Report of the President* for 1983 (Council of Economic Advisers, 1983, p. 145) stating that "large federal budget deficits would preclude the continuing declines in real interest rates that are necessary for healthy growth in all sectors of the economy."[85]

Against this background, in 1981 and the following few years, Milton Friedman would articulate his new position downplaying the link between deficits and interest rates. In his *Newsweek* columns in 1981, he had been more tentative than previously about the influence of budget deficits on interest rates but still seemed willing to accept the notion that budget deficits produced by tax cuts could boost real yields (*Newsweek*, July 27 and September 21, 1981). But in an October 1983 appearance Friedman contended that there was only a "small" connection between deficits and interest rates.[86] And, writing at the end of the year, however, he described this mechanism as of second-order importance (*Newsweek*, January 2, 1984). Correspondingly,

[83] The emphasis on debt stocks rather than deficits also underlies much empirical work on the relationship between public borrowing and real interest rates—including some studies, such as Morris (1988) and Laubach (2009) that have found, for some sample periods, a significant influence of public debt on interest rates,. Engen and Hubbard (2004), however, provided a perspective on such work that emphasized that the relationships found, while statistically significant, were quantitatively modest.

[84] Friedman did not publish this analysis. In both an op-ed that appeared in the *Wall Street Journal* (April 26, 1984) and an April 1984 panel appearance in New York City (H.R. Heller and others, 1984, p. 47), he referred to it as work he had done "some years ago" and that had allowed also for international developments as factors bearing on the relationship between public debt and interest rates.

[85] Similarly, Feldstein wrote of the "high real interest rates in the United States that result from the large prospective budget deficits" (*The Economist* (London), June 11, 1983, p. 91).

[86] Friedman (1984f, p. 38).

he and Rose Friedman wrote in 1984's *Tyranny of the Status Quo* book that deficits had only been a minor factor behind interest-rate behavior.[87]

Later in 1984, Friedman remarked (*California* magazine, October 1984, p. 163), "I don't believe the deficit has much to do with interest rates." And in a conference held in Claremont, California, in March 1986, Friedman stated that the budget deficit had a very secondary—indeed, a negligible—role in interest-rate determination.[88] A position Friedman stated in 1984 and repeated in later years was that he was not aware of any analysis finding a very close and significant empirical relationship between real interest rates and U.S. public borrowing (H.R. Heller and others, 1984, p. 47; United Press International, October 9, 2001).

To reconcile this revised assessment of the empirical evidence with his theoretical framework, Friedman had to do no more than take his own permanent income hypothesis and do what Robert Barro, Robert Hall, and others had already done: hold that an infinite horizon, along with rational expectations, was a good working approximation.[89] That led to the Ricardian equivalence position, something with whose basic idea Friedman had long been in sympathy but a position that now became his own baseline way of looking at household reactions to fiscal policy.

Twin deficits and Ricardian equivalence

As discussed in Chapter 8 above, the Ricardian equivalence proposition was something Friedman had accepted as empirically valid by around 1978. He maintained that position.[90] In the Reagan years, it affected the way in which he interpreted not only the deficit/interest-rates relationship, but also the factors driving the current account deficit in the United States.

After moving into surplus during the late Carter period, the current account balance moved back into deficit in 1982. In both 1985 and 1986, the current account deficit was over $100 billion (Council of Economic Advisers, 2011, Table B103, p. 108). As already indicated, to some, the

[87] Friedman and Friedman (1984, p. 127; 1985, p. 123).
[88] See Hinshaw (1988, pp. 60, 61, 200). See also Hinshaw (1988, p. 15) on the details regarding the conference.
[89] Kormendi (1983, pp. 994–995) took the standard permanent income hypothesis as one in which government debt was taken to be net wealth and future taxes were not incorporated into households' calculations today. But this terminology seems inconsistent with Friedman's (1957a) original presentation. The latter allowed for an infinite horizon and, as discussed in Nelson (2020a, Chapter 5), even applied rational expectations in some implementations of the hypothesis.
[90] For example, in *Election 2004: The Economy* (KQED, San Francisco), October 15, 2004, Friedman remarked that today's government spending was paid for by the current generation via taxes and also "in the prospect of future taxes."

fact that budget deficits and current account deficits widened very considerably in the 1980s reinforced the Keynesian, or finite-horizon, view of the effects of the Reagan fiscal policy (especially its tax-cut component). A representative discussion along these lines was that of Dornbusch and Fischer (1987, p. 665), who stated that "both high interest rates and a foreign trade deficit are implied by the policy mix of exceptionally easy fiscal policy and tight monetary policy" and that "the evidence to date is on balance unfavorable to the Barro-Ricardo proposition" (p. 605). In opposition to this interpretation, Barro (1989, pp. 50–51) pointed out that U.S. budget and current account balances had not exhibited a positive correlation with one another prior to 1983. This feature of the U.S. data raised the possibility that the onset of the "twin deficits" in the 1980s reflected something other than a deviation of household behavior from Ricardian equivalence.

Friedman, too, doubted that the twin deficits arose from finite-horizon behavior or from the federal deficits drawing down national or international saving. There had been, he conceded, prominent episodes in the experiences of other countries when high budget deficits had coincided with high current account deficits (*Wall Street Journal,* April 26, 1984, and December 14, 1988).[91] And he did see instances in which this outcome had been undesirable. In this connection, he remarked in the course of the Latin American debt crisis that, in the case of some of the affected countries, the foreign debt accumulation had been problematic because it had arisen when national governments borrowed abroad to finance non-capital public expenditures (see Hulbert and Meltzer, 1983, p. 73).[92] Friedman gave Mexico and Argentina as cases in which the public sector had raised the funds and the borrowed "money did not go to assets that can be used to pay off the debt" (*Boston Globe*, October 6, 1982).[93]

Nevertheless, Friedman viewed those episodes not as evidence of finite-horizon private sector behavior but, instead, as being cases in which the public sector had significantly extended its

[91] James Lothian (personal communication, June 23, 2010) recalled that, during his time in the late 1960s and early 1970s as a graduate economics student at the University of Chicago, he was not taught that current account deficits were not regarded as inherently undesirable. Lothian remarked: "The qualification centers around monetarily driven current account deficits. These quite rightly were considered unsustainable." In the 1970s, Friedman cited the current account deficits of Italy and the United Kingdom in this latter context (Instructional Dynamics Economic Cassette Tape 154, September 24, 1974).
[92] With regard to public borrowing, Friedman was, of course, also critical when it was made with the covert aim of bolstering the exchange rate in a floating-rate environment (Instructional Dynamics Economic Cassette Tape 185, February 1976, Part 1), as an explicit measure to enforce fixed exchange rates (see, for example, Friedman, 1981, p. 20), or to finance overt foreign exchange intervention (see, for example, (Instructional Dynamics Economic Cassette Tape 200, October 1976, Part 1, and Friedman, 1978a, p. 157).
[93] The contrary case would be one in which public sector borrowing adds to the capital stock, generating short-run fiscal and current account deficits, but expands national productivity and the capacity to run and repay foreign debt. For an analysis of this case, see Clarida (1993).

command over economic activity and/or had engaged in inflationary policies. In such instances, he viewed the simultaneous expansions of the budget and current account deficits as symptoms of disruptive economic policies. Friedman did not see the expansion of the budget deficit in the Reagan years as fitting into this scenario.[94] He consequently did not believe that the U.S. current account deficits of the 1980s sprang from the budget deficits or that either deficit in itself was a cause for alarm.

Monetary policy, the dollar, and safe havens

Friedman eventually credited President Reagan's economic policies with promoting major U.S. capital inflows and so cited them as a key factor explaining the behavior of both the dollar and the current account balance. But he indicated that he regarded the relevant policy changes as largely those involving tax schedules—and not the expansion of the fiscal deficit (*Wall Street Journal,* December 14, 1988). If fiscal policy, and specifically budget deficits, had been largely ruled out, what did explain the behavior of the capital account, the dollar, and the current account, in Friedman's view?

Here, Friedman did conform somewhat to the conventional account of the dollar appreciation in allowing a role for monetary policy in exchange-rate determination. Longer-run monetary policy patterns, as represented by different country's inflation rates, had been cited by Friedman as a reason for the depreciation of the dollar in the late 1970s. They were, however, unlikely to be the reason for the U.S. dollar appreciation in the 1980s, as this period saw the U.S. inflation rate become much closer to the rates in the Federal Republic of Germany and Japan, and neither Friedman nor others appealed to the fall in inflation as a major reason for the appreciation. But another key means by which monetary policy affects exchange rates—via its short-run influence on domestic real interest rates and hence on cross-country real interest rate differentials—*did* likely play an important role, and Blanchard (1997, p. 278) plotted what he called the "strikingly good" relationship between the U.S. real exchange rate from 1980 to 1990 and the spread between longer-term real interest rates in the United States and the Federal Republic of Germany. In *Tyranny of the Status Quo,* Milton and Rose Friedman confirmed their acceptance of this factor as part of the reason for the strength of the U.S. exchange rate, as the Friedmans acknowledged that, indeed, the net capital inflow toward the United States was in part attracted by higher interest rates.[95] Both in this discussion and in *National Review* (April 6, 1984, p. 44),

[94] Friedman was even less willing to view the Reagan-era current account deficits in these terms once, in 1985–1986, he had accepted that inflation would not be making any major comeback.

[95] Friedman and Friedman (1984 pp. 126–127; 1985, pp. 122–123).

however, Friedman was keen to emphasize also that capital inflows, once attracted, made interest rates at home lower than otherwise, even under floating exchange rates.[96]

The coverage of the capital account in these 1984 Friedman accounts illustrates a broader point about Friedman's views on international economic relations. In their study of international economic relations, Laffer and Miles (1982, p. 390) treated Friedman's 1953 case for exchange rates as taking the overall balance of payments and the trade balance to be identical and so charged him with omitting the capital account when comparing fixed and floating exchange rates. In fact, however, Friedman's 1953 case for floating exchange rates had made mention of interest-rate-induced capital movements.[97] Furthermore, in that essay and his other writings over the decades, he had pointed to the demand for foreign financial assets as one driver of demand for foreign exchange (see Nelson, 2017), while the *Monetary History* had noted that capital flows put pressure on the exchange rate.[98] Consistent with the analyses, noted above, that Friedman gave in 1984, the *Monetary History* had also repeatedly pointed out that increases in home interest rates both attracted, and were moderated by, inflows of foreign capital.[99]

Friedman believed that the appreciation of the U.S. dollar in the 1980s indeed reflected rational behavior occurring on the capital-account side. But interest rates were only part of his explanation for the move of funds to the United States. There was an "even more" important factor underlying the capital inflow, he argued (*San Francisco Chronicle*, April 18, 1985, p. 20). This was the safe-haven demand for U.S. assets that the rest of the world possessed. "We are an island of stability and powerful growth in a sea of stagnation and uncertainty," Friedman observed (*Newsweek*, August 27, 1984). The Friedmans highlighted the contrast between political uncertainty elsewhere and the situation in the United States. The upshot, they contended, was that "the United States, for all its problems, is regarded as the safest haven for capital."[100]

The safe-haven interpretation of the situation facing the United States was also articulated by others—for example, by John Rutledge, who suggested that the United States' political and

[96] See also his remarks in *San Francisco Chronicle,* April 18, 1985, and in *The MacNeil/Lehrer News Hour,* PBS, November 12, 1985, p. 6 of transcript.
[97] See Friedman (1953a, p. 166).
[98] Friedman and Schwartz (1963a, p. 485). Likewise, in Instructional Dynamics Economics Cassette Tape 194 (June 1976, Part 3), Friedman remarked: "And, of course, if a country borrows capital abroad, that tends to make the exchange rate higher than it otherwise would be."
[99] See Friedman and Schwartz (1963a, pp. 52, 70, 147, 496).
[100] Friedman and Friedman (1984, p. 124; 1985, p. 121). See also Friedman's remarks in *San Francisco Chronicle,* April 18, 1985, and during his June 28, 1985, appearance at the Commonwealth Club (Friedman, 1985c, p. 217).

economic outlook made placing funds in the country the "least worst" option available to international investors (*American Banker*, September 22, 1983). President Reagan himself expressed the same sentiment in more positive terms when, in September 1985, he suggested that "the billions of dollars of foreign capital… invested in our private industries, invested in our government bonds" were a reflection of the fact that "we are the best and safest investment in the world today."[101]

As noted in Dornbusch and Fischer (1987, p. 665), however, the safe-haven argument regarding the dollar's strength, though it was heard in many quarters, was a minority position in explaining the U.S. dollar's strength. Its credibility as a description of developments in the 1980s has, however, been reinforced by two developments that occurred after Dornbusch and Fischer wrote. First the notion that capital flows into the United States often reflect safe-haven demands has been confirmed by later events. William Poole noted (interview, April 30, 2013) noted that "we saw that in spades during the financial crisis—a flow of money here into Treasuries for safekeeping. So that's what motivates those private flows."

Second, the same notion is buttressed by the chronology of the course of U.S. net capital inflows and international tensions over the 1980s. Although Friedman observed that safe-haven demand for U.S. assets as "what keeps the dollar high" (*Newsweek,* August 27, 1984), his more specific point was that safe-haven demand drove U.S. capital inflows.[102] The dollar peaked in 1985, but net capital inflow did not reach its maximum until later in the decade. With the flipside of expanding U.S. net capital inflows under floating exchange rates being a widening of the current account deficit, the peak in net capital inflow as a share of GDP can be seen as not having occurred until 1986, when the current account deficit reached 4.3 percent of GDP (see Figure 2). Analyses of the current account that emphasized responses of the demand for goods to exchange-rate movements treated as a puzzle the fact that U.S. dollar depreciation began in March 1985, but U.S. net exports did not stop declining until late 1987 (see Abel and Bernanke, 1992, p. 509). If, however, one views the U.S. capital inflow as having been kept high by international tensions, which then fell in 1987 as U.S.-USSR relations improved through the INF Treaty and other developments, it makes more sense that reductions in the current account deficit would be correspondingly postponed, as it is the sum of the current and capital accounts that had to balance under floating exchange rates.

[101] In Reagan (1985).

[102] See, in particular, Friedman and Friedman's (1984, p. 127; 1985, p. 123) reference to "the 'safe haven' reason for the capital inflow."

On the basis of the discussion above, it is clear that, far from viewing the exchange rate as driven solely by goods flows, Friedman saw the capital account as an important part of exchange-rate determination. Furthermore, far from regarding a widening current account deficit as *prima facie* evidence of a malfunctioning floating exchange-rate system, he saw such a scenario as something that could emerge in the normal course of events and in the absence of externalities.

Intertemporal behavior and the current account

The fact that Friedman was rejecting the notion that the U.S. budget deficit drove the country's current account deficit itself amounted to a claim that the American private sector would have generated a current account deficit in the 1980s, even absent the Reagan fiscal deficits. This was indeed Friedman's position. And there was another key aspect to his stand on current account deficits. This related to the interaction of the capital and current accounts: he did not believe that net capital inflows or outflows adapted passively under floating exchange rates to finance or offset a matching current account imbalance. Instead, Friedman contended that current account deficits or surpluses were often driven by capital flows—and that this could be an appropriate outcome of optimizing private-sector behavior.

In these ways, although rarely cited in this context, Friedman was an exponent in his public statements since at least the 1960s of what has been called the intertemporal approach to the current account.[103]

This approach was intimately linked to the permanent income hypothesis, especially its infinite-horizon version. And as Friedman's settled on the infinite-horizon version of the hypothesis in his economic analysis, he became a prominent, if basically nontechnical, exponent of the intertemporal approach to the current account in his public discussions of U.S. economic developments.

The dependence of the dollar on payments imbalances

Several of the main points embedded in Friedman's views on the current account balance are brought out by the unfolding of 1980s economic developments. The fact that the U.S. dollar exchange rate continued to appreciate from 1982 to 1985 (see Figure 1), even as the current

[103] For a discussion, which cited formalizations of the idea but not the Friedman statements of it, see Obstfeld and Rogoff (1995). See also Nelson (2017) for a related consideration of Friedman's views on the current account.

account deficit reemerged and deteriorated, was seen by some as confounding views of exchange rate determination.[104] Tobin and de Macedo (1980, p. 7), for example, noted that much open-economy analysis of the 1950s and 1960s viewed a freely moving exchange rate as driven by developments in the current account balance. This continued to be the case in much later work such as Dornbusch and Fischer (1980), in whose model the exchange rate adjusted, under floating rates, to deliver a zero current account balance. Likewise, *The Economist* (May 8, 1982, p. 15) had editorialized that it was a "commonsense assumption of the 1970s" was that current accounts surpluses would generate exchange-rate appreciation. Some observers consequently regarded the sustained dollar appreciation in the face of current account deficits as a blow to advocates of floating exchange rates.

Friedman did not see things this way, however. His 1953 analysis did contend that an exchange rate depreciation would improve the current account balance—a position in line with his assumptions of short-run nominal price stickiness and of the relevance of the Marshall-Lerner condition.[105] Correspondingly, he did believe that, starting from a position of a current account deficit, there was some exchange-rate depreciation large enough to eliminate the deficit (*The Economist* (London), January 3, 1953). But he did not contend that the exchange rate, if allowed to float, moved to that value. As discussed in Chapter 3 above as well as Nelson (2020c), it is not the case that Friedman's case for floating exchange rates embedded a claim that properly functioning floating rates would deliver a zero current account balance. In fact, he rejected the position that the current account necessarily converged to zero under floating exchange rates: rather, its appropriate value could be nonzero (*Wall Street Journal*, June 30, 1975).

This reality of Friedman's position puts into perspective claims such as those of Walker (1983, p. 119): "The experience with floating exchange rate regimes in the 1970s has questioned the long-held view [held] by many academic economists that such a system can eliminate balance-of-payments problems. Imbalances, both surplus and deficit, do not seem to be completely eliminated by a flexible exchange rate." Walker's error lay in imputing to Friedman's case for floating rates a position that the individual capital and current account balances would be zero under this system. Instead, Friedman only maintained (correctly) that the bottom line of the two accounts would sum to zero: the imbalances of the current and capital accounts would be of equal magnitudes in absolute value and also opposite in sign to one another.[106] Correspondingly,

[104] See Aschheim, Bailey, and Tavlas (1985) for a discussion.
[105] Friedman reaffirmed his (1953a) position that devaluations or depreciations improve the trade balance on several later occasions, including in his commentary in Instructional Dynamics Economics Cassette Tape 14 (February 1969).
[106] That is, what has been called the official settlements balance would be zero.

once experience with floating rates accrued, Friedman affirmed that his position that no balance-of-payments problem existed under floating rates held true in the presence of ongoing current account imbalances.[107]

Interaction between the capital and current accounts

This stance of Friedman's was linked to his emphasis on the joint determination of the capital and current accounts of the balance of payments. In fact, he gave a vital role to changes in the capital account in the shaping of the current account balance—an assignment reflected in the Friedmans' observation: "In any event, the trade balance would improve only to the extent that the capital inflow was discouraged."[108]

William Poole, who was on the Council of Economic Advisers during Reagan's first term, had a similar perspective to Friedman's. Poole noted (interview, April 30, 2013) that "obviously, the international accounts are simultaneously determined. But there's so much discussion that seems to view the capital account as simply financing [the current account imbalance] passively... But… the only thing that private financing is doing is financing the capital investment. They're not financing any particular flow of goods into the United States. It's determined by their views on where good investments are, and also, of course, risk and return considerations, safety." The perspective in which the operation of the balance of payments involved the determination of the current account balance, with the capital account surplus then determined residually was, for Poole a case in which "people so often to have it upside down. It just annoys me." Those who expounded things this way included "not just people in the lay audiences that I spoke to [as a reserve bank president]. But it seems to me that, a lot of times, some economists [also] make that mistake."

The longstanding tradition in economic discussions in which the capital account balance was a residual generated by developments centered on the current account was identified by Debelle and Galati (2007, p. 992), who observed: "The literature tends to ignore the fact that the current account and the capital account are jointly determined, and that the causality may run from the

[107] *Milton Friedman Speaks*, Episode 8, "Free Trade: Producer Vs. Consumer," taped April 27, 1978, p. 13 of transcript.

[108] Friedman and Friedman (1984, p. 127; 1985, p. 123). Similarly, Friedman remarked that Japan's current account surplus arose because "Japanese residents find it desirable to invest part of their savings abroad" (*National Review*, November 15, 1985).

capital account to the current account, rather than vice versa."[109] The deep-seated nature of this tradition was apparent in 1983 when CEA chairman Martin Feldstein, in a piece that lamented the fact that "so much of the current discussion of exchange rates assumes incorrectly that the only purpose of the exchange-rate system is to balance trade and that capital flows are merely an offsetting adjustment," himself slipped into the standard phraseology when he referred to "trade flows and the *resulting* capital flows" (*The Economist* (London), June 11, 1983, p. 91, emphasis added).[110]

An emphasis on the joint determination of the capital and current accounts was linked to the idea that a current account deficit might be appropriate. The notion that persistent current account imbalances could reflect optimizing behavior by the private sector, and not be grounds for concern, can be traced to a number of places in the literature on open-economy macroeconomics, including items that were the work of figures at the University of Chicago other than Friedman.[111] But Friedman had spelled out the argument himself for years before the 1980s. For example, in discussing what would happen if Canada's domestic supply of savings was insufficient to achieve its desired capital stock, he stated that, under a market-determined exchange-rate arrangement, "the exchange rate will then move and this will tend to create the balance-of-trade difference required to accommodate the capital movement."[112] It was something that he further articulated in his research writings in the 1980s, not only in his aforementioned public-policy writings during that decade, but also in some of his research writings over the same

[109] Of course, there was a countercurrent—especially prevalent among those accustomed to viewing the economy in simultaneous-equation terms—in which it was realized that, conditional on overall balance-of-payments equilibrium, a capital inflow implied a current account deficit and that it was inappropriate to think of the capital account as generated residually by the current account. R.J. Ball (1962, p. 619), for example, warned against relying solely on analyses in which a current account deficit "induces a capital inflow[,] rather than the other way about."

[110] The treatment of the capital account as passive was also prevalent in media discussions in the United States—and in Australia, whose political and economic discourse centered on the current account deficit in the years from 1986 to 1988. Notably, the economics editor of the *Sydney Morning Herald* insisted that the "only fundamental solution to the foreign debt is continued reduction of the current account deficit" (November 14, 1988) and that the amount of overseas borrowing made by the private sector had no implications for the aggregate current account deficit—his contention being that the public sector would have had to make those borrowings to finance the deficit, had the private sector not done so (*Sydney Morning Herald*, February 21, 1987).

[111] See, for example, Johnson (1976, p. 3). Other key statements to this effect also were presented in University of Chicago forums: for example, a 1975 *Journal of Political Economy* article (Bowers, 1975, pp. 1074–1075), and Corden (1977), which was cited by Congdon (1992, p. 263) as a key reference in this connection and which consisted of guest lectures delivered at the University of Chicago. Other statements of similar positions by others who had had University of Chicago affiliations included those by Larry Sjaastad (1989) and by Robert Barro (1985b). The latter remarked (p. 684): "it is hard to see why the current account deficit is particularly a 'policy problem' that calls for any governmental action." Another clear statement of the proposition that fluctuations in the current account balances can reflect optimizing behavior—one not of University of Chicago origin—was Tsiang (1977, p. 332). Pitchford (2003, p. 35) also pointed toward a number of non-University of Chicago sources on the intertemporal approach to the current account.

[112] Friedman and Roosa (1967, p. 95).

years. Notably, 1982's *Monetary Trends* had described a situation in which a capital inflow tended to generate a current account deficit as part of the normal operation of floating exchange rates.[113]

He spoke of the United States' nineteenth-century trade deficits as having been appropriate and beneficial—there was "nothing wrong" with the creation of such a deficit in the circumstances prevailing at that time.[114] Specifically, Friedman emphasized that the onset of a current account deficit facilitated capital flows into the United States in that era.[115]

For a long stretch of the postwar period, the Bretton Woods system made the scenario of largely floating rates alongside a current account deficit of limited applicability. And even as that system loosened from the late 1960s onward, Friedman was initially inclined to see this combination as a rare occurrence. He thought that foreign nations—with the exception of countries like Canada, which seemed more favorably disposed toward floating rates, and the United Kingdom, which seemed likely to float in order to avoid recurrent balance-of-payments problems—would want to accumulate dollar assets and would fix their exchange rates against the dollar with that aim in mind. In the event, however, from 1973 onward, floating by countries of their exchange rate against the U.S. dollar was much more prevalent than Friedman expected. And it proved to be compatible with countries' desire to accumulate U.S. assets: instead of needing to run balance-of-payments surpluses to do so, they could run current account surpluses against the United States in the context of a zero overall payments balance. This could happen on a large scale because the United States' long postwar period as a net exporter of capital funds ended and, as a counterpart to its new status as a net capital importer, it ran aggregate current account deficits—a change evident between the time of the writing of *Free To Choose* (when the Friedmans took for granted that the U.S. economy exported capital) to that of *Tyranny of the Status Quo* (when they discussed the new situation in which the U.S. imported capital).[116] The analysis that Friedman had applied to the situation of the nineteenth-century situation therefore became relevant again for the United States in the 1980s.

In the 1980s, Friedman's position that the current account deficit was a natural outgrowth of

[113] See Friedman and Schwartz (1982a, p. 335).
[114] *Milton Friedman Speaks*, Episode 8, "Free Trade: Producer Vs. Consumer," taped April 27, 1978, p. 12 of transcript; *San Francisco Chronicle*, January 23, 1979.
[115] See Friedman and Friedman (1980a, p. 43), and *Milton Friedman Speaks*, Episode 8, "Free Trade: Producer Vs. Consumer," taped April 27, 1978, p. 12 of transcript.
[116] See Friedman and Friedman (1980a, p. 43; 1984, pp. 124–127; 1985, pp. 121–124). The transformation of the United States to a net capital importer was discussed in Council of Economic Advisers (1983, pp. 53–54).

optimizing behavior made him scornful of interpretations that the opening of the U.S. current account deficit reflected the lack of national competitiveness. The current account deficit reflected, in his view, the United States' status as a safe haven for foreign investors—and it would tend to persist as long as the U.S. remained an especially attractive place to invest. The United States had "a more attractive economy in which to hold assets than almost any other," Friedman remarked, and the current account deficit signified "the strength, not the weakness, of the U.S. economy." (*San Francisco Chronicle*, April 18, 1985, p. 20.)[117]

"Favorable" deficits

As well as emphasizing lower interest rates and higher investment in the United States as potential benefits arising from a capital account surplus, Friedman stressed the positive aspects of the corresponding current account deficit.

The *Tyranny of the Status Quo* book criticized the fact that current account deficits for being "misleadingly termed" an unfavorable balance of trade."[118] This remark echoed a passage in the *Free To Choose* book, in which the Friedmans had stressed that a country's gain from foreign trade consisted of the imports acquired and that as many of these as possible should be sought for a given volume of exports.[119] Friedman's highly critical attitude toward terminology that described a trade deficit as an unfavorable balance of trade, or an increase in the trade balance as a deterioration, stemmed from his belief that the terminology was flawed because it was exports, not imports, that involved a sacrifice of goods for U.S. consumers: " 'good' and 'favorable.' This is the usual terminology whereby imports are bad, and exports are good—which is the opposite of the economic case." (Instructional Dynamics Economics Cassette Tape 21, April 1969.) "It cannot be repeated too often that the benefit from foreign trade is the imports that we get," he testified in September 1971.[120]

"In both our countries," Friedman remarked in Japan in the spring of 1972, "there is the idea that you gain by exporting and what you lose is what you import. The truth is that it is the other way around. But, because the voice of the producer is louder than that of the consumer, this kind of mercantilism still persists." (*Japan Times* (Tokyo), April 15, 1972.)

[117] For more on competitiveness, see the discussion titled "Jack Kemp" in Section III below.

[118] Friedman and Friedman (1984, p. 126; 1985, p. 122).

[119] Friedman and Friedman (1980a, p. 41).

[120] From Friedman's testimony of September 23, 1971, in Joint Economic Committee, U.S. Congress (1971, p. 702).

Friedman therefore looked on a higher current account deficit in much the same way as he viewed an improvement in the terms of trade. Such an improvement allowed more imports to be purchased for the same volume of U.S. exports, while a higher current account deficit likewise typically meant imports could be higher for a given volume of exports. "The U.S. gains from imports[,] not exports," Friedman observed in early 1978. "Imports contribute to our standard of living. Exports are a cost… The larger the volume of imports we can get per each unit of exports the better."[121] His "defense of dumping," discussed in Chapter 9, reflected this view.[122]

Foreign debt and current account deficits

Friedman's benign view of trade and current account deficits when the United States' trade account, expressed once deficits first emerged in the early 1970s, had been previewed by the experience of a U.S. trade deficit with Japan, which was of longer standing than the aggregate U.S. trade and current account deficits. "If the Japanese want to send us wonderful televisions and transistors in return for pieces of paper, that's wonderful," Friedman remarked in this connection on one occasion. "There's no industry we have a greater advantage in than the printing of government paper." (*St. Petersburg Times* (Florida), November 17, 1971).[123]

This statement was made in reference to the case in which the financial item flowing to the foreign country was noninterest-bearing base money. But Friedman indicated that this conclusion prevailed also if the government paper received by foreigners in international transactions when the U.S. ran a trade deficit consisted of bills or bonds, rather than noninterest-bearing assets (Instructional Dynamics Economics Cassette Tapes 5, November 1968, and 120, May 11, 1973). As he increasingly emphasized during the 1980s, he had the same view if the capital inflow matching a current-account deficit took the form of foreigners' purchases of debt or equity that had been issued by the U.S. private-sector (*National Review,* April 6, 1984, and November 15, 1985).

[121] Friedman (1978a, p. 157).

[122] In the same vein, William R. Allen, in a January 1986 broadcast in his economic-commentary series, cast trade deficits as conferring both extra savings and extra goods on the deficit country—"it would be great to have all those foreigners… shower us with valuable stuff as a gift, taking none of our production in payment" (Allen, 1987, p. 90). In the same vein, the *Chicago Tribune* (September 1, 1985, Section 7, p. 1) had quoted an unnamed U.S. government economist as having observed: "Why is everyone complaining about our debt to foreign countries? It's a very nice arrangement. They give us goods, and we give them paper."

[123] Friedman made similar statements in *National Review,* August 11, 1970, p. 818, and in his testimony of September 23, 1971, in Joint Economic Committee, U.S. Congress (1971, p. 702). Some years later, he characterized the accumulation of dollars by central banks in other countries as being, in effect, a gift to the United States (*Milton Friedman Speaks*, Episode 9, "The Energy Crisis: A Humane Solution," taped February 10, 1978, p. 36 of transcript).

Friedman conceded that a current account deficit involved incurring liabilities to the rest of the world. But he regarded this concern as of less primacy under circumstances in which the current account deficit arose from private-sector behavior. Here, Friedman viewed the private investment financed by capital inflows as likely to generate income that would service the liabilities (*Wall Street Journal*, April 26, 1984, and December 14, 1988).

This prediction was conditional on the inflow actually having the effect of "strengthening our capital stock" (*National Review,* November 15, 1985), and it is on this matter that Friedman entered his main qualifications about the private-sector agents' borrowing from abroad that accompanied by current account deficits. "Insofar as they do so for long-range investment purposes, that is fine," Friedman observed.[124] In this connection, in 1997 Friedman remarked that a capital account surplus "can be a surplus for productive investment, or it can be a surplus for portfolio investment." He implied that the latter could be "a real problem" under some circumstances, although even here he cited government policy at home as what might generate such inflow (*Wall Street Journal*, February 12, 1997).[125] Again, then, Friedman's perspective on current account deficits that were associated with private-sector behavior remained benign. Even in the event that the investment involved did not generate the expected return, it was the lenders whose funds were at risk: "It's the foreign investors, who are hostages to fortune, not we." (*Christian Science Monitor* (Boston), July 2, 1987.)

III. PERSONALITIES IN INTERNATIONAL ECONOMIC POLICY AND GEOPOLITICAL DEVELOPMENTS, 1982–1986

RUDIGER DORNBUSCH

In 1985, Rudiger Dornbusch of the Massachusetts Institute of Technology was commissioned by London's newly formed Employment Institute to give a talk—and write an associated booklet, which would appear as Dornbusch (1985b)—making the case for more expansionary economic policy in the United Kingdom. In celebrating Dornbusch's involvement, Richard Layard, the head of the new institute, described him as "the Milton Friedman of the 1980s: a popularizer, and

[124] Friedman (1978a, p. 157).

[125] Along the same lines, he had remarked on television in 1985: "Why do we have a trade deficit? We have a trade deficit because people around the rest of the world want to acquire dollar assets. The other side of the trade deficit is the capital surplus. It's the fact that people elsewhere are investing in U.S. [capital] goods, in government bonds, and other assets. Now, insofar as it goes into bonds [presumably here meaning U.S. Treasury securities], that's not a very good thing. Insofar as it goes into factories and structures, that is a good thing." (*The MacNeil/Lehrer News Hour*, PBS, September 30, 1985, p. 9 of transcript.)

the best economist of his generation." (*The Economist* (London), April 27, 1985*b*.)

Although Friedman could hardly complain about being, in effect, described as the best economist of his *own* generation, Layard's implication that there was a vacancy to be filled when it came to the position of the "Milton Friedman of the 1980s" was not, of course, something he could altogether welcome. That said, Layard's statement had some validity despite the fact that the original Milton Friedman was still around. Based at the London School of Economics, Layard had experienced at close hand the times in which Friedman had been ubiquitous in economic discussions in U.K. public debate. And it was legitimate to recognize that that situation was now in the past. It was increasingly clear by the early months of 1985 that Friedman's profile outside the United States had receded.

By the mid-1980s, Friedman no longer had the virtual omnipresence in the international economic scene that had seemed to characterize the years from 1969 to 1982. The description of Friedman in a book review in a London publication (*Director*, January 1986) as being "frail at 73"—presumably an allusion to his heart attack in October 1984, discussed in the previous chapter—was certainly an overstatement: Friedman remained active after 1984, and he bounced back before very long from his heart attack and subsequent surgery. But Friedman, who had been known to say that he felt he carried the weight of the world on his shoulders (*Los Angeles Times*, December 14, 1986, p. 14), certainly did cut back on the pace of his international travel. Reflecting this situation, in January 1986, having last visited the United Kingdom for a Mont Pelerin Society meeting at Cambridge University in September 1984, Friedman wrote to Peter Jay that he had no plans to visit the country again in the foreseeable future.[126] And although Friedman visited Tokyo, post-heart attack, for the Bank of Japan's biennial monetary policy conference in May 1985, this proved to be his final attendance of that conference series. When, in November 1986, the Japanese government gave Friedman the Order of the Sacred Treasure for his contributions to the nation's economic policy, he opted not to travel to Tokyo for the event (*Gettysburg Times* (Pennsylvania), November 3, 1986).[127] International coverage of, and esteem for, Friedman's views on economic topics had also taken a knock by 1985–1986, in the wake of widespread disaffection with monetary targeting as a monetary policy strategy. In addition, Friedman was also much less active in the research world than previously.

In contrast, Rudiger Dornbusch remained a prolific producer of economic research in the mid-

[126] Letter from Milton Friedman to Peter Jay, January 30, 1986, provided to the author by Gloria Valentine.

[127] On Friedman's decision not to go to Japan on this occasion, see Friedman and Friedman (1998, p. 413).

1980s and had increasingly, as Layard noted, become a popularizer of his own positions on economic policy. And Dornbusch, originally himself from Europe, did this popularizing on the world stage. Dornbusch's 1985 critique of the Thatcher Government's economic policy was a prime example. In making an intervention of this kind in the U.K. debate, Dornbusch was following in the footsteps not only of Friedman but also of his MIT colleagues Paul Samuelson (who had written a couple of times a year for the *Financial Times* for decades until 1981, and who had also blasted Friedman and monetarism in occasional pieces in London's *Sunday Telegraph* during the late 1960s and early 1970s) and Robert Solow (who had made some contributions to U.K. debates on monetarism and the Phillips curve during the 1960s). It was the lineage between Dornbusch and the Samuelson-Solow tradition that prompted one critic to remark on Dornbusch's 1985 activities that he "had hoped that the days when Massachusetts Institute of Technology professors were wheeled in to solve our [that is, the United Kingdom's] economic problems were behind us." (*Financial Times* (London), July 30, 1985.)

The same critic acknowledged, however, that "Professor Dornbusch has made distinguished contributions to exchange rate theory." Indeed, it was in this context that Dornbusch's and Friedman's names were most likely to be found together. The floating exchange-rate system had been in force for a decade by the mid-1980s. In public-policy and research circles alike, Friedman was still frequently mentioned as the most eminent advocate of that system. And Rudiger Dornbusch was widely recognized as having, in his research work, taken major strides toward the development of a dynamic model of how a floating-rate system operated.

The Dornbusch overshooting model

Dornbusch had done so primarily through his "overshooting" model, expounded in a 1976 *Journal of Political Economy* paper "Expectations and Exchange Rate Dynamics" (Dornbusch, 1976).[128] Although Dornbusch had a background as both a student and a teacher at the University of Chicago, the article was published after Dornbusch joined MIT in 1975. Stanley Fischer recalled: "It was written as an explanation of why it was, to the surprise of so many, real exchange rates fluctuated a lot—much more than I think anybody would have expected *ex ante*. And I remember [at MIT] that you'd go to seminars, and you'd hear these guys," including Dornbusch, who had "all walked around saying, 'After all, the exchange rate is an asset price.' I must confess that I heard that story and I couldn't quite believe it. [Or] I couldn't quite figure

[128] Dornbusch (1976) did not cite Friedman's work. In an article published roughly simultaneously, however, Dornbusch and Krugman (1976) named Friedman as a pioneer in the literature on floating rates. See also Dornbusch (1986b).

out why it was so important. I think I now understand, [and] I think they were *right* to emphasize that… But it implied it would behave more like stock prices than like GDP—and I think it does, in terms of variance and so forth. And they knew that, and then they were trying to formalize it. And I think Rudi got that formalization. I mean, Rudi's formalization doesn't have much formal discussion of the asset markets. It just uses covered interest arbitrage. But he certainly, out of that, managed to generate an exchange rate which would be quite variable in response to a monetary shock." (Stanley Fischer, interview, August 30, 2013.)

In his 25-year retrospective on the Dornbusch (1976) paper, Rogoff (2002, p. 5) implied that the article's content amounted to an attack on floating exchange rates as well as on Friedman's case for them. The notion that the article was an attack on floating rates was also implied by Keegan's (1989, p. 173) claim that references to financial market overshooting amounted to "a euphemism for getting things wrong, and failing to produce the optimal outcome so beloved of the pure market theory in the economics textbooks." However, as Fischer noted with regard to Dornbusch (1976), "It was *not* written as an attack" on floating rates (Stanley Fischer, interview, August 30, 2013). The article instead constituted an attempt to understand the dynamic operation of a floating exchange rate, and the rate's interaction with other variables, in a situation of sticky prices, rational expectations, and flexible exchange rates. In this context, "overshooting" of the exchange rate did not mean a misaligned or bubble-driven value of an exchange rate but, instead, the property that the exchange rate's reaction to a monetary shock in the short and medium run might exceed its ultimate reaction.

Nor with respect to overshooting was Rogoff's (2002, p. 5) position that Dornbusch's finding was "[c]ontrary to Friedman's rosy depiction of life under floating" really accurate. Friedman's exchange-rate essay in 1953 had included some inconclusive speculation about whether, under a float, exchange-rate dynamics would feature overshooting or monotonic responses (undershooting) to shocks under various circumstances.[129] But his writings elsewhere—in particular, his many discussions of how monetary policy shocks spread gradually throughout the economy—confirmed that he was comfortable with—indeed, espoused—the idea that asset prices' initial response could exceed their ultimate response. And in the years after Dornbusch

[129] See Friedman (1953a, p. 183)—a passage that stated that it was "clear" that there would be initial overshooting but then tentatively contemplated a sequence of overshooting and undershooting. (It is this passage that underlay Meltzer's, 1993a, p. 105, contention that Friedman's chapter anticipated the later literature on exchange-rate overshooting.) James Meade's case for floating rates in the same era had more categorically endorsed overshooting, in the statement (Meade, 1955, p. 14): "At first this depreciation will go further than is necessary to achieve a long-run equilibrium in the country's balance of payments; whereupon the currency will appreciate again as the price adjustments due to the depreciation work out their favorable effects…"

(1976) appeared, Friedman’s own descriptions of exchange-rate dynamics certainly incorporated the overshooting phenomenon. For example, in April 1981, he remarked, “if there were a time lag, the exchange rate would go down some more” to secure a zero balance of payments in response to a shock.[130]

Non-insulation from real shocks under floating

Although the Dornbusch (1976) analysis did not really amount to a critique of floating rates, Dornbusch did elsewhere voice reservations about the floating-rate system. One of these reservations pertained to country insulation. Dornbusch (1983) implied that advocates of floating exchange rates had claimed that floating rates allowed a country to block transmission to the home economy of shocks from abroad. Similarly, in a paper on the implications of the open economy for the U.S. business cycle, prepared for a major National Bureau of Economic Research conference convened in Puerto Rico on March 22–25, 1984, Dornbusch and Fischer (1986, p. 459) stated: “Supporters of a shift to flexible exchange rates—and by the end this included most economists—believed that a shift to floating rates would enable countries to insulate themselves from foreign disturbances. That did not happen.”[131]

These statements might be implied as being critical of Friedman’s case for floating rates. But they did not actually provide a valid basis for such criticism. Friedman had made a much more limited claim than that what Dornbusch and Fischer attributed to proponents of floating rates. Friedman had not contended that flexible exchange rates shut the home economy off from the influence of real shocks abroad—such as those arising from foreign countries’ fiscal policy. Indeed, in contending that such a broad claim regarding insulation had been made in the literature, Dornbusch (1983, p. 3) could, in fact, only provide a non-Friedman quotation, from Harry Johnson (1969, p. 12), making the claim in question.

Friedman based his own argument concerning policy autonomy under floating rates on *monetary policy* insulation—and, by implication, insulation of the determination of nominal variables. So he was not vulnerable to this criticism of floating-rate advocates given in these 1983 and 1986 Dornbusch and Dornbusch-Fischer articles.

Indeed, Friedman’s writings and statements in the 1950s and 1960s implied that floating rates

[130] Friedman (1981a, p. 20).

[131] On the date and location of the conference, see Gordon (1986, p. xiii).

affected how an economy could respond to a real shock originating abroad but did not make the economy immune to that shock: see the discussion in Chapter 3 above and in Nelson (2020c). This was a position Friedman reaffirmed in the floating-rate era. For example, in Instructional Dynamics Economics Cassette Tape 203 (November 1976, Part 2), Friedman remarked that "with floating rates… countries are still affected by what happens in other countries—of course."

On television in September 1985—showing signs of having tired of being characterized as claiming that floating rates eliminated the influence of international factors on the economy—Friedman stated: "I think the world economy is very important, and I believe what happens in other countries will affect us, what we do will affect them. But we don't have much control [over] what other countries do. And, therefore, I tend to concentrate on those things over which we have some control."[132]

The basic Friedman position on floating rates and their implications for countries' economic interaction was embedded in Barro's (1984) undergraduate text *Macroeconomics*, which appeared in its first edition in 1984. Barro (1984, p. 543) observed: "The main point about flexible exchange rates is that they allow each country to pursue an independent monetary policy. Hence, countries can differ in their rates of inflation and monetary growth, as well as in their nominal interest rates." But he added that "flexible exchange rates do not isolate a country economically from the rest of the world." He affirmed in a later passage (p. 547) that "under flexible exchange rates, a country can pursue an independent monetary policy, which allows for an independent choice of inflation rate." These statements not only conveyed Barro's views. They also accurately reflected Friedman's longstanding positions. The main additional element in Friedman's perspective was that, being more inclined than Barro was to emphasize price stickiness, he saw the monetary autonomy conferred on the domestic authorities by a float as implying that they had a sizable influence on real economic outcomes over periods up to several years.

Inflation and exchange-rate depreciation

Something that Friedman *did* claim with regard to floating rates was, however, also challenged by Dornbusch (1983). Friedman had, as noted, argued that floating rates gave a country autonomy in the setting of *nominal* variables—most prominently, as Barro (1984) highlighted in the passage just quoted, inflation. Dornbusch (1983), however, implied that, for practical

[132] *The MacNeil/Lehrer News Hour*, PBS, September 30, 1985, p. 9 of transcript.

purposes, autonomy with respect to setting inflation might not hold. He emphasized exchange-rate fluctuations as a source of variations in inflation and suggested that these fluctuations might frustrate a country's monetary authorities' attempts to control inflation. In particular, he contended that the "exercise of policy autonomy becomes nearly impossible flexible rates," especially in the case of smaller economies than the United States (Dornbusch, 1983, p. 4).

This reasoning, under which depreciations meant that floating rates added to inflation instead of giving the monetary authority control over inflation, had been advanced previously in earlier years of the global postwar float, including by Paul Volcker when he was president of the Federal Reserve Bank of New York in the late 1970s.[133] But, as discussed presently, the argument now looks like a much less substantial criticism of a floating-rate system than it probably seemed to be to many observers in the late 1970s and the 1980s.

The extent to which exchange-rate movements might weigh on inflation was brought out when Dornbusch (1983, p. 5) laid out a model in which the price equation was $p = \alpha w + (1–\alpha)(s + p^*)$ in which p is log CPI, w is log nominal wage costs (governing the price of domestically produced output), s is the nominal exchange rate and p^* log the rest-of-world CPI, and α is the share of imports in the home CPI. The implied expression for the inflation rate $\pi = \alpha\Delta w + (1–\alpha)(\Delta s + \Delta p^*)$ implied powerful *ceteris paribus* implications of exchange-rate depreciations, Δs, as they would matter very strongly and directly for inflation.

This line of thinking was followed up by Dornbusch and Fischer (1986) in their paper for the 1984 conference mentioned above. Friedman did not attend the conference. But Anna Schwartz did—and was one of Dornbusch and Fischer's discussants. Her comment took exception to their statement (Dornbusch and Fischer, 1986, p. 484): "The exchange rate must play a part in explaining United States inflation and in assessing the impact of policy changes on the price level." Schwartz (1986b, pp. 504–505) argued instead that exchange-rate change "does not 'explain' inflation. It is a necessary adjustment to inflation that originates in monetary actions."

Schwartz's way of phrasing things did not contemplate the case in which exchange-rate movements could not be accounted for by monetary policy developments. It therefore possibly left room for the scenario that Dornbusch (1983) sketched: one in which exchange-rate movements not explicable in terms of macroeconomic phenomena occurred—and these variations, in turn, generated movements in inflation that frustrated the control of that series

[133] See Chapter 9 above.

through monetary policy.

Schwartz's basic position that the exchange rate offered little guidance in understanding inflation has, however, proven correct. Stock and Watson (1999, p. 305), for example, observed: "Models incorporating exchange rates do not perform as well as the benchmark Phillips curve model or the univariate autoregression." The corollary is that models such as that in Dornbusch (1983) have turned out to overstate greatly the link between exchange rates and inflation. In so doing, they have exaggerated *both* the extent to which inflation can depart from the course implied by monetary policy (for a weak exchange-rate/inflation link means than capricious variations in the exchange rate will not break the monetary policy/inflation link) *and* the extent to which inflation control in practice is accomplished via the impact of monetary policy on the exchange rate (as domestic spending or the output gap seem to have been much more reliable indicators of inflation than exchange rates have been).

Friedman, of course, going back to 1953 had conceded a short-run influence on exchange-rate variations (for given monetary policy and potential output): an exchange-rate induced change in import prices would be felt in the aggregate price level until other prices adjusted and the new, permanent relative-price structure emerged. Indeed, Friedman partially drew on this argument in 1984—when he erroneously believed that the U.S. inflation rate was going to head sharply upward (see the previous chapter)—in stating that "the dollar [being] high… keeps pressures against higher domestic prices very, very heavy" (*Newsweek*, August 27, 1984). This interpretation now seems misplaced. Inflation was not really poised to take off. And, in light of the evidence of a weak link between the exchange rate and inflation, it appears appropriate to conclude that the exchange rate was likely a much less important factor holding down inflation in 1984 than Friedman thought. A different influence of open-economy influences on inflation—the impact of the real exchange rate or terms of trade on output gap—is one Friedman had especially emphasized after the first oil shock and that seems more empirically durable than one based on a strong link between exchange rate changes and final consumer prices.

Trade deficits, jobs, and the "drift to protectionism"

Another point of contention that Schwartz (1986b) raised in her discussion of Dornbusch and Fischer (1986) concerned their specification of aggregate spending and so of the short-run determination of aggregate output. The authors had postulated that real aggregate demand was boosted by net exports and, hence, was lowered by imports (Dornbusch and Fischer, 1986, pp. 475, 496). This specification was founded on standard open-economy economics. But its possible policy implications troubled Schwartz, who argued (1986a, p. 507): "The notion of transmission through a multiplier effect of foreign purchases of home goods that leads to an increase in output and employment in my view encourages the drift to protectionism."

Schwartz's suggestion that analysis of this kind implied a negative impact on imports on output and employment was, in fact, reinforced by the manner in which Stanley Fischer described the channel in question in other work he put out at around the time of his open-economy paper with Dornbusch. In a 1984 NBER working paper, Fischer referred to "the unemployment resulting from the reduced demand for exports."[134] In the published version of the same paper, appearing in the *Quarterly Journal of Economics* in 1988, Fischer strengthened the wording, referring to "the unemployment caused by a trade deficit."[135] This seeming endorsement of the idea that trade deficits gave rise to unemployment gave credence to Schwartz's interpretation of the Dornbusch-Fischer (1986) analysis.

In elaborating on her reservation about models that presupposed a deficit-unemployment connection, Schwartz (1986b, p. 507) protested: "We know from the current recovery in this country that it is feasible to achieve a strong growth rate of real GNP with a balance of trade deficit." By the time she wrote these words, Milton Friedman had been giving considerable attention to the alleged negative implications of imports and trade deficits for jobs in the United States—and developing a challenge to the notion that imports lowered U.S. employment.

Friedman devoted a *Newsweek* column to the subject "Do Imports Cost Jobs?" (February 9, 1981). He was writing in Reagan's early weeks in office and was motivated by a recent call by Neil Goldschmidt, a member of the departing Carter Administration's cabinet, for restrictions on imports of Japanese automobiles. The subject of the connection between imports and U.S. economic performance was something that Friedman would return on numerous occasions over the next several years, as the "drift to protectionism" to which Schwartz referred became a notable, if contained, aspect of the Reagan years. Shortly after Reagan's reelection, Friedman lamented that the "measures that the government has taken to limit imports" had been "numerous" (*Wall Street Journal*, November 27, 1984*b*). Even only halfway through Reagan's first term, he had devoted a *Newsweek* column (November 15, 1982) to indicting Reagan's record on free trade. In that column, he noted that the Reagan Administration had reacted in 1981 to the concerns about car imports from Japan by securing ostensibly voluntary quotas and

[134] Fischer (1984, p. i).
[135] Fischer (1988a, p. 27).

had, he observed, more recently secured an agreement on the limitation of imports from the European Economic Community. These measures, he complained, belied the "administration's professed adherence to the principle of free markets." Friedman would have further cause for complaint when, in a speech delivered in early 1983 in San Francisco, Reagan made the case for free trade but went on to announce still more protectionist measures.[136]

Consequently, international trade was the area, Friedman remarked, "in which I would be most critical" of Reagan's economic record in his first three years in office (*Wall Street Journal*, October 25, 1983). As well as the Japanese car import quotas, he cited "the enormous jump in the tariff on heavy motorcycles… the recent measures on specialty steels from Europe, the subsidization of grain exports to Egypt." (*Wall Street Journal*, October 25, 1983.) The Friedmans repeated this catalogue, while also mentioning a couple of additional measures, in *Tyranny of the Status Quo* to support their judgment that "President Reagan… has approved one protectionist measure after another."[137]

Friedman's writings on the matter during these years dealt specifically with the issue of whether import limitation would lower either unemployment or the trade deficit. In both cases, he was highly skeptical that such a reduction would result from the imposition of increased protection. His "Do Imports Cost Jobs?" column acknowledged that some U.S. employment is "certainly lost to imports." But Friedman, in effect, appealed to general-equilibrium implications of changes in imports to argue against a net negative impact of higher imports on aggregate U.S. employment. He stressed the fact that import limitation would tend to reduce U.S. exports by lowering the proceeds—U.S. dollar receipts—available to the rest of the world for purchase of U.S. goods.

In terms of the model later laid out by Dornbusch and Fischer (1986), Friedman was in effect setting out an argument that did not contradict their specification of the net-exports/aggregate-demand connection but that appealed to channels that would operate in a more complete model. Dornbusch and Fischer had taken the exchange rate as determined outside their model and had concentrated on the reaction of net export demand to the real exchange rate. Friedman was highlighting the dependence of the value of the exchange rate on the relative demand for U.S. and rest-of-world currencies. And his analysis advanced the notion that this mechanism worked

[136] Reagan delivered the speech at the Commonwealth Club on March 4, 1983 (see Reagan, 1983b) and Friedman and Friedman (1984, p. 128; pp. 124–125) and Friedman's op-eds in the *San Francisco Chronicle* (April 18, 1985, p. 1) and the *Wall Street Journal* (April 20, 1987) made reference to it.

[137] Friedman and Friedman (1984, p. 124; 1985, p. 120).

to offset the negative implications of higher imports for U.S. output of the kind that Dornbusch and Fischer's specification would later highlight.

Friedman's former student Rachel McCulloch (1983, p. 17) criticized the analysis in the "Do Imports Cost Jobs?" column and judged that his conclusion had been incorrect. McCulloch took Friedman as having claimed that the imposition of a tariff would give rise to an exchange-rate appreciation that *exactly* offset the impact of the tariff on net exports. McCulloch argued that this conclusion was not actually implied by formal open-economy modeling and that his reasoning had reflected an inadequate model structure. It is true that Friedman's column certainly did not amount to a full model-based analysis. But the column had not, in fact, claimed that there would be an exactly offsetting exchange-rate movement when a tariff was imposed—only that reduced payments of U.S. dollars to the rest of the world that would arise from making imports more expensive to U.S. consumers would push the U.S. exchange rate up and so discourage exports. This position would be reaffirmed in *Tyranny of the Status Quo* and would be summarized in Milton and Rose Friedman's reference to the "self-canceling character of trade restrictions."[138]

Friedman followed these discussions up in a pair of op-eds in the *San Francisco Chronicle* on April 18 and 19, 1985. In these articles on international trade and trade deficits, he reiterated the analysis in the earlier 1980s writings, stressing that the exchange rate would (*ceteris paribus*) adjust upward to limit the incipient change in the net exports arising from tariff increases. So a tariff would reduce imports—but would also create an appreciation that reduced exports. But, perhaps sensitive to the McCulloch criticism of his earlier analysis, Friedman stated specifically that there was "no way of knowing" whether a new tariff would leave net exports higher, unchanged, or lower, on net (*San Francisco Chronicle*, April 19, 1985, p. 20). Instead, he underlined his perspective that it was really developments on the capital account of the balance of payments that drove the aggregate U.S. current account balance. And, as he had previously, Friedman emphasized that others' conclusion that import restriction could add to U.S. employment by no means followed even in the event that the imposition of a tariff was associated with a reduced trade deficit. For, in the event of a lower trade (and current account) deficit, the greater expense incurred to foreigners in obtaining U.S. dollars had, as a corollary, a lower net capital inflow into the United States. And as the capital inflow associated with a trade deficit provided funds that supported U.S. production—"facilitating the maintenance of employment," as Friedman put it (*National Review*, November 15, 1985)—a lower level of

[138] Friedman and Friedman (1984, p. 128; 1985, p. 124).

capital inflow would reduce the number of jobs in the United States.[139]

It was, therefore, once again, not obvious that imports cost the United States jobs, on net.[140] Indeed, Friedman suggested, as Schwartz had in her 1984 conference comments on Dornbusch and Fischer (1986), that the evidence of the current expansion pointed in the opposite direction: in the face of a widening current account deficit, the United States had had "a more rapid rise in employment during the [post-1982] expansion than in the typical expansion" (*San Francisco Chronicle*, April 18, 1985, p. 20).

It deserves stress that although their 1986 paper did give weight to employment-reducing implications of imports, Dornbusch and Fischer were themselves strongly opposed to protectionist measures against imports. Their *Economics* principles text had been critical of moves to protect the U.S. automobile industry from Japanese competition and concluded by telling readers that "the economist's basic attitude toward tariffs is opposition and skepticism" (Fischer and Dornbusch, 1983, p. 859).

JACK KEMP

Despite the shift by the Federal Reserve and other central banks during the period from 1982 to 1986 away from a focus on monetary aggregates, Friedman's position was widely regarded during this period as having prevailed on a key issue regarding monetary arrangements: Floating exchange rates had continued to be prevalent in the industrialized world.

Even the exchange-rate mechanism (ERM) of the European monetary system, launched in 1979, did not yet constitute a major exception to this pattern: membership in the ERM was limited, and major bilateral exchange rate changes within the ERM occurred during the first half of the 1980s. Furthermore, ERM members continued to float, jointly, against the U.S. dollar. So did many

[139] See also Friedman and Friedman (1984, p. 125; 1985, p. 123).

[140] This position and its implications for the effects of import restrictions were conditional on a system of flexible exchange rates. Friedman granted that, starting from a situation of deficient aggregate demand, a new tariff imposed in a situation of fixed interest rates could reduce unemployment, by serving as a *de facto* devaluation (Friedman, 1968a, p. 2; *The Economist* (London), June 4, 1983, p. 36). Of course, a key basis for this acknowledgment on this part was his familiar perspective linking money and aggregate demand. With changes in the overall balance of payments able to affect the money stock under fixed exchange rates, and with devaluations leading to lower balance-of-payments deficits (or increased surpluses), the introduction of a tariff in the context of fixed-exchange-rate arrangements amounted to a means of raising the aggregate money stock. This was a property that did not carry over to the floating-rate case, and Friedman remarked (*Euromoney*, October 1977, p. 25): "Given a floating exchange rate, trade protection does not reduce unemployment but only shifts it from export industries to import-substituting industries."

currencies outside the ERM—with both Australia and New Zealand becoming floating-exchange-rate nations in the mid-1980s, and the Japanese yen also continuing to float.

As already noted, the willingness of countries to float against the U.S. dollar contradicted Friedman's longstanding expectation, reaffirmed during the 1971–1973 breakup of the Bretton Woods system (Instructional Dynamics Economics Cassette Tapes 82, September 27, 1971, and 120, May 11, 1973) that the "dollar standard" nature of the modern international monetary system implied that most countries would decide to peg their exchange rates against the U.S. dollar even if they did not participate in a formal, multi-country fixed-exchange-rate system.[141] Instead, as Friedman noted on several occasions in the 1980s, the Western world's monetary arrangements had gone in a different direction, one without precedent: floating exchange rates at the international level, and, at the national level, monetary policy arrangements in which countries' domestic currencies were not redeemable, even on a restricted basis, in the form of gold or other concrete anchor to the monetary system.[142] Although he was critical of the absence of rules governing the setting of monetary policy in these countries, the new arrangement did conform to Friedman's desire for policy decisions to be governed by domestic considerations and exchange rates to be market-determined.

Consequently, for all his reservations about U.S. domestic monetary policy in this new regime, Friedman expressed satisfaction with the operation of the international side of it. His assessment around mid-1983 was that floating exchange rates have worked "extremely well" in the decade after the end of Bretton Woods: he cited specifically the way in which the system had allowed the world to adapt to the two oil shocks of the 1970s (Heertje, 1984, p. 43). In contrast, he wrote, it was "far from clear" that the Bretton Woods system had actually been a success (*Newsweek*, May 30, 1983). Friedman stressed, however, that the problems associated with that system had consisted not just of the rigidities usually associated with fixed exchange rates, but also of the considerable latitude that the system had given many countries to pursue domestic

[141] See Nelson (2020b, Chapter 15) and Chapter 3 above. As discussed under "The Dollar and the Current Account" in Section II of this chapter, a key reason for this change was that—contrary to Friedman's initial expectations—floating rates did not prevent the rest of the world from accumulating U.S. dollar assets, as U.S. current account deficits became the norm. Ronald McKinnon (interview, January 23, 2014) contended that, on the matter of being able to run current account deficits on an open-ended basis, some developing countries were at one extreme, while "the U.S. is at the other extreme: being the center of the world dollar standard, it has an unlimited line of credit with the rest of the world and can run deficits forever. I mean, that seems to be the case, the last 20 years, anyway, or 30 years. Because [we] can issue international money to cover the deficit, and so, everyone's willing to buy U.S. Treasury bonds, knowing that they can always be converted into current dollars, if necessary. So the U.S. has this easy line of credit in the current system, but other countries don't have it."

[142] See, for example, Friedman (1982a, p. 99; 1985c, p. 213; 1985e, p. 13; 1986d, p. 643; 1987d, p. 7; 1988d, p. xxi) and his remarks in *New York Times*, December 26, 1985.

economic policies that turned out to generate inflation (*The Economist* (London), June 4, 1983, p. 37).[143]

These judgments about the relative merits of floating and fixed rates, although shared by many other economists, put Friedman at odds with a number of vocal commentators, including those in policy circles. In particular, he continued to face notable opposition on the issue from people who were allies with him on many domestic-policy issues, such as tax policy and deregulation. Notably, as indicated in Chapter 12 above, numerous figures in the supply-side movement were closely associated with advocacy of systems of fixed exchange rates, particularly those centered on commodity standards. Arthur Laffer was one such figure, but in the 1980s the supply-sider whom Friedman most found himself pitted against on exchange-rate issues was Republican Congressman Jack Kemp of New York.

The continuing gold movement in the 1980s

Writing in mid-1982, Friedman lamented the fact that, notwithstanding their agreement on many issues, he and Kemp were so at odds on domestic and international monetary matters (*National Review*, July 23, 1982). On this occasion, Friedman objected both to Kemp's characterization of actual U.S. monetary policy as "monetarist" and to Kemp's proposal to restore an official peg of the price of gold.

Kemp pressed the attack on both the domestic and international monetary fronts, including via an endorsement in 1984 of a book, *Beyond Monetarism*. The book was by Marc Miles, a former student of Arthur Laffer who had become a member of the economics department at Friedman's Alma Mater of Rutgers University. Miles' book amounted to one of the first book-length critiques of monetarism to originate in the United States—albeit from a supply-side, not a Keynesian or central-banker, perspective. Once his book appeared in print, Miles was disappointed by the lack of reaction on the part of monetarists.[144] But Miles' book, which was critical of both monetary aggregates and floating exchange rates, did receive pre-publication endorsements from anti-monetarist supply-siders—including Jack Kemp, who called it a "lucid account by a brilliant young economist of exactly how and why monetarism has failed."[145]

[143] See also Friedman (1985d, p. 67; 1987d, p. 19).

[144] Miles observed: "The monetarists paid me the best compliment they could. They ignored it. If you attack something, you bring attention to it. If you ignore it, maybe it will go away—and that's what they did." (Marc A. Miles, interview, February 20, 2014.)

[145] A paperback edition appeared in 1986, by which time Miles moved from academia to the private sector. Kemp wrote a different endorsement for the paperback version.

By this time, the supply-side movement had suffered a loss to monetarists on the issue of international monetary arrangements. Pressure from Kemp and some other Republican politicians for a return to gold-oriented monetary arrangements had led Congress to set up a commission in late 1980 to study the role of gold in the domestic and international monetary systems. After the resulting report had appeared, Friedman would refer to the "eminent members of the Gold Commission," who included Federal Reserve Board governors, but also implied that he had high regard for the commission's staff (*Newsweek*, May 10, 1982). This was not surprising, in view of the identity of a prominent staff member. Although she was not herself one of the members of the commission, Anna Schwartz headed its staff as the executive director for the Gold Commission, and she was credited with "organizing the Commission's deliberations" (Gold Commission, 1982, p. vi). In this capacity, she was in effect, drafter of the first, and main, volume of its report, issued at the end of March 1982.

Schwartz's appointment had helped put paid to supply-siders' hopes that the Gold Commission would provide an endorsement of a return to a gold-based regime.[146] But the appointments of the commissioners themselves had also gone in this direction: what struck Schwartz was how little support for fixed exchange rates there was among the Gold Commission members (see Schwartz, 1982b, p. 543). The predominant sensibility of the commissioners was reflected in a passage of the majority report that Friedman later quoted: "We favor no change in the flexible exchange rate system."[147]

Friedman regarded even the written submissions and testimony provided to the Gold Commission by supporters of a gold standard or some other form of fixed exchange rates as having damaged their cause because that material had shown that, while the submitters all opposed the floating-rate *status quo*, they exhibited a wide range of views when it came to their favored alternative system.[148]

Friedman exuded confidence that gold was obsolete in monetary policy in a remark in his *New Palgrave* dictionary entry on the quantity theory of money, written in 1985 and published in 1987, he remarked: "The 'gold' that central banks still record as an asset on their books is simply the grin of a Cheshire cat that has disappeared."[149] He believed that the obsolete status of gold in

[146] See Friedman and Friedman (1984, p. 100; 1985, pp. 98–99) and Bordo and Friedman (1987, p. 5). See also Schwartz (1982).
[147] Gold Commission (1982, p. 20). Quoted in Friedman (1985d, p. 69; 1987d, p. 19).
[148] See his remarks in Friedman (1984b, p. 45), Friedman and Friedman (1984, p. 100; 1985, p. 98), Hulbert and Meltzer (1983, p. 76), and *Wall Street Journal*, December 18, 1985.
[149] Friedman (1987, p. 7).

the U.S. and world system should be more overtly recognized by the U.S. government: "I have long favored auctioning off our gold stock to the highest bidder." (*Newsweek*, May 10, 1982.) This selloff, he stressed, should include not only the U.S. holdings held in Federal Reserve vaults but also the governmental gold holdings in Fort Knox in Kentucky.[150] In early 1978, he suggested that a preannounced selloff, carried out over three to four years, might be desirable, in order to minimize disruption.[151] However, in October 1983 Friedman indicated that he would not object to the authorities of the world, including the International Monetary Fund, selling off their gold immediately and all at once: "Gold markets are capable of absorbing very large volumes of additional gold."[152]

Commodity price standards

Of course, if the gold-standard advocates had their way, such holdings, instead of disappearing from central banks' balance sheets, would return to prominence, as the Federal Reserve's obligation to manage the world price of gold would be restored. Among these advocates, Friedman identified Kemp specifically with the desire to restore the pre-1971 gold-price peg, with fixed exchange rates instituted in conjunction with the peg. That being the case, Friedman regarded Kemp's position as one of support for the "pseudo-gold standard" of the kind that had characterized the Bretton Woods system, rather than advocacy of a full-fledged gold standard (*National Review*, July 23, 1982).

Other proposals that had figured heavily among supply-side opponents of floating exchange rates were proposals to stabilize the prices of a basket of raw materials prices. The aforementioned critique of monetarism by Marc Miles had proposed a monetary policy directed toward this goal.[153] By 1985, Kemp himself described his preference as "a modernized, improved, forward-looking Bretton Woods international monetary system with stable exchange rates, with an

[150] See *Reason*, June 1975, p. 92, and *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978, p. 34 of transcript.

[151] *Milton Friedman Speaks*, Episode 7, "Is Tax Reform Possible?," taped February 6, 1978, p. 34 of transcript.

[152] In Hulbert and Meltzer (1983, p. 73).

[153] See Miles (1984, pp. 181–195). Among academic supporters of fixed exchange rates in the 1980s, a range of proposals was put forward. Ronald McKinnon, for example, proposed fixed exchange rates anchored not by gold but by a global monetary growth rule (see McKinnon, 1982). Underlying McKinnon's proposal was the notion that currency substitution had made it infeasible for a country to anchor its price level using domestic monetary policy tools. Friedman believed that McKinnon was underestimating the degree of monetary policy independence possible under flexible exchange rates (Friedman, 1984b, pp. 42, 59). Consistent with Friedman's contention, Woodford (2009) has presented an analysis supportive of the position that an independent monetary policy remains feasible for a country that lets its exchange rate float in a globalized financial system.

anchor. That anchor might be a basket of commodity prices."[154]

The revival of the Bretton Woods system alongside the U.S. authorities' pegging of a commodity-price basket would not necessarily have implied a return to what Friedman sometimes called an "honest-to-God" gold standard under which international flows of gold (or, more generally, of the commodities whose prices were pegged) drove variations in each country's money stock. In May 1985, he mocked the Bretton Woods arrangements for having relied on "the pretense that the U.S. was on a gold standard."[155]

But he could not deny that the adoption of a commodity-price-rule-based exchange-rate system would mean a major change in U.S. economic policy. Nor could he deny that, if adopted, a commodity-based exchange system would likely prove a far greater constraint on the monetary policy of the United States and other countries than had been the case in the original Bretton Woods system. This was because the features that had permitted so much leeway for the Federal Reserve under the Bretton Woods arrangements—such as foreign exchange controls and restrictions on residents' trading in the commodity, gold, whose price was pegged—were now gone, while foreign exchange controls had been abolished by 1985 in several other countries too, including the United Kingdom, Australia, and New Zealand.

Friedman was adamantly opposed both to a restored gold-price peg and to the institution of a more general arrangement of commodity price stabilization. Speaking on BBC radio in 1954, Friedman had remarked that commodity-price fluctuations "cannot easily be eliminated without, in the process, doing more harm than good."[156] Asked about gold-price and multi-commodity-price-fixing proposals thirty years later, Friedman affirmed, "Gold is a terrible index of general prices," and rejected the view that a restored peg would be conducive to more general economic stability (House Republican Research Committee, 1984, p. 36).

Friedman was also confident that his side would prevail in U.S. public debate: "there's not a snowball's chance in hell that you are going to get any kind of commodity standard, whether it be good or bad," he remarked in October 1983.[157] Nineteen months later, however, he granted that "a slim possibility exists" that gold might be assigned a meaningful role in a future monetary

[154] *The MacNeil/Lehrer News Hour,* PBS, November 12, 1985, p. 4 of transcript.
[155] Friedman (1985e, p. 12).
[156] "Stabilisation of Commodity Prices," BBC radio, September 29, 1954, p. 2 of transcript. During this same period, Friedman (1954a) was a written critique of a proposal involving commodity price stabilization (one offered to be part of agricultural policy).
[157] In Hulbert and Meltzer (1983, p. 76).

system."[158]

Sectoral output, industrial policy, and competitiveness

The latter remark was mainly made to recognize a particular scenario Friedman highlighted: the possibility that—some decades in the future—the United States could experience a very drastic inflation that produced a sharp reaction from the general public of a kind that might "ultimately force a return to a commodity standard."[159] But more immediately, the comment might have been something of an acknowledgment on Friedman's part of the backlash against floating exchange rates that Jack Kemp and other supply-siders had promoted. Although supply-siders had deplored the dollar depreciation that occurred in the late 1970s, the climbing U.S. exchange rate through 1985 also generated dissatisfaction among many of them, including Kemp. On this matter, Kemp had common ground with Democratic Senator Bill Bradley of New Jersey. Their unhappiness with the high dollar prompted them to co-organize a conference, held in Washington, D.C., in November 1985, whose theme was the exploration of alternatives to the floating-rate system (*San Francisco Chronicle*, November 13, 1985).

Bradley made clear that he was not attracted to a gold standard: "Well, if we are talking about the gold standard, I'd say it was unrealistic and impractical."[160] But he and Kemp shared a desire for abolishing floating and, as Bradley put it, "moving toward much more coordinated intervention in the exchange-rate markets."[161] Bradley summarized "the message from the conference that Jack and co-sponsored" as: "We want a change [to] the present system, because the floating exchange-rate system is not working."[162] In outlining his basis for this prescription, Bradley pointed to "a trade deficit of close to $150 billion."[163] He also emphasized job losses in certain U.S. sectors. In this connection, when confronted with Friedman's downplaying of a high-dollar/U.S.-employment link, Bradley replied: "I wouldn't want to ask someone in the manufacturing sector that question—I don't think they would agree with Dr. Friedman."[164]

In seeking to take steps to boost U.S. manufacturing output, Bradley was interested not only in exchange-rate management but also in interventionist domestic policy at the intersection of

[158] Friedman (1985e, p. 12).
[159] Friedman (1985e, p. 18).
[160] *The MacNeil/Lehrer News Hour*, PBS, November 12, 1985, p. 5 of transcript.
[161] *The MacNeil/Lehrer News Hour*, PBS, November 12, 1985, p. 4 of transcript.
[162] *The MacNeil/Lehrer News Hour*, PBS, November 12, 1985, p. 6 of transcript.
[163] *The MacNeil/Lehrer News Hou*r, PBS, November 12, 1985, p. 4 of transcript.
[164] *The MacNeil/Lehrer News Hour*, PBS, November 12, 1985, p. 7 of transcript.

macroeconomic and microeconomic measures. On this score, he differed from Kemp, who largely stuck to free-market prescriptions on the domestic front. Bradley was, in contrast, an exponent of "industrial policy." Bradley had told a Senate hearing in December 1980: "Industrial policy means many different things to different people, but at root it suggests deliberate government actions to influence the structure of industry."[165] As he saw it, such industrial policy could be part of "vigorous American efforts to improve the productivity and general competitiveness of the U.S. economy."[166]

Consequently, when the current account situation alongside declines in sectors of the economy gave impetus to interventionist policy proposals in the United States during the early 1980s under the label of "industrial policy," Friedman had multiple objections: the current account deficit did not reflect lack of competitiveness of the country as a whole; productivity improvement would not reduce the deficit; and the interventionist measures being offered would not improve productivity.

As far as specific sectors and American competitiveness were concerned, Friedman acknowledged in the *San Francisco Chronicle* (April 18, 1985) that the high exchange rate of recent years had indeed made U.S. exporting and import-competing firms less competitive.[167] He maintained nevertheless that it was mistaken to suppose that, in this situation, measures that raised productivity of the firms in question would lower the U.S. current account deficit. In Friedman's view, such a proposition embedded a confusion between the individual firm (which could take the value of the exchange rate parametrically) and the analysis of the economy as a whole (for which the exchange rate was an endogenous variable) (*San Francisco Chronicle*, January 23, 1979; *Wall Street Journal*, November 27, 1984*b*). Improvements in exporting firms' productivity would lead to upward pressure on the exchange rate. "The concept of a 'competitive edge' for the country as a whole is meaningless," Friedman remarked of the situation prevailing under floating rates (*Wall Street Journal*, November 27, 1984*b*).

Friedman's insistence that the current account deficit could not be reliably reduced by measures to improve firms' productivity also reflected his perspective on current account determination, discussed in Section II above. He maintained that introducing restrictions on U.S. capital inflow

[165] From Senator Bill Bradley's written submission for the hearing of December 9, 1980, in Committee on Finance, U.S. Senate (1981a, p. 104).
[166] From Senator Bill Bradley's remarks during the hearing of December 5, 1980, in Committee on Finance, U.S. Senate (1981a, p. 2).
[167] See also Friedman and Schwartz (1963a, p. 420) for a statement that a high exchange rate in an earlier era had the effect of rendering U.S. exporters uncompetitive.

would be a much more effective way of obtaining current account balance—although, being both a longtime advocate of free capital flows and undisturbed by current account deficits, he hastened to add that such a move would be undesirable (*National Review*, April 6, 1984, p. 44).

Finally, although Friedman did not see the trade deficit as a good motivation for policy measures that raised productivity, he did favor raising productivity as desirable. But the way to do so, as he viewed it, was via the adoption of free-market policies—and not through what Bradley and others were calling industrial policy. "The industrial policy we need is to let the market work," Friedman proclaimed (*U.S. News and World Report*, January 31, 1983, p. 67).[168]

The high dollar and U.S. employment

Jack Kemp was not enamored with the option of interventionist industrial policy. But, like Bradley, Kemp cited shifts in the sectoral composition of U.S. aggregate output during the 1980s as evidence of the failure of floating exchange rates. He saw "the workers of Buffalo [his constituency] and throughout the country," including "the agricultural sector of the Midwest" as being hurt by current exchange-rate arrangements, including the still-high U.S. dollar.[169] Relatedly, Kemp viewed the widening of the U.S. trade deficit as one metric that attested to the current problem. He maintained that "part of our trade deficit is a problem" on the grounds that it showed that "farmers, manufacturers and mineral producers… have been priced out of world markets by the dollar's rise." The conclusion he drew was that the dollar appreciation was a prime example of how "instability of exchange rates has cost many jobs in… the United States" (*Washington Post*, August 7, 1985).

Some whose perspective on the current account balance more closely overlapped with Friedman's nevertheless, like Kemp, saw the U.S. dollar appreciation as a cause for concern. Ronald McKinnon, for example, shared Friedman's view that the current account deficit should not be regarded as reflecting lack of competitiveness.[170] McKinnon nevertheless saw a case for intervention to hold the U.S. dollar down in order to protect the manufacturing industries of the United States. "I'm a great admirer of Paul Volcker, O.K.? But when he squeezed, as he had to, and he really came up with tight money policy in '79, '80, the dollar went through the ceiling in the foreign exchange market. And I think we should have had an agreement then to prevent it from going so high, because this created the rust belt in the U.S. Midwest. We should have had

[168] See also Friedman and Friedman (1984, pp. 117–123; 1985, pp. 114–120).
[169] *The MacNeil/Lehrer News Hour*, PBS, November 12, 1985, p. 4 of transcript.
[170] See McKinnon's remarks in the floor discussion in Boskin (1980, p. 46).

it varying within a much narrower range." (Ronald McKinnon, interview, January 23, 2014.)

Friedman partly challenged the premise of arguments along these lines by citing factors other than the high exchange rate that were depressing traditional U.S. manufacturing areas, including rigidity in those areas' real wages.[171] Although he rejected the notion that U.S. production as a whole was threatened by international competition (see *San Francisco Chronicle*, January 23, 1979, and the discussion above), Friedman did grant that the dollar appreciation had put downward pressure on employment in key manufacturing industries (*San Francisco Chronicle*, April 18, 1985). But it did not follow, in his view, that this fact justified exchange-rate intervention or other measures directed specifically at helping the affected industries. "Why should we do something for the textile industry? I'm not running for office!," he remarked bluntly in a television appearance in late 1985.[172]

In Friedman's view, there was a "mistaken perception of job loss" associated with the high dollar (*San Francisco Chronicle*, April 18, 1985, p. 20). In line with his analysis of the matter of whether higher imports cost the country jobs, he maintained that, in the aggregate, the high exchange rate had had no appreciable impact on U.S. employment. It instead tended to reduce employment in some sectors and to raise employment in others. "The plain fact is that the strong dollar does not cost any jobs—that's nonsense. It changes jobs. Some jobs become less available, and other jobs become more available."[173]

This interpretation of the high dollar was in keeping with the fact that under fixed exchange rates, a high dollar carried with it a tendency for domestic monetary contraction, while, under flexible exchange rates, no such implication of a high dollar applied. Rather, the combination of a sticky-price economy and floating rates gave policymakers' scope for policymakers to insulate aggregate real aggregate demand from a contraction occurring in the tradable-goods sector.

Confronting Kemp

Friedman's confidence in the virtues of flexible exchange rates was therefore resilient in the face of the long experience of a high U.S. dollar. As a hostile *Wall Street Journal* editorial of

[171] *Business Week*, November 14, 1983, p. 163 (p. 135 of international edition); *The MacNeil/Lehrer News Hour*, PBS, November 12, 1985, p. 7 of transcript. See also Friedman and Friedman (1984, pp. 111–112, 118, 130; 1985, pp. 110, 115, 126).

[172] *Wall Street Week*, Maryland Public Television, November 14, 1985, p. 8 of transcript.

[173] *The MacNeil/Lehrer News Hour*, PBS, November 12, 1985, p. 6 of transcript. See also *Wall Street Week*, Maryland Public Television, November 14, 1985, p. 8 of transcript.

December 4, 1985, noted that Friedman "continues to offer a theoretical defense of the float, notably in a TV debate with Rep. Jack Kemp."

The occasion for that debate had been the November 12, 1985, edition of public television's *MacNeil/Lehrer News Hour*.[174] In this debate, Kemp, while noting that he was "a big fan of Milton Friedman," criticized Friedman's focus on monetary aggregates and confronted him with a Friedman statement that the gold standard had delivered long-run stability of prices over 150 years.[175] Friedman replied by making a point he had made on several other occasions: the gold standard had indeed secured very long-run price level stability but had been associated with sharp short-run fluctuations in output and prices and considerable multi-decade instability in the price level.[176] Friedman further affirmed, against the arguments of his "good friend" Kemp, more-stable exchange rates would be desirable if they emerged from a floating-rate system but undesirable if they arose because they were an objective of economic policy.[177]

Friedman seized on the disagreement between Bradley and Kemp: "Everybody thinks some kind of monetary reform is essential, but everybody thinks a different kind is essential. And the fact of the matter is that there is no agreement, among either these two gentlemen or the people at their conference, as to what form an international rearrangement should take."[178]

Kemp criticized monetary targeting, his basis for doing so being that "you don't know what the demand for money is."[179] This was a standard and lasting criticism of monetary targeting, but an advocate of a commodity standard was not best positioned to make it. For the macroeconomic stability said to flow from that system ultimately depended on stable and well-grounded demand functions for commodities (even though the system's advocates rarely made this point explicit). As Anna Schwartz (1982b, p. 551) had remarked in reference particularly to supply-siders who advanced the gold standard: "The evidence of disturbances in demand and supply in the gold

[174] Apart from moderators Charlayne Hunter-Gault and Robin MacNeil, Senator Bill Bradley was Friedman's other interlocutor in the debate. Bradley, Kemp, and Friedman were all in separate locations during the debate—in Friedman's case, in a New York City PBS studio with MacNeil (Friedman being in the city to give an economic-outlook talk to clients of Oppenheimer and Company).

[175] *The MacNeil/Lehrer News Hour*, PBS, November 12, 1985, p. 7 of transcript. Kemp gave the source for the Friedman quotation as a "recently" published article in the *Journal of Political Economy*. But the item in question was not in Friedman (1984h), his most recent *JPE* article. The actual material quoted did, however, appear in the June 1986 issue of the *JPE* (see Friedman, 1986e, p. 643), as well as in Friedman (1988d, p. xxi).

[176] See, for example, Friedman (1984f, p. 45). Friedman would reiterate the point in *New York Times*, December 26, 1985, after the debate with Kemp, and in Friedman (1986d, p. 643).

[177] *The MacNeil/Lehrer News Hour*, PBS, November 12, 1985, pp. 6, 7; quotation from page 7.

[178] *The MacNeil/Lehrer News Hour*, PBS, November 12, 1985, p. 6 of transcript.

[179] *The MacNeil/Lehrer News Hour*, PBS, November 12, 1985, p. 7 of transcript.

market unrelated to monetary forces is ignored by the proponents." And, in any event, a belief in the merits of floating rates in favor of the gold standard did not rest on a belief in monetary targeting—though Friedman and Schwartz happened to expound both beliefs—but instead, ultimately on the monetary-autonomy argument Friedman had emphasized in 1953.

Friedman therefore remained resolute in advocating floating exchange rates. Compared with his discussions of exchange rates in prior decades, however, a change in the 1980s is detectable. The experience with floating rates had led Friedman to concentrate on the most robust aspects of his 1953 arguments—in particular, the aforementioned monetary-autonomy piece of it. During the Bretton Woods period, he had suggested that only under fixed exchange rates would an economy likely be prone to having large international short-term capital movements.[180] The facts of experience had shown that contention to be invalid. Furthermore, as far as the current-account side of the balance of payments was concerned, Friedman was now willing to give more weight than he once had to the possibility that, over a certain range of values for the dollar exchange rate, a depreciation might lead to a wider current account deficit, rather than to a narrower one (*Wall Street Journal*, November 27, 1984*b*).[181]

And with regard to what drove major economies' exchange rates, it was clear by the mid-1980s—especially on the basis of the work of Meese and Rogoff (1983)—that economists' knowledge was extremely limited, at least with respect to data of a monthly or quarterly frequency. Friedman himself acknowledged surprise at the behavior of exchange rates. "As someone who has always strongly favored floating exchange rates, I must admit I did not anticipate the volatility in the foreign exchange markets that we've had," he remarked in a talk given in New York City in late 1985 (*New York Times*, December 26, 1985).

But notwithstanding all these concessions, the paramount rationale for floating exchange rates—that they were, as Friedman put it in 1967, the "one way in which we can enable monetary policy to be free to concentrate on domestic policies"—still carried through.[182] That is, the monetary-autonomy argument survived, and Friedman, writing in 1985, was able to state in his *Palgrave* entry that experience had confirmed the trilemma.[183]

[180] See Friedman and Roosa (1967, p. 105) and Instructional Dynamics Economics Cassette Tape 74 (May 20, 1971).

[181] This was possibly not a major concession, as Friedman may have been referring to simply short-run "*J*-curve" dynamics in which it takes time for net exports (measured in real-volume terms, rather than in terms of real receipts) to adjust to movements in the real exchange rate.

[182] Friedman (1967a, p. 102).

[183] Friedman (1985d, p. 67; 1987d, p. 18). Unlike Kemp in the 1985 debate, another supply-sider, Marc Miles, did confront directly the monetary-autonomy argument, which he called the "Monetarist Dream." Miles (1984, p. 141)

The robustness of this argument served Friedman well when, in early 1986, he allowed for the possibility that the floating exchange rate might be behaving in a manner that confounded economic explanations. Even under those circumstances, he noted, it remained the case that floating rates served a worthwhile function because they prevented whatever was driving exchange rates from affecting the money stock, whose determination was instead in the hands of the central bank.[184] This line of reasoning followed up an argument he had made in 1969 that fixed exchange rates did not remove uncertainty but instead changed the form in which uncertainty would be manifested—allowing that uncertainty to generate greater fluctuations in domestic economic series.[185]

The monetary-autonomy property imbued by floating exchange rates likely underlay Friedman's statement (*Wall Street Journal*, December 18, 1985): "I predict that both inconvertible paper moneys and the present system of dirty floating exchange rates are here to stay for the foreseeable future."[186]

The Plaza agreement and agitation for fixed rates

Even during the year in which Friedman wrote those words, however, some moves were afoot in U.S. policy circles that raised the prospect of a major shift in exchange rate policy. Although, as indicated, there was some interest in Congress, there was not widespread support among leading policymakers for a return to a Bretton Woods system. But momentum nevertheless existed in favor of the restoration of greater management of exchange rates, to be obtained by coordination of the monetary policies of the major economies. James Tobin, for example, had advocated such an arrangement virtually from the time of the demise of the Bretton Woods system. Although he was usually classified as an opponent of fixed exchange rates, Tobin had stated in 1974: "International coordination of interest-rate policies will be essential in a regime of floating rates, no less than in a fixed-parity regime."[187]

argued that "this Monetarist Dream is but an illusion" because international investors held portfolios in multiple currencies. Such financial integration, however, does not actually imply the absence of monetary policy autonomy under flexible exchange rates (see Woodford, 2009). In particular, Friedman's argument does carry through under conditions of international financial integration: see Nelson (2020c).

[184] See Hinshaw (1988, pp. 163, 165). Blanchard (1997, p. 288) made a similar argument. See also DiCecio and Nelson (2010, p. 423) for a related discussion.

[185] See Friedman (1969b, p. 111).

[186] Friedman made similar predictions about the floating-rate system in *Newsweek*, May 30, 1983, and *New York Times*, December 26, 1985.

[187] Tobin (1974, pp. 91–92). Tobin had reaffirmed this position in 1980 (Tobin and de Macedo, 1980, p. 25). As discussed in Chapter 9, he was also in favor of an activist foreign-exchange control policy as a means of helping to stabilize exchange rates.

Friedman, in contrast, felt that cross-country connections between interest rates should emerge from the operation of international capital mobility and not from concerted coordination of monetary policy across countries. As discussed in Chapter 9 above, he had criticized the Carter Administration for its attempts in the late 1970s to encourage coordination of the economic policies of the United States, Japan, and the Federal Republic of Germany, as he saw this as tantamount to pressuring the latter countries to follow policies that would promote inflation in their economies. In contrast, displayed a hands-off attitude to the foreign exchange market (and disinterest in policy coordination) characterized the first term of the Reagan Administration. Writing from the perspective of an anti-floating supply-sider, Miles (1984, p. 141) criticized inaction with regard to the foreign exchange market with during the administration's first eighteen months, with "[n]either the assassination of [Egypt's president, Anwar] Sadat, the leftist [François Mitterand] victory in France, nor the Israeli invasion of Lebanon" prompting it to change its policy. Miles blamed Friedman's former student, then Under Secretary of the Treasury for monetary affairs Beryl Sprinkel, for insisting on the clean U.S. dollar float.

The Reagan Administration in its second term became more enamored with the idea of international policy coordination and exchange-market intervention than had been the case in the president's first four years in office. The ascension of James Baker to the position of Secretary of the Treasury at the start of the second Reagan term in 1985 helped to create the change in perspective. At a meeting in New York's Plaza Hotel on September 22, 1985, Baker was one of the finance ministers of G5 countries issuing a statement that the dollar's decline, which had started earlier in the year, was desirable and should proceed (see, for example, Frankel, 1994, p. 302).

Unlike the G5 meeting at the Plaza, the Bradley-Kemp conference discussed above, which took place a couple of months later, was not a policymaker forum but simply a public-policy conference at which policymakers were among the speakers. Secretary Baker was on the program and said in his conference speech on November 12: "I think it goes without saying that the current system has not been as stable as we would have liked." Other statements Baker made in the same speech were, however, more in line with the administration's previous orthodoxy of support for floating rates (*San Francisco Chronicle*, November 13, 1985).

Commenting on television that evening, Friedman insisted: "The fact of the matter is that the floating exchange rate system has worked extraordinarily well… It has existed now since 1971. It has existed during a period in which world trade expanded very rapidly. It has existed during a period where you had unusual disturbances in the form of oil and food crises, and it has enabled the countries of the world to weather them."[188]

Even during Baker's tenure at the U.S. Treasury, it was still the case that strong advocates of floating exchange rates had prominent posts within the Reagan Administration. These included George Shultz at the State Department. But Shultz felt constrained in his ability to contribute on the matter. "Shultz didn't get very involved in economic policy when he was Secretary of State, because he didn't want Jim Baker to get involved in [foreign affairs]," Allan Meltzer observed (interview, April 21, 2013). "And his own inclination is not to go where you aren't invited… He had his hands full with foreign policy." Confirming this, Shultz recalled that "I had to be very careful, having had this job—because people get very sensitive. So anything I had to say [to James Baker on economic policy], I would say privately, so that it wouldn't get out." (George Shultz, interview, May 22, 2013.)

Another major pro-float member of the administration did have an economic post. Beryl Sprinkel had become head of the Council of Economic Advisers in 1985 after serving in his Treasury undersecretary position from 1981 to 1985 (Europa Publications Limited, 1986, p. 1524). Concerned not only by the developments in the U.S. Treasury's thinking on exchange rates but also by the Bradley-Kemp push for a return to fixed rates—and feeling unable to take an active public stance on the matter himself—Sprinkel contacted the head of the Chicago Mercantile Exchange, Leo Melamed, who had been one of the early advocates of floating rates in the American financial community.

Melamed recalled that "my very good friend Beryl Sprinkel… [had been] one of my board members at the IMM [the International Monetary Market, a division of Melamed's exchange] from the inception—one of the founding board members—and so we were very close. And he called me to tell me of this movement that I was vaguely aware of, and said, 'You know, Leo, this is the kind of thing that has a slippery slope'—he used those words—'to it, and we've got to stop it. And it's up to you to stop it. I can't do that from where I'm sitting in the White House.'"[189] Melamed recalled how their conversation proceeded, beginning with his own reaction: "'What do you want me to do? How do you suggest I stop it?' He said, 'Well, it's ridiculous to let it go without some opposition. Think of some things you could do. You're a very respected individual'—I think he thought I was more respected than I thought I was—'and

[188] *The MacNeil/Lehrer News Hour*, PBS, November 12, 1985, p. 5 of transcript. See also his similar observation in *New York Times*, December 26, 1985.
[189] Although Sprinkel was an adviser to the president, his office was actually in the Old Executive Office Building, rather than the White House itself. See Europa Publications Limited (1986, p. 1524).

you could round up a lot of people and have them make statements [favoring floating rates]…'" (Leo Melamed, interview, June 19, 2013.)

In reaction to Sprinkel's suggestion, Melamed recalled, "The first thing I did was call Milton Friedman, of course... And so I said to him what Beryl had said to me. And he said, "You know, I really don't think so. I mean, I don't think it's a danger. But I've got to tell you the truth: Beryl is much more in tune [than me] with things in Washington, D.C. And if he thinks it's a slippery slope, [then,] you know, I would listen to him.' So I said, 'O.K., but what can I do? I'm [just] chairman of a little exchange, you know?' And he said, 'Well'—and he [then] pretty much said the same thing Beryl did: that is, assemble a group favoring floating exchange rates." (Leo Melamed, interview, June 19, 2013.)

Melamed formed the American Coalition for Flexible Exchange Rates in 1986 (Frankel, 1994, p. 326). With regard to this pressure group, Melamed mentioned that "I talked Milton" into being a member. "I knew that I needed the Milton Friedman name to get anything going… [as] the Milton Friedman name was still the heavyweight." (Leo Melamed, interview, June 19, 2013.) In the event, the new group had a low public profile. But Melamed himself did make the notable move of going behind enemy lines to publish an op-ed article championing floating exchange rates in the fervently pro-fixed-rates *Wall Street Journal* (April 24, 1986). In his op-ed, Melamed stated that "I do not for a moment question that Messrs. Bradley and Kemp have the best intentions"—but he labeled their efforts to revive a Bretton Woods system "misguided." Melamed's op-ed did not mention Friedman. But in a longer version of the piece that appeared in the Chicago Mercantile Mercantile Exchange's annual report, he noted that "Milton Friedman advocated flexible exchange rates as far back as 1950"—a reference to the original draft of Friedman's 1953 article (Melamed, 1986, p. 9).

Federal Reserve action and the dollar

In some accounts of events in the 1980s, it has been suggested that the focus on exchange rates and policy coordination exhibited by the Reagan Administration in its second term had a counterpart also in monetary policy decisions. Bernanke and Mishkin (1992, p. 194), for example, argued: "Beginning in early 1985, the Fed attempted to bring down the dollar by driving up M1 and M2 growth rates…."[190] If this interpretation is accepted, then international

[190] Likewise, Allan Meltzer testified (at a hearing held on February 18, 1987—see Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1987, p 33) that, as he saw it, the Federal Open Market Committee "has worked toward that goal [of U.S. dollar depreciation]… by printing money at an extremely rapid rate."

monetary arrangements constitute yet another area in which Friedman suffered a major setback during the 1982–1986 period.

But it does not, in fact, seem appropriate to interpret monetary policy in the mid-1980s as driven by exchange-rate policy. At the very start of the 1980s, Friedman had contended (see Feldstein, 1980a, p. 95) that the Federal Reserve tended under floating exchange rates to undertake actions motivated by domestic reasons even when it was citing international factors as the reason. That seems to be true also of mid-1980s Federal Reserve policy. Friedman himself regarded the outcome of the 1985 Plaza meeting as simply an official acquiescence in the U.S. dollar exchange-rate depreciation that was already in motion—and not as triggering substantive policy changes.[191]

Although he had a more negative view of the later (1987) Louvre agreement—which he saw as authorizing concerted action to manage exchange rates (see Chapter 17 below)—Friedman did not see either the Plaza or Louvre agreements as instigating major changes in U.S. monetary policy, which he would later characterize as having been concerned instead with domestic economic stabilization over this whole period (*Wall Street Journal*, August 19, 2003). Consistent with this interpretation of U.S. monetary policy, Taylor (1999a) and Clarida, Galí, and Gertler (2000) would find that U.S. monetary policy in both the 1980s and 1990s was largely driven by responses to the output gap and inflation, rather than involving direct reactions to the exchange rate or other international variables.

[191] See Friedman’s remarks in Ragan (1999, p. 52) as well as in *Wall Street Journal*, September 22, 1992, and in his letter to George Shultz of December 2, 1994, p. 1 (Box 179, Milton Friedman Papers, Hoover Institution).

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 16: Making Every Moment Count: Domestic Policy Debates, 1987 to 1992

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I. EVENTS AND ACTIVITIES IN DOMESTIC POLICY DEBATES, 1987–1992

Speaking at the ninety-ninth annual meeting of the American Economic Association, Bennett McCallum gave his assessment of the state of macroeconomics: "today (i.e., December 29, 1986) matters are rather unsettled."[2] McCallum noted that the 1980s had witnessed a "splintering of opinion" among researchers: for, although there had been widespread disillusionment with the monetary-surprise models associated with the early rational expectations macroeconomic literature of the 1970s, this disillusionment had led large segments of the economics profession to move in opposite directions. On the one hand, some researchers had embraced sticky-price models that were based on a greater degree of microeconomic foundations than had been the case in the pre-rational expectations era; but other researchers, McCallum observed, had embraced what had become known as the "real business cycle" (RBC) approach, according to which "output fluctuations are induced almost entirely by technology shocks, with money-output correlations occurring only because the monetary system responds to these fluctuations."[3] The momentum possessed by the RBC approach was confirmed in 1987, when one of McCallum's discussants, Robert Lucas, published a monograph on business cycles that emphasized the RBC literature's interpretation of cyclical fluctuations (Lucas, 1987), and again in 1988, when the *Journal of Monetary Economics* published a double-sized issue on real business cycles.[4] In the face of the RBC movement, Blanchard and Fischer (1989b, p. 3) judged that the professional consensus that monetary policy could affect output, consecrated by Friedman and Schwartz's work, had "dissolved" during the 1980s.

[1] Email: Edward.Nelson@frb.gov. The author is grateful to the interview subjects and Marcel Priebsch for their generosity in providing useful information for this chapter. 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, in the period since the research underlying this chapter began, five individuals—Robert Chitester, Lyle Gramley, David Lindsey, Henry Manne, and Edward C. Prescott—whose interviews with the author are quoted below have passed away.
[2] McCallum (1987b, p. 127).
[3] McCallum (1987b, pp. 127–128). The "real business cycle" terminology was due to Long and Plosser (1983).
[4] See King and Plosser (1988).

The paper that launched the real business cycle literature, "Time To Build and Aggregate Fluctuations," by Finn Kydland and Edward C. Prescott, appeared at the front of the November 1982 issue of *Econometrica*. The immediately preceding issue of *Econometrica* (September 1982) had contained at its back an advertisement for Friedman and Schwartz's newly released book, *Monetary Trends in the United States and the United Kingdom*.[5] The fact that the Friedman-Schwartz and Kydland-Prescott pieces of work were highlighted in consecutive issues of *Econometrica* proved to be a poignant sign of the path taken by many economic researchers during the 1980s as the RBC movement gathered steam. Its title notwithstanding, *Monetary Trends* had put great emphasis on the role of money in U.S. business cycles. In contrast, Kydland and Prescott suggested that an account of U.S. cyclical behavior was obtainable without *any* reference to monetary variations or monetary policy. And while *Trends* failed to repeat the achievement of *Monetary History* in galvanizing the economics profession, the Kydland-Prescott paper became a phenomenon, spearheading a large literature that, as indicated above, permeated even research journals that were ostensibly confined in subject matter to monetary issues.

From his occasional appearances at research conferences in the Bay Area, Friedman had been exposed during the early and mid-1980s to the emerging RBC work. Over that period and subsequently, both Kydland and Prescott presented some of their research at conferences held on or near Stanford University's main campus.[6] One occasion on which Friedman was in the audience saw him make a floor contribution, in which he urged Prescott to heed the empirical evidence on the role of money in business cycles (Charles Nelson, interview, September 9, 2013).

Initially, Kydland and Prescott seemed well disposed toward suggestions of this kind. In a 1988 exposition, for example, they stated that, alongside the technology shocks that they emphasized, "[t]here are surely other factors contributing to fluctuations such as terms-of-trade shocks, public finance shocks, preference shocks and monetary shocks."[7] Nor did Kydland and Prescott seem wholly committed to the postulate of perfect wage and price flexibility that was used in their 1982 analysis. Indeed, in June 1983, Prescott drafted a working paper that added nominal wage contracts to the real-business-cycle baseline (Prescott, 1983).[8] But Prescott never published the

[5] See University of Chicago Press (1982).

[6] For example, the NBER Conference on Macroeconomics at the Hoover Institution in July 1983 featured a presentation by Kydland (as noted in Kydland and Prescott, 1988, p. 360).

[7] Kydland and Prescott (1988, p. 357).

[8] Prescott presented this paper at Stanford University at the summer workshop of the Institute for Mathematical Studies in the Social Sciences (IMSSS), on July 28, 1983 (Prescott, 1983; Kydland and Prescott, 1988, p. 360). The IMSSS summer workshop was an event for which Kenneth Arrow and Frank Hahn were prominent organizers and participants during the 1980s and was not one that Friedman attended. However, Prescott recalled that Friedman

paper, and after the 1980s his attitude toward monetary accounts of the business cycle hardened. This hardening was brought out in Prescott's declaration in 2014: "It is an established scientific fact that monetary policy has had virtually no effect on output and employment in the U.S. since the formation of the Fed." (*New York Times*, January 28, 2014.)[9]

This negative attitude toward the importance of monetary policy in the business cycle reflected a growing disillusionment with monetarism on Prescott's part. His dissertation work had been on Friedman's monetary rule, but the 1980s would see him change his view so much that Prescott fell into the category of economists covered by Lucas' (1994, p. 13) observation: "The idea that 'money doesn't matter'… is now embraced even by many former monetarists." "In the '70s we all thought money had to be the big thing. That was the Friedman-Lucas contribution," Prescott recalled. "…When Finn and I were doing 'Time To Build,' we wanted to get [ultimately] to propagation of a monetary shock…" As already indicated, the Kydland-Prescott research was originally envisioned by the authors as a first step toward an explanation of the cycle in terms of both nonmonetary and monetary factors, with the latter to be allowed for by the introduction into the Kydland-Prescott framework of the demand for money and a mechanism by which monetary policy has short-run effects on output. But Prescott would become persuaded that these monetary features were not, in fact, needed. "Over time, it turned out that the simple theory that abstracts from these features of reality accounts well for the behavior of the economy. I certainly expected that in periods of financial and monetary turmoil, with the theory that abstracts from monetary and financial factors there would be big deviations [from the data]. But the surprise was that there weren't." In a related vein, Prescott observed: "I think Friedman underestimated the flexibility of prices." (Edward C. Prescott, interview, February 16, 2016.)

After the RBC literature had become well established, Kydland and Prescott actually had an exchange in print with Friedman.[10] But the exchange, which appeared in the *Journal of*

was in attendance for at least a couple of the former's presentations in the Bay Area over the years (Edward Prescott, interview, February 16, 2016). Among these was the presentation of Prescott (1993).

[9] Unlike Prescott, Kydland later made the study of money/output relationships a major part of his research agenda. However, most of this work (for example, Freeman and Kydland, 2000) sought to explain money/output correlations as an artefact of the monetary policy rule in force and as not reflecting a genuine influence of monetary policy actions on the behavior of real economic activity. This was a line taken by other RBC research output, including the King and Plosser (1984) study discussed presently. (It differed from the suggestions of Hall, 1982, and Tobin, 1970, that the correlation of money and other variables might not reflect a structural relationship, because Hall and Tobin had applied the argument to the correlation of money with *both* real income and nominal income.) During the second half of the 1990s, Friedman attended a presentation of one of Kydland's papers in this area (see Gavin and Kydland, 1999, p. 347).

[10] Several years earlier, Kydland and Prescott had published an article whose title, "Business Cycles: Real Facts and a Monetary Myth," might have suggested that they were taking on Friedman directly. But the article in question (Kydland and Prescott, 1990) contained only a passing mention of Friedman and Schwartz (1963a), and the

Economic Perspectives in 1997, proved to be a desultory one. Friedman's contribution to it consisted of a weak contribution in which he erroneously suggested that Kydland and Prescott had not taken Slutsky's (1937) work on business cycles into account in their research.[11]

A major message, albeit poorly communicated, in Friedman's 1997 critique was that the criteria on which Kydland and Prescott were reaching their favorable conclusions about the performance of RBC models were questionable. His 1997 discussion focused on the fact that the ability to generate dynamic patterns that resembled business cycles was something that RBC models shared with many other candidate models, and so that achievement in itself did not constitute strong support for the RBC approach.

Calibration and filtering

On other occasions, two other aspects of the RBC literature's approach to empirical work provoked criticism from Friedman. One was calibration. The RBC literature's emphasis on "calibrating" (that is, assigning values to) rather than estimating (by econometric methods) key model parameters was a practice to which Friedman had been exposed at his money workshop during the mid-1970s, when he had all but sabotaged a presentation, held on October 15, 1974, by Robert Lucas of the paper later published as Lucas (1975). "I gave that paper here in a workshop," Lucas recalled. "And Milton was just completely mystified. It was kind of a crazy session. In the paper that got published, there were no numbers, but [in the workshop version] I had a sort of calibrated model. I couldn't solve the model [analytically] because it was just too hard. So I calculated solutions and put in some numbers. And the numbers were just kind of made up, because you can't do a numerical calculation without numbers… So I put those numbers in there, and Milton just said: 'Well, where did that number come from?' And I said something like: 'Well, I just made it up.' And Milton just thought, 'My God, I'm in the company of a crazy person.' So I took them out of the paper." (Robert Lucas, interview, March 12, 2013.)[12] The RBC literature's practice of calibration differed from that which Lucas followed, as the literature's typical procedure was to provide sources for parameter choices. Frequently, however, these choices were based on microeconomic studies rather than

principal "monetary myth" that the authors challenged was the alleged procyclicality of the price level. Friedman had little stake in the issue of whether the price level was procyclical (or was instead, as Kydland and Prescott contended, procyclical).

[11] See Friedman (1997a) and Kydland and Prescott (1997). Kydland and Prescott had, in fact, explicitly discussed Slutsky's (1937) work on cycles in their already-mentioned 1990 piece.

[12] Friedman's objection apparently was to the lack of a concrete source for Lucas' numerical choices. Friedman did not object in principle to the idea of using prior empirical studies as the basis for the value of parameters used in a numerical application. He had done so in Friedman (1969a, p. 42; 1971a, p. 851), for example.

macroeconomic estimates. As in Lucas' paper, therefore, calibration in the RBC literature involved using parameter values that were not obtained from econometric estimation of the macroeconomic model.

A second key aspect of the RBC literature's approach to empirical analysis was its use of the Hodrick-Prescott (HP) (1980) filter prior to obtaining second moments of series, with the second moments of the filtered series used to depict the data's variance/covariance configuration at the business cycle frequency. The origination of the HP filter was distinct from the development of the RBC approach. The research that led to the Hodrick and Prescott (1980) paper began when Prescott asked Robert Hodrick, his colleague in Carnegie Mellon University's business school, what were the cyclical properties of velocity—which he expected Hodrick to know because Hodrick had received his Ph.D. was from the University of Chicago.[13] Hodrick correctly recalled that velocity was procyclical, but what ensued from the conversation was the authors' development of their filter, which decomposed time series into a cyclical and trend component.

For a long time, the Hodrick-Prescott paper was one of the most famous unpublished papers in economics. That it went unpublished for so long was due, in part, to Friedman's actions.

In the early 1980s, after the authors failed to secure acceptance of their manuscript by the *American Economic Review*, William Dewald invited Hodrick to submit the paper to the *Journal of Money, Credit and Banking*, which Dewald edited. However, Dewald assigned Friedman to be a referee, and Friedman—who had developed strong but idiosyncratic views on filtering over the years, largely from his NBER monetary project—turned in a negative report on the Hodrick-Prescott submission. The paper was consequently rejected by the *Journal of Money, Credit and Banking* and languished unpublished, although heavily cited, for fifteen years. In the mid-1990s, however, the *Journal of Money, Credit and Banking* invited the authors to submit the paper again, and it was subsequently published, essentially in its original form, as Hodrick and Prescott (1997). (Robert Hodrick, interview, January 23, 2016.)

The role of money and the RBC literature

Although they triggered strong reactions from Friedman, the debates over calibration and filtering were both tangential to the central issue on which the RBC literature represented a

[13] Prescott's interest in velocity behavior was consistent with the fact that, as of the early 1980s, Prescott still expected that his research on business cycles would continue to lead him to emphasize the interaction of money and output.

challenge to Friedman's outlook on economics. This central issue was, of course, the issue of the role of money in the business cycle. The basic position of the RBC camp on money was that money/output correlations could be reconciled with conditions of instantaneous monetary neutrality and no nominal rigidities. As already noted, most of Kydland and Prescott's early contributions to the RBC literature were not on the role of money *per se* but on how far one could go in explaining the cycle without appeal to monetary factors. The matter of money's role was confronted more directly in another component of the early RBC literature. In R.G. King and Plosser (1984) and other contributions, Robert King and Charles Plosser added money to a flexible-price RBC model and considered some of the grounds for believing that the nominal money stock in such an environment would behave in a procyclical manner (and, therefore, generate money/income correlations of the kind that Friedman had emphasized).

Plosser recalled the early RBC literature in these terms: "That work basically said: 'Wait a minute. Maybe Milton was wrong.'" In retrospect, Plosser saw these models with instantaneous monetary neutrality as allowing the discussion of the role of monetary policy to proceed in a more orderly basis. "What role does money play? You can't answer that question unless you have a benchmark with which to judge it against." In practice, much of the debate with monetarists that King and Plosser had concerning their work on money was not with Friedman but with Karl Brunner, who was their colleague at the University of Rochester. "Bob King and I had a couple of papers in the *Journal of Monetary Economics* that Karl argued vehemently with us about… Karl did not like the real business cycle stuff very much." (Charles Plosser, interview, April 2, 2015.)[14] Nevertheless, Brunner passed over the position of editor to the *Journal of Monetary Economics* in 1985 to King and Plosser. In later years, both King and Plosser would move to using dynamic general equilibrium models in which monetary policy had real effects on output in the short run because of price stickiness.

The idea that money could matter for nominal but not real variables, and that consequently money/output correlations did not testify to the effects of monetary policy, was one to which Friedman had been exposed even before the RBC literature was launched. During the 1970s, several of the University of Chicago's specialists in the field of finance had made plain in the Workshop on Money and Banking that they did not accept Friedman's view of the role of money in the business cycle. Among these had been Fischer Black, who in the 1980s would take exception to a reporter's statement in the *Wall Street Journal* that economists unanimously

[14] Late in his life, Brunner outlined his reservations about the real business cycle research program (including its interpretation of money/output correlations) in a lecture published as Brunner (1989).

subscribed to the view that money mattered for the business cycle. Black objected that he was an economist who did not agree that money mattered (*Wall Street Journal,* November 27, 1984*a*). Another workshop participant who had taken this position was Eugene Fama: "Fischer Black and I went to the money workshop regularly, and he [Friedman] did not like us, because we kept challenging him on why he didn't think about things from the perspective of what portfolio theory was saying about money and all of that stuff, and what it implied for monetary economics." Indeed, while Lucas (1987, 1994, 2004) repeatedly affirmed that his belief in the validity of the RBC literature's account of the business cycle did not extend to the case of analyzing the Great Depression—for the understanding of which Lucas insisted that the Friedman-Schwartz emphasis on money was essential—this perspective was not shared by figures in finance such as Fama, who observed with regard to the Depression, "I think Friedman and Schwartz were way off the mark." (Eugene Fama, interview, September 11, 2013.)[15]

It may well have been someone in the University of Chicago's RBC-sympathetic finance tradition who was assigned to serve as a referee after Friedman, in 1987, submitted an article for consideration by the *Journal of Political Economy*.[16] Friedman's outline in that article of the possible connections linking real money balances and the behavior of the U.S. stock market prompted a response from the referee, who said that Friedman's outline was predicated on outcomes for nominal and real variables being determined as part of an interactive process. The referee objected that output might be determined wholly by real forces, with monetary policy being fully neutral and with stock prices and money balances (both real and nominal) merely providing advance indications of output movements. Friedman declined to revise the main text of his paper in light of the referee's argument. Instead, he confined his response to the objection to a footnote, in which Friedman affirmed, "I believe that the bulk of the evidence contradicts a purely real theory of business fluctuations." The referee's explanation for money's correlation with real variables, Friedman suggested, required a combination of predictable output movements and passive money-market behavior that was not empirically plausible.[17]

[15] Some major contributors to the RBC account literature shared Fama's view that depressions could be accounted for by purely real explanations. See especially Cole and Ohanian (2002) and the contributions in Kehoe and Prescott (2007). See also Nelson (2020a, Chapter 2) for a discussion of the RBC literature's account of U.S. output behavior during the 1930s.

[16] The article was published as Friedman (1988a) and is discussed further below. The referee's argument, under which money predicts output because it reflects the market's processing of information about future real activity, was similar to one Fischer Black had outlined to Christopher Sims on the day Sims presented what became Sims (1972) to a University of Chicago workshop around 1971 (L.P. Hansen, 2004, p. 278; Christopher Sims, interview, March 15, 2013). The counterargument that Friedman offered to Black was that Black was attributing to the nominal money stock properties that were better viewed as those possessed by real money balances (see Mehrling, 2005, p. 157).

[17] Friedman (1988a, p. 223).

Disinterring the plucking model of economic fluctuations

At the end of 1988, Friedman again confronted the RBC movement when he acknowledged the "surge of renewed interest in business cycles in general, and [in] real business cycles in particular." The occasion for his remark was a paper that he issued in the Hoover Institution's Working Papers in Economics series.[18] The paper, "The 'Plucking Model' of Business Fluctuations' Revisited," promoted a proposition that Friedman felt had been unjustly neglected since he had expounded it in the mid-1960s: that strong recoveries followed deep recessions, and mild recoveries followed mild recessions. The "plucking model" underlying this proposition was hardly actually a model at all; rather, it was just a statement of this claimed cyclical regularity. Nor was the working paper itself altogether new: in the paper, as in many of his writings by this time, Friedman followed a disconcerting practice in which a brief preamble containing new text led into slabs of material that he had published in decades past. In this case, the quoted text was from Friedman's contribution to the 1964 annual report of the NBER.[19]

Amidst the recycled material, Friedman did provide some freshness to the discussion by providing an update of his empirical evidence on the plucking regularity (albeit only to 1982, the date of the end of the most recent recession) as well as mildly negative remarks on the real business cycle approach. The plucking regularity, he conceded, by itself was consistent with both real and monetary accounts of the business cycle. But he pointed to other features of the cycle, including the comovement of real income and nominal income, that suggested significant monetary nonneutrality in the short run. Friedman further expressed the judgment that the RBC literature had exaggerated the role of technology shocks.[20]

Although Friedman's new paper on the "plucking" regularity was a very slight piece indeed, he succeeded in publishing it essentially unaltered in the journal *Economic Inquiry* in 1993. He was also successful, through the publication of the article, in generating new interest in the "plucking" notion, and a small literature that grew out of the new article formalized and tested the idea and applied it to modern business cycle models (see, for example, Kim and C.R. Nelson, 1999; Morley and Piger, 2006; Levin, López-Salido, E. Nelson, and Yun, 2008, p. S55; Sinclair,

[18] See Friedman (1988b).

[19] That is, Friedman (1964a).

[20] Friedman (1993a, pp. 173, 175). It should be noted that Prescott has come to favor the term "productivity shocks" to "technology shocks," in order to reflect the dependence of productivity on the legal, political, and regulatory environment in which the market sector operates, and to discourage the notion that there is a mechanical link between the rate of inventions and the behavior of total factor productivity. (Edward Prescott, interview, February 16, 2016.) This shift was signaled in Hansen and Prescott (1993, p. 286).

2009; Bordo and Haubrich, 2012; and Dupraz, Nakamura, and Steinsson, 2018).

"*There are no more young monetarists*"

Friedman's work on the plucking model was only tangentially related to his prior main body of research—that in connection with monetarism—and 1987 and 1988 continued the rough ride that the monetarist position had had since 1982. The widespread interest in the RBC literature in the 1980s was, as already stressed, a blow to the monetarists' account of money/output correlations. And, in any event, considerable doubt was being cast by commentators about whether money/nominal income correlations still existed in the U.S. data, especially in view of the well-publicized problems encountered with M1 in policymaking and in empirical studies. Against the background of this barrage of critical comment on the status of money, Robert Hall's blunt assessment was: "There are no more young monetarists." (*U.S. News and World Report*, February 1, 1988.)

Some comfort for monetarists could be taken in the fact that the New Keynesian movement that was shaping up as the main opposition to the RBC camp included key monetarist propositions on inflation and unemployment in its body of thought. New Keynesian work accepted the natural rate hypothesis—indeed, in 1988 Alan Blinder used his *Business Week* column (February 15, 1988) to explain and endorse the natural-rate-of-unemployment concept, which he explicitly credited to Friedman—and it put emphasis on the links between monetary policy and inflation. Furthermore, although it usually did not support the constant-monetary-growth rule or other policy rules that did not respond to the output gap, the emerging New Keynesian analysis tended to offer a critical perspective on old-style Keynesian approaches to stabilization policy, including the emphases on fiscal policy and on what Jeffrey Sachs called the desire "to fine-tune the economy from month to month" (*U.S. News and World Report*, February 1, 1988).

The New Keynesian literature also initially continued Friedman's emphasis on the money stock as the key monetary-policy variable (although this would change in the 1990s).[21] But Hall was correct in stating, in the remark quoted above, that it was exceptionally rare for newer members of the profession to be self-identified monetarists. The absence of an emerging new generation of monetarists was compounded by the fact that the passing of time was thinning the ranks of monetarists of earlier generations. Robert Weintraub died in 1983, Leonall Andersen in 1985,

[21] See the next chapter.

Michael Hamburger in 1986, and Karl Brunner in 1989.[22] As discussed in Chapter 14, another monetarist who passed away in these years was Friedman's former teacher Homer Jones, who died in early 1986.

Farewelling Arthur Burns and George Stigler

In June 1987 came the death of another of Friedman's former teachers, one who had served for a long time as a mentor figure: Arthur Burns. For Burns' memorial service in Washington, D.C. on July 22, 1987, Burns' son Joseph assembled a lineup of high-profile speakers including Friedman, President Reagan, Secretary of State George Shultz, outgoing Federal Reserve Chairman Paul Volcker, Chairman-designate Alan Greenspan, and former presidents Nixon and Ford. Friedman, having been on excellent terms again with Burns since the late 1970s, used the professional experience of his fellow speakers as a basis for refraining from himself offering a detailed discussion of Burns' years as Chairman of the Federal Reserve Board. After giving his usual account of the virtues of Burns' vision of the role of Chair of the Council of Economic Advisers—that is, as a technical position, rather than as a public advocate of administration policies—Friedman mentioned Burns' return to U.S. economic policymaking in 1969 but added: "Those who will follow me at this podium are far better qualified than I to comment on that phase of Arthur's life."[23]

The division of labor among speakers that Friedman unilaterally assigned allowed him to sidestep a discussion of his acrimonious relations with Burns over the first half of the 1970s and his disappointment with the monetary policy pursued by Burns' FOMC. On other occasions in the period after Burns' death, however, Friedman—although he did not go out of his way to do so—was prepared, if the subject of Burns' period as Federal Reserve Chairman came up, to reiterate his criticisms of Burns' record.[24] Friedman did this, for example, in a meeting with

[22] Friedman participated in an event in Washington, D.C., in late 1983 in honor of his recently-deceased former student Weintraub; he also subsequently contributed an article (Friedman, 1985b) to a publication released by Congress in Weintraub's memory.

The *New York Times* (August 15, 1986) contained a news item on the death of Hamburger (a former student of Allan Meltzer's) at only 47. Friedman's copious citations of Hamburger's research in his (1988a) article may have stemmed in part from looking back at Hamburger's work after his death.

Another major death in monetary economics in the 1980s was that of Henry Wallich in 1988. Wallich, who stepped down from the Federal Reserve Board in 1986, had been ill for some years, and one of the few changes Friedman made when revising his own *Encyclopaedia Britannica* entry, "Money," for a new edition of the *Britannica* in 1986 was to add a citation of a compilation of Wallich's speeches and papers on monetary policy (Wallich, 1982; Friedman, 1986c, p. 329).

[23] Friedman (1987c, p. 10).

[24] For public criticisms of the Burns record that Friedman made *before* Burns' death, see Chapter 14 of Book 1 and the earlier chapters of the present volume.

Athanasios Orphanides in May 2000 (Athanasios Orphanides, interview, June 27, 2014). And even in the more immediate aftermath of Burns' death, Friedman restated his disdain for monetary policy during the 1970s. He did so, for example, in April 1988, in a piece in which Friedman contended that if his constant-monetary-growth rule had been followed, "the country (and the world) would never have suffered the accelerating inflation of the '70s and the accompanying stagflation" (*Wall Street Journal*, April 15, 1988). And in *Money Mischief* in 1992, Friedman referred to 1970—Burns' first year in office—as the point at which U.S. monetary policy began un unrestrained course that he said gave rise to both economic instability and the deterioration of the thrift industry.[25]

In early 1992, Friedman was once again speaking in memory of a close friend in the economics profession. This time the friend was George Stigler, who had died in December 1991. Although their areas of research had overlapped exceedingly little since the late 1940s, Friedman and Stigler had been seen as a two of a kind for much of that period. In large part, this image reflected their camaraderie, which was evident to colleagues both at the University of Chicago and the Hoover Institution (at which Stigler was based part of the year). Friedman had been pleased to see Stigler receive a Nobel award in economics in 1982—all the more so because, as with the Nobel award that would go to James Buchanan in 1986, the award confirmed professional esteem for the public-choice literature that Friedman himself rated so highly.[26]

As might have been expected, Friedman was greatly shaken by Stigler's death. In January 1992, after an hour of discussion of monetary matters, Friedman picked up his own copy of a much-published photograph of himself and Stigler and recounted his friendship with Stigler to the present author (Milton Friedman, interview, January 22, 1992). The following year, in the *Journal of Political Economy* Friedman remarked that the world was a "far dimmer and less joyful place" for him with Stigler's death.[27]

Research on money and the stock market

As stressed in Chapter 14 above, Stigler, unlike Friedman, had retained an active University of Chicago affiliation. As part of his duties at the university Stigler had been a coeditor of the

[25] Friedman (1992c, pp. 251–252).

[26] Friedman reaffirmed his high esteem for the public-choice literature on a number of occasions in the 1987–1992 period, including in comments he sent to Stanley Fischer in 1987 (see Fischer, 1990, and Nelson, 2020a, Chapter 8) and in an interview with Michael Parkin (Parkin, 1990, p. 100).

[27] Friedman (1993b, p. 773).

Journal of Political Economy when Friedman submitted his already-mentioned "Money and the Stock Market" manuscript. The opening paragraph of this paper identified it as a "note," and Friedman may indeed have intended the article—which was twenty-five pages in length in its published form—to be brief. He had been spurred into writing the paper by a piece of research by the financial firm Oppenheimer and Company in which the relationship between M2 velocity and stock market activity had been brought out in a time-series plot.[28] Friedman's research on this matter developed into a piece issued in 1987 as a Hoover Institution working paper.[29] Thereafter, with minor additions arising from the aforementioned *Journal of Political Economy* editorial process, the paper appeared in the April 1988 issue of the *Journal of Political Economy,* as the lead article, preceding an unrelated piece by Paul Samuelson.[30]

The article was in large part a follow-up to the U.S. demand-for-money analysis that had appeared in *Monetary Trends*. The 1988 article proved in the event to be a minor addendum to the *Trends* analysis. For although Friedman's new paper found that stock prices entered significantly when added to a money demand function specification like that estimated in *Trends*, the coefficients on current and lagged stock prices were of mixed sign in Friedman's quarterly estimates, and stock prices appeared to wash out completely when annual data were considered.[31]

Friedman's 1988 *Journal of Political Economy* paper therefore added little to the demand-for-money specification considered in *Trends*. Two notable areas of alignment of the paper with the modern-day econometric literature on monetary relations do deserve mention,

First, Friedman reaffirmed something embedded in the narrative account in the *Monetary History*: notwithstanding considerable stability in monetary relationships over time, it was known to happen that the monetary policy regime *could* matter for money demand (for given scale and opportunity-cost variables in the money demand function.) This acknowledgment came as he indicated that he had started his regressions' sample periods in 1951 or later in order to avoid the money demand estimates being based on sample periods that included the Federal

[28] Friedman identified the origin of this graph only as "a financial institution" (Friedman, 1988a, p. 221) in the paper, but it came from Oppenheimer and Company, for whom Friedman regularly gave talks, and was produced by Oppenheimer's economist Rudolf Hauser.
[29] See Friedman (1987a).
[30] Two other articles appearing in the same issue—Fama and French (1988) and Hall (1988)—proved far more influential than the Friedman (1988a) and Samuelson (1988) articles.
[31] See Friedman (1988a, Tables 3 and 4, pp. 233, 237).

Reserve's pegging of securities prices in the 1940s and early the 1950s.[32]

Second, Friedman offered a vigorous criticism of the practice of using differenced data to analyze structural monetary relationships. Such a practice, Friedman suggested, might buy better statistical properties of the equations' error terms but could well do so at the expense of gaining insight into the economic linkages of interest.[33] This passage—which restated a longstanding Friedman position—so paralleled a major message of the modern cointegration literature that it has been quoted in that connection by Cochrane (2018).

Reaffirming the quantity theory of money

The *JPE* article also provided the opportunity for Friedman to reaffirm his position that the money/income relationship had withstood the test of time, notwithstanding the plethora of obituaries for monetarism that were appearing in the 1980s. Friedman also used a string of contributions to the *Wall Street Journal* (February 12, 1987; April 15, 1988; June 22, 1988) to press the same point. Like the *Journal of Political Economy* article, these analyses used the M2 definition of money. Friedman's 1987 *New Palgrave* entry, "The Quantity Theory of Money," was another contribution in which he affirmed the durability of the money/income and monetary-growth/inflation relationships, although the entry did not go into detail about the behavior of recent years' data. And, having been—as discussed in the previous chapter—largely drafted in 1985, the *New Palgrave* entry was composed *before* Friedman had settled on an M2-oriented account of U.S. cyclical developments in the 1980s.

[32] See Friedman (1988a, p. 229). The explanation for the effect on money demand of pegging and the subsequent Accord was, however, better in the *Monetary History* than that in Friedman's 1988 article. The latter discussion stated that the pegging regime made short-term securities an unattractive alternative to money but did not make it clear that the regime also made bonds more attractive than both. Furthermore, his chronology was muddled, as it attributed the 1950–1951 rise in M2 velocity to the end of the Accord and the 1951–1953 above-normal values of velocity to the Korean War. In contrast, in his other solo-authored and coauthored accounts, the initial rise had been attributed to the Korean War, and the strength of velocity in 1951–1953 being due to continuing fairly rigid targeting by the Federal Reserve of longer-term interest rates (at a time when short-term interest rate management was becoming more flexible, as discussed in Anbil and Carlson, 2019, for example).

Finally, a reader might take from Friedman's (1988a) quick account that the fact that the Treasury bill rate was fixed during (some of) the pre-Accord years *ipso facto* rendered that rate invalid as a measure of the opportunity cost of holding money. It is true that the money demand literature has occasionally offered the argument that, if an interest rate is administered by the authorities, doubt is cast upon the practice of putting that rate in the money demand function (see, for example, Johansen and Juselius, 1990, p. 172). However, if short-term interest rates are administered by monetary policy actions, then those interest rates remain market-clearing rates even though they are policy-determined—and so could well remain valid measures of short-term credit costs. It could be objected that, in the pre-Accord period, the pegged interest rates were unrepresentative of U.S. securities-market interest rates more broadly. This, however, does not appear to have been the case for most of the pre-1951 period (see Chaurushiya and Kuttner, 2003, p. 8).

[33] See Friedman (1988a, pp. 230–232).

The *New Palgrave* entry was published in the extremely expensive 1987 dictionary of economics, a deluxe, multi-volume set of hardbacks. However, except for a title change (one possibly prompted by copyright considerations), the *New Palgrave* entry was available in a more affordable form as *The Essence of Friedman* (Leube, 1987b), a bumper-sized trade paperback collection of reprinted Friedman articles that the Hoover Institution issued to commemorate Friedman's seventy-fifth birthday. Friedman was not involved with the process of assembling *Essence*, but the selection of articles in the volume was well-chosen, with 1970's "The Counter-Revolution in Monetary Theory" and Friedman's Congressional testimony the main notable omissions from the volume's coverage of his monetary writings.[34]

The 1987 stock market crash

Friedman's finding in his recent research that consideration of stock-market variables did not much alter his prior findings on money put him in good stead to react to the U.S. stock market crash of October 1987. At a time when many commentators took for granted that at least a mild recession was in prospect in the wake of the stock market crash, Friedman was a voice of calm. He mocked the emphasis put by commentators on the shock to private sector wealth arising from the crash. The crash undid the gains recorded in the stock market since the end of 1986, Friedman noted, and just as the rise in stock-market values during the first three quarters of 1987 had not led to a consumer boom, the nullification of the equity-price gains would not induce a collapse in consumption (*Wall Street Journal*, December 2, 1987). He dismissed comparisons with 1929 because the pattern of Federal Reserve policy observed after 1929 would not be repeated this time around (*The Independent* (London), October 28, 1987; *National Review*, November 20, 1987). On a panel on *Nightline* in November 1987, in which Friedman featured with Robert Solow (who had recently become a fellow Economics Nobel award winner), Friedman emphasized that central bank action could easily offset and overwhelm the negative pressure on aggregate demand arising from the stock market crash (*Nightline*, ABC, November 6, 1987). By early December, he was able to highlight signs that this was already occurring: the

[34] A Congressional submission (Friedman, 1958) was, however, included in the *Essence* collection. As well as reprinting or excerpting various Friedman articles, *Essence* also had excerpts from a couple of his books (although *Monetary Trends*—which was listed on *Essence*'s back cover as one of the books excepted—did not in fact feature in *Essence*, possibly because the hardback and trade paperback versions of *Trends* were still in print in 1987).

Although it was absent from *Essence*, Friedman (1970b) was reprinted in *Monetarist Economics* (Friedman, 1991a). This book was a (non-exhaustive) collection of Friedman items that over the years had been written for, or reissued by, London's Institute of Economic Affairs (several of which were not, in fact, on the topic of monetarism). Friedman may not have been aware of the plan to publish *Monetarist Economics* and likely did not see it until well after it was published in 1991, and the book was not included in his official bibliography during his lifetime. He did, however, have a copy of the book in his apartment at the time of his death in 2006. (Information from Gloria Valentine.)

stock of wealth held in the form of fixed-income securities was much larger than the total value of equities, Friedman observed, and the value of bonds had risen since the stock market crash (*Wall Street Journal,* December 2, 1987).

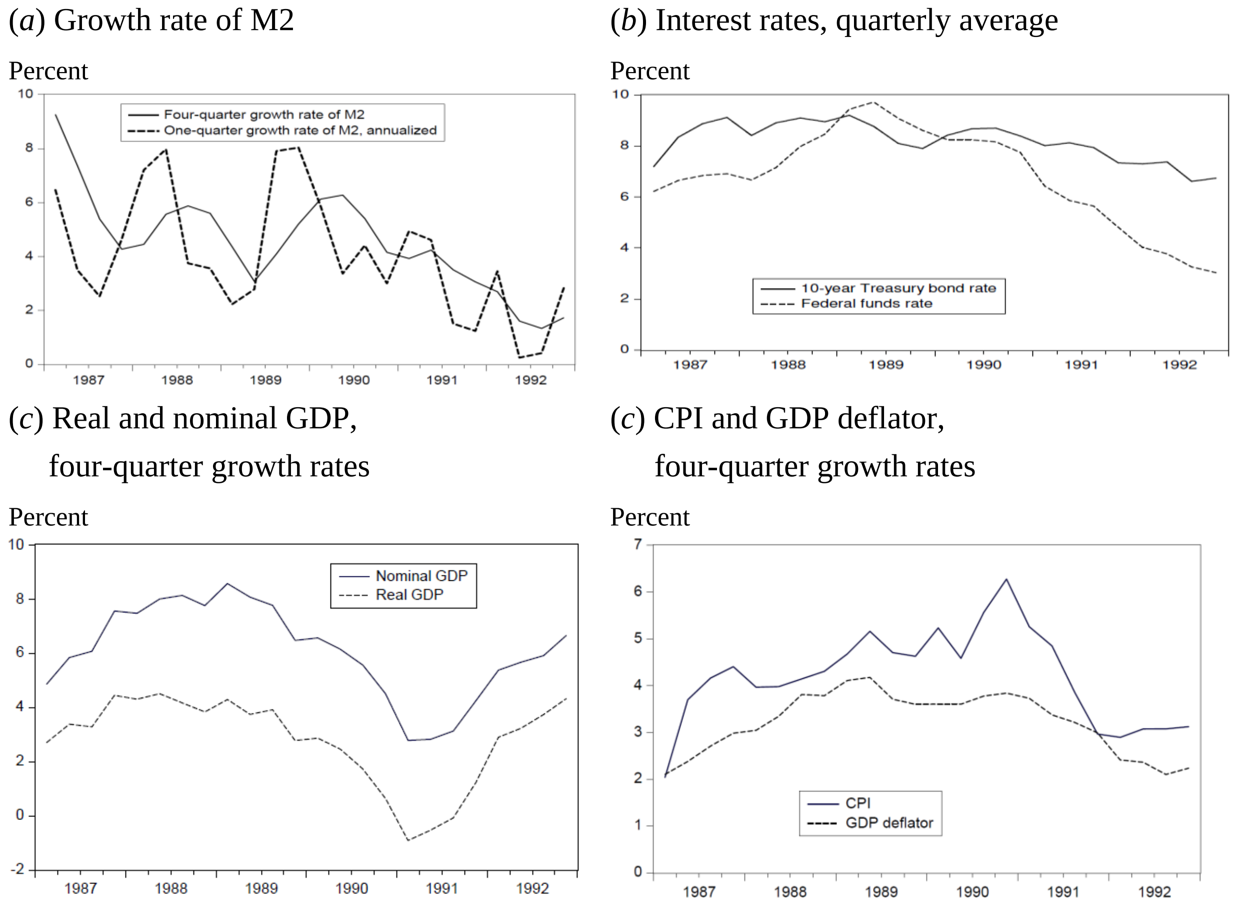
One of Friedman's observations in the wake of the crash proved especially apposite. "The crash, ironically, makes recession less likely because of indirect effects on Federal Reserve policy." (*The Independent* (London), October 28, 1987.) Speaking a couple of weeks prior to the crash, Friedman had warned that a recession was likely for 1988 if a recent downturn in M2 growth continued (Reuters, October 7, 1987). With the Federal Reserve's swing to ease following the crash, M2 growth picked up in the fourth quarter of 1987. In the first half of 1988, M2 growth proceeded at an annualized rate of about 7.5 percent, more than double the rates observed in mid-1987. (See Figure 1(*a*).) Likewise, the monthly average for federal funds rate—which the Federal Open Market Committee continued to use, albeit without giving much public acknowledgment, as its policy instrument—peaked for 1987 in October and did not exceed its October 1987 value until June 1988. (See Figure 1(*b*) for the quarterly values.)

By this mid-1988 point, it was clear that the stock market had not led to a recession. Commenting on this development, Anna Schwartz pointed to the fact that "the Fed's performance this year has been different than in 1930," while Friedman noted that his contention that consumers regarded stock market-induced changes in wealth as ephemeral had been borne out: "It was a transitory shift in wealth, not a permanent one. For most people, it was easy come, easy go."[35] In a joint Friedman-Samuelson television appearance in 1990, Samuelson observed that the 1987 stock-market crash had shown that the economy and the stock market could be decoupled for extended stretches of time. Friedman responded: "Unaccustomed as I am to agreeing with Paul, I agree with everything he says."[36]

Friedman and the later Reagan years

A year almost to the day after the crash, Friedman was at the White House to receive the Presidential Medal of Freedom from Ronald Reagan. Notwithstanding his long acquaintanceship with Friedman, Reagan's performance at the ceremony reads as wooden: he merely stated Friedman's citation, before moving on to the next recipient. However, Reagan had hosted a lunch for Friedman and the other recipients earlier in the day, and he had recently personally

[35] Both the Friedman and Schwartz remarks are from *Business Week,* April 18, 1988, p. 38.
[36] From *The MacNeil/Lehrer NewsHour,* August 27, 1990, PBS, p. 6 of transcript.



(*a*) Growth rate of M2

(*b*) Interest rates, quarterly average

(*c*) Real and nominal GDP, four-quarter growth rates

(*c*) CPI and GDP deflator, four-quarter growth rates

Figure 1. Selected macroeconomic series, 1987:Q1–1992:Q4.
Source: Federal Reserve Bank of St. Louis' FRED portal. Quarterly averages are taken of M2 and the CPI.

penned a reply to a letter from Friedman, with Reagan's letter making mention of his regret at missing Friedman's receipt of the National Medal of Science the previous July.[37]

An examination of the text of the Medal of Freedom citation reveals the tension between Friedman's status in the economics profession as a key contributor to research and his public face as an advocate of free markets. Indeed, Reagan's citation seemed calculated to offend Friedman's fellow economists, especially those colleagues in the profession who did not line up closely with Friedman on the matter of the appropriate role of government. Reagan's citation did not use the word "research" at all, contained the overblown assertion (discussed in Nelson, 2020b) that Friedman possessed a "technical mastery of his profession [that] is unchallenged,"

[37] Reagan (1988). Reagan's letter to Friedman, which included the reference to the conferring (on July 15, 1988), conferring of the National Medal of Science award, was dated September 13, 1988. It was published in Skinner, Anderson, and Anderson (2003, p. 326).

claimed Friedman “restored common sense to the world of economics,” and stated that Friedman was getting the award “for his celebration of the human spirit” (even though Friedman cast his advocacy of the market primarily not in terms of a positive attitude to the human spirit, but in terms of devising a way in which self-interested individuals can work together).

By the time of the ceremony, Friedman had put on record on a number of occasions his evaluation of Reagan’s economic record. As of the second half of Reagan’s first term, Friedman felt warmly toward the present but was disappointed with much of his economic record, being particularly critical of the continued expansion of federal government spending.[38] Friedman also felt negatively during this period about the administration for ratifying what he saw as a too-expansionary monetary policy.[39] By mid-1986, it was clear that Friedman had been wrong in his evaluation of the consequences of the Federal Reserve policy for 1982–1983, and his new evaluation, while remaining ungenerous to Paul Volcker, was that the monetary restraint that had ended the Great Inflation was due to Reagan’s presence in the White House.[40]

Also by 1986, enough evidence of domestic spending restraint had emerged for Friedman to praise him as having been consistent and stuck to his goals.[41] Friedman was further pleased to see his former student Michael Darby serve as an undersecretary in the U.S. Treasury in the last two-and-a-half years of Reagan’s tenure. “When I went to Washington,” Darby recalled, “he and Rose advised me against it on the grounds that I’d never be able to do research again, after I’d been ruined by Washington. Fortunately, my wife was back here [in California] and lured me back into research, and I never caught Potomac Fever, which so many people do… And even though Friedman advised against my going to Washington, he was always very kind, both to me and particularly [in remarks] to others that would get back to me, about what I accomplished in Washington.” (Michael Darby, interview, October 15, 2013.)

Friedman’s praise for Darby’s years in government service was one example of a larger reassessment on Friedman’s part of the Reagan administration’s record. Friedman’s cautious, or even somewhat jaded, outlook toward that record evolved into an extremely positive one in Reagan’s last two years. Thus, writing in April 1987, Friedman referred to “the excellent

[38] See, for example, Friedman and Friedman (1985, p. 39).
[39] See, for example, Friedman (1984i, p. 41). See also the previous chapter.
[40] See Friedman (1986b, p. 245; 1988c, p. 381) and *Wall Street Journal*, April 20, 1987. The position expressed in these articles was one Friedman maintained in later years, as is evident in his observation in 1999 that “you have to give the credit there [for the disinflation] really to Reagan” (*Uncommon Knowledge*, February 10, 1999). See also Friedman’s remarks (made in 2000) in Taylor (2001, p. 107) and the more extensive discussion in Section III below.
[41] See Friedman (1986b, p. 245).

performance of the U.S. economy in recent years," and he cited the continued economic expansion, lower interest rates, and lower inflation. Friedman attributed these results largely to a series of domestic-policy initiatives, including the initial policy of disinflation, the reduction in federal tax rates, government expenditure restraint, deregulation, and certain other measures such as privatization and a much-reduced degree of wage and price control (*Wall Street Journal*, April 20, 1987). Speaking after the stock market crash, Friedman described Reagan as having presided over the longest peacetime expansion.[42] The danger, as Friedman saw it, was that Reagan might succumb to pressure to abandon the policies he had followed so far (*ABC Evening News*, November 2, 1987; *Panorama*, BBC1, December 7, 1987).[43] It became clear over subsequent months that no such *U*-turn was occurring, and Friedman's assessment in July 1988 was upbeat: "The domestic policies of the Reagan administration have been very good."[44]

The sting in the tail in this assessment, of course, was Friedman's disapproval of the international dimension of Reagan's economic policy. On exchange-rate policy at least, and notwithstanding the moves in the direction of international policy coordination, Friedman had cause for relief. In early 1988 he observed that proponents of international monetary reform had not got their way and the dollar continued to float (see *Wall Street Journal*, March 4, 1988, and the next chapter). Trade policy, however, continued to be a different matter. In the second half of 1988, Friedman would refer to the "protectionist movement that we've been on [for] the past eight years."[45] During the same period, he expanded upon his assessment of the Reagan record: "I yield to no one in my approval of the domestic policies the Reagan administration has followed, particularly tax reform and the attempt to hold down government spending, but I have been disappointed in the administration's international trade policies."[46]

Friedman's disillusionment with Reagan's record on matters concerning international trade had begun early, as discussed in Chapter 14. "I am a strong proponent of President Reagan's general

[42] *National Review*, November 20, 1987, and *Panorama*, BBC1, December 7, 1987. Recall that Friedman regarded the 1966–1967 period as featuring a recession, in which case the period from 1961 to 1969 would not count as a continuous expansion even if classified wholly (the Vietnam War notwithstanding) as a period of peacetime.

[43] In *Stanford Review* (California) (November 1987, p. 7), Friedman cited the Iran-Contra affair (of 1986–1987) and the defeat (in October 1987) of Reagan's Supreme Court nominee Robert Bork as factors that had lowered Reagan's stature and that pointed toward the possibility that Reagan might bow to pressure to change his economic program. Just before the stock market crash returned his focus to economic commentary, Friedman had been participating publicly on the losing side of the push to have Bork put on the Supreme Court. See, in particular, his article, coauthored with Gerhard Casper, in *Wall Street Journal*, October 21, 1987.

[44] Friedman (1988c, p. 380).

[45] Again, see Friedman (1988c, p. 380).

[46] From Friedman's July 28, 1988, remarks, in Friedman (1989b, p. 12).

policies, and he is a strong believer in free trade," Friedman had said in March 1982.[47] Even by then, however, Friedman already had cause to be unhappy with Reagan's record on trade, and in *Newsweek* in November 1982, in berating "recent protectionist measures by the Reagan Administration," Friedman noted that actions beginning early in the administration had shown that free traders had been "clearly wrong" in their original belief that Reagan would hold the line against the protectionist movement (*Newsweek*, November 15, 1982). Friedman declared himself "severely disappointed (*Newsweek*, November 15, 1982) and continued to be so.[48] The various protectionist measures came, Friedman complained, despite eloquent statements by Reagan about the benefits of free trade.[49]

Friedman's dissatisfaction with U.S. trade policy continued in the second Reagan term, and in April 1987 Friedman lashed out at trade sanctions newly imposed on Japan, describing them on television as "one of a long measure of protectionist actions which has been taken by this administration."[50] CNN broadcaster Larry King quoted Friedman as saying (*USA Today,* April 27, 1987): "Mr. Reagan is dead wrong on this one. I've been a strong supporter of most of his economic policies, but on this one, he couldn't be more off the mark." He added that while Reagan had consulted him in the past, "nobody from the White House called me on this one."

On the other hand, Friedman recognized that Reagan was attempting to hold the line against proposals for still-greater restrictions on trade, and in 1988 he praised the president's veto of what Friedman regarded as a protectionist trade bill (*Wall Street Journal*, May 26, 1988; *Lodi News-Sentinel* (California), May 26, 1988). Proposals to restrict capital movements into the United States were also resisted by the administration. Friedman encountered proponents of such measures himself, as in March 1987 when Friedman appeared on *Nightline* to speak against calls for restrictions on foreign investment in the semiconductor industry—restrictions that were being advanced on what Friedman regarded as specious sovereignty and national-security arguments (*Nightline*, ABC, March 17, 1987). The *Nightline* episode was subtitled "Japanese Hegemony?," but talk of this kind receded in the 1990s with the anemic performance of Japan's economy. Friedman would be one of those quick to spot Japan's entry during the early 1990s into a period of stagnation (see the next chapter).

[47] Friedman (1982a, p. 43).

[48] See also Friedman (1984i, p. 40).

[49] See Friedman's remarks in *Wall Street Journal*, October 25, 1983. See also Friedman and Friedman (1985, pp. 124–125) and the previous chapter.

[50] *The MacNeil/Lehrer News Hour*, PBS, April 17, 1987, p. 4 of transcript.

The savings and loan crisis

In the interval between George Herbert Walker Bush's election to the U.S. presidency in November 1988 and his inauguration in January 1989, Friedman was one of several Nobel economists asked by the *Wall Street Journal* to indicate the direction in which the new administration should go. One of Friedman's recommendations was that there should be a federal takeover of financially-vulnerable savings and loan (S&L) institutions, after which the institutions would be returned to the private sector on a sounder financial footing (*Wall Street Journal*, December 20, 1988). Such thrift institutions, whose financial condition in aggregate had shown some signs of better health in the mid-1980s, had weakened during 1988, and a major federal rescue and reorganization took place during Bush's period in office. Friedman's support for the idea of a federal takeover in principle again demonstrated that he had not become an advocate of free banking. He did, however, trace some of the thrift industry's problems to the public sector. For example, like many other analysts, Friedman saw the Great Inflation of the 1970s and the accompanying upsetting of the term structure of interest rates as a major reason for the deterioration in the S&L institutions' financial condition.[51]

The S&L crisis also prompted Friedman to look again at deposit insurance. *The Monetary History* had given a glowing judgment about deposit insurance, and Friedman's praise had continued in later years. For example, in July 1986, Friedman stated: "Deposit insurance is doing its job. It has prevented a repeat of the situation we had in 1931 and 1932, when depositor panics forced the closing of perfectly sound banks." (*Los Angeles Times*, July 27, 1986, Part 1, p. 17.) Although, as we have seen, Friedman voiced some qualms during the 1970s about possible complacency in bank management produced by the federal backstop, his general practice had been to downplay the likelihood of moral hazard from deposit insurance. But the problems in the thrift industry produced a more nuanced judgment from Friedman, who perceived an interaction between the presence of deposit insurance and lending practices, with the capital of the S&L's having reached levels low enough that Friedman regarded moral hazard as becoming a danger. He thus became more attracted to arrangements that coupled deposit insurance with measures designed to forestall excessively risky lending (see his remarks in Levy, 1992). Friedman's earlier writings suggest that he believed that capital requirements (that is to say, the requirements that loan-making be associated with specified minimum levels of issuance of bank capital) should be the means to generate this incentive (see Nelson, 2013). Indeed in the wake of the S&L crisis, Friedman later noted that "a substantial equity cushion" gives banks

[51] See Friedman's remarks in *National Review*, June 30, 1989, and in Friedman (1992c, p. 251).

"ample incentives to avoid excessive risk."[52]

At the same time, as a more radical way of preventing institutional crises, Friedman continued to speak favorably, even nostalgically, about a measure he had advocated in years past: 100 percent deposit insurance. Friedman pointed to what he saw as strong similarities between his old 100 percent reserves proposal and the "narrow banking" proposal that Litan (1987) and others were offering in the wake of the S&L crisis.[53]

Friedman even ventured to state that, had his 100 percent reserves proposal in *A Program for Monetary Stability*, the S&L crisis would certainly not have occurred. However, this suggestion reflected poor memory on Friedman's part, because in *A Program for Monetary Stability* he had specifically limited the 100 percent reserves proposal to a definition of money that included demand and time deposits but *excluded* the liabilities of thrift institutions. More fundamentally, the 100 percent reserves proposal continued to be a nonstarter for the reasons that Schlesinger (1961) had outlined in response to Friedman's *Program for Monetary Stability* proposal and that Benston and Kaufman (1993, p. 42) and Kashyap, Rajan, and Stein (2002, p. 35) had occasion to restate in the wake of the narrow-banking proposals. That is to say, there are synergies between issuing deposits and making loans, and prohibiting a depository institution from doing so invites the creation of lending/deposit institutions outside the regulated banking sector. As noted in Nelson (2020a, Chapter 2), Friedman seemingly recognized this point, and even his latter-day warm words for 100 percent reserve requirements went alongside discussions of the feasibility and desirability of a zero-reserve-requirement system.

The end of the Cold War

On the geopolitical front, the closing years of Reagan's term and the early years of his successor were witnessing enormous changes. "The much-vilified idea attributed to the Reagan Administration in 1981–82 of, in effect, spending the USSR into bankruptcy, no longer looks quite so primitive or foolish," a national-security commentator, Colin Gray, noted.[54] Gray's observation was underscored by the fact that, by the time his article saw print in 1992, the fall of the Berlin Wall in 1989 had been followed by the dissolution of the Soviet Union at the end of

[52] Again, see *National Review*, June 30, 1989, and Friedman (1992c, p. 251).

[53] See *National Review*, June 30, 1989, Friedman (1992b, pp. x, xii), and Oppenheimer and Company (1992, pp. 6–7).

[54] Gray (1992, p. 40).

1991.[55] Shortly after the USSR was dissolved, Friedman would judge that the 1980s U.S. defense buildup "had a big return in helping bring Communism down."[56] However, as discussed in detail in the next chapter, Friedman, like many others, was caught off-guard by the collapse of the Communist bloc. For example, writing in 1987, he and Rose Friedman had predicted that the liberalization of public discourse in the Soviet Union observed under Mikhail Gorbachev would stop short of a surrender of the Communist Party's status as the USSR's only legal political party.[57] But once the end of the Cold War occurred, this event came to be seen as part of what the Friedmans had characterized in their *Free To Choose* book as a cross-country turning of the intellectual tide in favor of free-market ideas.

Friedman's pleasure at the end of the Cold War—a development discussed further in the next chapter—was tempered somewhat by U.S. developments. Writing at the very end of the 1980s, Friedman lamented the fact that, just as the liberation of Eastern Europe was signifying the victory of market economics, support for free-market policies seemed to be losing momentum in the United States (*New York Times,* December 31, 1989). He would come to see the difference in economic policies between the Reagan administration and the successor Bush administration as confirming the United States' drift away from market-oriented policies.[58]

The failed relaunch of ***Free To Choose***

In the meantime, during 1990 Friedman's impression that support for free-market ideas was softening in the United States was reinforced by the disappointing outcome of his attempt to revive the *Free To Choose* television series. In that year the Friedmans reteamed with *Free To Choose* producer Robert Chitester to film a new episode of *Free To Choose*, in which Friedman

[55] For his part, as we have seen, Friedman supported the Reagan defense buildup (see the previous chapters). He remained cautious in speaking about foreign policy issues during the late Reagan period, observing: "Obviously, I'm not an expert in national security." (*Nightline,* ABC, March 17, 1987, p. 4 of transcript.) As he put it on another occasion: "Fortunately I am not a military expert." (*Jerusalem Post*, November 10, 1987.)

A detailed account of the role that a strategy of undermining of the Soviet economy might have played in the first six years of the Reagan Administration was given in Schweizer (1994). However, Schweizer's account was marred by an overemphasis on the linkage between trade and economic growth (and he therefore likely severely overestimated the effect that formal and informal economic sanctions on the Soviet Union—which, in any event, the United States only imposed in a piecemeal fashion—had on the USSR's economic growth rate) as well as by what was surely an overstatement of the role that the Reagan Administration consciously played in creating the oil price decline of 1985–1986. The factor stressed by Gray—inducing economic strain by obliging the USSR to increase its defense spending further—was a more clear-cut U.S.-imposed burden on the USSR, though one whose importance has probably been overstated in relation to other factors bearing down on the Soviet Union's economy. (See the next chapter for further discussion.)

[56] *Wall Street Week*, Maryland Public Television, February 21, 1992, p. 6 of transcript.

[57] See Friedman and Friedman (1988, p. 466).

[58] See the discussion titled "George Herbert Walker Bush" in Section III of this chapter.

would take a victory lap of sorts in the Eastern European countries.[59] The repackaged *Free To Choose* series consisted of this new episode plus four other episodes. These four episodes consisted of the filmed portions of four instalments of the 1980 series, together with, Chitester recalled, "totally new debate discussions, which none of us were as happy with as the originals." The debate portions were, indeed, not a patch on their 1980 counterparts. Whereas in 1980 Friedman had faced a panel of mostly-hostile interlocutors, the new debates were more sedate, and in them Friedman was given a teammate on the free-market side. The latter arrangement created the impression that he was no longer able to hold his own in give-and-take exchanges.

The United States' Public Broadcasting System was not interested in broadcasting the new series. In 1993 Friedman seemed to be still smarting about this decision, which he characterized as reflecting an anti-market stance on PBS' part (CSPAN, May 7, 1993). But the decision was understandable: the series was only partially new; transmission of the series would involve putting on air in the 1990s four documentaries that had been made in the 1970s and that had already been shown by PBS in the 1980s.[60] In the event, the cable channel CNBC broadcast the repackaged series—an arrangement that Chitester remembered turning out "very unsuccessfully… [I]f it had 200,000 viewers, I would be pleased to find that out." (Robert Chitester, interview, July 9, 2013.)[61]

Perhaps the most notable part of the 1990 *Free To Choose* project was the appending of the episodes with newly-recorded introductions by celebrities. Those providing the introductions—which would also appear on a videocassette release of the series—included ex-president Reagan and Arnold Schwarzenegger, the latter approaching the peak of his success as a film actor.

The narrative approach to monetary policy analysis

For all the success that *Free To Choose* had had, Friedman acknowledged in 1994 that "[t]here's no question that the most influential book I've written is not *Free To Choose*, but a book that sold probably one-twentieth as many, five percent as many copies, namely *A Monetary History*

[59] See Friedman and Friedman (1998, pp. 505–515).
[60] The *Free To Choose* book was reissued in 1990. The Friedmans provided a preamble for the reissue in which they stated openly that they were unwilling to conduct the effort needed to update the book, whose main text was largely left unchanged (Friedman and Friedman, 1990a, p. x)—but for an exception, see Chapter 11 above.
[61] The CNBC airings began on February 10, 1991. Chitester cited, as did Rose Friedman in Friedman and Friedman (1998, p. 515), a change in business model on CNBC's part once it had purchased the series, which led to disinterest in the series and to the eventual broadcasts receiving very little promotion (Rose Friedman going so far as to say that there was *no* promotion). Notwithstanding this, the upcoming transmission of the series was mentioned in the *Chicago Tribune* (January 26, 1991) and the *Wall Street Journal* (February 7, 1991).

of the United States, which I wrote jointly with Anna Schwartz…"[62] Although, around the same time, Lucas (1994b, p. 13) would look back on the 1980s as a period of "recession" for the influence of the *Monetary History,* by the end of that decade there were definite signs of a revival of interest in the book. A seminal paper by Romer and Romer (1989) examined postwar periods of U.S. monetary policy tightening. The authors' subtitle laid bare their inspiration: "A New Test in the Spirit of Friedman and Schwartz." Their "new test" represented partly an endorsement of, and partly a departure from, the Friedman-Schwartz approach. Like Friedman and Schwartz, Romer and Romer's "narrative" approach would lay great stress on the study of official records of U.S. monetary policy deliberations, and they were able to expand on Friedman and Schwartz in this dimension both by considering post-1960 events and by consulting documentation of early postwar decisions that was not available to Friedman and Schwartz. Also in the Friedman-Schwartz tradition was the fact that Romer and Romer endeavored to use historical information to isolate changes in monetary policy that could not easily be traced to the usual reaction of the monetary system to changes in the economy, and in so doing ascertain the effects of monetary policy on real economic activity.[63]

A key respect in which Romer and Romer broke with Friedman and Schwartz lay in the fact that the former authors largely eschewed an analysis of monetary policy that was centered on the behavior of the money stock. On this score, Romer and Romer anticipated the direction in which monetary analysis would go in the 1990s—a direction initially obscured by a revival of interest in money at the policymaking level during the early Greenspan years.[64]

Friedman himself had had occasion to reflect on the *Monetary History* in the late 1980s. He had refereed, for the *American Economic Review,* a paper on the early history of the Federal Reserve that the journal went on to publish in June 1987.[65] In October of the same year, he spoke of his

[62] CSPAN, November 20, 1994, p 8 of transcript.

[63] Specifically, Romer and Romer constructed a list of dates associated with unusual (that is, not rationalizable in terms of the Federal Reserve's usual response to the economy) tightenings of monetary policy. A research agenda of this kind had been anticipated by De Leeuw and Kalchbrenner's (1969, p. 11) observation: "Possibly a detailed examination of Open Market Committee records would be helpful in constructing a better measure of monetary policy."

In their (1964a) study, Brunner and Meltzer had analyzed Federal Reserve statements to construct an index of policy stance. However, unlike Romer and Romer, the date of their study meant that they were not able to go much beyond Friedman and Schwartz's sample period or use the minutes of FOMC meetings for the early postwar years.

[64] On this revival, see the discussion titled "Alan Greenspan" in Section III below.

[65] Mankiw, Miron, and Weil (1987). Jeffrey Miron (interview, June 20, 2013) recalled that Friedman signed his referee report on this paper. This was his standard practice, and he followed it when in the early 1980s he carried out his already-noted assignment of refereeing Hodrick and Prescott (1980) for the *Journal of Money, Credit and Banking* (Robert Hodrick interview, January 23, 2016). In addition, Taylor (2001, p. 122) recalled Friedman signing a referee report that Friedman wrote, at Taylor's request, on an *American Economic Review* submission. The report in this instance was on what became Hendry and Ericsson (1991).

collaboration with Schwartz (focusing on the *History* rather than the anticlimactic sequels *Monetary Statistics* and *Trends*) at a conference in Schwartz's honor in New York City.[66] The conference was intended to mark Schwartz's retirement from the NBER. Schwartz, however, although formally an emerita, stayed working at the NBER's New York City office on a daily basis, until a stroke in late 2009 ruled this out.

Money Mischief

At the Anna Schwartz Festschrift, Friedman affirmed that he had no interest in revising the *Monetary History* (Bordo, 1989, p. 76). He stuck to that judgment, but the following years would see him revisit, in a piecemeal fashion, the events covered in parts of the Friedman-Schwartz 1963 book. It may have been Friedman's satisfaction with, and fuller appreciation of the significance of, the *Monetary History* project that led him to produce a semi-popular book, *Money Mischief,* in 1992. As its subtitle indicated, the new book was centered on "episodes in monetary history." The drafting of the book largely took place over the 1989–1991 period. For example, the first chapter ("The Island of Stone Money") was issued as a Hoover Institution working paper in 1991.[67] Chapters 3, 6, and 7, which covered selected aspects of pre-World War II U.S. monetary history, appeared in individual-article form in the *Journal of Economic Perspectives* and the *Journal of Political Economy* over the 1990–1992 period.[68]

Money Mischief has some major admirers. Lars Christensen and David Laidler, for example, have hailed the book as a high-quality nontechnical introduction to monetary analysis.[69] Thomas Sargent, while disagreeing with a number of the book's historical judgments, has also assessed it as a fine book, whose approach to tackling issues showed Friedman's liking for the Irving Fisher tradition of writing.[70] When the book appeared in 1992, however, the present author was disappointed by the content of *Money Mischief.* That view did not change and, when it was

[66] See Friedman (1989a).

[67] Friedman (1991b).

[68] Friedman (1990a, 1990b, 1992d). After Friedman's death, the University of Chicago Press issued its own book collection of Friedman's writings (Friedman, 2007) that included the *Journal of Political Economy* versions of the *Money Mischief* chapters—thereby, in effect, duplicating what was already available in book form.

[69] See Laidler (1993b) as well as his and Lars Christensen's remarks in *The Market Monetarist,* July 1, 2013.

[70] Asked to describe what he liked about *Money Mischief*, Sargent replied: "There's a whole bunch of things… I like his things about the Crime of 1873. He has a couple of papers in it that are very influenced by Irving Fisher's *Purchasing Power of Money*… And so, I like that part. He [also] explains things about the demand for money really well. At the end, he has some things about [how]… we never really have become as divorced from gold as we had become at that time. He said it was basically a new experiment, and he was tentative about whether it was going to work. He had really interesting things to say about that." Sargent added that the book also "had some stuff about China and silver that I disagree with. And he knew that I did; we had a discussion of that." (Thomas Sargent, interview, March 26, 2014.)

voiced to Anna Schwartz in 2009 shortly before her aforementioned stroke, she expressed a similar assessent, referring to the book as possessing a "catchy title" but not enough substance or ambition. (Conversation with Anna J. Schwartz, September 18, 2009.)[71] The present author had been expecting something along the lines of a 1992-vintage version of *Dollars and Deficits*, in which Friedman tackled recent issues in a way that was nontechnical but nonetheless recognizably from the same perspective that had guided the *Monetary History*.[72] To this end, the book might have confronted monetary policy developments both in the period 1979 to 1984 (with an account of these years especially needed in light of the fact that much of Friedman's initial analysis of that period had, by 1992, to be considered as having been repudiated by him) as well as the years since 1984 (a period wholly beyond that covered by his *Newsweek* columns).

Instead, Friedman's book avoided a discussion of recent developments. He essentially sidestepped the controversies in the 1980s over monetary aggregates by devoting well over half of the text of his book to pre-1945 events.[73] The outcome was that much of the content of the book was surely not of interest to the general reader. Yet at the same time *Money Mischief* risked cheapening the memory of the *Monetary History*, because Friedman's choices of topics involved partly retreading, and partly expanding upon in a haphazard way, material that he and Schwartz had covered.[74]

In the chapters touching on post-1945 developments, *Money Mischief* contained some writing—such as his discussion of Chile's and Israel's different experiences with fixed exchange rates in the 1980s—that could reasonably be said to be largely new material from Friedman. Nevertheless, a good deal of the material in the book was reworked from items published before 1989. The chapter on inflation was a lightly-updated version of that in the *Free To Choose* book, and Chapter 10's "Monetary Policy in a Fiat World" was a rewriting of a mid-1985 talk by Friedman that had appeared in print at least three times by 1992. *Money Mischief*'s early chapter on monetary analysis includes both a lighthearted redrafting of the 1969 article "The Optimum Quantity of Money" and a concluding section based closely on Friedman's 1987 *New Palgrave*

[71] On the "catchy title" assessment, Schwartz was in agreement with Laidler (1993).
[72] Until talking to Friedman in January 1992, the present author had also been expecting *Money Mischief* to contain the cyclical analysis of money demand on which Friedman (1988a, p. 236) had said he was working. In the 1992 conversation, however, Friedman dispelled that notion when he discussed the content of his then-forthcoming book.
[73] That is, Chapters 3 through 6 (pp. 51–188) on U.S. monetary history as well as the whimsical first chapter (pp. 3–7). Friedman did briefly allude to the 1980s decline in velocity on page 46 of *Money Mischief*, but in doing so he provided a discussion considerably shorter than that available in his coverage of the matter in the *Wall Street Journal* (such as that in the February 12, 1987, edition).
[74] The haphazard character of the book is amplified by the absence of an index and the zigzagging in the chapters' coverage, with a passage in Chapter 6 (on page 127) considering the definition of bimetallism, which had already been the subject of Chapters 3 and 4 of the book.

entry (which, by 1992, was available in at least two paperback publications, as well as the hardback *Palgrave* version).[75] The bridging material in the monetary-analysis chapter left much to be desired, even when viewed as a nontechnical contribution. Most notably, Friedman misstated the content of standard money-multiplier analysis when he stated that "bonds," as well as deposit creation, are linked to the issuance of base money.[76]

Research activities

Other than the items that would appear in *Money Mischief*, Friedman produced a number of other brief journal articles in the early 1990s. In 1991, he contributed to the *Economic Journal* a tribute to that journal, which had recently reached its centenary. Although written in a breezy manner, the article revealed considerable delving on Friedman's part into the articles published in the *Economic Journal* over the years. The article in effect indicted the profession for inadequate scholarship, as Friedman cited examples in which recently-published articles had not traced back the history of their topic. Friedman's piece also criticized what he saw as a drift to over-technical econometric analysis. The latter criticism also was expressed in Friedman and Schwartz's reply, published in the *American Economic Review* in March 1991, to Hendry and Ericsson's (1983, 1991) critique of the study of U.K. money demand study in 1991. In trying to provide a comprehensive rebuttal to the Hendry-Ericsson critique, however, Friedman and Schwartz made some errors in interpreting Hendry and Ericsson's work. Indeed, they even misstated the content and arguments of their own *Monetary Trends*.[77]

[75] The criticism given here of *Money Mischief*'s focus on reprints may seem at variance with the favorable remarks offered above for *Dollars and Deficits*, which was largely a collection of preexisting writings. A key difference, however, is that *Dollars and Deficits* contained previously-unpublished memoranda by Friedman to the Federal Reserve Board which constituted both an encapsulation of the *Monetary History* and an extension of the *History*'s analysis to cover the 1961–1966 period.

[76] Friedman (1992c, p. 18). In using the term "bonds" here, Friedman may have been trying to get across the idea that—reflecting his confidence in the links between the monetary base and M2—he saw the volume of commercial bank reserves as related not just to demand deposits but to items that were regarded as bonds or nonmoney from the perspective of a narrow M1-style definition of money.

[77] Friedman and Schwartz's errors in interpreting Hendry and Ericsson's results were discussed in Ericsson, Hendry, and Prestwich (1998, p. 411). Friedman and Schwartz's (1991) errors in interpreting *Monetary Trends* are as follows: (1) They accepted Hendry and Ericsson's characterization that *Trends* contained no formal statistical tests for constancy of money demand, but *Monetary Trends* did include formal tests of this kind (see Friedman and Schwartz, 1982a, Table 6.5, p. 232). (2) They suggested that Hendry and Ericsson's specification, in which the dependent variable in the short term is nominal rather than real money balances, cannot be viewed as a money demand function. But contrary to what has sometimes implied by contributions from both sides of the monetarist/nonmonetarist debate (for example, by Hetzel, 1984b, and Hendry, 1985, respectively), a nonunitary coefficient on prices in the short-run version of a money demand function can be consistent with standard quantity-theory style propositions in which prices and/or inflation are pinned down by monetary policy. What is required (*inter alia*) is that the long-run money demand function takes a form that is in real terms—a condition that was stressed in Nelson (2020a, Chapter 6). Friedman and Schwartz recognized this point in *Monetary Trends* (see, for example, their p. 254), but their discussion in Friedman and Schwartz (1991) was confused on the matter (with

The debate with Hendry and Ericsson had highlighted the approach to econometrics to which Friedman had subscribed since the 1930s. That approach emphasized "errors in variables" and the possible influence that measurement errors have on regression estimates. It was this perspective that led Friedman to conduct new research in 1991–1992 outside the monetary area. Friedman's jumping-off point was a review in the *Journal of Economic Literature*, which he was still reading regularly, of a book coauthored by William Baumol on international productivity performance.[78] Friedman felt that both the book and its *Journal of Economic Literature* review had taken at face value regression evidence to the effect that an economy grew faster if its initial condition was one of low productivity in relation to that of other countries. Friedman approached the editor of the *Journal of Economic Literature*, John Pencavel of Stanford University, and indicated that he would be interested in publishing a critique of this finding. Pencavel responded positively: "as editor, I gave him space to write this commentary." What was more, because of the proximity of the Hoover Institution and Pencavel's base of Stanford University's department of economics, the editor/author interaction for the article took place largely in person: "he was writing that at the Hoover Institution, and I was just across the road, and we did talk about it [in person] at length."

Pencavel was very happy with the published version of the article: "it's very well-written, like much of his stuff… It's an articulate statement, yet again, of the errors-in-variables problem that he made famous in his *Theory of the Consumption Function*." But the process of the production of the article left a strong impression on Pencavel. A profile of Friedman during this period described Friedman as "hugely energetic despite his years" (*The Independent on Sunday*, July 26, 1992), and Pencavel's own experience with Friedman during the writing of the *Journal of Economic Literature* piece was consistent with that description. Pencavel recalled that "what was most striking about our conversations, was not [just] the amount of economic content, but the manner in which he conducted these conversations. He was as vigorous, as earnest, as involved, as a 30-year-old. He felt that there was a message that needs to be delivered to the economic profession as a whole. He saw it as his task to enlighten people of the errors that he suggested were all too common in this literature. It was really very impressive. He treated me as if I was a fellow advocate of a particular position, and it was really quite charming to see an old man so involved and engaged in issues, and to see him sort of assume that any reasoned person would see things the same way as he. So, it was a relatively small issue [under discussion], but,

conflicting judgments given on pages 41 and 46 of the article). Appropriate treatments of the issue of the short-run specification of the money demand function, which emphasize how prices appear in the function, appeared in White (1978, esp. p. 574) and Goldfeld and Sichel (1987, esp. p. 511).

[78] The review was Williamson (1991). The book under review was Baumol, Blackman, and Wolff (1989).

as I say, the manner in which he got involved in this was really quite remarkable for an old man." (John Pencavel, interview, May 12, 2014.)

In the article that eventually saw print in December 1992, Friedman presented evidence that the regression evidence in favor of the hypothesis of productivity convergence that the authors and reviewers was spurious (even though Friedman conceded that the hypothesis was actually probably correct). The article—his first journal paper to appear since he turned 80 (on July 31, 1992)—allowed Friedman to come full circle by returning to the theme of the "regression effect" that had featured in Harold Hotelling's teaching in the 1930s, in his work with Schultz and Kuznets in the 1930s and 1940s, in (as already noted) his consumption function work in the 1950s, and that had featured, with varying degrees of emphasis, in his work with Anna Schwartz right up to 1991. In addition, the article appeared in print forty years to the month since the final, 1952, Friedman-Savage article, which had also been a critique of work by William Baumol. The *Journal of Economic Literature* article represents a worthwhile self-contained piece of work on Friedman's part—one better than anything else he produced in the 1987–1992 period or later, and a far more effective coda to his body of research than is *Money Mischief*.

The publication of this article and *Money Mischief* did indeed mark a signing-off point on Friedman's part. Up to this point, he had kept his toe in the water as far as research was concerned and had also stayed on top of unfolding monetary events. From now on he would be much more detached on both counts. With regard to research, Robert Gordon's expressed an informed conjecture about Friedman's frame of mind by this point in his life: "I could see him saying, 'O.K., I've had my influence… Why reinvent the wheel?'" With regard to Friedman and the topic of money, Gordon cited the fact that "if you look at what he wrote in the last fifteen or twenty years of his life, it was much more following up on the themes of *Capitalism and Freedom*" and much less the monetary contributions. (Robert Gordon, interview, March 15, 2013.)

The drift away from research had been evident in Friedman's activities in the early 1970s. But his behavior in the early 1990s signified a more decisive break. In late 1991 Friedman indicated that he would not be keeping close touch on monetary developments by giving up, after nearly a quarter-century, his semi-regular briefings to Oppenheimer and Company on monetary developments.[79] He had tried to pull away from the study of money with the release of *Monetary*

[79] His final briefing was in November 1991 (Oppenheimer and Company, 1992; Rudolf Hauser, interview, June 22, 2012).

Trends in 1982, and in that year Friedman had stated: "As the years pass and the time left shrinks, what economists call the marginal utility of time has risen sharply."[80] But the beating that monetarism took in 1982–1986 had brought him back into the fray.[81] In the early 1990s, however, with the disinflation of the previous decade locked in, *Money Mischief* having reached completion, and Friedman's refocus on M2 having put his monetary analysis seemingly back on the rails, he could turn away once again from monetary analysis.

Indeed, despite his continuing strong interest in monetary issues until near the end of those years, the 1987–1992 period would see Friedman shift his focus to topics that he had covered only sporadically prior to the 1980s. He would later summarize his new orientation with the observation that, in the United States, "[o]ur real problems are social" (*Australian Business Monthly*, October 1993, p. 54). It was time to make every moment count and to discuss some key social issues in greater detail than before. Foremost among these was a topic that would put Friedman at odds with both the administrations of both Reagan and Bush.

II. ISSUES, 1987–1992

DRUGS

As was discussed in previous chapters, in the late 1970s and throughout the 1980s the *Wall Street Journal* editorial and op-ed writers were often viscerally critical of Friedman's views on domestic and international monetary arrangements. By the late 1980s, *Journal* items in favor of Friedman's views on monetary matters had become very uncommon—and a good amount of these items consisted of Friedman's own occasional contributions to the *Journal*.

One particularly strong attack on Friedman came in October 1986 from in a "Viewpoint" op-ed by one of the *Wall Street Journal*'s regular writers, Alexander Cockburn. The critique of monetarism that Cockburn laid out was similar to that in many other *Journal* contributions, but the analogy that Cockburn chose for the basis of his critique was unusual. That analogy was brought out in the title of Cockburn's article: "World of Finance Still Hooked on Friedman's

[80] From the preface, dated November 20, 1982, in Friedman (1983a, p. ix).

[81] This participation in monetary discussions included Friedman being an assigned speaker for a session titled "Comments on Macroeconomic Theory," for the Fall Academic Conference of the Federal Reserve Bank of San Francisco, held at the bank in the 12:15 p.m. to 2:00 p.m. slot on November 30, 1989. In light of Friedman's aversion to the term "macroeconomics," it can safely be assumed that the title of the session was not Friedman's choice. The author is grateful to Glenn Rudebusch and the librarians of the research department of the Federal Reserve Bank of San Francisco for information about this session.

Dope" (*Wall Street Journal*, October 2, 1986). One irony of the years that followed was that Friedman experienced a new dimension of his fame, in which it would not be his views on money that were the focus, but his views on a topic that Cockburn had regarded as only figuratively associated with Friedman: drugs. Indeed, at the close of the 1980s, an official 1500-word editorial in the *Wall Street Journal* would contain another condemnation of Friedman's views. This time, monetary topics were not discussed at all. The *Journal*'s critique this time focused solely on the fact that Friedman was one of several public figures who had advocated legalization of narcotic drugs (*Wall Street Journal*, December 29, 1989).

The hostility that Friedman encountered from the *Journal* at 1989's close underscored the fact that the issue of drugs set him apart from many who had been supporters of the Reagan revolution. The coalition that formed the basis for Reagan's support encompassed supporters of free markets and others who held, and emphasized in their policy proposals, a conservative perspective on social issues. To some extent, Friedman took positions that seemed to unite both camps. For example, he would suggest that the growth of the role of government had been detrimental to the stability of the family unit.[82] In this connection, he would allege that the U.S. welfare system provided incentives for single-parent families.[83] And in 1972, he had used his *Newsweek* column to highlight the incentives in the U.S. tax system for himself and his wife to divorce and thereafter still live together (*Newsweek*, April 10, 1972). The column was jokey in tone, but the archaic language that Friedman used for unmarried couples who were sharing a home—"living in sin"—might have led readers to conclude that Friedman was in the camp of social conservatives.

However, over the 1980s it would become clear that this was not the case, as Friedman became outspoken in his disagreements on social issues with many on the Reagan side. An item that was particularly important in bringing out these disagreements out into the open was an article

[82] See, for example, his remarks in *Newsweek,* November 19, 1979, Friedman and Friedman (1980a, p. 33; 1984, p. 136), Evers (1990, p. 75), and Oppenheimer and Company (1992, p. 3).

[83] See, for example, *Playboy* (February 1973, as reprinted in Friedman, 1975a, p. 28) as well as Friedman's remarks in Friedman and Tobin (1990, p. 78) and in CSPAN (May 7, 1993). The last of these remarks was made to Republican legislators, and the notion that government welfare and transfer arrangements discourage two-parent family arrangements (and may encourage one-parent arrangements) has been traditionally associated with right-of-center political parties. However, among economists, with their emphasis on the private sector's response to incentives, the notion has enjoyed fairly wide support, including with economists associated with Democratic administrations and campaigns. James Tobin, for example, wrote that it "is almost as if our present programs of public assistance had been consciously contrived to perpetuate the conditions they are supposed to alleviate," that the programs' incentives led many to be "essentially forced to be both idle and on the dole," and that "[p]ublic assistance also encourages the disintegration of the family." (Tobin, 1965, p. 890. Tobin, 1965, p. 891, went on to list one advantage of his own income-support proposals as that it "should provide some disincentive to the creation of large families.")

published in the Summer 1984 issue of the right-of-center magazine *Policy Review*. The article, which consisted of a symposium on social issues, drew out the views of many Reagan supporters, Friedman among them. In addition to reaffirming that he did not accept the label "conservative" as a description of himself, Friedman set himself apart from the majority of those in the forum in his answers to the symposium's questions. He indicated that he was pro-choice with regard to abortion.[84] He opposed discrimination against gays. And he stated that he did not have enough evidence to be able to ascertain whether the rise in the U.S. divorce rate had been a good or bad thing.[85]

These positions paralleled the parting of company that Friedman had already made between himself and many conservatives in the 1960s when the conscription issue had been so prominent. But, at least on domestic policy, the greatest rift that Friedman would create between himself and other conservatives, and also with mainstream public opinion, was via his advocacy of drug legalization.

By the mid-1980s, Friedman's support for drug legalization had been on the record for years. It took considerable time, however, for this to become a really well-known Friedman position. That support for legalization of transactions in narcotic drugs might follow from Friedman's economic liberalism was conjectured by Daniel S. Ahearn in, of all places, a 1963 book on monetary policy. In critiquing Friedman's opposition to direct controls on credit, Ahearn (1963, pp. 194–195) stated: "Friedman's rejection of interference with individual contracts which are regarded as mutually beneficial cannot be regarded as justification for the rejection of selective credit control. It is too sweeping. A moment's reflection is enough to show that this sort of argument would reject interference with a 'pusher's' sale of narcotics to an addict; both presumably would be satisfied by the transaction, others would not be harmed, and hence there would be no grounds, in Friedman's view, for interference by society." Ahearn's discussion thus used advocacy of drug legalization as a *reductio ad absurdum*, and not as a position that Friedman actually endorsed. And, at that time, Ahearn's judgment that Friedman would not

[84] In addition, both Friedman and his wife indicated their support for the pro-choice position in an interview later in 1984 (*California* magazine, October 1984). In the course of that interview, Friedman specifically criticized Reagan's stand on the abortion issue. More oblique support for the pro-choice position may be discerned from Friedman's 1989 statement that "government has no business meddling in our family planning" (*National Review*, June 16, 1989). Prior to the Reagan years, Friedman had indicated his support for the pro-choice position when discussing the abortion issue with Arthur Laffer during Laffer's years at the University of Chicago. (Arthur Laffer, interview, June 10, 2013.)

[85] In contrast, a brief discussion in Friedman and Friedman (1985, p. 131) invited the interpretation that rising divorce rates had had a detrimental effect on U.S. society. This passage may have reflected Rose Friedman's influence on the jointly-written book.

actually endorse drug legalization may well have been correct.[86]

The early occasions on which Friedman made interventions on the role of government with regard to drugs pertained not to narcotics but to cigarettes, whose consumption became an issue of national public policy with the 1964 U.S. surgeon general's report. Friedman's scattered discussions of smoking did not give many pointers toward his subsequent discussions of narcotic drugs. No doubt a reason for this was that the circumstances were different: nicotine cigarettes were a legal drug whose consumption the government was discouraging but not prohibiting.

All the same, Friedman's statements about the government's efforts to discourage cigarette usage were decidedly muddled. This surely partly reflected the fact that his own instincts were in conflict. He was a former smoker who considered smokers "fools" in light of the evidence of the harm from smoking.[87] He himself had suggested in print as early as 1951 (All Participants, 1951, p. 251) that cigarettes were dangerous, and he gave up smoking around 1957 (*Newsweek*, June 16, 1969). This decision, he indicated, was on the basis of the already-strong evidence of the harm from smoking.[88] Yet his inclination against government actions that he regarded as condescending led him to criticize the mandatory inclusion of a government health warning on cigarette packages (*Newsweek*, June 16, 1969). He reaffirmed this criticism in 1979.[89] Indeed, in that year Friedman even took his stand to the point, in debate with Ralph Nader, of claiming that the health warning was an obstacle to successful legal action (by smokers or former smokers) against cigarette companies. This was another case of Friedman overemphasizing legal action as an option available to consumers.[90] Nader surely had the better side of the argument when he riposted with the observation that lawsuits were a trivial part of the process of discouraging smoking, and that deterring smoking through the official health warning was much more effective.[91]

[86] In an article that appeared after the first draft of this chapter was written and circulated, M. Thornton (2016) offered the opposite view, by suggesting that Friedman's opposition to drug prohibition might have dated back to his youth. However, this is a suggestion that is a *premise* of Thornton's argument, rather than one that he documents using evidence in the form of contemporaneous Friedman statements. Indeed, the earliest Friedman statement advocating drug legalization cited by Thornton—the 1972 *Newsweek* column—is more than two years later than the earliest such statement provided in the present chapter's analysis. There is considerable basis for doubting that Prohibition made a large impression on Friedman's life at the time, and on his thinking about drugs in his younger years.

[87] *Milton Friedman Speaks*, Episode 3, "Is Capitalism Humane?," September 27, 1977, p. 34 of transcript. Similarly, Friedman (1979, p. 14 of 1987 reprint) expressed the judgment that "it's terrible to smoke cigarettes."

[88] *Milton Friedman Speaks*, Episode 12, "Who Protects the Consumer?," September 12, 1977, p. 32 of transcript.

[89] See Friedman (1979, pp. 13–14, 17 of 1987 reprint).

[90] For further such cases, see the discussion in Nelson (2020b, Chapter 14).

[91] For Friedman's remark see Proprietary Association (1979, p. 32). Nader's rebuttal to Friedman's remark appeared on page 35.

Friedman's interventions on the smoking debate were therefore not merely scattered; they were scattershot. And, because the issue of passive smoking was not as prominent in the 1960s and 1970s as it later became, Friedman did not recognize an important externality of smoking. In addition, for all his criticisms of U.S. public policy regarding smoking, that policy embodied several features that Friedman would subsequently, in his discussion of narcotic drugs, accept as appropriate: illegality of minors' access, heavy taxes on consumption, and public-awareness campaigns regarding the addictive nature of the drug and its danger to health. Indeed, the subsequent debate on narcotic drugs would lead Friedman to concede that, notwithstanding his libertarian instincts, there was a case for advertising restrictions, which had also been a key aspect of government policy in the case of both cigarettes and alcohol.[92]

The debate proper on narcotic drugs began, as far as Friedman was concerned, in the 1970s. An early occasion on which Friedman indicated that he believed drugs should be made legal was a lecture he gave at Florida Presbyterian College on February 19, 1970.[93] The student revolution had brought prominence to illegal drug use, and Friedman's reaction was that

> there is no doubt in my mind that the use of drugs is a highly undesirable activity, that people who have the intention of reducing the extent of the use of marijuana, LSD, heroin, and all the rest are on the side of the angels. But what is the device by which they are to do it? The same device that failed miserably with Prohibition, with alcohol, and it is having the same results. It is reducing on a widespread scale the respect for the law, and it is not preventing the use of drugs… We would achieve our objectives far better by eliminating these prohibitory laws than by seeking to enforce them.[94]

[92] Friedman offered qualified praise for restrictions on advertising drugs in the question-and-answer portion of his presentation in Krauss and Lazear (1991). On that occasion, he expressed concern that advertising could glamorize drug use. On an earlier occasion, he had indicated that a case for advertising restrictions lay in the fact that the mass public should not have to receive information on purchasing drugs. Instead, he suggested that prospective drug consumers would have to seek out that information (*This Week With David Brinkley*, ABC, December 17, 1989, p. 5 of transcript). These remarks indicated some support for U.S. government policy on cigarette usage. Nonetheless, Friedman's conflicting feelings toward government policy concerning smoking made themselves felt in the fact that even in the late 1990s he supplied an endorsement for a smokers'-rights book (Sullum, 1998).

[93] In a 1991 radio interview, Friedman would say he could not remember a time when he was not in favor of drug legalization. ("America's Drug Forum," no. 223, 1991, as transcribed in Trebach and Zeese, 1992, pp. 67–68). But as it is clear that Friedman's memory, already very imperfect by the 1970s, had deteriorated badly by the 1990s, this statement has to be taken with a grain of salt.

A couple of early references in the public record by Friedman to drugs did not bear directly on the legalization issue. In radio appearances in the late 1940s and early 1950s, he had referred to the dangers of drug addiction (NBC, 1947, p. 4; NBC, 1950, p. 2). These statements by themselves do not establish a difference from Friedman's later position, as he continued to disapprove of drugs in his years as a well-known legalization advocate.

[94] Friedman (1970a, p. 7).

Friedman briefly alluded to this argument in his 1971 critique of the morality of wage and price controls when he stated that the evasion, corruption, and disdain for the law were promoted by measures such as Prohibition, World War II's price controls and rationing, and again "today with drug laws." (*New York Times*, October 29, 1971.)

In mid-1972, Friedman took up the issue in a *Newsweek* column ("Prohibition and Drugs," in the May 1, 1972, edition) in which he made many of the points he had made in his 1970 and 1971 discussions.[95] He indicated that while he opposed drug use, he did not want government efforts to prohibit it; and even though "men of goodwill may well disagree" on whether government had a legitimate role in preventing addition, forbidding drug use was bound to be a "hopeless" endeavor. He added that the fact that drugs were only available at considerable expense in the underground economy was boosting crime rates.

As has already been indicated, Friedman's opposition to drug prohibition did not arise from any misapprehension that drugs were not harmful. In the column itself, this was clear enough from the fact that it made a reference—which was rare in Friedman's writings, and uncharacteristic in view of his lack of religious beliefs—to prayer, with Friedman suggesting that praying for and with a drug addict was an appropriate reaction for someone close to the addict.

The parallels that the column repeatedly invoked between drug addicts and alcoholics, and between the Prohibition era and drug criminalization, also testified to Friedman's belief in the harm of drug addiction. The fact was that, as was mentioned in Chapter 3, his professional experiences had made him all too aware of the seriousness of alcoholism. Friedman himself was a notably light drinker, but the same was not universally true of his fellow professors of economics at the University of Chicago. In particular, the tumultuous period in the late 1960s and early 1970s of heavy drug use on U.S. campuses had an additional characteristic in the economics world of the University of Chicago. Claudia Goldin, who was a graduate student in the economics department in that era, recalled: "The students were into drugs, and the faculty were into liquor." If anything, Goldin observed, the alcohol problem of the academic staff was more severe than the drug problem of the students. (Claudia Goldin, interview, September 20, 2013.) Stanley Fischer got this impression, too, when in the late 1960s he went from being an MIT graduate student to being a teacher at the University of Chicago. Assigned the office of a senior department member who spent half of each year at an overseas university, Fischer found

[95] Shortly ahead of the column, Friedman discussed his preference for drug legalization in Instructional Dynamics Economics Cassette Tape 95 (March 22, 1972).

that the eminent professor's desk drawers were full of bottles of whiskey (Stanley Fischer, personal communication, May 8, 2015).

Friedman watched at close hand the pattern of living hard and working hard, in which highly productive colleagues could also be extremely self-destructive in their degree of alcohol consumption. It was a pattern to which he alluded on television in 1975 when he confirmed that there had certainly been times when he had encountered "one of those unfortunate creatures who have been an alcoholic."[96] "Drinking was absolutely not a part of his life whatsoever," Robert Gordon recalled (interview, March 21, 2013). "He was even disdainful of the whole aspect of it, probably because he had seen so much damage that it had done at Chicago."

Friedman's 1972 column on drugs was quoted prominently on the back cover of his *An Economist's Protest* collection of *Newsweek* pieces, released later in the year, and during the late 1970s Friedman's secretary Gloria Valentine sent a copy of the column to a member of the public who wrote to Friedman labeling him a conservative. Nonetheless, as Valentine observed, Friedman articulated his position on drugs "long before people seemed to notice," and his stance on drugs did not become very widely known during the 1970s (Gloria Valentine, interview, April 1, 2013).

In addition, when the Friedmans and Robert Chitester decided on topics to cover in the *Free To Choose* television series, they opted not to include the issue of narcotics on the grounds that inclusion would divert attention from Friedman's other proposals to reduce government involvement. Chitester noted: "We wanted to focus mainly on economic freedom and touch on the fact that economic freedom was critical to the other types of freedom, and we did so." (Robert Chitester, interview, July 9, 2013.)[97] As a result of this choice of coverage, and of similar choices that Friedman made on other occasions, Friedman's support for drug legalization figured only sporadically in the public record in the decade after the appearance of his *Newsweek* column on the matter.[98]

[96] *Monday Conference*, Australian Broadcasting Commission, April 14, 1975, p. 4 of transcript.

[97] A passage of the *Free To Choose* book was excerpted in the 1992 collection *Friedman and Szasz on Liberty and Drugs* that is discussed below. However, that passage dealt with pharmaceutical drugs (for which the Friedmans advocated greater and easier consumer access), not narcotics. The book did fleetingly mention "forbidding the purchase and sale of heroin" as a command element of many economies (Friedman and Friedman, 1980, p. 11).

[98] Examples in which Friedman in the years 1973–1982 reaffirmed his support for narcotic drug legalization included the question-and-answer portion of *Milton Friedman Speaks*, Episode 12, "Who Protects the Consumer?," September 12, 1977 (p. 32 of transcript), his *Donahue* appearance in April 1980 (NBC, April 16, 1980—an occasion on which Donahue noted that drug policy was an area in which Friedman differed from presidential candidate Ronald Reagan), and his talk at the Federal Reserve Bank of Dallas conference on supply-side economics in May 1982, in which Friedman's talk included the digression: "Incidentally, one way to reduce the [federal budget] deficit

The Reagan administration's "war on drugs," spearheaded by First Lady Nancy Reagan, prompted the Friedmans to restate the legalization line at the end of the chapter on crime in their book version of *Tyranny of the Status Quo* in 1984.[99] Although the 1972 *Newsweek* column was not cited, the Friedmans' 1984 discussion was essentially a rewrite and expansion of the *Newsweek* column, with much of the 1972 column reappearing verbatim. In *Time* magazine in 1988, Friedman updated his argument in recognition of a drug that had emerged as a major problem during the Reagan years: "The harm that is done by drugs is predominantly caused by the fact that they are illegal. You would not have had the crack epidemic if it [crack cocaine] was legal." (*Time*, May 30, 1988.) Later in the year, Friedman laid out his advocacy of drug legalization in an article in *Reason* magazine (October 1988).

It was, however, in President Bush's first year in office that Friedman finally secured sustained attention for his advocacy of legalization. William Bennett, whom Friedman knew, had been put in charge of the new administration's anti-drug campaign, and Friedman used a *Wall Street Journal* op-ed (September 7, 1989) to challenge the administration's drug policy.[100] As was so frequently the case by this point, Friedman's new contribution proved not to be an entirely new piece of writing. His op-ed contained a large excerpt from his 1972 column, albeit this time explicitly indicated as being a reprinting. The new part of Friedman's op-ed was far from a stellar writing effort: it was written in an irritating "open letter" format, included Friedman's cloying statement that the letter expressed feelings "from the bottom of my heart," and verged on self-congratulation in implying that both he and Bennett qualified for the label "friend of freedom." But the op-ed was an undoubted success in generating public interest. It generated a public reply from Bennett (*Wall Street Journal,* September 19, 1989), and a rebuttal from Friedman, which was considerably better written than his initial contribution to the exchange (*Wall Street Journal,* September 29, 1989).

Even more important, in the wake of the op-ed, Friedman's position on drugs gained wide coverage outside the pages of the *Journal*. After two decades—"the many years I've been sort of peripherally engaged in discussing the issue of drugs" (CSPAN, November 16, 1991)—his

would be to legalize marijuana and tax it." (Friedman, 1982a, p. 58. This aside also provides an example of Friedman's tendency, noted in the previous chapter, to acknowledge that policy-induced tax increases might well have a role in deficit reduction, notwithstanding his emphasis on spending control as the principal method to be used for that purpose.)

[99] Friedman and Friedman (1984, pp. 137–141; 1985, pp. 132–136).

[100] A little ahead of putting out this op-ed, Friedman had outlined the legalization case again while answering questions in an appearance at the Commonwealth Club of California in San Francisco (Friedman, 1989a, p. 369).

stand on the issue finally made a major, sustained impression on media coverage of the drug issue, and from 1989 onward he was interviewed on multiple occasions on the topic.
One of the earliest of the media appearances that Friedman would give specifically on the topic of drugs was as a studio guest on ABC's Sunday morning news discussion program *This Week With David Brinkley* on December 17, 1989. In this appearance, Friedman said: "Drugs are a terrible thing. I'm not defending drugs; heavens, no. What I'm saying is that we have a better chance to control them if we do it in an intelligent, sensible way. You make it a medical problem and not a criminal problem."[101]

In further rounds of interviews, in conferences on drug policy, and in additional op-ed contributions during the 1990s and the 2000s, Friedman would become closely identified with the advocacy of drug legalization.[102] He was, for example, a speaker at a November 1991 conference held by the Drug Policy Foundation. At this event, which occasioned a now-rare visit by Friedman to Washington, D.C., he stated: "There's overwhelming evidence the war on drugs is doing more harm than good." (*Washington Times*, November 17, 1991.) The Drug Policy Foundation Press would publish in 1992 a book titled *Friedman and Szasz on Liberty and Drugs*, containing some of Friedman's public statements on drug legalization (see Trebach and Zeese, 1992).

In the same year, Friedman's name would come up in the first presidential debate (October 11, 1992) between President Bush, Governor Bill Clinton, and Ross Perot, when a questioner asked the president: "And are you at all of a mind that maybe it ought to go to another level, if not to what's advocated by William F. Buckley, Jr. and Milton Friedman, legalization, somewhere between there and where we are now?" Bush replied: "No, I don't think that's the right answer. I don't believe legalizing narcotics is the answer. I just don't believe that's the answer."

However, when, less than a year before Bush's remarks, Friedman reflected on the issue, he expressed satisfaction that his position on drugs was finally becoming respectable and that the mail he was receiving in reaction to his views on legalization was mostly supportive (Oppenheimer and Company, 1992, pp. 8–9).

In the course of his activism on the drug issue during the early 1990s, Friedman tried to add somewhat to his 1972 case for legalization with statistics. For example, he pointed to the

[101] *This Week With David Brinkley*, ABC, December 17, 1989, p. 5 of transcript.
[102] Anna Schwartz would, owing to the association of her name with Friedman's, be bemused to receive a considerable amount of literature from drug-legalization groups in her office mail over the years.

increases in U.S. crime rates and prison population that had occurred in the 1970s and 1980s, decades in which successive U.S. presidents had pursued anti-drug campaigns (*Wall Street Journal,* March 7, 1991; Krauss and Lazear, 1991, pp. 53−67). But these pieces of evidence were largely back-of-the-envelope-style calculations. Even with these contributions to his name, Friedman could not truly be considered an economist carrying out detailed, journal-standard empirical work on the issue of drugs.[103] He described himself as having read a considerable literature on drugs but described his activity on the subject as an "avocation," that is, a hobby, rather than "a vocation" (CSPAN, November 16, 1991). Friedman left economic research on the matter to researchers such as Jeffrey Miron, with whom Friedman was sometimes in touch on the drugs issue.[104] Like Friedman, Miron was working in both the fields of monetary history and drug policy, but in 1987−1992 Miron was doing much more fundamental research on *both* issues than Friedman could claim to be doing by this point.[105] Indeed, Friedman conceded that his own case for legalization was not putting its main emphasis on the economic aspects of the argument. "I'm an economist," he said in a radio appearance in 1991, "but the economics problem is strictly tertiary. It's a moral problem. It's a problem of the harm which government is doing."[106]

In its editorial condemning Friedman's position on drugs, the *Wall Street Journal* had suggested that he should celebrate the start of the 1990s not by drinking champagne but by taking hard drugs (*Wall Street Journal*, December 29, 1989). Although this observation was made in jest, Friedman would in time feel the need, when reiterating his case for legalization, to clarify not only that he disapproved of drugs but also that he had never taken them. His perspective, he said in a 1996 speech, implied that the law should not prohibit a person from electing to "ingest cocaine, which I have never done, as it happens." (CSPAN, December 26, 1996.) Friedman looked upon other drug-taking from the same perspective, as he had stressed in an appearance before Republican members of Congress in May 1993. "I've never personally imbibed any drug

[103] Friedman did advocate drug legalization in an economics journal, albeit in the letters section of the often-nontechnical *Journal of Economic Perspectives* (Friedman, 1997b). In this piece, as in Friedman and Friedman (1985, p. 132), he segued from a discussion of crime to a consideration of drugs by stating that one way of reducing crime was to reduce the number of activities that were illegal.

[104] Miron recalled of his encounters with Friedman: "In the small number of times that I interacted with him sort of one-on-one, he was just sort of genuinely charming and friendly. He just was a very warm human being. And I think that must have been part of his success, because even the people who disagreed with him were, I think, struck by the fact of his innate decency and warmth. And so it tempered any disagreements you might have with his policy views… I mean, he was certainly tough. He never gave any quarter. But he somehow always did it in a calm and in a professional way. He never seemed to lose his cool." (Jeffrey Miron, interview, June 20, 2013.) Consistent with this impression, a journalist who interviewed Friedman during the period covered in this chapter observed: "Despite his reputation for skewering opponents in debate, Professor Friedman is courteous, chatty, patient, and unhurried on the phone." (*Independent on Sunday* (London), July 26, 1992.)

[105] Miron's research on drugs includes, for example, Miron and Zwiebel (1995). Friedman penned an endorsement and introduction to a later Miron study (see Friedman, 2005a, and Miron, 2004).

[106] "America's Drug Forum," No. 223, 1991, as transcribed in Trebach and Zeese (1992, p. 71).

whatsoever, any of these drugs whatsoever," he had told that audience. "So I'm not speaking from personal experience." (CSPAN, May 7, 1993.)

INSIDER TRADING

Taking questions after a talk in February 1977, Friedman said to an audience member, "[Your] question is: What is the future of the prosecution of white-collar crime? Well, I have answers for most questions, but not for that one."[107] In the late 1980s, however, with insider-trading activities on Wall Street hitting the headlines, Friedman would become outspoken about this aspect of white-collar crime.

A *Newsday* profile of the Chicago School that appeared on October 19, 1986, used Friedman's name the lead off the article but proceeded to discuss the interests of the current members of the University of Chicago's economics, business, and law arms. In this connection, it noted that "several Chicago School people have been proposing the elimination of rules that prevent corporate executives from using inside information in their decisions concerning purchase of their firms' stock, claiming that insider trading ultimately makes the market more efficient."

By the time insider trading became a major news topic in the 1980s, one of the principal individuals located at the University of Chicago making an argument of this kind was Daniel Fischel of the law school. Fischel's most well-known research on the issue (with Dennis Carlton of the university's business school) had appeared in a law journal and came out long after Friedman had left the University of Chicago.[108] Friedman nevertheless was familiar with the basic case for making insider trading legal, owing to the fact that the case had been made some two decades earlier in the University of Chicago's principal economics journal, the *Journal of Political Economy*, by a friend of his.

Henry Manne, whose law degree was from the University of Chicago and who had had Friedman's brother-in-law Aaron Director among his teachers, published an article, "Mergers and the Market for Corporate Control," in the April 1965 issue of the *Journal of Political Economy* (Manne, 1965). In the article, Manne expressed the following judgment: "Insiders, those who have the most reliable information about corporate affairs, are strongly motived financially to perform a kind of arbitrage function for their company's stock. That is, given their

[107] Friedman (1977c, p. 14).

[108] Carlton and Fischel (1983). Fischel was interviewed for a *Newsday* article on insider trading that had appeared earlier in 1986 (*Newsday*, June 15, 1986, pages 75 and 80.)

sense of what constitutes efficient management, they will cause share prices to rise or decline in accordance with that standard." Manne subsequently elaborated upon this argument in a book, *Insider Trading and the Stock Market* (Manne, 1966).[109] On top of these contributions was Manne's bluntly-titled article, "What's So Bad About Insider Trading?," which appeared in an issue of the economics magazine *Challenge* in early 1967 (Manne, 1967).[110]

Manne had numerous interactions with Friedman, some of which were recounted earlier in this book. In the course of the various exchanges, Manne received "a whole lot" of feedback from Friedman on the former's position concerning insider trading. Most notably, in "one of the highlights of my career, he said he agreed with everything I said." (Henry Manne, interview, April 30, 2014.) Consistent with this position, Friedman made a public intervention on the issue in 1973 when he included new anti-insider-trading measures in a list of recently-imposed undesirable curbs on firms' behavior (Instructional Dynamics Economic Cassette Tape 122, June 11, 1973).

"I am not an expert on commercial law," Friedman had acknowledged in 1976.[111] This fact presumably had underlay his noncommittal reply in the question-and-answer session in 1977 quoted above. The topic of insider trading was far from his main area of expertise. It was a topic on which experts on commercial law or on the economics of the corporation were far better qualified to speak than he was. Nevertheless, with insider trading becoming major news in the 1980s, and with both Friedman and his interviewers considering any matter related to economics an appropriate topic for discussion, it was inevitable that Friedman would join the public debate on inside trading. When he did so, he essentially restated the argument that Manne had made.[112] A (hostile) *Financial Times* piece in 1988 alluded to Friedman's public interventions on insider trading when it referred to "Milton Friedman, free-market fanatic and a man who believes that insider trading can make markets work better" (*Financial Times* (London), October 8, 1988).

By this time, the high-profile insider trading cases in London and New York had produced an

[109] Gordon Tullock (1975) cited Manne's 1966 book in the conference volume in Friedman's honor, although not in the context of a discussion of insider trading.
[110] Manne continued until his death to be a prominent critic of insider trading laws (including in *Wall Street Journal*, April 29, 2014).
[111] January 22, 1976, testimony, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1976a, p. 2187).
[112] Manne observed that insider trading was "a fairly esoteric topic, given all the topics in the world of economics. But he did, occasionally, discuss it [publicly] and drew heavily on my argument." (Henry Manne, interview, April 30, 2014.)

upsurge of economic research on the topic.[113] The basic messages from the sum total of that research would seem to be that (*i*) there was support for the notion—advanced by Manne and endorsed by Friedman—that insider trading could enhance the amount of information contained in traded equity prices, but that (*ii*) appreciable economic costs of insider trading also had to be taken into consideration. When this literature is viewed from the standpoint of monetary economics, perhaps the most notable item in the literature was a 1988 article coauthored by Mervyn King. King would become immersed in monetary policy issues as a senior Bank of England official from 1991 onward—and in that capacity would get to know Friedman—but in the late 1980s he was still at the London School of Economics and a recognized expert on the economics of the corporation. M. King and Roell (1988, p. 171) cited Manne (1966) as a key reference on insider trading but judged that "it is doubtful whether many would take the sanguine view" embodied in Manne's thesis. King and Roell conceded that one effect of insider trading was to increase the information content of stock prices, but they also highlighted costs in the form of higher bid-ask spreads induced by investor concern that they were trading with insiders. King and Roell's (1988, p. 187) judgment was that it was indeed to err "on the side of strict controls on insider trading."[114]

For his part, Friedman, who was likely not keeping tabs on recent research on the area, continued to voice support for legalization: "so-called insider trading should not be a crime," he said in 1992, adding—with a lapse into amateur-lawyer mode—"they've never been able to properly define insider trading" (*Frisko*, 1992, p. 68).[115] Friedman's profile as an advocate of legalization of insider trading was augmented when remarks he made in a 2003 appearance on CNBC became widely quoted on the internet. Friedman said, *inter alia*: "You want more insider trading, not less. You want to give the people most likely to have knowledge about deficiencies

[113] No survey is attempted here, but papers on the topic beyond the M. King and Roell (1988) study discussed presently include Demsetz (1986), Manove (1989), Ausubel (1990), Dennert (1991), Fishman and Hagerty (1992), and Leland (1992).

Another topic associated in the public mind with the insider-trading affairs of the 1980s consisted of the activities of (along with the subsequent prosecution and conviction of) securities trader Michael Milken. Friedman was outspoken in suggesting that Milken should not have been prosecuted (see, for example, *Frisko*, 1992, p. 68). Ben Stein recalled: "I wrote a great deal about financial fraud for *Barron's* magazine, which was then a much, much bigger magazine than it is now. And my main area was exposing management buyouts as ethically impossible, and also exposing the Michael Milken junk-bond fraud. I did an awful lot of work on that. And Friedman and I, at one point, had a conversation about that, and he very much disagreed with me. He thought that Milken had done a useful service." Stein made the case against Milken to Friedman. "I don't think I persuaded him of it, but I think he sort of got out of the habit of apologizing for Milken after that." (Ben Stein, interview, March 18, 2015.)

[114] However, one of King and Roell's discussants, Charles Wyplosz, expressed dissent from their conclusion and had supportive words for the Manne position (see Wyplosz, 1988).

[115] Claims of this kind were disputed by regulators. For example, in 1986, Ira Sorkin, then the director of the New York office of the Securities Exchange Commission, stated, "The SEC is not creating the law on insider trading. The courts have defined it." (Quoted in *Newsday*, June 15, 1986.)

of the company an incentive to make the public aware of that." (CNBC, March 12, 2003.) In these remarks, Friedman was referring to what the research literature on insider dealing had long recognized as a beneficial factor that was in operation—but one that the bulk of that literature had concluded was outweighed by the costs of insider trading. Another point that has been raised by supporters of restrictions on insider trading—a point buttressed more recently by the empirical work of Levine, Lin, and Wei (2015)—is that the disclosure requirements associated with insider-trading laws themselves boost the efficiency of financial markets by adding to the information available to all investors.

In closing, it should be stressed that, despite his opposition to insider-trading laws, Friedman recognized that they, like laws on drugs, should be obeyed by all citizens, including those who were critical of the laws in question. True, he voiced the position that citizens were more likely to obey laws with which they agreed and that the criminalization of activities to which there was not was likely to undermine the respect for the legal framework as a whole.[116] But he maintained that citizens should be law-abiding. "There is a big difference between our role as citizens in which we try to affect public policy and our obeying the law after it is passed," he had observed in 1980 (*The Register*, January 11, 1980), and some years earlier he had listed the responsibilities of corporate executives as including that they "stay within the law."[117]

III. PERSONALITIES, 1987–1992

ALAN GREENSPAN

In 1983, Friedman had acknowledged that Paul Volcker's Federal Reserve had "brought inflation down."[118] Inflation had stayed down over 1983 at what was, Friedman conceded, a relatively low level. At that stage, however, Friedman played down the achievement by complaining about what he perceived as an unnecessarily erratic path of monetary growth and foreshadowing a prompt revival of inflation.[119]

By early 1987, Friedman had accepted that Paul Volcker's tenure had seen inflation come down permanently. But he continued to find it hard to praise Volcker for the latter's stewardship of

[116] *Milton Friedman Speaks*, Episode 3, "Is Capitalism Humane?," September 27, 1977, p. 13 of transcript; Friedman and Friedman (1980a, p. 145; 1985, p. 131). See also Friedman's 1970 remarks given in Florida quoted earlier in this section.
[117] *Business and Society Review* (Spring 1972, p. 6; also excerpted in Friedman, 1975a, p. 240).
[118] Friedman (1984i, p. 41).
[119] See Friedman (1984a, p. 3) and the discussion in the previous chapter.

monetary policy in general and for the achievement of disinflation in particular. As already discussed, Friedman emphasized the support that President Reagan provided for monetary restriction in the 1981–1982 period as the crucial foundation of the early 1980s disinflation. Friedman took this line in spite of the fact that the slowdown in both money and nominal spending actually started before Reagan's inauguration in 1981. In addition, Friedman continued to point to the swings in monetary growth and other variables since 1979, especially that associated with the Federal Reserve's nonborrowed-reserves procedure of the 1979–1982 period, as evidence that the disinflationary policy had been implemented in a disorderly and unnecessarily costly way. Thus in a *Financial Times* interview in 1987 Friedman stated that Volcker took a "seat of the pants" approach to policy and claimed of the 1979–1982 episode: "If somebody had wanted deliberately to discredit monetarism, they would have done what Volcker did." (*Financial Times* (London), February 23, 1987.)[120]

Friedman may have subsequently tempered his critical attitude toward Volcker. John Taylor recalled: "I really never heard him criticize Volcker very much in the years I engaged with him… So I did not think of him as a critic of Volcker." (John Taylor, interview, July 2, 2013.) Nevertheless, when Taylor interviewed him in 2000, Friedman continued to suggest that Reagan rather than Volcker was the principal figure behind the early-1980s U.S. disinflation, and he voiced this perspective also in other retrospectives on the disinflation.[121] Friedman's advocacy of this argument sat awkwardly alongside some of his previous statements. For example, in late 1985—a time when he was unconvinced that inflation had really been beaten—Friedman observed that "you can't call monetary policy Reagan's policy; you have to call it Volcker's policy." (*The Margin*, January 1986, p. 5.)

Something of a reconciliation between Friedman's Reagan-focused interpretation of the disinflation and the Volcker-focused accounts advanced by other commentators lies in the fact that 1981–1982 corresponded to a period in which a different U.S. president might have put pressure on the Federal Reserve to ease monetary policy. This was a point that Friedman emphasized in his interview with Taylor.[122] It is notable that Lyle Gramley, who overlapped with Volcker as a member of the Board of Governors from 1980 to 1985, largely confirmed this

[120] This attitude to Volcker had already characterized Friedman's title for his 1985 paper (Friedman, 1985b). In addition, Friedman had used "seat of the pants" to summarize post-1982 policy on another occasion—in July 1986 (see Darby and others, 1987, p. 12)—although his 1986 discussion suggested that Volcker's predecessors had also followed a seat-of-the-pants approach.

[121] See the items cited in Section I above, as well as Friedman's remarks in *Wall Street Journal*, January 31, 2006, and in *San Jose Mercury News*, November 5, 2006*a*.

[122] See Taylor (2001, p. 107). See also Chapter 12 above.

interpretation of the 1981–1982 period. "What I heard Paul say repeatedly is that if the president, President Reagan, hadn't been willing to let the Fed do what it did, then there would have been no possibility of continuing on that course of action." (Lyle Gramley, interview, June 24, 2013.)

When in mid-1987 it was announced that Volcker was departing the post of Federal Reserve Chairman, Friedman displayed a touch of magnanimity, stating that "Paul Volcker is a fine and able person." But in comparing Volcker to Volcker's approved successor Alan Greenspan, Friedman made no bones about the fact that "I prefer Alan to Paul Volcker for a variety of reasons," and he declared Greenspan "a splendid choice and a fine person."[123] Indeed, as mentioned in the previous chapter, Friedman had suggested publicly as early as 1981 that Greenspan would be a good candidate for President Reagan to consider as a future Federal Reserve chair.

Friedman had only rarely mentioned Greenspan in his writings.[124] But the two were old friends. Indeed, in the period leading up to Greenspan's assumption of the post of Federal Reserve Chairman, Friedman referred to Greenspan as "one of my best friends."[125] For his part, Greenspan would recall of Friedman: "I met him through correspondence on an article I had written in 1959 for the American Statistical Association. I sent him a copy of it, never expecting to get a response. But he sent me back a long letter, which I thought was very thoughtful; I was at that point young and largely unknown. I met him several times thereafter, but it wasn't until the mid- to late 1960s that I began to see a great deal of him, largely at meetings of economic policy groups for Presidents Nixon and Reagan." (Alan Greenspan, interview, August 19, 2013.)[126] In addition to numbering among the economists whom Nixon and his official advisers saw, Friedman and Greenspan were also involved in the Nixon Administration as part of the commission urging the end of the draft.[127]

[123] Friedman (1987b, p. 365).
[124] Exceptions include Friedman's remarks in Friedman and Kristol (1976, p. 41), *Newsweek*, October 16, 1978, and Friedman (1986d, p. 8). Friedman referred to Greenspan numerous times on his cassette commentary series, both before and after Greenspan became Chairman of the Council of Economic Advisers in 1974. Other than those given below, examples of these mentions included those in Instructional Dynamics Economic Cassette Tapes 97 (May 3, 1972), 121 (May 24, 1973), 150 (July 24, 1974), and 201 (October 1976, Part 2).
[125] Friedman (1987b, p. 365).
[126] The Greenspan article in question was Greenspan (1959). Greenspan also recalled his early correspondence with Friedman in Greenspan (2004).
[127] See, for example, Friedman (1986d, p. 8) as well as Nelson (2020b, Chapter 14) and Chapter 5 above. The meeting of the Commission for an All-Volunteer Force on February 21, 1970, was the only occasion on which Friedman and Greenspan visited the Nixon White House on the same day. (Information from Nixon Presidential Library, May 23, 2014.)

Greenspan and Friedman also had considerable agreement when it came to market economics. For example, in common with Friedman, Greenspan believed that monopolies were unlikely to survive in a market economy unless they were supported by the state (*Detroit Free Press*, July 29, 1974). Greenspan was also a critic of the use of incomes policy to interfere in the private sector's wage- and price-setting decisions.[128] It is true that Greenspan's free-market views owed much to Ayn Rand, and that Greenspan cited Rand's work but not Friedman's in Greenspan (1971), for example. Rand disliked Friedman's work, and Rand's case for the market rested very heavily on philosophical arguments instead of economic analysis. However, Greenspan did not share Rand's disdain for Friedman. And, in discussing Greenspan's career with friends in later years, Friedman emphasized the differences in viewpoint between Rand and Greenspan (Michael Mork, personal communication, May 20, 2013).

President Nixon nominated Greenspan to be Chairman of the Council of Economic Advisors in 1974. Friedman reacted favorably, praising Greenspan as a "very good man" who would resign on principle if there were another 1971-style lurch to expansionary policies (*Chicago Tribune*, July 17, 1974), and Friedman would interact with CEA Chairman Greenspan when both participated in the conferences on inflation early in the Ford Administration. From early 1977, Greenspan was again out of government and had returned to his Townsend-Greenspan consulting firm, while Friedman, of course, had relocated to San Francisco. "I had a lot of interactions with him at that time," Greenspan recalled, "in addition to, of course, very substantial personal contacts, which accelerated when I moved to the Fed. I [had] read much of what he wrote, including, obviously, the Friedman-Schwartz opus. I had heard of Rose Friedman before I'd ever heard of Milton, having read an article she wrote with Dorothy Brady for the National Bureau of Economic Research on the relationship between income and consumption in 1947. Subsequently, I saw a great deal of the two of them together. I visited him many times in San Francisco, for example. We would go out for an invariably lively lunch in some neighborhood café. Obviously, when I became Fed Chairman, there was a limit to what I could talk to him about. But we remained close to the end." (Alan Greenspan, interview, August 19, 2013.)

During his 1977–1987 period back in the private sector, Greenspan had, like Friedman, been one of the economists supplying advice to Reagan. They were both signatories for the 1980 memorandum, mentioned in Chapter 19, to President-elect Reagan, and they both served as

[128] See, for example, Greenspan (1966), as well as Nelson (2020a, Chapter 15).

members of the Presidential Economic Policy Advisory Board during Reagan's presidency.[129] In 1983, Greenspan had written of Friedman: "I'm a long and ardent admirer of his."[130] In 1986, he had praised Friedman as one of the twentieth century's major intellects (*Los Angeles Times*, December 14, 1986). In light of this background, it is not surprising that very soon after Greenspan was nominated as Chairman, Friedman expressed delight. His July 1987 remarks have already been quoted. Earlier, when Greenspan's nomination had been announced, Friedman observed: "I'm a good friend of Alan Greenspan. He's an able person." (*Minneapolis Star-Tribune*, June 3, 1987.) Friedman maintained, however, he would still rather have the Federal Reserve replaced by a computer. "I send my congratulations to Alan. And my sympathy." (*San Francisco Chronicle*, June 8, 1987).

Friedman remained insistent that the Federal Reserve should move to using total reserves or the monetary base as its operating instrument. In early 1992, he told the present author that he had believed that Greenspan had been inclined in that direction. Friedman went on to express disappointment that the FOMC under Greenspan had continued with a federal funds rate instrument (Milton Friedman, interview, January 22, 1992).

In retrospect, it is surprising that Friedman had ever had the impression that the Federal Reserve might shift from a federal funds rate instrument to a reserves instrument under Greenspan. Possibly Friedman got that impression from conversations in the late 1970s, when disillusionment with the federal funds rate instrument was very widespread. In fact, in his market commentaries for the Townsend-Greenspan firm, Greenspan had shown himself amenable to viewing monetary policy actions and stance in terms of the federal funds rate—and had in fact been criticized by Friedman for his tendency to do so (Instructional Dynamics Economics Cassette Tape 106, August 24, 1972). Greenspan's period as Chairman would see the FOMC's use of the federal funds rate as an instrument continue and become transparent. Furthermore, as discussed in the next chapter, among academics the notion that a short-term interest rate was a viable instrument of monetary policy would receive very wide acceptance during the Greenspan years. Indeed, Alan Greenspan's period at the Federal Reserve would see the development of a consensus in the economics profession in favor of centering monetary policy conduct on the federal funds rate.

[129] Another public Friedman/Greenspan connection was that Greenspan contributed a chapter to the Hoover Institution Press' volume *The United States in the 1980s*: Greenspan's (1980) chapter was adjacent to the Friedmans' (1980b) chapter (which, as mentioned previously, was excerpted from the *Free To Choose* book)

[130] Quoted in Friedman (1984f, p. 21).

In contrast to his views on policy instruments, Friedman's emphasis on M2 did find favor in the early Greenspan years. Writing in *Business Week* about the lessons of the 1980s, Alan Blinder had claimed that among these lessons was that "we should forget about monetarism," on the grounds that the swings observed in velocity had "delivered a resounding rejection of monetarist doctrine." (*Business Week*, November 27, 1989.) Such a categorical judgment concerning the 1980s, however, rested heavily on judging monetarism using the M1 aggregate. If, as Friedman had decided in 1986, M2 should be preferred to M1 as the measure of money, relationships between the money and the economy in the 1980s were easier to discern: not only was M2 growth correlated with nominal income growth and inflation, but the relationship between *levels* of M2 and other variables appeared stable. Notably, M2 velocity was stationary on U.S. data from the mid-1950s onward.[131]

Alan Greenspan was a longtime M2 watcher. Jerry Jordan, who participated in FOMC deliberations as president of the Federal Reserve Bank of Cleveland from 1992 to 2003, would recall that "Greenspan was a fan" of M2 during Greenspan's years on Wall Street. "Alan, with his company, was looking for things that tended to work [for forecasts] in the consulting business. And so he became an M2 advocate long before others did, simply because of the empirical work and the track record that Anna and Milton had set forth in the early '60s." (Jerry Jordan, interview, June 5, 2013.)

This book has also had already occasion (in Chapter 2, for example) to refer to the fact that, as Chairman of the Council of Economic Advisers in 1974–1977, Greenspan linked price-level behavior to money, and to M2 behavior in particular. For example, in testimony to the House of Representatives' Committee on the Budget in September 1974, Greenspan presented a plot of the U.S. GNP price deflator against the "unit money supply," defined as (old) M2 divided by real GNP. He also stated the need to "reduce the rate of increase of M2."[132] Later, in a 1981 appearance at a Brookings Institution meeting, Greenspan stressed the value of (modern) M2 as a

[131] Blinder (1998, p. 28) would subsequently argue that the relationship between (logs of) M2 and nominal income —specifically, cointegration between the logs of these two series—was only detectable once data from the late 1980s was included in the sample period. This does not seem to be an appropriate conclusion, as the original cointegration study of Engle and Granger (1987) had concluded that M2 velocity might be stationary on data through 1981, and Benjamin Friedman (1988b) found that there was cointegration between the logs of M2 and nominal income—on a 5 percent significance criterion—for both 1959 to 1979 and 1959 to 1982. (Benjamin Friedman also found a weakening of the evidence in favor of the cointegrating relationship once post-1982 data were included, but Rasche, 1990, and Hallman, Porter, and Small, 1991, suggested that this apparent deterioration reflected changes in the opportunity cost of holding M2 during the 1980s rather than a breakdown of cointegration.)

[132] See his testimony of September 25, 1974, in Committee on the Budget, U.S. House of Representatives (1974), for the quotation (p. 85) and the chart (p. 88).

monetary indicator, citing the long-term historical stability of its velocity.[133]

This perspective was felt early in Greenspan's period as Chairman. Greenspan commissioned Federal Reserve Board staff to work on the relationship between M2 and prices. The public version of the research that resulted (Hallman, Porter, and Small, 1989) made front-page news in the *New York Times* (June 13, 1989). However, that newspaper article and related coverage gave the false impression that the authors' "P-star" model, which specified a link between M2 behavior and aggregate economic series, had become the Federal Reserve Board staff's primary forecasting tool. In fact, the main macroeconomic forecasting for the U.S. economy was, and is, done by the Board's Division of Research and Statistics, while the Hallman-Porter-Small paper was a product of the Board's (then-new) Division of Monetary Affairs.

Furthermore, many aspects of the "P-star" model never gained wide acceptance among monetarists: Robert Rasche and Bennett McCallum, for example, both took issue with the model's specification of dynamic adjustment of prices, with Rasche criticizing it for treating inflation as a unit-root process and McCallum concerned that the use of the specification could encourage the misconception that Phillips-curve specifications of price adjustment were inconsistent with the monetarist view of inflation.[134] When the Hallman-Porter-Small paper saw print (in a revised version that noted Friedman as among those who had sent comments on the paper), it was the long-run M2-and-prices relationship that was the focus of the study rather than the specification of inflation dynamics.[135] And this long-run relationship was a reason why M2 featured quite heavily in monetary policy discussions in the first four and a half years of Greenspan's tenure as Federal Reserve Chairman.[136]

The long-run relationship exhibited between M2 (per unit of output) and prices implied a relationship in the long run between M2 growth (adjusted for output growth) and inflation. The latter relationship, rather than the corresponding levels relationship, naturally figured in the FOMC's consideration of M2, in light of the fact that the Committee set targets for monetary

[133] Brookings Institution (1981, p. 198).

[134] The sources for these statements are, respectively Rasche (1993a, pp. 293–300) and the present author's lunch conversation with Bennett McCallum, Carnegie Mellon University, 1995.

[135] This is brought out by the introduction of the phrase "long run" to the title of the authors' paper: Hallman, Porter, and Small (1989) was titled "M2 per Unit of Potential GNP as an Anchor for the Price Level," while the 1991 published version bore the title "Is the Price Level Tied to the M2 Monetary Aggregate in the Long Run?"

[136] Benjamin Friedman (1997, p. 159) stated that "there is no evidence of renewed [policymaker] attention to M2 targets until the early 1990s." The basis for this statement was the fact that M2 growth entered his estimated interest-rate reaction function significantly only in samples starting in 1991 or 1992 (pp. 156, 159–160). But the increased coefficient in the early 1990s may well have been a delayed reflection of Greenspan upgrading the role of monetary policy starting in the late 1980s.

growth rather than the level of money and was in practice concerned with inflation rather than the absolute price level.[137] Under these circumstances, a key question was—if setting a course for M2 growth could be used as a basis for setting a longer-run rate of inflation—what rate of inflation should be the objective? Early in Greenspan's period as Chairman, a prevalent outside perception was that the Federal Reserve would be comfortable with a situation in which inflation settled at about 4 percent. A 4 percent rate was roughly what Greenspan had inherited: the four-quarter CPI inflation rate crossed 4 percent in the third quarter of 1987 and was generally in the 4 to 4½ range through the end of 1988 (see Figure 1(*d*) above). Some of this rebound, from the rates below 2 percent observed in late 1986, reflected the passing from the series of the impact of the 1985–1986 decline in energy prices. But the rise also reflected, in Friedman's view, an overly permissive monetary policy in 1986. He believed the Federal Reserve had overdone things in endeavoring to accommodate the recovery in desired real balances that had come with the end of the Great Inflation (Idea Channel, 1987; *The Aden Interviews*, June 1987, p. 1).[138]

However, the presence of lags means that the inflation rate for 1987 and even that for 1988 cannot be judged as having been very susceptible to influence from the FOMC that met under new Chairman Greenspan in late 1987. For the Committee, the more relevant question regarding inflation was what rate would be appropriate for the United States in 1989 and beyond.
For his part, Friedman continued to prefer a zero percent rate of inflation. As before, however, he indicated that he did not have strong objections to a steady-state inflation rate a percentage point or two above zero. In April 1988, for example, Friedman expressed the wish that M2 growth had been held at a steady 4 percent for the prior quarter-century, a policy that he said would have delivered an average inflation rate of about 1 percent (*Wall Street Journal*, April 15, 1988). The fact that the inflation rate that he identified with this policy raised an issue that would recur in the Greenspan era. Friedman wrote in 1992 that the desirable rate of inflation corresponded to "a level that would make it [inflation] irrelevant to individual and business decisions" (*Wall Street Journal*, October 23, 1992). It was such a perspective that made Friedman hostile to deflation; it also led him to concede—for example, when he advocated indexation for the United States in the 1970s (see Chapter 2 above)—that the harm done by inflation really came from rates 3 percent or higher.[139]

[137] Gorodnichenko and Shapiro (2007) characterize the Greenspan Federal Reserve as having been concerned with stabilization of the absolute price level rather than inflation. But this characterization seems to rely on an over-literal interpretation of phrases such as "price pressures" that policymakers' statements have in practice used when discussing inflation.

[138] Friedman would blame the continuation of inflation into 1989 on the monetary growth of the mid-1980s (*U.S. News and World Report*, September 11, 1989).

[139] For example, in American Enterprise Institute (1974, p. 51), Friedman stated that 2 or 3 percent inflation would mean that agents would find indexation not worth the nuisance, and Friedman (1984f, p. 165) similarly noted that

Alan Greenspan, too, would become well known for the position that the acceptable level of price stability might not coincide with literal constancy of prices. Greenspan, like Friedman, emphasized the necessity for an inflation rate that did not distort economic decisions. And Greenspan became identified with the notion that this rate might not be literally zero percent, especially in light of the likelihood—with which Friedman had considerable sympathy—that measured inflation was biased upward by recorded price increases that reflected quality improvements. Although it has become customary to cite latter-day Greenspan statements for his articulation of this position (notably Greenspan, 2002), Greenspan became associated with the position at an early stage. Friedman himself, in referring in May 1989 to "what Alan Greenspan says is zero inflation," implied that Greenspan was seeking mildly positive measured rates of inflation.[140] Another commentator a couple of months later discussed the shape that monetary policy would take "if your goal is to take inflation out of the decision-making picture, as Alan Greenspan says he wants to do."[141] Indeed, sentiments along these lines had prevailed at the Federal Reserve in the 1980s even ahead of Greenspan's ascension, with Orphanides (2006, p. 179) tracing them to Chairman Volcker's talk at the December 1983 American Economic Association meetings, a talk that Friedman probably attended.[142]

Although Volcker and Greenspan shared a similar concept of price stability, it was the early Greenspan period that saw the FOMC take concerted steps to move further in the direction of achieving inflation rates consistent with that concept. Robert Heller, who served as a Federal Reserve Board member from 1986 to 1989 and as such witnessed the transition from Volcker to Greenspan, vouched for the resolve of the FOMC in the early Greenspan years to get the longer-term inflation rate to a lower single-digit range. "If you have an inflation of minus 1 percent, there's enormous economic destruction of capital goods that goes along with it," Heller would note. "And so an inflation rate of 1 percent or maximum 2 percent was what I thought of as the

the prevalence of indexed contracts would decline once an economy's inflation rate became low. In terms of his own normative economics, Friedman had stated in 1981: "I think the best of all worlds is one where you have no inflation and no indexation. Indexation is not a good thing in and of itself." (*New Zealand Herald* (Auckland), April 18, 1981.) (See also Friedman, 1974, p. 158 of 1975 reprint.) This sentiment was also echoed in policy circles in the early 1980s. Asked at a Congressional hearing about the indexation of the federal tax system, which was scheduled to begin in 1985, Paul Volcker remarked: "I would like to get the economy back on a base where price stability is a normal presumption and [so] the issue of that indexing wouldn't arise." (From his February 24, 1982, Congressional testimony, in Committee on Finance, 1982, p. 105.)

[140] Quoted in *Seattle Times*, May 2, 1989. Along similar lines, in his October 1992 *Wall Street Journal* article Friedman wrote as though the definition of price stability he gave was also that stated by the Federal Reserve.

[141] Donald Ratajczac on *Wall Street Week*, Maryland Public Television, July 14, 1989, p. 9 of transcript.

[142] Specifically, Orphanides highlighted a passage in Volcker (1983). (Orphanides, 2006, and Lindsey, Orphanides, and Rasche, 2005, p. 227, both gave the title of this Volcker talk as "Can We Survive Prosperity?" The actual title was the more optimistic "We Can Survive Prosperity.") On Volcker's attitude to the matter of the appropriate inflation rate, see also the 1982 Volcker remark quoted in an earlier footnote of the present chapter.

Holy Grail... And we were trying to get there. We didn't quite get there." (Robert Heller, interview, September 9, 2013.)

The behavior of M2 growth in the late 1980s reflected this resolve. In 1986, M2 growth had come within the FOMC's 6 to 9 percent target range, but only just: an outcome of 8.9 percent. M2 growth came down during much of 1987, including during Volcker's closing months, and the year's outcome of 4.3 percent was actually below the announced target range of 5.5 to 8.5 percent. The target range was then lowered to 4 to 8 percent for 1988, a year for which M2 growth outcome proved to be 5.2 percent; and to 3 to 7 percent for 1989, for which the outcome was 4.7 percent.[143]

Despite his familiarity with Greenspan's basic posture toward monetary matters, Friedman had not expected monetary policy strategy to change materially. He had said just before Greenspan took over that he did not expect the change in Chairman to "make much difference," largely because of the institutional continuity in the Federal Reserve.[144] In the wake of the October 1987 stock market crash, Friedman indicated that he thought that the Federal Reserve would respond in the right direction by expanding the money stock but, "in their usual way, they will probably expand it too much." (*The Independent* (London), October 28, 1987.) The behavior of Greenspan's FOMC confounded this expectation: the FOMC was indeed accommodative for a few months after the stock market ahead, but it thereafter withdrew the stimulus and pressed ahead with plans for a lower money growth rate. Friedman was impressed, as Robert Heller discerned when he would see Friedman on Heller's visits to San Francisco. "I remember conversations with Friedman in which he was very supportive of the policy direction, and the modest monetary growth that we were achieving during the years that I was there, which was pretty close to his ideal of, you know, sort of five percent monetary growth. And I remember him saying that. You know, 'Hey, you're doing the right thing, you know? You're on the right track.' Words like that." (Robert Heller, interview, September 9, 2013.) In addition to making such remarks privately, Friedman also put on the public record his satisfaction with Greenspan's tenure so far. Friedman stated in early 1989 that Greenspan "on the whole has been doing very well, as compared with his predecessors," and he added that Greenspan's tenure had thus far featured "a pretty even course." (*Toronto Star*, March 11, 1989.)

A danger inherent in a strategy of shifting to lower rates of monetary growth and inflation was

[143] All numbers given in this paragraph are from Bernanke and Mishkin (1992, p. 191).
[144] Friedman (1987c, p. 365). See also Nelson (2020a, Chapter 8).

that a recession might be triggered along the way. The deceleration in monetary growth in 1987 and 1988 had been achieved without a recession, but in 1989 Friedman was citing evidence that the prior sustained monetary tightening was showing up in real economic activity. "For all we know, a recession may have started in May, based on what has been happening to the economy since then." (*U.S. News and World Report*, September 11, 1989.) The NBER would eventually date the U.S. output contraction to an altogether later period: according to the NBER's chronology, the 1982–1990 economic expansion culminated with a peak in July 1990, and the subsequent recession troughed in March 1991.[145] When that NBER-designated recession was in progress, Friedman would defend dating the recession back to mid-1989 or late 1989, on the grounds that a distinct slowdown in the economy began at that point (*Chicago Tribune*, January 26, 1991). In the modern vintage of data, however, four-quarter growth in real GDP from mid-1989 to mid-1990, while declining from the 4 percent-plus rates of 1988, is consistently 2.5 percent or higher. There is consequently little case for dating the recession earlier than the second half of 1990. However, once the recession proper was in motion, Friedman would cite monetary policy as a factor that had exacerbated the economic weakness. He specifically pointed to "the exceedingly restrained monetary growth of the last four months of 1990."[146]

There is considerable agreement from others who have assessed this period with the judgment that monetary policy contributed to the economic slowdown of 1989 and the recession of 1990–1991. Some analysts, like Martin Feldstein, followed Friedman in explicitly citing weak M2 growth as a demonstration of the monetary tightening (*Wall Street Journal*, June 10, 1991). But the contention that monetary policy played a significantly restrictive role over these years has also emerged from analyses that did not focus on the behavior of monetary aggregates. Ball (1993, p. 6) and Romer and Romer (1994, pp. 81–84), for example, both cited the Federal Reserve's shift to a more restrictive monetary policy in the late 1980s as a factor behind the 1990–1991 recession, with the latter pair of authors pointing (p. 82) at December 1988 as a date at which the FOMC intensified its policy tightening in order to put additional downward pressure on inflation. The picture of monetary tightness during the late 1980s is also confirmed by the nearly 300 basis-point rise in the federal funds rate from 1987:Q2 to 1989:Q2—a rise that brought the quarterly average of the funds rate to nearly 9.75 percent. (See Figure 1(*b*) above.)

As the economy slowed down and entered recession, the FOMC instituted a series of reductions in the federal funds rate. Although in the early 1990s the Federal Reserve was not yet

[145] See http://www.nber.org/cycles/cyclesmain.html.
[146] *The MacNeil/Lehrer NewsHour*, PBS, March 6, 1991, p. 7 of transcript.

completely forthcoming with regard to the fact the FOMC used the federal funds rate as its instrument, policymakers were open about their intention to support a recovery, and the publicly-announced measures included reductions (made by the Federal Reserve Board) in the discount rate in late 1990 and in the course of 1991.[147] Testifying in January 1992, Greenspan cited weakness of M2 growth as a reason the Federal Reserve had put downward pressure on short-term interest rates. But he acknowledged that this reaction had proceeded incrementally in the face of persistently weak M2 outcomes.[148]

The most recent policy move in this direction had been a Federal Reserve Board, rather than FOMC, action: a 100-basis-point cut in the discount rate in December 1991. Friedman had deprecated the actual significance of the move: "The discount rate cut is irrelevant. It's a public relations gesture." Here Friedman may have been referring to the fact that the Federal Reserve's actions on short-term interest rates were effected by FOMC actions in the open market, not by administrative changes by the Board in the discount rate. A significant aspect of the latter rate at the time, however, was that changes in it were publicly announced in Federal Reserve press releases, whereas the FOMC did not have postmeeting policy statements until 1994. Friedman himself conceded some communications value in the discount-rate cut: "But as a signal that the Fed realizes that it needs growth in M2 and was keeping M2 too low even by its own targets, it's helpful." (*Barron's*, December 23, 1991.)

The gradual approach that the Federal Reserve had taken in its interest-rate reductions in 1991 had had a counterpart in a lack of a rapid rebound of M2: the M2 growth outcome for 1991 was, at 2.7 percent, at the lower end of the announced 2.5 to 6.5 percent target range (Bernanke and Mishkin, 1992, p. 191).

However, the FOMC's disinclination toward taking a more aggressive approach to achieving the M2 growth targets reflected something beyond a desire for gradual interest-rate behavior. Events since 1990 had reduced the confidence that Greenspan and other Federal Reserve officials had in the M2 aggregate. A paper issued in one of the Federal Reserve Board's working paper series in late 1992 (Feinman and Porter, 1992) documented the anomalies that had emerged from internal staff analysis of M2 demand behavior in recent years. The Board staff had found it useful to connect M2 velocity behavior to the spread between short-term market interest rates and the own-rate on M2 (see Small and Porter, 1989). Indeed, that money demand relationship had

[147] The discount rate, held at 7 percent through the first eleven months of 1990, was reduced in December 1990 and stood at 3.5 percent in early 1992. See http://research.stlouisfed.org/fred2/data/MDISCRT.txt.
[148] January 10, 1992, testimony, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1992a, p. 65).

provided a basis on which policymakers could judge what value of the federal funds rate would best deliver the M2 growth target. But as the authors of the 1992 study observed (p. 1), "Over the last few years, M2 growth has consistently run below the forecasts of the Board staff's standard model…" M2 velocity had risen slightly since 1990, when the historical relationship of velocity to the opportunity cost of holding M2 would have suggested that velocity should decline. It was in light of such evidence that the FOMC had assessed that there had been a shift down in the real demand for M2, and that the appropriate amount of policy easing might well be less than what would be required to generate M2 growth at the center of its target range.

Watching events from the outside, Friedman emphasized the durability of the relationship between M2 and the economy, and he became critical of the Greenspan FOMC. The focus on interest rates was a sore point: "I have criticized the Fed for at least three decades for using interest rates rather than the quantity of bank reserves as their operating basis," he observed (*Christian Science Monitor* (Boston), February 7, 1992), and, as noted above, was particularly disappointed in light of his belief that, prior to becoming Federal Reserve chair, Greenspan had been interested in a reserves-oriented approach.

In May 1992, Friedman wrote that the United States had experienced "three years of stagnation" in the period since mid-1989.[149] Later in 1992, he suggested that monetary policy of recent years showed signs of "perverse fine-tuning"—by which he meant that monetary policy had been inadvertently restrictive, by placing too much weight on low interest rates when assessing policy stance and too little on weak M2 growth.[150] Friedman called for a more vigorous injections of commercial bank reserves, on a scale that would bring M2 growth back well inside its announced target range (*Wall Street Journal*, October 23, 1992; see also *Forbes*, August 17, 1992, p. 42). He noted that, owing to the fact that deposit growth could be matched by growth in commercial bank assets other than loans, a retrenchment of commercial bank loans—what in the early 1990s was being called the "credit crunch"—need not prevent monetary policy from achieving an increase in M2 growth (*Wall Street Journal*, October 23, 1992).

Thus, at the end of 1992, Friedman's position was that the Federal Reserve had been contributing to economic weakness by permitting low M2 growth—in considerable contrast to the Federal Reserve's own position that low M2 growth gave a misleading impression because the demand for M2 had shifted downward. In the 1970s, Friedman had been generally correct in his

[149] Friedman (1992b, p. ix).
[150] Anna Schwartz likewise described Greenspan's Federal Reserve as having adopted an "unwittingly restrictive" posture (*Christian Science Monitor* (Boston), February 7, 1992).

arguments with Federal Reserve policymakers about monetary aggregates and the stance of monetary policy. In the middle Volcker years, in contrast, Friedman had generally been wrong and policymakers had been correct. The verdict on the early 1990s is more nuanced. The Federal Reserve Board staff and the FOMC are widely regarded as having been correct in pointing to a shift downward in the real demand for M2 during the 1990s. Because the shift was apparent in money demand forecasts (which were based on predicting velocity conditional on other variables) ahead of when it showed up in the unconditional behavior of velocity, the Federal Reserve noticed this shift years ahead of Friedman, who was focusing on velocity.[151]

However, it would seem that monetary policy behaved in a manner that made the rate of M2 growth lower than what the money demand shift in itself would justify. Put differently, monetary policy was tighter than intended in the early 1990s, and actions that would have made M2 growth closer to target would have contributed to economic stabilization. The appropriate conclusion would appear to lie between the Friedman and Board interpretations. Friedman was mistaken in presuming that the relationship between M2 and the economy had not shifted in the early 1990s. But the policy he called for during that period—measures to bring M2 growth up by a couple of percentage points—would have been more stabilizing for real and nominal GNP growth than the monetary policy actually followed.

Official acknowledgment of the basic point underlying Friedman’s criticism came in testimony that Greenspan delivered in July 1992. The historical relationship between M2 and economic activity “broke down” in 1990, he stated, yet M2 still had “significant forecasting and analytical properties” (*Wall Street Journal*, July 23, 1992). Further acknowledgment came later in 1992 in the form of a research paper by senior Federal Reserve Board staff member David Lindsey. Lindsey’s paper was distributed to non-Federal Reserve readers on a restricted-circulation basis at the time of its delivery; it was subsequently excerpted in a Lindsey study that was declassified in 2009. In the 1992 paper, Lindsey stated: “Despite the evident uptrend in M2 velocity in recent quarters… weakness in M2 relative to its annual ranges in 1991 and 1992 did turn out to portend sluggish economic growth.”[152] Consistent with this conclusion, Laurent (2000) and Whitesell

[151] That is, M2 velocity was fairly constant from 1990 to 1992, but it was persistently high compared with predictions based on its historical relationship with opportunity-cost variables. Friedman, primarily paying attention to the unconditional behavior of velocity as opposed to its behavior in relation to money demand equation forecasts, failed to notice a shift until velocity actually rose sharply in 1992–1995. On the velocity rise in this latter period, see the next chapter.

[152] Lindsey (1992, p. 366); also quoted in Lindsey (1993, p. 133). Likewise, in an interview for this book, Lindsey said: “we [the Federal Reserve Board staff] found in the early to mid-’90s, following money actually was helpful… [as] it did give some hints about the future of the economy… [T]here was a shift in M2 demand, but when M2 was

(1997) would find that the bivariate relationship between M2 growth and growth in measures of aggregate income was maintained, or even improved, as the years 1990 to 1992 were added to the sample period, notwithstanding the shift in M2 demand that commenced during those years.

The notion that M2 behavior was, on balance, conveying a correct signal about the direction in which monetary policy should have gone would be consistent with the idea that M2 growth was inversely related to a variety of yields in the U.S. economy. In this connection, it deserves mention that the resilience of high levels of long-term interest rates in relation to short-term rates (see Figure 1(*b*)) was being cited in some commentaries in the early 1990s as a factor restraining the recovery from the recession (see Nelson, 2021, for a discussion and references). M2 demand might be (for given short-term interest rates) inversely related to the longer-term rate—perhaps because, as Friedman and other monetarists had long argued, longer-term rates entered the money demand function.[153] Alternatively, this inverse relationship could have arisen because, as a number of analysts were arguing in the early 1990s, instruments linked to longer-term rates were becoming more easily accessible to households.[154] Under either scenario, high long-term rates would tend to have as their corollary weakness in the M2 aggregate (in relation to income). One could then view the fact that conventional M2 demand functions exhibited instability in the 1990s as partly reflecting the omission of longer-term securities from the analysis; and efforts to raise M2 growth by larger-scale open market purchases would likely have been helpful in providing downward pressure on longer-term interest rates. It is therefore possible to rationalize Friedman's emphasis on stimulating M2 even in a context in which money balances appear in the money demand equation of a model, but not in any of the other structural equations of the model.[155]

Although he had once more turned critical of Federal Reserve policy, Friedman was more restrained and qualified in his criticisms during 1991 and 1992 than he had often been in the 1970s and 1980s. This moderation no doubt partly reflected his good relations with Greenspan, but it also was a sign of the fact that his disagreement with current monetary policy was of a more limited nature than on previous occasions. Friedman had supported the general course—pursed by Greenspan since 1987 and reaffirmed by the Chairman in July 1992—of shifting

weak, it was signaling not only the weakness in M2 demand, but also the weakness in the economy." (David Lindsey, interview, May 2, 2013.)

[153] See the discussion in Chapter 6 in Book 1.

[154] See, for example, Feinman and Porter (1992) and Duca (1995).

[155] In particular, this argument does not rest on any direct role for money balances in term-structure determination. It only requires that longer-term rates enter the money demand and IS equations. Of course, if M2-stimulating measures operate on longer-term interest rates through channels other than the pure expectations theory of the term structure, the case that M2 stimulus would have added to recovery would be reinforced.

monetary policy settings toward towards consistent with price stability.[156] Thus, even when holding Greenspan's Federal Reserve responsible for worsening the recession, Friedman contended in early 1991 that it "in the past three or four years has on the whole done an extremely good job… directed at bringing inflation down."[157]A year later, he acknowledged that the recession had not in fact been very deep: "the people at large are very pessimistic, much more pessimistic than the statistical data on the severity of the recession would justify," he observed.[158]

This assessment on Friedman's part has received affirmation by retrospective evaluations. Within a few years of the events in question, Parkin (1996, p. 522) would observe: "The recession of 1990–1991 seemed pretty severe as we were passing through it, but compared with earlier recessions, it was relatively mild." Subsequently, the 1990–1991 period, the recession notwithstanding, would be regarded as part of the 1984–2007 "Great Moderation" period. On annual-average rate data, real GDP does register a decline in 1991, but only of 0.1 percent. Retrospective evaluations also tended to make the output gap for 1991 and 1992 less negative than suggested at the time (see, for example, Orphanides, 2001, p. 969).

The danger of an excessively expansionary policy reaction to the recession was one factor motivating the gradual character of Greenspan's easing steps (see López-Salido and Nelson, 2010). Friedman shared this concern: as emphasized above, he wanted M2 growth stepped up, and he stated in August 1992 that the Federal Reserve "deserves to be criticized" for the fact that the recovery had been rapid enough.[159] But he also noted that the mildness of the recession meant that a sharp economic recovery was not desirable and did not want monetary stimulus that exceeded that necessary to bring monetary growth to the center of its target range.[160] And the recession itself had been magnified by a major nonmonetary factor, in the form of the oil price rise that was set off by Iraq's invasion of Kuwait in August 1990.

This oil shock also produced a new, albeit temporary, blot on Friedman's inflation projections. In July 1989, he had said that inflation was "likely to decline sharply over the next several years" reflecting the step-down in M2 growth (*Wall Street Journal*, July 5, 1989). Indeed, he had

[156] Greenspan referred to "our long-term goal of price stability" in his Congressional testimony of July 22, 1992. See *Wall Street Journal*, July 23, 1992.

[157] *The MacNeil/Lehrer News Hour*, PBS, March 6, 1991, p. 7 of transcript.

[158] *The MacNeil/Lehrer News Hour*, PBS, March 12, 1992, p. 10 of transcript.

[159] Friedman (1992d, p. 523). See also the discussions in Chapters 17 and 18 below.

[160] For example, *The MacNeil/Lehrer News Hour*, PBS, March 6, 1991, p. 7 of transcript, and *The MacNeil/Lehrer News Hour*, PBS, February 21, 1992, p. 5 of transcript.

specifically suggested that inflation would likely peak in the first half of 1989 (*Toronto Star*, March 11, 1989). U.S. inflation series did indeed decelerate in the second half of 1989, and in the second quarter of 1990 four-quarter CPI inflation and GDP deflator inflation both stood below rates observed since the end of 1988.[161] But the 1990 oil shock then sent four-quarter CPI inflation to 6.3 percent in 1990:Q4. This was the highest rate since mid-1982.

In the face of this spike, Friedman reaffirmed in a January 1991 appearance that inflation would nevertheless decline to 2 or 3 percent over the next few years (*Chicago Tribune*, January 26, 1991; see also *Futures*, March 1, 1991). And on television several weeks later, Friedman said that "inflation will be coming down, will be coming down fairly sharply over the next year or two."[162] He was proved correct: as the effect of the oil shock faded from the growth rates of U.S. price indices and the prior years of restrained aggregate demand policies made themselves felt, four-quarter CPI inflation moved to about 3 percent at the end of both 1991 and was still around that level a year later, with GDP deflator inflation about half a percentage point lower still in both years. Levin and Piger (2004, Tables 2 and 3, pp. 25–26) would later point to 1991 as the year in which trend inflation in the United States underwent a distinct shift down from the rate prevailing since 1983. Once a second consecutive year of low inflation was on the books, Friedman felt motivated to write to the *Wall Street Journal* to complain about the fact that an article declining the shift to low inflation neglected to mention money (*Wall Street Journal*, February 12, 1993.)[163]

Thus, as of late 1992, Friedman saw little reason not to have confidence in M2 as an indicator of monetary policy: its sluggish growth had been followed by a step-down in nominal GDP growth, weakness in real GDP growth, and a lasting decline in inflation. What was more, as of 1992 the velocity of M2 had exhibited rough stability for nearly forty years. Friedman therefore surely felt he was in a strong position to brush aside the doubts being placed on M2 by Federal Reserve staff and policymakers. But as Friedman moved into 1993, the problems with M2 as an indicator—of monetary policy stance, and of where the economy and inflation were heading—were about to become impossible for him to miss.[164]

[161] At the time, the deflator series on which analysts focused was actually the GNP deflator. The United States conformed to international practice starting in 1992 in focusing on GDP rather than the GNP as the central national income aggregate.

[162] *The MacNeil/Lehrer NewsHour*, PBS, March 6, 1991, p. 7 of transcript.

[163] Friedman's letter, however, gave a muddled representation of his own position by uncharacteristically linking the behavior of inflation to monetary growth in the same year (in this case, 1992). His emphasis on lags in the relationship should have led him to cite slow monetary growth observed since 1987.

[164] See Chapter 18.

GEORGE HERBERT WALKER BUSH

In 1986, Ben Stein played a high school economics teacher in the film *Ferris Bueller's Day Off.* Stein was asked to improvise his dialogue for his scene, which involved giving a monologue to an economics class. Stein chose to make the scene consist of an exposition of the Laffer curve. The exposition culminated in Stein's character making mention of the fact that in 1980, when campaigning against Ronald Reagan for the Republican nomination, George Bush had referred to supply-side ideas as "voodoo economics." On the basis of this well-known scene in the film, Stein would observe: "I've often said I am by no means even remotely in the top tiers of economists in terms of education, but I am the most famous economics teacher in the history of American economics." (Ben Stein, interview, March 18, 2015.)

Friedman, of whom Stein was a longtime family friend, saw the film.[165] To Stein's surprise, although Friedman was complimentary about Stein's scene, he also expressed definite qualms about it. It seemed that Friedman did not wholly approve of the implication of the scene. As Friedman had himself questioned the empirical validity of the Laffer curve, it is unlikely that the criticism of the Laffer curve embedded in Stein's exposition was the main source of Friedman's misgivings. It seems more likely that Friedman did not like the fact that the scene put the economic views of Vice President Bush in a good light. To Friedman, it was not a virtue that Bush had been opposed to Reagan in 1980. Furthermore, as will be seen below, in the years since 1980 Friedman had acquired further reservations about Bush's command of, and perspective on, economics.

These reservations did not prevent Friedman from supporting the vice president when Bush became the Republican nominee for president in 1988. But a hint of Friedman's lack of great personal esteem for Bush surfaced even when, in July 1988, Friedman was dismissing the current opinion polls favoring Michael Dukakis and predicting a Bush victory: "I think Bush will win, and I think he will win handily—not because of any particular merits of his, but because the American people are not going to vote [in] a McGovernite Democrat."[166]

During Bush's first year of office, Friedman was optimistic that the Reagan domestic economic agenda would be continued by the new administration. It has already been indicated (in Section

[165] Stein initially knew Friedman through Stein's father Herbert. Ben Stein later had considerable interaction with Friedman starting with Friedman's spell in 1964 at Columbia University (at which Stein was an undergraduate student). They subsequently saw each other frequently from the late 1970s through the mid-2000s, a period over which both Stein and Friedman were based on the West Coast. (Ben Stein, interview, March 18, 2015.)
[166] Friedman (1989b, p. 13).

I of this chapter) that by the end of 1989 Friedman was dismayed by the tendency of U.S. public discourse to move away from a free-market orientation. But he believed that the Bush administration could well carry out policies that were contrary to this tendency. Friedman was consequently very optimistic about the prospects for the U.S. economy for the next decade. "There's no reason why the '90s shouldn't be as good as the '80s, or better," Friedman told *Time* magazine (January 1, 1990). "There's no reason we shouldn't have a decade of rapid growth and relatively low inflation."

When the data were in for the 1990s as a whole, it was clear that Friedman's optimism had been vindicated. A little over a year after he made his bullish appraisal of economic prospects, however, Friedman was not inclined to reaffirm it. Rather, he observed in January 1991: "It is very hard to be optimistic about the growth prospects for the 1990s, because the 1990s are starting out under a series of economic policies that are very adverse to growth." (*Chicago Tribune*, January 26, 1991.) In this connection, Friedman placed emphasis not on the recession—which he indicated he expected to be mild—but the economic policies of President Bush. Using a formula that he would repeat multiple times over the next couple of years, Friedman stated that Bush had reversed President Reagan's policies in three directions: by moving from a policy of cutting marginal tax rates instead of raising them, by increasing the pace of regulations, and by increasing the share of public spending in national income. During Reagan's final year in office, Friedman had said, "The policies introduced by Reagan have been extremely successful in reigniting the forces of enterprise and initiative in the United States." (*Lodi News-Sentinel*, May 26, 1988.) Now, in 1991, he felt that the policy framework conducive to entrepreneurship was being dissipated by the policy changes occurring under Bush.

Friedman had, as already noted, expected there to be continuity with Reagan's economic policies. After Bush was elected, Friedman stated that the incoming administration's top priority should be to cut the ratio of government spending to national income (*Wall Street Journal*, December 20, 1988). In Bush's first year in office, Friedman was confident that such a reduction was in prospect. He acknowledged that Reagan had reduced the government spending share less than Friedman had hoped, but Friedman judged that the reduction achieved was "striking," especially if interest payments were excluded from the government spending total.[167] With

[167] Friedman argued that interest payments were a breed apart from other government outlays because they were a pure transfer. His 1989 assessment judged Reagan in terms of the reduction in the government spending share achieved from its peak in Reagan's first term, which Friedman said largely reflected inherited programs. This represented something of a shift from the position taken in *Tyranny of the Status Quo* (for example, Friedman and Friedman, 1985, pp. 10, 39), in which the Friedmans viewed the rise in the government spending share observed through 1983 as an indication of a failure of President Reagan to achieve his goals. Part of the difference in this

regard to the years ahead, Friedman observed: "As long as President Bush sticks to his guns with respect to taxes, the ratio of spending will continue down as spending is restrained and the economy grows."[168] Friedman expected that Bush would, indeed, not acquiesce in a tax increase.

The 1990 agreement between the Bush Administration and Congress, in which a tax increase was part of the package to reduce the budget deficit, therefore represented a turning point in Friedman's assessment of the administration. Friedman was angry. "There was absolutely no need for Mr. Bush to back down on his famous [1988] statement, 'Read my lips, no new taxes.'" (*Courtlandt Forum*, March 1992, p. 75; see also *Forbes*, August 17, 1992, p. 43). Friedman complained that the tax increase disturbed firms' expectations of stable or declining tax rates and in so doing discouraged risky investment projects (*Courtlandt Forum*, March 1992, p. 75). Furthermore, from Friedman's starve-the-beast perspective, the tax increase created the precondition for a rise in the share of government spending in national income (for example, *New York Times*, February 2, 1992). He also expected that there would be further tax increases that would reinforce the momentum of spending (*Forbes*, August 17, 1992, p. 43).

In addition, as Friedman emphasized in his January 1991 talk and on later occasions, the government spending share had already started rising even before Bush's policy change regarding taxes. Friedman would characterize the rise in public spending under Bush as very substantial.[169] But in relation to GDP the rise in federal government spending under Bush does not stand out as historically large. On this criterion, total outlays are 21.3 percent in Reagan's last full year of fiscal 1988, and the numbers in subsequent years are 21.2 percent in fiscal 1989, 21.9 percent in fiscal 1990, 22.3 percent in fiscal 1991, 22.1 percent in fiscal 1992, and 21.4 percent in fiscal 1993 (Council of Economic Advisers, 2011, Table B‒79, p. 284).

Friedman's critique largely centered on the fact that, in Bush's initial years, the overall federal spending share was allowed to rise by about 1 percentage point at a time when defense expenditure's share of GDP declined by over 1 percentage point. Friedman had hoped that this decline would be accompanied by nondefense spending restraint, so that the decline in the

assessment was due to the greater government spending restraint from 1983 onward under Reagan, but it also reflected a change in the point used for comparison: in *Tyranny of the Status Quo*, the Friedmans took fiscal year 1981 as essentially a year for which Carter Administration decisions dominated government spending patterns but took fiscal year 1982 as one on which Reagan could be evaluated (Friedman and Friedman, 1985, p. 35). However, after Reagan left office, Friedman took government spending in fiscal year 1982 as basically reflecting Carter-era decisions (Friedman and Tobin, 1990, p. 76).

[168] Friedman in Friedman and Tobin (1990, p. 76).

[169] *Wall Street Week*, Maryland Public Television, February 21, 1992, p. 6 of transcript; *Frisko*, 1992, p. 68; *Forbes*, August 17, 1992, p. 44.

defense spending share would be allowed to translate into a decline in the total spending share. Instead, domestic spending initiatives had expanded and produced a growing overall government spending share. Nevertheless, members of the Bush Administration's economic team such as Michael Boskin have suggested that the degree of public spending restraint under Bush has been under-appreciated and that the 1990 fiscal agreement included considerable restriction—which was carried out—on federal nondefense outlays (Michael Boskin, interview, July 3, 2013).

Friedman's other complaints about the direction of economic policy under Bush included the fact that Bush had presided over an increase in regulatory staff and had allowed greater environmental regulation, including through his signing of the Clean Air Act.[170]

On a number of occasions after he had become disillusioned with Bush, Friedman would point to signs prior to Bush's period in office that he may have had a different economic outlook from that of Reagan. One remark that was often thrown back at Bush was his already-noted description of candidate Reagan's fiscal proposals as "voodoo economics." Friedman himself would cite this remark in his bill of charges against Bush (*New York Times*, February 2, 1992). However, insofar as Bush's phrase had been intended to express skepticism that the Kemp-Roth tax cuts would raise revenue, it was expressing a sentiment that Friedman himself shared. Perhaps a more jarring Bush comment for Friedman was the vice president's remark in a 1984 television debate, in response to criticisms of high real interest rates. Bush seemed to reject the concept of the real interest rate, emphasizing that nominal interest rates were what mattered: "The 'real' interest rate is what you pay when you go down and try to buy a TV set or buy a car, or do whatever it is."[171] This remark could not have gone down well with Friedman. There are some arguments available in economic analysis for attaching significance to nominal interest rates in borrowing and spending decisions. But to reject outright the significance of the real interest rate for those decisions ran counter to the Irving Fisher tradition that Friedman had been pivotal in reviving.

As for his direct contacts with Bush, Friedman indicated that he was unhappy that he and others associated with the Reagan era had not been approached by the White House for advice (*Forbes*, August 17, 1992, p. 44). Friedman had only a hazy impression of Bush on the strength of their 1980s encounters: "I'm sure Mr. Bush is a fine man. I don't know him, although I've met him on many occasions when he was vice president, and I was a member of Reagan's economic

[170] See Friedman's remarks in *New York Times*, February 2, 1992, *Frisko*, 1992, p. 68, and *Courtlandt Forum*, March 1992, p. 77.
[171] From the October 11, 1984, vice presidential debate between George Bush and Rep. Geraldine Ferraro.

advisory board." But these encounters were also a source of dissatisfaction with Bush for Friedman: Bush, he said, "was very quiet" at the meetings, behavior that gave Friedman little basis for judging Bush's views and that invited the interpretation that Bush lacked a strong interest in economics (*Frisko*, 1992, p. 68). As Jerry Jordan noted, however, Bush's approach was that "he would give his view and his opinion only to the president," and so he could not be expected to be vocal at the meetings at which advisers were present (Jerry Jordan, interview, June 5, 2013). This was a point that Friedman briefly acknowledged when, in his memoirs, he again complained about Bush's silence.[172] Thus it would not seem that Bush's quiet conduct at the PEPAB meetings implied a lack of interest in economics. Jordan further observed that Bush's tendency to refrain from comment at the PEPAB meetings may have been reinforced by the degree of argumentation that occurred among the PEPAB economists. But this reaction would not have set Bush apart from Reagan, as we know from Reagan's diaries that Reagan himself was taken aback by the disagreement that PEPAB members expressed with one another at the meetings.[173]

Friedman's disagreement with the Bush Administration's economic policy did not extend to its position on monetary policy. He acknowledged that the monetary restraint in force when Reagan left office had continued under Bush. Against the background of support for the overall direction of monetary policy, Bush Administration economic officials did express the view that M2 growth had been allowed to become too low for stretches of the early 1990s.[174] In some instances, this view was expressed in amicable dialogue between the Federal Reserve and key Bush Administration economists, such as the members of the Council of Economic Advisers. In other cases, however, the disagreement took a more acrimonious form, and Greenspan (2007) has made it plain that the Federal Reserve Board's relations with the George H.W. Bush Administration became very poor. Particularly galling for Greenspan was the extent to which the administration made public its view that monetary policy had been too tight.

But this criticism coincided with that Friedman himself had voiced. As someone who had long been negative about the idea of central bank independence and who shared the basic thrust of the administration's critique of current monetary policy, Friedman was not in a strong position to take issue with the fact that Bush Administration members were putting on record their views about FOMC decisions. Nevertheless, he could not have been pleased that it was Alan

[172] Friedman and Friedman (1998, p. 392); see also *Forbes*, August 17, 1992, p. 44.
[173] See the previous chapter.
[174] See, for example, Michael Boskin's remarks as reported in *Wall Street Journal*, July 29, 1991, and Dow Jones News Service, July 23, 1992.

Greenspan bearing the brunt of this pressure. In any event, Friedman took note of the administration's outspokenness on monetary policy, but he emphasized that he was not defending the Federal Reserve and that he felt the administration should not be blamed for the recession, nor could the administration be regarded as advocating an inflationary monetary policy (*New York Times*, February 2, 1992; *Forbes*, August 17, 1992, p. 44).

Friedman's position with regard to Bush's economic policy contrasted with his outlook toward Bush's foreign policy. In a January 1991 briefing to investors, Friedman declared himself a strong supporter of Bush's foreign policy, including the president's conduct of the Gulf War (Oppenheimer and Company, 1991, pp. 8–9).[175] Having indicated his support for the war, Friedman discounted its importance for the U.S. economy. "One thing is always true: people grossly overestimate the importance of events that hit the headlines and grossly underestimate what's going on behind the scenes," he said. "In terms of the economy, [the war] is a trivial phenomenon." (*Chicago Tribune*, January 26, 1991; see also *Futures*, March 1, 1991.) Friedman again referred to Bush's "strong principles in some areas like foreign policy" when, roughly a year later, he wrote an op-ed on Bush's record (*New York Times*, February 2, 1992). But this praise came with an acerbic qualification: "He clearly has none on economic policy." Friedman then proceeded to catalogue his grievances with Bush's economic record while expressing hope that Bush might have recently embraced the cause of restraint on the public sector.

Later in the year, the picture that Friedman painted of the president was centered on the judgment, "People don't change." In pursuing this theme, Friedman contrasted the small-government sensibility of Reagan with the more managerial and conciliatory approach to holding office that Friedman saw as characteristic of Bush and as having stemmed from Bush's political background (*Forbes*, August 17, 1992, p. 44). In fact, according to Bush's CEA Chairman, Friedman's assessment of Bush on economic policy was erroneous. As already noted, Boskin has emphasized the role that the 1990 fiscal agreement played in the return, from fiscal 1992 onward, of a downward path in the share of federal spending in national income. Friedman tended to be dismissive of the government spending component of fiscal agreements, like those

[175] See also Ruger's (2011) discussion of Friedman's position on the Gulf War. Although Ruger's account of Friedman's views on many issues is very far from satisfactory, including on economic matters but also with regard to Friedman's position on pre-1990 foreign policy (for examples, see Nelson, 2012a, as well as Chapters 6 and 15 above). But on the Gulf war, Ruger's account has merit, particularly as it refutes the notion that Friedman opposed the Gulf War, a notion fostered by Ebenstein (2007) and Friedman's own recollections on the basis of his foggy very latter-day memory. Friedman himself informed the present author of his support for Bush's foreign policy in the course of correspondence in 1991.

in 1982 and 1990, that consisted of combinations of tax increases and public-spending reductions—his grounds being that the spending restraint evaporated or never materialized but the tax rises (which themselves promoted future rises in spending) were locked in. However, in a retrospective evaluation in 2004, Friedman categorized the whole of the 1990s as a continuation of the slower path for nondefense federal outlays achieved under Reagan.[176] In so doing, Friedman in effect conceded that Bush's efforts at government spending restraint had been, on balance, successful. (*Wall Street Journal*, June 11, 2004.)

In 1992, however, that reassessment was far off. When, a few months before the 1992 presidential election, Friedman gave an interview to *Forbes* magazine and stated: "I believe the Bush presidency has been very close to a disaster." (*Forbes*, August 17, 1992, p. 43.) He indicated that he would nevertheless vote for Bush over Democratic candidate, Governor Bill Clinton (*Forbes*, August 17, 1992, p. 44).[177] Indeed, Friedman's criticisms during 1992 of Bush's record had been accompanied by criticism of the economic measures enacted or proposed by the Democratic-controlled Congress.[178] Earlier in the year, Friedman had criticized a letter, signed by 100 economists, and sent to Bush, Greenspan, and members of Congress, that had called for major fiscal and monetary stimulus measures (Levy, 1992).[179] During the campaign proper, in a television appearance in October 1992, Friedman voiced opposition to the Clinton campaign's job-creation plan.[180] However, in his *Forbes* interview Friedman had raised the possibility that, if elected, Clinton might turn out presiding over a reduction in the role of government. Friedman cited the fact that this had occurred under center-left governments in Australia and New Zealand in the 1980s (*Forbes*, August 17, 1992, p. 44). In the same interview, Friedman indicated that he felt it "very dubious" that Bush would be reelected (*Forbes*, August 17, 1992, p. 44). Clinton went on to defeat Bush in the election the following November.

[176] He therefore arrived at an interpretation of Bush's spending record that was not dissimilar to that Boskin has offered (for example, in *Project Syndicate*, January 16, 2014).

[177] Prior to this, Friedman had publicly contemplated voting for the Libertarian candidate. (Oppenheimer and Company, 1992, p. 9; *Wall Street Week*, Maryland Public Television, February 21, 1992, p. 9 of transcript.)

[178] See, for example, *New York Times*, February 2, 1992, and *The MacNeil/Lehrer News Hour*, PBS, March 12, 1992, p. 9 of transcript.

[179] The letter was reported in *Wall Street Journal*, March 31, 1992. See also Rasche (1993b, p. 2) for a discussion of the letter.

[180] *The MacNeil/Lehrer News Hour*, PBS, October 21, 1992, pp. 5, 13 of transcript.

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 17: Debates on International Economic Policy and Geopolitical Developments, 1987 to 1992

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I. EVENTS AND ACTIVITIES RELATED TO INTERNATIONAL ECONOMIC POLICY AND GEOPOLITICAL DEVELOPMENTS, 1987–1992

As 1987 began, an article in the U.S. business press offered an overview of the state of knowledge regarding macroeconomic policy. The contents of this *Duns Business Month* piece, including its declaration that monetary policy had lost much of its effectiveness, infuriated Milton Friedman—as he would make clear when he made reference to it some weeks after its publication.[2]

One aspect of the article that likely compounded his negative reaction was what it said about international economic policy. The piece cited "the growing internationalization of the U.S. economy" as the reason for concluding that "traditional economic policies act in unexpected ways." It also quoted Washington, D.C.-based economist John Williamson as suggesting that international considerations should shape U.S. monetary policy decisions: "It doesn't make much sense to think of monetary policy being used for purely domestic purposes." (*Duns Business Month,* January 1987.)

Friedman had, of course, suggested that such an approach absolutely *did* make sense—and he had, over a period now spanning longer than thirty-five years, advocated floating exchange rates partly on that basis. As far as Williamson was concerned, however, the results were in by 1987 on the performance of floating rates—in particular, with regard to their suitability for the United States and its major trading partners—and he judged them to be a failure. Furthermore, Williamson, who had exchanged views directly with Friedman at a March 1986 conference on

[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, George Tavlas, and Mark Wynne for their help and generosity in providing information drawn on in this chapter. The author regrets to note that, in the period since the research underlying this chapter began, Allan Meltzer, whose interviews with the author are quoted below, has passed away.
[2] See the discussion titled "Paul Krugman" at the end of this chapter.

the world economy (see Hinshaw, 1988), had long believed that Friedman's monetary analysis took international factors too far from center stage. "The two leading macroeconomic theorists of the twentieth century, Keynes and Friedman, both constructed their principal theoretical analyses for closed national economies," Williamson had written in his 1983 textbook account. "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."[3] The title of Williamson's text—*The Open Economy and the World Economy: A Textbook in International Economics*—and the name of the organization at which he was based—the Institute for International Economics—both aptly reflected Williamson's desire for a more internationally-focused U.S. economic policy.

Those who came into 1987 urging a greater role for international factors in the setting of monetary policy had reason to be pleased by the outcome of the Group of Seven (G7) summit early in the year. The meeting—held in Paris, but known as the Louvre summit because of its specific venue—convened on February 21–22, 1987 (Frankel, 1994, p. 306). Out of it came an agreement by the participating countries (other than Italy) on the appropriate course of the U.S. dollar. A declaration regarding what should happen to the U.S. exchange rate had come from an economic summit previously—in 1985's Plaza agreement. But, in Friedman's assessment, "The Louvre agreement was more important" (Hinshaw, 1993, p. 67). This was the case because the Louvre accord, rather than simply expressing a view about what course the dollar exchange rate should follow, involved an agreement by most G7 countries to target exchange rates to achieve the desired outcome. The specific aim of the Louvre accord was to arrest the dollar decline that had begun in early 1985 and that had accelerated since then.

In the event, for several members of the G7—the United States, Canada, and Continental European countries—the Louvre accord had only a minor and ephemeral effect on economic policy. The Canadian dollar and the Continental European countries largely continued to float against the U.S. dollar—the latter countries doing so jointly, as part of the Exchange Rate Mechanism (ERM). Frankel (1994, p. 306) noted that the published Louvre agreement "contained little hard information," and Friedman's later assessment was that the pact led to actions that only held back dollar depreciation for about eight months (*Wall Street Journal*, September 22, 1992). In particular, the agreed exchange-rate target ranges, which were not published and so did not become official, were, in turn, dropped by the various governments after some time, again without a public announcement (Krugman and Miller, 1993, p. 280).

[3] Williamson (1983, p. 372).

Consequently, a year after the Louvre accord, Friedman poured scorn on a recent public statement made by France's finance minister that the "Louvre agreements marked the end of the floating-exchange-rate system"—Friedman's response being: "If so, the markets have not yet noticed." On the contrary, Friedman suggested, the floating-rate system would not end for "a very long time, if ever" (*Wall Street Journal*, March 4, 1988).

The increased weight on exchange-rate stability implied by the accord did, however, have more major repercussions on the shaping of monetary policy in two other G7 economies. The aftermath of the Louvre meeting saw the U.K. authorities put in place an informal pegging of the pound against the mark. Stabilization of the sterling/mark exchange rate, against a background of an ostensibly floating pound, continued to be a U.K. policy concern through October 1990, when, with the country entering the ERM, a fixed exchange rate became official, and a formal target band was instituted. The policy of a stable exchange rate against the mark led, in the United Kingdom, first to a loosening of monetary policy that revived inflation in the late 1980s and, later, to a very restrictive policy stance, associated with a prolonged recession and then the country's exit from the ERM in September 1992. In a commentary on the early effects of the United Kingdom's pegging policy, Friedman cited the episode as a "cautionary tale" about the consequences of fixed exchange rates (*National Review*, June 11, 1990*a*, p. 30). The United Kingdom's later departure from the ERM not only confirmed a prediction Friedman had made in *Money Mischief* in 1992 that the country would likely abandon the ERM or realign its target, rather than go along with intensified monetary restriction.[4] It also reinforced Friedman's long-expressed reservations about the ERM. It "seems to me a fundamentally unstable arrangement," he had remarked in early 1991 (Hinshaw, 1993, p. 9). After the U.K. ERM exit, Friedman deplored the *Wall Street Journal*'s continuing support for fixed exchange rates and, writing in that newspaper, asked rhetorically how "many more fiascoes will it take" before a wider consensus against fixed exchange rates materialized (*Wall Street Journal*, September 22, 1992).

It was the most prominent critic of the United Kingdom's pegged-rate experience, fellow monetarist Alan Walters, who suggested that an even better current example of the macroeconomic costs of pegging was available to Friedman. This was that of Japan (*National Review*, June 11, 1990*b*). The boom-and-bust cycle that followed the changes in Japan's macroeconomic policy in the wake of the Louvre agreement was more drawn out than the corresponding U.K. experience, and it did not feature a dramatic episode comparable to the ERM

[4] Friedman (1992c, p. 247). At the dawn of the ERM, Friedman had remarked that it "may work for a while if the intervention limits are flexible enough, but it will fail eventually" to link the countries' currencies, unless superseded by monetary union (*Irish Times* (Dublin), March 26, 1979).

exit. But it was a policy odyssey that would transform Friedman's assessment of Japan's economic management. On American network television in November 1987, Friedman remarked that "the Japanese… [have] had a better monetary policy than we have" since the mid-1970s.[5] In contrast, ten years later, he opened a *Wall Street Journal* op-ed (December 17, 1987) by observing: "At decade of inept monetary policy by the Bank of Japan deserves much of the blame for the current parlous state of the Japanese economy."

The contrast between what Friedman called the "golden years" for Japan of 1977 to 1987 (*Wall Street Journal,* December 17, 1997) and the subsequent decade had its origins, he believed, in the Louvre agreement, which prompted heavy unsterilized intervention to hold down the yen.[6] There followed a major inflationary boom in Japan.[7] The subsequent policy tightening was felt in both higher short-term interest rates and slower monetary growth—but not immediately, of course, in economic activity: a mid-1991 analysis in *The Economist* noted that "the Japanese economy is still growing rapidly while its money supply has virtually stopped expanding." This piece was sanguine about Japan's economic prospects: "Most economists…would welcome a slowdown [in the economy] and (with hindsight) have welcomed the Bank of Japan's moves in the past two years to raise its discount rate from 2.5 percent to 6 percent... [and] the country can afford to risk a few quarters of slow growth" (*The Economist* (London), June 22, 1991). In the event, Japan's slow monetary and economic growth lasted years, not just quarters—with Friedman reporting average output growth in the five years to mid-1997 of only 1 percent and of M2 growth, only 2.1 percent (*Wall Street Journal,* December 17, 1997).

Posen (2010, p. 6) would later claim: "The idea that Japan's difficulties were serious and demand-related was first advanced independently by Krugman (1998) and Posen (1998)." Posen was mistaken. Earlier than 1998, Milton Friedman was among those who traced Japan's difficulties to aggregate demand restriction—including in a *Wall Street Journal* article (October 23, 1992) in which he noted that Japan was a country in which "M2 has slowed sharply in recent years, and so has the growth of the economy."[8]

[5] *Nightline,* ABC, November 6, 1987, p. 21 of transcript.

[6] See his comments in *Wall Street Journal,* September 22, 1992, and in Ragan (1999 p. 52). Bernanke and Mishkin (1992, p. 214) similarly stressed a major change in the behavior of Japan's monetary policy in the late 1980s. The basic outline of future years' developments had been anticipated in Meltzer's (1986, p. 145) closing remark: "Past experience in Japan suggests that a renewed attempt to control or influence exchange rates, should it occur, will increase variability, reduce the predictability of prices and output[,] and lower welfare."

[7] Friedman had visited Tokyo in 1988 during the boom phase and had criticized the state of affairs of "nominally floating exchange rates that are in practice manipulated by governmental central banks… We do not need international coordination among central banks." (Friedman, 1988f, p. 202.)

[8] As noted, Friedman covered Japan's record at length in 1997. This article's title, unusually for a Friedman piece, referenced a motion picture (1985's *Back to the Future*). Krugman's (1998) own article on the matter also referenced a movie (1986's *Poltergeist II*).

Resolution of the Latin American debt crisis

As discussed in Chapter 15, the Latin American debt crisis—a major strain on the so-called money-center banks in the U.S. commercial banking system during 1982–1984—eased considerably from 1984 onward even as the fact of major debts continued. Speaking in summer 1988, Friedman was confident that concerns would continue to de-escalate. He has remarked in 1963 of banking practices that involved "changing the form of loans to make the figures look good."[9] Twenty-five years later, Friedman believed that the rescheduling and other changes made in response to the Latin American banking crisis were another example of this phenomenon: "Don't look at the artificial accounting books of the banks… [T]hose foreign loans are only going to be collected in part… There will continue to be renegotiations and covering up." But the outcome would be mundane: "There aren't going to be overall defaults."[10]

This scenario was the one realized. In late 1992, the World Bank's Constantine Michalopoulos remarked that, although in 1982–1983, "there was severe concern… [that] either the international financial system would collapse because of the very severe indebtedness of the developing countries; or the commercial banks, whose portfolios had too much debt, would need to be rescued." In fact, Michalopoulos went on to note, neither event materialized, and although serious "difficulties in a number of banks"—most notably in the case of the Continental Illinois bank on which Friedman focused (see Chapter 15)—did emerge, it turned out that "no massive bailout of the commercial banking system was necessary."[11] Michalopoulos cited as factors preventing a larger-scale crisis emerging from the 1982 emergency the U.S. commercial banks accepting some losses on loans, as well as both internal adjustment by, and the provision of additional funds to, the debtor governments.

U.S. international debt and national saving

Friedman lamented in early 1991 that "there is all the talk about the United States being a major debtor nation."[12] The official statistics had, over the course of the 1980s, indeed shown the United States move from net creditor to net debtor—and then to major net debtor. Friedman's belief was that "we aren't, [but] lousy statistics make it appear that we are."[13] His criticism of the statistics in part rested on his belief that assets abroad owned by U.S. residents, and the assets'

[9] From Friedman's testimony of November 14, 1963, in Joint Economic Committee, U.S. Congress (1963, p. 458).
[10] From his remarks of July 15, 1988, in Friedman (1988c, p. 381).
[11] Remarks of October 29, 1992, in Michalopoulos (1992, p. 345).
[12] In Hinshaw (1993, p. 22).
[13] Friedman (1988c, p. 380).

value, were "greatly underestimated"—a contention that he felt was supported by the large flows of investment income U.S. residents were receiving from international sources (*Christian Science Monitor* (Boston), July 2, 1987). He did not, however, deny that the United States was running current account deficits—a situation that, if it continued, likely made it only a matter of time before the United States did unambiguously become a debtor country. As he had during the high-dollar years (see Chapter 15), Friedman was not concerned about this eventuality, as he felt it likely attested to the United States' status as a safe and attractive location for funds. "I have no objection to being a debtor nation—that just means we are using other people's capital, as well as our own," he remarked in July 1988.[14] "What the trade deficit means," he had affirmed several weeks earlier, "is that the U.S. is a better place [for a foreign resident] to invest [in] than one's own country. What's wrong with that?" (*The Orange County Register* (Santa Ana, California), May 26, 1988.) In the closing months of the decade, he affirmed that it had "always been a puzzle to me" that observers had regarded as "a sign of weakness" in the U.S. economy the fact that "[f]oreigners have found the United States… a better place to invest than elsewhere in the world." (*Las Vegas Review-Journal* (Nevada), July 18, 1989.)

Friedman was similarly largely untroubled by data suggesting historically low rates of national saving. In a short piece, "What Is the 'Right' Amount of Saving?" published in mid-1989, Friedman prefaced his answer by again voicing criticism of the official statistics. In this case, he cited two factors that likely biased downward official estimates of the national saving rate: first, the statistics counted durable goods spending as consumption (a practice Friedman had challenged in *A Theory of the Consumption Function*) and "the exclusion from income and hence from saving of any capital gains, whether nominal or real." (*National Review*, June 16, 1989, p. 25.)[15] He nevertheless expressed a willingness to take the figures at face value when answering the question he had posed. Friedman's conclusion: Provided that government-imposed, or other, distortions were not lowering the saving rate, then a low saving rate should not be regarded as a policy problem—even a zero net saving rate, and even if this implied no long-run economic growth. (*National Review,* June 16, 1989, pp. 25–26.) As a practical matter, however, it is clear

[14] Friedman (1988c, p. 380).

[15] See Nelson (2020a, Chapter 5) for a discussion of Friedman's treatment of purchases of durable goods. On Friedman's contention regarding the need to include capital gains, a related point was articulated by Obstfeld and Rogoff (1995, p. 1735): "Unfortunately, the saving and investment flows reported in national income and product accounts (NIPA)… don't always conform closely to theoretically correct concepts of saving and investment, particularly when international capital mobility is extensive. One especially serious defect is the failure of NIPA national income measures fully to reflect capital gains and losses…" Similarly, Dekle and Summers (1991, p. 71) stated that "income and saving should include real capital gains." Dekle and Summers associated this edict with what they called the "Haig-Hicks-Simon (HHS) concept of income" (p. 71)—although their naming of "Simon" was apparently intended to refer, in fact, to Friedman's University of Chicago predecessor Henry Simons.

from his statements both before and after 1989 that Friedman *did* believe that government policies were discouraging productivity growth and, correspondingly, he likely did feel that U.S. saving was too low. That said, he rejected, as both ineffective and undesirable, one of the most prominent devices (tax increases) that commentators advanced during the Reagan and Bush years as a means of increasing national saving.[16]

II. ISSUES RELATED TO INTERNATIONAL ECONOMIC POLICY AND GEOPOLITICAL DEVELOPMENTS, 1987–1992

BEYOND THE COLD WAR

"The Berlin Wall is a dramatic monument to the superiority of the free economy," Friedman observed in the *New York Times* on October 11, 1964 (p. 35). Just over twenty-five years later, on November 2, 1989, the wall was brought down by Germans on both sides of the structure. This occurred as part of the liberation of Eastern Europe—a revolutionary process that had been underway over much of 1989.

The fall of the Berlin Wall and the surrounding events in Eastern Europe and the Soviet Union were largely a surprise to Friedman, as they were for countless others. These developments caught him unawares even though he had a long record of emphasizing the poor performance of centrally planned economies. In doing so, Friedman had frequently cited the German case specifically. When talking about the historical evidence indicating that sustained economic advance was uniquely associated with private-sector-focused market arrangements, one of the devices Friedman had frequently used was to "contrast West Germany and East Germany" (*Human Events*, July 2, 1966, p. 8).[17]

This example had been available since the late 1940s, with the economic success of the Federal Republic of Germany (whose success continued the economic growth that had followed liberalizing reforms made when the Western German region was still an occupied zone). But the contrast had been rendered more graphic by the Communist authorities' construction of the Berlin Wall in 1961, separating the citizens of the so-called German Democratic Republic (that

[16] One analysis that was along the same lines as the commentary to which Friedman was opposed was that of Dekle and Summers (1991, p. 77), who combined their concern about low U.S. saving with a less-than-prescient observation about Japan's prospects for real growth: "Unless policymakers urgently address the issue of low American saving, American real economic growth may continue to lag behind Japan's."

[17] He made a remark of this kind to a (West) German readership in 1971 (in a piece that appeared in its original English-language form as Friedman, 1994a, p. xvi).

is, East Germany) from the most physically accessible part of the Federal Republic of Germany. The reason the Soviet and East German leaders had installed the Berlin Wall, Friedman observed, was that, if offered the opportunity, populations "vote with their feet" for a democratic market economy over an undemocratic controlled one. "The fact that East Germany had to build a wall to keep people from going to West Germany is striking evidence of which country had the better conditions of life," he remarked.[18]

This was an example Friedman invoked again in May 1985, a couple of months after Mikhail Gorbachev took office as head of the USSR—becoming, in effect, head of what the Soviet Union called the "socialist commonwealth" and what Friedman had called the "Russian empire" (*Newsweek*, November 10, 1980): the East bloc of the Soviet Union and its satellites, including East Germany. "On one side of the Berlin Wall is a relatively free economic system; on the other side, a collectivist society," Friedman observed in his 1985 remarks, delivered in Texas.[19]

Friedman was, more than most, aware of the severe failings of nonmarket systems—and in particular of the conceptual and practical weaknesses of the planned-economy model. He had never subscribed to the notion that the Communist countries were serious economic competitors against the United States or the Western world. But Friedman did not anticipate the freeing of Eastern Europe, the process of political liberalization in the USSR, or the collapse of the Soviet economy. He had viewed the Soviet bloc nations as being economically moribund but had not perceived them as being in a terminal economic or political state.

Strains on the Soviet economy

Nor had the Reagan Administration, which left office at the start of 1989, regarded the collapse of the Soviet bloc as either imminent or inevitable. During its tenure, the administration had

[18] In Friedman (1983a, p. 63), a discussion adapted from Friedman (1976a, p. 6). Friedman also used contrasts in relative positions to illustrate the effect of the Communist system on Eastern European countries. In particular, he noted in a 1991 panel discussion that Czechoslovakia in 1938 had living standards quite comparable with those prevailing in Western Europe—but fifty years later no longer did so. "It's an enormously powerful example of the effect which the wrong institutions can have. Because what produced that? Communism—central collective planning." In response to this, a Czech panelist (though an official in the government that had replaced the Communist one) felt obliged to defend the 1988 living standards of Czechoslovakia, arguing that they had held up better than was the case elsewhere in Eastern Europe. (In the videotaped panel session, "Out of the Red: Economic Transition in Central and Eastern Europe," Hoover Institution, May 1991.)

[19] In Friedman (1986a, p. 89; p. 91 of 1988 paperback edition). Friedman's remark would subsequently be quoted by Collier and Siebert (1991, p. 196) in an article on German reunification that appeared in the proceedings issue of the *American Economic Review* in May 1991 (an issue that immediately followed the one that included Friedman's final contribution to that journal).

maintained the longstanding U.S. government position that desirable outcomes for the world included the restoration of *bona fide* national sovereignty and democracy in the Eastern European countries as well as the reunification of Germany. But these were statements of aspirations, and the administration did not have a concrete strategy seeking the dissolution of the Soviet Union or decoupling the USSR from its satellites.[20]

For one thing, of course, neither the Reagan Administration nor its predecessors had the aim of ending Soviet control of Eastern Europe by military means. Indeed, the fact that the United States had a political consensus that both favored the freeing of Eastern European and rejected military conflict as a means of achieving this outcome had worked against the 1964 Barry Goldwater presidential campaign on which Friedman had served. Goldwater had expressed a wish for ending the division between Eastern and Western Europe as a long-term goal in his speech accepting the Republican party nomination (see Goldwater, 1964). In making this statement, Goldwater was not taking a stand that was out of line with existing, official American policy. But Friedman believed that Goldwater came to be stuck with the perception that he, if elected president, would provoke an all-out war with the USSR (see Nelson, 2020b, Chapter 12).

In addition to not seeking military conflict in Europe, the Reagan Administration, although aware of the economic weakness of the Soviet bloc, did not have a strategy aimed at using this weakness to produce the breakup of the USSR or the loss of its Eastern European satellites. Certainly, the motivation of generating additional strains on the Soviet Union's economy did shape the Reagan Administration's policies, particularly in its first three years in office. But this effort was not undertaken with the intention or expectation of generating a collapse of the Soviet economy or forcing the USSR to end its hold on Eastern Europe. Rather, the plan to intensify economic strains was designed to make the Soviet Union more amenable to negotiated arms reductions and to reduce its global ambitions, as reflected in its military and other activities in the Third World. The rationale for a Western domestic military buildup was expressed along those lines by Robert Conquest, a historian who in 1981 would become a senior fellow at the Hoover Institution. In a Hoover Institution Press-issued book, Conquest (1979, p. 27) stated that "the Soviet economy is grossly overstrained in keeping up its present armament" and so a strengthening of Western military spending was unlikely to be matched by the USSR.[21] The aim of such a buildup would be "to convince the Soviet leaders that a forward policy [that is, expanding Soviet influence beyond Eastern Europe] cannot be sustained." President Reagan

[20] See Garthoff (1994, p. 777; 1995).

[21] Similarly, Gay and Payne (1981, p. I–71) contended during roughly the same period: "The level of Soviet defense spending probably is determined very largely by constraints imposed by economic imperatives."

himself articulated a similar line of reasoning in October 16, 1981, when he stated that Soviet citizens were "on a starvation diet," so a large increase in USSR military allocations was ruled out—a situation that in turn raised the prospect that a U.S. defense buildup would lead the Soviet Union to "meet us realistically on a program of disarmament" (quoted in Garthoff, 1994b, p. 11).

With regard specifically to the peaceful dissolution of the Soviet Union, Garthoff (1995, p. 198) noted that "no one in the first half of the 1980s expected that outcome." Reagan himself stated in the second televised presidential debate of 1984: "I have said on a number of occasions exactly what I believe about the Soviet Union. I retract nothing that I have said… But I also recognize that as the two great superpowers in the world, we have to live with each other… [W]e don't like their system. They don't like ours. And we're not going to change their system..."[22]

Friedman, too, was not in the 1970s and 1980s a prominent exponent of the view that the Soviet system was bound to collapse in the foreseeable future. After the fact, he remarked: "It was going to collapse sooner or later. Its internal structure was irrational." (*San Jose Mercury News* (California), November 5, 2006*b*.) But he gave little indication before the late 1980s of thinking that such a collapse was anything other than a very long-term prospect. The distortions associated with planned economic systems were the reason Communist countries "have such low standards of living; that is why they are so inefficient," he observed in 1981.[23] But he saw such command systems as capable of continuing, albeit in a growth-free fashion. Karl Hess, who had worked with Friedman on the Goldwater campaign, observed in the mid-1970s: "I remember one time that we worked together on a project, Milton said about capitalism or any other system [that] they *all* can survive, it just depends on how much you want to put into it—it's the cost that's the question." (*Donahue*, NBC, September 30, 1975.)

For similar reasons, Friedman was not particularly sympathetic to the idea that defense spending had by the early 1980s reached a near-ceiling in USSR. Having the economist's perspective that allocations could usually always be varied on the margin, he believed that still more increments in defense efforts could be eked out of the total resource pie in an economy. And notwithstanding Reagan's 1981 statement that expansion of Soviet defense resources might be close to a limit, the Reagan Administration was itself not altogether cohesive on the issue, and some of the rationale for its own military buildup and attempted expansion of capabilities (such

[22] "The Second Reagan-Mondale Presidential Debate," October 21, 1984, p. 3 of transcript.
[23] In Friedman (1984e, p. 8).

as the SDI) seemed to design to force more Soviet expenditures and thereby amplify the strain of maintaining a high share of military spending in total output.[24]

In the event, the Soviet government did not engage in a military buildup in the 1980s on the scale seen in the United States during that decade. As one description of the modern research consensus put it, "Reagan's buildup didn't really increase the Soviets' military burden."[25] The USSR's defense expenditures rose as a share of output in the early 1980s, but the share was basically steady from 1985 onward, and the cumulative percentage increase in the spending level during the 1980s was much less than that seen in the United States. The U.S. defense buildup seems to have prompted the USSR to keep up its military efforts until the late 1980s (when a policy of cutbacks began) but not to *step them up* dramatically.[26]

But even though military spending did not rise very strikingly in the Soviet Union during the 1980s, the country's share of defense spending in output nevertheless became a greater strain on the economy because the denominator of this share—national production—registered so poor a performance during the decade. The Friedmans had remarked at the end of the 1970s that, although once erroneously thought to be a productivity dynamo, "The Soviet economy is hardly a model of efficiency now."[27] That economy then underwent a further decided slowdown in the average growth rate in the 1980–1985 period (Fischer, 1994, p. 229). It was against this background that U.S. Secretary of State George Shultz remarked in early 1985 on the fact that "the Soviet empire is weakening under the strain of its own internal problems and external entanglements."[28] Defense had not, in the event, commanded a much greater slice of the total resource pie—but the pie itself was no longer expanding as it once did.

Subsequently, over the rest of 1985 and during 1986, the USSR's economic problems were intensified by a development that Friedman was well known for having predicted: a collapse in the world oil price to mid-single-digit dollar values. Stanley Fischer would later note that, on account of its implications for the USSR's terms of trade, "many Russian economists attribute the collapse of the Soviet Union to that price [decline] and not to the difficulty of running the system."[29]

[24] See, for example, Garthoff (1994, p. 765) and Schweizer (2002, pp. 142, 154).
[25] Nintil, "The Soviet Union: Military Spending," blog entry, May 31, 2016.
[26] See Garthoff (1994b, pp. 506, 764).
[27] Friedman and Friedman (1980a, p. 10).
[28] Quoted in Garthoff (1994b, p. 693).
[29] Remarks of December 17, 2014, in Federal Open Market Committee (2014, p. 122).

Aid, advice, and the end of the USSR

Speaking in July 1989—prior to many of the year's major events in Eastern Europe—Friedman considered the case of the Soviet Union itself. In response to questions about Mikhail Gorbachev's attempts to reform the USSR's economic system, Friedman observed that "an improvement of the Russian economy enables it to strengthen its military forces and resume its centuries-old push outward. And that is not in the interest of the U.S."[30]

Concern that economic reform might make the USSR militarily stronger was also frequently expressed by U.S. defense officials in the late 1980s.[31] The tone that it embodied contrasted with the support for Gorbachev's reform agenda being articulated by many commentators in the United States—including, in 1989, by former president Reagan.[32] Friedman's own expression of concern about the dangers associated with a successful Soviet economic transformation may have been influenced heavily by the case of China—a country whose experience he had been discussing at the same July 1989 event, and whose economic reforms had been accompanied by continuing hardline political control and, most recently, by the Tiananmen Square clampdown.

Nonetheless, in referring to military strengthening and further geopolitical expansionism as possible consequences of USSR economic reform, Friedman invoked eventualities that proved to be the opposite of those that occurred. Having already made concessions in arms negotiations with the West, Mikhail Gorbachev made numerous unilateral reductions in military forces and commitments in the late 1980s and also allowed the Eastern European liberation process to go ahead. Friedman was also not prescient in suggesting in 1989 that "a major political change… is unlikely" in the Soviet Union, as internal political liberalization did proceed under Gorbachev.[33]

Friedman was closer to being on the right track when he expressed doubt during 1989 and 1990 that the Soviet Union would really follow China's post-1978 path of economic liberalization.[34] In

[30] Remarks of July 21, 1989, in Friedman (1989a, p. 369).
[31] For example, the U.S. Department of Defense's publication *Soviet Military Power* stated (1988, p. 35): "To the extent that the new economic reforms are successful, they will, in the long run, benefit the military by providing a modernized production capacity and a reliable supply of high-quality raw materials, subcomponents, and other products necessary for advanced weapon systems." The following year's edition of the publication similarly contended (U.S. Department of Defense, 1989, p. 35): "Gorbachev's economic modernization program offers the prospect of long-term benefits for the defense industrial sector." See also Garthoff (1994b, p. 777).
[32] Reagan remarked about five months after leaving office: "I believe Mikhail Gorbachev is the Soviets' best and probably only hope to turn things around." (*Los Angeles Times*, June 13, 1989.) See also Garthoff (1994b, p. 378).
[33] The remark was in Friedman (1989a, p. 369). He had already made this false prediction in 1988. See the previous chapter.
[34] In addition, although Friedman was usually reluctant to assign national culture a role in whether a country was amenable to a market economy, he did imply that China's national character might allow it to get more benefits out

his July 1989 appearance, he remarked that "I am no expert on Russia" but expressed doubt that Gorbachev could make "sweeping changes."[35] On this point, Friedman was partly right, as Gorbachev's economic reforms, although certainly on a grand scale, did not amount to true economic liberalization, and the botched reform accelerated the collapse of the Soviet economy.

Before it was clear that Gorbachev's economic reform measures had gone awry, Friedman admitted frankly to having "mixed feelings" about the prospect of the USSR economy getting on the right track and announced that he would abjure supplying public or private advice on how the Soviet Union should proceed: "I went there 25 years ago. I have had repeated invitations to go back, but I have never been willing to do so… I don't intend to give any advice to [the] Soviet Union, they can do without my advice."[36]

Friedman broke this self-imposed ordinance on June 4, 1990, when George Shultz—who had joined the Hoover Institution in 1989 after his years in the Reagan Administration—organized a luncheon for Mikhail Gorbachev, held at Stanford University's business school. At the event, senior academics affiliated with the university, Friedman among them, offered their advice to the Soviet president.[37] Just days earlier, on June 1, Friedman had remarked publicly: "Mr. Gorbachev is a dedicated Communist. It is not clear to me that he wants to do what I would want to do—which is to convert the Soviet Union into a free private market economy. All three of those words are necessary."[38] Despite his subsequent pleasantries when meeting Gorbachev, this remained Friedman's view.

Friedman also opposed economic aid to the USSR—in doing so, reaffirming his longstanding position that aid tended to strengthen the public sector's role in the economy.[39] Many U.S. economists did not share his opposition. Stanley Fischer, for example, suggested that it could be beneficial: "The economic potential of the Soviet Union is enormous." Fischer nonetheless underscored the scale of the task facing those providing aid and advice: "Those of you who have spoken to the people from the republics will understand the extraordinary lack of economic

of a market system than was the case for the Soviet Union or Russia (*Forbes*, December 12, 1988, p. 168; Friedman, 1989a, p. 369).

[35] Friedman (1989a, p. 369).

[36] Friedman (1989a, p. 369).

[37] For the date of the event, see *San Francisco Chronicle*, June 5, 1990. The occasion was subsequently mentioned in Shultz's letter to Friedman of June 7, 1990 (available in Box 179, Milton Friedman Papers, Hoover Institution).

[38] Friedman (1990c, p. 388).

[39] Friedman (1990c, p. 388). In the same discussion, he also indicated that he opposed the idea that the United States should provide subsidized credit to emerging private-sector ventures in the USSR. Friedman also discussed the appropriate next steps for the Soviet economy in a 1990 Radio Free Europe interview (see Bush, 1991).

sophistication or understanding of the politicians—and, indeed, of the economists... They need technical assistance. They may not listen to it. But they certainly should get it—and be helped to avoid their worst instincts." (CSPAN, October 28, 1991.)

Friedman's own frosty attitude toward the Soviet Union and Russia had melted only a little. "I have been repeatedly asked to go to the Soviet Union and give advice, and I have repeatedly refused… I was there in 1962, and I have no desire to go back," he remarked. He also commented acerbically that there should be a moratorium on "Jim Baker [going] to the Soviet Union," although he also indicated that Baker (U.S. Secretary of State at the time) was only one example: "We're doing a great disservice by sending [over] too damn many advisers." Friedman's feeling was that the U.S. government's advice on how economic reform should proceed was not market-oriented enough (*Frisko*, 1992, p. 69). In these remarks, Friedman slipped into the habit of referring to the country to which the United States was offering advice during 1992 as the Soviet Union. In the same remarks, however, he also used the term he actually meant: "Russia"—the Soviet Union having been dissolved at the end of 1991.

Eastern Europe

In contrast to his attitude toward advising Russia, Friedman was far better disposed toward providing economic advice to the former Soviet satellites in Eastern Europe and even did so via an on-the-ground visit in 1990. The *Free To Choose* series being made that year, for transmission on CNBC in 1991, consisted mainly of repackaged versions of some episodes broadcast in 1980, with a selection of the original series' filmed portions now accompanied by newly recorded debate portions. But a special all-new episode was made for the series on the topic of the reform of Eastern Europe's economies.[40]

The filmed portion of this episode was based on Friedman's trips to Hungary, Czechoslovakia, and Poland in the middle weeks of September 1990.[41] In keeping with the format of the original

[40] *Free To Choose* (revamped U.S. version), Episode 3, "Freedom and Prosperity," CNBC, February 24, 1991. Owing to the fact that the filmed parts of the relaunched series had been broadcast previously on public television starting in 1980, this all-new episode was given in some published listings (such as *Billings Gazette* (Montana), February 23, 1991) as a rerun—which it was not. The newspaper synopsis of the episode described it as one in which "Ronald Reagan speaks about free markets as an answer to the Eastern European economies." (*Billings Gazette* (Montana), February 23, 1991.) In fact, Reagan's participation was limited to a brief videotaped introduction to the episode.

[41] The September 1990 date was not specifically given in the Friedmans' account in Friedman and Friedman (1998, pp. 505–514). But Friedman gave it in *Wall Street Journal*, January 8, 1991, and in Hinshaw (1993, p. 49), and Milton Friedman in Friedman and Friedman (1998, p. 514) also implied that their week in Poland ended on September 20, 1990. The trip to Eastern Europe followed the Friedmans' attendance of a Mont Pelerin Society

Free To Choose series, the episode included not only on-camera exposition by Friedman but also off-screen narration by him of vignettes of expanding marketplace activity in Eastern Europe. Some of this material covered Czechoslovakia. Friedman's narration regarding that country named one of the figures on his own, highly-free-market side of the debate on economic reform: its finance minister Vaclav Klaus, who also spoke to Friedman in an insert in the filmed program.[42]

By the time of the making of this new *Free To Choose* episode, Friedman had already written a short piece on the steps that the Eastern Europe countries—other than East Germany, which he excluded because of the prospect of its integration into the Federal Republic—should take to free their economies. He did so in the conservative magazine *National Review* (May 14, 1990). It was, however, a slightly later Friedman *National Review* article (June 11, 1990*a*)—one mainly concerned with the Western economies—that actually had Vaclav Klaus participate, as an invited commentator, as part of a symposium on Friedman's article. As had a similar piece Friedman wrote in the London *Financial Times* six months earlier (December 18, 1989), Friedman's 1990 article on international economic arrangements had come out strongly in favor of floating exchange rates and suggested that members of the Exchange Rate Mechanism like France could have achieved disinflation on their own (by floating and instituting domestic monetary restraint) without being part of the ERM. Klaus' comment stated that Friedman's "Case for Flexible Exchange Rates" was "a starting point for discussions of international monetary reform both in the world in general and in the former Comecon world in particular."[43]

Friedman himself engaged during the early 1990s on the issue of the appropriate exchange-rate arrangement in Eastern Europe. He had some favorable things to say about Eastern European countries like Poland moving to a system in which they linked base money issuance to the central bank's holdings of marks and dollars.[44] In view of the fact that countries like Poland were larger than the very small states (like Hong Kong) for which he had in the past seen currency boards as appropriate, this was a slightly jarring recommendation. The fact that he articulated it

meeting in Munich (Friedman and Friedman, 1998, pp. 505–506), and that meeting was on September 2–8, 1990 (Winiecki, 1990, p. 789).

[42] *Free To Choose* (revamped U.S. version), Episode 3, "Freedom and Prosperity," CNBC, February 24, 1991, p. 4 of transcript. See also Friedman and Friedman (1998, p. 506).

[43] *National Review*, June 11, 1990c. Klaus incorrectly referred to the essay as having been Congressional testimony. (Friedman, 1953a, had originated in part from a U.S. government memorandum, but it was not Congressional testimony.) He may have confused its origin with that of Friedman's much-reprinted testimony on exchange rates given to Congress' Joint Economic Committee (1963).

[44] Comments he made to this effect in January 1991 appeared in Hinshaw (1993, pp. 70–71). Those he gave in May 1991 remarks were mentioned by A. Anderson (1995, p. 154).

partly reflected Friedman's view that newly formed or reformed central banks in Eastern European countries would lack the credibility to set up a stable domestic monetary framework (Hinshaw, 1993, p. 71). Nonetheless, as it became clear that these countries were seeing their main choice as being between fixed and floating rates and were not treating a currency board as a leading option, Friedman underlined his preference for floating when he noted in May 1991 that "the only feasible way to have a currency convertible is by allowing the exchange rate to be a free-market price."[45]

With regard to reform of the goods-and-services sectors, Friedman had a longstanding position: "The way to have a free economy is to have a free economy." (*American Banker*, June 15, 1973.) In his discussion of Eastern Europe, he therefore urged a large number of non-phased reforms: immediate decontrol of prices, abolition of restrictions on imports and exports, legal protection of private property, and the right to sell property on free terms.[46]

When it came to the state-owned enterprises, the recommendation Friedman made in 1989 in the context of China and the Soviet Union applied also to Eastern Europe: "privatize, privatize, privatize."[47] Here he granted that there was an element of gradualism necessary: "you can't do that overnight."[48] Friedman's writings during 1990 and 1991 on how privatization should proceed gave more elaborated versions of his somewhat cryptic remarks in the *Free To Choose* episode: on Eastern Europe, "The assets of Hungary belong to the people of Hungary. I do not believe they should be sold."[49] This statement did not signify a belated conversion on Friedman's part to the case for public-sector ownership. It was, instead, a reflection of Friedman's desire for what he called his "favorite form of privatization."[50] This was a form that he had advocated prominently since the 1970s for the Western world—one in which privatization occurred through the issuance (at no charge) of marketable equity holdings in previously state-owned firms to the country's entire citizenry.[51]

[45] From Friedman's May 1991 remarks in the taped panel session, "Out of the Red: Economic Transition in Central and Eastern Europe." This event, already mentioned above, was part of a conference on Economic Transition in Central and Eastern Europe held at the Hoover Institution on May 8–10, 1991 (A. Anderson, 1991, p. 162).

[46] *National Review*, May 14, 1990, pp. 34–35. Friedman also predictably opposed U.S. government economic aid to Eastern Europe (p. 36).

[47] Friedman (1989a, p. 368).

[48] Friedman (1990c, p. 388).

[49] *Free To Choose* (revamped U.S. version), Episode 3, "Freedom and Prosperity," CNBC, February 24, 1991, p. 3 of transcript.

[50] Friedman (1989d, p. 577).

[51] This attitude had led to Friedman being dubbed the "give-away professor" (*Evening Standard* (London), February 28, 1980). For Friedman's advocacy of this privatization procedure specifically in the case of Eastern Europe, see *National Review*, May 14, 1990, p. 35.

Political upheaval

Friedman's natural focus was on the economic side of Eastern Europe's situation—its move away from a centralized economic system. "The marketplace won," he remarked when asked at the end of the 1980s how to sum up the message of world developments in that decade (*Daily Republican Register* (Mount Carmel, Illinois), December 29, 1989). This economic outcome was indeed an epochal event—and it contrasted dramatically with a declaration that Mikhail Gorbachev had made a few months before he became Soviet leader: "Capitalism has no future."[52]

But Friedman was, of course, also surprised and delighted by the "political upheaval in Eastern Europe," in the form of the overthrow of totalitarianism (*National Review*, May 14, 1990, p. 33). "I find it exciting to watch the rebirth of freedom in Eastern Europe," he remarked on the new *Free To Choose* program. "Being free to choose should be every person's birthright."[53] For political liberation to occur in the Communist world was especially heartening for Friedman in part because he regarded Communist regimes as among those least amenable to give way to democratization.[54]

Friedman's wish to consolidate democratization in Eastern Europe was one reason he favored strongly free-market reforms, as opposed to what he called the "market socialism" model being advanced by those in Eastern Europe who "hanker after socialist control of the markets."[55] In the case of Poland, for example, Friedman regarded the labor and intellectual movements that had helped depose the Soviet-backed regime and institute democracy as being highly anti-Communist but as favoring largely socialist economic arrangements.[56] Friedman's longtime position, however, was that liberal economic arrangements were essential for the maintenance of political freedom. "In all of human history," he had remarked when the Soviet Union was still firmly in control of Eastern Europe, "I know of no example of a country with a large degree of political freedom which has not relied in the main on private markets and private arrangements for organizing its economic activity."[57]

[52] From Gorbachev's remarks in *Pravda*, December 11, 1984, as quoted in Weeks (1987, p. 3).
[53] *Free To Choose* (revamped U.S. version), Episode 3, "Freedom and Prosperity," CNBC, February 24, 1991, p. 1 of transcript.
[54] See, for example, his remarks in *Daily Mail* (London), September 30, 1976, and in the *Chicago Maroon* of October 1975, as quoted in Friedman and Friedman (1998, p. 595).
[55] *Free To Choose* (revamped U.S. version), Episode 3, "Freedom and Prosperity," CNBC, February 24, 1991, p. 4 of transcript.
[56] See *Wall Street Journal*, January 8, 1991, and his 1988 remarks in Block (1991, p. 82).
[57] Friedman (1983d, p. 17).

Friedman attended with other past Nobel laureates an event in May 1991 in Stockholm marking the centenary of the beginning of Nobel prizegiving.[58] He also participated in a panel of Nobel winners in various areas. Friedman told session moderator David Frost with regard to the "experiment [that] is going on" in Eastern Europe's transition from Communism, "I wish I were twenty-five years younger, so I could live long enough to see how that's going to work out."[59] In 2003, about halfway through the quarter-century stretch implied by his statement, Friedman was still around, and he seemed pleased at the results observed so far: "Let's take a broader view. Look at the world as a whole since the fall of the Berlin Wall. Freedom has spread enormously. You've got all of the countries which were in the [orbit of] the Soviet Union, under their control in the Soviet bloc, who are now free countries." (*Power Lunch*, CNBC, March 12, 2003.) These comments suggested that Friedman believed that Eastern Europe had largely avoided the scenario he feared in 1990 in which they merely settled on half-measures toward a market system.

In his 1990 discussion, Friedman had stressed the need for Eastern Europe to avoid such a halfway outcome by invoking an example: "It must either move directly to a pure free market, or it will get stuck just as Yugoslavia has."[60] In terms of its economic arrangements, Yugoslavia had, over the postwar period, made more efforts than other Communist countries in Europe to introduce market elements. And in the 1970s it had tried to integrate itself into the Western international economic system, with the country hosting an IMF conference in 1979 at which Paul Volcker, G. William Miller, and Arthur Burns were among the speakers. But, although it had broken away at an early stage from the Soviet bloc, Yugoslavia had continued to have a political dictatorship. Friedman had agreed to a return trip to Yugoslavia in 1973 after his initial 1962 visit and, prior to his second tour, he had been encouraged by what he understood to be the "creeping capitalism" process underway in the country (Instructional Dynamics Economics Cassette Tape 58, October 4, 1970, and Tape 117, March 14, 1973). But after his second visit, Friedman was critical of its attempts to design, and settle upon, a hybrid market/planned economic system.[61] He would conclude that Yugoslavia never attained "a real honest-to-God

[58] As a past John Bates Clark medal winner, Marc Nerlove also attended the occasion, including the main evening event. "We were all at this event, the king [of Sweden] came, and the [past] Nobel prize winners all marched in, to the sound of some wonderful music—by Halvorsen, I guess it was. And they went up steps. And little Milton was there—he was so tiny—he went up these steps in this funny suit [tuxedoes were supplied to attendees by the Nobel Foundation] to be greeted by the king and queen. That will stick forever in my memory. And there was a big dinner at which we all sat around... And then there was dancing afterwards, and Milton and Rose were a lovely couple dancing there." (Marc Nerlove, interview, September 26, 2013.)
[59] In Geeslin (1991, p. 17).
[60] *Free To Choose* (revamped U.S. version), Episode 3, "Freedom and Prosperity," CNBC, February 24, 1991, p. 4 of transcript.
[61] See Instructional Dynamics Economics Cassette Tape 118 (April 13, 1973) and Friedman (1984e, pp. 15–19).

capitalist system" (*Forbes*, December 12, 1988, p. 170).

By 1990, however, Friedman's description of the country as being merely "stuck" between systems was obsolete. The former Yugoslavian state had moved into a state of civil war and genocide, and Friedman cited the unfolding situation in May 1991 in that region as underlying his judgment that it was "not proved" on the matter of whether, in a sustained manner, "the human race really is able to organize itself in such a way as to avoid destruction."[62]

Characteristically, Friedman then tied this political-philosophical observation back to economic arrangements. And, over the 1980s and 1990s, it remained the case that when it came to those foreign-policy and geopolitical issues on which Friedman believed an economist's perspective was important, he was outspoken. In the leadup to the end of the Cold War, this was especially so with regard to economic sanctions, which—despite the Reagan Administration's lack of interest in a concerted economic war on the Soviet Union—became a major issue once more in the mid-1980s on account of proposals to impose U.S. economic sanctions on South Africa. In 1982, Friedman had affirmed his view that, as a general matter, trade embargos were "a terrible policy. I'm in favor of free trade all over."[63] After the U.S. House of Representatives passed a bill proposing major sanctions against South Africa in mid-1986, Friedman appeared in an NBC news report clip conceding that sanctions would bear on the South African economy but suggesting that affluent South African residents might benefit from the sanctions' imposition, as they would being able to purchase local companies that were in distress (*NBC Nightly News*, June 18, 1986). He had already contributed an op-ed to the *New York Times* (May 16, 1986) sketching this scenario, while also arguing that U.S. university activists seeking to put financial pressure on South African businesses should do so via their own purchasing and investing decisions, as opposed to their current practice of calling for university administration to divest holdings of securities issued by U.S. companies that engaged in commerce with South Africa.

President Reagan and Secretary of State Shultz also had had negative views about sanctions, but the Reagan Administration ultimately extended sanctions at Congress' urging. Although Friedman declared that "[s]anctions never work, they will not achieve the purpose for which they are adopted," the dismantling of apartheid in South Africa did occur within several years of the United States joining in the employment of large-scale sanctions.[64] This dismantling was

[62] In Geeslin (1991, p. 17).
[63] Friedman (1982a, p. 56).
[64] The quotation is from Friedman's June 13, 1986, remarks in Friedman (1986b, p. 245). For an analysis emphasizing the sizable impact of the U.S. 1986 sanctions on South African exports, see Evenett (2002).

underway when, in 1991, Friedman and 1984 Nobel Peace Prize winner Bishop Desmond Tutu—a major advocate of sanctions against the apartheid regime during the 1980s—were co-panelists at the event marking the Nobel centenary. At this event, Friedman did not restate his opposition to sanctions, and his only reference to Tutu was to praise the account the bishop had just given of the disasters that had occurred in the world over the twentieth century.[65]

TOWARD INDEXED BONDS

In 1991, Friedman produced for the *Economic Journal*'s centenary celebration a selective account of the past century's developments in economics, both in research and practice. With regard to the latter, he remarked that "mankind has not succeeded in evolving a satisfactory monetary system in several millennia."[66] Friedman elaborated on this theme in 1992 when he observed in *Money Mischief* that efforts were underway to break this historical pattern. As he saw it, the world—by implication, mainly the Western economies—was currently engaged in an experiment that would ascertain whether it was possible in practice that self-imposed government restraint could produce a monetary framework that anchored the price level.[67]

When Friedman wrote, his characterization was especially true of the United States—featuring as it did a floating exchange rate and a domestic economic discourse in which it was being made increasingly explicit that monetary policy was the only reliable means available for the control of inflation. Friedman's characterization would gradually become still more applicable across economies, as a number of advanced countries followed the example set by New Zealand in 1989 of establishing an explicit inflation target for monetary policy to pursue against the background of an exchange-rate float.

For the immediate period ahead for the United States, Friedman was optimistic about the Federal Reserve's prospects for delivering price stability. Although the CPI inflation rate in the United States had gone above 6 percent during 1990, he assessed in a presentation in early 1991 that recent years' trends in M2 growth implied that inflation would fall to about 2 percent over the next few years.[68] U.S. inflation did indeed fall to low single-digit rates during the 1990s, and the CPI inflation rate never crossed above 5 percent in the final fifteen years of Friedman's life—the

[65] See his remarks in Geeslin (1991, p. 17). Friedman and Tutu had been joint guests at an event before, as both had received honorary degrees at a graduation ceremony at Harvard University (*Boston Globe*, June 8, 1979).
[66] Friedman (1991c, p. 39).
[67] Friedman (1992c, p. 42).
[68] Oppenheimer and Company (1991, pp. 4–5). See also the discussion in the previous chapter.

fifteen years through 2006. A similar situation prevailed in many OECD countries: as noted in the previous chapter, Levin and Piger (2004) found that there was a distinct step-down in the longer-run average inflation rate across economies commencing in 1991.

Beyond his favorable medium-term prognosis regarding U.S. price trends, Friedman in the early 1990s still saw a *longer-term* danger of high inflation. It was now widely accepted, he noted, that, in the case of the United States, "inflation under the fiat money system is made in Washington and nowhere else." But it remained the case that "there are many different reasons why you might get inflation," depending on what considerations drove monetary policy (*Money*, April 24, 1992, part 2). In particular, in 1987, after the stock market crash, he explicitly reaffirmed the analysis he had given in his 1954 talk "Why the American Economy is Depression-Proof" (see *The Post-Standard* (Syracuse, New York), October 24, 1987, and *National Review*, November 20, 1987).[69] One element of that analysis was the position that, in the modern world, in which aggregate demand policy's assignments included delivering full employment, policymakers were prone to overestimate the amount of demand provision consistent with their price-stability and employment goals—and so were likely to generate, on average, excessive inflation. That this was a possibility even beyond the Great Inflation period, and even after policymakers' important doctrinal changes in the late 1970s, was underlined by the fact that the final three years (1988 to 1990) of the Thatcher Government saw it preside over high inflation in the United Kingdom.[70]

Against this background, Friedman insisted early in 1991 with regard to the United States that he remained "also in favor of the government issuing purchasing-power securities"—that is, longer-maturity U.S. Treasury bonds that contained a provision automatically indexing them to the price level, as well as offering a real coupon rate.[71]

From caution to renewed advocacy

In correspondence in September 1973, Friedman had urged Secretary of the Treasury George

[69] See Friedman (1954b).
[70] In another reaffirmation of his 1953 analysis, Friedman nearly a decade on stated: "If you have inflation in the future, my prediction is that it will only be as an attempt for full-employment purposes and not as a way to raise revenue." The period in which he was speaking (in January 1996) was a time when policymakers put heavy emphasis on the possibility that full employment was obtainable largely by policies that focused on price stability, and, against this background, Friedman added that he was "confident that you are not going to have a major inflation in the future." (Snowdon and Vane, 1997, p. 209.)
[71] In Hinshaw (1993, p. 29).

Shultz to galvanize his department into issuing indexed securities.[72] But, by the end of the decade, this sense of urgency on Friedman's part had been replaced by caution. The public debate on indexation in which he had engaged during 1974 was not one of his most successful public-policy ventures, and the evolution of U.S. discussion of indexation evidently took some wind out of his own sails on the subject of indexed bonds. In the early 1980s, apparently discouraged by the degree of opposition in the United States to inflation-linked government securities, Friedman actually pulled back from forthright advocacy of the issuance by the federal government of marketable indexed bonds—instruments whose creation he had pushed publicly for thirty years.

This change in attitude was reflected in a memorandum Friedman wrote to Shultz when the latter was coordinating economic-policy matters in the Reagan presidential campaign. Friedman's memo, dated April 30, 1980, contained a number of proposed reforms to U.S. federal debt management. Friedman's recommendations included the creation of an indexed savings (that is, nonmarketable) bond but specifically stated that, though he himself also favored the issuance of marketable indexed Treasury securities, a near-future administration should not make the issuance of marketable indexed Treasury securities one of its initial policies.[73] After Reagan took office, Friedman revised his 1980 memorandum to Shultz into a new memorandum (dated March 31, 1981) to Secretary of the Treasury Donald T. Regan and observed that it was not wise at this point to launch indexed marketable bonds, in view of the controversy associated with them.[74]

But this proved to be a rare instance—another one being the adoption of floating exchange rates in 1973—in which Friedman underestimated the momentum in favor of a change he favored. As in the case of floating, the impetus for change in the United States largely came from developments abroad.

In particular, in the United Kingdom, a breakthrough occurred when, in March 1981, the Thatcher Government introduced an index-linked gilt (longer-term government security).[75] Although a marketable security, it was available for purchase only by U.K. pension funds and so was neither internationally tradable nor universally available to U.K.-based market participants (*Financial Times* (London), March 11, 1981).[76] The following July, however, government

[72] September 3, 1973, letter to George Shultz, Box 33, Milton Friedman Papers, Hoover Institution.
[73] Friedman (1980a, pp. 10–11).
[74] Friedman (1981c, p. 9).
[75] See Oliver and Rutterford (1981) on the internal deliberations that led to this decision by the U.K. government.
[76] Later in the year, the government also issued an indexed and universally-available savings bond (*Financial Times* (London), July 31, 1981).

minister Nigel Lawson indicated that the authorities were considering issuing an indexed bond for which there would be no restrictions on purchasers—a prospect that prompted one commentator to note that "Britain would certainly be unique among major industrialized countries in offering such an inflation-proofed security" (*Financial Times* (London), September 8, 1981). In March 1982, this development came to pass, with the U.K. government issuing a six-year indexed bond available to all customers at home and abroad (*Glasgow Herald* (Scotland), March 11, 1982).

Developments during 1981 in the United States and internationally would swing Friedman back into renewed advocacy of the immediate creation of indexed, marketable U.S. government securities. In June 1980, even while putting indexed bonds on the back burner in the U.S. case, Friedman had advocated that the Thatcher Government issue indexed bonds and had stressed a fresh reason for doing so. The U.K. government was committed to ending inflation, but it was issuing new longer-term securities at rates that embedded an expectation of high inflation. This implied that there would be a higher real cost of servicing public debt if the inflation expectations embedded in the bonds were confounded—that is, if the Thatcher Government's disinflation policy proved successful. It also meant that a conflict of messages: a dedication to ending inflation in the official statements about monetary policy strategy, but a validation of market expectations of continuing inflation in the government's debt-management policy.[77] To avoid these undesirable implications, Friedman suggested that the Thatcher Government either issue no new long-term debt or borrow long-term only through indexed securities.[78] For the United States, his corresponding advice, as recorded in Friedman's March 1981 memorandum to Secretary Regan, excluded the option of indexed bonds. Friedman suggested, instead, that all new Treasury loan raising during the disinflation period should take place through the issuance of short-term securities.

When the Reagan Administration, in fact, continued to make substantial new longer-term borrowing, and with the Thatcher Government having laid down a marker via its 1981 issuance of an indexed bond, Friedman devoted a *Newsweek* column to the subject of "Sending Mixed

[77] Prior to writing the memorandum, Friedman had aired this concern publicly during a visit to the United Kingdom in February 1980. "If the government is serious about bringing down the rate of inflation, it is impossible to justify raising long-term finance at the rates your government is paying." (*Evening Standard* (London), February 28, 1980.) He was not alone in making this criticism, with the London *Financial Times* (March 25, 1981) recording: "Critics of the government's fund-raising methods have long pointed out that for the Treasury to continue issuing long-dated stocks [meaning bonds] carrying double-digit yields into the twenty-first century offers little evidence of confidence in permanently cutting the rate of price increases." In the United States, as discussed below, Friedman criticized the issuance in 1981 of a high-yield twenty-year bond on the same grounds.

[78] Friedman (1980b, p. 59, paragraph 20; p. 56 of 1991 reprint).

Signals" (October 19, 1981). The title referred to the conflict between Reagan's public statements calling for markets to adjust their expectations to line up with his policy of "elimination of inflation" and the U.S. Treasury's "telling the financial community the precise opposite," most recently by a new twenty-year bond bearing a 15.78 percent interest rate, put on sale on September 30, 1981 (a day, as it turned out, that would be recorded in history as seeing the ten-year bond rate reach an all-time high in the secondary market).[79] Friedman returned to propounding indexed securities as an urgent U.S. policy reform. His previous suggestion that the U.S. Treasury refrain from longer-term borrowing was still mentioned—but only as a fallback measure, to be taken if indexed bonds were not introduced.

Nevertheless, in contrast to policy developments abroad, inaction with regard to the idea of indexed bonds continued to characterize the Reagan Administration. Against this background, in the mid-1980s Friedman tried to force the pace of U.S. reform by stressing the virtues of indexed bonds in a couple of pieces: a short 1984 contribution to the *Journal of Political Economy* and his 1985 keynote address at the 1985 Bank of Japan conference.[80] In the 1984 article, he urged that the private sector should take more of the initiative in the area—by, Friedman suggested, setting up trading that was specifically concerned with the prediction of future price levels. In the 1985 speech, he considered indexed bonds more directly—lamenting the U.S. Treasury's continuing resistance to the idea of issuing indexed securities even in the face of the fact that the U.K. government now did so.[81]

Both Friedman's 1980 U.K. government memorandum and his 1981 *Newsweek* column had restated the case for indexed bonds, previously articulated in his 1970s writings on indexation, that lay in the fact that they provided a means of protecting the private sector from pernicious losses in the real value of their investments arising from unforeseen inflation. In his 1985 keynote speech—reproduced in lightly revised form in 1992's *Money Mischief*—Friedman stressed another point, also present in his 1970s discussions: that indexed bonds reduced the incentive of governments to inflate.[82] This discussion, like so many that Friedman had provided in the past on government revenue and inflation, did not adequately distinguish between (*i*) the fact that postwar inflation had indeed tended to lower real government debt in the United States and other advanced economies and (*ii*) the notion that those countries' governments had

[79] On September 30, 1981, ten-year Treasury securities ended the trading day at a record 15.84 percent (Mironas, 2021).
[80] See Friedman (1984f, 1985e).
[81] Friedman (1985e, p. 17).
[82] See Friedman (1985e, pp. 16–17; 1992c, pp. 256–258).

consciously sought inflation in order to lower their debt burden. The validity of (*i*) by no means implied the empirical relevance of (*ii*).[83]

Friedman's 1984 *JPE* article mentioned an additional attractive feature of having a market for purchasing-power-linked securities—a feature that had not been prominent in his 1970s writings on indexation. This was the *information value* that such a market would provide to economists and others. "We have great uncertainty, for example, about how to interpret interest rates on long-term securities. How shall those interest rates be divided between the 'expected inflation' and the 'real interest rate'?'" In the face of such uncertainty, it was useful to have arrangements that shed light on the matter by providing direct "market judgments" regarding the behavior of the individual components of long-term interest rates.[84]

The Hetzel proposal

The notion that indexed bonds could provide a public indicator of longer-term expectations also underlay a policy proposal involving indexed bonds that emerged in the early 1990s and to which Friedman devoted considerable attention.

Progress toward indexed bonds in the United States continued to be glacial in the later Reagan years and during the early years of the Bush Administration. The lack of action on the indexed-securities front did not occur because of waning public appetite for longer-term government securities. On the contrary, not only was there a very large increase in federal government debt during the Reagan-Bush years, but much of the new debt issued was long-term: Friedman noted that marketable U.S. public debt had reached a maturity less than three years on average in the mid-1970s but exceeded six years by 1990.[85] In particular, so-called Treasury notes (that is, intermediate-maturity government securities) had risen from about 45 percent of the outstanding marketable debt in the mid-1970s to around 60 percent by the early 1990s, and Treasury bills had declined from about 40 percent to a little over 20 percent over the same period (see Kuttner and Lown, 1999, p. 169).

Such figures underlined the popularity of long-term nominal bonds as a financial instrument and

[83] Friedman's attraction, in his *ex post* accounts of the period, to the very shaky notion that policymakers consciously inflated in the 1970s, underlay some expressions of optimism he gave on occasion about U.S. inflation prospects. An example was his statement in July 1988: "I do not believe we will have a resurgence of inflation, because it is no longer profitable to inflate." (Friedman, 1988c, p. 381.)
[84] Friedman (1984h, p. 167).
[85] See Friedman (1992c, p. 257).

implied that it was unlikely that Friedman's past suggestion that the United States should countenance prolonged periods in which all new long-term debt issuance take the form of indexed bonds would ever be accepted.

In this environment, Robert Hetzel, who had been a Ph.D. student of Friedman's in the mid-1970s and now worked in the Federal Reserve Bank of Richmond, advanced an idea that involved the issuance of indexed bonds but was predicated also on the continuing existence of a large market for new nominal-rate bonds. In an article, "Maintaining Price Stability: A Proposal," published in his bank's research journal, Hetzel (1990) argued that an indexed bond and a regular bond of the same maturity be issued and that the Federal Reserve's success at establishing a climate of long-term price stability could be assessed by its success in making the spread between the yields on the two securities low, as this spread amounted to a measure of expected inflation. Hetzel subsequently published a capsulized version of his proposal in an op-ed in the *Wall Street Journal* (April 25, 1991).

These Hetzel articles did not offer the proposal as anything more than a means of assessing the success of whatever monetary policy was chosen and not as a prescription for monetary policy. They explicitly left open to the Federal Reserve the choice of its own strategy for delivering price stability. For example, M2 growth targeting was not ruled out as the means by which the FOMC sought low inflation—although in his *Wall Street Journal* piece Hetzel expressed doubt about this approach, as he accurately foreshadowed the possibility that U.S. M2 velocity might soon lose its historical stability. The Hetzel proposal's use of the two long-term rates as indicators was also consistent with the employment of either the federal funds rate or a reserves variable as the policy instrument—although Hetzel's *Wall Street Journal* piece implied that the funds rate would most likely remain the FOMC's policy instrument.

Friedman acknowledged that the Hetzel proposal was a "method of accountability" and "not" a policy rule (*Money,* April 24, 1992, part 2). But in his own commentary on the proposal, Friedman considered the further step under which it was made into a prescription or rule—that is, the Federal Reserve undertook to keep the spread between indexed and non-indexed bond rates low and stable. In particular, Friedman discussed in *Money Mischief* the "extension" of Hetzel's proposal under which keeping the spread between the two bond rates would be a (legislated) rule.[86]

[86] Friedman (1992c, p. 229).

With regard to its possible role as a rule for monetary policy, Friedman was supportive of the Hetzel proposal but only if it was regarded as a place-holder. “I think the Hetzel proposal is a very good first start,” he remarked (Milton Friedman, interview, January 22, 1992). His ideal remained a monetary-growth rule—"I’ve championed [it] for a long time” (*Money*, April 24, 1992, part 2)—specifically, targeting M2, using a reserves instrument, or just using a monetary base target. “I think a lot of the problems that emerge in these discussions is that people are not clear about the level on which they’re talking—whether they’re talking about an ideal world, or whether they’re talking about the expedient in the world as it is, or of a step toward your ideal world. You ought to be clear as to what your ideal is, so you can judge whether a particular proposal in that direction towards that ideal goal or away from it. I think you have a real obligation, in an imperfect world, to be willing to discuss expedients.” (Milton Friedman, interview, January 22, 1992.) In this context, he was willing to discuss an interest-rate-oriented monetary policy like the rule version of the Hetzel proposal even though he still favored a monetary-growth rule. As the Hetzel proposal involved “the difference between two interest rates,” he understood that, compared with monetary targeting, it was likely to more amenable to those more inclined to cast monetary policy decisions in terms of interest rate.[87]

But, as his main reason for liking the Hetzel proposal, Friedman came back to the potential information content conferred by a market in indexed bonds: a continuously available measure of longer-run inflation expectations. “I regard the Hetzel proposal as one of those expedients which would provide the public with a greater ability to monitor what the Federal Reserve does. I’m not so interested in it from the point of view of its effect on the Federal Reserve itself—but on the fact that it provides a very readily available, contemporary statement [made by] the informed public about what the Fed’s doing.” (Milton Friedman, interview, January 22, 1992.) This property of the Hetzel proposal underlay Friedman’s remark in *Money Mischief* that “a market measure of expected inflation would make it possible to monitor the Federal Reserve’s behavior currently [because current actions affect future inflation] and to hold it accountable.”[88]

A flaw in the Hetzel proposal was that the bond market’s track record in forecasting longer-term inflation was simply not very good. Notwithstanding its forward-looking character, the U.S. bond market evidently made large one-sided errors in practice. That market, unlike Friedman, had not predicted the return of double-digit inflation in 1979, and both the bond market and Friedman had, over much of the 1980s, failed to grasp the permanence of the 1982–1983 decline

[87] Friedman (1992c, p. 229).
[88] Friedman (1992c, p. 228).

in U.S. inflation. Furthermore, as Hetzel (1992, pp. 17–19) himself acknowledged, the indexed bond market in the United Kingdom had failed to anticipate much of the renewed bout of high inflation that occurred in that country in 1988–1990.

As of the early 1990s, however, progress toward an indexed Treasury bond continued to languish, and the notion of inflation-linked government bonds remained a hypothetical one in the United States. In contrast, the same period saw progress with regard to another longstanding Friedman proposal regarding U.S. debt management—his recommendations concerning auctions of Treasury securities.

The "hot debate" on auctions

Friedman's highlighting in the 1980s and the 1990s of the possibility that long-term indexed securities could provide a direct reading on inflation expectations was predicated on the continued existence of nominal (that is, unindexed) long-term Treasury securities. Their presence was needed so that rates on indexed and non-indexed securities could be compared. As noted, Friedman had, in 1981, proposed a moratorium the issuance of new long-term Treasury debt, and his expositions of indexation in the inflationary conditions of the mid-1970s had envisioned the authorities permanently eschewing long-term borrowing except in indexed form.[89] But, as indicated above, this was not a realistic expectation, and Friedman did see some advantages in the Treasury continuing to issue some of its long-term debt in dollar-denominated form. Furthermore, in the case of short-term securities, he did not think that the issuance of any indexed security was necessary—the horizon pertaining to the securities (such as three months ahead) was sufficiently short, and near-term inflation was sufficiently predictable, that differences between *ex post* and *ex ante* real rates were likely not to as severe as they could be for longer-term rates, and could be corrected by repricing in the next period.[90]

So if new offerings of both short- and long-term unindexed Treasury securities were going to be a permanent part of the financial landscape, how should they be marketed? This was a matter on which Friedman had views that were of long standing and—ultimately—highly influential on actual U.S. policy practice.

The early skirmishes on the matter were referred to in a 1988 article, by Loretta Mester, on different procedures for carrying out auctions. Mester (1988, p. 6) remarked: "During the 1960s

[89] Friedman (1974b, p. 97; p. 154 of 1975 reprint). See also Friedman (1978b, p. 36; 1991a, p. 33).
[90] Friedman (1974b, p. 97; p. 154 of 1975 reprint). See also Friedman (1978b, p. 36; 1991a, p. 33).

a hot debate developed about whether U.S. Treasury bills should be sold in a uniform auction or a discriminatory auction." Although Mester did not mention him in her discussion, in this "hot debate" Milton Friedman was a key player—as he seemed to be in so many economic debates during the 1960s.

Friedman's prominence in the 1960s Treasury-security auctions debate resulted from his urging that the existing auction method be abandoned in favor of a uniform-price procedure. And a few years after Mester wrote, developments in 1991 would see a set of reforms in Treasury practices that saw the culmination of the debate in Friedman's favor.

Debt management and monetary policy

An article on Treasury security auctions by Hortaçsu, Kastl, and Zhang (2018, p. 148) referred to a "large theoretical literature, dating back to the classic work of Friedman (1960)"—that is, his book *A Program for Monetary Stability*—while Malvey and Archibald (1998, p. 3) traced Friedman's statements on the matter to his testimony to the Joint Economic Committee of October 30, 1959, in which, they contended, he presented an "analysis based on auction theory."

Both these accounts gave the impression that Friedman's concern with the subject arose heavily from the theory of auctions. In fact, Friedman's 1959 and 1960 discussions were much more closely related to his home turf of monetary economics. His coverage of Treasury security auctions arose from his desire to generate more stable behavior in the monetary base and the money stock, by reducing the influence of the U.S. Treasury's debt-management operations on the behavior of both series.

With the end of interest-rate pegging in 1951, the Federal Reserve was no longer obliged to create money to finance the fiscal authority. This meant that the Treasury had no automatic effect on the average rate of monetary base growth. But the ongoing institutional fact that the central bank was the banker to the federal government remained. The Treasury therefore held an active transactions account at the Federal Reserve. In this situation, transactions involving the Treasury were liable to be a source of variability in the monetary base (and in commercial bank reserves, in particular).[91] In its engagements with the nonbank private sector in the form of

[91] Meltzer (2004, p. 199) argued further that the debt-marketing arrangements of this era had an effect on the *mean* of monetary growth—arguing that in the pre-auction era Federal Reserve actions creating reserves to support debt financing in "the 1960s and 1970s… were a main source of excess money growth." This strong assertion does not seem supportable. After 1951, the FOMC was free to set its own monetary policy reaction function and had no obligation to monetize fiscal operations, on average. In the post-Korean War years, the Treasury debt management

receiving tax payments, dispensing federal government outlays, and raising and redeeming public debt, the Treasury's transactions tended to affect the volume of bank reserves outstanding. For example, tax receipts often amounted to payments out of household commercial bank accounts as well as corresponding debits to commercial banks' reserve-balance accounts, as the Treasury's receipt of the funds was registered. And payment of interest and principal associated with Treasury borrowing from the nonbank public involved the Treasury drawing down its account at the Federal Reserve and writing checks to the private sector—payments whose settlement involved increases in bank reserves.

In principle, the Federal Reserve could make open market operations that continuously offset the effect of the Treasury's operations on the monetary base. But the scale of this task was magnified by the sheer degree of variation in the Treasury's account balance—a situation reflected in Kaufman's (1973, p. 167) observation: "The cash position of the U.S. Treasury has had a tendency to swing from feast to famine." Friedman wanted to curtail the Treasury's scope to be a source of variation in reserves and money by instituting arrangements in which there would be less abrupt movements in the Treasury's account at the Federal Reserve. To this end, in *Program for Monetary Stability* he urged reforms designed at "smoothing Treasury debt operations."[92] One of these reforms proposed in the book was something that Friedman also raised in his aforementioned October 1959 Congressional testimony and that he summarized in the testimony as "Sell all securities exclusively by auction[,] so the market can set the price."[93]

Treasury bills auctions: multiple-price versus single-price

At the time, bonds—medium- or very long-term Treasury securities—were offered to the market

arrangements were much the same as those in the 1960s, but the FOMC was able to instill a noninflationary monetary policy reaction function in that earlier period (see Romer and Romer, 2002). And it does not seem appropriate to view the inflationary monetary policy of the 1970s as arising from any automatic or technical link between monetary policy and fiscal policy. Instead, it arose from the doctrine guiding the FOMC's choice of monetary policy reaction function.

[92] Friedman (1960, p. 55). Friedman downplayed the importance of another institutional rigidity in force at the time—a legislative ceiling of 4.25 percent on the coupon rate that could be offered on long-term Treasury securities (see Friedman, 1960, pp. 58–59). One factor that implied that this restriction did not prevent the U.S. Treasury from marketing longer-term debt was that it *was* able to offer coupon rates above 4.25 percent on five-year securities (see Meltzer, 2009a, p. 238). Such five-year instruments may be considered long-term debt from an economic point of view despite their being formally categorized as intermediate-term securities (or "notes") rather than very long-term debt (bonds). Friedman himself noted that in the presence of the 4¼ percent ceiling, the government could work on the notion that long-term bonds were securities of a seven-year maturity—a maturity that was, also, not subject to the ceiling—and issue new debt on that basis (Instructional Dynamics Economics Cassette Tape 53, June 24, 1970). The ceiling was largely removed in the early 1970s (see Committee on Ways and Means, U.S. House of Representatives, 1971, and Kaufman, 1973, pp. 161, 165).

[93] Testimony of October 30, 1959, in Joint Economic Committee (1959, p. 3024). See also Friedman (1960, p. 64).

at a take-it-or-leave-it price set by the authorities. Friedman characterized this arrangement as one in which “people sit around at the Treasury and try to figure out… what rate they can offer, and then sometimes they make a mistake”—in which case the Treasury either failed to raise the amount of borrowing intended or marketed the debt at too low a price (Instructional Dynamics Economics Cassette Tape 53, June 24, 1970).[94]

The situation was, however, different in the case of Treasury bills. Meulendyke (1998, p. 94) observed: “The Treasury has sold bills at competitive auctions since bills were introduced in 1929.” In light of this reality, did Friedman support the *status quo* with regard to the marketing of short-term securities? The answer was no. The reason lay in the fact of the method of auction. As of the start of the 1960s, the U.S. Treasury’s procedure for selling its bills indeed used competitive auctions but did so via the “multiple-price” method—under which successful bidders had securities dispensed to them at the price they had named. Friedman advocated instead a single-price auction method under which a single market-clearing price prevailed. “The present method involves payment of different prices by different purchasers, [an arrangement] which tends to limit the market to specialists and to establish a strong incentive for collusion among bidders,” he argued, while offering an alternative in which prospective purchasers were asked to nominate the quantities they would buy at different prices, and the authorities then would “determine a single price so as to clear the market, and charge all purchasers that single price.”[95] That is, he favored a Dutch-auction method—although Friedman himself rarely called it that in his public writings until the 1990s.[96]

Friedman favored this method for the marketing of short-term and longer-maturity Treasury securities alike. But because it was Treasury bill issuance that was already subject to auctions, it was on the bill market that the debate in the 1960s centered. The onus was, therefore, on Friedman to argue that his preferred method of auctioning Treasury bills was better than the multiple-price method already in place.

Friedman set off a debate in research journals via his Congressional testimony advocating the single-price auction method. What Goldstein (1962) called the “Friedman proposal” gave rise to

[94] See also the Friedman remarks reported in *New York Times*, June 11, 1970, p. 71.

[95] From Friedman’s testimony of October 30, 1959, in Joint Economic Committee, U.S. Congress (1959b, p. 3024).

[96] See *Wall Street Journal*, August 28, 1991, Friedman (1992b, p. xi), and Friedman and Friedman (1998, pp. 385–386). On account of his usage of the term in his 1991 op-ed, the *Wall Street Journal Index* gave the (incorrect) impression that Friedman originated the terminology: “Milton Friedman proposes a new method for the U.S. Treasury to use in the trading of Treasury securities, which he calls the ‘Dutch auction.’” (Dow Jones and Company, 1992, p. 1393.)

the appearance of a number of critiques in journals, including Brimmer (1962) and Goldstein's piece. Friedman himself discussed and defended his proposal in articles published in research journals during the first half of the 1960s.[97] Here he was in dangerous waters, as the debate focused on topics—auction theory and the details of current institutional arrangements—that were outside the monetary-control and aggregative matters with which his previous statements in the area of public-debt marketing had been concerned. The critiques that Friedman generated established that his initial expositions of the single-price proposal had been marred somewhat by misconceptions he had labored under regarding the then-current institutional arrangements for Treasury bills—errors that he acknowledged when restating his case in 1964 in the face of the criticisms he had received.[98] But Friedman's bottom line in favor of the single-price method remained, and, in the early 1970s, he continued to press for his method of auctioning bills, remarking of the Treasury: "It auctions its bills in a way such that people who bid for a certain price for bills, pay the price they've bid, regardless of what the market-clearing price is." (Instructional Dynamics Economics Cassette Tape 53, June 24, 1970.)

As the discussion of auctions proceeded in the 1980s and early 1990, it was clear that the bulk of economists in the research world—as distinct from those in financial markets—shared Friedman's strong inclination in favor of Dutch auctions. "Milton Friedman advocated a uniform-price auction in testimony to the Joint Economic Committee in 1959... We think a large majority of economists support the proposal," remarked Chari and Weber (1992, p. 11). The contrast in the perspective of research-oriented economists with that of some in the marketplace was evident in the language used. The research article of Mester (1988), as noted above, called the multiple-price system the "discriminatory" method, while Henry Kaufman—at the time, partner and economist at Salomon Brothers, and an outspoken opponent of the single-price system—referred to the "controversial 'Dutch' auction" (Kaufman, 1973, p. 165).

Bond auctions

The U.S. Treasury did start conducting auctions when selling some of its longer-term bonds in 1970 and, over the subsequent several years, auctions, rather than the predetermined-price method prevailing up to that time, became the prevalent procedure for marketing new U.S. government bonds (Kaufman, 1973, p. 165; Garbade, 2004, Meulendyke 1998, p. 94). Friedman remained dissatisfied, however, because, as with Treasury bills, bonds were auctioned via the

[97] See Friedman (1963, 1964b). The discussion in Skousen (2008, Chapter 16), like the coverage in Friedman and Friedman (1998) that Skousen cites, likely gives the incorrect impression that Friedman advocated this change only in non-journal outlets.

[98] See Rieber (1964) and Friedman (1964b).

multiple-price method rather than the single-price procedure. Although the Treasury had "moved to auction all its securities," he later wrote, "it is still far from adopting the key elements of my proposal, in particular a single price for all successful bidders."[99]

During 1972–1974, however, Friedman had a sympathetic ear at the top of the Treasury in the form of Secretary George Shultz. With Friedman's encouragement, the Shultz Treasury did experiment with Dutch auctions in marketing bonds, with a couple of bond issuances conducted in this way beginning in December 1973 (*Wall Street Journal*, August 28, 1991; Kaufman, 1973, p. 169). Under Shultz's successor, William Simon, however, further attempts at Dutch auctions were discontinued.

Simon's Under Secretary of the Treasury for Monetary Affairs, Jack Bennett, would later testify: "My recollection is clear that the Secretary of the Treasury at that time, William E. Simon, made the decision to discontinue the Dutch auction as a result of his judgment, based on his extensive experience in the market for Treasury securities, that the Dutch auction would bring in fewer dollars for the Treasury."[100]

Friedman had a high regard for Simon but also a skeptical attitude toward judgments that were derived from time spent in financial markets. Those kinds of judgments had underpinned some of the opposition to some of his other proposed reforms over the years, including floating exchange rates and contemporaneous reserve accounting. "For all his admirable qualities," Friedman contended, "Bill Simon was too recently a bond trader on Wall Street to welcome an experiment vigorously opposed by professional government bond dealers." Friedman also suggested that unpublished research by former Treasury officials had confirmed that the Dutch auctions had produced cost savings for the Treasury (*Wall Street Journal*, August 28, 1991). As for the present situation, he maintained that in their loan-raising activities, Treasury officials "are paying more than they should because of the method they have chosen to auction" their securities (*Washington Post*, September 8, 1991, p. H7).

Friedman was certainly on solid ground in contending that existing bond dealers were opposed to Dutch auctions. At the time of the Shultz experiment with the method, Henry Kaufman of Salomon Brothers declared: "I disagree with the 'Dutch' auction procedure. I do not believe that it accomplishes its two basic objectives, which are to minimize costs and, even more

[99] Friedman (1992b, p. xi).
[100] Written answer in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1992b, p. 409).

importantly, to achieve a wider distribution [of buyers]."[101]

The "Salomon Brothers incident" and reform

It was, however, still another advantage Friedman stressed for Dutch auctions that in the 1990s would lead to the victory of the Friedman side in the debate over the appropriate method for marketing Treasury securities. This advantage was the avoidance of collusion on the part of sellers—a point that, as noted above, Friedman mentioned in his 1959 testimony.

Henry Kaufman left Salomon Brothers in 1988 to start his own firm (Kaufman and Sicilia, 2021, p. ix). Long after Kaufman's departure, Salomon Brothers would set off a chain of events that would lead to the victory of the Friedman side in the auctions debate.

What Federal Reserve Bank of New York President Gerald Corrigan called the "Salomon Brothers incident" and what Friedman more colorfully called the "Salomon Brothers scandal" consisted of the bond-trading firm's involvement during Treasury bond auctions held in the first half of 1991 in collusion and the arrangement of a fraudulent bid.[102]

"It is nearly impossible to undertake major reforms except at a time of crisis," Friedman had remarked in a January 1986 memorandum to the Reagan Administration, restating an adage that he had long held.[103] Now, five-and-a-half years later, under the Bush Administration, there had emerged a major loss of public confidence in the procedures that the U.S. Treasury used to auction securities. Furthermore, those procedures consisted of a method Friedman had criticized for decades; they had now exhibited the very failing—the capacity to produce collusion among buyers—that he had warned about in 1959; and an alternative method that he had advanced in 1959 had become well known and commanded wide support. In the wake of the Salomon Brothers scandal, Friedman was therefore entitled to believe that, if this was not a time of crisis that created an atmosphere conducive to a major reform, then he was a monkey's uncle—or (to use a less-prevalent, but more apposite, variation of the same proverb) he was a Dutchman.

[101] Kaufman (1973, p. 169). With regard to the second objective, Kaufman argued that as the securities dealers were not the ultimate purchasers of the new bonds, their activity in the market did not prevent wide ownership of bonds and actually helped promote it (p. 170).

[102] The quotations are, respectively, from Corrigan's written submission for the hearing of September 11, 1991, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1992b, p. 171) and Friedman's op-ed in *Wall Street Journal*, August 28, 1991. A detailed chronology of the Salomon Brothers matter was provided by Jerome H. Powell, Assistant Secretary for Domestic Finance, U.S. Treasury, for the hearing of September 11, 1991, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1992b, pp. 220–223).

[103] Friedman (1986f, p. 5). See also Friedman and Friedman (1988, pp. 456, 465).

Friedman seized his chance. In response to the renewed public interest in the U.S. Treasury's auctioning method and the associated widespread dissatisfaction with existing marketing arrangements for Treasury debt, he swooped in with an op-ed in the *Wall Street Journal* (August 28, 1991) that reaffirmed his proposal, while noting his 1959 warning about the opportunity that the multiple-price system provided for collusion.

A couple of weeks later, Friedman's views featured prominently in a Congressional hearing on the recent irregularities. The banking committee's chair, Senator Donald W. Riegle (D-MI), remarked on the second day: "This antiquated system [of marketing bonds] raises an even broader question: Are we selling our debt the best way possible? For example, Milton Friedman has advocated a Dutch auction system for over thirty years."[104] The previous day, Friedman's views had been invoked explicitly in a question to Federal Reserve Bank of New York President Corrigan, whose answer left Friedman unmentioned: "I will simply say that I think it [the multiple-price method] has worked incredibly well. I personally do not think there is any magic in Dutch auctions."[105] In contrast, former Under Secretary of the Treasury Jack Bennett responded to a written question that also referred to Friedman's views by remarking that "the Treasury would now be well advised to return to the Dutch auction."[106]

This is what the Treasury did. As Lawrence Summers wrote in October 1998 when he was Deputy Secretary of the Treasury, "the Treasury inaugurated its experiment with regular uniform-price auctions of 2- and 5-year notes in September 1992."[107] Until 1998, all remaining longer-maturity securities (both notes and so-called bonds) were still auctioned using the multiple-price method (Meulendyke, 1998, p. 96). In the same month in which Summers wrote, the Treasury decided that the uniform-price approach would be extended to the issuances of all categories of Treasury securities (Garbade and Ingber, 2005, p. 2). By this point, the auction procedure for Treasury bills also implied that these securities were being sold at a single price to

[104] From Riegle's remarks of September 12, 1991, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1992b, p. 80). Riegle added: "the very informality of this marketplace raises a host of questions. Where else do we have a marketplace regulated by Treasury 'Press Release'?" This remark partially echoed a complaint Friedman had made a decade earlier (*Newsweek*, October 19, 1981) on the need for "a definite and firm schedule" of proposed new loan raisings by the Treasury, replacing a situation in which "the financial pages trumpet a fresh Treasury borrowing—as if it were a completely unexpected event."

[105] From Corrigan's testimony of September 11, 1991, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1992b, p. 32).

[106] Written answer in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1992b, p. 409).

[107] From Summers' foreword in Malvey and Archibald (1998). The 1992 decision came in the wake of a joint U.S. Treasury-Securities Exchange Report-Board of Governors of the Federal Reserve System report, released in January 1992, that devoted considerable attention to "Friedman's 'Dutch' auction regime" (1992, pp. B–21–B–22) and to the associated 1960s research literature.

all customers (Meulendyke, 1998, p. 95; Garbade and Ingber, 2005, p. 2)—a feature that Friedman had long highlighted as an important part of his proposal.[108]

It therefore transpired that a matter that Friedman had raised in public debate in 1959 came to a head in the early 1990s and generated reforms, starting in 1992, that implied that newly issued federal debt was marketed in the manner that he had recommended back in 1959–1960. Without having to devote many of his writings and talks to the issue, Friedman had been able to gain attention for, and be recognized as a leading proponent of, an option—Dutch auctions—that ultimately became the official debt management policy of the U.S. Treasury. A half-century after Friedman worked as a junior official at the Treasury, he had helped generate a major change in the practices of that agency. The fact that the "Treasury auctions long-term securities, as Friedman proposed in the 1960s"—and, furthermore, that it did so using the method he proposed—came to be counted as an item in the "win" column when a 2003 conference paper attempted to tally Friedman's successes and failures in getting his policy proposals adopted.[109] The author of the paper in question was someone who was selected because his research interests and policy views were perceived as being closely aligned with Friedman's. This was Allan Meltzer—and the perception of alignment was only partially accurate.

III. PERSONALITIES IN DEBATES ON INTERNATIONAL ECONOMIC POLICY AND GEOPOLITICAL DEVELOPMENTS, 1987–1992

ALLAN MELTZER

In the fourth edition of their macroeconomics textbook, Dornbusch and Fischer (1987, p. 241) listed the leading monetarists in the United States as including, in addition to Friedman, "Professor[s] Karl Brunner and Robert Barro of the University of Rochester, Allan Meltzer of Carnegie Mellon University, William Poole of Brown University, and Anna Schwartz of the National Bureau of Economic Research."[110] The names on this list would be depleted in the late 1980s, with Brunner in poorer health after 1986 and passing away in May 1989, and with Barro largely moving away from monetary economics.[111] In this setting, and with Friedman himself

[108] See the quotations above as well as Friedman (1964b, p. 513).
[109] Meltzer (2004, p. 198).
[110] Barro actually moved to Harvard University around the time Dornbusch and Fischer wrote.
[111] The later catalogue of U.S.-based monetarists in Dornbusch and Fischer (1994, p. 538) had omitted Barro from the list and added Bennett McCallum and Phillip Cagan. (By this point, however, Cagan had become quite inactive in economics, other than teaching at Columbia University. He would then retire altogether from his university position in 1995—see American Economic Association, 1997, p. 98.)

now somewhat less active in macroeconomic debate, Allan Meltzer—more than fifteen years Friedman's junior, but already long established as one of the core monetarists—further increased his own role as the spokesperson for monetarist views in research and policy dialogues.

This pattern was evident in the fact that, in contrast to Friedman, who gave no Congressional testimony in person after 1979, Meltzer—already a familiar face at hearings held during the 1960s and 1970s—testified in Washington, D.C. prolifically in the 1980s and 1990s—and indeed far beyond, the final such occasion occurring when Meltzer appeared before the House of Representatives' Committee on Financial Services in January 2014.[112]

In a couple of other instances during the later 1980s, Meltzer directly inherited positions that Friedman had occupied. Although he was based on the East Coast, Meltzer succeeded Friedman in the largely honorific position of president of the Western Economic Association, delivering his presidential address in July 1986 (see Meltzer, 1987a), a year after Friedman's corresponding talk. And at the Bank of Japan's June 1987 biennial monetary policy conference, Meltzer filled the position of the monetarist keynote speaker that Friedman had taken at the 1983 and 1985 conferences (with James Tobin continuing in 1987 as the Keynesian keynote speaker).[113]

Meltzer implicitly put Friedman and himself, along with Tobin, in the same basket when, in 1989, he categorized himself as being part of the generations who took the view that "economics is a policy science"—and, in particular, being among those "who were attracted to economics from the 1920s through the 1950s" and whose perspective involved seeing "economics as more than a set of abstractions, more than a collection of theorems and empirical regularities, and more than a branch of applied mathematics."[114]

Yet the 1987–1992 period would also underline what Meltzer had made clear in the past: that he did not accept descriptions like those embedded in Dornbusch and Fischer's (1987, p. 241) statement that "Friedman is the recognized intellectual leader" of the monetarists. On this point, Meltzer's attitude was similar to that Karl Brunner had in the past exhibited toward Friedman (see Nelson, 2020b, Chapter 13).

But some of Meltzer's activity in 1987–1992 set himself further apart from Friedman. These years would see Friedman and Meltzer, although not face to face in debate with one another,

[112] See Committee on Financial Services, House of Representatives (2014).
[113] See Meltzer (1987b).
[114] Meltzer (1989, p. 159).

being at odds in the perspectives they provided to the public regarding what monetarist analysis implied for the U.S. economic outlook and current policy settings. The differences between them would also be brought out in how they presented past research contributions to the Keynesian and monetarist literature. In the late 1980s and early 1990s, each of them would put out encyclopedia entries on the quantity theory or monetarism that did not mention the other's work, and they would also offer incompatible interpretations of the theoretical framework of John Maynard Keynes.

Early encounters

By 1987, Meltzer's interaction with Friedman stretched back over thirty years. As a graduate student, Meltzer had, around late 1955, seen Friedman give a presentation: "He came and gave a seminar at UCLA, on the permanent income theory, which was remarkable—I mean, it was the best seminar I'd ever gone to at that point." (Allan Meltzer, interview, April 21, 2013.)[115] Meltzer would receive his Ph.D. from UCLA in 1958 (Blaug and Sturges, 1983, p. 264), after which his continuing involvement in monetary economics led to more direct encounters with Friedman. The "first time I met him was in 1961 at the Econometric Society meetings"—at a session held on the afternoon of December 27 at which Meltzer was a presenter (see Econometric Society, 1962, pp. 581–582). "I gave a paper on the demand for money. And, in those days, the profession was much smaller, and Friedman was the chairman of the session. And I gave my paper, and there was a very good audience—Tjalling Koopmans and Leo Hurwicz and so on were in the audience. Anyway, I gave the paper, and it was critical of Milton, so he said, 'I want you to come to my workshop and present that.' And I, of course, said OK. People warned me, you know, 'He'll eat you alive. He's really a master of debate, and he'll just take your skin.' Well, I listened to that, but I was going to go no matter what the consequences might be. Well, it was absolutely totally different." (Allan Meltzer, interview, April 21, 2013.)

This workshop visit took place in the first half of 1962. "I went to Chicago, I met him in his office, we went to lunch with the people in the department. Afterwards, he said to me, 'Come [back] to my office. I want to talk to you.' He said, 'I have three main questions about your paper,' and he gave me the questions. He said, 'Now, you have two hours to prepare your

[115] Meltzer had been a graduate student at UCLA starting in the 1952/1953 academic year (Brunner, 1988b, p. 3). Friedman, who was in northern California in the 1957/1958 academic year, may have made a seminar visit to UCLA during that time, but this was after the consumption-function book (Friedman, 1957a) was completed. In addition, Meltzer had apparently already left the UCLA campus by the end of 1956 (Brunner, 1988b, p. 4; McCallum, 1998, p. 243). However, while still working on the book, Friedman was in Los Angeles in early October 1955, *en route* to Asia (see Friedman and Friedman, 1998, pp. 257–258).

answers.' In a long career, no one has ever done anything like that for me [or] to me. We started the seminar, he asked the questions, I gave him my answers, and we went on. And, at the time, Franco Modigliani, who was [previously] my colleague—we actually had a carpool together at Carnegie Tech—was at Northwestern. So he came down to the seminar. So Friedman's way of running the workshop was we start on page one, and we ask the students [and other attendees] if there are any questions about page one, then we go to page two. So Modigliani said, 'I have a question,' and Milton said, 'Have you read the paper?' He said no. He [Friedman] said, 'Then you can't speak in this workshop.' (*Laughter*)." (Allan Meltzer, interview, April 21, 2013.)[116]

Meltzer recalled Friedman's conduct during the workshop appearance as "very cordial and helpful" (Allan Meltzer, personal communication, February 18, 2014). Friedman's questions at the seminar had not focused on the respect in which the Meltzer paper—subsequently published in the *Journal of Political Economy* (Meltzer, 1963)—had taken issue with Friedman—namely, the fact that Friedman, as Meltzer put it, "excluded interest rate" terms from the empirical demand-for-money function in his 1959 article on the demand for money.[117] At the seminar, Meltzer largely put this matter in abeyance: "I was a young squirt and not prepared to challenge him more than I had in the paper." (Allan Meltzer, personal communication, February 18, 2014.)

Meltzer did, however, press his case against Friedman in his review article (Meltzer, 1965) concerning the Friedman-Schwartz *Monetary History*. The *Monetary History* had not, in fact, postulated that the demand for money was interest-inelastic, in the case of either M1 or M2, though it had cast doubt on whether M2 velocity's postwar rise reflected interest-rate behavior. Meltzer (1965), however, took Friedman, in this and previous work, as having imposed an interest-insensitive money demand function. Friedman decided to respond to this, as well as to related commentary by others, in an article in the October 1966 issue of the *Journal of Law and Economics*. Meltzer was incensed by the article—"He later [in 1966] tried to justify doing that [his treatment of interest rates in 1959] despite the evidence that he was wrong." (Allan Meltzer, personal communication, February 18, 2014). His feelings were intensified by the fact that he saw the article only many months after it appeared in print: possibly due to an oversight, Meltzer had not been shown a draft of Friedman's paper before its publication.[118] Friedman, in his turn, had produced the article because he was irritated at being characterized by Meltzer and others as having denied the interest sensitivity of the demand for money.

[116] Modigliani was at Northwestern University from 1960 to 1962 after having previously been at the Carnegie Institute of Technology from 1952 to 1960. See Blaug and Sturges (1983, p. 272). Meltzer joined the Carnegie Institute of Technology in 1957 (see McCallum, 1998, p. 243).
[117] That is, Friedman (1959).
[118] See Meltzer's letters to Friedman of June 27 and July 6, 1967, Milton Friedman Papers, Hoover Institution.

Friedman also rejected the contention—mainly made by critics other than Meltzer—that an interest-inelastic money demand was essential for obtaining key quantity-theory results. On this matter, Meltzer concurred with Friedman: they agreed that key neutrality propositions about prices, rather than output or real interest rates, eventually absorbing monetary changes did not depend on money demand being interest elastic.[119]

Branches of monetarism

The height of Meltzer's being in step with Friedman was reached when, in Friedman's absence, Meltzer served as a visiting professor at the University of Chicago's economics department over the 1964/1965 academic year (American Economic Association, 1970, p. 294). "I taught his monetary theory course, and I ran his workshop." In conducting the workshop, Meltzer followed "the Friedman format"—"I mean, that was the format, the students were used to that, [and] I was a visitor, [so] I wasn't going to change that." (Allan Meltzer, interview, April 21, 2013.)

From then on, however, a divergence became more apparent. In his 1965 review of the *Monetary History* and in numerous other ways in the later 1960s, Meltzer made it clear that he felt that Friedman was open to challenge on several fronts as an authority on monetary matters. With Karl Brunner, his UCLA dissertation adviser and, from the early 1960s onward, frequent coauthor, Meltzer published an account of the reasons for the Great Depression. Although this study—Brunner and Meltzer (1968a)—concurred with Friedman and Schwartz on the monetary source of the depression, it challenged their emphasis on changing Federal Reserve leadership as the reasons for the U.S. monetary policy errors. Instead, Brunner and Meltzer emphasized the effect of monetary decisions in the 1930s of the continuation in policymaking of a doctrinal framework that had been operative since the 1920s. In arguing this case, Meltzer had engaged in sharply worded correspondence with Friedman and Schwartz, mainly the latter—with disagreement expressed on the weight to be attached to, and interpretation put on, specific statements by Federal Reserve officials.[120] The Brunner-Meltzer interpretation, articulated in print in the 1960s, of 1930s monetary policy developments would later underlie Meltzer's (2003)

[119] "He simply did not want to let financial markets matter. I suspect that was because he considered that transitory, but I don't know." (Allan Meltzer, personal communication, February 18, 2014.) In Nelson (2020b, Chapter 12), Friedman's (1959, 1966) analysis is interpreted as implying that interest rates matter for money demand but play only a transitory role in affecting the relationship between (rates of change of) money and nominal income (see also Friedman and Schwartz, 1982a, p. 162). See also Belongia and Ireland (2021) for a discussion of the link between money and income under different settings of the interest elasticity of money demand.

[120] See, for example, Meltzer's letter to Schwartz (with Brunner and Friedman CC'd) of June 28, 1967, Milton Friedman Papers, Hoover Institution.

account of the Great Depression in his history of the Federal Reserve—an analysis based much more heavily on access to internal documents than the previous studies had been.

The Brunner-Meltzer reinterpretation of monetary history and the Meltzer/Friedman argument over the interest elasticity of money demand presaged an era in which Meltzer, marking out his views more forcefully, had more frequent, albeit cordial, disagreement with Friedman, in contrast to their friendly 1961–1965 relations.[121] This new state of affairs was associated with a situation in which "Allan Meltzer used to say to me, 'I can never send anything to Friedman that he doesn't tear apart,'" Anna Schwartz observed (Nelson, 2004, p. 405). To others, too, Meltzer also expressed a jaded view of Friedman's reaction to his research: James Lothian recalled that when Meltzer lamented "what a hard time [he] got" presenting work before Friedman, "I said, 'Well, you know, welcome to the club, Allan.'" (James Lothian, interview, October 24, 2013.)

Meltzer himself became known in the economics profession as having a tendency to be combative. Franco Modigliani, speaking in August 1982, reported he had detected a change in Meltzer's outlook since their 1957–1960 period as colleagues, saying he now had "a lot of problems with" Meltzer, whom he still considered a friend but judged to be "prone to pick a fight" (Klamer, 1984, p. 119).

One of the matters on which Meltzer was likely to enter dispute was a label Modigliani gave him of "the people associated with Chicago" (Klamer, 1984, p. 119). He and Brunner repeatedly emphasized that their development of a monetarist framework was distinct from the work produced by Friedman or at the University of Chicago. "I always sensed a little bit of a rivalry and that Allan Meltzer, particularly, kind of resented [how] Milton Friedman got all the glory…," Ann-Marie Meulendyke observed (interview, April 29, 2013). "He carved out his own niche, but I suspect that, back then, he somewhat resented that Friedman got both the arrows and the praise."

Not only were they operating independently of Friedman, but also Brunner and Meltzer were, as they saw it, making strides in analytical grounding to monetarist analysis that Friedman did not. One aspect of this effort was in the area of money supply theory—specifically, the detailed theoretical analysis of the monetary-base/money-multiplier process—an endeavor in which Brunner and Meltzer were far more involved than Friedman was.[122]

[121] This attitude came more from Meltzer than Brunner. "Karl Brunner generally looked up to Friedman. But that was not Allan's position," Anna Schwartz told the present author (personal communication, May 29, 2009).
[122] See Nelson (2020b, Chapters 12 and 13) for further discussion.

Another aspect of Brunner and Meltzer's theoretical research concerned a matter on which Friedman had offered analytical commentary in his past writings, and would again in 1987−1992, but not theoretical modeling: the transmission mechanism of monetary policy. Both Brunner-Meltzer and Friedman stressed the notion that multiple (real) interest rates or asset yields mattered for the private sector's real spending decisions, that the assets associated with these interest rates were imperfect substitutes for one another, and that monetary policy's effect on yields did not work solely through effects on interest rates on short-term, low-risk assets.[123] Proceeding from this logic, Brunner-Meltzer and Friedman had, in particular, insisted that a model of monetary policy transmission should ideally have at least two assets—and not only one—distinct from money. But it was the Brunner-Meltzer team that had actually set out such three-asset models.

The monetarist literature and the transmission mechanism

One of the initial Brunner Meltzer articles along these lines was published in the *Journal of Political Economy* in early 1968, after the paper had been presented at various locations, including the University of Chicago, and following the receipt of written feedback that included what the authors called the "the detailed comments of Milton Friedman."[124] The paper concerned the liquidity trap—the suggestion that monetary policy's scope to stimulate nominal and real spending can run out, particularly once short-term interest rates have been brought to zero. The authors came out firmly against the liquidity trap, as "there is always some policy action that reduces" the interest rates that matter for aggregate demand. The "policy action" in particular was one involving monetary quantities: "Since interest rates can always be reduced and the money supply can always be increased, there cannot be an absolute liquidity trap…"[125]

With regard to the specifics of the 1930s, Brunner and Meltzer made a declaration that even as far as securities-market yields were concerned, claims "that interest rates reached a 'floor'" in the early 1930s—and so could not be lowered further by monetary means—were contradicted by the "decline in long-term interest rates after 1938."[126] This observation implied a concrete proposition that, in the 1930s, the authorities could have stimulated the economy by buying longer-term bonds. This proposition has become a well-known part of the monetarist position, with Federal Reserve Chair Jerome H. Powell observing in November 2021: "The other thing

[123] For further analysis, see, for example, Orphanides (2004), Ireland (2019, pp. 50, 52), and Nelson (2020b, Chapter 5).
[124] Brunner and Meltzer (1968b, p. 1).
[125] The quotations are from Brunner and Meltzer (1968b, p. 18).
[126] Brunner and Meltzer (1968b, p. 2). See also their page 24.

you can do is you just go buy those securities, buy longer-term securities. That will drive down longer-term rates… [and] encourage more economic activity... I think Milton Friedman said that that was what you could do if you were pinned at the lower bound, many, many years ago."[127]

Friedman had indeed articulated this point himself.[128] In addition to expounding on numerous occasions since the 1950s the notion that monetary policy could affect the prices of multiple nonmonetary assets, Friedman had specifically suggested that long-term rates were susceptible to monetary policy influence for a given path of short-term rates. For example, in 1962, he had stated: "There was no liquidity trap in the late 1930s in any meaningful or Keynesian sense."[129] The "Keynesian sense" of a liquidity trap had been one in which monetary policy was rendered incapable of affecting the long-term interest rate.

Friedman continued to stress the multiple-yield channels in later years: for example, in *Money Mischief* in 1992, he remarked that, although yields on different assets converged to similar values over long periods, "at any one point in time, real returns may vary widely for different classes of assets."[130] And in a 2001 interview he stressed that, faced with zero short-term nominal interest rates, the Bank of Japan should buy long-maturity securities to put downward pressure on longer-term yields (United Press International, 2001).[131]

As already indicated, however, it was Brunner and Meltzer, and not Milton Friedman, who actually put the monetarist challenge to the liquidity trap into a model-based analysis—and, in their 1968 paper, it was they who applied it to the analysis of the Great Depression, including with regard to the behavior of longer-term interest rates. "I would say we tried to spell out in more detail a lot of the ways in which the mechanisms would work," Meltzer later recalled. "I don't think there was ever a conflict between what we were doing and what he was doing, but I

[127] Powell (2021, p. 21).

[128] Meltzer (2004, p. 201) himself acknowledged that in Friedman's "model, money can affect a wide spectrum of asset prices." See also the Brunner and Meltzer references to Friedman quoted in Nelson (2020b, Chapter 13).

[129] In Ketchum and Kendall (1962, p. 52).

[130] Friedman (1992c, p. 25).

[131] Friedman did not hold that purchase operations directed at longer-maturity securities would *keep* long-term rates down—but was confident that they provided a means of boosting monetary growth and of delivering upward pressure on overall asset prices, with a resulting stimulation of aggregate nominal and real spending. His belief in the strong influence of monetary policy on aggregate demand and in the feedback between interest rates and the economy meant that, in contrast to Keynes (1936, p. 206)—who seemed to hold that the primary means of refuting the idea of a liquidity trap would be to show that the authorities could push longer-term interest rates below a perceived floor—Friedman (1970c, p. 215) and Friedman and Schwartz (1982a, p. 55) implied that a liquidity trap did not exist if a bond-rate pegging policy (one pursued once short-term rates were already near zero) generated reactions of aggregate spending and prices that tended to *raise* bond rates and that also required a tightening of monetary policy on economic-stabilization grounds. This was also a message of various Friedman analyses of interest-rate behavior and of rate-pegging policies published during the 1960s, including Friedman (1968b).

think he really took a very broad brush to many of those things—[he] got to the right place, but without spelling out quite how he got there." Meltzer argued that Friedman "did theoretical work that stood the test of time" but less so in this area.[132]

As far as Meltzer was concerned, in the 1968 article and in other contributions appearing in the 1960s and 1970s, he and Brunner had both accepted the challenge of formally modeling a multiple-yield transmission mechanism (in a situation in which assets were imperfect substitutes for one another) and had succeeded in meeting that challenge. "If I recall correctly," Meltzer once emailed Bennett McCallum, his longtime fellow monetary economist in Carnegie Mellon University's business school, following a conversation they had had on the subject of monetary policy channels, "you did not disagree (or agree) with this when I began to describe the process [of monetary transmission under imperfect asset substitution] in the hall. Your only response was that it would be hard to model. I agree. It was hard."[133]

In view of Brunner and Meltzer's contributions on the transmission mechanism, it was therefore a jarring omission when, in Friedman's *New Palgrave* entry on the quantity theory of money, published in 1987, neither Brunner nor Meltzer was mentioned at all.

A couple of passages of Friedman's entry discussed the traditional Keynesian view of monetary policy transmission and the corresponding monetarist view. In the first of these passages, Friedman simply repeated the practice of an encyclopedia entry on the quantity theory he had produced earlier (in 1968), citing Tobin (1961) as a reference that discussed the older Keynesian tradition (from which Tobin himself departed) of consolidating the alternatives to money into a single financial security.[134] In the second passage on the transmission mechanism, when stating that quantity theorists believed that "the total spectrum of rates" mattered in monetary analysis, Friedman gave no references at all.[135] A counterpart to this sentence in *Monetary Trends* in 1982 had cited Friedman's writings but also the discussion of the transmission mechanism in Brunner (1970a). As part of this citation, it had referred to Friedman and Schwartz and Brunner and

[132] Allan Meltzer, interview with Federal Reserve Bank of Dallas President Robert McTeer, October 2003, provided by Mark Wynne.
[133] Email from Allan Meltzer to Bennett McCallum, August 14, 1999, with the present author CC'd.
[134] Friedman (1987a, p. 13). Compared with the corresponding passage in his 1968 entry (Friedman, 1968c, p. 439), Friedman did add a reference to Hansen (1957, p. 50) alongside the Tobin reference. But the actual Hansen passage cited pertained to material discussed earlier in the paragraph to which Friedman attached it—and not to the point about asset substitution.
[135] Friedman (1987a, p. 13). The *New Palgrave* entry had no footnotes, but Friedman had the option available of including references in the text of his article, as he did in the case of his own writings and numerous other authors, including items appearing in the 1970s and 1980s.

collectively as “we.”[136] This impression of solidarity with Brunner and Meltzer was, however, absent from the *New Palgrave* entry, as was any acknowledgment by Friedman of their work. The upshot was that the entry had no mention of Brunner or Meltzer whatsoever.[137]

Friedman was certainly familiar with Brunner and Meltzer’s formal modeling work on the transmission mechanism, including the 1968 *Journal of Political Economy* paper and another one in the same journal in 1972.[138] But he was not taken with the work, notwithstanding his polite public remark in 1972 that he applauded and welcomed it.[139] The truth was that “Milton thought that Brunner and Meltzer’s macro-model was too formalistic and complicated,” as Michelle Fratianni observed (interview, April 25, 2016).

Meltzer very likely took note of Friedman’s *New Palgrave* omission. In 1992, Meltzer was commissioned by Warner Books to write an entry, “Monetarism,” for the *Fortune Encyclopedia of Economics*, to appear in summer 1993. His entry (Meltzer, 1993b) showed that just as Friedman could write an encyclopedia entry on the quantity theory without mentioning Allan Meltzer, Meltzer could produce an entry on monetarism whose text made no mention at all of Milton Friedman.[140] The only appearance of the word “Friedman” in the entry was a citation in the bibliography of the Robert Gordon-edited book *Milton Friedman’s Monetary Framework* (which had included a Brunner-Meltzer paper).

Disagreement on monetary policy stance

These slights notwithstanding, it was possible, in the later 1980s and the early 1980s, to find evidence of more collegial signals toward one another emanating from Friedman and Meltzer. Some of this was in evidence during 1986. As discussed in Chapter 13 above, Friedman had coauthored an article that appeared in a Karl Brunner tribute that appeared at the start of that year. And later in 1986, Brunner and Meltzer praised Friedman’s achievements in the U.S. press (see Nelson, 2020b, Chapter 13).

[136] See Friedman and Schwartz (1982a, pp. 58–59; quotation from p. 58).
[137] Although it had not cited Brunner and Meltzer specifically in connection with the transmission mechanism, the Friedman (1968c) entry on the quantity theory of money had cited Brunner and Meltzer’s (1963) empirical work on money demand. This reference was absent from the *New Palgrave* entry, which used more recent references on the subject.
[138] See Brunner and Meltzer (1972a), mentioned by Friedman (1972a, p. 912).
[139] Friedman (1972a, p. 912).
[140] Its text partially overlapped with an article in *National Review* (November 4, 1991) that Meltzer had written and that also did not mention Friedman.

Brunner and Meltzer's esteem for Friedman was also evident in a guest lecture program held under the auspices of the Raffaele Mattioli Foundation, delivered by them in September 1987 and put in final written form by Meltzer after Brunner's death for 1993 publication.[141] The location of the lectures—Italy, specifically Bocconi University in Milan—and their title (*Money and the Economy*) underlined the fact that Brunner and Meltzer had, ever since the 1950s, been far more active in Continental Europe's debates on monetarism, and much more attentive to European affairs, than Friedman had been.[142]

The Brunner-Meltzer lectures had discussed the countermovement in economics against Keynesianism beginning in the 1940s, particularly that emphasizing the importance of money. After noting that Clark Warburton's contributions on this matter had been notable but had made little impact, Brunner and Meltzer noted laconically that "Milton Friedman was more difficult to ignore." In the ensuing discussion, the authors made the important point that Friedman's critique of Keynesian economics had actually begun during the 1940s and went on to discuss in detail his more specifically monetarist contributions as well as his other work on macroeconomics.[143]

Another factor acting to promote good Friedman/Meltzer relations was the number of fellow monetarists who were friends of them both.[144] Most notable among these was Anna Schwartz, who would see Meltzer regularly at SOMC meetings throughout the 1980s and 1990s. But Friedman's and Meltzer's mutual friends in the monetarist movement also included Jerry Jordan, William Poole, and Beryl Sprinkel—all three of whom served at various times in the Reagan Administration's Council of Economic Advisers. Meltzer would actually follow in the footsteps of these three when, at the tailend of the Reagan years, from September 1988 to January 1989), he served as a "consultant" to the CEA—in practice, an acting (but not Congressionally confirmed) CEA member and *de facto* CEA chair (see Meltzer, 1995, p. xxi).

Meltzer was therefore in Washington in time to see Reagan give Friedman the Presidential Medal of Honor. The ceremony itself, held in October 1988 and discussed in the previous chapter, underlined the fact that Reagan's economic team had not only had prominent members,

[141] See Brunner and Meltzer (1993, pp. xv, xvi, 5). Meltzer confirmed to the author (in personal communication, October 30, 2000) that Meltzer made final revisions of the lectures after Brunner's death.

[142] This reality had been reflected in *Business Week*'s reference (June 27, 1977, p. 86) to "the monetary theories of Nobel laureate Milton Friedman, as adapted to Europe by Karl Brunner, who splits his time between the University of Bern and the University of Rochester." Of course, the characterization of Brunner as having merely adapted Friedman's analysis to European conditions was one that he and Meltzer would, with justice, have rejected.

[143] Brunner and Meltzer (1993, p. 18–19; quotation from page 18).

[144] The mutual friends also included figures like Alan Greenspan, Charles Plosser, and John Taylor, who, although not monetarists, had considerable sympathy with the monetarist literature and movement.

most recently Meltzer, who typically had a large amount of agreement with Friedman on policy questions, but also those with whom Friedman was at odds. One of Friedman's fellow medal recipients at the ceremony, former Secretary of Commerce Malcolm Baldridge, was a prime member of the latter group: Friedman had previously lamented the "blatantly protectionist measures" advanced by Baldridge (*Wall Street Journal*, April 20, 1987).

Another of the economists glad to call himself both a monetarist and a longtime friend of both Friedman and Meltzer was Robert Lucas. The year 1988 saw the publication of a Lucas study that concerned an issue close to the heart of both Friedman and Meltzer: the demand for money. Lucas' (1988) paper had been produced for the November 1987 Carnegie-Rochester conference, held in Pittsburgh and organized as a tribute to Meltzer, who was turning sixty the following February. Lucas, himself about to turn fifty and largely working outside monetary economics, had initially told conference organizer Bennett McCallum that he could not contribute a paper, as he was becoming more sparing in the number of research projects that he took on. McCallum was therefore surprised to find a paper arrive in the mail a few weeks before the conference. Lucas had had a change of heart and had produced a full draft, which McCallum added to the conference program.

The Lucas paper, "Money Demand in the United States: A Quantitative Review," was explicitly an update to Meltzer's (1963) money demand study, which had appeared in print during the same year in which Lucas became a colleague of Meltzer's at the Carnegie Institute of Technology. Lucas (1988, p. 137) noted that Meltzer (1963) "followed earlier work… especially Friedman." Lucas found, however, that it was a particular estimated equation due to Meltzer—an M1 demand function featuring a unitary income elasticity and a sizable interest-rate elasticity—that delivered numerically stable parameter estimates, if extended on annual data from 1958 (the end of Meltzer's sample period) to 1985 (the end of Lucas' estimation sample).[145] As part of the process of replicating and extending Meltzer's work, Lucas used the national-income and price data for the years 1884–1975 published in Friedman and Schwartz's *Monetary Trends*.[146] He

[145] Lucas (1988, p. 153) noted that his results might be interpreted as "non-monetarist" in view of the simple arguments—income and a short-term nominal interest rate—he used in his specification of money demand. Friedman and Meltzer, in contrast, favored the inclusion of permanent income and a variety of asset-price variables in the money demand function. If, however, Lucas' estimated equation is interpreted (as it was by Stock and Watson, 1993, for example) as a cointegrating regression, his specification is consistent with the Friedman-Meltzer approach. Lucas' parsimonious specification is then a permissible description of the long-run money demand equation implied by monetarist analysis, with income standing in for permanent income, and the asset-price variables omitted by Lucas either being stationary or being cointegrated with the short-term nominal interest rate.

[146] This meant that Lucas' data incorporated the adjustments of these series that Friedman and Schwartz had made in order to allow for the effects of price controls in World War II and the Nixon years.

also sent a draft of the conference paper to Friedman, who provided Lucas with comments.[147] Lucas' finding of a long-run stable demand for M1 was bound to be of less interest to Friedman, who by 1987 was largely back to centering his monetary analysis on M2, than it was to Meltzer, who had consistently favored a focus on monetary aggregates narrower than M2.

By the late 1980s, Meltzer's particular favored monetary aggregate had become the monetary base. It was Meltzer's confidence in the base as an indicator of U.S. monetary conditions that led, in 1992, to a conflict between his evaluation of Federal Reserve policy and that being provided by Friedman.

The two of them interpreted the decline in nominal interest rates during 1991 and into 1992 differently. Friedman saw it as a delayed acquiescence by the Federal Reserve to market forces associated with the recession and as occurring in conditions of monetary restraint: interest-rate declines in 1991 were "primarily, in my opinion, the result of two things. One is the sharp reduction in inflationary expectations. And the other thing is the recession… which has reduced sharply the demand for loans." (Milton Friedman, interview, January 22, 1992.) Meltzer, in contrast, viewed the interest-rate decline, particularly that observed late in 1991, as largely reflecting a precipitate move by the FOMC and the Federal Reserve Board to push down yields—and so as a symptom of undue monetary ease.

Each of the two monetarists had a monetary aggregate to back up their interpretation: M2 in the case of Friedman, the monetary base in the case of Meltzer. M2 growth had been slow in the 1990s so far, while monetary base growth had been rapid.

In op-eds published on different coasts of the United States, Friedman and Meltzer each went beyond their interpretation of current monetary conditions to express a judgment about whether the Federal Reserve was, under Greenspan, basing its policy decisions on technical or political judgments. Friedman came out on the side of the former interpretation. To Friedman, who believed that the Federal Reserve had sought a "stable and noninflationary" policy and had been unintentionally tight, it was the case that "monetary policy has continued on a restrained path" despite pressure applied by the administration (*New York Times*, February 2, 1992).[148] In contrast, Meltzer rushed to the judgment that the "allegedly independent" U.S. monetary

[147] See Lucas (1988, p. 138). In the event, however, the final version of Lucas' paper was little different from the conference draft.
[148] By late in the year, though he had reservations about the way the administration had made its views known, Friedman agreed with the administration that monetary policy should be eased.

authority had engaged in a "capitulation" so that following "a four-year effort [Greenspan's first four years] to restore price stability, the Federal Reserve caved in to pressures to reinflate in advance of the [1992 presidential] election." (*Los Angeles Times*, February 2, 1992.)

At a January 1993 Congressional hearing, Senator Paul Sarbanes (D–MD), a critic of monetarism, seized on the schism that had emerged between Friedman and Meltzer regarding current monetary policy. Sarbanes quoted Friedman's *Wall Street Journal* (October 23, 1992) op-ed statement that the "Fed needs to speed up sharply monetary growth." Sarbanes asked: "So you disagree with Professor Friedman?" "Indeed," Meltzer answered. Monetary growth "hasn't slowed?," Sarbanes asked further. "No," Meltzer replied, "My chart shows that it has been about constant… somewhere between 7 and 8 percent." With regard to Friedman's warning four months earlier that the Federal Reserve's monetary stance was too stringent, Meltzer proclaimed: "the facts have not borne that out, of course."[149]

Jerry Jordan, who became president of the Federal Reserve Bank of Cleveland (making him a policymaker-participant at FOMC meetings) on March 10, 1992, had direct dialogues with both Friedman and Meltzer over this period.[150] Jordan remarked wryly that, as Friedman considered the FOMC's stance to be too tight and Meltzer was warning that, on the contrary, it was too loose, U.S. monetary policy was likely doing something right (Jerry Jordan, personal communication, October 31, 1993). Meltzer considered this reaction to be little comfort (Allan Meltzer, personal communication, August 25, 1994).

In retrospect, how should one arbitrate between these conflicting monetarist judgments of what the stance of U.S. monetary policy was at the end of five years of Alan Greenspan as Federal Reserve chair? The appropriate judgment seems to be that both Friedman's and Meltzer's assessments were flawed—but that Friedman proved to more correct than Meltzer was.

Friedman assessed accurately that monetary conditions in 1992 were insufficient to generate a strong recovery, that inflation was poised to fall further under current policy settings, and that further policy easing (something the FOMC went on to provide during 1993) would be consistent with the FOMC getting closer to both its employment and price-stability goals. He was also correct in seeing the decline in U.S. nominal interest rates in the early 1990s as largely reflecting

[149] From the hearing of February 11, 1993, in Joint Economic Committee (1994, p. 91).

[150] On Jordan's ascension date, see *https://fraser.stlouisfed.org/title/statements-speeches-jerry-l-jordan-3769?browse=1990s*. Jordan would jointly organize, with Walter Oi, the Western Economic Association meetings session in July 1992 marking Friedman's eightieth birthday, which featured the Meltzer (1993c) article discussed below.

a fall in the natural real rate of interest (as well as the Fisher effect on the expected-inflation component), rather than overt monetary stimulus: see, in particular, the steep decline in the estimates of the natural real rate in this period in Laubach and Williams (2003, p. 1067). Nevertheless, Friedman overestimated the degree of monetary stringency during 1992 (and 1993), as conditions were not actually as tight as the low rate of M2 growth suggested.

Meltzer, in contrast, was more seriously in error. He was off base, so to speak, in regarding U.S. monetary conditions in the early 1990s as being loose. In reaching this judgment, he had placed altogether too much weight on the raw monetary base growth-rate data. In this connection, Meltzer during the early 1990s underestimated the degree to which rapid base growth was driven by flows of American physical currency abroad—as the economies of Russia, other one-time Soviet republics, and its former Eastern Europe satellites became partially dollarized, thereby absorbing U.S.-created base money.[151] A major geopolitical event—the end of the Cold War—had scuttled Meltzer's analysis of U.S. monetary developments.

Challenging Tobin and debating Patinkin

To watchers of the media, it might seem that Friedman's debating activity with his longtime Keynesian sparring partners was continuing apace during the late 1980s and early 1990s. He debated Robert Solow (who had recently become a fellow Nobel economist) in a November 1987 network television discussion (mentioned in the previous chapter) regarding the stock market crash, was arrayed against James Tobin in a magazine piece at the turn of the decade, and appeared opposite Paul Samuelson a couple of times on U.S. public television in the early 1990s.[152]

What this public-policy activity disguised, however, was Friedman's general absence from debate with Keynesians in the research literature over this period. Allan Meltzer filled this void. In the late 1980s and early 1990s, at a time when Friedman had ended most of his exchanges in research forums with his longtime Keynesian opponents, Meltzer was stepping them up.

[151] See R.G. Anderson and R.H. Rasche (1997, p. 32). By the summer of 1994, Meltzer was much happier with the state of U.S. monetary policy. Indeed, he was so satisfied by that point with how policymakers' actions and statements had developed under Alan Greenspan that he was contemplating winding up the Shadow Open Market Committee (Allan Meltzer, personal communication, August 25, 1994).
[152] *Nightline*, ABC, November 6, 1987; Friedman and Tobin (1990); *The MacNeil/Lehrer News Hour*, PBS, August 27, 1990; and *Wall Street Week With Louis Rukeyser*, Maryland Public Television, February 21, 1992. Samuelson and Friedman were also together at a January 1991 academic symposium (see Hinshaw, 1993).

Having already been James Tobin's opposite number at the 1987 Bank of Japan conference, Meltzer unilaterally escalated hostilities by publishing, in the *Journal of Monetary Economics*, a highly critical review of a new Tobin collection (Tobin, 1987). Meltzer's (1989) review did not mention Friedman by name. But it zeroed in on the same critique of Tobin's position on inflation control that Friedman had made in his 1977 televised debate with Tobin. On that occasion, Friedman had asked Tobin to put aside the case of the United States in the early 1960s and to name *another* instance in which, according to Tobin, an incomes policy helped deliver price stability. In response, Tobin would not name any instance (*The MacNeil/Lehrer Report*, PBS, April 18, 1977).[153] Twelve years later, Meltzer (1989, p. 169) noted that Tobin still had not. "Why must Tobin rely on a few questionable claims about the success of guideposts in 1961 to 1965…?," he asked.

The principal Keynesian whom Meltzer faced during the 1980s and early 1990s was, however, not Tobin, but another old antagonist of Friedman's: Don Patinkin. In the 1972 symposium at which Patinkin and Meltzer had been among his critics, Friedman had noted that "Brunner and Meltzer [1972b]… do not comment in any detail on my interpretation of Keynes."[154] Meltzer would make up for his reticence in interpreting Keynes' views in the 1972 symposium by producing an article (Meltzer, 1981) and then a book (Meltzer, 1988) on Keynes' monetary theory. Each of these publications would give rise to withering critiques by Patinkin (1983, 1988). Patinkin's challenge to Meltzer's book would then lead to an acrimonious exchange between the two in the *Journal of Monetary Economics*, published as Meltzer (1992) and Patinkin (1993).[155]

The references to Friedman in Meltzer's *Keynes's Monetary Theory* book were mostly in passing. Several of them were in connection with the permanent income hypothesis—with Friedman's monetary work receiving coverage only briefly in the book.[156] And Friedman's interpretation of Keynes was not mentioned at all, despite Meltzer's inclusion of a reference (Friedman and Schwartz's *Monetary Trends*) that had expounded this interpretation.[157] Meltzer's preface, dated January 1988, did, however, single Friedman out for praise, on account of his having "in characteristic fashion, pushed" Meltzer into covering "the central issues of

[153] See also Chapter 8 above.
[154] Friedman (1972a, p. 907).
[155] This exchange was so acrimonious that Meltzer, usually prone to re-engage in debates in debates with opponents, remarked that, as far as he was concerned, communication with Patinkin was now over (Allan Meltzer, personal communication, August 25, 1994). In the event, Patinkin passed away about a year after Meltzer made this remark.
[156] For example, Meltzer (1988, p. 289) mentioned Friedman's (1969a) optimum-quantity-of-money analysis—which Meltzer erroneously took as implying that Friedman "favors" a rule involving steady deflation.
[157] *Monetary Trends* was used in the book only as a data source. See Meltzer (1988, pp. 30, 32).

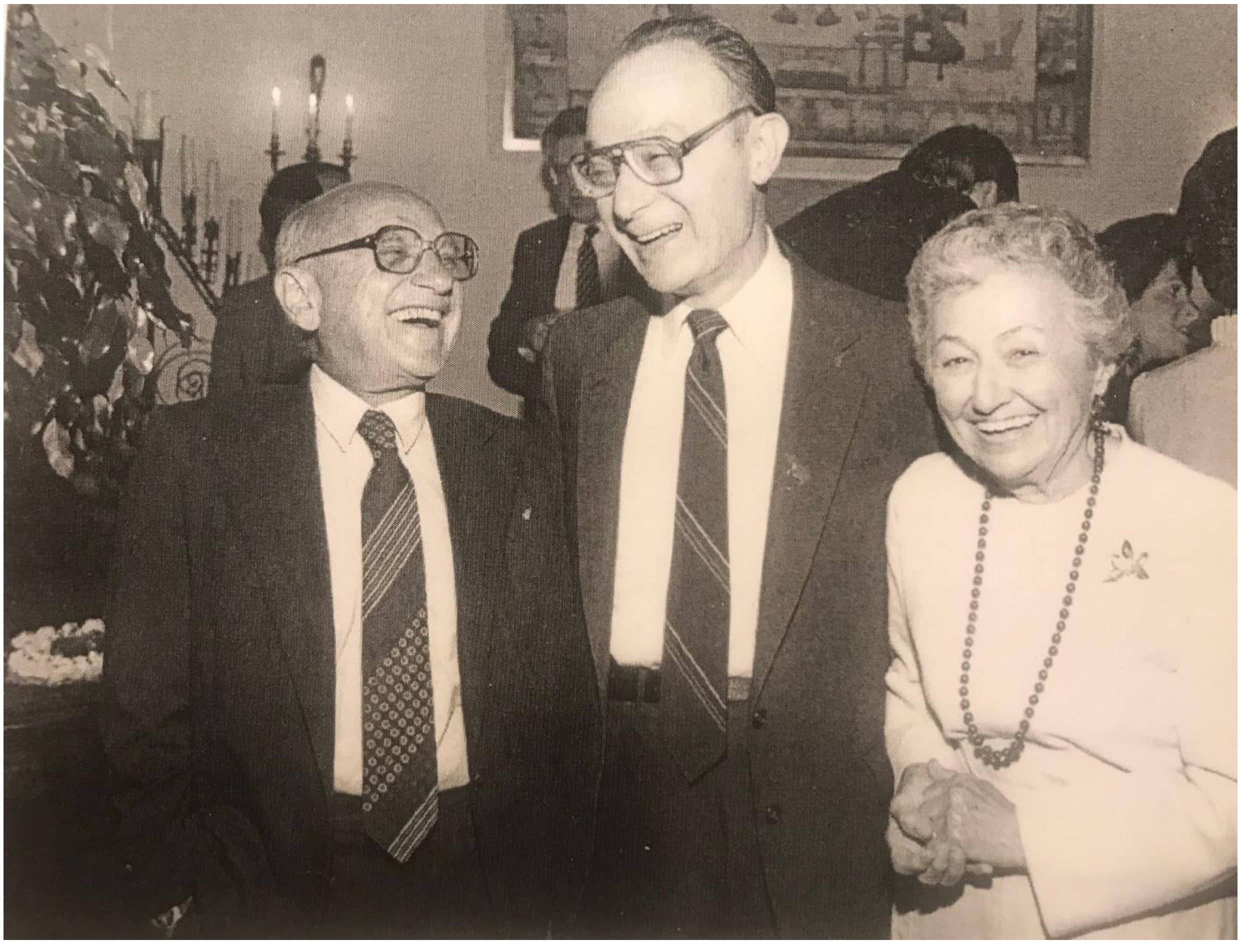

Figure 1. Milton Friedman, Allan Meltzer, and Rose Friedman in San Francisco, October 26, 1986.
Source: Photograph provided to the author by Allan Meltzer, May 2002.

economics." Friedman had done so, Meltzer explained, at a conference, organized by the Cato Institute, specifically concerned with discussing Meltzer's book draft.

This conference had taken place on October 26, 1986. A conference to discuss Allan Meltzer's work was hardly a natural fit for Friedman. His presence as a participant (see Figure 1) was likely driven by several factors. First, the conference was held in San Francisco—and so attendance involved no travel on Friedman's part. Second, monetarists with whom he was much friendlier than he really was with Meltzer would be attending—among them Anna Schwartz and David Laidler.[158] Third, Friedman was himself glad to opine on what Keynes' views were—and

[158] Meltzer specifically told the present author (personal communication, August 25, 1994) that he and Friedman were not close personal friends. This characterization lined up with their friendly and quite frequent, but not particularly close, interaction over the years.

had continued to do frequently during the 1980s. Fourth, the fact that Don Patinkin (who was not attending the conference) had already shown a dislike for Allan Meltzer's accounts of doctrinal developments in monetary economics, just as Patinkin had expressed disdain for Friedman's accounts of monetary thought, likely made Friedman more receptive to hearing what Meltzer had to say.

In retrospect, Meltzer maintained of Friedman: "He was MUCH closer to my interpretation [of Keynes] than to Alvin Hansen's. My interpretation, and Milton's, leave Keynes as much more of an economist than the Keynesians did. Milton seemed to go along with and even to reinforce my interpretation." (Allan Meltzer, personal communication, November 17, 2014.) A comparison of Meltzer's 1988 book with Friedman's writings on Keynes—including those written after Meltzer's book appeared—does not, however, support this interpretation. Instead, Friedman adhered to the traditional "U.S. Keynesian" view of the *General Theory* taken by Alvin Hansen, Paul Samuelson, Franco Modigliani, Robert Solow, and others.[159]

The principal aspect of Meltzer's interpretation of Keynes that he shared with Friedman was the emphasis on Keynes' advocacy of public-sector control of investment spending.[160] Meltzer's (1988, pp. 36, 189−191, 303–305) emphasis on this point lined up with Friedman's 1967 characterization of Keynes as desiring to "replace where necessary private investment by government spending."[161] But Friedman and Meltzer parted company on the crucial issue of whether Keynes took a monetary view of inflation—and, relatedly, whether he emphasized rigidity of nominal wages.

In this connection, Meltzer (1988, pp. 255−257, 312−313) contended that Keynes had not meant the assumption of wage rigidity to be vital in his account of how the economy operated and that Keynes did not believe in cost-push inflation.[162] Meltzer's proposed evidence in support of this point consisted of observing that Keynes saw workers as understanding the nominal-wage/real-

[159] For documentation of this point, see Nelson (2020a, Chapter 4; 2020b, Chapter 14).

[160] Meltzer recalled (interview, April 8, 2015): "It really was a much stronger economic argument about an externality [connected to investment spending] causing the lack of employment. Now, he [Keynes] might be right or wrong about that. But it was, in my judgment, a much firmer, stronger economic argument [than articulated in textbook Keynesianism]. After all, he was a student of Pigou's, so he understood about externalities. It was a much better argument than the arguments which were made in his name."

[161] Friedman (1967b, p. 9). This was an aspect of Keynes' policy prescriptions that leading U.S. Keynesians tended to downplay: for example, James Tobin remarked in 1986 that he did not put "much stock… in the [Keynes] phrase *socialization of investment*" (in Reese, 1987, p. 53). Similarly, Robert Solow remarked of Keynes (interview, July 7, 2014): "I don't think he had any interest in government control of investment as a method of resource allocation, of deciding which industries get the investment, and which industries don't, or anything of that sort."

[162] Meltzer also rejected the view that Keynes relied on the liquidity trap (p. 279) but did not refer to the textual evidence assembled by Friedman (1972a) in support of that interpretation of the *General Theory*.

wage distinction (1988, pp. 5, 163) and of simply asserting that "Keynes always regarded inflation… as a monetary problem, not as a problem of cost-push" (p. 313). In fact, neither of these contentions constituted valid evidence. The absence of long-run money illusion among agents does not itself imply the absence of rigidities in wages or prices.[163] And Keynes, while allowing for monetary forces as an influence on inflation when excess demand emerged, certainly *did* believe in cost-push forces as a major driver of inflation.[164]

Friedman continued to subscribe to the view that inflexibility of, and the dominance of autonomous forces in driving, nominal wages and prices formed a key part of Keynes' analysis. His adherence to this position, even after the appearance of Meltzer's book, was made clear in 1989 when Friedman and others, including Don Patinkin, were asked to contribute to a deluxe, limited-edition German-language book on Keynes. The lucrative assignment saw Friedman produce a chapter, which was published in Germany in 1989 and which Friedman had put out as a Hoover Institution working paper in the previous year (with this original English-language version eventually seeing print in 1997).[165] In a section of his chapter titled "The Message of the *General Theory*," Friedman stated: "To Keynes, it seemed clear that this process [the wage/price mechanism] had been inoperative or ineffective."[166] Friedman was evidently unmoved by the argument to the contrary offered by Meltzer—whom he did not cite or mention in the article.

Unlike a number of discussions of Keynes that Friedman had made earlier in the 1980s, the "John Maynard Keynes" article did not mention Keynes' anticipation of the trilemma idea, which had been extensively developed in Friedman's exchange-rate essay in 1953. The trilemma concept, and the associated notion of monetary policy autonomy under floating rates, would also lack prominence when, at a Western Economic Association session in July 1992, held to mark Friedman's eightieth birthday and with Friedman in attendance, Allan Meltzer presented a retrospective on Friedman's exchange-rate essay. In his list of four benefits of floating exchange rates given in the essay, Meltzer (1993c, p. 1999) buried the monetary-autonomy argument under the category of "harmonization of internal monetary and fiscal policies," although he brought the

[163] In the extreme instance in which wages and prices are driven, even in the long run, only by autonomous cost-push forces, it can still be the case that wage- and price-setters calculate their positions in real terms and have no money illusion.

[164] For documentation of this point and a discussion of its relationship to Friedman's interpretation of Keynes, see Nelson and Schwartz (2008, pp. 838–839).

[165] See Friedman (1988c, 1989c, 1997c).

[166] Friedman (1997c, p. 8).

point out somewhat more clearly in his subsequent discussion (p. 200).[167]

Meltzer's remarks were generous to Friedman regarding his other contributions: "It's hard to think of Milton Friedman at eighty… Few economists in the history of our discipline have contributed as much as Milton" (Meltzer, 1993c, p. 198). The only hint of his current disagreement with Friedman about U.S. monetary policy was provided by Meltzer's use of M1 as the measure of money in the empirical results he presented.[168] That empirical work, like the preceding discussion, concerned exchange rates, with Meltzer praising Friedman's 1953 exchange-rate essay as the "landmark piece on that subject." He noted, however, that among economic researchers there remained "critics of flexible rates."[169] As a "recent example" of such critics, he immediately named Paul Krugman.[170]

PAUL KRUGMAN

Paul Krugman, then based at the Massachusetts Institute of Technology, won the John Bates Clark Medal in 1991, exactly forty years after Friedman received the same award.[171] The official information released in connection with the conferring of the prize to Krugman described him as "the outstanding international economist among his generation" and as having "played a leading role in virtually every important development in international economics during the past decade."[172]

As this description indicated, Krugman—who, as of the early 1990s, was starting a major move from research to public-policy writing—was heavily focused on trade and other international issues, notably exchange-rate behavior. This orientation left a decided imprint on his early discussion of macroeconomic issues. Although Krugman would later become an exponent in the public square of textbook-style Keynesian positions on aggregate demand management, in the 1980s he implied that openness and the floating exchange-rate system had seriously compromised the ability of countries, including the United States, to exercise stabilization policy.

[167] Meltzer was apparently using the word "harmonization" differently from Friedman. Friedman (1953a, p. 199) had used "harmonization" to refer to pressure for internal monetary policies of different countries to be uniform under fixed exchange rates—not as an advantage of floating rates.
[168] Meltzer (1993c, p. 201).
[169] Meltzer (1993c, p. 199).
[170] Meltzer (1993c, p. 199), specifically referring to, but not formally citing, Krugman and Miller (1993).
[171] The biographical summary on the dust jacket of Krugman (1996) incorrectly gave his Clark award as having been given in 1992. (At the time, the medal was awarded only every two years. Therefore, no award was given in 1992.)
[172] American Economic Association (1991).

Along these lines, in early 1987 Krugman was quoted as saying: “No one has a reliable theory of exchange rates. That makes it very difficult to be sure of the effects of macroeconomic policy.” He was further quoted as implying that the floating exchange-rate system might well have made the U.S. aggregate supply curve quite upward-sloping in the short run: “If the Fed wants to weigh the effects of expansion versus inflation, that depends a great deal on whether monetary policy works on interest rates or on the exchange rate. That’s a significant inhibition on the Fed right now. We’ve lost certainty.” (*Dun’s Business Month*, January 1987.)

These remarks appeared in an article titled “Economic Policy: The Old Tools Won’t Work” and previously referred to at the start of this chapter. Friedman, though himself not an advocate of activist deployment of the “old tools,” explicitly took exception to the article, criticizing it for its attempt to up-end established work on the effects of monetary policy (*Wall Street Journal,* February 12, 1987). Friedman did not single out Krugman’s remarks for criticism. But, in its implication that floating meant that the proposition that monetary policy did not have decided effects on real economic activity in the short run could not be counted on to carry over to an open economy, Krugman’s stated position in the article had clashed sharply with Friedman’s longstanding views on floating rates.[173] Friedman’s exchange-rate essay had, in essence, stated that floating rates conferred on domestic demand-management policy much the same influence on real variables in the short run and prices over longer periods that it possessed in a closed-economy model. The upshot was that, according to Friedman, monetarist analysis of the macroeconomy needed little modification in the presence of openness, provided that the exchange rate floated. The same was true, for that matter, of Keynesian analysis: Friedman remarked, in his 1989 essay on Keynes, that “international trade… could readily be integrated into the [*General Theory*] analysis without affecting its substance.”[174]

Within a few years of his remarks quoted in 1987, Krugman had, in fact, largely come round to the view that openness or a floating exchange rate did not imply a steep aggregate supply curve. An early sign of this was his remark in lectures delivered in London in 1988 that “there is in fact tremendous inertia to nominal prices” (Krugman, 1989a, p. 24) and that currency depreciation in advanced economies tended not to see a close link between exchange-rate depreciation and inflation (p. 25). Krugman later became a proponent of the position that the aggregate supply curve was near-horizontal in the short run and that monetary policy could target real domestic

[173] At the time, the argument that it was not clear that monetary stimulus could be relied on to be felt initially mainly in real aggregate demand, due to the alleged danger that it would trigger a large depreciation that would make most of the response take the form of inflation, was a not uncommon view, albeit a minority one. See, for example, Argy (1992, p. 160) for another expression of it.
[174] Friedman (1997c, p. 8).

variables with considerable precision—reflected in his later remark that "the Fed's actions are the most powerful determinants of job growth in America," able to move employment by half a percent just by "a change in interest rates of a fraction of a percentage point."[175] Indeed, at a conference appearance alongside Friedman in early 1991 that is discussed further below, Krugman spoke of the "enormous power" the Federal Reserve had to stimulate the real economy: "The Fed has an unlimited supply of bucks."[176]

The fact that the large U.S. exchange-rate depreciation since 1985 had occurred alongside generally low inflation in the United States likely shaped Krugman's rethinking of the exchange-rate/inflation connection. Some Keynesian economists professed to see major inflationary forces as having been released on the U.S. economy by the depreciation. For example, writing in 1989, Walt Rostow (1991, p. 49) stated that the dollar's fall "has led to an increased inflation rate." But Krugman did not concur, and he judged that "the inflation rate... [was] far more stable in the second half of the 1980s than… for a long time."[177]

Nevertheless, in the 1988 lectures given in London, Krugman, despite already downgrading the repercussions of exchange-rate movements for inflation, came out against floating rates—"the markets have looked a more like [anti-floating rate writer] Ragnar Nurkse's vision than like Milton Friedman's," he suggested.[178] Krugman went on to indicate that he had dropped his own longtime support for floating rates. "I have now changed my mind… I am now an advocate of an eventual return to a system of more or less fixed rates subject to discretionary adjustment."[179]

The verdict that floating rates had hurt the world economy contrasted with Friedman's own judgment. For example, in rebuttal to the criticism that floating rates had held back U.S. economic performance, Friedman had cited the fact that the country's employment-to-population ratio and its share of trade in output had risen since the early 1970s (*Wall Street Journal*, October 29, 1987).

As for Krugman, however, his new posture of 1987–1988 favoring fixed rates over floating rates was not one to which he would adhere for very long. To be sure, in subsequent years, Krugman

[175] Krugman (1996, pp. 158, 159).
[176] In Hinshaw (1993, p. 35). Krugman added that as far as the provision of economic stimulation was concerned, "I don't see anything that needs to be done outside the Board of Governors." In saying this, he apparently conflated the Board, which set the discount rate, and the FOMC, which made overall monetary policy decisions.
[177] Krugman (1990, p. 85).
[178] Krugman (1989a, p. 98).
[179] Krugman (1989a, p. 99, emphasis in original). See also Krugman (1989b).

would continue to stress that exchange rates were not only hard to explain but also behaved perniciously. For example, in January 1991, he proclaimed in Friedman's presence: "exchange markets[,] in particular, have behaved just as badly as their critics, such as John Maynard Keynes, might have expected."[180] Even if valid, however, this contention was not devastating for Friedman's advocacy of floating rates. For, as discussed in Chapter 15, Friedman had on numerous occasions laid stress on the argument that monetary policy autonomy prevails under a floating rate even when the exchange rate is behaving in a puzzling way. Krugman evidently came to accept a version of this argument himself.[181] In the 1990s, he increasingly pressed the desirability and feasibility of domestic demand management, particularly in pursuit of full employment, in open economies.[182]

Krugman's attitude toward international macroeconomic policy coordination also soured over the 1990s. In 1991, he remarked: "Milton Friedman has delivered a verdict against it but has not offered an argument for that verdict." Krugman on that occasion stated that there was a theoretical case for coordination, although he himself expressed doubts about it.[183] By 1992, he was suggesting that the exchange-rate mechanism might have provided an example of "surprisingly durable" cross-country policy coordination.[184] In contrast, by the late 1990s, Krugman strongly advocated orientation of countries toward domestic goals: he urged that Japan pursue a domestic inflation target, and he was a critic of the planned European monetary union. Krugman also stressed the scope of U.S. demand management to insulate the home economy from weakness in economic activity abroad: "The U.S. economy is strong enough to ride it out." (*Evening Post* (Wellington, New Zealand), August 12, 1998.)

Krugman's convergence to the view that a floating exchange rate, even when volatile, did largely leave a country able to pursue domestic economic objectives was evident in the more positive remarks about Friedman's exchange-rate essay that he expressed in articles written in 1992 and published in 1993. A Carnegie-Rochester conference paper coauthored by Krugman was critical of the view that floating rates adjusted reliably to economic forces. But its only reference to Friedman's exchange-rate essay was favorable, quoting and endorsing his view that exchange-

[180] In Hinshaw (1993, p. 11). For the date of the conference, see Hinshaw (1993, p. xi).
[181] In Krugman (1993, p. 521), he acknowledged a closely related argument, which he attributed to James Tobin.
[182] For example, Krugman stated: "I don't think a small country like New Zealand should attempt to protect an uncompetitive clothing industry. If there is an unemployment problem, that should be addressed through macroeconomic policy." (*Evening Post* (Wellington, New Zealand), August 12, 1998.)
[183] See Hinshaw (1993, pp. 65–66, 73).
[184] Krugman (1993, p. 521). This observation, evidently made just ahead of the 1992–1993 exchange-rate crises, contrasted with Krugman's more negative verdict (given in early 1991) in Hinshaw (1993, pp. 42–43).

rate flexibility helped compensate for inflexibility of wages and prices at home (see Krugman and Miller, 1993, p. 280). And in an essay on exchange rates for the aforementioned Krugman praised the "seminal" 1953 Friedman essay, including its argument that it meant that "nations could pursue independent monetary policies."[185] Krugman argued that speculation drove exchange-rate variations more than Friedman had predicted, but he also conceded that exchange-rate volatility under floating rate had not given rise to major fluctuations in prices.[186]

Analyzing Japan

The microeconomic side of Krugman's research in international economics concerned how country-specific industrial policies contributed to national economic and trade performance. The case of Japan—widely regarded in the late 1980s as an ongoing success story, one with which the U.S. and other major economies compared unfavorably—was of particular interest to researchers on this score. Krugman organized a National Bureau of Economic Research conference on the matter, which was held on October 19–20, 1989, and resulted in a conference volume in 1991. In 1995, in light of Krugman's success as a popular writer on trade, a paperback version of the book was issued, with Krugman's name in massive type on the cover. Krugman supplied a new preface for the book (preceding the introduction he had written for the 1991 version). In it, he stated, "All too often, Americans project their own fears and desires onto Japan." As an example, Krugman asserted: "In *Free To Choose*, Milton Friedman held Japan up as a proof of (surprise!) the power of free markets."[187]

Krugman did not indicate whether he was referring to the Milton Friedman television series or the Friedmans' accompanying book. But either way, it was clear he had not read the Friedmans' June 1980 preface to the U.K. paperback version of *Free To Choose*, which had ended with a "final note" designed to correct a misapprehension about both the television program and the book: their case study of the effect of free markets was not "Japan today but… [instead] Japan during the first thirty years of its entry into the Western world (1867–97)."[188] It is that era, they stressed, that they had pointed toward as one in which Japan had been "predominantly a free

[185] Krugman (1993, p. 519). The same passage implied that the Friedman position that floating rates "eliminate payments balances" in a straightforward manner had been defied by experience. But Krugman did not make clear that Friedman was referring to the overall payments balance—which indeed is automatically zero under floating rates—and not to the individual current and capital account balances.
[186] Krugman (1993, p. 520).
[187] Krugman (1995b, p. xi).
[188] That is, the period following the Meiji restoration—a period also highlighted in Friedman and Friedman (1988, pp. 458–459) and *Free To Choose* (revamped U.S. version), Episode 3, "Freedom and Prosperity," CNBC, February 24, 1991, p. 4 of transcript.

enterprise country."[189]

The fact that the Friedmans did not cite postwar Japan as an example of a highly free-market economy, and that they acknowledged in 1980 that "Japan today relies very much less on free enterprise" than in the nineteenth century," did not mean that there were no Milton Friedman statements available in which he had stressed that the success Japan had enjoyed in the postwar period largely reflected the weight it did give to free-market mechanisms.[190] For example, speaking in 1980, he had remarked of Japan that observers "grossly overestimate the importance of the visible hand" of public-sector intervention and "grossly underestimate" the "exceedingly important" role that market forces played in Japan's economy.[191] The following year, he affirmed that in "the postwar period, … there was again very rapid growth [in Japan]; and I believe that is again attributable to the prevalence of a predominantly free market with relatively limited intervention by the state."[192] And on television in 1987, he observed: "The Japanese government is no more efficient in determining what's a sensible use of capital than is the American government."[193] Krugman could have cited these items, instead of pointing to *Free To Choose*.

Face to face

Krugman and Friedman were face to face in late January 1991 at a conference on international economic policy held at Claremont University, California. The conference was held some months after, as Avinash Dixit (1991) would later observe, Krugman "reached the general public with *The Age of Diminished Expectations*." In the conference proceedings (published as Hinshaw, 1993), Friedman did not give any indication of having read Krugman's new book. This was just as well, as Krugman—although generous to Friedman regarding his contributions on the Phillips curve—argued that monetarists "almost seem like relics now," and that Friedman's predictions in the 1980s about inflation had come to be "ridiculed, then ignored."[194]

At the conference, Krugman argued that the General Agreement on Tariffs and Trade (GATT) system had led to more liberal trading arrangements than each country separately would have

[189] Friedman and Friedman (1980c, p 16.)
[190] The quotation is from Friedman and Friedman (1980c, p. 16).
[191] Friedman (1981b, p. 22).
[192] Friedman (1984e, p. 25). Friedman dated this revival particularly to the period after 1950. Likewise, John Taylor (2012, p. 189) argued that Japan's postwar economic revival "owed much to the American model of economic freedom."
[193] *Nightline*, ABC, March 17, 1987, p. 5 of transcript.
[194] Krugman (1990, p. 85).

achieved, though “I don’t know what Milton thinks.” Friedman did not specifically object to GATT, but noted that “GATT did not prevent widespread movement toward protectionism,” and reaffirmed his longstanding position (see Chapter 9 above) in favor of unilateral trade liberalization by countries.[195] Friedman would underline this stance again in May 1992, when, at a conference in Mexico City, he indicated that he would prefer that individual countries took the initiative to liberalize trade, rather than engage in negotiated reductions—such as those in train in the emerging U.S.-Mexico-Canada trade pact, the North American Free Trade Agreement (NAFTA).[196]

With regard to macroeconomic policy, Paul Krugman expressed a desire at the January 1991 conference for the U.S. government’s fiscal deficit to be reduced but evidently indicated he saw this as best achieved in the context of increases in both taxes and domestic federal spending—a posture that led Friedman to remark that “I sharply disagree with Paul Krugman’s view that higher spending is a good thing” and to restate his starve-the-beast basis for favoring tax cuts.[197]

There was, however, one point on which there was a unity ticket linking Friedman and Krugman. This was the issue of globalization. Friedman, echoing a sentiment expressed in *Monetary Trends* in 1982 and in remarks he had made in the interim, argued that the modern degree of international economic integration was not unprecedented.[198] International economic links had been as strong, or stronger, in the nineteenth century, he contended. Krugman buttressed Friedman’s point almost immediately.[199] Their harmony on this point was reinforced by other remarks the two made separately during the early 1990s in which each downplayed the importance of globalization for the real performance of the U.S. economy. But, just as they were at one during the early 1990s in deprecating the significance of globalization, Friedman and Krugman would, later in the decade, each make a distinct about-face on the issue.

[195] Hinshaw (1993, p. 73).
[196] Friedman was an opening-day speaker at a conference held in the city and titled “Liberty in the Americas: Free Trade and Beyond.” At a press conference held on the same day, he remarked: “In my opinion, worldwide reduction of tariff barriers is much to be preferred than regional trade agreements. Unfortunately, at the moment, we are stuck with these second-best solutions.” He added: “The problem is that even those second-best solutions have turned into third-best solutions, in the sense that they are labeled free-trade agreements, but—in point of fact—they are not free-trade agreements. I think they would be more accurately described as managed-trade agreements. I saw in this morning's little paper distributed by the hotel the statement that automobiles were going to be left out of the [North American] free-trade agreement. Now, what kind of free trade do you have if automobiles aren’t included?” (United Press International, May 19, 1992.)
[197] Hinshaw (1993, p. 35).
[198] For Friedman’s earlier articulations of this point, see Friedman and Schwartz (1982a, p. 292) and Friedman (1988c; 1988f, pp. 198–200; 1989b).
[199] See Hinshaw (1993, pp. 17–18).

Milton Friedman and Economic Debate in the United States, 1973–2006
Chapter 18: Still a Monetarist, 1993 to 2006

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I. EVENTS AND ACTIVITIES, 1993–2006

The coverage in this chapter spans a longer stretch of years than any of the prior chapters in this book on Friedman's post-1973 activities or any of the chapters in the preceding volumes on 1932–1972. That the period from 1993 to 2006—over which Friedman went from age eighty to age ninety-four—can be covered in one chapter partly reflects the fact that, although he remained a public figure and of sound mind, Friedman was much less active over these final fourteen years of his life than had been the case previously. But it also flows from the reality that the statements emanating from Friedman in 1993–2006, along with his activities over this period, even though they were appreciable in number, simply do not merit the coverage that those of earlier periods justify. The rule that Friedman laid out in 1949 for examining Alfred Marshall's contributions—that "I am inclined to give little weight" to comments Marshall gave "some twenty years after the fundamental analysis… had been completed"—is an appropriate one to apply to Friedman's own body of statements.[2]

That Friedman's fundamental analysis had long been completed was obvious to anyone working in the field of monetary economics in the 1990s. That did not mean that Friedman's current views did not generate widespread interest. Indeed, in March 1998, at a research conference organized by the Federal Reserve Bank of San Francisco and held at Stanford University, the whole auditorium went into silence as Friedman made a contribution from the floor during an afternoon session on the first day of the event. The fact that a crowd of hundreds had been hushed in this manner was a reflection of the poignancy and significance of the occasion: here was someone whose participation in debates in the economic-research journals stretched back

[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, in the period since the research for this chapter was begun, nine of the individuals whose interviews with the author are quoted below—Gary Becker, Robert Chitester, Paul Evans, Martin Feldstein, Dale Jorgenson, Robert Lucas, David Meiselman, Edward C. Prescott, and Julio Rotemberg—have passed away.
[2] See Friedman (1949, p. 494) for the quotation.

more than sixty years, whose first major article on monetary policy rules dated to fifty years earlier, and whose celebrated paper "The Role of Monetary Policy" had been published exactly thirty years previously, who was participating in a new conference on monetary policy rules—making an intervention during the conference session's discussion of the international experience in the 1990s with inflation targeting and of considerations related to the possibility of the United States adopting that regime.

When Friedman showed up at events like this while in his eighties and nineties, few conference participants could not have been struck by the startling improbability of such a historical giant, who was so closely associated with debates on monetary policy now decades in the past, being present and expounding his views on recent and prospective developments in the monetary area. Widespread near-disbelief that Friedman was still around was a concomitant of these latter-day appearances. Such occasions inevitably stirred comparisons with being in the presence of a living dinosaur.

But if it was frequently the case that the encounters that interviewers and conference attendees had with Friedman from 1993 onward had echoes of *Jurassic Park*, his own reactions to unfolding events fitted in better with *Groundhog Day*. Typical behavior on his part was to draw a parallel between a recent event under discussion to one in the past that he had witnessed, and to resort to quotation or rephrasing of things that he had said in earlier decades. In other instances, Friedman did not refer to what he had said on prior occasions, but he nevertheless essentially gave restatements of those views. Consequently, when observers ascribed novelty to positions that Friedman expressed from 1993 onward, their attributions often reflected their lack of great familiarity with positions that Friedman expressed before 1993. For example, an interview that Friedman gave on the then-prestigious PBS discussion program *The Charlie Rose Show* very late in life (December 26, 2005) essentially saw Friedman repeat the benign view (taken as being a novel one in Rose's follow-up questions) of the current account deficit, and the interpretation of the U.S. economy as a safe haven, that he had put on record at length in 1984 and multiple times thereafter. When Friedman appeared on CNBC in March 2003, in a guest shot discussed further below, and stated that insider trading should be legal and that it helped facilitate the operation of market pricing, he was merely reiterating the views that he had relayed when asked about the matter in interviews fifteen years earlier—and that Henry Manne had advanced in his own work in the 1960s on the subject of insider trading.[3] Friedman's casting of, and associated praise for,

[3] See Chapter 16 above.

President Ronald Reagan as the alleged prime mover behind the 1980s disinflation—something that he expressed in his 2000 interview with John Taylor (Taylor, 2001, p. 107) and again later in other forums—put him at odds with much of the economics profession due to its lack of generosity to Paul Volcker, but it was an interpretation that Friedman had voiced ever since mid-1986 when he finally accepted that high inflation was not coming back.[4]

Repetition of past statements also characterized Friedman's writings to a notable extent. When Ebenstein (2007, p. 234) refers to an article on healthcare that Friedman published in 2001 in *The Public Interest* as "a twenty-eight-page contribution," he overlooks the fact that the article, although it contained some new material, was a restatement and elaboration, with considerable verbatim reproduction of the text used in the original versions, of arguments that Friedman had advanced in the *Wall Street Journal* in 1991 and had issued in an expanded piece in 1992.[5] And when, in the mid-1990s, the *New York Times* published a condensation of what the cover of a fresh reprint of *The Road to Serfdom* was describing as a "new introduction" that Friedman had provided for the book, the newspaper granted that the introduction was actually a revision of an introduction that Friedman had written for a 1971 printing in the Federal Republic of Germany of Hayek's book.[6]

Once one took away from Friedman's post-1992 contributions to the public record the items that amounted largely to reiteration and repackaging of what he had said earlier, there was not a great deal left. This was especially so in the case of Friedman's advocacy of market economics. As before, his statements in this area were overwhelmingly relayed via the public square, not in formal research. Friedman's contributions to the literature in the microeconomics or price-theory field had essentially ended with the new edition of *Price Theory* in 1976. His semi-popular work in the 1990s and 2000s on healthcare was the principal post-1976 output of Friedman's that could be categorized as a contribution to research in microeconomics.

Op-eds and interviews continued to be the main vehicles through which Friedman discussed nonmonetary issues, and what he said was highly consistent with what he had entered previously into the public record. The period 1993–2006 saw many more interviews in which Friedman

[4] See Chapters 15 and 16 above.
[5] See the discussion titled "Healthcare" below.
[6] See *New York Times*, August 13, 1994, and Friedman (1971d, 1994). Friedman himself remarked shortly after the 1994 version appeared: "I wrote an introduction to a German edition 25 years ago. It was the twenty-fifth anniversary. My introduction here is, primarily, the same one. It's just as applicable now as it was then." (CSPAN, November 20, 1994, p. 4 of transcript.)

recited the benefits of free markets and emphasized the desirability of reducing government spending. His statements also continued to be peppered with acerbic remarks about the U.S. public school system, such as when he described California's public school system as an "utter and complete failure" (*Cal-Tax News*, October 15, 1993).

It really took strikingly new events to shake Friedman into what could be considered more novel reactions on his part. In the nonmonetary field in the 1990s, one such item was globalization, which Friedman (as discussed under "Into the New Economy" later in this chapter) conceded was having a larger effect on the U.S. economic behavior than he had previously reckoned—an enhanced effect that he believed would trigger political repercussions as it produced a changed distribution of income in the country.

In the monetary field, too, it took decisive developments—such as the Great Moderation and the behavior of M2 velocity in the 1990s—to force Friedman from 1993 onward out of his reflex and pat answers. Although he did not repudiate his preexisting monetarist views—hence the title of this chapter—he did make some modifications to earlier positions, particularly with respect to the feasibility in practice of an activist monetary policy that was economically stabilizing. When confronted with the new developments on the monetary scene, Friedman did offer something a bit fresher, giving an interpretation of recent events rather than a rehash of his prior statements. He nevertheless often repeated his earlier views on monetary policy. This posture showed his consistency—but also was testament to the reality that Friedman's proclamations on monetary policy from 1993 onward really added little to his prior body of statements.

The explanation for the fact that Friedman had little new to say after 1992 is mundane. One word summarizes the change: retirement. His move to California in early 1977 had been described at the time, including by Friedman himself, as retirement, and the whole post-1976 period was designated "Life After Retirement" in the chapter title of the Friedmans' 1998 memoirs.[7] But Friedman's activity in public policy—and, more intermittently, in research—during the 1977–1992 period meant that Friedman's exit from teaching and dissertation supervision was not followed by a *bona fide* retirement. Something approaching that genuine retirement did, however, set in from the early 1990s. Speaking in early 1996 to Brian Snowdon and Howard Vane, Friedman candidly acknowledged that he had not really been doing any work on monetary issues for the prior three or four years, and that his engagement in monetary matters

[7] Friedman and Friedman (1998, p. 559).

even prior to then had been a step down from his engagement up to 1982. He added that he had not followed research developments closely.[8]

Friedman continued to have, right through to the end of his life, what was nominally a full-time job as a senior research fellow at the Hoover Institution. But the limited responsibilities associated with the position gave him considerable leeway in splitting his time between leisure and work. The 1990s saw Friedman make much use of that leeway. In that decade, he would have some days consisting of a tight schedule of meetings at the Hoover Institution or of speaking engagements elsewhere. He would also have days when, working at his Hoover Institution office or, more typically, from home, he assigned himself a full agenda, largely consisting of reading, writing, or dictation into a tape-recorder. On many other days of what used to be the regular work year for him, however, he was not working. He was also known to come into the office, only to change his mind about devoting the day to work—as occurred in 1993 when, on being told that an Emma Thompson film (*Much Ado About Nothing*) was screening at a local theater, Friedman simply departed from the Hoover Institution building to watch an afternoon screening of the movie by himself.[9]

Friedman still produced op-eds on monetary policy from time to time. When, in 2002, it was pointed out that both he and Anna Schwartz were still writing about monetary policy twenty years after the appearance of *Monetary Trends* (which, at the time, had been advanced as their final word on monetary matters), Friedman remarked: "I can't speak for Anna, but I at least would have given you long odds in 1982 that I would not be around in 2002, let alone still able to write something somebody is willing to publish."[10]

But if people looked to Friedman for an informed view of the present research literature on monetary economics, they were gravitating toward the wrong person. And although, if they wanted a discussion of recent monetary policy developments, they would certainly find that he had something to say, the resulting commentary was better characterized as that of a casual watcher of the scene rather than that of someone closely monitoring monetary policy.

[8] Snowdon and Vane (1997, pp. 197, 202). This characterization lines up with the account of the previous chapters. The main qualification that should be entered is that Friedman remained active on monetary issues in the first couple of years after the publication of *Monetary Trends*, via his running commentary on monetary policy developments from 1982 to 1984. See Chapter 14 above.

[9] Gloria Valentine, personal communication, August 14, 2007. This behavior was not completely out of line with Friedman's conduct during his University of Chicago years. Rose Friedman had told a newspaper interviewer Milton Friedman's traits included "sneaking off to the circus" (*Daily Mail* (London), March 18, 1980).

[10] Email from Milton Friedman to the present author, February 11, 2002.

Friedman had been a Fed watcher in the 1950s, 1960s, and 1970s, before that term became a widely-used job description.[11] During the 1990s, however, he abdicated from that role. About forty years earlier, Friedman's University of Chicago teacher on monetary matters, Lloyd Mints, had looked forward to retirement as the time when he would not have to read the *Federal Reserve Bulletin* anymore. David Meiselman recalled of Mints: "He kept saying that as soon as he had the chance to retire, he was going to quit and go back to Colorado and burn every Goddamn copy of the *Federal Reserve Bulletin* ever made. Which is basically what he did." (David Meiselman, interview, April 30, 2013.) Friedman had not initially followed that template when he himself left the University of Chicago, but he eventually did so to a considerable extent over the 1990s and 2000s. And even when producing a research paper on monetary policy in the early months of 2006, he referred to "my relative retirement" from engagement in monetary matters.[12]

Both before and after 1993, Friedman granted masses of interviews. And, as already indicated, his post-1992 activities included writing, in a sporadic manner, what became a large volume of op-eds. But before 1993, and especially until the 1980s, the media contributions were merely the tip of Friedman's activities: there was a mass of research work going on in parallel with them. Friedman did engage in a major project in the 1990s in the form of his memoirs, coauthored with Rose Friedman and discussed further below. Even the book that resulted, however, bore witness to Milton Friedman's drift away from research interests, as the memoirs' coverage was heavily weighted toward the Friedmans' family, friends, and tourism, rather than toward his debates with policymakers and other economists or toward his research.

Educational vouchers

One area in which the Friedmans remained active in the 1990s and the 2000s was that of vouchers for school education in the United States. Greenspan's (2007, p. 405) characterization that, in these years, the Friedmans were primarily "devoting the end of their distinguished careers to advancing the [voucher] policy" accords with the judgment of the present author.

The most famous item among Friedman's 1950s writings on free markets was that on school vouchers: 1955's "The Role of Government in Education."[13] A factor that made elementary and

[11] See, in particular, the discussion in Chapter 12 above.
[12] Email from Milton Friedman to the present author, March 30, 2006.
[13] Friedman (1955a).

high school education a prominent one to be faced by a free-market U.S. economist in 1955 continued to be present when the Friedmans wrote a chapter on schools in the *Free To Choose* book a quarter century later: the United States had very longstanding public-school arrangements that dominated the so-called "*K*-through-12" educational scene. Whereas, in the areas of telecommunications, healthcare, and public utilities, the U.S. economic system was often noted as possessing more private-sector features than did many other market economies—with the contrast particularly marked before the 1980s—when it came to primary and secondary school education, the U.S. public sector (principally, state and local governments) had a much larger role than was the case in various other non-Communist countries, and the private sector (particularly profit-making institutions) a much smaller role. The Friedmans summed the matter in *Free To Choose* as one in which "the school system in the United States [is] an island of socialism in a free-market sea."[14] Likewise, in one of his last-ever *Newsweek* columns (December 5, 1983), Friedman referred to "our present socialist school system."[15]

The public sector's dominance of the U.S. school system was an institutional fact that formed the background for Friedman's discussions of vouchers. In that connection, one of the points that Friedman made in his discussions of education was terminological: he disliked the term "public education." Friedman had set the tone by criticizing the "public" terminology even in his 1955 essay. Concerning the "education" part of this phrase, he would also remark that "it is important to distinguish between 'schooling' and 'education,'" as the two concepts only partially overlapped.[16] Paul Evans—a graduate student in economics at the University of Chicago from 1969 to 1976—recalled:

> Friedman did not approve of the phrase 'schools engage in education.' So he would say 'schools engage in schooling.' [Re] schooling: you can have someone with a Ph.D. in economics, or physics, or whatever, who knows nothing about the world and can be ignorant in many dimensions, but he would be very schooled. On the other hand, you might have someone who, because of poverty, … had to leave school after grade six but was very literate, knew a wide literature, knew many things about the world and so forth, and that person would be educated. Education derives, he would say, from the Latin word meaning 'lead through or lead to,' so it was sort of [what amounted to] leading a person to a body of knowledge out there, and it didn't have to take place in a school… [He] was very picky about proper use of words. (Paul Evans, interview, February 26, 2013.)

[14] Friedman and Friedman (1980, p. 154).

[15] Later, in *Reading, Writing & Rukeyser* (Maryland Public Television, May 12, 1995), Friedman stated along similar lines that "schooling is the biggest socialist industry in the United States." In Friedman (1994b, p. 94), he gave 90 percent as the share of "elementary and secondary students going to government schools." Conversely, in *Washington Post*, February 19, 1995 (reprinted in Bonsteel and Bonilla, 1997, p. 199), he gave 10 percent as the share attending private schools.

[16] The quotation is from Friedman (1962a, p. 86).

As his reference in his 1955 vouchers essay to “the so-called ‘public schools’” made clear, Friedman also disliked using the term “public” for “government.”[17] Stressing that private-sector-owned universities like Harvard University, the University of Chicago, and Stanford University, as well as the United States’—proportionately very small, as already implied—private-sector-run elementary and high schools, served the public, he rejected the restriction of the term “public school” to cover state-owned institutions.[18] Friedman did not press this terminological point in all his writings, however. Friedman used “public” for “government” in much of his research output. For example, the Friedman-Schwartz *Monetary History* used the term “public expenditures” in reference to U.S. government spending.[19]

Friedman also, on occasion, used the term “public education” when expounding his voucher proposal. He did so, for example, in the late 1960s, when he declared: “We have a mess in public education because parents have no effective way of registering their protest about the quality of the schools, except through a political process that is very time-consuming.” (*Chicago Daily News*, March 26, 1968, p. 39.)[20]

The lack of a non-political means of effectively conveying disquiet with schooling, Friedman repeatedly stressed, arose from the dominance of the public sector in the U.S. system. Not only were parents obliged to demand the product, but there was also an absence of a significant alternative supplier of the product. Consequently, “the public schools… have a captive market,” he suggested.[21] “You cannot make a monopolistic supplier of a service pay much attention to its customers’ wants—especially when it does not even get its funds directly from its customers,” Friedman later contended (*Newsweek*, December 5, 1983).[22]

The final clause in this 1983 remark was crucial. In his microeconomic and macroeconomic discussions alike, Friedman had stressed that monopolies *were* quite sensitive to customer demand, on several grounds: demand was price-elastic; buyers could devote spending power

[17] See Friedman (1955a, p. 128) as well as Friedman’s remarks in Ketchum and Strunk (1965, p. 47). Friedman repeated his criticism of the term “public” for “government” on many later occasions: see, for example, *The Listener* (London), May 30, 1974 (p. 690), Friedman and Friedman (1980, p. 319), *Newsweek*, January 17, 1983, and *Firing Line*, PBS, April 10, 1986 (pp. 8–9 and 11 of transcript).

[18] See, for example, *Newsweek*, March 13, 1967, Friedman and Friedman (1980, p. 319), and *Donahue*, NBC, April 25, 1984.

[19] Friedman and Schwartz (1963a, p. 688).

[20] See also his column in *Newsweek*, November 18, 1968.

[21] *Milton Friedman Speaks*, Episode 11, “Putting Learning Back in the Classroom,” taped September 15, 1977, p. 14 of transcript.

[22] See also Friedman’s remarks in *The Detroit News*, November 17, 1976.

elsewhere; and monopolies' positions would ultimately also be vulnerable, as competitors would emerge, enticed by the monopolist's high selling prices. In the case of the U.S. education near-monopoly, however, these elements were absent—because, from the point of view of the parents, school services were free.

With regard to the comparative dearth of U.S. private schools, Friedman accordingly highlighted the price issue: "Of course there aren't [many] now, because there's no market for them. If somebody down the street is giving a product away, it's a little hard for you to go in[to] business if you have to sell it." (*Chicago Daily News*, July 27, 1970, pp. 3–4).[23]

The voucher proposal that Friedman propounded was designed to generate more market-like conditions. It would give parents a claim on funds that could be spent for their children's (elementary and high school) education at an institution of their choice, with private schools permitted as an option. The government would continue to operate public schools, but taxpayer funds channeled toward education would now take the form of spending on these schools as well as tuition payments made to private schools—whose quantitative importance, Friedman suggested, would increase considerably: "if you established the voucher system, it would establish a market for schooling at that [*K*-through-12] level, and [the number of] new schools would grow to meet that demand." (*Chicago Daily News*, July 27, 1970, p. 4.)

Friedman referred to his proposal as "denationalizing schooling," or as he later put it, using the terminology that had become common, "privatization."[24] These descriptions notwithstanding, state spending played a crucial role in the policy recommendations in Friedman's voucher proposal as laid out in his 1955 article on education and in his advocacy of vouchers over the subsequent half-century. Adding private schools as an area of use of taxpayer funds meant that Friedman's voucher proposal involved making a government-spending commitment.[25] True, the

[23] Likewise, Friedman and Friedman (1980, p. 163) observed: "Try selling a product that someone else is giving away!"

[24] See, respectively, Friedman (1962, p. 91), and *Reading, Writing & Rukeyser*, Maryland Public Television, May 12, 1995.

[25] It is evident that, because of the predominance of public schools in the U.S. school system, Friedman did not regard a scheme under which taxpayer funds would go to both public and private schools as amounting to a major additional spending commitment. However, Friedman also advocated the voucher system for systems, like that of the United Kingdom, in which private schools constituted a larger share of elementary and secondary schools than in the United States (see, for example, *Daily Express* (London), November 30, 1976), and, therefore, he seemed willing to accept an increase in the public funds allocated to education as a consequence of his proposal. (In the United Kingdom, private schools constituted a larger share of elementary and secondary schools than in the United States—with Jenkins [2000, p. 1650] contrasting the U.K. situation with "the tiny percentage—a little over 1 percent—of

voucher funds allocated to private schools might be classified as transfer spending rather than government purchases. But Friedman's discussions of government spending, once he became a monetarist, indicated that he thought that transfers and purchases were over-distinguished—that transfers were not really negative taxes but instead were akin to purchases (see Nelson, 2009, p. 494, and Chapter 6 above). Friedman granted that the proposal entailed government expenditure but stressed that, as the U.S. system was largely a public-sector one anyway, it reallocated existing expenditure: "In fact, it doesn't give an additional subsidy. It converts a subsidy [that a parent] is now offered… into a subsidy in money," with the stipulation that the money in question had to spent on school education (*Chicago Daily News,* July 27, 1970, p. 4).

The fact that Friedman's voucher proposal did not seek to curtail government spending was one reason it has not always found favor with free-market advocates or with libertarians.[26] In the case of the latter group, however, an additional reason for taking exception lay, of course, in the fact that under Friedman's proposal, as he noted, "you would still have compulsory schooling" (*Dinah*!, March 30, 1977).

The voucher proposal was, therefore, not a no-government proposal, and as Sam Peltzman observed, "a lot of people criticized Friedman for this type of thing. [They would say,] 'How can you talk about school vouchers? You should talk about getting the government out of the business entirely,' and that kind of thing. He [instead] had an appreciation of 'you don't let the perfect defeat the good.'" (Sam Peltzman, interview, March 1, 2013.) In addition, Friedman's proposal differed from that advanced by some libertarians because he was invoking an economic argument: namely that, left to itself, the private sector would likely underspend on education because its expenditure choices would neglect the positive externality received by society as a result of having its population educated up to high-school level.[27] Although so often characterized as neglecting externalities in his work, Friedman was, in fact, partly appealing to the existence of externalities when he laid out his proposals for the education system—albeit in his 1950s and 1960s writings using "neighborhood effects," which during his career became

American children who are educated at the equivalent of our fully independent schools—I am not talking about parochial schools because they are not at all equivalent.")

[26] For example, Friedman noted (in *Reason* magazine, June 1995, p. 36) that libertarian Murray Rothbard "used to berate me for my stand on education vouchers… Murray used to call me a statist because I was willing to have government money involved."

[27] See Friedman (1955a, p. 127; 1962a, pp. 85–86, 88). Friedman had previously noted the benefits flowing to society from community-wide education in NBC (1952, p. 9).

obsolete terminology, to refer to externalities.[28]

Over time, when discussing the ideal policy, Friedman made considerable concessions to the libertarian line—doing so particularly from the 1970s onward. In an article published in 1976 and a television interview given in the same year, he indicated that research by E.G. West (1965) had persuaded him that 90 percent or more children were likely to go to school in countries like the United Kingdom and the United States, even if school attendance was not compulsory.[29] He had consequently become convinced that the externalities that he had cited as providing the basis for compulsory education were less important than he had previously perceived. In light of this changed assessment, Friedman suggested that his ideal-world preference would be to give parents the option of not sending their children to school, and, in the case of most parents, providing no specific public-sector funding of their children's school education, even via a universal-voucher system. In such a situation, he saw government payments in connection with schools as being limited to the provision of funds to lower-income groups to finance their children's education—possibly as part of the negative-income-tax payment.[30]

This change in outlook, however, comprised primarily a concession at the conceptual level on Friedman's part, rather than one that affected his practical policy prescriptions. In the *Free To Choose* book, the Friedmans indicated that they no longer supported "compulsory attendance laws" and public-sector provision of funding for universal education. But they immediately granted that this position "will appear to most readers to be extreme" and would not proceed on the premise that it was accurate.[31] Instead, as before, they advocated universal school education alongside universally-issued school vouchers. This remained Friedman's position for the rest of his life: he had "become more radical" and "more extreme" on education, over time, he suggested, on account of his endorsement in principle of the notion that "the government [should get] out of the education business entirely" (*Reason* magazine, June 1995, p. 36). But, as he

[28] In discussing Friedman's voucher proposal, Ebenstein (2007, p. 224) seems to be under the misapprehension that "neighborhood effects" remains current terminology in economics. Friedman (1976b, p. 10; 1977e, p. 10) acknowledged that "external economies and diseconomies" (or externalities) had become the standard terminology, and Friedman himself had used "external economies" in his discussion in Friedman (1961e, pp. 537, 538). He also used "externalities" at length in some discussions—for example, *The Jay Interview,* ITN, July 17, 1976.

[29] See Friedman (1976h) and *The Jay Interview*, ITN, July 17, 1976. See also Friedman's discussion in *Reason* magazine (June 1995, p. 36) of the "change in my knowledge of the factual situation and history" that E.G. West 's work had wrought. See also the citation of West's work in Friedman and Friedman (1980, pp. 153−154, 171).

[30] *The Jay Interview,* ITN, July 17, 1976. See also Friedman (1962a, p. 87) for discussion indicating that under a scenario—one to which he did not subscribe at the time—in which there were no major externalities arising from an arrangement of compulsory schooling, it remained desirable for "very needy families" to have government-provided funds to finance their children's education.

[31] Friedman and Friedman (1980, p. 163).

indicated in a very late interview (R. Kuttner, 2006) when he distinguished "the utopia" that he associated with such a libertarian model from his own policy prescription: "In reality, I want universal vouchers." In later decades, therefore, Friedman favored, as he had in the 1950s and 1960s, the universal issuance of vouchers, with these vouchers non-redeemable except for payment of school expenses. They would be capable of being spent "for one purpose and one purpose only," he suggested in a 1977 speech: paying for elementary and secondary education at public or private schools of the parents' choice.[32]

The issue of whether vouchers should be income-tested was, of course, logically separate from the matter of compulsory versus voluntary schooling. Gary Becker noted of voucher advocates: "Some believe they should be for the poor only. Which Friedman objected to strongly. And I argued with Friedman on that one." (Gary Becker, interview, December 13, 2013.) In R. Kuttner (2006), Friedman stated: "Everybody pays taxes, [so] everybody is entitled to vouchers. I believe that if you have vouchers only for low-income people, it would be a very bad program for several reasons. [One is that] a program for the poor would be a poor program. They say that about Social Security… [But] the main reason I believe the poor would benefit much more from universal vouchers than from vouchers for the poor only, is because universal vouchers would open up the educational market to innovation and for experiments in new ventures."[33]

The process that would be set off by vouchers, he suggested in his 1977 talk, "would alter beyond belief the character, quality, and climate of education… It would do exactly the same good that competition does in every other area."[34] Vouchers, he later elaborated, provided a means to "break the monopoly, introduce competition and give the customers alternatives" (*Newsweek*, December 5, 1983).

With regard to the specific effects of vouchers on the U.S. school system, Friedman suggested that the rigidity of the present educational structure discouraged many able potential schoolteachers from choosing this line of work.[35] He believed that a voucher system would provide individual teachers with greater scope to influence their schools, while also linking their salaries more closely to their individual performance. Friedman also strongly believed that

[32] *Milton Friedman Speaks*, Episode 11, "Putting Learning Back in the Classroom," taped September 15, 1977, p. 12 of transcript.

[33] See also Friedman (1997d). Friedman similarly argued in *Investor's Business Daily* (April 15, 2004, p. A8) that innovations in the form of "new schooling methods" were better promoted by a universal-voucher program.

[34] *Milton Friedman Speaks*, Episode 11, "Putting Learning Back in the Classroom," taped September 15, 1977, pp. 11, 14 of transcript.

[35] See Friedman (1962a, p. 96) and Friedman and Friedman (1980, p. 152).

parents' influence on schools would be enhanced under vouchers, and he contrasted this with the existing system, in which the Friedmans considered power has instead gravitated to professional educators" and, in particular, to "educational administrators."[36]

Friedman also believed that public schools would become better-performing under a voucher system. "They would be under enormous pressure to improve their quality [in order] to keep the children they serve. If the public schools did not improve, they would decline [in enrolment], and the private alternatives would grow." (*Chicago Daily News*, July 27, 1970, p. 4.) On this reasoning, he suggested that in response to the introduction of universal vouchers, "the public schools would improve a great deal, too—they would benefit. Competition is the only thing that makes people jump and skip and do something different." (*Reading, Writing & Rukeyser*, Maryland Public Television, May 12, 1995.)

Private schools would be set to increase in number under Friedman's proposal. But the ramifications of vouchers for the framework in which private schools—both existing and newly constituted ones—would operate provided another reason why free-market advocates had mixed reactions to Friedman's proposal. In *Capitalism and Freedom,* which contained a chapter that was a rewrite of his 1955 essay on vouchers, Friedman suggested that governmental monitoring of all voucher-financed schools, public and private, would include "insuring that the school met certain minimum standards" including a minimum common curriculum.[37] Indeed, in 1996, Friedman claimed that there was one legitimate argument against school vouchers: that their existence would increase government supervision, and degree of control, of private schools. He acknowledged that, with taxpayer funds channeled into private schools on a large scale, vouchers would increase the presumption that private schools should be subject to a government regulatory framework.[38]

Even in the presence of criticisms of his position coming from the libertarian side, however, it remained overwhelmingly the case that, over the decades, the main opposition to Friedman's voucher proposal came from advocates of public education. He acknowledged that high-quality public schools existed, and he pointed to the ways in which a voucher system would bolster those schools. And, as noted, he suggested that vouchers would also facilitate improvements in

[36] The quotations are respectively from Friedman and Friedman (1980, p. 152) and *Milton Friedman Speaks,* Episode 11, "Putting Learning Back in the Classroom," taped September 15, 1977, p. 2 of transcript.
[37] Friedman (1962a, p. 89).
[38] See his remarks of November 21, 1996, in Indianapolis, broadcast on CSPAN on December 1, 1996.

other public schools.[39] Nevertheless, Friedman also made statements on the record that very likely reinforced the perception that vouchers were a threat to the public education system. Not only did he make sweeping and harsh comments on public schools, but he also contended, "If you allowed it [the voucher system] to work, State schools would disappear." (*Daily Express*, November 30, 1976.)[40] He was later quoted saying of a voucher system, "It will probably destroy the public schools" (*San Francisco Chronicle*, September 29, 1979), and he would remark to Louis Rukeyser, "I wouldn't mind seeing them eliminated ultimately, but it's not feasible in the short run." (*Reading, Writing & Rukeyser,* Maryland Public Television, May 12, 1995.)
In an earlier interview with Rukeyser, Friedman had highlighted how he had come to favor a policy that "make greater use of referenda, appeals to the public at large. I'm a populist in that sense."[41]

By the time of this Friedman statement, statewide referenda had become a key means by which supporters of a voucher system attempted to have their proposal become official policy. It was on the subject of one such referendum, scheduled to take place in his home state of California, that Friedman made a guest commentary on a nationally broadcast business program in the summer of 1993. "Ask anyone anywhere in the world which country has the best institutions of higher education," Friedman's contribution began, "[and] you will get the same answer everywhere: the United States. [But] ask the same question about elementary and secondary education, and you will get that answer nowhere. How come the difference? After all, the students who attend our institutions of higher education come from our elementary and secondary schools. And the teachers in our elementary and secondary schools come from the institutions of higher education. The paradox is answered in one word. That word is 'choice.' The youngster who goes to college has a choice among many institutions, in his own state and in other states. The youngster who goes to elementary and secondary schools mostly has no choice: he attends a school chosen for him, he is taught a curriculum determined by others. He or she and his or her parents has very little to say. Choice produces competition, and competition produces quality. In order to improve our educational system, we need choice." (*Nightly Business Report,* August 2, 1993, p. 9 of transcript.) Friedman then turned to the school-choice proposal, a compromise voucher plan, that would be voted on in his state shortly: "California is going to have an initiative on its ballot this fall that would give parents choice. The state will

[39] See again, for example, his November 21, 1996, remarks, broadcast on CSPAN on December 1, 1996.
[40] Friedman was speaking in a U.K. interview in which "State school" was used for government-operated school. ("Public school" was and is a term used in the United Kingdom for certain private schools, while, in the U.K. context, due to its nonfederal structure, "the State" is a natural synonym for the central government.)
[41] *There Goes Our Money*: *A Louis Rukeyser Special,* Maryland Public Television, April 8, 1994, p. 23 of transcript.

give them a voucher equal to half of what it costs the state… to school them now. And the parents can choose whatever school they want for their children. That is the way to improve our educational system. Nothing else will do it. This is Milton Friedman," he signed off (*Nightly Business Review*, August 2, 1993, p. 10 of transcript).

That ballot was heavily defeated. This was a recurrent fate of voucher proposals in the United States. Even in 1977, Friedman had observed: "I have had a great deal of personal experience on this subject, and I have been increasingly frustrated."[42] "Several attempts have been made to get states to introduce a voucher system, on either an experimental or a statewide basis," Friedman remarked nine years later (*San Francisco Chronicle*, September 7, 1986)—naming New Hampshire, Michigan, and California as instances in which he had personally been part of the efforts. He conceded: "Each attempt has been defeated." Parents or individual teachers were not hostile in the main, Friedman suggested, but defeat had been brought about "primarily by the opposition of educational administrators and trade union officials[,] with the cooperation of self-styled 'liberals' defending the monopoly of government schools."

A further decade on, in September 1996, the Friedmans took a leading role in creating a specific institution intended to mobilize the public-policy activities of themselves and others on the voucher. They put a substantial amount of their own money into the Milton and Rose D. Friedman Foundation in Indianapolis, Indiana. The foundation's premises opened in late September 1996, when a local report described the new venture as having a "sole mission of promoting educational choice throughout the country among public, private, and parochial schools" (*The Indianapolis Star* (Indiana), September 29, 1996). Milton and Rose Friedman appeared in Indianapolis on November 21, 1996, to participate in a press conference and be panel members at a gala event on the foundation's formation.

The unsuccessful efforts that the University of Chicago's economics department in the late 1970s made to institute a Milton Friedman Fund to finance graduate students suggested that naming a fund or foundation after Friedman was not a particularly auspicious step. Unlike the Milton Friedman fund, the Indianapolis-based foundation did stay in existence, including well beyond Friedman's lifetime. But it changed its name from the Friedman Foundation to EdChoice in 2016.[43]

[42] *Milton Friedman Speaks*, Episode 11, "Putting Learning Back in the Classroom," taped September 15, 1977, p. 16 of transcript.

[43] See the organization's website at *https://www.edchoice.org/who-we-are/our-legacy/*.

In the final decade of Friedman's life, among the books specifically promoting the voucher proposal appeared were two that highlighted him on their cover. One of these—*A Choice for Our Children: Curing the Crisis in America's Schools*—appeared in 1997. This publication was marketed as a Friedman-coauthored book, and Friedman was listed with three others on the "Other books by the authors" interior page, with the cover of the book suggesting that the pages inside would contain an exchange of views between himself and another author. The book (Bonsteel and Bonilla, 1997), issued by San Francisco's pro-free-market Institute for Contemporary Studies, was not really one for which Friedman was a full-fledged coauthor. But it did indeed have a short (three-page) new chapter contributed by Friedman, reaffirming his view that vouchers should be universal.[44] The book also reprinted a two-year-old, but most significant, op-ed on vouchers that Friedman had written for the *Washington Post* (February 19, 1995) and that is considered below in the discussion titled "Into the New Economy."

The other book, *Liberty and Learning: Milton Friedman's Voucher Idea At Fifty*, had a much-used Friedman portrait on its cover. This book had closer ties to the Friedman Foundation, as it coedited by Robert C. Enlow, a foundation executive.[45] Despite its title, the book, published by the Cato Institute, was issued in 2006, and so its appearance lagged the actual fiftieth anniversary of Friedman's initial essay on vouchers. Friedman had nonetheless had occasion to mark the fiftieth anniversary publicly— making a late-in-life appearance on the East Coast to deliver a talk in New York City in June 2005 on the voucher movement (*Wall Street Journal*, October 24, 2005, p. A11), and publishing a preliminary version of what became his preface to *Liberty and Learning* as an op-ed in the *Wall Street Journal* (June 9, 2005). This piece candidly acknowledged that, in his own lifetime, Friedman had largely lost the competition in the political arena. He had earlier, in a 2004 interview, observed that "progress… [has] been disgustingly slow… like pieces of grass coming up through the concrete"—and he further suggested that the teachers'-union movement had been "highly successful in preventing the expansion of school choice "the opposition of the teachers' unions and the educational administrators" had seen the movement experience a multi-decade sequence of defeats.[46] "Opposition like this explains why progress has been so slow," he elaborated in his 2005 piece, while maintaining that vouchers were a "good cause" that opponents had maligned.[47]

Friedman not only felt that it was a good cause, but, also, that it was not a lost cause. One of his

[44] Friedman (1997d).
[45] Se Enlow and Ealy (2006).
[46] *Investor's Business Daily*, April 15, 2004, p. A8.
[47] Friedman (2006a, pp. viii [first quotation], ix [second and third quotations]).

final speeches to a live audience was in San Francisco on July 21, 2006, when—seated in light of the weakness of his legs, and with a slightly scratchier voice than that seen in early years—he gave a spirited exposition of the case for the voucher system (CSPAN, July 21, 2006).

Links to the research and monetary policy worlds

John Taylor noted of Friedman that "on the education stuff, he felt very strongly that he wanted to use economics to improve people's lives. He saw that [a voucher system] as a good way to do it, and he was an educator himself." Taylor observed of Friedman's post-1992 activities in the school-voucher area *vis a vis* other longtime Friedman interests, "He may have had more interested in that than other things, but I don't think of it as a substitute." Taylor stressed that he and Friedman "talked about monetary policy until the very end," and Taylor added, "he did [monetary] research. It would not necessarily be the kind of research you'd present in highly technical academic seminar, but he'd be doing research all the time, empirical things. He'd get his data out, and be analyze things, and be showing charts, and asking questions. …[M]ore like Friedman-Schwartz type of research." (John Taylor, interview, July 2, 2013.)

One way in which Friedman preserved some links with the research world in the 1990s and 2000s was via providing written comments on papers sent to him by correspondents. Among these were papers were mailed to him by old friends who had been colleagues or students at the University of Chicago, including Gregory Chow, David Laidler, Charles Nelson, and Arnold Zellner. But, as an inveterate correspondent, Friedman also would send comments on papers sent to him by researchers whom he had not met in his University of Chicago years and, indeed, some whom he had never met and never did meet. Although his comments would often confirm that he was no longer following the research literature closely, and that he had become progressively less inclined to read technical work, the suggestions that he would make for the improvement of papers would often testify to Friedman's longstanding expository talents and to his continuing strong intuition on matters of economic substance.

Friedman's occasional participation in research conferences was now mainly confined to those in the Bay Area. But it, too, bore witness to his continuing interest in analytical economics. The new pieces of work that Friedman produced himself, referred to in the Taylor quotation above, tended to be decidedly primitive regression or graphical analysis. Although he would, as will be detailed in later sections of this chapter, put some of it into research outlets in 2005 and 2006, the work was fairly rudimentary, and the monetary policy-related items were—judged as a contribution to the economic-research literature—generally not ready for prime time (or for any other time slot).

Nonetheless, in one of his bouts of enthusiasm for monetary research, Friedman submitted work that he had recently done on money demand for consideration for inclusion as a paper in the program of an NBER mini-conference due to be held at the Federal Reserve Bank of San Francisco on February 5, 2000.[48] Specifically, Friedman had submitted a paper on money demand in response to the Call for Papers that the NBER issued regarding the event.

Northwestern University's Martin Eichenbaum, a conference organizer, then found himself in a dilemma: he had been taken aback by a giant such as Friedman having, in effect, lined up alongside the economic-researcher rank-and-file to have a submission considered for inclusion on the program agenda, but the piece of work proffered by Friedman manifestly did not meet the criteria required to be part of the conference program—and including it in the conference, at which Friedman would have had an assigned discussant, would only make all too clear its bad fit for the event. Eichenbaum described the problem to his wife, who responded with a suggestion that became the outcome. Friedman's submission was turned down, but he was invited to be the luncheon speaker at the event, and Friedman agreed.[49] The lunch-speaker role aligned with Friedman's elder-statesman status while making allowance for the fact that the contribution that he made to the event via his talk would be less technical than those made by the presenters of the papers at the conference.

In addition to performing this speaker role, Friedman made interventions during the floor discussions of other sessions of the mini-conference. These interventions included critical remarks on a paper by Simon Gilchrist (then of Boston University, later at New York University) and John Williams (a Federal Reserve Board economist who was, at the time, seconded to the Council of Economic Advisers, and who be a policymaker on the Federal Open Market

[48] The conference, held as part of the NBER program on economic fluctuations and growth, was subsequently written up for the Spring 2000 issue of the *NBER Reporter*. One of the final meetings of the economic-fluctuations-and-growth group attended by Friedman was one held four years later, on February 6, 2004, at the Westin St. Francis Hotel in San Francisco. Friedman had, in fact, sporadically attended the Bay Area meetings of the NBER's economic-fluctuations-and-growth group since 1983. Lawrence Summers, based on the East Coast but a regular attendee of NBER events around the country during the 1980s, recalled: "I can't remember when I first met him, and I didn't correspond with him a lot. I met him a couple times when I was young at NBER macro program meetings which were held at Stanford. He [also] sent me brief notes two or three times about papers that I had written or things that he had heard that I said." (Lawrence Summers, interview, November 22, 2013.)

[49] This information came from the author's personal communications on the matter with Martin Eichenbaum (March 21, 2014, and December 19, 2016).

Committee from 2011, becoming the FOMC's vice chair in June 2018).

The Gilchrist-Williams paper concerned theories of capital formation and the production function, with an application to the postwar economic-growth record of Germany and Japan.[50] The hopes that Friedman had expressed in the 1960s about writing a book on capital theory had never been realized, but decades later he was using the mini-conference session to display his strong opinions on the subject and register vociferous dissent with regard to aspects of the Gilchrist-Williams paper's approach. After the session, Gilchrist expressed disappointment at the rough ride that he and Williams had had, in part because of Friedman's intervention. Williams replied to Gilchrist that they should look on the bright side: whatever else happened in their careers, from now on they would always be able to say that Milton Friedman had talked about, and provided comments on, their research.

Friedman had earlier, in the mid-1990s, been on the program of a couple of the Federal Reserve Bank of San Francisco's March research conferences.[51] In March 1994, he and Guillermo Calvo were the assigned discussants of Robert Lucas' paper "On the Welfare Cost of Inflation," which Lucas did not publish until 2000 but of which Lucas gave prominent conference presentations during 1993–1994.[52] In addition to building on work such as Bailey (1956), Lucas' work, like Lucas and Stokey (1983), was something of a homage to, and extension of, Friedman's work on the optimum quantity of money. Indeed, Friedman's 1969 paper on the subject seemed to be a source of permanent fascination to monetary theorists, even those who—unlike Lucas—had very limited interest in Friedman's other monetary work. By the 2000s, it was a safe bet that when a research paper used the term "Friedman rule," it was referring not to Friedman's constant-monetary-growth rule but, instead, to the optimum-policy result derived by himself (among a

[50] Gilchrist and Williams ended up working on this paper for the first five years of the decade, putting it out as an NBER paper in August 2004, 4½ years after the NBER event. See Gilchrist and Williams (2004).

[51] At the time, these conferences were co-organized by the Federal Reserve Bank of San Francisco and the Stanford Center for Economic Policy Research (which, around the turn of the century, renamed the Stanford Institute for Economic Policy Research, likely to avoid confusion with the longstanding London-based institution, the Centre for Economic Policy Research, with which it was not connected). The instalments of this March conference series that Friedman attended during the 1983–2006 period were those in 1993, 1994, 1996, 1998, 2001, 2002, 2004, and 2005. Each of these conferences was held in March. Frequently, Friedman attended only the first day of the conference (but there were exceptions, including in 2001, when Friedman was present on both days; information received at the time from Bennett McCallum). The author is grateful to Glenn Rudebusch and the librarians of the research department of the Federal Reserve Bank of San Francisco for this information.

[52] See Lucas (2000). The conference session was held in the Federal Reserve Bank of San Francisco's Fourth Floor Conference Room on March 4, 1994, at 9AM, as part of "Monetary Policy in a Low Inflation Regime," Federal Reserve Bank of San Francisco and Stanford University Center for Economic Policy Research conference, March 4 and 5, 1994.

number of other 1960s writers) that steady deflation was welfare-improving.

For his part, Friedman used the occasion of his remarks on Lucas' paper to disown the deflation rule as a desirable practical policy prescription—underscoring the position he had already given at the end of his 1969 paper. Contributing to an event held in Tokyo in late October 1995—the seventh instalment of the Bank of Japan's monetary policy conference series that Friedman had helped launch in 1983—David Laidler had observed: "Friedman himself did not seriously argue that the Federal Reserve System, or any other central bank, should aim at a secularly falling price level."[53] Friedman's remarks as Lucas' discussant, at the March 1994 Federal Reserve Bank of San Francisco conference, had provided further confirmation of this characterization of his views. His generation, Friedman told the delegates, knew from their own experience that deflation was harmful—and that monetary policy should be conducted in a manner that avoided a falling price level. "He said, 'People of my generation—we are still very, very worried about price deflation—so bringing the inflation down to zero is O.K., but don't forget that there's a risk of deflation, and that could be very costly.'" (Guillermo Calvo, interview, April 1, 2014.)

Two years later, Friedman was again on the program of the conference series, as one of the discussants of Calvo's paper "Capital Flows and Macroeconomic Management."[54] Calvo greeted Friedman before the session began. "And so, I come to the podium, he's there. I say, 'Oh, hi, Milton, how are you doing? Nice to see you.' And he looks at me, and he says, 'Well, I'm not sure that you will think that way when I finish discussing your paper.'"

Calvo was unnerved by the remark and was relieved to find that Friedman's remarks merely amounted to a statement of a different viewpoint of the kind that Calvo was used to from conference discussants. "He thought I was going to be mad after the discussion," Calvo recalled. That was not the case: "there was room for disagreement" and "I was not mad" that Friedman had taken issue with the paper's argument (Guillermo Calvo, interview, April 1, 2014).

The focus of Calvo's paper was the emergency support that the United States authorities had provided to Mexico's government in early 1995. Friedman opposed the support to Mexico, and

[53] Laidler (1997, p. 71).

[54] Published as Calvo (1996). The session was held at the Federal Reserve Bank of San Francisco's Fourth Floor Conference Room, on March 1, 1996, at 8.30AM, as part of the conference "Monetary Policy: Measurement and Management," Federal Reserve Bank of San Francisco and Stanford University Center for Economic Policy Research conference, March 1, 1996. Friedman was the first discussant, the second being Stanley Fischer, who was then still a professor of economics at MIT but would permanently leave the university the following September, initially to become First Deputy Managing Director of the International Monetary Fund.

in his 1996 conference remarks he essentially gave a restatement of the intertemporal approach to the current account. Friedman used this as the basis for suggesting that the U.S. government should have stood back and allowed Mexico's financial problems to be resolved by a floating exchange rate in conjunction with wholly private sector-based renegotiation of loan arrangements. Calvo was very familiar with the argument but felt that imperfections in capital markets made it inapplicable to the Mexican situation.

On later occasions, Friedman restated his opposition to the U.S. official assistance package regarding Mexico (for example, in *The Times* (London), October 12, 1998).[55] His arguments echoed his early statements on the LDC or Latin American debt crisis in which he had poured scorn on the need for a role for the U.S. government in the international negotiations. As we have seen, however, the limits of that position became clear when the Latin American debt crisis spilled over into major problems for U.S. commercial banks, at which time Friedman became very supportive of the federal government interventions that occurred to stabilize the U.S. banking system.

In defending the U.S. government's response to the Mexican crisis, Federal Reserve Chairman Alan Greenspan testified that he regarded the U.S. support to Mexico as the "least worst of the various initiatives that present themselves as possible solutions," and that a purely private sector response did not seem capable of "removing the threat of widespread contagion affecting the international financial system."[56] From this perspective, the U.S. government assistance to Mexico helped forestall the possibility of a U.S. financial crisis.

Friedman's criticism of the international-assistance packages like that to Mexico begged the question of whether the help given to the Mexican government prevented major spillovers of banking problems from Mexico in the United States; if the assistance had not been given and major spillovers to U.S. banks had then occurred, it is conceivable that there would be a repeat of the 1984 scenario, in which the federal authorities shored up the U.S. banking system with Friedman's vocal support.

Friedman attended the 1996 San Francisco conference only with the assistance of a manually

[55] See also Friedman (1998). Here, and in the *Wall Street Journal* of February 12, 1997, Friedman criticized the Mexican government for having pegged the exchange rate in the period leading up to the crisis.
[56] Greenspan (1995, p. 263).

operated walker.[57] His mobility had been limited since he developed back problems that were followed by an operation.[58] Further surgery and convalescence greatly restored Friedman's mobility and, in late 1997, John Taylor was struck by how frequently Friedman was coming into his Hoover Institution office.[59] Friedman then attended the March 1998 Stanford University/Federal Reserve Bank of San Francisco conference, and later ones, without a walker.

In the period of 1994 through 1997 over which Friedman was repeatedly waylaid by illness, he and his wife did continue their progress with their memoirs, which duly appeared, under the main title *Two Lucky People*, in 1998 (with a trade paperback edition following in 1999). The publication of the memoirs led to the appearance, in Friedman's old location of *Newsweek* magazine, of a profile of him by Robert Samuelson, who had dislodged the Friedman and Lester Thurow column space in *Newsweek* in 1984 to become its economics columnist-reporter (*Newsweek*, June 15, 1998). Robert Samuelson felt that the book had too little introspection. Anna Schwartz and many others, in contrast, felt that the memoirs' key drawback was too little economics.

A feature of the book that made it vulnerable to both these criticisms was that, despite its length, it was broad-brush in nature. At an early stage of the drafting, Friedman had remarked, "when you start digging back into your past, you find that you've forgotten so much, and there's so much to dig out." (CSPAN, November 20, 1994, p. 11 of transcript.) The lack of detail of *Two Lucky People* on numerous economic debates and events in which Friedman had been involved suggested that the Friedmans had had little enthusiasm for an extensive digging-out exercise and preferred to write an account that concentrated largely on their travels and television work.

Changing administrations in Washington

In the first week of May 1993, Friedman made what would turn out to be his only visit to Washington, D.C., over the whole eight years of the Bill Clinton presidency. This trip, taking place less than four months into Clinton's tenure, would consist mainly of activities on May 6. During the daytime, Friedman appeared as a guest speaker to a gathering of Republican members

[57] Noted to the author at the time by Bennett McCallum, who attended the conference.
[58] See Friedman and Friedman (1998, pp. 565–566).
[59] Author's conversation with John Taylor and Bennett McCallum, Monetary Policy Rules pre-conference, NBER office at Cambridge, Massachusetts, October 31, 1997.

of the U.S. House of Representatives.[60] In this appearance, Friedman combined criticism of the—still mostly prospective—Clinton economic policies with those of Clinton's immediate predecessor: "There's a good deal of talk around about how the Clinton program is a program of change. That's nonsense. The *Bush* program was a program of change. I think the talk about 'twelve years of Reagan-Bush' is utter nonsense." The message of Reaganomics, he suggested, was "lower marginal tax rates, less regulation, restrain government spending." In contrast, the economic program of the Bush Administration "as it worked out, raised marginal tax rates, increased regulation, increased government spending… [It] was reverse Reaganomics." With regard to the new administration, Friedman asked: "What is the Clinton program? Still higher tax rates, still more regulation, still more spending. So, far from being a change, the Clinton program is 'Bush writ large.' It's an expanded version of Bush's program." (CSPAN, May 7, 1993.)[61]

The fact that the direction of economic policy had reversed course after Reagan's departure in 1989 had led Friedman to move away from the guardedly positive outlook that he had adopted during the late Reagan period and back to the kind of pessimism about reversing the tide of the expansion of government that he and Rose Friedman had articulated in *Tyranny of the Status Quo* in 1984. On the evening of May 6, in a further event during his D.C. visit, Friedman spoke at a 1000-attendee gala dinner hosted by the Cato Institute at the Washington Hilton. Although he had been speaking to many U.S. legislators earlier in the day who seemed sympathetic to what he had said, Friedman did little to exclude them from the bleak generalization that he offered in his dinner speech: "With few exceptions, the people in Congress are decent people who want to good… [but] are simply immersed I an environment in which all the pressures are in one direction—to spend more money. Recent studies demonstrate that most of the pressure for more spending comes from the government itself. It's a self-generating monstrosity."[62]

Friedman also declared in the speech: "We are not governed by the people; that's a myth carried over from Abraham Lincoln's day."[63] He had suggested in a television interview in the previous year that the Lincoln generalization had been true once but no longer was: "The government is no longer run by the people. It's run by the bureaucrats in Washington." (*Wall Street Week,* Maryland Public Television, February 21, 1992, p. 6 of transcript.) In this vein, in his May 1993

[60] During the same visit to Washington, D.C., Rose Friedman took the opportunity to have lunch with Arthur Burns' widow, Helen (Gloria Valentine, personal communication, May 10, 2013).
[61] See also Friedman (1993d, p. 12).
[62] Friedman (1993d, p. 14). See also Friedman (1993c, p. 11).
[63] Friedman (1993d, p. 12).

Cato Institution speech Friedman repeated a formula to which he had become attached during the late Bush years as a summary of his dissatisfaction with the modern situation in the United States: “We have government of the people, by the bureaucrats, for the bureaucrats.”[64]

With regard to prospective policy developments under Clinton, Friedman had told the Republican legislators: “The most important thing, I think, is to prevent a rise in taxes. Any kind, any shape. Because that’s going to increase spending. It’s not going to reduce the deficit.” (CSPAN, May 7, 1993.) Friedman’s initial expectation that government spending would expand in relation to the economy under Clinton (*Wall Street Journal*, March 23, 1993; also *Australian Business Monthly*, October 1993, p. 55) and were likely reinforced in August 1993 when Clinton secured Congressional approval of an increase in income taxes. In fact, although real government outlays certainly increased under Clinton—nominal federal outlays rising cumulatively by about 32 percent from 1993 to 2001, compared with an increase in the U.S. price level of about 16 percent—real outlays declined notably as a share of the economy, as discussed below.[65]

Asked in late 1994 to characterize Clinton’s own posture, Friedman replied, “Oh, he’s a socialist,” citing as evidence the healthcare-reform proposals (discussed in the next section) that the administration had advanced in Clinton’s first year and a half in office but had recently allowed to lapse, in the face of considerable opposition.[66] The harshness of this criticism was somewhat blunted when, nearly a decade later, Friedman labeled the Reagan era as having been one of “creeping socialism” (*Wall Street Journal*, June 11, 2004; *Investor’s Business Daily*, p. A1; see also Pringle, 2002, p. 22). Furthermore, he viewed this label as something of a compliment to Reagan and his three immediate successors (George H.W. Bush, Clinton, and George W. Bush), who together, Friedman contended, had presided over slower growth in nondefense spending and in regulations than had characterized what Friedman called the “galloping socialism” of the 1960s and 1970s.[67]

[64] Friedman (1993d, p. 12). He made similar formulations in *Frisko* in 1992 (p. 68), *Forbes*, August 17, 1992 (p. 42), and in a lecture that Friedman gave in New York City on November 19, 1991, organized by the Manhattan Institute and published as a booklet in 1993 (see Friedman, 1993c, p. 11).

[65] The numbers were obtained from Council of Economic Advisers (2011, Table B–7, p. 198, and Table B–78, p. 283). The spending totals in these tables referred to fiscal years and the price data to averages of calendar years. Consequently, the 1993-2001 increases given in the text using each of these tables cover similar but not identical timeframes.

[66] CSPAN, November 20, 1994, p. 14 of transcript.

[67] Friedman had earlier cited slower annual increments in federal regulations as an achievement of Reagan in *Courtlandt Forum* (March 1992, p. 77) and even during assessments that he gave in Reagan’s first term (see, for example, Friedman and Friedman, 1985, pp. 9–10). See also Chapter 15 above.

As this 2004 assessment confirmed, Friedman came to be happier with economic policy for the Clinton years as a whole than he was with the administration's early initiatives. The possibility that the Clinton Administration would not, in the event, oversee a major expansion of the public sector's role was heightened by the fact that the Republican party won control of both houses of Congress in 1994, with the party's preceding campaign stressing a reduction in the federal government's role in the economy. In discussing how the midterm election outcome would likely affect economic policy in 1995, Friedman was cautious: he indeed perceived a prospect of a reduction in the role of government but noted that he had previously seriously overestimated the amount of change in that direction that would occur under Reagan. Although at the recent Congressional election it appeared that the "public at large has made it clear that it want[s] a smaller government," he was not sure that this outcome would eventuate: "I may say, I've been wrong [in thinking it would] before. I thought that in 1980." (*Louis Rukeyeser's 1995 Money Guide*, Maryland Public Television, January 1, 1995, p. 7 of transcript,)

In the event, the share of federal government outlays in GDP, which had already resumed declining late in the George H.W. Bush period, fell every year under Clinton, and in fiscal years 2000 and 2001 was, at 18.2 percent, the lowest share since fiscal-year 1966 (Council of Economic Advisers, 2011, Table B–78, p. 283). The behavior of the spending share was a factor underlying Robert Chitester's assessment that Friedman "was ecstatic with what happened in the eight years of Clinton." (Robert Chitester, interview, July 9, 2013.)

That said, the fall in the share of government spending in GDP was less than what the Republicans were initially seeking after the 1994 Congressional election. And the Republicans' spending-restraint agenda was, in turn, much less ambitious than what Friedman wanted. He remarked in a television interview given during the January 1996 American Economic Association meetings, held in San Francisco, that "government spending—all levels, federal, state, and local, relative to national income was in 1928 … 10 to 12 [percent]. And it was [at] that level throughout American history, except in time of great war. So," he inferred, "we have made a tremendous change" over the previous seventy years, toward a larger government sector. With regard to his own preference regarding the spending share, Friedman indicated: "In my opinion, that's not a bad number, 10 to 12 [percent]—[it] would be about the ideal amount of government spending. It would cover the basic functions. But, today, we're [instead] at 43." "So the House Republicans aren't tough enough for you?," the interviewer asked. "They certainly are not," Friedman replied. "They're making a minor step. But I don't blame them. Because you've got to start with a small step." (*The NewsHour With Jim Lehrer*, PBS, January

9, 1996, p. 7 of transcript.)

Despite some pleasant surprises that he had received about how things were proceeding under Clinton, Friedman still strongly preferred that a Republican be U.S. president and endorsed Bob Dole, the Republican candidate, when Dole ran against Clinton in 1996, with Dole doing so partly on a tax-cut program.[68] Subsequently, President Clinton himself implemented one of the tax-cut measures that Dole had promised, by signing a cut in the capital gains during his second term (see Chapter 16 above). In the early Clinton years, Friedman had continued to question the priority that Republicans were giving to a cut in the capital gains tax rate (CSPAN, May 7, 1993), although by late 1994, shortly after the Republican Congressional victory, he seemed to have become more sympathetic to such a move, and cited the benefits of the cut in the capital gains tax rate that had been made in the early Reagan years, subsequently retracted under Reagan and Bush (*Sydney Morning Herald*, November 24, 1994).

After 1994, the Clinton years saw very little in the way of poor economic news. The president's second term, in particular, was characterized by a stream of favorable economic developments. Friedman downplayed, albeit with good humor, the credit that he believed was due to the president for this success. In early 1999, Peter Robinson in the Hoover Institution interview series *Uncommon Knowledge* asked: "Milton Friedman, we are in the sixth [sic; seventh] year of the Clinton Administration, the nation is at peace, the economy is booming, the federal government has gone from a budget deficit of 290 billion dollars in 1992, the year Bill Clinton was elected, to a surplus of at least 76 billion dollars for this year. Don't you want to give Bill Clinton an *A*?" Friedman replied: "No, I want to give the economy an *A*." (*Uncommon Knowledge*, Hoover Institution, February 10, 1999, p. 1 of transcript.)

After the 1994 Republican victory, Clinton had regained political dominance. He was mostly in a strong bargaining position from late 1995 until the start of his sixth year in office in early 1998 (after which good economic news continued, as did the president's popularity, but the Monica Lewinsky scandal affected his political standing). Clinton was able to prevail from late 1995 to late 1997 over Republican party efforts to introduce more ambitious attempts to restrain domestic federal government spending, and Friedman expressed disappointment at the degree to which the fiscal policy agreements that were reached had ended up maintaining federal expenditure at existing levels (*Wall Street Journal*, July 8, 1997).

[68] Friedman stated: "This an excellent economic program." See *https://www.writersreps.com/Trusting-the-People*.

Friedman had suggested that reductions in federal spending might prove more popular than politicians realized (*Wall Street Journal*, June 15, 1995). But he was also conscious of the fragility of the apparent U.S. political consensus in favor of free markets and restraint of the public sector. In particular, Friedman had noted in 1994 that "the bulk of the intellectual community almost automatically favors any expansion of government power so long as it is advertised as a way to protect individuals from big bad corporations, relieve poverty, protect the environment, or promote 'equality.'"[69]

The fiscal policy outcomes seen in the final six years of the Clinton Administration, Friedman suggested in one retrospective, were largely an "accident of divided government… a Democrat in the White House, a Republican House and/or Senate." (*Wall Street Journal*, January 15, 2003.) "What really improved matters in the Clinton years," he similarly argued when looking back on another occasion, "was that you had a gridlock. You had a Democratic president and a Republican House and Senate, and you could not pass spending bills." (*Election 2004: The Economy*, KQED, October 15, 2004.) This held true to some extent with the passage of U.S. laws more generally, with Friedman telling Peter Robinson (*Uncommon Knowledge*, Hoover Institution, February 10, 1999, p. 1 of transcript): "Because you have a Democrat in the White House and Republicans control the Congress, it's hard to get any laws passed. And that's been a great advantage."[70] It was mainly spending bills rather than other laws, however, that Friedman saw as being stifled: "while there's no shortage of laws passed, there are no major programs." (*Forbes*, May 3, 1999*a*, p. 141.)

Although the explicit applause that Friedman gave to political gridlock would have appeared to some as indicating a cynical or nihilistic attitude, it was a reaction that complemented his longstanding position that tax revenues had an important role in driving governing spending. President Clinton had established political credentials in the area of deficit control, and the Republican side in the 1990s was also focusing on deficit control and on spending restraint as a means to this end. These positions were brought out in a January 1998 news item (cnn.com, January 5, 1998). Clinton was quoted as saying that "between 1981 and 1992 we projected all kinds of things [about deficits declining] and went out and spent the money on tax cuts and spending—both. We spent the money. And we quadrupled the debt, drove up interest rates and

[69] Friedman (1994a, pp. xv-xvi).

[70] Friedman had similarly remarked during the Ford years (when Democrats controlled both houses of Congress) that there was an advantage to have presidential and Congressional control in different parties' hands: "Congress passes laws that are mostly bad, and the harder [it is] to pass the laws, the better." (*Milwaukee Journal*, October 30, 1976.)

put our country in a terrible hole." Senior Republican member of Congress John Kasich was quoted in the same news report as saying: "The idea that we ought to take any of this money and expand government for anything, to have any additional spending beyond the budget agreement, would be a terrible mistake." Clinton therefore believed that new outlays had to be paid by increases in revenue, while Kasich expressed opposition to increases in either spending or tax rates. In a situation in which these were the main positions taken by each side, there was little likelihood of major tax or spending reductions—but also little likelihood of major tax or spending increases. As deficit control diminished as a political priority after the 1990s, so did the perception that the need to maintain a budget surplus or prevent a return to large surpluses put a limit on increases in government spending or on proposals for tax cuts.

The momentum for smaller government had largely dissipated by early 2001, when Bill Clinton's successor, George W. Bush, prepared to take office. Bush was in favor of a major tax cut—something that, by itself, was bound to find favor with Friedman. A sting in the tail, however, was that one of the arguments being advanced by his economic team in support of his proposed tax cuts was that the U.S. economy was softening and that a fiscal stimulus to aggregate demand was warranted. Paul Krugman remarked in his *New York Times* column (January 10, 2001), "If Milton Friedman weren't still alive, he'd be spinning in his grave."[71] This particular Krugman account of Friedman's views was on the mark. As Taylor (2012, p. 34) noted, Friedman was indeed disturbed by those arguments being put forward during 2001 for tax reduction that appealed to the notion that a short-term boost to aggregate demand was achievable via fiscal measures and was desirable. Such arguments actually appeared in the administration document that outlined Bush's 2001 proposal: "Over the past several months, the economy has slowed dramatically. President Bush's tax cut will give the economy a timely second wind by placing more money in the hands of consumers and entrepreneurs."[72]

Friedman did, however, support the tax cuts of the early 2000s on the basis of his usual starve-the-beast rationale as well as the supply-enhancing aspects of some of the tax cuts (see, for example, *San Jose Mercury* (California), November 5, 2006*a*).

[71] Turns of phrase along the lines that Krugman used in 2001 had appeared in print before, although not frequently. For example, in *The Comic Reader* #172 (September 1979), Michael Tiefenbacher wrote in an entertainment news column with regard to recent casting decisions on the part of Zorro: "[Actors] Douglas Fairbanks and Tyrone Power would turn over in their graves. Guy Williams would, too, if he were dead." An item in the Australian magazine *Nation* (December 21, 1968, p. 19) had said of a recent London play that it would, "if he were dead instead of merely institutionalized at his desk at the National Theatre, have Kenneth Tynan spinning in his grave."

[72] White House (2001, p. 1).

President George W. Bush actually secured the passage of tax cuts in both 2001 and 2003. The passage of the second tax-cut package in 2003 reflected in part the improvement since 2001 in Bush's political popularity and in the Republicans' Congressional numbers. To advance a further tax-cut package in 2003 after a package had already been delivered in 2001 was nevertheless a highly controversial move, and it was criticized, as in 2001, by commentators such as Krugman, as being a departure from the stress on control of federal budget deficits and debt. Furthermore, in a development that was anathema to Krugman and Friedman alike, the notion that tax cuts were desirable as an anti-recession measure was utilized by many proponents of the tax cut in 2003 as it had been in 2001. Nevertheless, among those inclined to favor tax cuts on supply-side grounds, the 2003 package was often regarded as offering improvements on the 2001 package, while also expediting the cuts legislated in the earlier package. Along these lines, Friedman suggested: "President Bush's tax proposals rank very high… Making the already voted tax reductions permanent, bringing their effective dates forward, and lowering the rates further" were desirable "changes [that] increase the restraint on government spending and increase incentives for taxpayers to work, invest, and take risks." (*Wall Street Journal*, January 15, 2003.)

Friedman, who had written a short piece on George W. Bush against Al Gore in the 2000 presidential election campaign (*Wall Street Journal*, November 1, 2000), met President Bush on a few occasions, including in May 2002 when, on Friedman's first visit to Washington, D.C., in nine years, the White House held a ceremony—attended by Anna Schwartz and Allan Meltzer, among others—in recognition of Friedman's then-forthcoming ninetieth birthday (July 31, 2002).

By the time of that visit to Washington, the United States was known to have entered a mild recession in early 2001, although it was not yet firmly established that a recovery from the recession had already begun in late 2001. Economic issues, including the recession, were tending to take a back seat in the early 2000s to national-security and foreign policy issues following the terrorist attacks of September 11, 2001.

Friedman's reaction to the September 11 attacks is discussed in the section below titled "Into the New Economy." Part of his posture was to support U.S. military actions in Afghanistan. But he did not support the Iraq war that began in 2003. Donald Brash, former Reserve Bank of New Zealand governor and a longtime Friedman friend, recalled (interview, July 9, 2013) that on an occasion when he and others dined with the Friedmans in the mid-2000s, "he glanced across the table at Rose and said, 'This is an issue on which we [the Friedmans] disagree.'" Milton Friedman was critical of the U.S. Iraq operation, while Rose Friedman favored it—a disagreement that carried over into the pages of the *Wall Street Journal* in an interview that the couple gave shortly in July 2006, just ahead of Milton Friedman's ninety-fourth birthday (*Wall Street Journal*, July 22, 2006).

It was primarily the issue of the Iraq war that had been the basis for a student protest against the president three months earlier, on April 21, 2006, on the Stanford University campus. The occasion for Bush's visit to the campus was a meeting that had been arranged for him with various Hoover Institution fellows. One of the fellows, Robert Hall, recalled (interview, May 21, 2013): "I remember we had a rather strange meeting with President Bush—I think it was within a few months of him [Friedman] being gone. And it was a complete fiasco, logistically, because the meeting was originally supposed to be at Hoover. Well, how do you get the president of the United States to Hoover?" The student protest blocked the street that provided access to the Hoover Institution.[73] "So when a bunch of students just laid out on the streets, that was it—that was the end. (*laughter*) There was no way the Secret Service was going to be videoed hauling away lying[-down] students. The thing had to be relocated to George Shultz's house. So, anyway, we all trooped out there." Friedman was among those talking to the president at the relocated event.

In between the 2002 and 2006 meetings that Friedman had with President George W. Bush, Greg Mankiw had served, from May 2003 to December 2005, as head of Bush's Council of Economic Advisers. "I had one opportunity to go to Camp David," Mankiw recalled of his tenure (interview, September 24, 2013). "President Bush invited the whole economic team to come up for the weekend, and so my wife and I and some of the other economic team and their spouses all went to Camp David for the weekend. And, as a thank-you, I gave him a copy of *Capitalism and Freedom*. I don't know if he ever read it. But I have it in my files—I have a thank-you note, 'Thank you for the copy of *Capitalism and Freedom*,' from George Bush."

The author of *Capitalism and Freedom* had a warm relationship with President George W. Bush that had not characterized Friedman's relations with the previous President Bush. But, on public spending (including nondefense outlays), as distinct from taxes, Friedman made it clear that he was disappointed with the George W. Bush record. He had remarked at the start of the Bush

[73] See *www.paloaltoonline.com*, April 22, 2006.

years that he was not expecting very much: "The American public is committed to creeping socialism. The only question in the [2000 presidential] election was whether to creep slowly with Bush or fast with Gore."[74] Nearly five years later, Friedman stated that it was "terrible" that federal government expenditures had been rising at a considerably more rapid rate since the end of the Clinton presidency (*The Charlie Rose Show*, PBS, December 26, 2005, p. 9 of transcript).

The state government scene

In the Friedmans' now-longtime home state of California, Friedman's friend Arnold Schwarzenegger became the governor in late 2003. Two years later, policy initiatives that Schwarzenegger had advocated, covering government spending limitation and education, were defeated when put to a statewide vote (*The Philadelphia Inquirer*, November 10, 2005). Schwarzenegger would come to be seen as taking the defeat of these ballots as a signal that the public did not favor a major scaling-back of the size of the state government. Friedman, badly out of touch with public opinion on these matters, had predicted that the propositions would be passed by the voters. Michael Mork recalled that he saw Friedman "about three months before… and I said, 'They're all behind in the polls—how can they be losing?' He [Friedman] said, 'No rational person could vote against these. They're all extremely rational.' He said, 'Mike, I guarantee they're going to pass.' Well, they all went down in flames. The labor unions, the teachers, the nurses, all these unions, they made anybody who voted for that thing sound like the meanest person on earth. And they all went down in flames. And Friedman said he was just stunned that they got beaten." (Michael Mork, interview, June 4, 2013.)

Friedman had a loose, but official, affiliation with Schwarzenegger's government. On October 4, 2004, Schwarzenegger had his first meeting with what was called his Council of Economic Advisors. The council's chair was George Shultz, and Friedman was among the members. Unlike, however, the federal government body to which it had a near-identical name, the California government advisory group was not a permanently-in-being committee but, instead, a group of outside advisers that met with the governor sporadically. It was therefore much more like the old Reagan PEPAB than a counterpart to the national CEA. Like PEPAB, the California CEA was quickly perceived as largely being a talking shop, with a media commentary on its first meeting with Schwarzenegger declaring it to be a "show about nothing."[75] The same occasion

[74] In Ullmann (2001, p. 17).
[75] *The Sacramento Bee*, October 5, 2004. This was not a first-hand assessment, however, as the bulk of the meeting was closed to the media.

did, however, provide a notable photo opportunity, with Schwarzenegger and Friedman posing for a picture together. Schwarzenegger, who was a foot or more taller than Friedman, quipped: "This is *Twins 2*." (*The Sacramento Bee*, October 5, 2004).[76]

The changing faces

Friedman appeared as a guest on the CNBC daytime television program *Power Lunch* in March 2003. The interview, in which Friedman had spoken favorably about insider trading and had offered critical (but, by his standards, restrained) remarks about the U.S. public-school system, was a prerecorded portion of what was mainly a live broadcast. Near the end of the program, the host Michelle Caruso-Cabrera quoted from emails that viewers had sent in, during the broadcast, concerning the Friedman interview. One of the viewers quoted was not impressed. "Like Michael Jordan, it would be best if he had gone out at the top" was the viewer's verdict on Friedman. (*Power Lunch*, CNBC, March 12, 2003.)

In Friedman's case, going out at the top would not have been a very realistic option, as it would have meant leaving the scene very much earlier: his most pathbreaking research had been in the 1950s and 1960s, while the point at which support for his views on economic issues had reached a maximum, both in the economics profession and public opinion, had probably been in the early 1980s. Furthermore, by 2003 Friedman was already, in any event, adhering closely to the CNBC viewer's suggestion of being in retirement—even though his continuing stream of media interviews was creating the contrary impression. His availability for interviews about current developments was not a reflection of a lifestyle in which he was working anything close to full-time—he was not. But it did bear testimony to Friedman's ongoing interest in the unfolding of economic and social developments. As the 2005 California referendum results confirmed, Friedman did not have a good grasp of the state of public opinion in the new century, but he was still interested in understanding current trends.

This interest was brought home to Thomas Jordan, a close friend of the Friedmans during the final couple of decades of Friedman's life, when Jordan visited their property in Sea Ranch, California, and walked in to see Friedman reading a book. "And I said, 'Milton, what in the world are you doing reading *Harry Potter*?' And he said, 'Well, there's so much of a to-do

[76] Subsequently, the *New York Review of Books* could not resist including the photograph as its main image in the hardcopy version of Paul Krugman's article on Friedman (see Krugman, 2007, p. 27), although the 2004 event really had very little to do with Krugman's piece, which focused on Friedman's activity through the 1980s.

about this that I just wanted to read it for myself.' So his curiosity knew no bounds: Not only was he most particularly interested in economics, he was [also] interested in everything going on around him." (Thomas Jordan, interview, March 24, 2013.)

Friedman's curiosity would also be manifested in his continuing practice of dropping into conferences to see what was going on in the research world. This world now consisted, to a considerable degree, of faces that were fairly new to him. Although Anna Schwartz and Paul Samuelson, both of whom turned 90 during 2005, would outlive Friedman, many of Friedman's other contemporaries or near-contemporaries in the economics profession died during the years from 1993 to 2006. Among these were Friedman's brother-in-law, Aaron Director, who died in September 2004, aged 101, after having been ill for several years. Some key Keynesians with whom Friedman had dueled in debates in print during the 1950s, 1960s, and 1970s also passed away, including Don Patinkin in May 1995, Robert Eisner in November 1998, James Tobin in March 2002, Albert Ando in September 2002, Charles Kindleberger in July 2003, Franco Modigliani in September 2003, and John Kenneth Galbraith in April 2006.[77]

But the debates in which Friedman had been involved had spanned more than one generation and had correspondingly included participants born much later than himself. As discussed in Chapters 4 and 15 above, his sparring partners had included Alan Blinder. Although he was a full generation younger than Friedman, by the time Blinder attended the March 2004 Federal Reserve Bank of San Francisco conference to serve as a discussant of a paper, he was himself a veteran of the profession.[78] 2004, during which Friedman would turn 92, was the twentieth anniversary year of Blinder's debate with Friedman at the University of California, Davis, on fiscal policy. To Blinder's surprise, the Federal Reserve Bank of San Francisco conference that year would turn out to be another occasion for meeting Friedman, who attended the event. "I remember seeing him and just speaking to him briefly, but mainly being so stunned to see him, at one of these Federal Reserve of San Francisco conferences… And the reason I was stunned was—first of all, as he was quite old, that he actually got there; secondly, less surprising, knowing Friedman, that he was very lucid. I mean, he didn't just sit there, he spoke. … [I]t was fairly late in his life; these were technical papers, loaded with math, and equations, and things

[77] Among those who had been pitted against Friedman in debates largely in non-research contexts, as well as outside the Keynesian-monetarist disputes, George W. Mitchell died in January 1997, William McChesney Martin, Jr., in July 1998, and Jude Wanniski in August 2005.

[78] The event was "Interest Rates and Monetary Policy," Federal Reserve Bank of San Francisco and Stanford Institute for Economic Policy Research conference, March 19-20, 2004. Blinder was on the conference program as the discussant of the paper by Sharon Kozicki and Peter Tinsley, "Permanent and Transitory Policy Shocks in an Empirical Macro Model With Asymmetric Information," a study related to Kozicki and Tinsley (2005).

like that. And, you know, it surprised me that Milton, at age, let's just say, 91—I don't really remember how old he was then—either had the inclination or the ability to plow through these papers. But it was clear that he had. I mean, he didn't just come in the room and fall asleep in the back, he was sitting up near the front, and he was commenting on the papers. I was incredibly impressed—and thinking, 'When I'm 91, I'll be dead for ten years, I will not be doing that.'"[79]

II. ISSUES, 1993–2006

HEALTHCARE

Over the postwar period, the U.S. healthcare system came to be regarded as one that relied to a much lesser degree than that of other major countries on government involvement and that, correspondingly, had closer to free-market arrangements than that seen elsewhere. The U.S. system was, in particular, contrasted with the healthcare arrangements prevailing in other key market economies, in which payment for particular medical services was predominantly made by the central government, rather than directly by the patients receiving the services, and in which many hospitals were public-sector-owned bodies. Such arrangements prevailed from early on in the postwar period in the United Kingdom, with the introduction in 1948 of the National Health Service, but also came into force later in various other market economies—with Canada one example that would figure importantly in U.S. discussions.

This backdrop meant that, in those interventions that he made on healthcare once he had become well established as an exponent of market economics, Milton Friedman was liable to be perceived as a defender of the *status quo* as far as U.S. arrangements were concerned.

This was, of course, not the case. Well before he became very widely known as an advocate of free markets, Friedman had, in public statements dating back to 1939, been a critic of the existing U.S. healthcare system, in good part through his work with Simon Kuznets on restrictive

[79] Alan Blinder, interview, December 6, 2013. Friedman was indeed age 91 at the time of the conference. Lawrence Summers also recalled (interview, November 22, 2013) that at a point in the period "between 2001 and 2006, I gave some kind of speech at Stanford, and he asked a very thoughtful question. I made some remark referring to a variety of his earlier work that he and the audience appreciated. And I don't think I said it explicitly, but certainly the impression I had at the time, and the impression I left the audience with, was that I sure hoped that I was half that sharp, or a quarter that sharp, when I was in my nineties."

practices regarding the supply of doctors in the United States.[80] This work provided a basis for Friedman's more explicit criticism of medical licensure and his advocacy of reform, during the 1950s and in *Capitalism and Freedom*.

By the start of the 1980s, Friedman's assessment, as outlined in the Friedmans' discussion in the book version of the *Free To Choose*, was that the American Medical Association's (AMA's) market power over the supply of medical services had diminished since the time of the Friedman-Kuznets study, in part because other medical leadership organizations had drawn away some of its authority and in part because the increase in the government's role in medicine had, to some extent, had the effect of lowering the AMA's ability to set conditions.[81] He also believed that, by the 1970s, the AMA was also following a less concerted policy than it had in previous decades to restrict the number of doctors.[82] Nevertheless, *Free To Choose*—in both its U.S. television and book versions, but especially the latter—continued to push for a major expansion of the range of medical services for which a doctor's license was not required.[83] Friedman would also make his objection to medical licensure a major part of his talks to an audience of American doctors at Minnesota's Mayo Clinic in May 1978 and to a gathering of Australian doctors at an event held in Sydney on April 13, 1981.[84]

But despite his continuing outspokenness after the 1940s on the matter, it was clear that the Friedman recommendation of abolishing, or greatly relaxing, medical-licensure requirements continued to be perceived as being well outside the realm of practical suggestions for healthcare reform in the United States. The main lines of U.S. healthcare debates largely took for granted

[80] Friedman and Kuznets (1945). This paper, although covering issues largely different from the medical issues of concern in the economic literature in the 1970s—discussed below—on the handling of healthcare payments on the demand side, was occasionally cited in that literature. See, for example, Feldstein (1974, p. 430).

[81] Friedman and Friedman (1980, p. 231; 1998, p. 72).

[82] *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978, p. 19 of transcript.

[83] Friedman and Friedman (1980, pp. 238-240). In the television version of *Free To Choose*, the argument was soft-pedaled to some extent by focusing on the need for greater authority to be delegated to other medical professionals, such as paramedics (*Free To Choose* (U.S. television version), PBS, Episode 8, "Who Protects the Worker?," March 1, 1980, pp. 1–2 of transcript).

[84] The Mayo Clinic lecture of May 19, 1978, was videorecorded to become *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care." Friedman's prepared text for this lecture was later the basis for much of the discussion that appeared in Friedman and Friedman (1980, pp. 112-115) of the public sector's role in healthcare.

Friedman's talk in Sydney was advertised in the Australian national press (see, for example, *The Bulletin* (Australia), April 7, 1981). The advertising gave the specific date of the talk, and that date was confirmed in Valentine (1987, p. 546). The Hoover Institution's website usefully includes the text of the published talk, but this text is preceded by an incorrect notation that the date of the talk was "late Spring 1981." In fact, the actual date of April 13 was not a date in late spring in the United States and, in its place of delivery of Sydney (a city located in the southern hemisphere), was not a date in spring at all.

the continuation of the existing procedures by which doctors entered the medical profession. Instead of dwelling on the question of the supply of doctors, they centered either on the demand side or on the demand-supply interface: on the choice between alternative arrangements regarding the insurance, oversight, and provision of medical services, and the cost and quality of healthcare implied by the various alternatives. In order to contribute to the main lines of the medical-services debate, Friedman had to take a stand on these issues, rather than concentrate mainly on the topic of licensure.

The role of government in medicine

Friedman did indeed make, from the 1950s onward, multiple interventions regarding the demand for medical services and regarding the government's role in the payment for, and provision of, medical services. And it was on these issues that Friedman did, in the 1950s through the 1970s, take a stand that was largely a defense of free-market arrangements and opposed to the United States moving in the direction of the United Kingdom and other countries.

During the 1950s, this stand on his part was manifested most notably in debates held on the radio: in a *University of Chicago Round Table* program "Who Should Pay for Hospital Care?" (NBC, 1955), discussed further below, and in a January 1956 debate on the short-lived successor program to *University of Chicago Round Table*, titled *New World*.[85] The *New World* appearance cast Friedman explicitly as a defender of the U.S. medical system over its U.K. counterpart. This forum consisted of a transatlantic hook-up between U.S. and U.K. radio studios, with Friedman being one of the American participants and Aneurin (Nye) Bevan—who, as Minister of Health in the first postwar U.K. Labour government, had introduced the National Health Service—being one of the London speakers (*Chicago Tribune*, January 29, 1956).

Similarly, Friedman's discussions of health policy would evolve over the years and, as discussed below, by the early 1990s were less fully free-market in their policy prescriptions than was the case in his 1950s broadcasts. An element of consistency over the years, however, was Friedman's opposition to the adoption of a U.K.-style system in which the government played a large role. He did grant from an early stage that the public sector did have a clear-cut role in one area of medical care. As discussed in Chapter 10 above, Friedman accepted that the government had a public-health responsibility—"public" here meaning not government but, instead, the U.S.

[85] *New World* was "discontinued on June 16, 1957" (Federal Communications Commission, 1972, p. 1871).

community or general public. The *Free To Choose* book defined the government's appropriate functions in this area as relating to "sanitation, contagious diseases, etc."[86]

In an early intervention, made in a 1952 radio appearance (earlier than the aforementioned radio debates that were specifically concerned with healthcare), Friedman conceded the spread-out benefits (what would come to be called the positive externality) associated with public-health activities of government. But he maintained that the externality argument was limited: "To some extent this is true of certain parts of public health, but in the main it seems to me not true of most of the new items which have been added in Britain under the heading of socialized medicine."[87]

"Socialized medicine"—a less pejorative phrase in 1952 than in later years (when Friedman continued to use it)—was a term frequently applied to the NHS in the early period of its existence. Although other examples, such as Canada, emerged in later years, the U.K. health service remained a key example of what Friedman would describe as "predominantly governmental" healthcare system and that he would compare unfavorably with that of the United States (*Newsweek*, November 7, 1977).[88]

Medicare and Medicaid

In the years between Friedman's 1952 and 1977 commentaries that rejected the United Kingdom's NHS as a model for the United States, the contrast between the U.K. and the U.S. systems remained strong, but did it diminish significantly. Reflecting the developments that had occurred over this period, the Friedmans in their *Free To Choose* book judged with regard to the case of the United States: "Medicine is the latest welfare field in which the role of government has been exploding."[89]

Major increases in the federal government's role in the U.S. healthcare system had taken place in the mid-1960s, with the Johnson Administration's introduction of Medicare (which covered the medical costs of citizens over age 65) and Medicaid (covering low-income citizens). Although he declared in late 1974 that "Medicare is a failure," Friedman would acknowledge in a *Free To*

[86] Friedman and Friedman (1980, p. 112).
[87] NBC (1952, p. 9).
[88] In 1970, for example, he stated that it would be undesirable to "foist on this country what has been going [on] in Great Britain—which the British themselves have been liking less and less as time goes on… I really think it would be disgraceful for us to imitate a program if it has not been achieving what its proponents had hoped for in Great Britain, and I think it would not achieve it here." (*Chicago Daily News*, July 28, 1970, p. 4.)
[89] Friedman and Friedman (1980, p. 122).

Choose television panel discussion that "many people… have benefited from Medicare."[90] His position was nevertheless that "Medicare is another program that has done more harm than good." (*Chicago Daily News*, July 29, 1970, p. 4.) As a means of supporting those in need, Friedman felt that Medicare was too widely spread, as it applied to a whole age group: "the very notion underlying it is absurd. If you're going to help people in trouble, you ought to help people in trouble, not all people who happen to be over 65." (*Chicago Daily News*, July 29, 1970, p. 4.)[91] He therefore faulted the scheme for not having concentrated on those members of the age group who needed help in meeting medical expenses: "It's illogical to give something to 20 people because one of the 20 may need it." (*Medical Economics*, April 16, 1973, pp. 114, 119.) With regard to how he would change matters, Friedman observed (*Medical Economics*, April 16, 1973, p. 114): "Certainly, I'd end Medicare. I'd make it possible for genuinely poor people to get the medical care they need, but [I would] not [provide free medical services to people] merely because they're over 65."

The lack of targeting those in need that Friedman saw as a flaw in Medicare was linked to his criticism of its effect on the medical system as a whole: the value that it was providing by helping those who could not have afforded medical services was not occurring "in a cost-effective way."[92] By providing a free-for-user medical service to a whole age group, Medicare's effect had been to "cause an increase in the demand for services on the part of a particular group. Since there hasn't been any increase in the volume of services, that has to come out of somebody else." (*Chicago Daily News*, July 29, 1970, p. 4.) The wherewithal that Medicare provided this whole age group to obtain medical services had, on this reasoning, resulted in the crowding out of provision of medical service to other groups. In addition to its having had this effect on the recipients of medical services, Friedman believed, as discussed below, that the advent of Medicare had raised healthcare costs generally.

Being income-tested, Medicaid was not really vulnerable to the criticism of being inadequately targeted that Friedman applied to Medicare. He was, however, also a critic of Medicaid, on several grounds. He did support some scheme for covering the healthcare costs of the poor: Friedman was in favor of the notion that the community pay through taxes the medical expenses

[90] The quotations are respectively from Friedman (1975d, p. 16) and *Free To Choose* (U.S. television version), PBS, Episode 1, "The Power of the Market," January 12, 1980, p. 8 of transcript.

[91] In March 1982, Friedman observed (Friedman, 1982f, pp. 47-48): "some of the lower middle class have enjoyed an amelioration of their position—but at a staggering cost, because the universal nature of the program has meant that there is much spillage, or slippage, of the benefits."

[92] *Free To Choose*, PBS, Episode 1, "The Power of the Market," January 12, 1980, debate portion, p. 8 of transcript.

of low-income citizens (see Vaizey, 1975, p. 74).[93] But he felt that Medicaid was the wrong way to go about this. Instead, as indicated in Chapter 10, he favored a direct government transfer of funds to those receiving low incomes, in the form of a negative income tax, with the recipients allowed to make the decision on how allocate the funds among various expenses, including health expenses.[94]

Friedman also felt that, as Medicaid in part took the place of private-sector devices for providing relief to the poor in their medical expenses, its role in providing new support was liable to be exaggerated. He suggested that the program was, to a considerable degree, covering recipients who had previously received relief in other ways, such as via doctors waiving their fees (*Chicago Daily News*, July 28, 1970, p. 4) or charitable services paying the bills.[95] Related to the latter point, Friedman believed that the introduction on a large scale of government-paid medical bills had introduced more rigidity into the fee structure, with uniformity of fees across patients tending to supersede arrangements in which fees were graduated according to patients' income: "the enormous medical bills underwritten by the government have created a trend toward all patients having to pay" fairly uniform prices, possibly doing so indirectly through the government Medicaid or Medicare programs (*Medical Economics*, April 16, 1973, p. 99).[96]

This effect on pricing formed part of a broader indictment that Friedman made against federal health programs to the effect that, at the national level, they had boosted the expenses associated with the provision of U.S. medical services. By the 1970s, Friedman had concluded that the introduction of Medicare and Medicaid had raised medical costs in the United States substantially (see Nelson, 2020b, pp. 97, 375).[97] "Everybody is complaining about the rise in costs of medical care and health facilities," Friedman observed in mid-1970, and he used the same occasion to state his belief that the federal government had contributed notably to the rise, having "poured billions of dollars into the medical field" in recent years (*Chicago Daily News*,

[93] See also his remarks in *Review: The Magazine for Hospital Management* (April 1977) on the necessity of the government involving itself in covering the health expenses of the needy.

[94] For Friedman's statements of this recommendation, see the references in the discussion of this matter in Chapter 10 above as well as *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," May 19, 1978 (pp. 20–21 of transcript) and Friedman (1977i, pp. 55, 56) (also in Friedman, 1991a, pp. 160, 161).

[95] On the latter point, see his remarks in Friedman (1982f, p. 47).

[96] In the same vein, Friedman much later maintained (*Wall Street Journal*, April 17, 1996): "For the first 30 years of my life, until World War II, that kind of practice [a decentralized approach to medical care] was the norm. Individuals were responsible for their own medical care. They could pay for it out-of-pocket or they could buy insurance. 'Sliding scale' fees plus professional ethics assured that the poor got care."

[97] In addition to the Friedman remarks during the 1970s referenced in the text, see his observations on the matter in *Donahue*, NBC, September 6, 1979.

July 29, 1970, p. 4). In a television appearance in early 1972, Friedman cited a metric that would, by the 1990s, be central in the U.S. national debate on medical services: the share of medical spending in national income (*Firing Line*, PBS, January 5, 1972, p. 12 of transcript). He gave this share as having risen from below 5 percent to about 10 percent. In a later accounting (*Newsweek*, April 21, 1975), he gave it as 8 percent. Both numbers were in the right ballpark, as indicated by Hall and Jones' (2007, p. 39) summary: "The share was 5.2 percent in 1950, 9.4 percent in 1975, and 15.4 percent in 2000."[98]

As was the case with many others' discussions of the rise in this share, Friedman's analyses expressed doubt whether the additional devotion of resources to medicine had been manifested in comparable additions to healthcare services: "There's some improvement, of course," he remarked in early 1972, "but there's not been anything like a doubling. Most of it has just gone into bidding up the prices."[99] Friedman similarly assessed three years later: "Higher government spending has mostly gone to raise the incomes of physicians and other healthcare personnel, pay for the duplication of expensive equipment, and support other forms of waste." (*Newsweek*, April 21, 1975.) And he maintained in *Newsweek*, February 9, 1976, with regard to the increase in the federal government's involvement in healthcare over the prior decade that, in aggregate, its "major effect has been to raise the cost of medical care without improving its quality."[100]

In his assessment, Medicare and Medicaid unambiguously made demand for health services higher than previously: "medicine appears to the user to be a 'free' good," price ceased to be a limit on quantity demanded (*Newsweek*, April 21, 1975). And, consistent with his negative verdict on the supply response over this period, for the ultimate payers, as opposed to the users, of health services, prices paid had risen sharply: "You create a demand that didn't exist before… it's no wonder the cost of care keeps spiraling upward." (*Medical Economics*, April 16, 1973, p. 119.) Friedman also saw the change as introducing more inertia on the supply side, so that

[98] As well as using later vintages of data than those Friedman was employing, Hall and Jones had as the denominator of their share nominal GDP, a somewhat larger total than the nominal-national-income concept that Friedman often used.

[99] *Firing Line*, PBS, January 5, 1972, p. 12 of transcript. Friedman had remarked earlier in the decade that "the [U.S.] government, by its Medicare and Medicaid expenditures, has increased the gross income of the physicians and of the hospitals, but it is very hard to see that it has provided any additional medical service." (*Chicago Daily News*, July 28, 1970, p. 4.)

[100] Likewise, in the filmed portion of *Free To Choose*, PBS, episode 10, "How To Stay Free," March 14, 1980, Friedman remarked (p. 4 of transcript) of the federal government's Department of Health, Education and Welfare (whose health and welfare arms became the Department of Health and Human Services in 1979) that it had been "spending increasing amounts of our money each year on health. One effect is simply to raise the fees and prices for medical and hospital services without a corresponding improvement in the quality of medical care that we receive." See also Friedman and Friedman (1980 pp. 96–97, 113, 126).

effects on demand and supply had jointly produced "rising costs of medical care—as government has expanded its role and bureaucracy has multiplied." (*Newsweek*, November 7, 1977.)

Friedman did not deny that, separate from the expenditures being paid for by the public sector, demand for medical services financed by private-sector payments was also a major factor producing the rise in costs.[101] And, as discussed below, would adopt a more negative attitude toward the United States' private health insurance arrangements, which had mediated much of these payments, over time.

Friedman also realized that, notwithstanding his criticism, Medicare and Medicaid were likely to stay: the move to these measures "has been politically very popular and very hard to resist." (*Firing Line*, PBS, January 5, 1972, p. 12 of transcript.)

Martin Feldstein's research on healthcare costs

Friedman's discussions quoted above of a medical-costs/federal-intervention linkage stemmed from his observations as a public-policy analyst and from his exposure to the work of others. It was not based on a body of formal research on his own part: in contrast to the 1930s and 1940s, in the 1970s and 1980s Friedman was no longer involved in economic research on medical services.[102] This field was, however, becoming very active, with Feldstein (1995, p. 28) noting in a retrospective that "it has only been in the past three decades that health economics has attracted a substantial number of economists using modern tools of economic analysis."

With regard to Milton Friedman and the economic analysis of U.S. public policy, Feldstein would observe: "I think he was just enormously influential. Certainly, the work on monetary policy, but more generally, just his getting people to question the role of the government in all of the tax and social-insurance areas. So even if, in my judgment, he went quote, 'too far,' I think it had the effect of shaking up people's thinking." (Martin Feldstein, interview, November 21,

[101] See Friedman and Friedman (1980, p. 113) and the account below.
[102] Friedman did indicate that, in the wake of his heart problems in 1972, he became a follower of medical matters in the area of cardiac treatment (see Nelson, 2020b, Chapter 15). But it is worth noting that, notwithstanding this change in interest, Friedman did not follow the medical journals closely enough to know that an article critical of universal health insurance article had been published in the *Annals of Medicine* by Gunnar Biörck (1977). Instead, in 1978 and later, Friedman used the manuscript version of the Biörck piece, which had been presented at the University of Chicago in 1976 (*Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978, p. 8 of transcript) and described it even in 1979 as an "unpublished paper" (Friedman and Friedman, 1980, p. 317).

2013.) Within social insurance policy, however, Friedman's interventions on healthcare policy before 1970 had been sparing. With *Capitalism and Friedman* having been completed a few years before the introduction of Medicare and Medicaid, Friedman's engagement on the matter of government healthcare insurance had been limited, as indicated above, and frequently concerned with the U.K. case. By the time he became more outspoken, in his public-policy commentaries in the 1970s on actual and prospective U.S. government healthcare programs, others in the economics profession had made medical insurance a lively area of research activity.

Martin Feldstein was a leading figure in this activity. Feldstein's large outpouring of research on U.S. social insurance policies included, during the 1970s in particular, a series of theoretical and empirical studies of the U.S. healthcare system.[103] The topics that Feldstein considered included the effects of Medicare and Medicaid on U.S. healthcare costs, and they largely supported Friedman's inferences.

Although he himself produced early studies in the area, including Feldstein (1970, 1971a, 1971b), Feldstein's assessment at the end of 1972, in an appraisal he gave of "Econometric Studies of Health Economics," was: "The two major programs of public insurance, Medicare (for the aged) and Medicaid (for the 'medically indigent') have received very little quantitative economic analysis."[104] But, in a paper prepared for a June 1974 NBER conference on the economic analysis of U.S. health insurance, Feldstein and Bernard (Barry) Friedman, a former research assistant of Feldstein's who had specialized in health economics since graduate study at MIT, did examine this issue. The Feldstein-Barry Friedman study "The Effect of National Health Insurance on the Price and Quantity of Medical Care," presented simulation analysis, rather than econometric estimates. But their judgment on the empirical record regarding medical costs was quite similar to what Milton Friedman had articulated in his media discussions. Feldstein and B. Friedman (1976, pp. 524–525) summed matters up as follows: "An analysis of hospitals' response to the growth of private insurance in the 1960s and to the introduction of Medicare and Medicaid suggests that hospitals respond to insurance by increasing the cost per patient day through more sophisticated care and higher staff wages."

[103] In the 1960s, he had worked in this area but was then concentrating on the U.K. healthcare system. See Feldstein's retrospectives in Feldstein (1995, p. 31), in Poterba (2003, p. 296), and in *The Region* (Federal Reserve Bank of Minneapolis), September 2006.

[104] Feldstein (1974b, p. 423). Feldstein's appraisal was written as his contribution to a set of papers on the state of quantitative economics delivered at the Econometric Society meetings in Toronto, December 28–30, 1972. These meetings were held in conjunction with the meetings (December 27–30, 1972), also in Toronto, of the American Economic Association—meetings that Friedman missed because he was convalescing after surgery.

As these remarks indicated, Feldstein and Barry Friedman certainly cited the strong upward trend in private insurance as an important driver of higher costs. This trend had set in before the start of the new federal programs in 1965−1966, and so had the steep rise in U.S. medical costs—with Feldstein (1977b, p. 1681) noting that hospital costs had increased 600 percent since the late 1950s compared with a doubling of the general price level. But the importance of Medicare and Medicaid as a driving force of cost increases, as well as the observation that sharp price increases had tended to be a very sizable part of the medical industry's reaction to the previous decade's rising demand for medical services, had been confirmed.

The national health insurance push in the 1970s

Feldstein (1977b, p. 1699) observed that the backlash against the "rapid increase in hospital costs" was generating further pressure for "increased public insurance." Friedman himself referred, in a talk given on October 13, 1977, to "the drive for national health insurance" in the United States.[105] In the mid-1970s, both Friedman and Feldstein had seen the adoption by the federal government of national health insurance as likely to take place during the coming few years.[106] They held this expectation despite the fact that each of them opposed national health insurance and believed, furthermore, that there was not a true popular majority in favor of adopting that system.[107] For his part, Friedman believed that supporters of national health insurance might be able to get their package passed into law. It might be possible to assemble a political coalition sizable enough to secure passage, he believed, despite the absence of a wide consensus in favor of a universal-payer system, by capitalizing on the growing dissatisfaction with existing arrangements. Friedman implied also that the unpopularity of the present healthcare system and a fear of antagonizing national politicians had, in the course of the 1970s,

[105] *Milton Friedman Speaks*, Episode 2, "Myths That Conceal Reality," taped October 13, 1977, p. 15 of transcript.

[106] Friedman's statement in late 1974 that he expected this move was noted in Chapter 10 above. Similarly, Feldstein (1973, p. 251) wrote: "Some form of national health insurance is very likely to be enacted within the next few years."

[107] In *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978 (p. 16 of transcript), Friedman remarked: "in my opinion, there is no widespread public pressure for national health insurance." He made a similar observation in *Milton Friedman Speaks*, Episode 2, "Myths That Conceal Reality," taped October 13, 1977 (p. 15 of transcript). With regard to Feldstein, Paul Samuelson (Instructional Dynamics Economics Cassette [Paul Samuelson series] Tape 200, mid-June 1976) stated: "Martin Feldstein is an expert on medical economics... and he's very much concerned with, and rather against, the kind of position that Senator Edward Kennedy represents in the public-health field. He's afraid of compulsory medical insurance for the American people, Martin Feldstein is, because he thinks it'll run to 70 or 80 billion dollars. It's like adding a third or a quarter to the already existing [federal government] budget… [and] he doesn't think [that] this is what the American people really want but [believes that] there's a danger, under populist democracy, that this is what they will get."

led doctors to be reticent in articulating opposition to the idea of national health insurance. As of late 1972, he took it as commonplace that "physicians say they are" strongly opposed to national health insurance (*Medical Economics*, April 16, 1973, p. 119). But, by 1977, Friedman sensed a reluctance on the part of leaders of the medical profession—including professors of medicine who were at the University of Chicago—to speak out against the idea.[108]

In contrast to what he described as the "diminishing resistance by the medical profession," Friedman in the 1970s remained, as he had been in the 1950s, strongly against the introduction of national health insurance.[109] His objections began with the name. He continued to use the now unambiguously pejorative label "socialized medicine"—with the Friedmans, on grounds discussed below, arguing that this was the "proper name" to apply to the scheme.[110] The label "national health" ran against Friedman's sense that the health matters in question that would be added to the government's functions by a single-payer system would primarily consist of individuals' ailments, as opposed to the aforementioned public-health responsibilities of government that he accepted.[111] Furthermore, he did not believe that a government-payer system really had the features of an insurance policy.[112] Rather, he believed that the appropriate conclusion was that "there is no insurance about it, because it's simply government subsidization."[113]

Beyond the objection to the label, Friedman in his 1970s interventions on health policy, and with Rose Friedman in the *Free To Choose* book, opposed national health insurance on the basis that it would lead to a less efficient medical system. Using the U.K. example as a precedent, he argued it amounted to *de facto* nationalization of health provision. Even though proposals for national health insurance typically couched it as a change in the arrangement for the payment of patients' medical bills, Friedman saw in the government as, in practice, being liable to become not just the payer with regard to healthcare services but also the controller-setter of medical fees.

[108] See Friedman (1978b, p. 5; 1979, p. 12 of 1987 reprint). Both of these talks were delivered in 1977.
[109] The quotation is from *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978 (p. 17 of transcript).
[110] Friedman and Friedman (1980, p. 113). See also *Milton Friedman Speaks*, Episode 2, "Myths That Conceal Reality," taped October 13, 1977 (p. 15 of transcript) and *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978 (p. 16 of transcript) for related criticisms of the "national health insurance" label. Friedman's usage of the term "socialized medicine" during the 1970s included in *Medical Economics* (April 16, 1973, p. 119) and in Friedman (1978b, p. 5).
[111] *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978, pp. 16–17 of transcript.
[112] Friedman and Friedman (1980, p. 113).
[113] *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978, p. 17 of transcript.

The U.K. government, in fact, made no bones about this feature of the system, with the Secretary of State for Health and Social Services, Barbara Castle, remarking in late 1975 that the National Health Service was "a system financed and therefore regulated by the State."[114]

With regard both to full-blown universal health insurance and the smaller-scale U.S. government interventions of Medicare and Medicaid, Friedman acknowledged that there was justification in the public sector becoming involved in the oversight of and containment of costs of medical services, in view of the fact that it had already decided to pay the bills for many of those services.[115]

In the case of a universal-payer system, it was inevitable that the government had, in addition, a dominant role in establishing the total amount of medical services demanded. This, in effect, made the public sector able to determine the equilibrium quantity of services supplied and therefore, put it "in a monopoly position in hiring physicians," as Friedman would describe the situation.[116] In practice, he saw this command over the amounts supplied as being accompanied by direct government management of the medical industry's supply function, especially in a "completely socialized" version like the U.K. system: "It's a program for making physicians government employees."[117] These direct supply interventions would, Friedman believed, not only involve increased regulation of medical services but also lead to widespread government ownership of the supply side, as was the case in the U.K. hospital system (*Newsweek*, April 21, 1975).

Via their effects on the supply function, Friedman saw the increased interventions as having adverse effects on the efficiency and dynamism of the medical industry. In particular, once the government became the predominant source of expenditures on healthcare services, that spending was likely to become constrained by fiscal policy considerations. Competition among government departments for spending allocations would consequently limit additions to appropriations in the area of medical care. And, he suggested, it would now tend to be the case

[114] Quoted in Seldon (1976, p. 47). As indicated earlier, in the U.K. context "the State" was the national government.

[115] *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978 (p. 1 of transcript) and Friedman and Friedman (1980, p. 113). In practice, he saw these regulations as having amounted to "a futile attempt to hold down costs" (*Wall Street Journal*, April 17, 1996).

[116] Friedman (2001, p. 17).

[117] The quotations are respectively from *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978 (p. 1 of transcript) and *Milton Friedman Speaks*, Episode 2, "Myths That Conceal Reality," taped October 13, 1977 (p. 15 of transcript).

that those resources that would be put into the healthcare system would generate disappointing results in terms of output. Drawing in particular on a study made by a U.K. doctor, Max Gannon, of the operation of the NHS in the 1970s, Friedman emphasized that there had been a magnification of the medical bureaucracy, with a major diversion of resources toward paperwork and administrative staff having the result that appreciable increases in the medical sector's inputs gave rise to meager additional quantities of productive output.[118] The fact that the government was merely the payer for—not the ultimate recipient of—medical care also mattered materially, because the payments on healthcare did little to coordinate demand and supply. In the context of limited output of medical services and patients' demand for those services not being restricted by a price system, Friedman declared, "government guidelines" would tend to decide "who gets medical care and who goes to the end of the queue." (*Newsweek*, April 21, 1975.)[119]

In making these criticisms of universal-payer healthcare arrangements, Friedman pointed to the U.K. system as "a premonition of what would happen here." But he also suggested that "our own experience" in the United States was relevant for judging the merits of public-sector intervention, in view of the fact that the government's role in the U.S. healthcare system had increased since the mid-1960s (*Newsweek*, April 21, 1975). In particular, Friedman questioned the logic of suggestions that developments in the United States pointed to the need for a national healthcare system: "much pressure for national health insurance reflects the perceived failure of Medicare and Medicaid," he observed—referring particularly to recent years' increases in costs—but the proposed solution prevalent in public debate was "a still larger program" consisting of a single-payer system (*Newsweek*, April 21, 1975).[120]

Posing the problem in this way made it look like there was a basic inconsistency in the arguments made by those advocating universal health insurance. But, in the wake of the cost rise from the mid-1960s to the mid-1970s, there was, in fact, a logic inherent in the recommendation of moving from an arrangement in which the government paid the bills incurred by a segment of the population to a government-as-only-payer system. The trace of an acknowledgment of the

[118] See Gammon (1976) and Friedman's drawing on this study in *Newsweek*, November 7, 1977, in *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978 (p. 6 of transcript), and in Friedman and Friedman (1980, p. 114, 317; see also their pp. 155, 319).

[119] Martin Feldstein—who, as noted above, had made the NHS a topic of their own research—would reach this conclusion, too. For example, Feldstein (1995, p. 31) observed that "missing [from]… the British healthcare system… was any way for the total level of healthcare spending and its allocation among different kinds of services to respond to the preferences of the British people. Since all healthcare was free, there was always excess demand and rationing by queues."

[120] See also Friedman (1975d, p. 16).

economic case for the single-payer system appeared in Friedman's observation (in Vaizey, 1975, p. 74): "The compulsory provision of health care may be a good thing, though I rather think it is not." With greater specificity, the Friedmans briefly acknowledged in their *Free To Choose* book noted that one of the "major arguments" made by national-health-insurance proponents was that it would "reduce costs."[121] In particular, having the government as a universal payer offered the prospect of conferring to the public sector monopsonistic power with which it could be a source of restraint on the equilibrium price of health services—whereas when it was just one of many purchasers (as in the U.S. system), it lacked this power. The point regarding monopsony was granted by Friedman in his 2001 piece on healthcare, when he acknowledged that a "mixed system" such as that of the United States had generated higher medical costs than a "wholly collective" system.[122] Here, too, however, he reaffirmed his view expressed in the 1970s and 1980s that a national health insurance system worsened the productivity and flexibility of the medical system.[123]

But, as already indicated, in the first half of the 1970s Friedman regarded the introduction of national health insurance, his opposition notwithstanding, as likely to occur in the decade. This possibility was enhanced, as he saw it, by the increase in funds readily available to the federal government occurring due to the rise in the tax-revenue share seen in the 1970s: in October 1972, for example, he mentioned "socialized medicine" as something quite likely to result from federal revenue inflows (Selden, 1975, p. 195). In the event, however, increased pressure, from 1975 onward, for economies in the federal budget created political pressures in the opposite direction, and in May 1978 Friedman judged that those pressures would hold off the prospect of the introduction of national health insurance under President Carter.[124]

Significantly, this was a judgment on Friedman's part about what the outcome of a debate on a federal program would be—and not a belief that the push for a national insurance program would imminently recede. Indeed, the nomination challenge that President Carter was set to receive within his own party enhanced the political prominence of healthcare policy during 1979. In particular, as discussed in Chapter 10 above, in 1979 Senator Teddy Kennedy offered a national healthcare system. Writing during this year, the Friedmans saw President Carter as having been

[121] Friedman and Friedman (1980, p. 115).

[122] Friedman (2001, p. 18). See also page 17 of the same article.

[123] See Friedman (2001, p. 11), whose discussion of government as a source of higher medical costs through supply-side effects echoed earlier ones in *Newsweek*, November 7, 1977, and in Friedman and Friedman (1980, p. 115).

[124] *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978 (pp. 15–16 of transcript).

pressured to offer a health plan that introduced national health insurance "in a limited form," while Kennedy offered a full-fledged universal-payer system. The Friedmans indicated that they did not like either plan, but they conceded that the trend of current policy developments seemed to be making it likely that the next few years would see the introduction of some kind of a universal-health-insurance scheme in the United States.[125]

The healthcare-spending share in the United States

The Reagan years—which would be frequently remarked on by Friedman as notable for the absence of a new major new domestic federal spending program—featured a stalling in the progress of the national health insurance campaign in the United States. But, because the upward trend in the share of medical spending in U.S. aggregate income continued, the same period saw the conditions develop for healthcare to figure heavily in political debate over the first half of the 1990s.

Even during the 1970s, the rise in this share had divided economists. Various analysts offered different interpretations of the development, with some seeing it as a manifestation of consumer sovereignty and others seeing it as an instance of market failure. Friedman himself, as discussed below, had a mixed reaction to the increase in this share.

Over several decades, it remained the case that those who saw the rise in largely benevolent terms attributed it to an above-unity income elasticity of consumer demand for health services, as well as the interaction of demand with advances in medical technology. A formalization of this argument appeared in print a few months after Friedman's death in an article in the *Quarterly Journal of Economics* by Robert Hall and Charles Jones. Hall and Jones (2007, p. 40) concluded: "Standard preferences imply that health is a superior good with an income elasticity well above one. As people grow richer, consumption rises but they devote an increasing share of resources to health care." The same reasoning underlay their suggestion that the share might reach 30 percent by 2050.

The Hall-Jones interpretation of the spending share was endorsed in the U.S. media by Gregory Mankiw (*New York Times*, November 4, 2007). Mankiw's commentary, in turn, generated an adverse reaction on the part of those who saw his position as an attempt to discourage major U.S.

[125] Friedman and Friedman (1980, pp. 112–113; quotation from page 112).

federal government reform of healthcare-payment arrangements.

Among economists, however, it is not the case that the interpretation of the behavior of the healthcare spending share is something that lends itself easily to the demarcation of economists along free-market-supporting-versus-interventionist lines. Notably, Robert Solow has long been associated with the position expounded by Hall, Jones, and Mankiw. Solow (1972, pp. 1–2) suggested: “We don’t find it surprising that as a society we spend more on health care every year, and even a larger fraction of our aggregate income on health care… That’s not surprising because we expect a family or a nation to spend a larger fraction of its income on health and education, and a smaller fraction on food and clothing and shelter, as the family or nation gets richer.”

More than forty years on, Solow emphatically reaffirmed his 1972 position: “as I think now about healthcare costs, I would certainly agree with what I said then—that it’s ridiculous for observers to say the American people spend too much on healthcare. Just as it would be egregious to say they spend too much on movies, or television sets. The issue about healthcare, it seems to me, is whether all of that expenditure is, in fact, buying what the public thinks its buying. The problem with the American healthcare system is not that it spends too much, but it doesn’t buy enough health for what it spends. But healthcare is a service, it’s a luxury, in the sense that the income elasticity is probably pretty high. And I don’t see that an observer is entitled to moralize about whether a given nation, or segment of it, likes healthcare more than the observer thinks they ought to like healthcare.” Solow went on to underline his view that the “problem with the system is what it delivers, how much—what all of that expenditure is buying.” (Robert Solow, interview, July 7, 2014.)

The contest between interpretations of the U.S. medical-spending share—as a desirable equilibrium outcome or as a failure of the healthcare system—was also manifested in Milton Friedman’s fluctuating perspective over time regarding the share. By the 1990s, as will be seen, he had decided that policy changes were needed to lower the share. But, as of the late 1970s, Friedman had a mostly favorable interpretation of the rise. Certainly, he had, as indicated above, seen Medicare and Medicaid as factors boosting the share and straining healthcare resources. And he reaffirmed this position in subsequent years, by contending that the demand had overwhelmed additions to supply, merely raising the relative price of health products. Nevertheless, Friedman’s posture as of 1978—conveyed in his talk in May of that year to an audience of medical specialists at the Mayo Clinic—entailed a mostly benign verdict with regard to the overall rise in the medical-spending share. He cited an income elasticity above unity as the basic catalyst for the increase: "private expenditure on medical care has been growing as a percent of income as people have become more affluent and have wanted to spend more of their money [sic; income] on medical care."[126] Against the background of medical services being a luxury good, he saw the rising share as reflecting the interaction of consumer sovereignty and the development of new medical technology: "The major reason [for rising expenditures] is an increase in the number and the variety and the complexity of the procedures that are being used, of the tests that are being made, of the services that are being rendered to the American citizen…. [T]he public at large has wanted to buy more medical care, and they have been—[and] the market has been responding to their demands."[127]

In the same address, Friedman acknowledged that the medium through which this higher private-sector spending on health had taken place had, to a considerable degree, been private health insurance arrangements. All told, including Medicare and Medicaid, the Friedmans in *Free To Choose* reported, "90 percent of all hospital bills are paid through third-party payments."[128] And the picture Friedman gave of this development was predominantly favorable. In the radio discussion of healthcare in which he participated in May 1955—prior to the big increase in Americans' usage of medical insurance—he described himself as an outsider to the issue but indicated: "My own view that individuals ought to be free to make the particular arrangements to suit them best."[129] If individuals wanted to purchase insurance from a private insurance company they should be allowed to do so, but they should also be allowed the option to "insure themselves"—by which he meant a family having no health insurance plan at all and, instead, treating medical problems as one contingency for which to accumulate savings.

By the mid-1970s, however, Friedman had come to the view that the purchase of private health insurance as the most advisable method of protection against medical problems: it was a "highly desirable thing" that there were such insurance schemes, he remarked (Vaizey, 1975, p. 74), and he noted (*Medical Economics*, April 16, 1973, p. 119): "The record amount of voluntary

[126] *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978, p. 11 of transcript.
[127] *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978, p. 9 of transcript. As a related matter, Friedman regarded the share of health spending in the United Kingdom as being held down below what it would be in that country if U.K. consumers' preferences had been allowed to have a more direct bearing on outcomes. (*Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," May 19, 1978, p. 5 of transcript.)
[128] Friedman and Friedman (1980, p. 15).
[129] NBC (1955, pp. 1, 4).

purchases of private health insurance—like my group policy—shows Americans will protect themselves if given half a chance." In his 1978 discussion, he correspondingly referred to "one of the great American innovations: the spread of voluntary, private, hospital-and-health insurance among the population at large."[130]

Friedman's 1978 discussion did, however, include a reservation: "I have no doubt that those insurance arrangements could be improved." He listed his preferred changes as including a greater concentration on catastrophic expenses, as well as more user-pays elements in the form of "larger deductibles, more coinsurance arrangements, and other devices." Friedman implied that U.S. private-sector insurance methods would move in this direction. The fact they did not, and that an overt price system hardly emerged in the U.S. healthcare system, would lead over time to his becoming disillusioned with that system and to his advocacy of a replacement.

As ofthe late 1970s, however, Friedman was, as just indicated, largely pleased with private-sector insurance schemes, and their existence and the large subscription to them by Americans underlay the Friedmans' conclusion: "the costs of ordinary medical care are well within the means of most American families."[131] Indeed, with regard to the insured population to which this comment applied, Feldstein and Amy Taylor (1977, p. 2) suggested that "the net-of-insurance cost of hospital care has remained essentially unchanged in real terms" since the 1950s.[132] This judgment, made on the basis of data through late 1976, accorded broadly with Friedman's contention that it was wrong to suggest that "there has somehow been a terrible rise in hospital costs which the American people… cannot afford."[133] As a characterization of who was bearing directly the costs of higher medical services, Feldstein and Amy Taylor's statement would become less true after the 1970s: patients would experience notable real increases in the medical expenses they had to pay, net of insurance.[134] These rising expenses, together with increased costs of purchasing insurance, would provide momentum for healthcare to reemerge as a leading national political issue in the 1990s and subsequently.

Even ahead of this revival of healthcare's political prominence, Friedman had shown signs in his public statements that he was backtracking from his 1978–1980 circumspect position on the rise

[130] *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978, p. 9 of transcript.
[131] Friedman and Friedman (1980, p. 115).
[132] Feldstein (1981c, p. 5) included the important qualification: "In the end, consumers do pay for it—in ever-higher insurance premiums."
[133] *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978, p. 5 of transcript.
[134] Chandra, Flack, and Obermeyer (2021, p. 1) noted: "As insurers place more emphasis on cost-sharing via deductibles, coinsurance, and copayments, patients pay more out-of-pocket for health care."

in healthcare's spending share. In a March 1982 interview on Canadian radio, he observed: "In the United States for many years, spending on medical care was about 4 percent of the national income. It has now gone up to 10 percent. That increase from 4 percent to 10 percent is almost entirely explained by increasing government spending on Medicare and Medicaid."[135] In light of his earlier granting of the point that the growth of private insurance was a major driver, he probably did not actually mean that the rise in the share was due to the new programs. But it was clear that Friedman had abandoned the bright perspective that he had once articulated on the *fact* of the rising healthcare spending share. In important respects, this 1982 statement foreshadowed the perspective that he would take in his activity on healthcare a decade later: that the United States' healthcare-spending share had become excessively high and that the particular ways in which the government subsidized or paid for healthcare arrangements were contributing importantly to the magnitude of the share.

Friedman embraces an insurance mandate

In 1991, Friedman noted the "recent surge in concern about the rising cost of medical care" (*Wall Street Journal*, November 12, 1991). The long op-ed on healthcare in which this remark appeared, as well as his subsequent work (which included an expanded version of the op-ed, released in 1992 as a Hoover Institution booklet), made it evident that he believed that this heightened concern was justified.[136]

Friedman's policy position had also undergone a very notable shift. From the 1950s to the 1980s, his position about the public sector's healthcare responsibilities had changed little: over that period, he had maintained his belief that—cases of the needy, which did justify a public-sector role, aside—individuals should make their own arrangements in meeting expenses. And, although by the mid- to late 1970s, he looked favorably on private health insurance schemes as a means of meeting these expenses, he believed that individuals should not be required to be part of an insurance scheme.[137] In contrast, the early 1990s would see Friedman both less happy about the existing private-sector component of the American healthcare system and in favor of a new element of U.S. government intervention that was not part of the present system. He had come to the conclusion that healthcare, along with education, constituted one of the "areas in

[135] Friedman (1982f, p. 48).
[136] See Friedman (1992e) for the booklet version.
[137] See the items noted above as well as Friedman's remarks in *Review: The Magazine for Hospital Management*, April 1977.

which we obviously need to do something" (*Investor's Business Daily*, April 15, 2004, p. A8). This later period would see Friedman express a much more jaded position about existing private insurance arrangements and also champion a policy proposal that entailed a degree of universal mandatory insurance.

In the 1990s and 2000s, although Friedman characterized the investigation in which he was engaged as one pertaining to the input and output of medical care, his analysis hinged on the demand side, which he treated as the "input." Although Friedman continued to grant the fact of major technological improvements in medical care, he now contended that the increase in input, in terms of higher spending on the U.S. medical sector over the past quarter century, had nevertheless given rise to a disappointingly small amount of extra output of medical services.

Friedman's analysis of the demand for healthcare led him to recommendations that were a break from the *laissez-faire* positions on medical insurance that he had expressed in earlier decades.

On the one hand, the gravity of the burden of healthcare on the U.S. economy had pressed itself on him. The growth in medical spending had exceeded what Friedman was comfortable with and, in contrast to his relatively sanguine position through about 1980, he no longer stressed growing household income, and the associated appeal of new medical products to households, as the primary force behind the increase. Reflecting his latter-day perspective, on television in early 1992 Friedman cited the rise in the share of medical spending in national income from "4 percent" to "12, 13, 14 percent," in stating his judgment, "We do have a crisis in medical care."[138] In the same appearance, Friedman noted that he had developed his own reform proposal. "And your program isn't long enough, I'm afraid, for me to explain that in detail, but I did write it up in an article that appeared in the *Wall Street Journal*."[139]

That article (the aforementioned piece of November 12, 1991) showed that Friedman had come to a critical view of the effects of both public-sector and private-sector insurance on healthcare costs in the U.S. economy.[140] Both sets of insurance schemes, he suggested, separated the patient from the price system. Friedman later summarized his latter-day analysis as having identified "the major reason health care costs have risen from 5 percent of GNP to 14 percent:

[138] *Wall Street Week,* Maryland Public Television, February 21, 1992 (pp. 8, 9 of transcript).

[139] *Wall Street Week,* Maryland Public Television, February 21, 1992 (p. 8 of transcript).

[140] As the private-sector insurance schemes were encouraged by the tax system, Friedman was reluctant to consider expenditures associated with them as wholly private-sector outlays. Friedman (2004) implied that they could be considered, to some extent, "indirect [government] spending [generated] through tax subsidies."

Few consumers of health care now pay for their care directly, and no one spends someone else's money—the employer's, the insurance company's, or the government's—as carefully as he or she spends his or her own." (*Wall Street Journal*, February 9, 1994.)[141]

The most familiar aspect of Friedman's critique was his negative verdict on the public-insurance or payment programs, Medicare and Medicaid. As in the 1970s, he judged that these programs had added considerably to the healthcare spending share in the United States. "In the area of health, we used to spend about four percent of the national income on medical care. Now it's thirteen percent, and half of the increase is simply because of government spending."[142] As he had previously, Friedman viewed a large amount of spending as having resulted in higher factor prices, including higher incomes of healthcare personnel (*Frisko* magazine, 1992, p. 68) and also blamed these programs for having helped generate "bureaucratic structures" that raised medical costs (*Wall Street Journal*, November 12, 1991).

But in contrast to his mostly favorable remarks in the 1970s on the spread of private health insurance, Friedman now attached considerable blame to health-insurance schemes as a factor raising costs. Edward C. Prescott observed (interview, February 16, 2016): "Friedman says the problem with the whole healthcare system is the third party's payment. Empirically, he appears to be right. It's so costly. So many resources get used up. And that's a big sector of the economy. A lot of resources are used up in providing insurance."

As a factor separating patients from the cost of healthcare, Friedman viewed the prevalence of employer-provided healthcare as a major source of difficulty. He had actually had longstanding reservations about the employer being the provider of more than just a regular wage to employees. These reservations partly reflected his philosophical aversion to anything that could be perceived as paternalism. Ever since the 1940s, Friedman had viewed employer-provided outside-work amenities and other products to their staff as creating a paternalistic relationship between the employer and the employee (*Wall Street Journal*, February 3, 1993).

Friedman's objections to employer-provided healthcare also had more specific economic dimensions. For one thing, he was concerned about devices beyond the employment relationship that tended to forge a link the employer to the employee. These included, alongside employer-

[141] Similar sentiments have been expressed in John Cochrane's writings on healthcare reform, including Cochrane (2014) and his op-eds in the *Wall Street Journal* of December 26, 2013, and July 30, 2018.
[142] *Frisko* magazine, 1992, p. 68.

provided services, measures like some of firms' compensation to employees partly taking the form of issuance of shares in the firm. Such linkages between the employee and their workplace, Friedman believed, discouraged labor market mobility—and a reduced degree of labor mobility, he believed, was a source of economic inefficiency.[143]

With regard to the market for health specifically, the analysis that Friedman conducted in the 1990s concluded that employer-provided services were undesirable because they separated employees from the selection of insurer: "it leads the employee to rely on the employer… to finance and provide medical care," ceding the "job of monitoring healthcare providers."[144] This delegation, Friedman believed, had hindered competition in healthcare payment arrangements and had contributed to the spiraling of U.S. medical costs.[145] Furthermore, he contended that although employer-provided healthcare insurance was a commonplace arrangement in the private sector, this outcome had been encouraged by the government through the tax deductibility of employers' provision of healthcare insurance to their employees.[146] Friedman therefore proposed the abolition of the tax exemption of those insurance costs.[147] This step, he contended, would

[143] See Instructional Dynamics Economics Cassette Tape 118 (April 13, 1973) and Friedman (1977b, p. 459).

[144] Friedman (1992e, p. 14).

[145] The notion that the private-sector aspects of the U.S. system did not generate a price system comparable to that seen in other goods markets was also stressed over the years by many other free-market-oriented economists, including Cochrane (2014, p. 19), who argued that "at the margin, the consumer needs to be paying a lot closer to full marginal cost of health care." Similarly, Feldstein (1995, p. 31), while suggesting that the U.K. healthcare system "lacked a price mechanism" that could accurately relay individuals' preferences, strongly implied that the U.S. system was subject to a similar criticism. Consistent with this, Feldstein (1996, p. 136) argued that the U.S. insurance system "distorts the decisions of patients and their doctors at the time of care, inducing a consumption of medical care that patients and doctors value at far less than its cost of production."

[146] See Friedman's remarks in the *Wall Street Journal* editions of November 12, 1991, February 3, 1993, and April 17, 1996, and in Friedman (1992e, pp. 13–14; 2001, pp. 7, 8, 10, 17). Friedman also believed that wage controls in World War II had encouraged the spread of employer-provided healthcare insurance because such insurance provided an unregulated means by which employers could top up workers' incomes (*Wall Street Journal*, February 3, 1993). This interpretation was widely shared (see, for example, Aaron, 1996, p. 113), but in Friedman's case there was a firsthand dimension: As he had been a U.S. Treasury economist in the early 1940s, he accepted some of the blame for the arrangement. Even though he was not himself involved in the details of the setup of the controls, he believed that his opposition to wage and price controls during that period had left an imprint on the controls' formulation. Friedman told his friend Thomas Jordan that, once the government had decided to impose mandatory controls on wages and prices, it was regrettable that employer-provided healthcare benefits had not been part of the wage control package. "And one thing he told me [was]: 'Tom, the biggest mistake I ever made was when I was in Washington during the period of [wartime] price control. I helped make healthcare not a part of price control, so that you couldn't raise someone's salary, but you could give them a healthcare policy.'" In one of Friedman's numerous flourishes about his past mistakes, "He said, 'That was the biggest mistake I ever made.'" (Thomas Jordan, interview, June 24, 2013.)

[147] Although his own work put less stress than Friedman did on the fact that the insurance was employer-purchased as a factor, Martin Feldstein, too, had cited the tax deductibility of medical insurance as a distortion to the healthcare market. Feldstein (1981c, p. 306), for example, remarked: "The favorable tax treatment of health insurance purchases has grown over time to become a $10 billion inducement to overinsurance. The resulting excess insurance has… driven up the cost of care."

encourage employees to act more vigorously on their own behalf for healthcare insurance and put downward pressure on medical costs.

Another part of Friedman's reform package represented a bigger change in his position from that of earlier decades: "a requirement that every U.S. family unit have a major medical insurance policy" (*Wall Street Journal,* November 12, 1991). He repeated this prescription in later years. Most notably, when writing on medical care in 2001, Friedman considered some other reform proposals but ended with his preference for the "more radical reform" combining the abolition of tax exemption of employer-provided insurance with a mandatory requirement that every U.S. family purchase medical insurance.[148] He stressed his judgment that this reform—which would imply very different arrangements both from the existing American system and from a national-health-insurance setup—would curb healthcare costs.

Friedman stipulated that the health insurance that he would require U.S. households to purchase would have a high deductible.[149] That element of his proposal allowed him to refer to his proposed scheme as one of "high-deductible catastrophic insurance" (*Wall Street Journal,* February 9, 1994).[150] He also emphasized the private-sector orientation of his proposals: he continued to believe that routine health expenses should be borne by the household, without third-party payment (for example, *San Jose Mercury* (California), November 5, 2006*a*), although he was also open, as a substitute for existing insurance packages, to "Medisave" accounts that households would open specifically for medical expenses *Wall Street Journal*, February 9, 1994) and that would be subject to favorable tax treatment comparable to that given to retirement accounts (*Wall Street Journal,* April 17, 1996).

But the shift from Friedman's initial position on the matter was profound. He was describing his position as one of "reprivatizing medical care."[151] His proposal did involve less government

[148] Friedman (2001, p. 21). Friedman's article appeared in the Winter 2001 issue of the public-policy journal, *Public Interest* (the same venue in which Feldstein had published some of his public-policy writings on medicine: see, for example, Feldstein, 1971c). It also appeared elsewhere, including in a condensed version (in paid advertising space, rather than a commissioned article) titled "Rethinking Health Insurance" in *The Economist* (London), June 23, 2001.
[149] See *Wall Street Journal*, November 12, 1991, and Friedman (1992e, p. 13; 2001, p. 21).
[150] This reference did, however, underline the change in his view from the 1970s. In *Playboy* (February 1973, p. 64; also in Friedman [1975a, p. 25; 1983a, p. 42]), he indicated that he regarded the purchase of health insurance covering regular medical treatment as something that should not be mandatory but that was likely prudent behavior. In the same remarks—mainly apparently referring to the costs of care, rather than their purchase of insurance—he opposed individuals' health costs being tax-exempt, although he did qualify this by expressing sympathy with the idea that the costs of catastrophic healthcare should be tax-deductible. It was therefore evident that Friedman, during the early to mid-1970s, did not believe that the purchase of any health insurance should be made mandatory.
[151] Friedman (1992e, p. 13). See also *Wall Street Journal*, November 12, 1991.

involvement in terms of government spending. But it was not reprivatizing in the sense of returning, for the population in general, to what he had favorably referred to as the "essentially voluntary private system" prevailing in, for example, the pre-1948 United Kingdom or the pre-1965 United States.[152] Rather, his proposal did have an element of universality and compulsion.

Even prior to Friedman's interventions, an insurance mandate had had some appeal to market-oriented economists as a move that addressed the same cost problem as that stressed by advocates of a single-payer system but that involved less government participation than a single-payer procedure would.[153]

Friedman himself had, however, been late in making such a break. The bottom line was that he had dropped the ostentatious *laissez-faire* stand that he had taken concerning medical insurance and payments that he had voiced in the 1950s and maintained into the 1980s. Friedman had come round to believing in the desirability of mandatory universal insurance, albeit not in the form of government-provided insurance or of all-expenses, comprehensive insurance.

The Clinton plan

Healthcare reform was a priority for the Clinton Administration when it took office in 1993. Friedman strongly opposed the reform proposals that the administration advanced in 1993–1994. He regarded them as featuring far too large a role for the public sector, and writing before the plans were essentially dropped in mid-1994, had referred to the "present discussion of a national program of health care" as an example of the still-keen interest in expanding government programs in the United States.[154]

"You can't think of a more socialist program than the healthcare program that he tried to get us to adopt," Friedman said of Bill Clinton a few months after the administration had let its proposals go into abeyance.[155] This characterization may seem surprising, in view of the fact that the administration's proposals stopped well short of proposing the single-payer or national insurance system prevailing in several other countries by the 1990s. But, as indicated earlier,

[152] *Milton Friedman Speaks*, Episode 10, "The Economics of Medical Care," taped May 19, 1978, p. 5 of transcript.
[153] For example, in a book issued by the free-market-oriented Institute for Contemporary Studies, Thomas Schelling (1976, p. 32) stated: "I reach the conclusion that at least some minimum level of universal mandatory medical-care insurance makes sense."
[154] Friedman (1994a, p. xvi).
[155] CSPAN, November 20, 1994, p. 14 of transcript.

there were various features that Friedman saw as likely to occur in practice under a single-payer system—including, on the supply side, the government's management and regulation of employment and pricing practices—that were among those he regarded as the most interventionist and likely to promote inefficiencies. He saw the Clinton Administration proposals as entailing features like these, especially because the plan included price controls. In early 1994, Friedman had put his name to an open letter, co-signed with over 500 other economists and ostensibly addressed to the president, singling out for criticism of the price-controls aspect of the Clinton Administration's healthcare proposals.[156]

Conversely, and in an echo of his defense of incomes policy in his debates with Friedman in previous decades, James Tobin stated publicly, "I was not particularly proud of members of my profession who signed that statement." In a paper that he wrote on healthcare, within a section headed "Can Price Controls Work?," Tobin complained about the economists' deployment of "the usual arguments" (which he implied were flawed) that relied on the historical failures of national price controls. Tobin also claimed that the arguments were not applicable to the imposition of price controls in the medical industry: "such ceilings are prevalent throughout the world, and they work."[157]

After 1994

The contrast between his own perspective on the state's role in medicine and that envisioned by the Clinton Administration early in its tenure allowed Friedman in later years to contrast his recommendations with the "sweeping socialization of medicine proposed by Hillary Clinton."[158] However, Friedman's own continued writings on the subject of healthcare after 1994 confirmed that he was himself dissatisfied with the existing system. They were also consistent with the reality that although the Clinton Administration's activism on the matter had essentially ended in 1994—allowing its proposals to lapse—healthcare reform was bound to be a major political issue again.[159] As Aaron (1996, p. 131) put it: "The trends that led President Clinton to place

[156] See *Wall Street Journal*, January 14, 1994. Other signatories had included Anna Schwartz, David Meiselman, Phillip Cagan, Michael Boskin, Benjamin Klein, John Taylor, Sam Peltzman, Gregory Mankiw, William Poole, Paul Evans, and Allan Meltzer.

[157] Tobin (1994, p. 10). In his paper, Tobin's principal policy recommendation was the introduction of a universal insurance mandate for U.S. citizens.

[158] Friedman (2001, p. 19). Both Hillary and Bill Clinton had been prominent in advancing the healthcare plan.

[159] Although a section title of Feldstein (1996, p. 139) was titled "The Legislative Rejection of the 1994 Health Proposals," it would probably be more accurate to characterize the administration as having ultimately decided not to press for a vote on the proposals' passage. The news reporting at the time suggested that, after having had talks

reform of healthcare financing at the top of his domestic legislative agenda will not change." These trends led, in particular, to a new healthcare reform push—one that had more legislative success, including the introduction of an insurance mandate—in the early Obama years.

Friedman, of course, did not live to see this development. But the fact remained that his own position on the U.S. health problem had not remained static over the postwar period and that, in the 1990s and 2000s, he prescribed considerably more state compulsion than he had previously favored. Friedman was also not averse to a federal government insurance policy being an element of his own favored mandate system: "there could be a government role… [i]n providing catastrophic insurance for people who cannot afford it."[160]

Friedman felt in his later years, as he had in the 1970s, that the public sector might ultimately play a dominant role in the United States comparable to that seen elsewhere. The country had "narrowly avoided another major step" toward a single-payer system, Friedman suggested in April 2004, when the Clinton Administration's early proposals languished. But he added: "A single-payer system has great political appeal." It might, therefore, lie in the United States' longer-term future, he suggested.[161]

In the meantime, however, the U.S. *status quo* was a powerful impediment to reform in any direction. Friedman acknowledged when he first advanced his proposed reform of medical arrangements that it was "politically infeasible"—citing in this connection the strong interest that healthcare administrators of various kinds had in maintaining the present system (*Wall Street Journal*, November 12, 1991). He counted private-sector administrators among those who would take exception to his proposal, as that proposal would, as already indicated, imply a reduction of the role that private-sector health insurance arrangements played in many medical transactions.[162]

Nearly fifteen years after his 1991 op-ed, the political infeasibility of Friedman's proposal no doubt held him back from offering it when President George W. Bush, at the July 2006 gathering of the president and Hoover Institution affiliates, asked for practical reform suggestions.

with key legislators, "White House officials indicate an acceptance of the reality that there are not enough votes to pass Clinton's plan." (*The Courier-Journal* (Louisville, Kentucky), July 22, 1994, p. 1.)

[160] In R. Kuttner (2006).

[161] Friedman (2004).

[162] Recall that, although Friedman proposed a universal insurance mandate, he also believed that households should not be required to be insured against those expenses that were below a high threshold.

Nevertheless, the actual suggestion that Friedman made at the 2006 meeting with the present did pertain to healthcare policy. Specifically, Friedman urged that Bush try to have the U.S. Congress pass a law allowing health insurance companies to compete across U.S. state boundaries. "Milton said, 'We've got to change this insurance regime that we have national competition,'" Robert Hall recalled (interview, May 31, 2013). "And, you know, he made a very cogent case for it. It was impossible to disagree with him. It was obviously a good idea. The more you knew about it, the more you knew it was a good idea. But I'd seen him do that [discuss this proposal] several times, very late in his life."

INTO THE NEW ECONOMY

"Computer Revolution Sneaking Up on U.S." was the headline given to a newspaper column in early 1966 (*Detroit Free Press*, February 21, 1966). The judgment expressed in the headline was certainly accurate, as was the assessment by the columnist, Sydney Harris, that the computer revolution would come to be seen as a successor to the previous shakeups of the U.S. economy: the pre-twentieth-century emergence of a manufacturing sector, and the more recent development of a modern services sector.

Less presciently, Harris went on to treat the increased automation associated with the computer revolution as bound to generate additional aggregate unemployment in the United States. This was the kind of position on technological improvement long disputed by economists—including Friedman, who at a conference held in Washington, D.C., later in the year (on December 9, 1966), had occasion to voice his objection to the idea that automation generated higher unemployment, in aggregate.[163]

Friedman objected again to this idea, which he called among "the outmoded fallacies of 150 years ago" when, at the very start of the 1980s, he appeared as one of the commentators on a U.K. television special, hosted by David Frost, on the topic of what to expect over the new decade. Friedman, joining proceedings via satellite from San Francisco, had largely completed his contribution to the program when he signaled that he wanted to speak again, after an in-studio panelist had suggested that the spread of computers would generate mass unemployment. Frost granted Friedman the floor.

[163] See U.S. Chamber of Commerce (1967, p. 9).

"If technological improvement meant greater unemployment, you'd have everybody unemployed today," Friedman remarked in his new intervention, "because we've had enormous technological improvement over the past 150 years. The fact is that computer chips and similar technological developments *do not replace people*. What they do is give people better tools to work with. As a result, it enables people to be more productive, it enables employment to be more rewarding, [and] it enables all of us to have a higher level of income. That's the way in which the market works." (*David Frost's New Year Special: We've Seen the Future*, Yorkshire Television, January 1, 1980.)

The main qualification to this remark that Friedman made in his related statements over the years on automation was that he granted that automation replaced certain jobs. Individual jobs were indeed likely to be displaced by "mechanization or automation," Friedman noted in 1966.[164] But at least an equal number of other jobs would be generated elsewhere in the economy.

On this dimension, automation was highly analogous to increases in imports. In fact, it proved to be the case that the phenomena of globalization and the computer revolution were intertwined both in timing—with both becoming manifest as the twentieth century reached its close, and the so-called New Economy came into its own—and in their economic implications.

The computer revolution takes shape

Many months after the 1966 Harris newspaper column on automation, Peter Gutmann, an associate professor of economics at the City University of New York, had published an article, "New Developments in the Computer Industry," in the mixed research/public-policy journal *Business Economics*. As Friedman in his 1966 and 1980 remarks did, and in common also with many other economists' analyses, this article stressed the productivity-increasing, rather than job-depleting, aspects of advances in computing. The Gutmann article was also notable in foreshadowing the additions to data-retrieval and communications capabilities that would develop in subsequent decades as technological developments were incorporated into the workplace. "The data processing revolution has been prodigious," Gutmann (1966, p 15) observed. "It has created a major industry, the computer industry, which has experienced rapid growth and shows every sign of continuing on a rapid growth path for some time. The industry is now undergoing great changes. The third generation of computers, far superior technologically to its predecessor, is here."

[164] In Brozen and Friedman (1966, p. 18).

As might be expected, Gutmann was off the mark regarding some of what he foreshadowed and, in particular, he was unduly conservative when he portrayed time-sharing as likely to be a permanent feature of computer usage.[165] But a number of his other predictions gave an accurate picture of developments that would materialize in the 1990s and 2000s: these included the scenario of universities and corporations being connected to networks via small computers, "perhaps around a city, across the country or around the world" and his contention in this connection that "U.S. telephone line costs make long-distance use feasible."[166]

Near the end of the 1960s, a syndicated U.S. news article had contended: "The electronics revolution is only beginning. Students will soon be able to rely on a network of computer terminals to gather information, perform calculations, and even provide tutoring." The piece further noted that Dartmouth College—a university whose library Friedman often drew upon during summertime research, owing to its proximity to his Vermont holiday home—"is utilizing a computer in this fashion and has installed terminals at high schools miles from the campus." The article added: "Huge data banks will be used by people in all walks of life," and it observed, as far as the retail market was concerned: "A telephone in every car is also a definite possibility, as is a wireless telephone that can be carried in a pocket or handbag." (*The Times Recorder* (Zanesville, Ohio), March 2, 1969.)

By the time of this syndicated article's appearance, Anna Schwartz had discussed in print the prospect of an era of research underpinned by computer networks. She did so in a book review that appeared in 1968. "The rapid development of computer technology presages the day when economists will work at individual consoles hooked into giant computers through the use of telephone lines," Schwartz (1968, p. 393) wrote. "The central computers will be equipped with comprehensive data banks and retrieval systems, so the need for data compilation by economists will be minimized: all the standard time series and cross-section tabulations will be available for analysis by appropriate signals to the computer."

Schwartz's own familiarity with computers' role in economic research stemmed from her NBER employment since 1941 and, in particular, her monetary work with Friedman. Schwartz's historical research with Friedman had, of course, involved considerable data transformation aided by computers of some kind. More formal statistical work by Friedman and Schwartz on the monetary and economic data had gone side by side with their analysis of historical episodes,

[165] Gutmann (1966, p. 18).
[166] Gutmann (1966, p. 18).

and the *Monetary History* had used both techniques—even though their book's emphasis on episodic analysis would be stressed in its concluding remark about the importance of examining the "antecedent circumstances whence arose the particular movements that become so anonymous when we feed the statistics into the computer."[167]

In addition to drawing, in part, on computer work in producing their study, Friedman and Schwartz in the *Monetary History* had had occasion to reflect on advances in communications in the century to 1960 that the computer revolution would come to be seen as continuing in the post-1960 period. They had observed that the introduction of the cable as a means of communications during the second half of the nineteenth century had generated a "commercial revolution" in the United States, by reducing the lag in the receipt of information between New York and London financial markets "to a matter of minutes or hours."[168] Friedman and Schwartz dated this change to the mid-1860s.

Reflecting on the further progress made in the following century and more, Friedman remarked in the mid-1970s: "It's amazing how easy it is to keep up with what's happening in the United States from the most distant points in the world." (Instructional Dynamics Economics Cassette Tape 167, early May 1975.) Accordingly, in 1982, in the final book of their series, Friedman and Schwartz were able to observe: "The jet aircraft now spans the ocean in a few hours. Satellite transmission and television and radio communications link countries instantaneously and at relatively low cost."[169] In July 1988, Friedman harked back to the advent of the transatlantic cable that he and Schwartz had discussed in their first book together, noting: "Satellites and computers have completed the process that the cable began."[170]

Friedman's own exposure to computers dated back to well before the Friedman-Schwartz project and included his wartime work on metallurgy. This involved occasions when he was able to secure work time on a large computer that was stationed at Harvard University and on which he could have a multiple regression run.[171] There was an echo of this experience in Friedman and Schwartz's computations for *Monetary Trends* during the 1970s. Despite all the improvements in miniaturization and computation in the interim, in the mid-1970s Friedman and Schwartz were

[167] Friedman and Schwartz (1963a, p. 686).
[168] Friedman and Schwartz (1963a, p. 26).
[169] Friedman and Schwartz (1982, p. 292).
[170] Friedman (1988c, p. 378).
[171] See his solo-authored remarks in Friedman and Schwartz (1991, p. 48). See also Nelson (2020a, Chapter 3) for a discussion of this experience.

relying on offsite computer facilities for their *Trends* work, which mostly consisted of linear regressions.[172] In 1976, they reported that they were sharing the usage of TROLL terminals located at Cornell University, having previously used card-input computers at Yale University.[173]

Friedman was sufficiently immersed in computers by the mid-1970s to draw amusement from a vintage Irving Fisher quotation in which when Fisher wrote that "I have had at least one computer in my office almost constantly at work on this project," when Fisher meant a staffer operating an electric calculator.[174] Friedman was not ideally situated to poke fun at Fisher on this matter: as late as the mid-1950s, he himself was using the term "computers" to refer to the human operators of calculators.[175]

Indeed, calculators for a long time were the closest Friedman had to home or office computer equipment. He had first bought a hand calculator, probably in the early 1950s, secondhand for $300.[176] Even in early 1977, when he remarked, "I rushed home to my Hewlett-Packard computer," he was referring to a calculator.[177] And it was desk calculators to which Friedman was referring when, on television in late 1978, he cited the downward trend in the nominal price of "hand-held computers."[178]

In the research projects in which he was engaged through the end of 1976, Friedman did have access to computer facilities via both the NBER, as already indicated, and the University of Chicago. But the prospect of having a personal computer of his own, let alone a portable one (other than a desk calculator), seemed remote enough to him that, on a visit to Australia in April 1975, he could joke to a questioner, "I am afraid that the limitations of time and the fact that I left my computer at home prevent me from giving you a better answer."[179]

[172] Some of the work required more complicated computations than that involved in estimating correlations and linear regressions. A small part of their study used nonlinear regression (see Friedman and Schwartz, 1982, p. 359), while other computations involved deriving dynamic reaction patterns from the regression estimates (see, for example, Friedman and Schwartz, 1982, p. 375).
[173] See Friedman and Schwartz (1976, p. 20).
[174] Fisher (1926, p. 786), quoted in Friedman (1976g, p. 215).
[175] Friedman (1954c, p. 310).
[176] See his remarks in the filmed portion of *Free To Choose* (U.S. television version), PBS, Episode 7, "Who Protects the Consumer?," February 23, 1980, p. 2 of transcript.
[177] Friedman (1977a, p. 9).
[178] *Meet the Press,* NBC, November 12, 1978, p. 5 of transcript. Friedman did, however, refer to the decline in the (relative) price of more-broadly defined computers on other occasions, such as *Controversy,* BBC2, September 23, 1974 (p. 20 of transcript) and Friedman (1976f, p. 314).
[179] Friedman (1975e, p. 22).

Friedman's move to the Bay Area in early 1977 and his residency there for the remaining nearly three decades of his life allowed him to be a witness at close hand as the computer revolution came to fruition. Early in his period in San Francisco, although portable computers were still far off as a routine business or household product, desktop computers were gaining a foothold in home and office use. Newspaper advertisements for what were then called "microcomputers" became prevalent. For example, an advertisement that Radio Shack placed in newspapers in late 1979 concerned its TRS–80 microcomputer, a retail product available "for business, learning, and entertainment," consisting of a keyboard and a "big 12-inch video monitor." The advertisement indicated that two versions of the machine were available to consumers, one for $499 and a "Level II" version for $899 (*Kansas City Star* (Missouri), November 1, 1979). Friedman, a gadget enthusiast, was a soft touch for new merchandise like this, and he purchased a desktop computer—for use in his home office study, his main place of work—with alacrity.[180] "He got one that Radio Shack put out, a TRS, and he loved it," Friedman's secretary Gloria Valentine would recall (interview, May 1, 2013). "… And, of course, he got this thing and started playing with it, and Mrs. Friedman told me that every time they had guests, he would break off one guest, usually a man, and take him to his study and go on the computer."

By the mid-1990s, Friedman was characterizing his main mode for writing as using the computer, describing his memoir-in-progress with Rose Friedman as being written "[i]n a word processor mostly. Sometimes by hand, but mostly in a word processor."[181] Nevertheless, he continued to rely heavily on dictation (including dictation of emailed replies), typed up by Valentine, in correspondence right until 2006.

When it came to his data analysis, by the start of the 1980s already using a desktop computer, as indicated in his closing words in a *Newsweek* column, "Back to the computer." (*Newsweek*, September 21, 1981.) The value of computers in his work was reflected in his purchases of later vintages of computers, and an early 1983 edition of the computer magazine *Info World* contained an interview with a 20-year-old Stanford University undergraduate, John Halamka, who had developed a desktop computer equipped with software that was convenient for data analysis. In the article, Alamka noted that he had "sold word processors all over Stanford. I sold one to Milton Friedman." (*Info World*, March 7, 1983, p. 37.) The article said that Friedman was using

[180] Gloria Valentine noted that Friedman took to working with new inventions "absolutely, always, and do them without reading the instructions. Mrs. Friedman told me right after they were married, he took her sewing machine apart." (Gloria Valentine, interview, May 1, 2013.)
[181] CSPAN, November 20, 1994, p. 11 of transcript.

the new computer purchase for economic forecasting, and Friedman's remarks during 1984 confirmed his use of a personal computer in exploring monetary relationships (Heller and others, 1984, p. 43; House Republican Research Committee, 1984, p. 43).

The new accessibility of computer-based data analysis made it easier for Friedman to make his ill-fated inflation projections in 1983–1984 on the basis of the behavior of M1 growth, as well as his reevaluation and switch to M2 in 1986. Friedman would reflect that the multiple regression that took the Harvard University computing facilities 40 hours to calculate for him in the mid-1940s would take less than thirty seconds on his desktop computer four or five decades later.[182]

It was in the course of this period that the Friedmans wrote in 1984 of the "electronic revolution."[183] But, as a couple of specialists in the field remarked that year (*Daily News* (New York), September 11, 1984): "While others are talking about the computer revolution as if it had already come and gone, we say the computer revolution hasn't yet begun." Developments in the subsequent decade and more would bear out the wisdom of this assessment—with the late 1980s and early 1990s seeing the spread of portable computers and mobile telephones. The era of information technology subsequently really set in via the widespread availability in the 1990s of internet and email access. Writing at approximately the mid-decade point, an economics textbook could refer to the "computer and information revolution of the 1980s and 1990s" (Parkin, 1996, p. 192).

The online world

Prior to the 1990s, the internetted environment of the kind that Anna Schwartz had sketched in 1968 had already made itself felt in some sectors of the U.S. economy, including that of financial services. It was the manner in which computer hook-ups allowed rapid interbank transfer that left Friedman with an early awareness of online technology, as this development was relevant for monetary issues in which he was interested—such as the administrative feasibility of lagged reserve requirements and the transferability of demand deposits and other classes of bank balances. In this connection, in 1977 he had referred to "computers that can be used to make such transfers more efficiently," and Friedman and Schwartz in 1982 had mentioned "faster computer clearing."[184] In 1985, with this technology having increasingly spread to nonfinancial

[182] See Friedman and Schwartz (1991, p. 48) and Friedman (1991e, p. 36).
[183] Friedman and Friedman (1984, p. 118; 1985, p. 114).
[184] See, respectively, Friedman (1977c, p. 9) and Friedman and Schwartz (1982, p. 499).

businesses, Friedman acknowledged that office work more generally was displaying the effects of the "information revolution… especially the greatly expanded capacity for recordkeeping on a detailed and near instantaneous basis."[185]

Pending a still more widespread dissemination of this technology to the U.S. nonfinancial private-sector economy, however, the "online" world largely remained a backwater to a considerable extent in the early 1980s, and one practitioner in the field, Barbara Quint, of the Rand Corporation Library and the Southern California Online Users Group, expressed doubt whether Friedman was familiar with it. But, Quint went on to speculate, in light of the innovation and competition going on, he would approve of it if he knew about it: "Milton Friedman would love the online industry. In an impure world, it may be as pure an example of the free market as one is likely to find." (*Online*, September 1982.)

This speculation proved to be an accurate prediction of Friedman's reaction. When the online world became entrenched in daily life, Friedman hailed the innovation and relished using it himself. By the end of 1990, he was himself using the terminology "download" when discussing accessing a networked database for use on one's own computer.[186]

More than a decade later, Robert Gordon was present at the March 2002 Federal Reserve Bank of San Francisco conference that Friedman also attended. "I went over to him, and I said, 'Oh, do you do email?' And he said, 'Oh yeah,' and he gave me a card which had his email address on it. We [then] had quite a nice correspondence, [over] the last three or four years of his life."[187] Friedman had in fact been using email regularly since the late 1990s.[188] In 1995, he had seemed to think that fax machines were the prime competitor with physical mail.[189] He may not have had much exposure to email at that stage and, possibly, may have believed that email

[185] Friedman (1985e, pp. 11‒12).
[186] Friedman (1991e, p. 36).
[187] Robert Gordon interview, March 21, 2013.
[188] For example, in mid-March 1998, he mentioned an email that he had recently received from a correspondent (Block, 2006, p. 64). He made this remark, however, in the context of writing a hardcopy letter to the same correspondent. Indeed, Friedman in the late 1990s and later continued to use physical mail heavily and was engaged in hardcopy correspondence even with Stanford University-affiliated economists during the first half of 1999. Nevertheless, his transition to heavier email usage was reflected in the contrast between 1997—when, in a discussion published as O'Driscoll and others (1997), Friedman's was one of the panelists for whom an email address was not listed, and when, also, no email address was given for Friedman in the final-ever hardcopy version of the American Economic Association (1997, p. 184) members' directory—and the early 2000s, when his Stanford University email address was listed in his electronic entry in the American Economic Association members' directory.
[189] *Washington Post*, February 19, 1995 (reprinted in Bonsteel and Bonilla, 1997, p. 198).

messages could or would consist only of plain text. But, in due course, Friedman became accustomed to including email attachments. Around 2001, he expressed his enthusiasm about the new communications tool to his longtime friend Michael Mork. “He said, ‘I cannot believe this email, how fantastic this is,’” Mork recalled (interview June 4, 2013). “He said, ‘My son [David] is in South America right now. We’re corresponding every day.’ He said, ‘This is one of the best inventions of all time.’ He just loved email.”[190]

Rethinking globalization

The 1990s saw still greater coverage being given in public discourse to the globalization of the U.S. economy. As detailed in the previous chapter, much media and policymaker discussion through 1988—that is, even before the breakup of the Soviet bloc and the prevalence of the internet—cast this development as an important phenomenon. The relevance of globalization had also been emphasized by various economists—including those who were not specialized in trade or international economics but who now saw increased world integration as bearing importantly on their own areas of interest. Alan Blinder, for example, was quoted in early 1988 as saying: “New Keynesians, like everybody else, have been slapped in the face by the importance of the world economy.”[191] Five years later, in the post-Berlin Wall environment, Bennett McCallum (1993, p. 39) ended a paper on monetary policy rules by observing: “Actual central banks are unlikely to officially adopt such rules, of course, but consideration of their implication could nevertheless prove helpful in practice, … as financial liberalization, technical innovation, and globalization phenomena continue to occur.”

As the previous chapter stressed, others, including Friedman, had remained resistant to acknowledging the importance of globalization. Friedman had, of course, long acknowledged major respects in which of economies were interdependent.[192] And in 1988 he felt it worth emphasizing that “the United States is not an isolated island by itself.”[193] But Friedman had a negative perspective on the stress being put on globalization in the analysis of the U.S. economy.

The period from 1993 onward would see a shift on Friedman’s part. He certainly continued to maintain important objections to the claims being made about the economic implications of

[190] Friedman was using hardcopy correspondence even with Stanford University-affiliated economists during the first half of 1999.
[191] *U.S. News and World Report*, February 1, 1988 (p. 45), reprinted in McClelland (1988, p. 83).
[192] He did so in, for example, *Wall Street Journal*, June 30, 1975, and Friedman and Friedman (1980, p. 51).
[193] *The MacNeil/Lehrer News Hour,* PBS, May 19, 1988, p. 11 of transcript.

globalization. But there would be a considerable wearing-down of his 1980s tendency to question the basic postulate that the economy had, in fact, become substantially more integrated.

In *Monetary Trends*, Friedman and Schwartz had argued that the restrictions on trade and, especially, on transactions in foreign exchange that were in force during the postwar period had prevented the United States and the United Kingdom from becoming more economically integrated and that the two economies might well be less intertwined in the mid-1970s than they had been in the past.[194] This position amounted to a claim that the interdependence of economics was an important real-world phenomenon but that it had not increased appreciably in significance in recent times.

But that position soon became untenable. *Monetary Trends*' data had stopped in the mid-1970s, and the United States' foreign exchange controls had been removed in 1974 and the United Kingdom's in 1979. In April 1980, Paul Volcker told a Congressional hearing: "We live in a very closely integrated world. You know, financial communication is probably closer between New York and London than it is between New York and Washington or someplace else in the United States. The telephone lines are open; the electronic facilities are open… People are always looking to shift money where they think they are going to come out ahead."[195] The conditions described by Volcker intensified over the following years, and international connections of financial, goods, and labor markets would also be boosted by developments in China, India, and Eastern Europe. It was against the background of these developments—what he called "a combination of political and technological change" (*Forbes*, August 17, 1992, p. 44)—that, from 1992 on, Friedman changed his assessment regarding the reality of globalization.

In a notable demonstration of this shift, he wrote in 1993 (*Far Eastern Economic Review* (Hong Kong), October 28, 1993) that it was now "possible, to a far greater extent than any time in the world's history, to use resources from anywhere to produce a product that can be sold anywhere."[196] Although he had now, in effect, acknowledged the *process* as being a key modern-day empirical phenomenon, Friedman indicated in early 1996 that he disliked the *term* "globalization" (see Snowdon and Vane, 1997, p. 205). But—as had been the case with

[194] See Friedman (1980b, p. 60) and Friedman and Schwartz (1982, pp. 6, 292).
[195] Testimony of April 16, 1980, in Committee on Banking, Finance, and Urban Affairs, U.S. House of Representatives (1980c, p. 144).
[196] Similarly, he would note in 1995: "The technological revolution has made it possible for a company located anywhere in the world to use resources located anywhere in the world, to produce a product anywhere in the world, to be sold anywhere in the world." (*Washington Post*, February 19, 1995, reprinted in Bonsteel and Bonilla, 1997, p. 197.)

"monetarism"—he found it hard not to use the term himself, and he did so—for example, in a conference held the following year (see Zak, 1999, p. 15).

Irrespective of his feelings about the term, what Friedman now referred to "so-called globalization" (Ragan, 1999, p. 52) was, he acknowledged, an important factor affecting business decisions and the behavior of potential output. In a television appearance in early 1995, Friedman acknowledged that the behavior of the real economy in coming years would be shaped by the "technological revolution and the opening up of the world to worldwide commerce." (*Louis Rukeyeser's 1995 Money Guide*, Maryland Public Television, January 1, 1995, p. 6 of transcript).

There remained some stubbornness on Friedman's part about seeing the modern era as the epitome of globalization: for example, in a late-life interview (*New Perspectives Quarterly*, Winter 2006, p. 5), he suggested that the nineteenth century better exemplified globalization because goods trade had been more free in that era. He also, as noted, remained critical of what he perceived as the economic fallacies that often came hand-in-hand with discussions of globalization. For example, as discussed in the previous chapter, Friedman rejected much of what was said about international competitiveness because it failed to take into account the endogeneity of the exchange rate. "I'm not going to talk about global competition," he told his hosts at a speaking engagement in 1992. "The whole notion of 'global competition' is a bunch of crap." (*Orange County Register* (Santa Ana, California), February 16, 1992.) And, as discussed below, Friedman continued to insist, with strong justification, that global financial linkages implied that inflation was a global phenomenon—and to maintain that inflation was, instead, generated by monetary policy at home.

A parallel to Friedman's shift in the 1990s was that made by Paul Krugman on the same topic. Krugman objected to much of the talk in the 1980s and 1990s on many of the same grounds that Friedman did. Krugman, like Friedman, eschewed accounts that saw globalization as having removed a country's autonomy: aggregate spending, and the short-run course of aggregate employment, would still be heavily influenced by the government's settings of stabilization-policy instruments. But at the start of the twenty-first century, Krugman was willing to grant that globalization had, nevertheless, been a key macroeconomic event. "I deliberately say world economy, not American economy," Krugman remarked in his debut *New York Times* column (January 2, 2000). "Whatever else they may have been, the '90s were the decade of globalization." Having made this concession, he went on to stress, as Friedman had many times, that the then-world economy had been substantially integrated before—in the late nineteenth century—only to fragment again.

Another similarity between the Krugman change in outlook and Friedman's own shift was that Krugman would accompany his acknowledgment of the fact of modern globalization with a continued rejection of the notion of directing domestic economic policies toward achieving international competitiveness. "So, if you hear someone say something along the lines of 'America needs higher productivity so that it can compete in today's global economy,'" Krugman had written in the mid-1990s, "never mind who he is, or how plausible he sounds. He might as well be wearing a flashing neon sign that reads: *I DON'T KNOW WHAT I'M TALKING ABOUT*."[197] Krugman and Friedman would both remain skeptics about the notion of international competitiveness of a nation, particularly because discussions of that notion often neglected the endogeneity of the exchange rate with respect to domestic policies.

But, although both Friedman and Krugman were very reluctant to concede that, under floating exchange rates, international trade developments could bear heavily on the *aggregate* volumes of output and employment in a nation, neither of them denied the existence of important short- and long-run effects of trade flows on the components of production and on sectoral employment.[198] Despite their playing-down of globalization's aggregate significance, their respective analyses were basically consistent with the many characterizations, such as that of James Tobin (1991, p. 304), of the U.S. economy having acquired a "Rust Belt, hit by changes in international and interregional comparative advantage."

It was, in fact, the effect of globalization on the composition of U.S. production and employment that led Friedman, in the mid-1990s, to change his view about the political primacy of the issue of the distribution of income in the United States.

Globalization and inequality

By the mid-1990s, Friedman had had fifty years of firsthand understanding of the controversy likely to ensue from expressing a view on appropriate public policy regarding the redistribution

[197] Krugman (1995a, p. 280). Also in *Fortune*, March 7, 1994, p. 115.

[198] In Friedman's case, see the discussion in Chapter 15 above of his reaction to the high U.S. dollar, as well as the items referenced in that chapter, including the discussion in Friedman and Friedman (1984, 1985) of smokestack industries and U.S. employment.

of income. A favorable reference that Friedman and George Stigler, in the draft of their 1946 pamphlet on rent control, made to the idea of incomes more equal via U.S. government policy measures had been resisted by their commissioned publisher, the Foundation for Economic Education (FEE)—which ultimately did publish the statement in question but attached to it an editorial disclaimer.

Decades later, this controversy over getting this passage into print would become a prominent one in the literature on Friedman's career and work—an early example being Silk (1976, pp. 69–70).[199] But the matter had actually come into the public record as early as 1950, owing to the fact that—in the atmosphere of the time of heavy Congressional inquiry into the political activities of individuals and firms—the FEE correspondence over the passage was subpoenaed and then publicly released. Consequently, the internal consternation at the FEE about the Friedman-Stigler passage was laid bare. Included in the disclosed correspondence was a letter by the head of the FEE, Leonard Read, to a distributor of the forthcoming pamphlet, in which Read remarked: "There is one paragraph in this whole thing that worries me, as well as the rest of us here." Read related his failed efforts to persuade the authors to delete the passage, and he assessed of its inclusion in the finished version: "No one except a sharp, free enterpriser economist would catch the offending paragraph but, then, our works are supposed to be above criticism by a free enterpriser." (*The Greensboro Record* (North Carolina), August 7, 1950.)

Read anticipated the reaction accurately. The Friedman-Stigler manifesto would primarily be received on its release as in favor of free-market solutions, thanks to its detailed outline of the case for the abolition of rent control, and not as an interventionist piece, notwithstanding its favorable mention of income-redistribution policy. Indeed, Friedman and Stigler's 1946 work *Roofs Or Ceilings*? would both be much reprinted in later years in public-policy outlets, becoming a standard reference on rent control, and be heavily cited in the economic-research literature on rent control (including a critique of rent control that appeared in the *American Economic Review* nearly five decades after *Roofs Or Ceilings*? first appeared: see Glaeser and Luttmer, 2003). Read was also correct, however, in believing that libertarians would both notice, and take umbrage at, the Friedman-Stigler endorsement of the principle of making incomes more equal via public policy. In later years, of course, Friedman, would provide further grounds for continued antagonism toward himself on the part of libertarians in the area of income redistribution—via his activism, from the 1960s onward, in favor of a negative income tax

[199] In the paperback version of Silk (1976), the passage in question appeared on pages 63–64.

(NIT).

By the time of his high-profile advocacy of NIT, however, Friedman was actually far less supportive than he had been in 1946 of concerted federal government policy to redistribute incomes. Rising U.S. incomes and Friedman's disillusionment with the operation of tax and welfare policy had led him to judge not only that tax-and-transfer methods should be streamlined—with the NIT playing a major part in the streamlining process—but also that these methods should operate on a decidedly limited scale.[200] He saw income redistribution via the federal government as something that should mainly take the form of a modestly graduated tax system, with the graduation arising principally from a negative income tax, the elimination of income deductions, and a tax-free threshold—and not through a large variety of welfare payments or of an elaborate tax-rate scale.

Therefore, when he appeared in a television panel discussion recorded in early 1976, Friedman was arrayed against three other panelists who all wanted far more ambitious redistribution policies than he did (*Bill Moyers' Journal*, PBS, February 29, 1976). The market system, he told the host, "doesn't assure" inequality of outcome—rather, "it permits unequal economic results in accordance with the productivity of the people involved."[201] Friedman saw it as appropriate for unequal incomes to result from different marginal products but, subject to that, he stated that he would "regard it as a desirable outcome of a set of arrangements that it ends up with equality of outcome in the sense…that you have the same [income] profiles for large, randomly-selected groups."[202] In order to achieve this income, "I would not give up the pursuit of equality of opportunity… We should pursue equality of opportunity."[203] Friedman also reaffirmed his belief that a floor income to be provided by the government, thereby avoiding distress cases.[204] But, he suggested, "once you've provided the floor," the objective of the government should be "removing obstacles" to equality of opportunity. Friedman argued vigorously on the program that vouchers were a key means by which this objective could be pursued.[205]

[200] For a discussion of Friedman's evolving views on income redistribution from the 1940s to the 1970s, see Nelson (2020a, Chapter 4).
[201] *Bill Moyers' Journal*, PBS, February 29, 1976, p. 9 of transcript.
[202] *Bill Moyers' Journal*, PBS, February 29, 1976, p. 14 of transcript.
[203] *Bill Moyers' Journal*, PBS, February 29, 1976, p. 13 of transcript.
[204] In the program as broadcast, Friedman did not indicate that the negative income tax was the means by which to provide the floor. But the negative income tax had been discussed favorably by Lester Thurow earlier in the program, in a separately recorded session in which Thurow was interviewed by Charlie Rose, the program's executive producer (*Bill Moyers' Journal*, PBS, February 29, 1976, p. 3 of transcript).
[205] *Bill Moyers' Journal*, PBS, February 29, 1976, p. 8 of transcript.

In addition to reflecting his more market-oriented perspective than his panelists, Friedman's objection to income-redistribution policies resulted from a different evaluation of the pressure in the U.S. community for policies expressly designed to generate greater income equality. Increased general living standards had, he believed, diminished the priority that society was likely to give to policies motivated by the need for redistribution. The backlash, during the late 1970s and the 1980s, against high marginal income tax rates would provide support for Friedman's belief that the general public had reassessed the costs and benefits of public policies designed to achieve major income redistribution. In the mid-1990s, however, Friedman himself acknowledged that there was likely to be a rekindling of interest in income inequality.

Income inequality would come to the fore of U.S. public debate, Friedman suggested in an op-ed in the *Washington Post* of February 19, 1995, because of the globalization of the economy. This development had, he noted, raised living standards in the developing world. But Friedman added: "The effect has been somewhat different in the advanced countries." The latter had seen "a sharp widening in the differentials" of wages within these economies.[206] He saw great political and economic trouble brewing: "If the widening of the wage differential is allowed to proceed unchecked, it threatens to create within our own country a social problem of major proportions. We shall not be willing to see a group of our population move into Third World conditions at the same time that another group of our population becomes increasingly well off. Such social stratification is a recipe for social disaster. The pressure to avoid it by protectionist and other similar measures will be irresistible."[207]

In a television appearance a few months later, Friedman elaborated that the kinds of jobs available in the U.S. labor market had been "changed completely by two phenomena: the technological revolution in communications, transportation, and the like, and the political revolution. The technological revolution has made it possible for a business enterprise to use resources located anywhere. And secondly, you've had a political revolution, the fall of the Berlin Wall, which has enormously increased the number of low-wage labor, the amount of low-wage labor available in the world, to be combined with the capital from the advanced countries. The result of that is that there no longer is a demand for low-skilled people in the United States at high wages. That's been ruled out." (*Reading, Writing & Rukeyser*," Maryland Public

[206] *Washington Post*, February 19, 1995, reprinted in Bonsteel and Bonilla (1997, p. 197). He had foreshadowed this argument in *Forbes* (August 17, 1992, p. 44) in discussing the "vast amount of low-cost labor" poised to be added to the international economy.
[207] *Washington Post*, February 19, 1995, reprinted in Bonsteel and Bonilla (1997, p. 198).

Television, May 12, 1995.)

He went on in the television interview: "Part of the problem arises because of our bad educational system."[208] In this appearance, as in his *Washington Post* op-ed, Friedman's diagnosis of the effect of globalization on the U.S. labor market and distribution of income had led him back to his longstanding prescription: school vouchers. For the rest of his life, the role of globalization in raising the need for worker skills was a key argument that Friedman deployed in advancing his school-choice proposal.[209]

"The important issue is not how much inequality there is but how much opportunity there is for individuals to get out of the bottom classes and into the top," Friedman suggested. "If there is enough movement upward, people will accept the efficiency of the markets. If you have opportunity, there is a great tolerance for inequality. That has been the saving grace of the American system." (*New Perspectives Quarterly*, Winter 2006, p. 5.) With regard to maintaining opportunity, he went on, "In the U.S., the problem now is primary and secondary education." He expanded on this point in another interview given shortly afterward (R. Kuttner, 2006): "One of my major reasons for being in favor of vouchers is because I believe that defects in our educational system play a major role in the growing inequality of income." In particular, Friedman believed that improved education outcomes could forestall the growth in the U.S. income inequality that the new worldwide configuration of production was tending to generate.

In these latter-day analyses appearing from the mid-1990s to the mid-2000s, Friedman made a fairly accurate prediction of developments in the decade after his death in 2006. He assessed that a backlash against the contraction of U.S. manufacturing sector, and the associated change in the wage structure, generated by the electronic revolution and globalization might produce U.S. national political trends promoting protectionism.

Friedman's stress on actual and prospective concerns about the distribution of income recognized that these concerns would give rise to calls for more economically-interventionist national

[208] *Reading, Writing & Rukeyser*, Maryland Public Television, May 12, 1995.

[209] The effect of globalization in potentially sharpening income differences within the United States was consistent with *cross-country average* income differences being diminished significantly by the economic progress of emerging nations. It was this prospective reduction in world inequality that Robert Lucas (2004) emphasized, in a piece that Friedman read. "Near the end [of Friedman's life], I had written something about economic growth, something I had done for the Minneapolis Federal Reserve Bank—you know, its annual report or something [the *2003 Annual Report*, published as the May 2004 issue of *The Region*]. It was a pretty nice thing, and he sent me a nice note about it." (Robert Lucas, interview, March 12, 2013.)

economic policies. But Friedman rejected such proposals, notwithstanding his acknowledgment of the concerns that were provoking them. Instead, the repercussions of international developments for the U.S. economy reinforced his belief in the importance of school vouchers as a means of promoting equality of opportunity.

From computer revolution to U.S. economic transformation

In the mid-1990s, Friedman acknowledged that major changes in the U.S. and world production process had been set in motion not only by globalization but also by computers. In his October 1993 article granting the reality of globalization, he noted that, in conjunction with the greatly increased scope for an international division of labor, breakthroughs in information technology had substantially altered what production arrangements could be, and were being, used. The real changes were in place, Friedman wrote, "for the making of a second industrial revolution" (*Far Eastern Economic Review* (Hong Kong), October 28, 1993).

Friedman reaffirmed this judgment in his already-noted February 1995 op-ed, referring in that piece to the "technological revolution."[210] About five months later, however, he added his voice to the many economists and other observers who had expressed puzzlement that U.S. productivity growth did not appear to have increased in the information age (*Wall Street Journal*, August 1, 1995). It seemed, instead, that the post-1973 slowdown in economic growth had continued. By 1995, the economic-research literature had already come to refer to the "Solow paradox" in recognition of Solow's dictum (*New York Times*, July 12, 1987): "You can see the computer age everywhere but in the productivity statistics."[211]

In his August 1995 op-ed, Friedman outlined his suspicion that the public sector's size in the U.S. and other major economies was holding back the aggregate productivity improvements that should otherwise be expected to stem from innovations in information technology. This argument was an application to the case of computers of a longstanding Friedman position—one that he had articulated in 1982 when he observed that "the effect of advanced technology can be entirely stymied by the policies of interventionist governments."[212]

The word "stymied" conveniently encompassed two possibilities that Friedman believed were

[210] *Washington Post*, February 19, 1995, reprinted in Bonsteel and Bonilla (1997, p. 197).

[211] The many deployments of Solow's dictum in the research literature included that in Oliner and Sichel (1994, p. 273).

[212] Friedman (1982f, p. 27).

relevant in this connection—the first being that regulation and taxes could inhibit firms from making full use of new technology, and the second being that, even if introduced, new technology's positive effect on productivity could be obscured and swamped by the negative effects on productivity due to various regulations. With regard to the second possibility, in an interview published in 1994, he suggested that the recent growth in U.S. government regulations, including affirmative-action legislation, might push the productivity trend down.[213]

It transpired that the U.S. productivity trend was undergoing an upward shift in 1994–1995 period, at just the time when Friedman was trying to diagnose the absence of such a shift.

The pickup came at a time when macroeconomists were largely resigned to the post-1973 slowdown. McCallum (1989b, p. 7) stated that "it is highly probable that the average rate of growth of real income or output for the U.S. will be about 2½ or 3 percent per year over the next decade." Indeed, of these two possible rates, by the 1990s the lower value had become the standard estimate of the United States' long-run economic-growth rate since the 1970s. Rudiger Dornbusch observed at the start of the 1990s with regard to the 2.5 percent number, "We certainly cannot do better than that" (CSPAN, January 16, 1990), and Hall and Taylor (1997, p. 6) assumed a 2.5 percent annual output-growth trend in describing real GDP behavior in the roughly quarter century through 1995. Instead, however, growth averaged 3.2 percent on annual data for 1990–1999 and 4 percent for 1994–1999.[214]

As these numbers indicate, the pronounced pickup occurred roughly mid-decade, and Fernald (2016, Table 1, p. 19) gave the annual growth rate in real GDP as having averaged 3.95 percent in 1948–1973, 2.86 percent in 1973–1995, and 3.36 percent in 1995–2004. The improvement in real GDP growth had a counterpart on productivity growth, with Fernald (2016, Table 1, p. 19) reporting that growth in real GDP per quality-adjusted hour, having averaged 2.49 percent per year in 1948–1973, slowed down 0.86 percent in 1973–1995 but was then 2.12 percent in 1995–2004.

Like many others, Friedman was slow to register the changed productivity trend, and an analysis

[213] See Snowdon, Vane, and Wynarczyk (1994, p. 177). Criticism of affirmative action laws, and of the government's application of related steps to the labor market and in academia, was a minor but persistent element of Friedman's discussions from the mid-1970s onward of public-sector interventions. He voiced his early criticisms during an appearance on *CBS Morning News* (November 27, 1975, p. 21) and in his column in *Newsweek* of December 29, 1975.
[214] These averages flow from the data on U.S. real GDP growth (annual data) in the Federal Reserve Bank of St. Louis' FRED portal.

that he produced in the second half of 1996 took the slower growth seen since the 1970s as still being in force (*Wall Street Journal*, September 24, 1996). But he would come to accept that "the 1990s in the United States was an incredible period of rapid expansion" (*Uncommon Knowledge,* Hoover Institution, September 25, 2001, p. 2 of transcript).

Nevertheless, in an echo of his old denial of the importance of globalization, Friedman admitted that in an important respect he was, as he put it, a "stick in the mud" with regard to the so-called New Economy. Specifically, he granted the reality of higher productivity growth but stressed, "We've been here before," with Friedman likening recent growth to the innovation-related surge in growth of roughly a decade from the mid-1870s to the mid-1890s (Ullmann, 2001, p. 15). Therefore, Friedman did not expect the higher productivity growth to last.

Initially, as the U.S. economy moved from what Blinder and Yellen (2001) called "The Fabulous Decade" of the 1990s to the first half of the 2000s, it seemed that Friedman was being proved wrong: the higher rate of growth in productivity continued. At a conference that the Federal Reserve Bank of Dallas held in the Friedmans' honor in October 2003, William Niskanen noted, in the course of a presentation on recent growth performance: "Until a few years ago, there was a big debate in the economics profession, between Bob Gordon on one side and Dale Jorgenson on the other, about whether this productivity [growth] spurt starting in 1996 was [just] a spike or [instead] a long-term phenomenon. And I think that Dale has won that argument, and… productivity growth stayed high during the recent recession." Niskanen judged of the shift up in the productivity trend: "It spread throughout the economy, and it looks like it's going to last for a while."[215]

In the event, it lasted for about one more year after these words were uttered. A decade after Friedman's death, Fernald (2016), in a title bearing the somewhat dismal title "Reassessing Longer-Run U.S. Growth: How Low?," reported that real GDP per quality-adjusted hour rose only 0.6 percent per year on average from 2004 to 2015, considerably lower than even in 1973–1995.[216] Of course, although he had thought that the 1990s pickup would not last, Friedman himself believed that a permanently higher productivity trend *was* possible, provided that the United States chose more market-oriented economic arrangements.

[215] From Niskanen's remarks at the Federal Reserve Bank of Dallas' *Free To Choose* tribute conference, session 3, October 23, 2003.

[216] Fernald (2016, p. 16, Table 1).

Friedman's mixed record in judging the course of U.S. economic growth in the 1990s and 2000s—missing the change in productivity trend, but accurately assessing that the improvement in the growth curve would not last longer than a decade—contrasted with his unambiguously poor record in gauging the prospects for real interest rates. In a television interview given at the midway point of the 1990s, he suggested that, if predictions of a higher productivity growth rate and a still more interdependent world economy did eventuate, the average U.S. real interest rate should become historically high (*Louis Rukeyeser's 1995 Money Guide*, Maryland Public Television, January 1, 1995, p. 6 of transcript). But, after being quite high through 1994, real interest rates over the second half of the 1990s tended to be *below* those prevailing over much of the 1980s. And, in the first decade of the new century, real and nominal interest rates stepped down sharply further. In the face of the low yields starting to be observed in the early 2000s, Friedman cited low demand for investment, tracing this low demand to what he perceived as a political and regulatory atmosphere that was uncogenial to corporations (*Investor's Business Daily*, April 15, 2004, p. A8). This in turn stemmed, he believed, from an anti-business backlash in response to the Enron scandal, with the change in public opinion that was part of this backlash creating concerns by corporations about future government policy (*Sunday Times* (London), September 8, 2002). In contrast, an enlargement of the world saving pool is more usually favored as the main explanation for the step-down in equilibrium real interest rates that was seen during the early 2000s (see Bernanke, 2005).

The new economy, monetary policy, and inflation

Friedman was on more solid ground in assessing the implications for nominal variables in the wake of globalization and the technological revolution. As stressed in the previous chapter, it had been the practice of numerous observers to characterize modern developments as so connecting different economies that an individual country would no longer be able to exercise meaningful monetary control over, or determination of, its own inflation rate. Friedman had rejected these arguments in the past, and he continued to do so vigorously in the 1990s and 2000s.

The distinction between financial linkages and monetary linkages was important in this connection. Friedman and Schwartz had conceded that international capital mobility (when it was permitted to occur) produced what they called the "financial unification" of different nations.[217] But, under floating exchange rates, financial unification did *not* imply an end to the central bank's control over monetary variables or the demise of its influence on economy-wide nominal aggregates. The Federal Reserve continued to have control over the monetary base even in an interdependent world financial system, Friedman stressed, and he noted that this monopoly power endowed it both with the short-run power to ability the federal funds rate and, ultimately, with its wherewithal to set the ongoing rate of inflation (*Wall Street Journal*, July 5, 1990). As mentioned in Chapter 16, in February 1993 he subsequently upbraided the *Wall Street Journal* for attributing, in a news piece, 1992's decline in inflation to developments other than U.S. monetary conditions. Global factors had been among the alleged major drivers of inflation that the news piece had emphasized.

Many other monetary economists—even those who, by the mid-1990s, had parted ways with Friedman on the matter of emphasizing monetary aggregates—shared his position that monetary analysis was largely cordoned off from the issue of globalization. For example, Christina Romer contended (in Parkin, 1996, p. 516) that "the effects of globalization on the U.S. macroeconomy are greatly overstated," while maintaining that "integration has not fundamentally altered… the effects of macroeconomic policy." Likewise, Woodford (2009) provided a formal analysis establishing that, in conditions of floating exchange rates, monetary policy autonomy, including the ability to set the short-term interest rate and influence economic activity and inflation, prevailed in an open economy even in a situation of globalization.[218]

This judgment left monetary policy as potent as it had ever been, and it was therefore appropriate to assess how the Federal Reserve was using its powers and use economic outcomes as the criteria for this assessment. By early 1995, in contrast to his concerns in 1992−1993 that monetary policy was too tight, Friedman was very pleased with Alan Greenspan's performance as Federal Reserve Chairman. Of Greenspan's recent record, he contended that "you have to give it very high marks," citing the "rather steady course" of monetary growth and low and steady inflation.[219]

Nearly three years later, on September 5, 1997, when making introductory remarks for a Greenspan appearance at a Stanford University conference, Friedman went much further—

[217] Friedman and Schwartz (1982, p. 311).

[218] See also Nelson (2020c) for an analysis that concludes that modern open-economy models support 1953-vintage Friedman propositions concerning the connection between monetary policy's autonomy and floating exchange rates.

[219] *Louis Rukeyeser's 1995 Money Guid*e, Maryland Public Television, January 1, 1995, p. 4 of transcript.

declaring Greenspan to be the best Federal Reserve Chairman ever. Greenspan was not expecting Friedman to say this. "It came as a surprise. I bit my tongue, because I didn't want to argue with him," Greenspan quipped when recalling the Friedman remark (Alan Greenspan, interview, August 19, 2013).

Four-quarter GDP deflator inflation had, by this time, been around 2 percent for about five years; the corresponding rate of measured CPI inflation had been consistently above 2 percent, but even it fell below 2 percent in the fourth quarter of 1997. A television interview that Friedman gave in January 1998 again reflected his satisfaction with the Greenspan record: "Right now, inflation is relatively low. Alan Greenspan and the Federal Reserve have done a good job of keeping monetary growth low and fairly steady." (*Moneyline,* CNN, January 23, 1998).[220]

The Greenspan performance continued to impress Friedman in the following eight years. Friedman did register dissatisfaction on occasion. After Greenspan had, in December 1996, expressed concern about what he called the "irrational exuberance" of the stock market (see Greenspan, 1996), Friedman was unhappy and said so publicly—stating that although Greenspan was a friend and doing a "fine job," it had been a "great mistake" for the chair to have commented on stock market valuations (Zak, 1999, p. 13).

Although Friedman's criticism was centered on a position about Greenspan's appropriate public role—that is, that no Federal Reserve chair should comment on equity prices—his dissatisfaction with Greenspan's statement on equity prices may, as of 1996–1997, also have reflected doubt on Friedman's part that the stock market really was overvalued. If this was an area of disagreement with Greenspan, it would melt away as the stock market indices continued to climb, and in the spring of 1999 Friedman himself concurred that stock prices had far exceeded economically-justified values (*Forbes*, May 3, 1999*b*).

Friedman believed, however, that this was not a development that should give rise to attempts by monetary policy to remedy it, and he seemed gratified that, following the Federal Reserve chair's 1996 expression of concern, Greenspan did not attempt to burst the bubble via monetary policy (Pringle, 2002, p. 19). "Central banks ought not to be concerned with what happens in the stock market," Friedman remarked, while adding that he did not object to the stock prices being

[220] In this interview, Friedman's interlocutor was Lou Dobbs. Like Charlie Rose, Dobbs was not considered a very controversial broadcaster in the period when he interviewed Friedman. With regard to both Dobbs and Rose, this situation would change in the course of the 2000s and 2010s.

included among the variables that policymakers might use in forecasting inflation.[221]

Friedman believed, despite his tendency to discount the behavior of the stock market, that some of equity prices' strength reflected economic forces, including the influence of monetary policy: "there is a very good argument that in retrospect the Fed was too expansive during the late 1990s."[222] Indeed, this was another point on which Friedman was critical of Greenspan's record, as Friedman believed that M2 growth had been allowed to rise too much in the late 1990s.[223] But neither M2 growth nor the emergence of the New Economy could, Friedman concurred, really rationalize the scale of the strength of the stock market in the late 1990s.

The so-called "tech bubble" in equity market valuations reached a crescendo in early 2000. The Dow Jones index peaked in January 2000. The S&P index—which Friedman tended to use when he studied equity price behavior—followed in March.[224] What policymakers then faced, Friedman would observe about eighteen months later, was the aftermath of an "excessively bullish stock market" that had now turned sour (United Press International, October 9, 2001).

"The monetary policy Greenspan is following I think really has no precedent," Friedman remarked on this occasion, adding that he had a "great deal of sympathy" with the interest-rate cuts that Greenspan had initiated in January 2001 (United Press International, October 9, 2001).

September 11

By the time of these remarks, the United States had been hit by the September 11, 2001, terrorist attacks. Interviewed two weeks after 9/11, Friedman seemed to want to adopt the posture, which he sporadically embraced from 1993 to 2006, that he was retired from detailed monetary policy commentary: "Alan Greenspan doesn't need my advice… He's a very smart fellow… And I am a great admirer of Alan Greenspan. I think he's done an excellent job." (*Uncommon Knowledge*, Hoover Institution, September 25, 2001, p. 7 of transcript.)

But, in another interview two weeks on, Friedman did comment on the appropriateness of the interest-rate reductions seen since 9/11. Whether these were sufficient, he remarked, depended on "what their effect has been on monetary growth." But he highlighted a consideration that

[221] Pringle (2002, p. 19). The quotation also appeared in *The Independent* (London), August 13, 2002.
[222] Pringle (2002, p. 19).
[223] See, for example, Friedman (2005b, pp. 147–148).
[224] See, for example, Friedman (1988a, 2005b) for his deployment of this index.

would intensify difficulties in interpreting M2: "there's no doubt one of the effects of September 11 will be to increase the demand for cash balances." (United Press International, October 9, 2001.) The powerful effect of national emergencies in increasing the demand for real money balances (equivalently, reducing velocity) was, of course, a longstanding Friedman theme.

The following January, he updated his analysis in an op-ed: "While the current rate of monetary growth of more than 10 percent is sustainable and desirable as a defense against economic contraction and in reaction to the events of Sept. 11, continuation of anything like the rate of monetary growth will ensure that inflation rears its ugly head again." (*Wall Street Journal*, January 22, 2002.)

With regard to 9/11 as a shock to the United States, Friedman remarked in his late-September interview, "I think that Mr. Bush did an excellent job of rallying the nation … And I believe what the duty of the nation is to hunt down the people who are responsible for this… and get rid of them, close them down… In terms of what he's doing in the area of [actions against] the terrorists [and] what he's doing in the area of trying to hold up the spirits of the country, those are all fine." (*Uncommon Knowledge*, Hoover Institution, September 25, 2001, pp. 4, 7 of transcript.) But he also expressed dismay at the apparent policy reaction of "dumping money right and left" in domestic federal outlays (*Uncommon Knowledge*, Hoover Institution, September 25, 2001, p. 4 of transcript). Not only were some of the specific outlays uncalled for in his view, but he also felt that they did not have merit as measures intended to provide economic stabilization: "there is nothing to be gained by having government spend more money. I think we don't really want the legacy of September 11 to be a larger federal government." The U.S. economy was in recession already, he granted. "Undoubtedly September 11 will make it deeper and longer… But with the amount of monetary stimulus you have, there is no reason why the economy should not turn around sometime next year, first or second quarter perhaps." (United Press International, October 9, 2001.) The recession trough would actually be judged to have occurred still earlier than this—in November 2001.[225]

As discussed earlier in this chapter, to Friedman's dismay, on the domestic scene 9/11 was indeed followed by a substantial increase in federal spending.[226] On the international scene,

[225] See *https://www.nber.org/research/data/us-business-cycle-expansions-and-contractions*.

[226] In the business sector, one effect of 9/11 was an upsurge in the use of videoconferencing (although not to the scale that would be reached in the 2020s). This proved convenient for the always reluctant-to-travel Friedman. "For at least a couple of years before his death Professor Friedman made ample use of a studio near his apartment to do

Friedman drew a link between 9/11 and his belief in market economics: “Economic freedom advances economic growth, reduces poverty and promotes other civil and political freedoms. It is also a tonic against terrorism because of the opportunity it creates. All of the nations behind global terrorism lack economic freedom.”[227]

M2 and economic activity

As detailed in the discussion below titled “John Taylor,” Friedman would come to the conclusion in 2005 that the U.S. monetary policy response during 2001, both before and after September 11, had been of the right magnitude. The behavior of M2 growth heavily shaped this assessment. Nevertheless, although he continued to emphasize money in his analysis of monetary policy right through to the end of his life, Friedman had, by the early 2000s, himself expressed heavy reservations about the value of M2, and monetary aggregates in general, as indicators—reservations that, previously, he had rarely voiced to the same degree.

As of 1997−1998, as we have seen, Friedman’s favorable evaluation of the Greenspan record at the Federal Reserve rested partly on the moderation and stability of monetary growth (in particular, growth in M2) observed during the 1990s. The mean and variability of M2 growth did indeed decline under Greenspan: on annual data, M2 growth had a mean of 8.9 percent over 1970−1987, with a standard deviation of 2.4 percent. In contrast, over 1988−1997—roughly Greenspan’s first decade in office—its mean was 3.5 percent, with a standard deviation of 1.8 percent. For 1988−2006—roughly speaking, the whole Greenspan period—the corresponding figures were a mean of 4.9 percent and a standard deviation of 2.2 percent.[228] It was on the basis of numbers such as these that Friedman frequently praised the Greenspan record.[229]

But, as Friedman’s 2001 remark about the relationship between money demand and economic and political uncertainty confirmed, he himself conceded that the liquidity-preference picture was sufficiently complicated that M2 growth could not be taken at face value as a measure of

videoconferencing or lectures rather than traveling to the various places.” (Gloria Valentine, personal communication, July 11, 2013.)

[227] From Friedman’s endorsement of Gwartney and Lawson (2004).

[228] These calculations, like those for Table 2 below, are obtained using percentage changes in annual averages of the M2 data, with the annual averages drawn from quarterly averages that have been rescaled, starting in 1982-1983, using Friedman’s (1988a) adjustment for the introduction of money market deposit accounts.

[229] An early instance in which Friedman praised Greenspan’s record on the dimension of volatility was in *Forbes* (August 17, 1992, p. 42), when he stated that “Greenspan has been more successful than Volcker in avoiding excessive volatility in the money supply.”

monetary conditions or policy stance. U.S. money demand did indeed undergo an apparent upward shift after 9/11.[230] By this point, M2 had already lost a great deal of attention. This was the case because the 2001 shift came in the wake of events that had led many commentators to discount the importance of M2 data. As late as 1993, it was accurate to refer, as Peter Ireland did on the basis of multiple studies, to "the stability of the demand function" for real M2.[231] But it would become increasingly clear that a large, protracted downward shift in the demand for M2 had occurred in the 1990s—one that forced both policymakers and, in time, Friedman himself to reconsider the stability of velocity and of relationships between M2 and economic aggregates.

As discussed in Chapter 16, the Federal Reserve, in staff analysis, had encountered problems with accounting for M2 demand behavior starting in 1990. These money demand anomalies did not initially give rise to outright disaffection with M2 in policymaking: Greenspan's FOMC was still citing M2 behavior as importantly informing its decisions in 1991 and 1992. Indeed, some key aspects of the relationship between M2 and aggregate economic behavior still seemed to be clearly present over the first few years of the 1990s. Notably, the level of M2 velocity remained in its historical range in 1991 and 1992. Furthermore, the weakness of M2 growth in the early 1990s seemed for a good while to give a reasonable picture of the weakness of real GDP growth in that period. Laurent (1999) found that the predictive content of real M2 growth increased from 1989 to 1993, and Anna Schwartz (in *The Region* (Federal Reserve Bank of Minneapolis), September 1993) suggested that weak M2 growth was a key to understanding what was perceived at that time as a disappointingly slow economic recovery.

But these aspects of the M2-national income relationship themselves underwent major change as the 1990s progressed. By the end of 1992, the shortfall of actual real balances below the historical relationship was clearly manifested in an elevated level of M2 velocity—see Figure 1 below, as well as Friedman's plot in Taylor (2001, p. 104)—and Miyau (1996, p. 379) would subsequently assess that "particularly after 1991… M2 velocity may not be treated as a mean-reverting series." The relationship between growth rates of nominal GDP and nominal M2 was also visibly much poorer after 1992 (see Figure 2). Friedman himself would later plot real GDP growth against real M2 growth and show that they exhibited little connection after 1992 (see Taylor, 2001, p. 103).[232]

[230] See, for example, Reynard (2007).

[231] Ireland (1993, p. 30).

[232] By this point, Friedman—a longtime skeptic about using series on real monetary growth in cyclical analysis—had been persuaded, in part through discussions with Michael Mork, that there was merit in using real M2 growth, as opposed to nominal M2 growth, in short-run analysis.

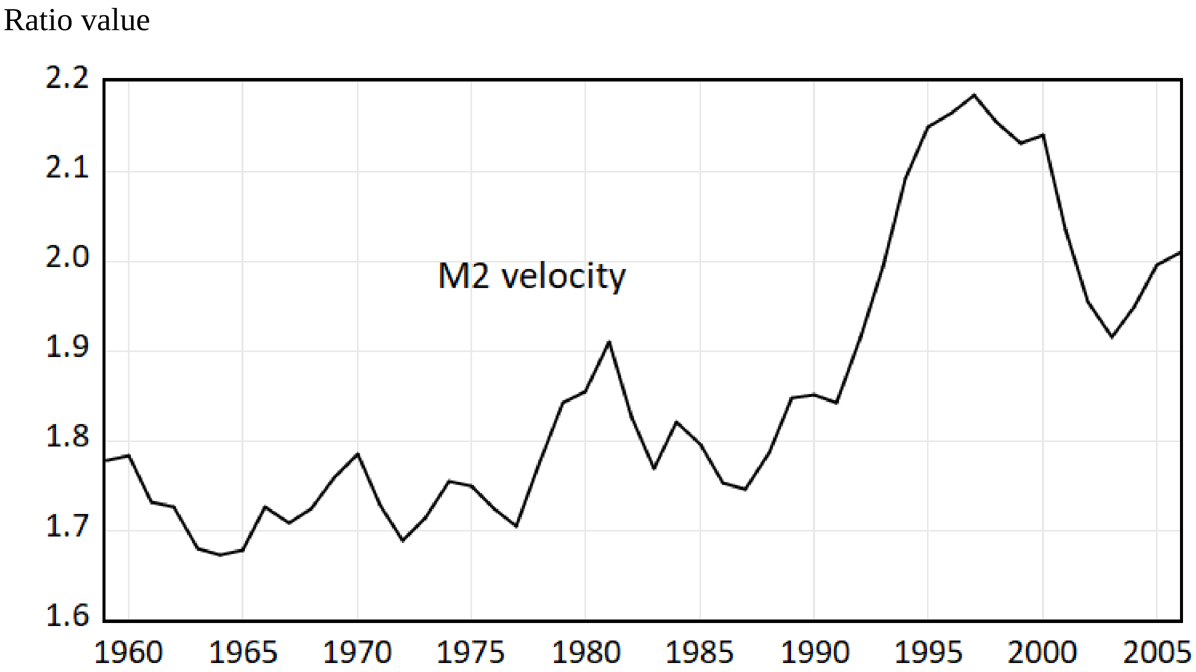


Figure 1. Velocity of M2, quarterly average, 1960–2006.
Source: Federal Reserve Bank of St. Louis' FRED portal.

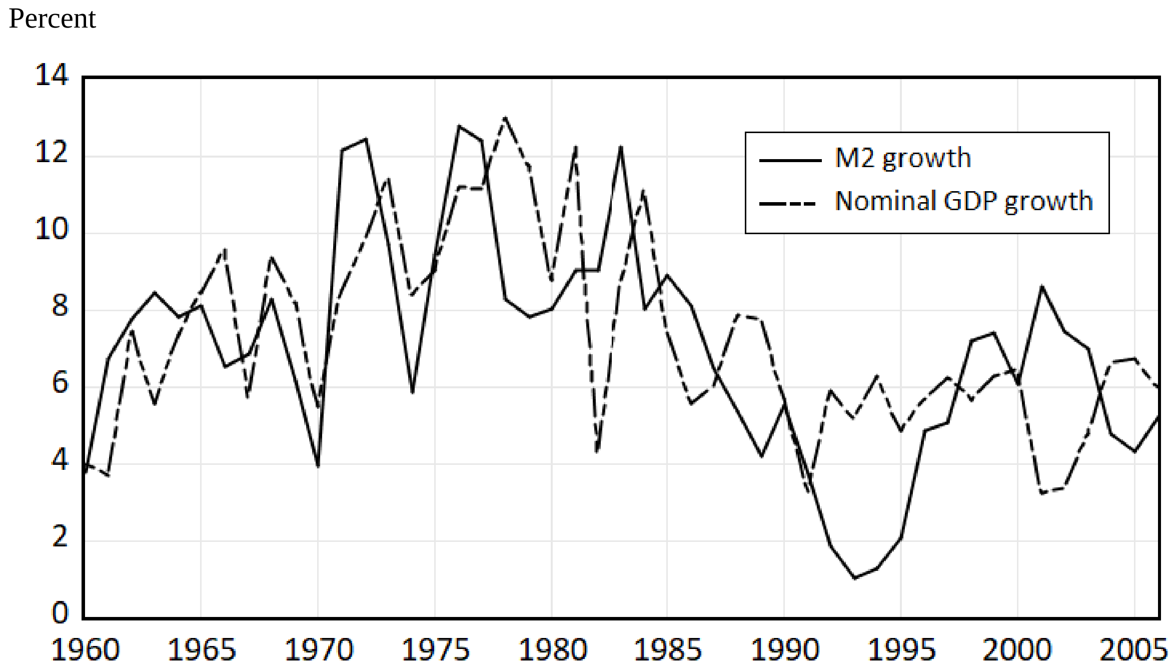


Figure 2. M2 growth and nominal GDP growth, annual data, 1960–2006.
Source: Federal Reserve Bank of St. Louis' FRED portal.

As Greenspan would recount in the September 1997 speech that Friedman introduced (Greenspan, 1997): "By 1993, this extraordinary velocity behavior had become so pronounced that the Federal Reserve was forced to begin disregarding the signals M2 was sending, at least for the time being." The Federal Reserve Board's February 1993 *Monetary Policy Report* noted that nominal GDP growth had risen in 1992 even though M2 growth had fallen, and it recorded that the FOMC, in seeking a lower M2 target range for 1993, was assuming that velocity would rise further.[233] And whereas the February 1993 report continued to use the "target" terminology when describing the M2 growth ranges, this terminology was absent from the August 1993 report.[234] The FOMC ceased stating monetary-growth ranges once the legal requirement to give these ranges lapsed in 2000 (with the expiration of the Humphrey-Hawkins law).

As indicated above, the instability of the M2-and-income relationship showed up on multiple fronts. The velocity plot in Figure 1 showed the shift in terms of the levels of nominal money and nominal income. On occasions in the past, shifts in the levels relationship had not necessarily been associated with much interruption in the relationship between the growth rates of these series. This pattern was, not, however, repeated in the case of the M2/income relationship in the 1990s. Rather, as Figure 2 indicated, on annual data, the connection between the growth rates of M2 and nominal GDP clearly showed a distinct, and enduring, weakening.

Another dimension of the shift in the growth-rates relationship is brought out by considering Friedman's generalization (given in the *Wall Street Journal*, February 12, 1987) that the growth rate of nominal money per unit of output—which, put in GDP terms, amounts to the difference between the growth rates of M2 and real GDP—had been consistently positive each year in the quarter century from 1962 to 1986. On annual data, this remained the case until 2010—except for the years from 1992 to 1995, for which it was negative.[235] The behavior in 1992–1995 highlighted a fact that Friedman himself would stress: that the relationship between M2 and income was loose after 1992 on an ongoing basis but that the three or four years through 1995 created *especially* notable aberrations in the relationship.

As the rise in velocity became more apparent from 1992 onward, Friedman moved away from his earlier emphasis on the stability of M2 velocity and acknowledged the shift. His comments

[233] Federal Reserve Board (1993, pp. 169-170).

[234] Federal Reserve Board (1993b, pp. 827-845).

[235] The 2010 shortfall of 0.2 percent of M2 growth below real GDP growth was less jarring from a monetarist perspective, as it could be seen as a correction of a large excess of M2 growth over real GDP growth in 2009.

on the Greenspan regime in an interview in an interview for *Barron's* (August 24, 1998) brought out his new perspective. Friedman again praised the outcome of "the lowest and least variable rate of monetary growth" in comparison with earlier periods (p. 31). But he also acknowledged some weaknesses of M2 as an indicator during the 1990s (p. 32): "I think there is no doubt that in the '90s, from '92 to '95, around there, there was a very sharp uptick in the velocity of M2 and that targeting money supply at that time in a rigid fashion would not have been a good thing to do… I only say in retrospect that Greenspan did the right thing in abandoning primary reliance on M2 during that period."

The degree of the shift

Even thirty years after the fact, research on money demand has not found a really satisfactory account for the shift in M2 velocity that occurred during the early and mid-1990s. The most promising explanation seems to be one advanced early on: the availability of instruments offering rates linked to longer-term yields, which were elevated in relation to shorter-term yields. It has proved difficult, however, to capture this change via a stable and parsimonious money demand function.[236] In addition, the puzzle arising from the strength of M2 velocity in the mid-1990s was followed by periods of sharp shifts down in M2 velocity in the 2000s. The upshot is that, although the factors accounting for M2 velocity's behavior during the 1990s and 2000s—notably the behavior of long-term yields and, in the 2000s, enhanced uncertainty—might not have been novel elements, as they were among those that Friedman had specified as drivers of velocity in his past analysis of the demand for money, the difficulties of modeling the demand for money unambiguously increased after the late 1980s, even when one used an M2 rather than the—by this point, very poorly behaved—M1 aggregate.

As Friedman's August 1998 comments, quoted above, suggested, and as indicated in Figure 1, it was the case that—starting in about 1995—M2 velocity retraced much of its prior rise. As the spike in velocity receded, Friedman warned that the instability of M2 velocity should not be

[236] See Duca (1993), and Carlson, Hoffman, Keen, and Rasche (2000). Another explanation advanced is that recorded M2 was pushed down by supply-side factors. For example, Spence and Lohn (1994) argued that time deposits had become more akin to managed liabilities in the 1990s and that the retrenchment of M2 balances reflected the more general credit crunch on the part of banks. This may tie in with another explanation, to the effect that the deposits in M2 that are most closely linked to market rates should be excluded from the aggregate, and that the money series excluding these deposits has a more-stable money demand function even though its velocity is historically less stable than that of M2 (see Reynard, 2004). The present author's view is that, as M2 is essentially a retail deposit aggregate, deposit-supply-based explanations are not the key to understanding the M2 velocity problems of the 1990s, while the pre-1992 stability of M2 velocity is too striking to justify Reynard's characterization of that stability as being a spurious phenomenon.

overstated (Parker, 2002, p. 54), and consistent with this characterization, the 1990s as a whole showed a reasonably close match between the means of nominal income growth and M2 growth. A comparison between 1963–1992 and 1993–2006 shows that the series broadly moved together: a 2.5 percentage point decline in M2 growth across the two periods, and a 2.8 percentage point decline in nominal income growth. See Table 1.

The rough continuation through the late 2000s of the mean relationship between M2 and nominal income overstates the extent to which the money-nominal income connection survived, as there was a seemingly permanent worsening of the shorter-term relationship between the two series.

As Table 2 shows, the correlation between M2 and GDP growth (both real and nominal) deteriorated—moving from positive and significant in the thirty years to 1992, to mildly negative and insignificant in 1993–2006. For example, the correlation between annual nominal income growth and the average of the same year's and the prior year's M2 growth was 0.61 in 1963–1992, but it was –0.24 in 1993–2006.

Over a longer period, even the unconditional-mean relationship between the growth rates of M2 and nominal GDP disappeared. As Table 3 shows, the two series had close to the same mean as one another in the quarter-century periods 1960–1984 and 1985–2009.[237] Subsequently, however—from 2010 to 2022—average M2 growth exceeded average nominal GDP growth by three percentage points.

Inflation and prior monetary growth continued to be highly correlated on spans of data that included the 1990s and early 2000s.[238] But inflation and prior monetary growth then had an approximately zero correlation over the first couple of decades of the twenty-first century.

The behavior of the standard deviations of the M2 growth and nominal GDP growth, discussed now, provides a further sign of the alteration in the relationship between money and income.

[237] The rough preservation of the average relationship between M2 and nominal GNP also underlay Greenspan's observation that, from 1959 to 2003, M2 per unit of output had grown at 3.7 percent per year and the GDP deflator at 3.8 percent per year, a phenomenon that led Greenspan to conclude, albeit in a footnote (Greenspan, 2003, p. 7), "in the tradition of Milton Friedman, that "it is difficult to disregard the long-run relationship between money and prices." (These percentage increases are the same when modern annual data from 1960 to 2002 are used.)

[238] See Orphanides and Porter (2001), Leeper and Roush (2003), Christiano and Fitzgerald (2000), Nelson (1998), Batini and Nelson (2001), and McCallum and Nelson (2011).

Table 1. Means and standard deviations of M2 growth and nominal GDP growth

	Means		Standard deviations	
	1963–1992	1993–2006	1963–1992	1993–2006
M2 growth	7.7	5.2	2.6	2.4
Nominal GDP growth	8.3	5.5	2.5	1.1
Real GDP growth	3.4	3.3	2.3	1.1

Note: Calculated using percentage changes in annual-average data (source: FRED portal). The annual-average observations for M2 are generated after adjusting the quarterly averages of M2 for the 1982–1983 introduction of MMDAs, discussed earlier.

Table 2. Correlation between M2 growth and nominal income growth

	Correlation with nominal GDP growth		Correlation with real GDP growth	
	1963–1992	1993–2006	1963–1992	1993–2006
Contemporaneous M2 growth	0.529	–0.328	0.273	–0.160
Lagged M2 growth	0.539	–0.142	0.304	–0.084
Two-year average M2 growth	0.605	–0.243	0.327	–0.126

Note: Calculated using data in Table 1.

Table 3. Mean percentage increases in nominal money and nominal income, United States, 1960 to 2022

	1960–1984	1985–2009	2010–2022
Nominal GDP growth	8.56	5.26	4.47
M2 growth	8.45	5.50	7.60

Note: Calculated using annual data in Tables 1 and 2, but not using the correction for MMDAs used in the calculations in those tables..

A recurring finding of the Friedman-Schwartz monetary studies was that nominal income growth was more variable over the cycle than monetary growth.[239] Although Friedman repeated this generalization as late as 1987 (*Wall Street Journal*, February 12, 1987), he and Schwartz had already noted that, in the postwar period, money was more variable than nominal income.[240] The latter finding also prevailed in the United States over 1963–1992: see Table 1. For that period, nominal income growth and M2 growth varied by comparable amounts—supporting Friedman and Schwartz's position, stated in *Monetary Trends*, that "we interpret fluctuations in the rate of change of nominal income as associated with fluctuations in the rate of change of money," as well as Friedman's observation in the same vein in 1968, in his well-known paper "The Role of Monetary Policy": "Periods of wide swings in the rate of monetary growth have also been periods of wide swings in economic activity."[241]

After 1992, however, although the variability of both nominal income growth and monetary growth did decrease together, the decrease in nominal income growth was far sharper (Table 2). In consequence, whereas in 1963–1992 the growth rates of nominal income and money had standard deviations that were quite similar in size, in 1993–2006 nominal income growth was less than half that of monetary growth.

Furthermore, the variability of nominal income growth, real GDP growth, and numerous other indices of aggregate economic activity became low in absolute terms. This phenomenon became

[239] See, for example, Friedman and Schwartz (1963a, p. 594;1963b, pp. 42, 43; 1982, p. 164).
[240] Friedman and Schwartz (1982, p. 404).
[241] The quotations are from Friedman and Schwartz (1982, p. 404) and Friedman (1968b, p. 16), respectively. See also Friedman and Schwartz (1963a, p. 286).

increasingly prominent in economists’ discussions in the early 2000s, as the Great Moderation came to be widely appreciated (see the references in Chapter 13 above). The fact that the 1990s had witnessed stable output growth in the absence of comparably low variability in M2 growth led Mankiw (2002, p. 33) to conclude that “the data give no support for the monetarist view that stability in the monetary aggregates is a prerequisite for economic stability.” While “no support” was certainly an overstatement—even Mankiw’s comparisons showed that M2 growth had been more stable in the two more recent full decades than in the 1970s—his point that the stabilization of output growth in the 1990s was out of proportion to that seen in monetary growth was correct.

The thermostat hypothesis

Part of Friedman’s acknowledgment in the second half of the 1990s of the problems with M2 demand involved his reaction to an unfolding of the Great Moderation, and early in the new century he noted that U.S. output in the 1990s, while growing strongly, had in cyclical terms tended to “vary little” compared with prior decades (Taylor, 2001, p. 129). The coincidence of M2 demand instability and lower aggregate economic fluctuations led to a major reconsideration on Friedman’s part of recent monetary policy behavior. The sharp decline in output variation in relation to monetary growth variability meant that it was not satisfactory to appeal solely, or even mainly, to a lower standard deviation of M2 growth in accounting for the economic stabilization achieved under Greenspan. A more fundamental break with past relations and practices had, evidently, occurred.

As Friedman tried to come to grips with recent developments, the area of monetary policy would become a rare instance in which he developed his thinking considerably in 1993–2006, instead of reiterating past positions. In the course of his reevaluation, he did not abandon a monetarist view of how the economy worked. But Friedman did change his view of what it was realistic to expect monetary policy to achieve. He would acknowledge that one could, on a sustained basis, do better in practice than a constant-monetary-growth rule, and not just in theory. As indicated above, by the end of the century he had cited U.S. monetary policy in the mid-1990s as an example of improving on the rule. Friedman would flesh out this judgment in his embrace of the thermostat hypothesis in the early 2000s.

What was the thermostat hypothesis? As applied to money/income relations, it was the phenomenon described by Alan Walters in a 1966 article: “If the [monetary] authority was perfectly successful then we should observe variations in the rate of change of the stock of money but not variations in the rate of change of income… [a]ssuming that the authority's objective is to stabilize the growth of income."[242] The basic idea behind this statement arose from the fact that if a variable *Y* has a stochastic relationship with a variable *X* that monetary policy can affect, a monetary policy strategy aimed at stabilizing *Y* will lead the policymaker to generate fluctuations in *X* in order to offset the effect on the aggregate variation of *Y* of the other determinants of *Y*. In the event of correct policymaker judgments concerning the determinants of *Y*, the consequence of the monetary policy designed to stabilize *Y* will be a weak reduced-form relationship—a low empirical correlation—between *X* and *Y*, despite the two series enjoying a structural connection with one another. The instrument *X* will have been varied in a manner to deliver stability of *Y*, and the *ex post* observation will be of fluctuations in the instrument *X* without corresponding fluctuations in the policy target *Y*, even though the behavior of *Y* very much reflects the choices made about monetary policy.

The idea was not due to Walters. Poole (1995, p. 135) argued: "This is a very old point, long a part of the optimal control and econometrics literatures." Poole was undoubtedly correct, although the specific reference that Poole (1995, p. 141) gave in this connection—Theil (1964)—actually *postdated* the appearance of this point in the monetary literature. The argument had already appeared in that literature in 1963.

Specifically, Kareken and Solow's (1963) study, produced under the auspices of the Commission on Money and Credit, had made the argument explicitly (p. 16). It is notable, in view of Friedman's later embrace of the argument, that when Kareken and Solow advanced it, they did so—as Peston (1972, p. 427) remarked—"in criticism of Friedman." Kareken and Solow had used it to discount the correlation- and timing-based parts of Friedman's work on money and income: they offered the case in which a stabilizing monetary policy eliminated the money/income relationship as a reason not to glean the effectiveness of monetary policy on the basis of simple correlations between money and income.[243]

The possibility that stabilization policy could result in a zero correlation between money and income (or their growth rates) thereafter appeared in print sporadically in later discussions in the monetary literature, including those of Sheppard (1971, p. 79), Dennis (1981, p 158), Blinder

[242] Walters (1966, p. 276). See also Walters (1970, p. 43).

[243] Fischer (1988b, p. 192) and Romer and Romer (1989, p. 122) also credited Kareken and Solow (1963) with the argument.

(1986, pp. 120–121), Bernanke (1986, p. 76), and Mankiw (1986, p. 243).[244] It was also a case noted by Bronfenbrenner (1965), in the course of building on Friedman's 1953 analysis of the effects of full-employment policy on national income—with Bronfenbrenner describing this complete-income-stabilization result as the "Superman case of complete perspicacity" on the part of the policymaker (p. 194).

Friedman's interest, prior to the 1990s, in this argument was decidedly limited. He acknowledged the validity of the Kareken-Solow argument as a matter of theory. Indeed, a paper that Christopher Sims published in the late 1970s acknowledged Friedman's coverage of the issue in comments that he gave to Sims on an earlier paper.[245] In addition, a graduate student in economics at the University of Chicago, Levis Kochin, worked in the 1970s on the applicability of the Kareken-Solow idea to the analysis of money-income relations and in 1975 produced a dissertation on the topic, partly under Friedman's supervision.[246] "My thesis was about endogenizing policy, which now comes naturally. But, then, none of the big models endogenized policy," Kochin recounted (interview, April 23, 2013).

It remained the case, however, that as an empirical matter, Friedman was very doubtful that a stabilizing monetary policy was what lay behind the observed U.S. data. He stated in 1984 that successful stabilization should mean that "high monetary variability… [is] associated with low economic variability." But he contended that the relevance of this point was contradicted by the positive relationship between monetary variability and output (and nominal income) variability in the data.[247] This assessment was, of course, consistent with the possibility that monetary policy routinely responded to the state of the economy but that the feedback policy was, as Friedman warned was likely, destabilizing in its net effect on output.[248] Consequently, his position—consistent with some of his theoretical analysis but also, Friedman felt, strongly confirmed by empirical studies—was that stabilization policy had often proved

[244] Blinder (1986) sourced the argument to Blinder and Goldfeld (1976), and Bernanke (1986) attributed it to Buiter (1984) and Sims (1972). The latter reference mentions the point briefly (p. 542)—with Sims (1977), discussed presently, offering a longer discussion.

[245] See Sims (1977, pp. 37–38). The comments to which Sims referred were those that Friedman had given on a version of Sims (1972) at a time when Sims was planning to put out a version of that study as an NBER paper.

[246] Kochin (1975), published in revised and abridged form as Kochin (1980). See also Kishor and Kochin (2004). Kochin (1980, p. 182) credited Kareken and Solow (1963) with the point linking successful stabilization policy and a zero money/income correlation. Friedman was not the formal supervisor of the 1973 Kochin dissertation but *de facto* was the principal adviser on the dissertation. Friedman cited the published version of Kochin's dissertation in Friedman (1984b, p. 58), and he also mentioned Kochin's work in the area in Taylor (2001, p. 120).

[247] Friedman (1984b, p. 34).

[248] The notion that Friedman (1953c) was a precursor of the Kareken-Solow argument was acknowledged by Peston (1972, p. 427).

counterproductive (see Nelson 2020b, Chapter 8).

Friedman consequently viewed historical monetary policy decisions as permeated by misjudgments about the state of the economy as well as more fundamental misconceptions about the relationships linking monetary policy, spending, and price-setting. The judgment flowing from this Friedman view was that it was empirically unrealistic to characterize U.S. monetary policy as having been highly stabilizing. Correspondingly, positive money/income correlations could be expected to be the norm.[249] Efforts to try to improve upon a simple rule had, in his view, not in practice delivered improved stabilization. It was therefore a simple rule, and not a rule designed to deliver optimal policy, that he offered in 1949 as one that would "build into our economic system… an automatic thermostat which would tend to turn itself on and off."[250] In 1949, this preferred rule had been his 1948 monetization rule. From 1956 onward, in contrast, the monetary policy rule upon which he believed U.S. practice was unlikely to improve was the constant-monetary-growth rule.

By mid-2000, however, in light of the much-reduced degree of output variability, Friedman had reached the view that stabilization policy *had* been executed successfully by the Federal Reserve in recent years, and that it was a major reason for high M2 growth variability in relation to output variability since 1992. In expounding the idea—both in an interview with John Taylor (2001, pp. 103–106, 129) and a couple of years later in a *Wall Street Journal* op-ed (August 19, 2003), Friedman made an analogy between successful stabilization policy and the behavior of a thermostat.

As indicated by the 1949 vintage Friedman's use of the analogy quoted above, he had used this imagery before. Indeed, it was imagery widely used in discussions of economic policy over the 1940s and 1950s. For example, a commentator on U.K. economic affairs, Anthony Crosland, wrote in 1956 that economic policy in practice would never achieve the ideal state in which "demand could be exactly set, as though by a thermostatic control, at just the right point."[251]

[249] Of course, once Friedman became a monetarist, his belief that money demand shocks were not a factor dominating the money-and-income relationship in the U.S. historical data was another key basis for this expectation.
[250] NBC (1949, p. 11). On this occasion, Friedman was picking up the thermostatic analogy from another panelist in the radio discussion. According to C.A. Phillips, T.F. McManus, and R.W. Nelson (1937, p. 120), the analogy between economic stabilization policy and thermostatic control had been advanced by Frank Knight. See also the discussion that follows.
[251] Crosland (1956, p. 398). Similarly, Warburton (1953, p. 10), in commenting on activist rules that Friedman had advanced in the 1940s, said that they were designed to be "a self-regulating mechanism, such as a thermostat…"

Stabilization policy had been pursued by U.S. policymakers for many decades—Friedman had dated its initiation to the first decade of existence of the Federal Reserve System—but, in his interpretation, it was only in the early 1990s that "they were able to install a good thermostat instead of a bad one."[252] U.S. monetary policy in the 1990s, as Friedman saw it, had come closer to achieving the "thermostatic" ideal than many, including himself, had thought practicable. In the interview with Taylor, Friedman cited the post-1992 higher variance of real M2 growth than of real output growth as evidence that a thermostatic (stabilizing) monetary policy had been in force. In 2003, he cited the improvement in inflation control since the 1980s, as well as the Federal Reserve's success in insulating the economy from the "veritable bubble in velocity" during the 1990s, as evidence of a thermostatic decision process (*Wall Street Journal*, August 19, 2003).[253]

Although the interview and op-ed in which Friedman exposited the thermostat hypothesis were published in high-profile outlets, it would be fair to say that his stand was largely ignored by practitioners and researchers in the monetary policy field. One reason is the obvious one: by the early 2000s, Friedman was in advanced old age and far away from the cutting edge of discussions of monetary policy. He still had close friends, including Greenspan, Jerry Jordan (president of the Federal Reserve Bank of Cleveland over 1993–2002), William Poole (president of the Federal Reserve Bank of St. Louis starting in 1998), and John Taylor, who were still very much immersed in the monetary policy world. Friedman's own familiarity with current monetary policy was nevertheless casual, and he was largely out of touch with the research literature on monetary economics.

A further reason for the lack of impact of Friedman's championing of the thermostat hypothesis was that, as far as many economists were concerned, monetary aggregates had been unreliable for a decade—or for multiple decades. These economists continued to focus on the M1 velocity break in 1982, and many were only vaguely familiar with Friedman's own retrospective accounts—in good part, material that appeared outside the economic-research literature—of

[252] Taylor (2001, p. 106).

[253] Friedman's August 2003 discussion of the 1990s velocity shift was marred by his apparent use of erroneous pre-1959 monetary data. The dataset that he was using led him to contend that M2 velocity in the postwar period had a historical upward trend before 1992—a characterization that contrasted jarringly with the stationarity that had been stressed in many past writings of himself and others as being a traditional property of M2 velocity. It also likely led him to understate the spike in velocity and overstate the extent to which velocity had since returned to normal levels. The footnote in Greenspan's (2003) talk, given ten days after Friedman's op-ed appeared, did not cite that op-ed but may have been alluding to staff difficulties in replicating Friedman's velocity chart, as the footnote devoted two sentences to the problems of constructing reliable M2 data before the commencement of the official series in 1959 (Greenspan, 2003, p. 7).

monetary developments during the 1980s and their focus on M2.[254] They therefore were likely to place little weight on accounts—such as Friedman's—of recent decades' monetary policy and its connection to the Great Moderation that were oriented on the behavior of the money stock.

To these factors—which were largely to do with marketing—that prevented Friedman from making much headway with the acceptance of the thermostat hypothesis, there should be added a further factor, one that has to do with substance. The thermostat hypothesis is weak as an explanation for the shifting money-income relationship. In fact, it would not be going too far to judge that it is a deeply unsatisfactory explanation. On three dimensions—as part of the research agenda on money, as a characterization of the theory of optimal policy, and as a description of monetary policy in practice—the hypothesis is found wanting. Let us consider each dimension in turn.

Viewed in terms of its implications for research on money, the thermostat hypothesis is superficial—indeed, complacent. The complacency of the hypothesis is clear: for *any* period in which the relationship between money and the economy weakens and the economy behaves satisfactorily, the thermostat story allows those who put stress on monetary aggregates to contend that the stability of the economy actually reflects policymakers' detailed knowledge of, and use as an input into their decisions of, a stable underlying relationship between money and income. Admittedly, Friedman did not push the hypothesis this far, as he conceded that the Great Moderation period had featured a major shift in the demand for M2. But the hypothesis nevertheless is prone to encourage the belief in the stability of the structural money/income relationship. It therefore offers no guidance about how to improve the measurement and monitoring of money.

In particular, the thermostat hypothesis does not help determine the choice between M1, M2, or other aggregates. Indeed, Poole (1995), in contrast to Friedman, used M1 rather than M2 when articulating his version of the thermostat hypothesis. M1, as we have seen, actually behaved very differently from M2 in the 1980s, and in the 1990s the divergences were magnified by the fact that M1 growth was heavily distorted by commercial banks' deposit-sweeping behavior (see

[254] For example, a paper on monetarism by DeLong (2000) neglected the stability of M2 velocity through the 1980s and emphasized the M1 velocity break—an emphasis that triggered a reproachful response by Friedman (2000). Likewise, interviewing Friedman in 2000, John Taylor was surprised to find that Friedman contended that the money-income relationship broke in 1992 (a contention that rested on Friedman's reliance on M2) rather than 1982: see Taylor (2001, p. 105). Even much later, Blinder (2022b, p. 86) seemingly views "Milton Friedman and M1" as being closely associated, with Friedman implied as having been a champion of M1.

R.G. Anderson, 1995). Yet because the thermostat hypothesis creates a *presumption* that the observed relationship between money and income will be weak, it allows researchers to view M1 as an appropriate definition of money even for periods like the 1990s, when some, or much, of the weak relationship between M1 and the economy reflected sweeps behavior rather than monetary policy behavior. By providing a blanket explanation for weak correlations between money and the economy, the thermostat hypothesis provides no guidance regarding the choice between different definitions of money. It therefore provides little incentive to researchers and data analysts to improve, or approach critically, monetary data.

The thermostat hypothesis is also unsatisfactory because it gives an oversimplified picture of control theory. There are actually grave doubts that optimal stabilization policy would eliminate the money/output or money/nominal income correlation. A zero correlation is not a logical implication of optimal policy. One consideration is that, as stressed by Kishor and Kochin (2007), policymaker uncertainty about the structure of the economy may give rise to nonzero correlations between money and other variables. But nonzero correlations may emerge even when policymakers know the economic structure. For example, tradeoffs in economic policy—even those purely on a short-run nature—may mean that neither nominal income nor output is completely stabilized and lead to the emergence of positive money/output correlation and money/income correlations. One frequently cited consideration, relevant here, is the possible tradeoff between the stabilization of real economic activity and stabilization of inflation that may emerge from the economy’s arrangements for setting nominal wages (on this matter, see especially Erceg, Henderson, and Levin, 2000). Even, however, in models that feature only price stickiness as their source of nominal rigidity and have no wage stickiness, a money/output correlation may emerge from the operation of optimal policy, on account of the presence of real shocks that move the natural level of output. Because the criterion for optimal policy is typically output-gap stabilization—that is, stabilization of output in relation to its natural level, not output in relation to a smooth trend—optimal monetary policy may give rise to a positive money/real GDP correlation, as the authorities accommodate shocks to potential output: Woodford (2003, p. 297) provided several illustrations that bring out this point.

The reasoning just sketched suggests that a positive correlation between money and (real and nominal income, and positive correlations of the growth rates of the series, should typically be expected to prevail under optimal stabilization policy. Therefore, the bivariate relationship between money and income may be contrary to the Friedman picture of what would result from a thermostatic policy. If, *ex post*, no such correlation is forthcoming, yet output and inflation have low standard deviations, one may well be justified in concluding that money demand shocks have been severe—perhaps reflecting problems in accurately measuring money.

The thermostat hypothesis, finally, gives a misleading picture of the actual conduct of monetary policy in the 1990s. Friedman's *Wall Street Journal* op-ed portrayed the Federal Reserve as using the equation of exchange to guide it to an appropriate stabilization policy. As Friedman earlier described it (*Barron's*, August 24, 1998, p. 31), he saw Greenspan as "threading his way through those [velocity] difficulties." In this vision, the Federal Reserve in the 1990s made accurate judgments about velocity movements, set monetary growth accordingly, and thereby secured smooth behavior of nominal income.[255] Even on those occasions when he acknowledged the Federal Reserve's use of an interest-rate instrument under Greenspan, Friedman still occasionally characterized the interest-rate decisions as *explicitly* motivated by a monetary growth choice.[256] This characterization of a money-oriented FOMC flew in the face of the fact that Federal Reserve staff analysis of monetary aggregates and of money demand did not become more refined and sophisticated during the Great Moderation period. On the contrary, it dissipated. It would be hard to find a period in which monetary aggregates have figured less prominently in U.S. monetary policy than in the period since 1992. An implication of this is that the thermostat hypothesis should be rejected as a positive-economics description of monetary policy.

A crucial shortcoming of the thermostat hypothesis as expounded by Friedman is one that is reflected in all the three preceding criticisms of the hypothesis laid out above; he largely sidestepped the choice of policy instrument. Friedman's August 2003 op-ed acknowledged that the federal funds rate as an instrument but then immediately recast policy choices as amounting to a choice for monetary growth. While, as he indicated, a decision on the federal funds rate does amount to an implied choice about monetary growth, by 2003 many practitioners and researchers would regard monetary policy analysis as usefully centered on interest rates. For one thing, they could note that the Federal Reserve's use of an interest-rate instrument allowed

[255] Arnold Harberger was even more explicit than Friedman in sketching this vision, stating (*The Region* (Federal Reserve Bank of Minneapolis), March 1999): "I think of Greenspan as having a whole corps of detectives doing detective work on the demand for money, trying to find out when it has shifted and when it has not."

[256] See, for example, his 2006 interview with Russ Roberts, as well as his letter of January 24, 2003, to Ben Cerruti saying, "As I understand the Fed's policy, it uses changes in the federal funds rate as an indirect way of chang[ing] the quantity of money." (Cerruti, 2014, p. 93.) See also the other instances given below. There is, of course, a certain rate of monetary growth implied by any specific decision regarding short-term interest rates. But in the 2006 interview, Friedman cast interest-rate decisions as expressly intended to deliver a particular rate of monetary growth. (Also, in contrast to his many past critiques of monetary policy using an interest-rate instrument, Friedman on this occasion gave the impression that the short-run mapping between interest rates and monetary growth was reliable.)

money demand shocks to be accommodated automatically—avoiding a situation in which policymakers had to identify these shocks and modulate the monetary growth rate accordingly. For another, they could point out that the literature on interest-rate rules had proceeded in leaps and bounds since the 1990s.

In this literature on short-term interest rates as a monetary policy instrument, one of the key figures, especially since 1993, had been Friedman's longtime colleague at the Hoover Institution, John Taylor.

PERSONALITIES, 1993–2006

JOHN TAYLOR

Having very recently received his Ph.D. from Stanford University, 26-year-old John Taylor was a presenter in the final session of a conference on stochastic control held at the University of Chicago's business school on June 7–9, 1973 (Chow and Atheos, 1974, pp. 1, 9). This was the time of year during which Friedman was away from the city of Chicago. Before the conference had started, he and Rose Friedman had already shifted location, to what, at that time, was their regular summer base of Vermont (Instructional Dynamics Economics Cassette Tape 122, June 6, 1973).

The fact that, at this early point in his career, Taylor was visiting the University of Chicago campus during a period when Friedman was absent typified a pattern that would recur over much of the period through mid-1984. This pattern seen over these years was that Friedman and Taylor had heavily overlapping interests in economics but hardly encountered one another. The pattern occurred despite the fact that in this period, as was also the case in his previous years of undergraduate and graduate work, Taylor would meet and work with many friends of Friedman's. Taylor's Ph.D. supervisor was one-time Friedman coauthor, statistician T.W. Anderson; Arnold Zellner was session chair at Taylor's presentation at the June 1973 University of Chicago conference; a colleague of Taylor's during his 1973−1980 appointment at Columbia University was Phillip Cagan; and it was Cagan who helped arrange Taylor's 1976−1977 service on the staff of the Council of Economic Advisers, initially headed by Alan Greenspan—with whom Taylor also worked for a while from 1977 onward, after Greenspan had returned to economic-consulting activity in New York City following the end of the Ford Administration.[257]

Taylor only had lengthy encounters with Friedman in the latter's post-University of Chicago years. Their interaction became frequent once Taylor moved to Stanford University in 1984. Taylor's appointment to the university "was originally just at the economics department. And then, gradually, I got more associated with Hoover over time." (John Taylor, interview, July 2, 2013.)

By the time he joined Stanford University's economics department, Taylor was—as has been discussed in earlier chapters of this book—well known in the economics world for research that brought nominal wage and price contracts into rational expectations models, thereby enhancing the scope in these models for short-run linkages between monetary policy actions and real economic activity. This work on contracts would become an important part of what came to be called New Keynesian economics—a term that Taylor himself was using as early as 1984—doing so when quoting the usage of it in Michael Parkin's undergraduate textbook.[258] Multi-period nominal contracts of the kind that Taylor had emphasized would continue to be a key feature of New Keynesian economics in the 1990s and 2000s.

The development of New Keynesian economics during the 1990s—including the respects in which the later version of New Keynesian economics both reflected, and diverged from, Friedman's monetary economics—will be discussed in more detail later in this section. One point worth noting at this stage is that, although New Keynesian models in the 1990s and 2000s differed from those of the 1980s in important respects—and in particular had firmer microeconomic foundations—they continued to exhibit the feature of their 1980s counterparts that they were typically small models—consisting (other than in the specifications of shocks) of systems that were well below ten equations. Indeed, as will be seen, Friedman would eventually find himself in the unaccustomed position of complaining that some of these systems were *too* small (because they omitted the stock of money).

Much of Taylor's research in the 1980s and 1990s used small models. But he did rely on them exclusively. Notably, and in contrast to numerous other proponents of the rational expectations revolution—as well as to Friedman's research interests—Taylor was, over this same period, involved in the development of larger-scale econometric models, of the type long associated with

[257] On Taylor's activities in the second half of the 1970s, see his remarks in Leeson (2012, pp. 315−316).
[258] Taylor (1986, p. 153), citing Parkin (1984b).

economic policy agencies.

Feast and famine cycle of econometric models from the 1960s to the 1990s

As is evident in Snowdon's (1997, p. 542) reference to "1960s-style Keynesian macroeconometric 'system of equation' models," large econometric models of the U.S. economy had enjoyed their heyday in the 1960s. And as Snowdon's description also indicates, these models were strongly associated with Keynesian economics. At the start of the 1960s, with pioneering work on macroeconometric models having been contributed by such figures as L.R. Klein who had been major developers of Keynesian analysis in the United States, these models were also noted for the low weight they assigned to monetary factors. Against this background, Jorgenson (1966, p. 78) wrote: "The impact of monetary and credit policy remains the main gap in current knowledge about the determinants of expenditures." Jorgenson also suggested that research by Keynesians through 1963 still suggested a "basic theme… that monetary and credit policy have little influence on the expenditures of consumers, businessmen, or state and local governments in the short run."

Judgments on this matter were, of course, drastically changing in the economics profession at the time of Jorgenson's remarks—in large part on account of the impetus provided by Friedman's work. The change would also be felt in the respecification of U.S. macroeconometric models during the 1960s. Niels Thygesen observed (interview, February 10, 2015): "in a way, Friedman provoked the very massive progress in macroeconomic model-building that took place in the '60s: the Brookings Project, the interest in the Federal Reserve in building a model, and of similar, other central banks, some of them advised, in fact, by Franco Modigliani and other, more Keynesian economists, and there was great attention [given] to detailed characterization of the transmission mechanism."

Modigliani—himself already more inclined to recognize important effects of monetary policy than many of his Keynesian contemporaries were—was, in particular, involved in the development of the FRB-MIT-Penn model used by the Federal Reserve Board. By the first half of the 1970s, these models implied that monetary policy played an important role in the behavior of output, and, in an application of the model that they presented at the 1974 conference on monetarism that Friedman attended, Modigliani and Ando (1976) characterized their main area of disagreement with monetarists on aggregate demand behavior as being on the monetarists' low rating of the importance of pure fiscal policy actions.[259]

Having made, by 1970, a partial accommodation with the monetarist movement, the large structural macroeconometric models would then be hit by three separate blows in the 1970s: the demonstration that these models forecast relatively poorly compared with the new time-series statistical approach (see, especially, C.R. Nelson, 1972); poor forecasts (judged in terms of the absolute magnitude of the errors) of the models over the period from 1973 onward of a renewed breakout in inflation; and the rational expectations literature, which made these models a prominent target. David Hendry, although sympathetic with the notion of developing macroeconometric systems, granted that the existing "systems mispredicted badly and their credibility fell sharply within the profession."[260]

Of the three blows to macroeconometric models, the last of them—the impact of the rational expectations revolution—would receive the most attention among economic researchers. Sims (1977, p. 30) had already referred, on the basis of the arguments articulated by Lucas (1976), to "this 'rational expectations' critique of [the] use of standard systems of behavioral equations."[261] But it was in June 1978 that a still more pointed critique of Keynesian econometric models appeared in a contribution by Robert Lucas and Thomas Sargent to a Federal Reserve Bank of Boston conference. Lucas and Sargent (1978, p. 55) suggested that the difficulty of separating expectations from other sources of dynamics "has extremely dire implications for the identification of existing macro models."

In the resulting conference volume, Lucas and Sargent's provocative discussion triggered strongly worded rebuttals, and defenses of the Keynesian position, by Benjamin Friedman (1978) and Robert Solow (1978), the latter likening the tone of the Lucas-Sargent paper to that of former vice president Spiro Agnew.[262] For his part, Milton Friedman had not been at the

[259] See also Rancan (2022) for an analysis of the evolution of the specification of this model.

[260] Hendry (1993, p. 119).

[261] Likewise, Leeper (1995, p. 313) would judge: "In many economists' minds, the critique provides the most plausible (that is, economically reasonable and empirically consistent) explanation of the empirical breakdown of econometric models." Leeper went on to cast the failure of econometric models (small and large), used in early-1970s research, that had failed to embed the Friedman-Phelps modification into their Phillips-curve specifications as the "outstanding example of an empirical breakdown" that demonstrated the Lucas critique's relevance (an assessment consistent also with Alogoskoufis and Smith, 1991, and McCallum, 2002, p. 80).

[262] See Solow (1978, p. 203). Solow recalled (interview, July 7, 2014): "I thought it was rather strange… And that was a noticeable thing—the tone of that paper was not the usual sort of thing at all." He added, however, that he had no objection to a strongly-worded or polemical paper appearing on occasion: "I don't know, people get irritated, I get irritated, so why shouldn't Bob Lucas get irritated?"

conference. But he later favorably quoted some of Lucas and Sargent's remarks about the forecast failures of Keynesian models, while also remarking himself on the frequency with which large econometric models had needed to be revamped after generating poor forecasts.[263]

John Taylor, likewise, had not attended the June 1978 conference. But within a few years of it, he would discuss Lucas and Sargent's (1978) critique of macroeconometric models in detail. That Taylor analysis (Taylor, 1980b, pp. 29–31; 1982b), although far less visceral in its reaction that Benjamin Friedman and Robert Solow had been, registered dissent from Lucas and Sargent. In part this was because, while agreeing with them on the need for the introduction of rational expectations in macroeconomic models, he opposed their continued advocacy of flexible-price models. As of 1975, Taylor had expected a rapid convergence to monetary policy models featuring rational expectations. He was subsequently disappointed to witness, instead, the large-scale splitting of economic researchers into two camps, neither of which he could bring himself to join: subscribers to Keynesian models that had sticky prices but no rational expectations; and proponents of rational expectations models that lacked sticky prices.[264] The latter camp, represented by new classical economics in the 1970s and early 1980s, would also be associated with a significant body of researchers from 1982 onward in the form of the real business cycle movement.

Taylor's disagreement with the Lucas-Sargent indictment also extended to the overall verdict on the viability of large econometric models. Taylor believed that these models, rather than being eschewed altogether, should be respecified in order to reflect developments in macroeconomics including rational expectations. In a section titled "Monetary Economics and Rational Expectations: An Overview" in his 1982 paper, Taylor observed: "It is now over ten years since an explicit method of analyzing endogenous or consistent expectations was introduced to macroeconomics under the name rational expectations. The original motivation came from the research of Edmund Phelps and Milton Friedman… [f]ocusing on the Phillips curve…" In the wake of the "fact that the Phelps—Friedman prediction seemed to come true so vividly in the 1970s," Taylor suggested that it was appropriate to make expectations fully endogenous, via the usage of rational expectations, across analyses in empirical monetary economics—including in the specification of macroeconometric models.[265]

[263] See the discussion titled "Otto Eckstein" in Chapter 13 above.
[264] See Taylor (1989, p. 186).
[265] Taylor (1982b, p. 50).

Taylor's own research in the 1980s reflected this agenda. With Ray Fair, he proposed an estimation-and-solution procedure for rational expectations systems (see Fair and Taylor, 1983). And he would recall that, after moving to Stanford University, he worked with "graduate students on a large econometrically estimated multi-country model" (Leeson, 2012, p. 321).

This is not the place for a detailed discussion of the travails that large econometric models went through from the mid-1970s to the 1990s. It is important to note, however, that when considering the economics profession as a whole—that is, including business economists and economists in policy institutions as well as academic institutions—it would be easy to overstate the degree to which they lapsed into disuse. For example, when he was a member of the Council of Economic Advisers in 1977–1980, Lyle Gramley, in making forecasts, drew on a set of macroeconometric models maintained in the private sector or elsewhere in the government: "We had the Fed model. We had the DRI model, the BEA model, and the Wharton [model]."[266] And in the 1980s, some commercial forecasting firms still maintained large-scale macroeconometric models: the examples of models maintained included the DRI model and those produced separately by economists such as Fair, Lawrence Meyer, and Michael Evans.

It had, nevertheless, become uncommon by the mid-1980s for economic researchers at leading universities to devote a major portion of their time to the development of large-scale econometric models. Those macroeconomic researchers who used large econometric systems were instead increasingly oriented toward using vector autoregression analysis. In VARs, the estimated equations largely took a uniform format, and the structural-modeling task (consisting of decomposing the VAR residuals) largely took place after most of the estimation had been done. Against this background, Taylor was unusual in continuing to work with structural macroeconometric models.

In the diminished, but not dormant, field of macroeconometric modeling, Taylor could record in the second half of the 1980s that things were moving in the direction that he favored. Much as he had advocated in Taylor (1982b), rational expectations had moved into the large econometric models, with Taylor (1989, p. 187) reporting that a 1986 Brookings Institution conference on large macroeconometric models, four of the twelve models used rational expectations. The process of the shifting perception of rational expectations—from something used by a particular school of macroeconomics to a standard approach used by mainstream macroeconomic modeling

[266] In Small and Slifman (2017, p. 17).

gave him considerable satisfaction and suggested that the initial "enormous resistance to rational expectations by Keynesians" was being worn down (Taylor, 1989, p. 186).

A decade after the 1986 conference, as discussant at a November 1996 Carnegie Mellon Conference on models for monetary policy, Taylor could express further satisfaction at what he called the culmination of a "paradigm shift": the Federal Reserve Board's new principal, staff-maintained model of the economy, the FRB/US model, included many forward-looking aspects in its equations and used rational expectations (Taylor, 1997, p.. 39). More specifically, this new model explicitly separated dynamics due to expectations from dynamics arising from other, specified sources, and the model could be simulated under the assumption that expectations were rational (see Brayton, Mauskopf, Reifschneider, Tinsley, and Williams, 1997).

In addition to incorporating Taylor's longstanding injunction to incorporate rational expectations, the FRB/US model incorporated various prominent elements of Friedman's framework. These elements included the predominance of monetary policy in determining inflation over time, as well as versions of the natural rate hypothesis and the permanent income hypothesis. In another respect, however, the FRB/US model would underscore the decidedly partial character of Friedman's success in bringing macroeconomists round to his own position. In common with other major models used in research and policymaking in the 1990s and beyond, the FRB/US model would assign a diminished role to, or exclude altogether, the money stock itself. This development in macroeconomic modeling—which was pervasive across the U.S. research and policy-analysis spheres—will be discussed further later.

In the case of the FRB/US model, the specification implied multiple channels of monetary policy. But whereas Friedman saw such channels as ways in which various interest rates, each related to the money stock, exerted influence on economic activity, the FRB/US model essentially had monetary policy's transmission to spending occurring *solely* via the federal funds rate. In defending this specification, Reifschneider, Tetlow, and Williams (1999, p. 3) took quantity-related transmission channels as requiring the operation of a real balance effect. They consequently cited the quantitatively minor place of the monetary base in total wealth as the basis for confining themselves to the federal funds rate channel. To monetarists, as well as to Keynesian exponents of general portfolio balance effects, like James Tobin, this was hardly an adequate response: the quantity effects (for a given baseline short-term interest rate) that these economists perceived as important were not wealth effects but, instead, a class of substitution effect: the influence that the relative proportions of financial assets outstanding could exert on the spreads between interest rates in a situation of imperfect asset substitution. From this perspective, the undisputed small nature of Pigou-Patinkin effects was not a sound basis for viewing monetary policy's effects as flowing solely via the federal funds rate.

Models like the FRB/US model could omit reference to money not only because the money stock did not play a role in their spending and asset-pricing equations but also because the monetary authority's behavior was specified in terms of setting of the federal funds rate. The prevalence by the late 1980s of models in which U.S. monetary policy was specified in terms of the federal funds rate owed a good deal to a Taylor contribution earlier in the decade—in the form of what became known as the Taylor rule.

Emergence of the Taylor rule, 1987–1992

Taylor would recall that, during the years after he moved to Stanford University, he "gradually started shifting from money supply rules to interest rate rules."[267]

"Determination of the level of short-term interest rates has traditionally been considered an important instrument of central bank policy," two Federal Reserve Board officials had written in 1947 (Thomas and Young, 1947, p. 102). Forty years on, however, the FOMC's reluctance to have itself publicly characterized as determining U.S. interest rates, as well as the legacy of the critiques by Friedman and others of policy strategies that entailed explicitly setting interest rates, had left the Federal Reserve entering the Greenspan era managing the federal funds rate but not being forthright in public statements about the fact of this management.

Other central banks in the world, however, were more open about the reality that they routinely intervened in financial markets with the express aim of managing short-term interest rates. John Taylor's immersion in the 1980s in the world of global central banking was a factor that likely helped tilt him toward thinking of monetary policy in interest-rate terms and doing so in a non-pejorative manner. His various advisory and fellowship affiliations included one with the Bank of Japan that began in 1987. After 1985, Friedman dropped out as a speaker in the Bank of Japan's biennial monetary policy conference and, after 1987, so did James Tobin. Starting in 1987, three more U.S. academics joined as recurring participants: Allan Meltzer, Stanley Fischer, and Taylor. Meltzer was clearly seen as the Friedman substitute, while Fischer and Taylor may

[267] In Leeson (2012, p. 321).

have been perceived as newer-generation Keynesians. In the event, Taylor—interacting with Friedman to an increased degree in this period—ended up being strongly associated with some major Friedman themes, including the value of monetary policy rules.[268] Late in the 1980s, Taylor did, however, break with Friedman on the choice of instrument to be used in the rule, by decisively aligning himself with the choice of an interest-rate instrument.

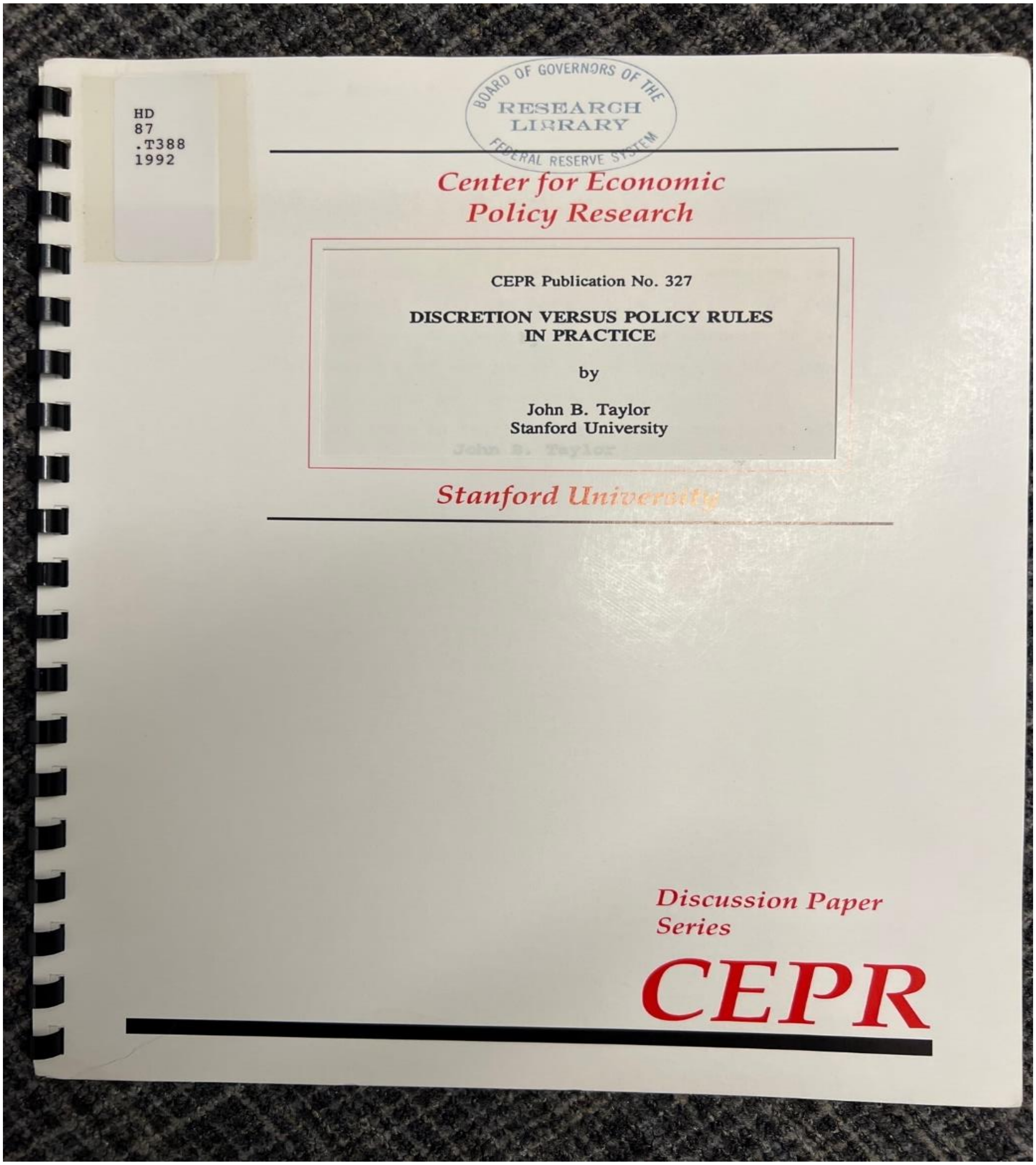
In addition to being a reflection of his exposure to international practice, this break was also partly due to Taylor's continuing interest during the early 1990s in large macroeconometric models. This interest, together with his continuing interactions with world central banks, led to his involvement in a multi-pronged project, spread across various sets of researchers, that led to the conference volume of Bryant, Hooper, and Mann (1993). The volume in question studied nine multicountry econometric models, most of them produced by policy agencies.

Soon after this project's completion, Taylor presented a paper on the first day of the Carnegie Rochester Conference in Pittsburgh on November 20–21, 1992 (Taylor, 1993). The rule that he analyzed in that paper—also released as a working paper during the month of its first presentation (Taylor, 1992b; see Figure 3)—was stimulated by the prior projects' results on the cross-model comparisons of the success of alternative policy rules. These results suggested that this particular rule performed well across models. In his Carnegie-Rochester paper, however, Taylor highlighted a further property of the rule. He found that it characterized well the first five years of Alan Greenspan's tenure as head of the Federal Reserve.

The rule in question consisted of an equation governing the setting of the federal funds rate, with the rate being adjusted in response to inflation (in relation to a 2 percent objective) with a coefficient of 1.5 and to the output gap with a coefficient of 0.5.

Several years later, after the Taylor rule had become part of the monetary policy idiom, Friedman remarked to Taylor, "I think it's almost impossible to predict what will be influential. You know that from your own work. You never dreamed when you presented the Taylor rule that it was going to become worldwide conventional wisdom."[269] At the Carnegie Rochester event, Taylor's paper was well received, but there was no hint of the sensation that the paper

[268] Taylor's interaction with Friedman was manifested in the fact that Taylor tended to know about and cite new Friedman publications soon after they appeared. For example, a paper that Taylor wrote for a July 1992 Reserve Bank of Australia conference cited Friedman's then very new book *Money Mischief* (Taylor, 1992a, pp. 12-13).
[269] In Taylor (2001, p. 129).



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DISCRETION VERSUS POLICY RULES
IN PRACTICE

by

John B. Taylor
Stanford University

Stanford University

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Figure 3. Cover of Taylor (1992b).

would eventually give rise to. As Taylor himself described it, he was trying in the paper to synthesize existing research findings by himself and others in recent years that had tried to make concrete the notion of monetary policy rules, while doing so in the context of federal funds rate policy and concentrating on a rule that had acceptable properties across various models.

The Taylor rule work would, in turn, generate a very large economic literature. In retrospect, the Taylor paper can be viewed as having been part of a surge of research activity starting in 1992 concerned with viewing U.S. monetary policy through the prism of rules—in particular, interest-rate rules.

The right-hand-side variables in the Taylor rule

A significant part of what Friedman called the "worldwide conventional wisdom" embedded in the Taylor rule was, of course, its use of an interest-rate instrument. The significance of this choice of left-hand-side variable in the rule is discussed at length below. It is worth dwelling first, however, on the rules' right-hand-side inputs—inflation and the output gap—and how they related to Friedman's thinking on these matters.

The interest-rate rules explored in the 1980s literature in such studies McCallum (1981) had largely limited themselves to the level of the money stock or the price level as the right-hand-side nominal variable.[270] In view, however, of the emphasis given to inflation, rather than to the absolute level of prices, in modern stabilization policy, an inflation term in the reaction function was also a logical candidate. Indeed, in a major theoretical analysis, Leeper (1991) had focused on nominal interest-rate rules in which inflation was the sole right-hand-side variable—that is, in which the monetary authority followed what would become known as a Taylor rule but with an output-gap response omitted. Leeper, in common with much of the later literature on the Taylor rule, had focused on the economic implications of responses to inflation below one and the contrast in economic outcomes with those associated with above-unity responses of the policy rate to inflation.

As far as Friedman was concerned, he had a longstanding objection to putting either the price level or the inflation rate into policy rules. This was that the reaction of inflation lagged monetary policy actions—and so policy actions taken in response to such reactions would not imply adjustments prompt enough to achieve a noninflationary stance.[271] By the 1990s, wide agreement prevailed in the economics profession that U.S. inflation typically took a year or more to register sizable responses to monetary policy actions. Consequently, there was force in the

[270] In good part, this focus stemmed from the fact that a major concern in this 1980s literature was the ability of an interest-rate rule to "pin down," or establish a specific value for, the price level.

[271] As of the 1990s, a fairly recent expression of this longstanding Friedman point had been Friedman and Friedman's (1984, p. 100; 1985, p. 98) statement that "using today's prices to determine today's monetary growth is like fighting the last war."

Friedman position that deviations of inflation from the central bank's objective largely reflected *past* monetary policy actions—and, therefore, responding to those deviations did amount to a belated course-correction. But this force was reduced somewhat in forward-looking rational expectations models of the kind that Taylor used. In these models, even a monetary policy response to past or current inflation rates was anticipated in advance by the private sector and so helped stabilize inflation before the policy action was actually taken.

As a description of the first five years of Greenspan's tenure, the Taylor rule cast new light on a suggestion that Friedman and some Keynesian critics of Greenspan's record so far had made: that monetary policy in the early 1990s had been too tight. A month before Taylor delivered his paper, Friedman had remarked (*Wall Street Journal*, October 23, 1992): "the Fed has temporarily overshot. Continuation of M2 growth at 2 percent per year would imply actual deflation, not negligible inflation." And a month after Taylor's conference presentation, Paul Samuelson testified about what he called "over-conservative monetary policy in the early 1990s," while adding: "The low grade earned in 1990–92 by our Fed is not unique to America. The 1990s have been bad years for rational central banking."[272] Taylor's evaluation contrasted with these judgments. His results suggested that the FOMC's policy rate adjustments over 1987–1992 had actually been approximately in line with a long-run inflation objective of 2 percent (as well as with a notable countercyclical response, via the federal funds rate feeding back on the output gap).

As has already been indicated, Friedman would later come to the view that monetary conditions in 1992 had not been as tight as he had believed at the time, as he did not anticipate the substantial upward shift in M2 velocity that started in 1992. But, notwithstanding Taylor's (1993) finding, the charge that Friedman and others had made against the Greenspan FOMC that it had been too tight—in 1991, in particular—likely did have considerable validity. The fact that the FOMC cut the federal funds rate in late 1991 and the first half of 1992 considerably below the prescription plotted in Taylor (1993) suggested that the Committee, to some extent, shared Friedman's and Samuelson's concern that monetary policy had, up to then, been too tight.[273] As the declining contour of inflation was broadly in line with what the FOMC had hoped for, its reductions in the funds rate in 1992 were likely principally motivated by concerns about real economic activity. Policymakers likely did not share Friedman's concern about "renewed

[272] From Samuelson's testimony of December 30, 1992, in Joint Economic Committee, U.S. Congress (1993, p. 11).
[273] The marked shortfall during 1992 of the federal funds rate below the Taylor rule's prescription was still evident when later revisions of the data were used. See Orphanides (2001, p. 971, Figure 3).

recession" (*Forbes*, August 17, 1992, p. 42), and they were discounting the evidence of M2 growth that he emphasized. But they probably did believe that financial conditions were tighter than what would be suggested by the level of the federal funds rate alone—and that the rate accordingly needed to be brought down substantially in order to forestall a future weakening in real economic activity.[274]

In the Taylor rule, the real-economic-activity term referred to current conditions, rather than consisting of a forecast. As with inflation, Friedman believed that current economic activity responded to monetary policy actions only with a lag, albeit a shorter lag than that pertaining to inflation. But Friedman's concern about the real-activity term in the Taylor rule did not center on lags. Instead, it lay in the fact that the Taylor rule responded to an estimate of the output gap. Friedman, of course, had grave reservations about responding to the level of the output gap. Taylor would recall of Friedman's reaction to the Taylor rule: "I think the notion of a rule he liked a lot. [But] I think he was very concerned about the gap. The measure of utilization was probably of the most concern to him." (John Taylor, interview, July 2, 2013).

Friedman could take some comfort from the fact that even the output-gap response in the Taylor rule reflected, after a fashion, the message from his work. The Taylor rule embodied a zero-output-gap target, in keeping with Taylor's (1988, p. 33) earlier remark: "I like to think of the ideal policy rule as minimizing the deviations of real output from normal or natural levels, with a correction for inflation." The zero-gap criterion was an obvious specification for Taylor because it captured the message of the natural rate hypothesis.[275] As already implied, Taylor had repeatedly endorsed this hypothesis in his writings. He had urged that models should incorporate into their specifications, and policymakers should take into account in their decisions, the notion that "the economy tends to return to the natural rate of unemployment" irrespective of the form of the monetary policy reaction function, and, conformably, "no long-term relationship exists between inflation and the deviation of real GDP from potential GDP."[276]

[274] As already indicated, long-term interest rates were high in this period, and in that light Samuelson's testimony had particularly highlighted the "very steep" yield curve (testimony of December 30, 1992, in Joint Economic Committee, U.S. Congress, 1993, p. 12). Friedman himself acknowledged: "Real interest rates for long-term money are very high by historical standards." (*Forbes*, August 17, 1992, p. 45.) This was basically consistent with the picture of tight monetary conditions given by M2 growth. Friedman maintained, however, that the relationship between the yield curve and real economic activity was less reliable over time and across countries than the money/income relationship (*Wall Street Journal*, October 23, 1992).

[275] After the present author expressed this view in earlier work (Nelson 2008, p. 96), Rivot (2012) registered strong disagreement. However, Rivot does not explain the reason for this disagreement, and, as indicated in the text above, this interpretation of the Taylor rule is easily supportable by considering Taylor's own words on the matter.

[276] These quotations are from Taylor (1987, p. 351) and Taylor (1994, p. 38), respectively.

Under circumstances in which monetary policy ultimately set the inflation rate, had a short-run influence on real income, and had no ambition or ability to determine the long-run level of real income, policymakers could, in the words of Bernanke and Mishkin (1997, p. 104)—in work discussed further under "Ben Bernanke" below—exercise "constrained discretion"—which was defined as the scope to stabilize the output gap in the short run without compromising the anchoring of inflation expectations.[277] Directing monetary policy toward output-gap stabilization, rather than toward an attempt to set the level of output, was a way in which central banks and the modern analysis of stabilization policy reconciled the existence of a real-activity goal for monetary policy with the natural rate hypothesis.[278]

Friedman could not take a great amount of solace from this way of looking at things because he was not someone who attributed past periods of inflation to deliberate policymaker pursuit of a positive output gap.[279] He certainly regarded an emphasis on the level of output in monetary policy as inappropriate.[280] But Friedman was not placated by the response that policymakers focused on output in relation to its natural level.

Friedman's objection to having the output gap in the policy rule instead centered on the likelihood of measurement errors with the output gap—a problem whose importance Taylor (1988, p. 33) had himself granted when he stated, with regard to any rule that entailed a reaction to the gap: "The main difficulty with this rule is determining what is the normal or natural level of output." Friedman had far less objection to a policy response to the *change* in real economic activity. A constant-monetary-growth rule entails an implicit interest-rate reaction to output growth (see Woodford, 2003, p. 109), and Friedman had pointed on many occasions—implicitly

[277] See Budd (1987, p. 190) for an earlier articulation of this idea. A hint of this idea appeared in a discussion by Friedman's long-time co-workers. Cagan and Schwartz (1975, p. 266) argued that if the central bank anchored expectations of the long-run monetary growth rate, it might thereby obtain some leeway to vary monetary growth in the short run for the purpose of stabilization of real economic activity.

[278] For further discussion, see Nelson (2020b, Chapter 13) and Chapter 8 above.

[279] The notion that policymakers in practice targeted a positive output gap had instead come from economists who believed, unlike Friedman, that belief in a long-run inflation/unemployment tradeoff had guided historical *policy* decisions (as distinct from academic analyses of inflation). The hypothesis that policymakers sought a positive output gap also figured in the time-consistency literature—a literature that emerged after Friedman had largely moved away from research. Taylor (1992a, pp. 14–15) was one of several monetary policy researchers in the 1990s who stressed that the time-consistency literature was very unlikely indeed to provide a convincing account of actual historical U.S. monetary policy behavior.

[280] See Nelson (2020b, Chapter 9). Friedman objected to the notion that FOMC participants should report real income forecasts because "[t]hey cannot control real income." (In House Republican Research Committee, 1984, p. 41.) (The real variable for which—summaries of—policymakers' forecasts on the level actually reported forecasts of growth rates of real income, though they also were reported in the 1980s was actually the unemployment rate rather than real income. See D.H. Romer, 2010.)

in some places, and explicitly in a few instances—to the desirability of relying on growth rates, rather than levels, of series (see Orphanides and Williams, 2013, and Nelson, 2020b, Chapter 9), The notion that monetary policy should refrain from responses to the level of the output gap had some support among those in the monetary policy research field in the 1990s and 2000s—see, for example, McCallum (2001), Orphanides (2003), and Orphanides and Williams (2013)—but was certainly a minority position among economists.

The interest rate instrument: the worlds of monetary policy and public commentary

The unveiling of the Taylor rule came in a period during which the Federal Reserve was becoming more overt about its usage of the federal funds rate as its main policy instrument. The staff documents that went to the FOMC before each policy meeting remained quite opaque until the end of the 1980s about the Committee's use of an interest-rate instrument. In this connection, Rotemberg (2013, p. 80) observed: "One has to wait until October 1989 to find a Bluebook [at the time, a document regularly prepared by Federal Reserve Board staff] that lays out policy alternatives in terms of levels of the federal funds rate."

Subsequently, in a public forum, Donald Kohn of the Federal Reserve Board's Division of Monetary Affairs explicitly discussed FOMC policy in terms of the federal funds rate—doing so in a conference (whose proceedings were promptly published) at the Reserve Bank of Australia in 1990 (Kohn, 1990; see also Nelson, 2021). And in the same month in which Taylor presented his policy-rules paper, David Lindsey, one of Kohn's senior staff, was at the Bank of Japan presenting the restricted-circulation paper on current monetary policy mentioned in Chapter 16 above. In his talk, delivered on November 2, 1992, Lindsey was blunt (p. 359): "Although several policy tools are manipulated by the Federal Reserve, the basic instrument can usefully be thought of as the federal funds rate—the overnight interest rate on interbank loans of reserves... The Federal Reserve itself, along with market participants, thinks of the federal funds rate as the main policy instrument." He noted that the interest-rate instrument had been reintroduced a decade earlier (p. 365): "Since late 1982... sustained, sizable movements in the federal funds rate have been the result of discretionary Federal Reserve decisions." With regard to the pattern of policy responses, Lindsey observed (p. 367): "U.S. monetary policy needs to be activist in adjusting the short-term interest-rate instrument when economic trends appear to be departing from objectives." The last of these remarks, at least, was in principle consistent with Friedman's recommendations concerning monetary policy, as the constant-monetary-growth rule always tended to imply a vigorous response of short-term interest rates to the economy.

Finally, from 1994 onward, the FOMC broke cover completely on the matter of its use of a federal funds rate instrument, by commencing its practice of issuing, after its meetings, a press statement in which it announced its decision—a decision expressed before long in terms of a target value for the federal funds rate. This practice, initially only followed when the policy decision involved a change in the rate target, had been extended by the end of the decade to cover all meetings, including those in which the Committee left its rate setting unchanged.

In the face of the growing acknowledgment of the Federal Reserve's use of a federal funds rate instrument, Friedman's observation in October 1992 was jarring: "The Federal Reserve cannot and does not control interest rates…" (*Wall Street Journal*, October 23, 1992). Coming just a month before Taylor presented his Taylor rule paper, such a remark must have appeared so badly out of touch as to be unintentionally humorous. As Friedman's surrounding remarks indicated, however, he was not actually denying that the FOMC had chosen a federal funds rate instrument and possessed the ability to set the value of the funds rate prevailing in financial markets. He was, instead, repeating his long-articulated point—one true of the models that Taylor and others were using—that the Federal Reserve's scope to influence the real-rate component of the nominal interest rate faded with the time horizon.[281] But his way of phrasing this point reflected Friedman's antipathy to viewing monetary policy decisions as choices about interest rates. "He certainly hated the targeting of interest rates," one of Friedman's former graduate students, Benjamin Klein, would recall (interview, March 4, 2013).

Although he never denied that monetary policy affected interest rates, Friedman's antipathy toward viewing monetary policy *as* interest-rate policy was deep.

This remained Friedman's posture to the end. But with John Taylor now a prominent champion of characterizing monetary policy through interest rates, Friedman had to pay more attention to the issue. "Milton was a very gregarious person—he was very, very interested in what I was doing," Taylor would recall (John Taylor interview, July 2, 2013). "I was flattered that he had any interest. He'd comment on papers, was always interested in the Taylor rule and things like that." Taylor had been exposed to Friedman's work since his undergraduate studies in the 1960s, and, in that context, had encountered *Capitalism and Freedom* and the *Monetary History*.

[281] Similarly, Friedman and Friedman's (1984, p. 100; 1985, p. 98) remark that the Federal Reserve had not been "successful… in controlling interest rates" was essentially a statement that past policies aimed at securing particular values for short-term nominal interest rates had ultimately led, via the Fisher effect, to higher nominal rates than these initially intended values.

Friedman's work on the consumption function and natural rate hypothesis also already made a major imprint on Taylor's research work in the 1970s. But in the construction of those models, another Friedman interest—monetary policy rules—figured importantly in Taylor's thinking, particularly in view of Taylor's focus on dynamic rational expectations models. "I would put it this way: in those kinds of models, you can't really think about policy without a rule." Monetary policy rules had even featured in Taylor's senior thesis work as an undergraduate. In the course pf working with dynamic models as part of his thesis, Taylor's interest in monetary policy rules had evolved from one motivated primarily by the "philosophical reasons" outlined in *Capitalism and Freedom* to the "operational reasons" associated with the need to complete a dynamic macroeconomic model (John Taylor, interview, July 2, 2013).

When Taylor's research moved on to the area of rational expectations and staggered nominal price contracts, the necessity for the specification of a monetary policy rule intensified. This was a setting in which it was not possible to lay out numerical values of the policy instrument and simply make these an exogenous input into the model: a law of motion for the instrument had to be specified, so that the model could be solved and the implications of nominal contracts for output and price dynamics worked out. Taylor's (1989, p. 186) judgment that the rational expectations revolution "placed emphasis on evaluating macroeconomic policy as a rule" would then only be reinforced when he moved to using interest-rate rules. In contrast to the case in which the central bank used a quantity variable as an instrument, it was not an option to specify a univariate law of motion for the instrument when this was an interest rate: it had to be written in terms of a feedback rule or reaction function.

Consequently, Taylor's perspective had long been different from that Olivier Blanchard's expressed in October 1995 with regard to Robert Lucas' research of the 1970s. Both Blanchard and Friedman were quoted in the national press about Lucas' new Nobel prize. Whereas Friedman's quoted remarks about Lucas' work were full of praise, Blanchard's remarks had a sting: "I think it has had zero impact on the way governments and central bankers perceive their job… I don't think it's had much effect on macroeconomic practice." (*Honolulu Advertiser*, October 11, 1995.) Whatever the merits of Blanchard's assessment at the time of Lucas' Nobel in 1995, it certainly became obsolete in a short time: the Taylor rule was used in inputs to FOMC discussions in November 1995 (Kahn, 2012, p. 73) and, as indicated earlier, over the following year the Federal Reserve Board introduced a new model that had forward-looking elements and that was, necessarily, solved by an assumed policy rule.

The interest-rate instrument: the research world

Taylor's work appeared in 1993 in an environment in which interest-rate rules were gaining more attention in policy-oriented research. In the United States context, Henderson and McKibbin (1993) was one such paper, but the process was also in motion in other countries. For example, a cross-country study of the international relationship between short-term interest rates study, written by Reserve Bank of Australia researchers and issued in mid-1991, reported that, in contrast to the 1970s, in the period since 1984 "the slope coefficient… is significantly greater than unity"—a result that the authors suggested was indicative of a shift to more inflation-responsive monetary policies (Bullock and Rider, 1991, p. 6). In a contribution to a July 1992 conference, Goodhart (1992, p. 324) suggested that a useful criterion by which U.K. monetary policy might be judged was a "rule" under which the short-term interest rate was adjusted by 1½ percentage points in response to a rise in inflation—the same numerical response later embedded in the Taylor rule.

These early studies, like the Leeper (1991) theoretical study mentioned above, all featured interest-rate responses that featured what Woodford (2001, 2003) would characterize as the "Taylor principle"—the idea that the appropriate response of the federal funds rate in the face of inflation overshoots should be greater than one-for-one.[282] Friedman had repeatedly suggested that an obstacle to a successful short-term interest rate policy was that it needed vigorous responses to the state of the economy. The Taylor principle reflected such a response. Indeed, such authors as Dornbusch and Fischer (1978, p. 517), Leeper (1991, p. 144), and Romer (2012, p. 543) saw a message of Friedman's 1967 presidential address as being that it was necessary for the nominal interest rate to rise sufficiently in the face of upward pressure on inflation, in order to prevent a decline in the real interest rate to that would magnify the rise in inflation.

Viewing anti-inflationary monetary policy as a rule

The Taylor rule literature did suggest that, depending on their specification, interest-rate or

[282] The notion that real rates should rise with inflation for inflation control was present in policy-related discussions before 1992. For example, Chouraqui and Price (1984, p. 33) observed: "If monetary growth norms are based on a constant long-run expansion of the money supply, the response to inflation shocks will be nonaccommodating, real interest rates being forced up." And the *Citibank Monthly Economic Letter* (September 1978, p. 9) stated: "The Fed can restrain the economy only when its policies cause market interest rates to rise faster than the public is raising its inflation expectations, which would mean a rise in the real rate." It should be noted that, in forward-looking models, because of contemporaneous interaction between interest rates and inflation, the stabilization of inflation implied by the Taylor principle may not actually feature an observed rise in real interest rates.

reaction functions could be destabilizing rather than stabilizing. In so doing, it provided an interest-rate-rule based account of historical monetary policy performance that preserved some of Friedman's critique of postwar stabilization policy, while challenging other aspects of that critique.

Taylor's characterization of U.S. monetary policy as following a parsimonious interest-rate feedback rule gained ground as empirical studies of the reaction function largely confirmed that the Federal Reserve's average responses took the form Taylor specified. The main modifications that the later econometric estimates put on his 1993 result was that the Taylor rule characterized both the Volcker and Greenspan regimes well, rather than just Greenspan's, and that dynamics appeared to feature in the estimated rule, in the form of an interest-rate smoothing term and, possibly, in the fact that expected future inflation rates, rather than current inflation, were the nominal variable to which the federal funds rate responded (Rotemberg and Woodford, 1997, pp. 304–305; Judd and Rudebusch, 1998; Clarida, Galí, and Gertler, 2000).[283] The newfound success of empirical interest-rate rules contrasted with the pre-Taylor *status quo*, in which attempts to estimate the Federal Reserve's monetary policy reaction function were widely regarded as having been unsuccessful (see Rasche, 1993b, p. 27).

The Taylor rule formulation proved a convenient way of characterizing not only recent years' monetary policy setting but also prior eras of U.S. monetary policy, as Taylor (1999a) emphasized. In particular, Taylor's (1995, p. 779) conjecture that "the adoption of a monetary policy with greater concern for inflation in the early 1980s" was a major epoch was borne out by subsequent estimates of the federal funds rate reaction across periods. These estimates showed that the response to inflation changed from a below unity until the late 1970s to above unity thereafter (Judd and Rudebusch, 1998; Taylor, 1999a; Clarida, Galí, and Gertler, 2000).

The fact that interest-rate reaction functions were quantitatively different from before 1979 put in perspective the Federal Reserve's *de facto* return to a federal funds rate instrument from 1982. At the time and for years thereafter, many outside economists, including Friedman himself, had taken the reversion to federal funds rate control as essentially a return to the pre-1979 state of affairs—including a resumption of other aspects of the pre-1979 Federal Reserve's approach (see

[283] Clarida, Gali, and Gertler's (2000, pp. 156–157) baseline estimate had the FOMC responding to the one-period-ahead expected inflation rate. In the case of Rotemberg and Woodford (1997), the estimated rule involved a response to lags 0–2 of quarterly inflation rather than an inflation series that included expected future values. Judd and Rudebusch (1998, pp. 5–6) used the current four-quarter inflation rate in their estimated reaction functions.

Chapter 14 above). One facet of these interpretations was that, after the post-1982 Volcker monetary policy was judged a success, some Keynesians saw it as a triumph of older approaches to stabilization policy. James Tobin, for example, had said in 1990: "The Federal Reserve has succeeded in producing a long and successful recovery through fine tuning."[284]

Against this, as Rasche (1985, p. 47) observed, "Federal Reserve officials are on record as indicating that the present procedures are not a return to the techniques of the 1970s." Although some of the initial denials by FOMC members that they were not targeting the federal funds rate now have to be discounted, they were fundamentally correct in contending that they were not returning to the 1970s policy. In 1982, a number of figures in monetary policy circles, including Federal Reserve Bank of Boston President Frank Morris (1982a, p. 81) and Benjamin Friedman (1982a, p. 16), while voicing doubts about monetary aggregates-based frameworks, had acknowledged that an interest-rate based operating procedure carried with it the danger of a procyclical policy, arising from an insufficiently prompt and automatic response of interest rates to the economy. Correspondingly, in the same year, a Federal Reserve Bank of New York official, Paul Meek, observed (see Meek, 1983, p. 70) that an interest-rate regime could be attractive "provided the authorities move rates enough in a timely fashion."

The fact that a Taylor rule, complete with more-than-unitary coefficient on inflation, seemed to capture well the behavior of U.S. monetary policy since the early 1980s suggested that policymakers had digested the monetarist critique of pre-1979 federal funds rate policy. The parsimonious, rule-based characterization of decisions since 1982 also contradicted Tobin's portrayal of that period as consisting of old-fashioned fine-tuning.

And it has already been indicated that, *conditional on the fact* that the Federal Reserve was following an interest-rate instrument, Milton Friedman believed that it was appropriate for monetary policy to respond vigorously to economic developments, including on the inflation front. The research literature's estimates of post-1979 monetary policy reaction functions indicated that U.S. interest-rate policy had moved in that direction.

The rules literature relaunched

The tone that the Taylor (1993) paper helped set over the decade after 1993 was captured by the

[284] Friedman and Tobin (1990, p. 77), reprinted in McClelland (1990, p. 11). In addition, in the *Wall Street Journal* (December 16, 1991), Tobin referred to "fine-tuning like the Fed's management of the 1983–88 recovery."

This trend was also reflected, *inter alia,* in an NBER conference on monetary policy rules in January 1998, organized by Taylor (Taylor, 1999b) and in the title of Chapter 1 of Woodford's (2003) *Interest and Prices: Foundations of a Theory of Monetary Policy*, "The Return of Monetary Rules."

Another example was provided by the September 2002 issue of the *Journal of Monetary Economics*, which had an article by James Bullard and Kaushik Mitra titled "Learning About Monetary Policy Rules." The title of the article, like that of Woodford's chapter, encapsulated much of what had happened in monetary economics over the previous decade: monetary policy rules had made a big comeback on the research agenda. In that respect, recent developments represented a rekindling of one of the main areas of Friedman's monetary analysis.

But Bullard and Mitra's paper also reflected a respect in which the modern literature had broken away sharply from Friedman's outlook, for, by "monetary policy rules," Bullard and Mitra were exclusively referring to *interest-rate* rules. By the early 2000s, monetary policy discourse, in the wake of the trends in research and policy discussions described above, was steeped in interest-rate rules.[285] In the face of this development, Friedman's own evolution over the 1993–2006 period might well be characterized as "learning to like interest-rate rules." He never reached the point of liking interest-rate rules, but by the end of the period he had largely made his peace with them.

Not completely reconciled

From what has already been said, it is appropriate to conclude that Friedman was opposed to Taylor-type rules that included a strong response to the output gap. Even in the case of interest-rate rules that moderated the response to the output gap or replaced it with a response to real GDP growth, it would be more accurate to characterize the evolution of Friedman's views in 1993–2006 not as unequivocally favorable, but with a phrase that Anna Schwartz had used in a different though related context: they became "progressively less negative."[286]

[285] Woodford himself was quoted as saying: "Friedman's rule involves a target for the growth rate of some definition of the quantity of money. I don't think that the best monetary rule involves a target of any kind for the growth rate of a monetary aggregate. Friedman's rule is not the worst sort of rule, as simple rules go, but we can do better." These remarks appeared in the sixth edition of Michael Parkin's undergraduate economics textbook (Parkin, 2002, p. 703). Parkin interviewed leading economists in each edition, usually changing the interview subject with each edition. Parkin had interviewed Friedman in the first edition of the textbook, published in 1990.

[286] *The Banker* (London), February 1985, p. 101. Schwartz used this description in tracking the Bank of England's views on monetary base control in the late 1970s and early 1980s.

Taylor benefited from the dialogue with Friedman on the subject: "And we talked about that a lot, and I don't know if I completely convinced him, but that was, I think, a fruitful exchange for me, in seeing his reaction to that. I'd say that I think he generally was quite positive about it." (John Taylor, interview, July 2, 2013.) Friedman accustomed himself to the profession's renewed emphasis on interest rates while not subscribing to it. "I am by no means willing to say that monetary targeting is a bad idea," he told an interviewer in the late 1990s, "but as a matter of practice it is dead."[287]

Continued advocacy of constant monetary growth

In an age in which the focus on interest rates was pervasive, Friedman continued to put priority on the behavior of monetary aggregates and to advocate constant monetary growth. He made some attempt to reconcile these preferences with the reality of U.S. monetary policy when he wrote an op-ed (*Wall Street Journal*, January 31, 2006) to mark Alan Greenspan's departure as Federal Reserve chair.

In this January 2006 op-ed, Friedman again emphasized that Greenspan had shown that it was possible to improve upon a constant-monetary-growth rule. He also noted that he was impressed with the degree to which seemingly continuous control of inflation had been achieved under Greenspan, in the form of a low, stable rate.[288] This was a significant observation in view of the fact that the precariousness of the link between monetary policy and inflation in the short to medium term had figured so prominently in Friedman's case for a constant-monetary-growth rule. Friedman had told Lars Svensson in early March 2002 that recent years' events in the United States and abroad had persuaded him that a monetary policy directly concerned with inflation was often likely to outperform one based on targeting monetary growth—a change in position from his 1967 presidential address. He elaborated in his 2006 retrospective that the shorter-term stability of inflation seen under Greenspan had actually exceeded what Friedman regarded as achievable.[289] Friedman suggested that this achievement provided a new baseline against which central banks should be judged. But his continuing reservations about the modern monetary policy framework were registered in his lack of emphasis on interest rates in the op-ed and in his suggestion that policymakers other than Greenspan might not be able to improve on a

[287] In Parker (2002, p. 54).

[288] U.S. inflation had actually picked up in recent years on a headline basis, but Friedman concentrated on the longer-term behavior of inflation.

[289] Especially in Friedman (1960, 1968b) and in *Newsweek*, February 7, 1972.

strict policy rule.

Indeed, Friedman remained an advocate of constant monetary growth right up to the end of his life. Two and a half years before his 2006 op-ed, a minor stir came about when a suggestion to the contrary appeared in the financial press. In June 2003, a profile appeared in a London *Financial Times* magazine supplement. It included brief remarks from Friedman on monetary targeting: "The use of quantity of money as a target has not been a success. I'm not sure I would as of today push it as hard as I once did." (*Financial Times* (London), June 7, 2003, p. 12.)

To readers of Friedman's interviews in 1998–2000 in *Barron's* and with Taylor (2001), the 2003 quotations were not a great surprise. Friedman had previously made it clear that he had been unhappy with how monetary targeting had typically been implemented in practice. In addition, as has been indicated, he had been impressed by how U.S. monetary policy in recent years had done better on the criterion of delivering economic stabilization than he had thought practicable and had outperformed what he would expect from a monetary-growth rule. The 2003 remarks were not a break with the position that he had articulated in these earlier interviews.

Some commentators, however, did not put these remarks into a broader context and instead interpreted then as being a repudiation on Friedman's part of monetarism. William Keegan—a doyen of U.K. financial journalism who described himself as "a good Keynesian" (*The Observer* (London), August 24, 1986) and who had a long record of being critical of Friedman—seized on the quotation in the *Financial Times* piece as the basis for an *Observer* commentary titled "So Now Friedman Says He Was Wrong." Keegan asserted that Friedman was simply repudiating his prior views and had capitulated in his long battle with Keynesianism. "Friedman, now 91 [*sic*; 90], … feel[s] it is time to own up. It is 'true confession' time." (*The Observer* (London), June 22, 2003.)

In light of this and similar "spins" on his remarks, Friedman decided to produce a lengthy exposition of his interpretation of monetary policy developments in the preceding fifteen years. The resulting article—the *Wall Street Journal* "thermostat" article discussed above—must have come as a disappointment to anyone seeking confirmation that he had turned his back on monetarism. On the contrary, while again praising recent years' U.S. monetary policy for doing better than a constant monetary growth rule, Friedman maintained: "Velocity is ordinarily very stable, fluctuating only mildly… The $MV = Py$ key to a good thermostat was there all along." (*Wall Street Journal*, August 19, 2003.)

But Friedman's fundamental doubts about the sustainability of successful activist policies remained. And the constant-monetary-growth rule also remained his own preference. During the time in which he was working on the *Wall Street Journal* article, Friedman stated to the present author:

"In my original support for a straight money target, I always emphasized that it was partly a case based on ignorance, based on the fact that we really did not understand sufficiently well the detailed relationship between money, income, interest rates, and the like to be able to fine-tune, that our goal should be to develop a detailed enough understanding so that we could do better than a simple constant monetary growth target.

"However, I believe still, as I did then, that constant monetary growth would produce a highly satisfactory price path, and, if it enabled you to get rid of the Federal Reserve System, that gain would compensate for sacrificing the further improvement that a more sophisticated rule could produce."[290]

Interest-rate rules and dropping money from models

These remarks were made during a period when the research side of monetary economics was making major strides along a road that practical monetary policy in many countries had already taken: the conduct of the analysis of monetary policy and economic developments without reference to the behavior of the money stock.

Four decades earlier, in a talk at the University of North Carolina, Friedman had discussed his recent, monetary-research-oriented, long trip over 1962–1963 to European and Asian countries. He inferred from that trip: "It's easy to find out whether or not you're talking to an economist." He explained: "Wherever I went, I was asked why I was there, or what I was trying to find out, or what was the object of my study." In response to his answer, "I am investigating money," Friedman recalled: "If the inquirer was *NOT* an economist, he laughed. If he *WAS* an economist, he did not laugh." (*Chapel Hill Weekly* (North Carolina), January 29, 1964, p. 1.)

By 2003, however, it would be reasonable to say that monetary economists who indicated that they concentrated on monetary aggregates in their analysis did so in danger of provoking

[290] Milton Friedman, email to the author, July 21, 2003.

laughter even from fellow economists. The decade through 2003 would simultaneously see a considerable revival of monetary economics as a research area and the continued dissipation, in monetary analysis, of the study of the quantity of money. Investigation into this research topic would become a desolate enclave within monetary economics.

Not long after his 1964 remarks, Friedman included in an introduction to some of the monetary research that he had overseen an observation regarding that research that future "workers… will in their turn hopefully render it obsolete."[291] When writing these words, he could surely have not foreseen that, although research on monetary matters would be thriving early in the next century, that era would also see a development that he hoped would not happen: the dominance of the perception that the study of the money stock was itself an obsolete element of monetary economics. Michael Woodford's 2003 *Interest and Prices* included a considerable amount of theoretical analysis involving the money stock, but much of this analysis appeared in the course of establishing that monetary aggregates were not actually needed in the design of monetary policy or in deriving model solutions for output and inflation.

Through this work and other research appearing in the 1990s and early 2000s, the notion that money could be dropped from models used for the analysis of monetary policy came to be especially associated with New Keynesian economics. But the same basic rationale for omitting outright the analysis of the money stock had also been applicable to many pre-New Keynesian models, in which monetary policy's channels could be expressed entirely in terms of the behavior of yields. Indeed, it was to the original Keynes (1936) *General Theory* that John Flemming, in a paper published in 1993 but prepared for a 1990 conference that had taken place when he was chief economist at the Bank of England, argued that a considerable simplification of Keynes' system was available by sidelining money. Flemming noted that, in his consideration of monetary matters, Keynes could be considered as having postulated that monetary policy influenced output via its effect on a specific interest rate. Flemming further observed that, in the case in which (*i*) the price level is fixed, (*ii*) the interest rate is a short-term yield, and (*iii*) monetary policy sets that yield each period, the system meriting study was as a self-contained two-equation model consisting of an IS equation and a monetary policy rule. Flemming suggested that Keynes had made the system requiring analysis unnecessarily large by assuming, in much of the *General Theory*, that the policy instrument was money.[292] The relevant system,

[291] The quotation is from Friedman (1965, p. xxviii).

[292] In his analysis, Flemming was focusing on the case in which short- and long-term interest rates were above their lower bounds and both were capable of being affected by stimulative monetary policy.

in that case, was larger, at three equations: a money demand equation, the IS equation, and the money-stock rule. With regard to expanding the model's economic structure, Flemming (1993, pp. 77−78) made the point that modifications in the direction of making the price level endogenous, and thereby allowing a clear-cut real/nominal rate distinction, as well as postulating (as Keynes often did) that real aggregate demand depended on the real long-term interest rate, meant (when the short-term yield was the policy instrument) a four-variable/four-equation system (a term-structure equation, an IS equation, a Phillips curve, and an interest-rate policy rule). That is, such a generalization of the model did not, in itself, prevent the money demand function from being recursive *vis a vis* the other equations. Consequently, money's behavior did not actually need to be ascertained when working out the model solutions for the other variables.

That in certain models (specifically single-interest-rate models, or multiple-rate models in which the relationship between yields did not depend on quantities), the money demand equation had a residual or recursive character if money was not the policy instrument, had been recognized by many researchers in the past, among them Milton Friedman himself.[293] But as monetary analysis increasingly focused on interest-rate policy in the 1990s, the liberty that this result provided for dropping monetary aggregates and the money demand equation from economic models was increasingly recognized and heavily acted upon. For example, in the category of models that were not New Keynesian (because they lacked many microfoundations) but had forward-looking elements, both the Fuhrer and Moore (1995b) model and the aforementioned FRB/US model introduced in 1996 dropped the money demand equation and modeled both monetary policy formation and monetary transmission channels entirely in terms of interest rates. Around the same time, this approach also became prevalent in New Keynesian models. Friedman's continuing emphasis on monetary aggregates therefore put him at odds not only with monetary policy practice and with efforts such as Taylor's to characterize and design monetary policy, but also with key developments in the analysis of price level determination taking place in the research world.

As discussed in Chapter 16, the New Keynesian models of the 1980s were focused on monetary policy and typically did so by giving the money stock a prominent place in the specifications of the behavior of both aggregate demand and monetary policy. For example, Rotemberg (1982, pp. 1193–1194) postulated that only a stationary, exogenous velocity process intervened in the relationship between real money and real income, while Rotemberg (1987, p. 78) assumed that

[293] For further discussion, see McCallum and Nelson (2011, p. 137) and Nelson (2020b, Chapter 10).

velocity was constant. And Rotemberg himself, and many others, specified U.S. monetary policy as following a money-stock rule.

The aggregate demand specification and policy rule were not the focus of most of these analyses, in which the main concern was the implications of incomplete price adjustment.[294] But the fact that, in particular, the models of 1980s New Keynesianism specified aggregate demand behavior in a manner that implied a tight money/income link was not lost on observers of the emerging literature. Julio Rotemberg recalled of Michael Woodford's perspective on the early New Keynesian work: "He thought we were all monetarists." Reflecting on this reaction on Woodford's part in the 1980s in light of Woodford's subsequent work on dropping money from monetary policy models, Rotemberg observed: "What I don't know is whether he, at the time, was already harboring an intention to, you know, go on the attack of that—I mean, that, I just don't know. But I think he was a little bit mystified that we were all, you know, calling ourselves Keynesians. I think that's what he found amusing: We called ourselves Keynesians, and we all basically did $MV = PQ$." (Julio Rotemberg, interview, September 5, 2014.)

Discussing New Keynesian economics at a 1991 symposium, Gregory Mankiw (1992, p. 560) acknowledged the resemblance: "In many ways, the Keynesian economics of the 1990s does not look like the Keynesian economics of the 1930s, or even that of the 1960s. To some old Keynesians, New Keynesian economics may be hard to recognize as Keynesian at all. Indeed, New Keynesian economics may appear more similar to the classical economics of David Hume, or even to the monetarist economics of Milton Friedman."

In 1990, Olivier Blanchard had similarly, in effect, acknowledged the debt that the new work owed to monetarism by stating that, although it had "been labeled Keynesian, it often bears only a distant resemblance to the earlier models."[295] But Blanchard also noted that New Keynesian analysis was a work in progress and did not yet "constitute a unified whole." And as a more cohesive New Keynesianism took shape over the 1990s, it would transpire that the emphasis on monetary aggregates that New Keynesianism had to date shared with monetarism would go away.

[294] Julio Rotemberg remarked (interview, September 5, 2014) of the assumption of a tight money/income link: "we all literally, to a man, used $MV = PQ$ as our aggregate demand equation… It was, like, what everyone did, [so] there was no reason that you needed to defend it, or anything. And I certainly didn't want to contribute to aggregate demand specifications. I mean, I was happy to have anybody tell me what they thought their favorite aggregate demand function was, and I was just going to put it in there and solve with price rigidity."

[295] Blanchard (1990, p. 781).

It had long been widely taken for granted that the acceptance that inflation control was a monetary policy responsibility meant that monetary policy analysis and monetary aggregates were interlocked. In keeping with this line of thinking, Phillip Cagan had stated in 1979: "monetary policy has to rely very greatly on monetary aggregates. The monetary authorities have to have a growth path of some total quantity of financial assets that they believe will help them control aggregate expenditures. I really don't believe we can get away from that. As much as looking at interest rates may help, we have got to rely on the growth of financial assets."[296] Similarly, Cagan (1982) remarked: "Monetary targeting is the only feasible method of stabilizing prices, whether one likes it or not." Statements to the effect that the analysis of money demand was intrinsically central to the making of monetary policy were also commonplace.[297]

Monetary policy practice increasingly defied injunctions like these, and Baumol and Blinder (1991, p. 293) remarked of the Federal Reserve, "since 1982 it has deemphasized money growth." During the 1982–1992 decade, this tendency was occasionally interrupted, most notably by Greenspan's interest in M2 during his first 4½ years in office. But the episode of heightened interest in M2 was followed by the changes in the relationship between M2 and the economy discussed earlier in this chapter, as well as with considerable disillusionment on Greenspan's part with monetary aggregates.

The FOMC's practice from 1993 onward showed that it was pressing ahead with an inflation-oriented monetary policy in which attention to monetary aggregates and money demand played little role. This development took time to seep into academic discussions: for example, in 1994 Robert King (1994, p. 121) characterized the monetary policy tightening sequence observed earlier that year as one in which "the Federal Reserve raised its short-term interest rate target three times so as to produce a deceleration of money growth." By the end of the decade, however, King was employing monetary policy analysis that dispensed with references to monetary growth (see R.G. King and Wolman, 1999). In the meantime, Michael Woodford had published a number of papers that analyzed price level determination in sticky-price models without reference to monetary aggregates (for example, Woodford, 1996, 1997; Rotemberg and Woodford 1997), an approach crystalized in the "neo-Wicksellian" framework of his *Interest*

[296] Testimony of May 14, 1979, in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1979e, p. 788).
[297] For example, Hafer and Hein (1980, p. 26) stated: "A stable money demand function is crucial to the formation and implementation of effective monetary policy." The durability of this notion during the 1980s is evidenced by the fact that the introduction to a study published at the end of the decade stated (Horne and Martin, 1989, p. 181): "A reliable empirical measure of money that bears a predictable relationship with nominal income and is controllable, is critical for the effective operation of monetary policy."

and Prices.

Richard Clarida observed that the linearized New Keynesian system that would become prevalent—consisting of an interest-rate reaction function that was a variant of the Taylor rule, an IS equation, and a Phillips curve, the latter two equations embedding forward-looking private-sector behavior—"were models using a technology that existed in 1983. It wasn't like there was some insight [arriving] between 1983 and 1995 or 1996. But it just took some time." (Richard Clarida, interview, April 6, 2021.)

It would be a mistake to view this approach as a wholesale repudiation of Friedman's analysis. Indeed, as stressed in Nelson and Schwartz (2008), the modern New Keynesian model that was utilized in work like Woodford's embedded a number of key Friedman propositions. The optimizing IS equation in New Keynesian analysis reflects the permanent income hypothesis. It also amounts to an affirmation of the sensitivity of private-sector spending to monetary policy actions, as well as the point that real, and not nominal, interest rates matter for that spending. Furthermore, in contrast to the real business cycle approach, New Keynesian analysis strongly emphasizes real/nominal interaction, and it does so via a Phillips curve that is basically of the expectational type outlined by Friedman and Phelps. And unlike the early rational expectations versions of this curve, the New Keynesian version discards the flexible-price assumption to which Friedman objected and, instead, rests on price stickiness.[298] The natural-rate terminology that Friedman used in reference to the baseline unemployment rate was used heavily (mainly in defining a baseline level of output) by New Keynesians, and New Keynesian analysis continued Friedman's stress on the stochastic character of the natural values of real economic series. Correspondingly, Friedman expressed satisfaction during the 1990s about the extent to which the

[298] In particular, as shown in Rotemberg (1987) and Roberts (1995), the New Keynesian Phillips curve can be derived from an environment in which there are staggered contracts of the type advanced by Calvo (1983). The Calvo specification was, in turn, a development of Taylor's (1980a) nominal-contracts model. Calvo recalled that he had tried to put finite-length nominal price contracts into a continuous-time model and ended up with a differential equation that was "really very tough to handle—and very complicated. So, out of desperation, I started to think to myself, well, what, if I extend the [contract] horizon to infinity—using the Poisson process that's really well known now. And I did that. And, all of a sudden—it was just like a miracle—it became so simple. It was beautiful!" Calvo initially drafted his paper while he had a visiting position at the University of Chicago in the early 1980s but was hesitant to give it at a workshop session because of concern that the atmosphere in the economics department had become hostile to sticky-price models: "I didn't *dare* to present it." (Guillermo Calvo, interview, April 1, 2014.) Ironically, later analytical developments of the Calvo price-setting framework took place at the University of Chicago in the 1990s via the work of Woodford, at the university until 1995, and his student, Tack Yun (see Woodford, 1995, and Yun, 1996; see also Rotemberg and Woodford, 1997, 1999, for subsequent developments of the framework). Other applications of the Calvo framework during the mid-1990s included King and Watson (1996).

natural rate hypothesis and related terminology had become commonplace.[299] And the New Keynesian model importantly captured Friedman's belief in the monetary nature of inflation, as discussed further below under "Ben Bernanke."

Contrary to what is often said, it would also not be correct to suggest that the fact that the New Keynesian baseline model did not in the late 1990s and the 2000s, and does not today, include money in the IS or Phillips curves itself marks a break with Friedman's framework.

Nor is the amenability of the New Keynesian model to analysis using Wicksellian techniques something that is inconsistent with Friedman's analysis. Friedman's work did not dispute the notion that it was possible to analyze the link between monetary policy and inflation in terms of the spread between actual and natural real interest rates.[300] Michael Keran discussed this issue with Friedman in the 1990s. "After I left the Fed, I went to work at Prudential as their chief economist, and there, I started to focus more on interest rates rather than the money supply. But I did it in the Wicksellian framework… And I talked to Milton about that a number of times—and asked, 'Well, what did you think about this way of looking at it?' He thought that was a perfectly valid way of doing it." (Michael Keran, interview, March 7, 2013.)

Friedman was himself implicitly appealing to variations in the Wicksellian short-term real rate of interest, as well as to incomplete policymaker knowledge of the private sector's expected rate of inflation, when he continued during the 1990s (for example, in *Wall Street Journal*, December 17, 1997) to argue that the stance of monetary policy could not be reliably gleaned by examining the behavior of short-term nominal interest rates. Indeed, up to the 1990s, this consideration had widely been cited by economists as tipping the balance in favor of money as an indicator: even in contexts in which interest rates were accepted as being highly relevant for spending decisions, the expected-inflation component in nominal rates as well as changes over time in the real-interest-rate value consistent with price stability were invoked as reasons to distrust short-term nominal interest rates as a measure of monetary policy stance. For example, Monhollan (1970, p. 14) observed: "An additional advantage of aggregates as policy targets is that it is probably somewhat easier to choose an appropriate growth rate of an aggregate than to choose the appropriate level of interest rates…"[301] Even about two decades later, Bennett McCallum, who

[299] See, for example, his remarks in Snowdon and Vane (1997, p. 211).

[300] In addition to what follows, see the discussion in Nelson (2020b, Chapter 6) as well as the references given therein.

[301] See also McCallum and Nelson (2011, p. 138) for 1985-vintage statements to the same effect.

had established in his research that nominal interest-rate rules amounted to a viable policy approach, nevertheless stressed (McCallum, 1989a, p. 340) that "the sign of the relationship between interest rates and monetary policy depends on the time horizon involved." McCallum maintained: "This pattern… makes any interest rate an ambiguous and potentially misleading indicator of monetary policy. For that reason, we conclude that a policy rule should be formulated in terms of the behavior of a quantitative magnitude…"

Friedman himself affirmed to Keran that, because of the unobservability of the natural rate of interest, Friedman "just thought that [analysis based on] the money supply was a much more straightforward and stable way of looking at it." (Michael Keran, interview, March 7, 2013.)

It was on this matter that the profession largely broke with Friedman from the 1990s onward, most notably in Woodford's influential work. Even before that work appeared, the natural-rate concept had attracted attention in Federal Reserve circles (an interest likely partly reflecting caution about the enthusiasm displayed by Alan Greenspan regarding M2). Among the Federal Reserve Board's senior staff, Kohn (1990, pp. 18–19) emphasized the need for the Federal Reserve to ascertain changes in equilibrium real interest rates, while Federal Reserve governors were reportedly interested in what *The Economist* (London) (April 28, 1990, p. 112) called a "neo-Wicksellian policy" approach of using estimates of the natural interest rate as a benchmark.

Although not all monetary policy analysis using New Keynesian models puts the explicit emphasis on the natural rate of interest that Woodford's work would, most monetary policy research from the late 1990s onward did, indeed, abstract from the money demand function and omitted monetary aggregates. A money demand function can be derived from the dynamic general equilibrium models associated with New Keynesian analysis (see, for example, McCallum and Nelson, 1999). Nevertheless, provided that monetary policy is specified in terms of a variant of the Taylor rule, the money demand function is not needed to solve for the behavior of any variables in the analysis other than money. A consequence of the renewed research emphasis on interest-rate rules in the 1990s and 2000s was that it reinforced a tendency already created by the fading of monetary aggregates from policy discussions: the analysis of monetary aggregates became a small corner of monetary policy research.

Friedman was gratified that the "shift in the theoretical paradigm," as he described it when discussing New Keynesian analysis in 2003, included the acceptance that inflation was a monetary phenomenon (*Wall Street Journal*, August 19, 2003). This aspect of the consensus will be discussed further below. Friedman was, however, unhappy about the fact that the focus on monetary aggregates in policy discussions had faded away. He likened the modern era to "the 1960s, when no one paid any attention to the money supply." (*Forbes*, May 3, 1999*a*, p. 138.) If models were now so small that they omitted money, and if they also did not allow money-supply rules to be considered, he felt that they had become too small.[302]

The deemphasis on monetary aggregates was a definite feature of the New Keynesian consensus by the early 2000s. But, in other respects, opinion on key matters was far from settled—so much so that Friedman's remark in another connection that "this so-called consensus is hilarious" is applicable.[303] One example of the fragility of the consensus is brought out by considering the question: How should an oil price increase be approximated in a New Keynesian baseline model that does not have energy explicitly as a production input? The answer, according to Goodfriend and King (1997, p. 271), was that the shock should be approximated by a decline in the markup. The answer, according to Blanchard (1997, p. 332), was that the shock should be approximated by an increase in the markup.[304] The fact that such basic disagreement existed about how to interpret and develop the basic New Keynesian framework made it likely that major refinements and extensions of the model were still in prospect—a likelihood that opened up the possibility that still more of Friedman's ideas would be incorporated into New Keynesian analysis.

In fact, in early 2005, two papers appearing prominently in journals proposed modifications to the New Keynesian baseline. In doing so, both papers invoked Friedman's work as a motivation. The January 2005 issue of the *Journal of Monetary Economics* contained an article by Michael Dotsey and Robert G. King in which the authors motivated their advocacy of a state-dependent price-setting specification in terms of its ability to match the dynamic relationship between money, output, and prices that Friedman had sketched in (*inter alia*) his 1987 *New Palgrave* dictionary entry, including the six-to-nine-month lag of output behind money and the slower response of inflation than output to monetary injections (Dotsey and King, 2005, pp. 214, 227).

[302] He expressed this sentiment in a letter to Bennett McCallum of July 1, 1999, in response to a version of McCallum (2002).

[303] The quotation is from *Meet the Press*, NBC, March 21, 1982, p. 3 of transcript.

[304] Another example of dissent in the New Keynesian literature on a seemingly straightforward issue comes in Galí and Gertler's (1999, p. 214) characterization of the New Keynesian Phillips curve as featuring unidirectional causation running from real marginal cost to inflation. It would seem more appropriate to view real marginal cost and inflation as, instead, being simultaneously determined in the New Keynesian model (see, for example, Cochrane, 2011, 2023). As Sims (1977, p. 31) stressed, aggregate wages and prices may be simultaneously determined even in a model in which firms treat their goods' prices as a choice variable that they adjust in response to movements in their costs of production.

The following month, February 2005, saw the publication of one of the most cited monetary policy articles of the new century: “Nominal Rigidities and the Dynamic Effects of a Shock to Monetary Policy” by Lawrence J. Christiano, Martin Eichenbaum, and Charles L. Evans (2005). These authors extended the New Keynesian model to include various elements designed to spread over time the response of output and inflation to monetary policy actions. The paper cited Friedman’s work on lags as a motivation and as representing “a longstanding view that many macroeconomic variables do not respond instantaneously to policy shocks.” And, in reviewing the empirical regularities that they proposed to match, the authors stated, “Interestingly, these results are consistent with the claims in Friedman (1968[b]).”[305]

Monetary research: a brief Friedman return

Each being based at Stanford University, John Taylor and Milton Friedman had seen each other frequently in the period in the 1990s and through early 2001. Taylor also persuaded Friedman to give a guest lecture on a few occasions (such as in 1997) over this period to Taylor’s Stanford University freshman Economics I class—an arrangement that added incrementally to Friedman’s very limited career-long exposure to undergraduate teaching. And after initially turning down an opportunity to be a subject in the series of interviews with economists that featured in the journal *Macroeconomic Dynamics*, Friedman agreed after Taylor was offered as the interviewer.[306] The resulting exchange, conducted in May 2000 and published as Taylor (2001) would be one of the definitive Friedman interviews.[307]

In the period from 2001 to 2005, Friedman and Taylor saw each other only infrequently, as Taylor was a U.S. Treasury official (Under Secretary for International Affairs) during the first term of the George W. Bush presidency.[308] Taylor returned in early 2005 to Stanford University

[305] Christiano, Eichenbaum, and Evans (2005, pp. 5, 8). The authors also indicated that Friedman (1968b) presaged their finding that “inflation responds in a hump-shaped fashion, peaking after about two years” (p. 8). Friedman (1968b) may have been used as an all-purpose reference here, as Friedman did not really spell out the two-year lag in a journal article until 1972 (see Nelson, 2020b, Chapter 15).

[306] Friedman’s initial refusal to grant a *Macroeconomic Dynamics* interview may have been motivated by a desire not to preempt the coverage of his career that would feature in his memoirs, which were forthcoming when the *Macroeconomic Dynamics* journal started.

[307] It would be widely cited. Unfortunately, however, some articles (for example, Mankiw, 2002, p. 43, and Ball and Tchaidze, 2002) cited the interview as a Friedman-authored article, omitting credit for Taylor and thereby giving the impression that Friedman interviewed himself.

[308] During this period, Taylor did occasionally attend research conferences, including the Carnegie Rochester Conference held in Pittsburgh in November 2002 and a Federal Reserve Bank of St. Louis conference in October 2004. Friedman was not at either conference, but Anna Schwartz was in attendance at both. The 2002 conference had many former Taylor students as speakers, and, reflecting this, Bennett McCallum introduced Schwartz’s

and, consequently, was in close touch with Friedman over what proved to be the final two years of Friedman's life.

A journal article on monetary policy appearing later in 2005 came from a most unexpected author: Milton Friedman. In an interview given in 2006, Friedman remarked that he was "still in the public arena [and] doing a bit of academic work" (*Rutgers* magazine, Fall 2006, p. 24), and his behavior during 2005–2006 bore this statement out.

Friedman's surprise return to publishing research in monetary policy took the form of an article, "A Natural Experiment in Monetary Policy Covering Three Episodes of Growth and Decline in the Economy and the Stock Market," in the Fall 2005 issue of one of the journals that he still read regularly, the *Journal of Economic Perspectives*.

Friedman used the article to compare the stock market boom and bust during the recent tech-bubble episode with the cases of the United States in 1929–1933 and Japan in the late 1980s and early 1990s. His judgment on U.S. monetary policy in the early 2000s was highly favorable. He maintained that the Federal Reserve, by keeping M2 growth high in the wake of the stock market decline, had stopped the equity price collapse from leading into anything more than a shallow recession.

As with *Money Mischief*, this is an article on which opinion is likely to be divided even among those who rate highly Friedman's older monetary research. The present author's view is that, even more than *Money Mischief*, the article diminished the average level of quality of Friedman's research output on monetary matters. It was not notably well written, was sparing in references to economic literature (other than the source for his data, Friedman cited only the *Monetary History*), and contained phrases like "inner dynamics" and "determinative effect" that a better-crafted exposition would have omitted.[309] The short paper, consisting of a set of graphs, a couple of tables, and a text shot through with categorical statements, certainly was not of a standard that would have been usually associated with a research journal, and the novelty of Friedman writing an article for the journal likely carried a great deal of weight. It was an impressive achievement that someone could compose such an article at the age of 93, and, once the article saw print, another achievement was registered in the fact that Friedman had now

contribution to the conference by observing that she was "not a student of John Taylor." Seated with other conference participants, Taylor could be heard saying that, via her writings, "I'm a student of hers."

[309] Friedman (2005b, p. 149).

published a journal article seventy years after his first such publication in 1935. But, on balance, little would have been lost if the piece had not been published in a research journal.

The article would have been much more suitable as a *Wall Street Journal* op-ed than as a journal article. In fact, the article was seemingly an expansion of a *Journal* op-ed by Friedman that had been published in early 2002. Furthermore, an adaptation of the 2005 *JEP* paper itself appeared as a *Wall Street Journal* op-ed as part of a tribute to Friedman on the day after his death (*Wall Street Journal*, November 17, 2006).

Viewed as a commentary on monetary policy during the later Greenspan years, the substance of the article did, in effect, represent a challenge to those—including John Taylor himself (see Taylor, 2012)—who would come to view monetary policy in the early 2000s as having been too loose. Friedman's article had praised the conduct of U.S. monetary policy through 2004:Q3. In a letter to the *Wall Street Journal*, Friedman extended by a further couple of quarters the period that his endorsement of Federal Reserve policy covered. He judged that monetary stimulus had been withdrawn at "just about the rate required for a rapidly growing noninflationary economy" (*Wall Street Journal*, April 7, 2005).

It is doubtful, however, that critics of monetary policy's conduct over this period—such as Taylor, who would judge negatively on Federal Reserve policy as executed in the period from 2002 to 2005—would be perturbed by Friedman's assessment. For they could argue that Friedman's writings used the wrong metric: whereas Taylor used the Taylor rule's prescription as a means of judging FOMC policy over the first half of the 2000s, Friedman's April 2005 analysis had used M2 and had not mentioned interest rates at all.

Friedman's emphasis on monetary growth would continue in 2006, when he produced a second, and better, latter-day research article. As has been indicated above, he advanced the thermostat hypothesis in his later writings as a means of explaining some of the looseness in money/income relations on U.S. quarterly data. It was also the case, however, that he continued to believe that longer-period stability in nominal income required moderate, as opposed to high, variability in money.[310] It was the positive longer-term relationship between monetary growth variability and

[310] Rivot (2012) seems to see a contradiction in Friedman's position, stating, "Yet in his 2006 paper, Friedman has got over his astonishment and emphasizes once more long-term correlation between money supply and output." If one tries to look through the fog of sarcasm in Rivot's remark, the message would appear to be that the author is criticizing Friedman for being inconsistent in his positions about the correlation of money and output. But the cited example is not a good one, because Rivot's criticism confuses real and nominal variables and also confuses first and

variability in income (real and nominal) that Friedman returned to in his paper "Tradeoffs in Monetary Policy," which he produced in April 2006 and lightly revised in August 2006.[311]

Although concerned, according to the title, with policy tradeoffs, the new paper quickly moved from a discussion of tradeoffs to a comparison of economic variability with monetary variability, using a dataset that consisted of 125 years of U.S. annual data on real GDP growth and monetary growth. In considering this longer stretch of data, Friedman stressed the large reduction in the *absolute* level of M2 growth variability seen in recent decades.[312] "The collapse of the variability of output," Friedman wrote, "is clearly an effect of the collapse of monetary variability. In my opinion, the same results could have been obtained at any earlier time and can continue to be achieved in the future."[313] The last sentence of the main text concluded—mischievously so, in view of Friedman's usual reluctance to use causality-related terminology—"What is involved is not a tradeoff but direct cause-effect."[314]

This last sentence testified to the fact that the paper was far from vintage Friedman and was not up to the standards of verbal rigor that he had established in the past. The paper also contained a clear mistake in its representation of the final figure plotted: Friedman's text and the figures' labels took the *x*-axis of the plot (of the standard deviations of real output growth and M2 growth) as referring to years, and so Friedman interpreted the observations from "70" to "79" as the 1970s, when, in fact, the *x*-axis merely registered the observation number.[315] The substance of his argument was, however, not affected by this error, and the paper was considerably more like a research piece than Friedman's *Journal of Economic Perspectives* article.

The article also allowed Friedman to cover an area of John Taylor's research that he had been

second moments. Friedman did not believe in the long-run correlation between money and *output*. The series that he believed had a long-run relationship were money and *nominal income*. The 2006 plot pertained not to any long-run correlation between nominal money and real output but, rather, to the relationship between the series' *second moments*. A more valid criticism of Friedman's 2006 paper—not the one Rivot made—would be that he provided no explicit reconciliation between the correlation between the second moments of money and output series with his notion that the Federal Reserve's alleged adoption of thermostatic control had eliminated the relationship between the two series' variability. It is the longer horizon (123 years) of data in the 2006 paper that is the source of the reconciliation, as stressed here and in Nelson (2007).

[311] Friedman (2006b), published posthumously as Friedman (2010).

[312] This reduction in the standard deviation of M2 growth observed over the Greenspan era was noted under "Into the New Economy" above.

[313] Friedman (2006b; 2010, p. 118).

[314] Friedman (2006b; 2010, p. 118).

[315] Although he did not correct this error in the light revisions to the paper that he made in August 2006, Friedman had apparently acknowledged it in correspondence by email with Samuel Brittan.

discussing with Taylor for some time. In the period since Taylor had returned to Stanford University in 2005 after several years in the Bush administration, Friedman and Taylor had been jointly contemplating the "Taylor curve"—an attempt to represent the tradeoff between inflation and output gap variability (Taylor, 1979b).[316]

Taylor recalled, "He said: Look, if you look at the data, you don't see a negative relationship between output variability and inflation variability: [instead,] you see this more positive relationship [in which], when inflation variability came down, so did output variability. And so our discussion was about whether that's because the curve shifted, as I would say." Friedman's discussion of the Taylor curve in his paper consisted of little more than an exposition of the logic behind the curve. But that he would discuss and cite what was—for Friedman—a comparatively late-vintage paper like Taylor (1979b) was itself unusual, as was the fact that the article was in *Econometrica*—a journal with which Friedman had once had an editorial affiliation but whose postwar articles he rarely cited.

Friedman had prepared the paper for a Festschrift for David Laidler, and he and Taylor, seated together in the Bay Area, appeared by videolink to participants at the London, Ontario, conference on August 18, 2006. Friedman, who had turned 94 two and a half weeks earlier, gave a summary of his paper. His presentation was clear, if delivered flatly. He had shown more spark and humor at a talk he had given a few weeks earlier (discussed in Section I above) to an audience of voucher advocates—perhaps because the subject of education reform energized him more these days than did the topic of monetary policy.

Friedman nevertheless went on to show more animation later in the August 2006 session once Taylor had concluded his comments and a back-and-forth exchange ensued between them. In this exchange, it became clear that, notwithstanding their shared interest in monetary policy rules, the two of them continued to differ on the appropriateness of responding to the output gap.

In the case of Friedman, the event was to be his final public appearance. In the case of Taylor, the occasion was one he would soon consider a career highlight. It had been an opportunity to appear alongside Friedman in a discussion of monetary policy: "a moment with the master." (Taylor, 2006.)

[316] Taylor (2006) recalled that he and Friedman had been discussing the Taylor curve in the months leading up to Friedman's drafting of the tradeoff paper. In elaborating on these discussions, Taylor (2012, p. 91) noted that, in the course of them, he brought to Friedman's attention Bernanke's (2004a) discussion of the Taylor curve.

BEN BERNANKE

The instance of John Taylor was something of an exception to a pattern as far as Milton Friedman's interactions with Stanford University's economics world were concerned. As stressed in preceding chapters, after Friedman became affiliated with the Hoover Institution in 1977, he did not become a regular face in the broader economics scene of Stanford University. In interacting with Friedman regularly soon after Taylor joined the Department of Economics (and well before Taylor became affiliated with the Hoover Institution), Taylor was rare among the university's economists. Like Michael Boskin and Robert Hall, who had longstanding dual Stanford Economics Department/Hoover Institution affiliations, Taylor had a lot of dialogue with Friedman. But, major campus events aside, Stanford University economists who did not have a Hoover Institution affiliation typically rarely saw him. Economics department members such as John Pencavel and Kenneth Arrow had some notable interactions with him, but these were spread out over Friedman's decades of affiliation with the university.

A representative case was Paul Evans, a University of Chicago graduate, who received written comments on some of his work from Friedman during Evans' time as an economics department member at Stanford University in the late 1970s through the mid-1980s, but who saw little of Friedman in person. "He went to the Hoover Institution when I was at the department at Stanford. But, despite being 100 or 200 feet distance from one another when he was at the Hoover Institution, … the contact would be negligible after I left Chicago." (Paul Evans, interview, February 26, 2013.).

An additional further exception to this general pattern arose with the arrival at Stanford University of Ben Bernanke. On completion of his graduate studies at MIT, Bernanke received offers both from the university's economics department and its business school, the Stanford Graduate School of Business. He accepted the offer from the latter institution. His remit was to raise the school's profile in macroeconomics. Bernanke's years of his affiliation with the business school spanned 1979 to 1985, and in the course of this period Bernanke would get to know Friedman well.

An early familiarity with Friedman's work

The editions of a couple of American newspapers over the weekend of May 8–9, 1965, contained items that harked back to the very beginning of Friedman's exposure to economics in

1928–1932. As it turned out, the weekend news items also highlighted a figure who would play a key role in national monetary debate in the closing years of Friedman's career in the 2000s. On Saturday, May 8, 1965, New Jersey newspapers reported that Friedman, then winding up a year based at Columbia University, would give a talk on the evening of May 11 discussing the the topic, "Intentions Versus Results on Economic Policy," at an event to be held on the campus of Rutgers University, in a rare return to the place of his undergraduate studies from 1928 to 1932 (*The Courier-News* (Bridgewater, New Jersey), May 8, 1965; *The Daily Home News* (New Brunswick, New Jersey), May 8, 1965).[317] On Sunday, May 9, South Carolina newspapers reported that the winner of the previous day's state spelling-bee championship was 11-year-old Ben Bernanke (*The State and the Columbia Record* (Columbia, South Carolina), May 9, 1965; *Greenville News* (South Carolina), May 9, 1965).

Bernanke was, at this stage, not yet a reader of Milton Friedman's work. But he soon would be. Although a *New York Times* profile (January 20, 2008, p. 38) of Bernanke would claim that "Ben Bernanke's first exposure to monetary policy was reading the works of Milton Friedman… when Bernanke was a graduate student at MIT," this characterization is erroneous—conflating as it does the time that Bernanke first read the Friedman-Schwartz *Monetary History*—which indeed was during Bernanke's graduate studies (see Bernanke, 2002a; 2015, p. 30)—and Bernanke's first exposure to Friedman's writings. Bernanke had, in fact, been reading Friedman since he was a teenager: for example, he read followed, after they began in 1966, the *Newsweek* columns—a practice that gave him early exposure to Friedman's views on monetary policy. "I know I used to read them when they came out." With regard to Friedman's writings in books, "I think I might have read some of it even as early as high school. I remember reading *Capitalism and Freedom* pretty early." (Ben Bernanke, interview, February 19, 2014.)

Subsequently, as an undergraduate at Harvard University, Bernanke became immersed in the economic-research world. Dale Jorgenson recalled (interview, September 12, 2014): "I first met him when he was a senior. He took my graduate course in econometrics. That was a first-year course for graduate students, and the normal requirement for undergrads, obviously, is an undergraduate course. And I'm pretty sure he'd had that, because he knew quite a bit of econometrics. And so he took this course, and he asked me to supervise his thesis, more or less

[317] Although Friedman stated on a number of occasions (for example, in *Pacific Business*, September/October 1972, p. 8) that he had hardly been back to the Rutgers University area since receiving his college degree in 1932, this was one instance in which he did make a reappearance on the campus. In addition—although, in this case, there was no return to the campus—Friedman had recognized his Rutgers University education by contributing his 1955 vouchers article to a Rutgers University Press volume published in honor of one of his former instructors.

at the same time. And I worked with him on his thesis. We subsequently turned that into a paper. And it involved a lot of interaction with him." Consequently, an outgrowth of Bernanke's time at Harvard college was publishing a paper at age 21 with Jorgenson (see Bernanke and Jorgenson, 1975).

It was also at age 21 that Bernanke began graduate study at the MIT economics department. Starting in his first year, Jorgenson recalled, "he had one of the great teachers, Stan Fischer, and had a close relationship with him." (Dale Jorgenson, interview, September 12, 2014.)

Bernanke would credit Fischer, his monetary-economics teacher and later Ph.D. dissertation adviser, with getting him interested in monetary economics.[318] Bernanke had, of course, been exposed to the macroeconomic debates in which Friedman was involved while at Harvard University—"at the time, macroeconomics was certainly portrayed as a Samuelson versus Friedman kind of thing"—but the Fischer course brought monetary analysis and policy into stronger focus. "I enjoyed the course, and I remember going to talk to him. And I don't remember how the conversation exactly moved in that direction, but I told him that I thought macro was really interesting because the stakes were so high—because macroeconomic policy decisions have very first-order effects on the economy and on welfare… I was talking with him about working in this area and, as I recall, he suggested that I read Friedman and Schwartz." (Ben Bernanke, interview, February 19, 2014.)

Fischer told Bernanke that if he did not like the *Monetary History*, he would not like monetary economics (Bernanke, 2015, p. 30). But Fischer added that the converse was true: "kind of a wry comment that if I liked that [book], then that would show that I was interested in monetary economics. I remember reading most of that. I'm not sure if I read the whole thing. I also read some other things, [including] a book by Bray Hammond [1957], called *Banks and Politics in America: From the Revolution to the Civil War*. It was a good complement to Friedman and Schwartz. I believe I read that in graduate school... I found the history interesting. I don't think the economic-history course was required, but, anyway, I did take economic history, from Peter Temin." (Ben Bernanke, interview, February 19, 2014).[319]

[318] He did so, for example, in remarks at a farewell event held on the evening of Bernanke's final FOMC meeting. In these remarks (January 28, 2014), Bernanke remarked of Fischer: "He bears the heavy responsibility of getting me interested in monetary economics in the first place."

[319] Unlike Fischer, Temin, of course, was not an admirer of the *Monetary History*. Rather, as Bernanke observed, "Peter had just written a book that was critical of Friedman and Schwartz, called *Did Monetary Forces Cause the Great Depression*? [1976]. And I wasn't sufficiently advanced to question or dispute his work, but I read it with interest." Bernanke added: "And of course, Temin later came on to the Friedman and Schwartz position when he

Jorgenson recalled of the influence of Fischer, in particular: "that was, I think, the determinant that led him to his interest. I was quite surprised when he became interested in Friedman's work. Through the [monetary] history—that was the main connection, as I see it. And so that certainly surprised me. I didn't recall anything in his undergraduate career that would have led him in that direction. But he's stuck with that for a long time, so that obviously reflects his orientation. That's what he's really interested in." (Dale Jorgenson, interview, September 12, 2014.)

Fischer remarked of those who took his graduate class in monetary analysis: "I certainly did say to them that I thought people who came out of MIT with a Ph.D. specialization in monetary economics ought to have read that book." (Stanley Fischer, interview, August 30, 2013.)

Bernanke would recall in 2002: "I first read *A Monetary History of the United States* early in my graduate school years at MIT. I was hooked, and I have been a student of monetary economics and economic history ever since." (Bernanke, 2002a.)

Frederic Mishkin, who was a few years ahead of Bernanke in MIT's graduate economics program, would recall: "Ben Bernanke and I were *very* influenced by Friedman. What I think is really actually, in a sense, remarkable, is that there was this whole group of MIT people, who eventually did work that was *extremely* influenced by Friedman… We were coming from a very different background. So, clearly, we were not very Friedman-like in the way that many University of Chicago people were. [We were] much more New Keynesian, in the sense that we worried about a lot of rigidities that can actually make monetary policy effective in different ways than Friedman thought about it, and… policies which would actually try to stabilize economic activity, which Friedman was less focused on, because he thought it would be done so badly. And yet the actual *style* of research that actually led to a lot of this thinking was, I think, really *very* influenced by Friedman." (Frederic Mishkin, interview, June 18, 2013.)

Bernanke finished his dissertation, "Long-Term Commitments, Dynamic Optimization and the Business Cycle," in 1979—"my thesis advisers were Rudi [Dornbusch] and [Robert] Solow, but

wrote the shorter book on the Gold Standard and the Depression." (Ben Bernanke, interview, February 19, 2014.) Although seemingly hardly less negative about monetarism than the 1976 book had been, the later Temin (1989) study did, in effect, break from the 1976 analysis, and conform with the *Monetary History*, by assigning an important role to monetary forces in the Great Depression (see, for example, the emphasis on high real interest rates on pages 64 and 92 of the later book). Temin's reservations about monetary explanations in the later book were mainly confined to specific issues—such as the value of monetary aggregates as indicators and whether the United States being part of the Gold Standard was an insuperable hindrance to the U.S. authorities providing monetary expansion.

Stan was a primary adviser" (Ben Bernanke, interview, February 19, 2014)—and joined Stanford University's business school in the same year.

Interactions at Stanford University

Bernanke who, as already indicated, had been hired to enhance the macroeconomic expertise of the business school, interacted extensively with other macroeconomists at the university—a pattern reflected in his co-teaching with Robert Hall of a graduate macroeconomics course in the 1979/1980 academic year (Colin Cameron, personal communication, September 1, 2015).

Friedman was away from Stanford University for much of Bernanke's first year on campus because of the completion of *Free To Choose* and the subsequent publicity tours. Bernanke recalls (see below) that he probably met Friedman during Bernanke's first three years of affiliation with Stanford University. But their interaction began in earnest in Bernanke's fourth year, when Bernanke took a year-long visiting-fellow position at the Hoover Institution.

"I may have met him informally or briefly, as he was spending part of the year at the Hoover Institution, which was across the street from the business school, where I was… [Physically,] it was situated immediately between the economics department and the business school," Bernanke recalled (February 19, 2014). He also observed, in connection with the visiting appointment described above: "In my second or third year at Stanford, I was given a Hoover fellowship, which was great because it gave me a year of not teaching, [allowing me more time] to do my research." For the 1982/1983 academic year in which Bernanke had the fellowship, "I remember in Hoover *per se*, I think the most regular function was a milk-and-cookies session every afternoon around four o'clock. And I used to try to go to those when I could, because I felt that they are paying my salary and I wanted to participate. And there would usually be a number of people there. Aaron Director would be one of them—and Friedman, certainly, when he was around, but he wasn't around all the time." As far as Bernanke's role as a visiting fellow was concerned, "the deal was that I would sit over in Hoover and socialize periodically with the group. And there were essentially no responsibilities to speak of, other than showing up [at events] once in a while. I got a lot of work done that year. But Friedman was around, and I remember meeting him at seminars and maybe a couple of dinners, Hoover events, at various times. His brother-in-law, Aaron Director, was also there, and I remember talking to him a few times. Friedman was obviously a pretty dominant personality at Hoover even though he wasn't there all the time." As far as Friedman-Bernanke interactions over this year were concerned, "I don’t want to overstate anything. But I talked to him a number of times. He was very friendly to me.” (Ben Bernanke, interview, February 19, 2014.)

“Well, you have to recognize what our relative status was,” Bernanke observed when putting their interaction at that time into perspective. “I mean, I was a brand-new assistant professor, and he was a god.” He added: “You couldn’t help but like the man.” A lasting impression for Bernanke, as was the case for so many in their recollections of their encounters with Friedman, was, “He was tiny, so he was like an elf in physical appearance,” together with the fact that “he was always cheerful and upbeat, and he loved to argue.” Bernanke went on: “I don’t think I would ever characterize any conversation I had with him as being a real debate. But, on a number of occasions, I would ask him a question and explore with him a little bit, though I’m sure I didn’t view myself as being someone who could stand toe-to-toe with him. It wasn’t really my nature to try—I was still really trying, at that stage in my career, to form my own views.” Bernanke noted that, of course, Friedman in addition “talked a lot about stuff that wasn’t monetary policy—he talked a lot about market solutions for all kinds of issues.” (Ben Bernanke, interview, February 19, 2014.)

Bernanke’s work on credit and the Great Depression

The 1982/1983 academic year that Bernanke spent at the Hoover Institution saw him complete his paper, “Nonmonetary Effects of the Financial Crisis in the Propagation of the Great Depression,” which would be published in the *American Economic Review* in June 1983. The subject matter of Bernanke’s paper confirmed the mark that the *Monetary History*, particularly its Chapter 7 on the 1929–1933 Great Contraction, had made on his research agenda. Mark Gertler would recall that when, in talking to Bernanke in the early 1980s, he was struck with Bernanke’s “fixation with the Great Depression” (wsj.com, December 17, 2013). “I just asked him, you know, why,” Gertler would observe—as work of this kind on the Great Depression “just seemed so different than what anybody else was doing. And I’ve said this on numerous occasions, but his answer was, ‘If you’re a geologist, you study the earthquakes.’ And he felt that by studying the Depression—an extreme situation—he’d gain a better understanding of how economies worked” in general. (Mark Gertler, interview, September 26, 2014.)

Bernanke’s paper emphasized the extent to which the contraction of bank credit—the asset dimension of the banking contraction of the early 1930s—and, in particular, of commercial bank loans had made the Great Depression worse. Because of this emphasis on the lending channel, the paper would sometimes be regarded as a challenge to the Friedman-Schwartz account of the Great Depression. Indeed, none other than Bernanke's former advisers Rudiger Dornbusch and Stanley Fischer presented matters in those terms in the fourth edition of their undergraduate text. Dornbusch and Fischer's (1987, p. 425) observation that the Friedman-Schwartz view "came close to being accepted as the orthodox explanation" of the Great Depression was appended by a footnote citing Bernanke (1983) as a study that "takes issue with the monetary view."

The Bernanke (1983) account is indeed a challenge to the Friedman-Schwartz view of the Great Contraction. But, although the paper's title might suggest otherwise, it was wholly not a nonmonetary account of the Depression, and it permitted an important role for monetary factors. The article's argument was that the contraction of bank deposits worsened economic activity not only for the reasons Friedman and Schwartz cited but also because the contraction in bank assets associated with the monetary contraction involved a squeeze on banks' lending to firms, which in turn lacked straightforward access to other external sources of funds. This account therefore supplemented, with another channel, the mechanisms that had been emphasized by Friedman and Schwartz. Because the Bernanke paper made the case for a transmission process that was not in conflict with the mechanisms stressed in the *Monetary History*, both Bernanke and Friedman emphasized the compatibility of their accounts.

Accordingly, Friedman would observe that the Bernanke story was "complementary" to that in the *Monetary History* because Bernanke's account "tend[s] to center around the monetary nexus" (Parker, 2002, p. 49). Bernanke (2002a) himself observed that "I have always tried to make clear [that] my argument for nonmonetary influences of bank failures is simply an embellishment of the Friedman-Schwartz story; it in no way contradicts the basic logic of their analysis."

He had, indeed, made this clarification on multiple occasions. Bernanke (1983, p. 257) had stated that his "paper builds on the Friedman-Schwartz work" and proceeded to suggest that money-centered approaches to explaining the 1930–1933 economic downturn might explain about half the decline in U.S. output (p. 269). Bernanke (1982b, p. 155) had remarked: "Financial crisis, acting both through monetary and credit channels, was an important factor in the Great Depression of the 1930s." Bernanke (1988, p. 5) indicated: "Proponents of the credit view do not exactly disagree with those who hold the money view." Rather, he went on, advocates of the credit view "just think that the money view does not go far enough in recognizing the pivotal role of banks (and other financial intermediaries) in the economy." And Abel and Bernanke (1992, p. 698) contended that "bank runs contributed to the severe drop in output during the 1930s both by causing the money supply to fall and by reducing bank lending."

More recently, in discussing the matter his Nobel lecture, Bernanke (2023, pp. 1148−1155), although he is certainly overtly critical of the *Monetary History* account on several grounds, including its "neglect of credit" (p. 1151), concludes that "credit-market disruptions complemented declines in the money supply as a force initiating and propagating the Great Depression" (p. 1155).

That Bernanke accepted the importance of the monetary forces that Friedman and Schwartz stressed was also brought out in 2000, in remarks that he wrote for a collection of his papers on the Great Depression. The preface of his *Essays on the Great Depression* stated: "I believe that there is now overwhelming evidence that the main factor depressing aggregate demand was a worldwide contraction in world money supplies."[320] By the time Bernanke was giving this retrospective at the end of the 1990s, the differences between his own account and that of Friedman and Schwartz had been put sharply into perspective by the advent of the real business cycle movement—some of whose members played down the importance of both the deposit and credit contractions in generating the Great Depression.[321]

Bernanke received positive feedback from Friedman on his 1983 paper. "He wrote me a few letters at various times. He wrote me a letter once complimenting me on my 1983 *AER* paper, saying that he thought it was really interesting work and that he thought that some of these credit effects were probably there." (Ben Bernanke, interview, February 19, 2014.)

This sympathy may seem surprising. Friedman definitely felt that credit had been given too much emphasis, *vis a vis* money, by central bankers and researchers. Frederic Mishkin recalled that in 1976 he was given a hard time by Friedman when presenting research at the University of Chicago money workshop (see Chapter 7 above), and "the paper I gave there was on household balance sheets in the Great Depression. And the reason, I think, that he didn't like it was that it

[320] Bernanke (2000, p. viii). The degree to which this passage represented an endorsement of money-centered accounts of business cycles *in the postwar period* should, however, be qualified. Because the wholesale banking system did not really blossom until the 1960s, a contraction in retail deposits in the 1930s tended to mandate a contraction of bank credit, likely including bank lending. Accordingly, a believer in the bank credit channel could still emphasize money supply contraction as setting off the process. In the postwar period, thanks to the growth of large managed bank liabilities not included in M2-type measures of money, it became much more feasible for bank lending and the M2 money stock to move in different directions.

[321] See also Nelson (2020a, Chapter 2) for a discussion of the differences between monetarist and real business cycle accounts of the New Deal-era economic recovery.

was very much in line with what Ben Bernanke [later] did… My paper was a precursor to Ben's paper, in that it was one of the things that stimulated Ben to write the paper on financial frictions and the Great Depression… The work that I presented was something I think that Milton had difficulty dealing with, because it was not a money supply-based discussion of the Great Depression." (Frederic Mishkin, interview, June 4, 2013.)

As argued previously (Nelson, 2020a, Chapter 6; and Chapter 12 of the present volume), however, Friedman's emphasis on money rather than credit stemmed partly from a conviction that money had a more stable relationship with economic activity—in particular because the demand function for money was more stable than that for either commercial bank or aggregate credit (or loans). His discussion of the 1980 credit-controls episode emphasized the importance of the spillover from the credit-market disturbance into deposit behavior. But it also confirmed that he believed that credit-supply shocks did indeed matter for economic activity in their own right. Indeed, in 1964 Friedman had written that variations in credit conditions could be "one of the channels through which changes in the quantity of money may affect income."[322] Friedman also acknowledged a role for credit supply shocks in the feedback he gave Bernanke on the latter's 1983 paper, as just indicated.

It would also be accurate to say, however, that Friedman did not emphasize the credit channel and that he believed that monetary policy exerted important effects in its absence—a position he reaffirmed in the 1990s (for example, *Wall Street Journal*, October 23, 1992, and December 17, 1997).[323] Bernanke's work did take a stance that differed from this position as well as from the consensus that Meek (1983, p. 71) had described in these terms: "U.S. economists would, I think, focus primarily on the pace at which bank assets and liabilities grow rather than on the composition of the assets."

Years at Princeton University

Bernanke moved to Princeton University in 1985, staying there until 2002. Whereas he saw

[322] Friedman (1964a, p. 9; p. 263 of 1969 reprint).

[323] In the first of these 1990s op-eds, Friedman emphasized that monetary expansion had occurred in the New Deal period even in the absence of expansion in bank loans and implied that this produced the economic expansion seen from 1933 (a position that he had articulated in the past—see Nelson, 2020a, Chapter 2). Bernanke (1983, p. 271) largely granted this point in stating that "the recovery of 1933−41 was financed by nonbank sources, with bank loans remaining at a low level." (Bernanke, 2023, p. 1151, however, links the weakness of bank loans to the slowdown in the recovery after "the initial burst of growth" in 1933−1934.)

Anna Schwartz often—both Bernanke and Schwartz being mainstays of the East Coast NBER meetings—Bernanke encountered Friedman very infrequently over this period. "I don't think I saw him. It's possible I never saw him—maybe I saw him once, but I didn't have any kind of regular relationship with him or anything during the time I was at Princeton." (Ben Bernanke, interview, February 19, 2014.)[324]

The imprint of Friedman's research on Bernanke continued, however. Princeton University's Gregory Chow was a former Friedman student, but in observing Bernanke at Princeton University, Chow often felt that it was Bernanke who seemed most like a Friedman student. "It surprised me," Chow would recall (interview, July 7, 2013). "Bernanke had an MIT Ph.D., but I think that Friedman had more influence on his work than anybody else that I know of."

Bernanke would later confirm that "much of my own research has followed up leads [coming] from the Friedman-Schwartz agenda" (Bernanke, 2002a). Indeed, even Bernanke's (1986) seminal contribution to structural vector autoregression analysis—although it pursued econometric techniques that owed next to nothing to Friedman's work—was motivated by a need to understand the money/output correlations that Friedman and Schwartz's 1960s publications had highlighted.[325]

This influence was evident not only in Bernanke's research but also in the undergraduate macroeconomics textbook that he produced with Andrew Abel. In the name index of the first edition (1992), Friedman's entry was two lines, and Paul Samuelson did not appear. In the fourth edition (2001)—the last to appear before Bernanke left university employment—Friedman's entry in the name index was now three lines in length, and Samuelson still did not have an entry.

With regard to the multiple mentions of Friedman, Abel remarked: "I think the reason is just that Milton had an impact in many different areas of macroeconomics. And he could've won more

[324] Bernanke and Friedman both attended the March 1998 Stanford University/Federal Reserve Bank of San Francisco conference discussed at the start of this chapter. But Friedman limited his attendance to the afternoon sessions of the first day of the conference. A greater opportunity for them to meet arose at the later conference in the series—that convened on March 2–3, 2001, titled "Asset Prices, Exchange Rates, and Monetary Policy," and held at Stanford University. Bernanke was a discussant at this event (see *https://www.frbsf.org/economic-research/events/2001/march/asset-prices-exchange-rates-and-monetary-policy-conference/*), and Friedman, unusually for him, attended both days of the proceedings.

[325] See Bernanke (1986, p. 49) and the discussion in Nelson (2020a, Chapter 1). Bernanke cited both of the 1963 Friedman-Schwartz writings at the outset of his article.

than one Nobel prize for his work, if they gave prizes for fields [within economics].” (Andrew Abel, interview, October 14, 2014.)

The emerging monetary policy consensus

As indicated above, Friedman occasionally tried to portray monetary aggregates and money demand as being crucial to FOMC policy discussions and to the economic stability of the Greenspan era—as he did in his outlines of the thermostat hypothesis. In a more realistic moment, Friedman suggested in a 2002 interview (Pringle, 2002, p. 15) that, if one had to look to a more lasting influence of his views on monetary matters, one should point to policymakers’ realization that inflation was a monetary phenomenon, as this realization had permanently changed the framework of monetary policy. “The major importance of monetarism was to establish once and for all that inflation is a monetary phenomenon, that monetary policies should be held responsible for inflation and that the task of monetary policy is to control inflation. That was more important than the emphasis on money itself.”

The implications of inflation being a monetary phenomenon was what Alan Greenspan focused on when he was one of the speakers at the White House event held on May 9, 2002, in Friedman’s honor. Greenspan suggested that Friedman’s work had made central banks accountable for inflation. “As I recall it, Alan Greenspan spoke from notes and did not have a written text, which may be why it is not listed on Fed's web page as all his formal talks are,” Friedman wrote a little later (email to author, May 18, 2002). “He was embarrassingly complimentary as were they all.”

Greenspan had previously, in 1997, gone on the written record, remarking of Friedman: “His views have had as much, if not more, impact on the way we think about monetary policy and many other important economic issues as those of any person in the last half of the twentieth century.”[326]

In central banking circles, an important consideration shaping monetary policy strategy in the 1990s was the acceptance of still another Friedman position: the natural rate hypothesis, which, together with the acceptance of the monetary source of inflation, carried the implication that it was not viable for monetary policy in the long run to achieve a level of real economic activity of

[326] Greenspan (1997).

its own choosing. "I find it remarkable how fundamentally stable our basic framework for analyzing inflation has remained over the past thirty-five years or so," Donald Kohn, of the Federal Reserve's Board of Governors, remarked in 2005. "That basic framework is essentially the expectations-augmented Phillips curve introduced by Milton Friedman and Edmund Phelps in the late 1960s."[327]

To state that this framework had been accepted for 35 years was an exaggeration as far as U.S. policy circles were concerned: 25 years would have been a more accurate estimate, as it was only in the period since 1979 that American central banking had really embraced, at the leadership level, the idea that inflation was systematically driven by an expectations-augmented Phillips curve, while also eschewing the notion that cost-push forces were a driver of ongoing inflation. That date of the change in thinking still meant that the framework was of long standing in U.S. policy circles by the 1990s, and in March 1994 Brian Madigan—at the time a senior Federal Reserve Board staff member, and later the head of the Board's monetary-affairs division during some of the Bernanke years—remarked: "modern economic theory generally holds that a stimulative monetary policy is not able to boost the level of aggregate real production and income in the long run… The natural rate hypothesis has proved particularly useful in the United States in analyzing the connection between real economic developments and inflation pressures. This hypothesis suggests that there is a 'natural' rate of unemployment, which is determined by structural labor market factors."[328]

In the same year, Alan Blinder, vice chairman of the Federal Reserve Board, was quoted as saying of the Federal Reserve's goals: "Inflation has to, by default, take primacy because that is what we can control in the long run."[329] On this score, prominent Keynesians like Blinder were helping provide the basis for David Laidler's (1997, p. 90) subsequent judgment that "one aspect of [monetarist] doctrine [that] still remains valid [is] that... the price level or the inflation rate... is the only variable over which [monetary] policy has any systematic long-term influence."

As Blinder often emphasized—and as was reflected in John Taylor's specification of the Taylor rule—neither the natural rate hypothesis nor the targeting of a long-run inflation rate precluded

[327] Kohn (2005, p. 5).

[328] Madigan (1994, pp. 112, 116).

[329] Quoted in Pakko (1995, p. 6). Blinder recalled of the prelude to his period (1994-1996) at the Federal Reserve Board: "When I got named to be vice chairman of the Board, a few people, who I thought of as intellectual adversaries, Milton being the most prominent, wrote me with good wishes—expressing how glad they were that I was going on the Federal Reserve, because they respected my views, my intellect, whatever. But I cherished that letter from Milton." (Alan Blinder, interview, December 6, 2013.)

there from being a real objective of monetary policy, provided that the real objective was appropriately specified. In particular, advocates of a policy response to the output gap in an inflation-targeting context like Bernanke and Mishkin (1997), Bernanke, Laubach, Mishkin, and Posen (1999), and Svensson (1999) had—in keeping with the "constrained discretion" that they advocated and that was described above—distinguished between the inflation objective and real objectives by stressing that only inflation could be controlled in the long run. They suggested that the appropriate policymaker concern with the output gap should be with its variance—which remains highly susceptible to central bank influence—rather than its mean—which the natural rate hypothesis states it cannot influence.[330] Equivalently, deviations of the output gap from zero should be the real-economic-activity objective and should constitute one reason for policy actions. As already stressed, this was a break from Friedman's nonactivist prescription. "The one idea he had that I think was well-motivated but was wrong in detail was his *k* percent money [growth] rule," Bernanke remarked (interview, February 19, 2014), while adding: "I mean, he had the right idea—he wanted some kind of rule that would constrain medium-term monetary policy and avoid wild swings in prices."

Research on monetary policy strategy

The Bernanke work of the late 1990s reflected a research program that he had pursued over the course of the whole decade—a set of projects concerned with empirical studies of the conduct of monetary policy. "Academic economists are often aloof [*vis a vis*] practical policy debates," Alan Blinder—who had frequently worked with Bernanke before Blinder left Princeton University for the Clinton Administration in 1993—observed in 1994, "but I have not been." In his documentation of this point, Blinder included a listing of one of "the last scholarly papers I published before joining the government"—the study of the federal funds rate and monetary policy transmission by Bernanke and Blinder (1992).[331]

Prior to this study, Bernanke and Blinder had usually followed the U.S. research practice of modeling central bank behavior in terms of quantities. In contrast, in the new study they argued instead for interpreting interest-rate innovations—in particular those coming from vector autoregression analysis—as monetary policy shocks. Such an approach had been explicitly advocated by McCallum (1983), but McCallum's suggestion had initially given rise to very little follow-up in the empirical VAR literature. Blinder recalled that he and Bernanke came to focus

[330] See Nelson (2020b, Chapter 13) for further discussion.
[331] Blinder (1994, p. 54).

on the interest rate in the empirical analysis of monetary policy actions due to "a combination of *a priori* beliefs, based on observing the behavior of the Federal Reserve, and also, then, statistical evidence that we unearthed, to our own satisfaction—that a better way to characterize the Fed's recent—circa '92, that's when that paper was published—behavior, except for the Volcker experiment, was that the Fed was controlling the interest rate, not the volume of reserves." (Alan Blinder, interview, December 6, 2013.) The notion that the FOMC had, since 1982, been controlling the federal funds rate, notwithstanding its continuing references to reserves in its public statements, was, of course, something that Friedman had stressed during the 1980s.[332]

In another 1992 study, Bernanke and Frederic Mishkin wrote about the evolution of monetary policy arrangements in the industrialized world over the previous decade and a half. Mishkin recalls that the authors partly modeled their approach on the analytical-narrative technique of the *Monetary History*, both he and Bernanke were "very influenced by [Friedman and Schwartz] saying that history could really inform us about what might be going on in terms of the transmission of monetary policy." (Frederic Mishkin, interview, June 18, 2013.). "Ben and I did this paper, which we thought was a really good paper," Mishkin continued, "[but] it was a disaster when we presented it." Their presentation in 1992 at the NBER's annual macroeconomics conference received negative feedback from both discussants, Martin Eichenbaum (1992) and John Taylor (1992c). Indeed, Taylor (2012, p. 90) would recall it as perhaps his most negative published discussion of a paper.

Although both Eichenbaum and Taylor made some arguments that Friedman likely would endorse—for example, Eichenbaum stressed the need for more discussion of data, and Taylor wanted more emphasis on rules for the monetary policy instrument—Mishkin was initially somewhat discouraged by the negative judgment given to their paper's attempt to revive a case study approach along Friedman-Schwartz lines. Later in the decade, however, Mishkin recalled, "we then engaged in a major project doing exactly the same thing," this time applying the case-

[332] See Chapter 13 above. As already implied, until 1992 it was prevalent in much U.S. research to treat short-term interest rates as largely market-determined, as well as to represent monetary policy reaction functions in terms of monetary or reserve aggregates (see, for example, DeLong and Summers, 1988, pp. 443–446). As of the early 1980s, Blinder had himself been inclined to model historical monetary policy in terms of reserves. Blinder (1994, p. 54), however, indicated that his recent years' experience involved having "watched the Fed's behavior closely from the outside," including as a magazine columnist, and in the process having "acquired a deep appreciation of the subtleties and difficulties of conducting monetary policy." With regard to his move toward treating the federal funds rate as being the policy instrument used in practice, Blinder suggested (interview, December 6, 2013): "I think that was a combination of me learning more, and another decade's worth of data coming in, of a more stable, less erratic-looking, monetary policy."

study approach to the experience that several countries abroad had had with inflation targeting. This culminated in the book, *Inflation Targeting: Lessons from the International Experience*, by Bernanke, Laubach, Mishkin, and Posen (1999). "That book has had tremendous influence," Mishkin observed. "Probably one of the things I'm proudest of in my career. It really made a huge difference in the world." (Frederic Mishkin, interview, June 18, 2013.)

In addition to following the Friedman-Schwartz template by focusing on an analysis of the documentary and historical record, the Bernanke-Laubach-Mishkin-Posen monograph also displayed evidence of the influence of other parts of Friedman's work. For example, the authors endorsed a two-year lag as the typical length of time between monetary policy actions and the major response of inflation, and as already indicated, they supported a monetary policy focus on inflation in the context of an acceptance of the natural rate hypothesis. They did, however, veer sharply from Friedman's strictures on the matter of the value for policymaking of monetary aggregates. Mishkin, in particular, was by now strongly critical of the usefulness of aggregates for U.S. monetary policy (see Estrella and Mishkin, 1997), although Bernanke had also become very doubtful about money stock measures (see, for example, Bernanke, 2006a).

After considering the international experience, the Bernanke-Laubach-Mishkin-Posen book came out in favor of an inflation target for the United States. Their suggested target rate was 2 percent.[333] This was in the range of values that Friedman had typically cited as corresponding to price stability. He had usually preferred a rate closer to zero, but he had modified his position somewhat, as the summary of a November 1991 talk that Friedman gave affirmed. The summary reported that "because of the difficulty in adjusting for quality changes," Friedman "would be very happy with a measured 2 percent inflation rate on the CPI as that would represent practical price stability."[334]

As already discussed, this rate of inflation was largely achieved from 1995 onward, particularly in the case of aggregate price indexes that were possibly more reliable than the CPI. Consistent with the natural rate hypothesis, the decline of inflation to a low rate was, as already discussed, largely achieved in the context of an expanding economy. This outcome contrasted with what had been foreshadowed in the testimony that Paul Samuelson gave at the end of 1992, when he postulated that the Greenspan Federal Reserve had an agenda to "create an inflation-less

[333] Bernanke, Laubach, Mishkin, and Posen (1999, pp. 316–317).
[334] In Oppenheimer and Company (1992, p. 5).

America by 1995, even if the cost of that is a half-a-decade loss in jobs and job opportunity."[335]

In January 2012, several years after Friedman's death and during Bernanke's second term as Federal Reserve chair, FOMC policymakers specified a longer-run inflation goal of 2 percent, measured using personal consumption expenditures (PCE) inflation, as being the price-stability component of the FOMC's objectives of price stability and maximum employment.

An explicit numerical inflation objective remained only a hypothetical possibility when, at the March 1998 Federal Reserve Bank of San Francisco annual conference, Frederic Mishkin had presented, in Friedman's presence, a summary of the forthcoming book's argument for a U.S. inflation target. In comments given from the floor, Friedman acknowledged the successes of inflation targeting in other countries, but he worried about accountability problems. Monetary policy actions took about two years to show up in force in inflation, Friedman noted. Therefore, if inflation overshot the target, "who's accountable" for the overshoot? Current policymakers or those—perhaps now out of office—who made decisions two years ago? Friedman pointed to a rule for growth in the monetary base as being preferable on the dimension of accountability.

The answer provided to criticisms of this kind by inflation-targeting advocates was that accountability could be secured by the formalization of central banks' reporting process. Bernanke, Laubach, Mishkin, and Posen (1999), as noted, acknowledged the two-year lag but argued that the appropriate monetary policy in that context was an inflation-targeting framework that focused on the inflation forecast. Indeed, the Bank of England already put great emphasis on the horizon of the target, and the forecast of inflation, as part of its inflation-targeting procedures in the United Kingdom. It was possible to generate a process of accountability in such a framework via a routine of regular publication by the monetary authority of its inflation forecast, by the articulation on the part of monetary policy committees of the reasons underlying their policy decisions, and by this information receiving scrutiny from members of the legislature and the general public.

Encounters with Friedman in the 2000s

Bernanke became a member of the Federal Reserve Board in the second half of 2002, and it was in speeches as a Board governor in 2002 and 2003 that Bernanke participated in two separate

[335] Testimony of December 30, 1992, in Joint Economic Committee, U.S. Congress (1993, p. 13).

events in honor of Friedman.

The first of these speeches was for an event at the University of Chicago on November 8, 2002, celebrating Friedman's ninetieth birthday of the previous July. Bernanke (2002a) opened by remarking: "I can think of no greater honor than being invited to speak on the occasion of Milton Friedman's ninetieth birthday. Among economic scholars, Friedman has no peer." The "economic scholar" formulation may have been a way of emphasizing Friedman's historical research. It also provided a means of avoiding any claim that Friedman was the world's greatest living economist—a difficult claim to sustain with Paul Samuelson still around.[336]

It was, however, the end of Bernanke's speech that would become widely known. Bernanke's speech had reviewed the Friedman-Schwartz work on the Great Depression in light of the subsequent literature. Bernanke judged that their hypothesis about the Federal Reserve's responsibility for the Great Contraction had been vindicated. Bernanke used the penultimate paragraph of the speech to speak directly to Friedman and Schwartz, both of whom were in the audience. "Let me end my talk by abusing slightly my status as an official representative of the Federal Reserve. I would like to say to Milton and Anna: Regarding the Great Depression. You're right, we did it. We're very sorry. But thanks to you, we won't do it again." (Bernanke, 2002a.)

The speech was not particularly widely discussed in the following six years.[337] In fact, a talk that Bernanke gave around the same time (again citing Friedman, this time in the context of Friedman's helicopter-money metaphor), on the subject of fighting deflation (Bernanke, 2002b), was initially much more well known. The 2002 speech on the Great Contraction came into its

[336] Robert Townsend, who had been at the Department of Economics at the University of Chicago since 1984, moved to MIT in the 2000s, initially as a visiting professor in 2001, 2006, and 2007, before joining permanently in 2008 (see *https://economics.mit.edu/sites/default/files/2023-05/cv_townsend_0.pdf*). Samuelson was a regular at the Wednesday departmental lunches and, on one occasion when Townsend discussed the University of Chicago with his new colleagues, Samuelson appeared—mentioning that he, too, had once been part of the University of Chicago economics scene, but leaving before elaborating. Townsend followed up on this and went to see Samuelson in his office. Although their ensuing conversations concerned the University of Chicago, Samuelson in the event did not much discuss his time (about 70 years earlier) as an undergraduate student at the university. Instead, he focused on the subject of Milton Friedman and their past debates and seemed to be contemplating what he could have done differently—Samuelson apparently being frustrated that he was not perceived as having come out on top in the exchanges. Townsend was puzzled at this focus, in view of all of Samuelson's career accomplishments. Townsend was also disinclined to judge between Friedman and Samuelson—as he considered both of them to be giants in economics and valued highly the research contributions of each of them. (Robert Townsend, interview, November 14, 2014.)

[337] It was discussed in Nelson and Schwartz (2007, 2008).

own from 2008 onward when commentators endeavored to explain the Federal Reserve's response to the Global Financial Crisis. Bernanke's direct words to Friedman and Schwartz then became among his most quoted remarks—"my famous statement about the Depression," as Bernanke recalled it (interview, February 19, 2014).

On October 24, 2003, Bernanke was again appearing at a conference in Friedman's honor, this time at the Federal Reserve Bank of Dallas event previously mentioned in this chapter. The conference was nominally about the Friedmans' *Free To Choose* project but, in practice, it covered various aspects of Milton Friedman's past research and public-policy activities.

The peculiarity of Bernanke giving speeches on Friedman in back-to-back years was one he acknowledged by joking, "I am ready and willing to praise Friedman's contributions wherever and whenever anyone will give me a venue."[338] Bernanke rejected the option of simply repeating his 2002 talk or giving a variant on it. He recalled: "I had a total of something like 300 speeches and testimonies [while] at the Fed. Maybe there were two that I repeated… But, other than those two, it was always my feeling that public remarks ought to be different." (Ben Bernanke, interview, February 19, 2014.)

The new speech, initially titled "The Influence of Milton Friedman's Monetary Framework on Contemporary Monetary Theory and Practice," and subsequently published in 2004 as "Friedman's Monetary Framework: Some Lessons," shifted focus from the 2002 speech's concentration on the *Monetary History* toward the matter of Friedman's propositions about monetary policy's effects. Notwithstanding the speech's title, the speech centered itself not on Friedman's 1970 "Theoretical Framework" paper but, instead, another 1970 Friedman publication—Friedman's September lecture on the counterrevolution in monetary theory.[339] This Friedman talk was one given outside the United States and had not been reprinted in 1987's *The Essence of Friedman*. It was not one of his best-known publications.[340] But it was a convenient one from the perspective of Bernanke's review, as it contained an enumeration of various Friedman propositions about monetary policy, including those expounded in his American Economic Association presidential address. Bernanke could therefore use this list and show that its propositions about lags and about the effects of monetary policy on real and nominal variables

[338] Bernanke (2004b, p. 207).

[339] That is, Friedman (1970b).

[340] Friedman had, however, cited it in Snowdon, Vane, and Wynarczyk (1994, p. 174) as the place in which he had listed the propositions of monetarism. Much of this list appeared in revised form in later Friedman writings.

lined up closely with modern perspectives.

"In preparing this talk, I encountered the following problem," Bernanke (2004b, p. 208) observed. "Friedman's monetary framework has been so influential that, in its broad outlines at least, it has nearly become identical with modern monetary theory and practice… His thinking has so permeated modern macroeconomics that the worst pitfall in reading him today is to fail to appreciate the originality and even revolutionary character of his ideas, in relation to the dominant views at the time that he formulated them."[341]

This was a point that various commentators had noticed when looking back at Friedman's work. His former student, Levis Kochin, would put it this way (interview, April 23, 2013): "There are some things with which he is identified which are accepted which were controversial once, when [originally] stated—but are now accepted by the great majority of economists who work in the area." Kochin added, however: "There are some things which got a substantial degree of acceptance at one point which are now rejected by almost everybody. I think that the literal monetarism of using movements in the money supply, some monetary aggregate, to predict movements in, for example, the price level is done [now] by almost nobody."

Likewise, Bernanke's 2003 talk, like Greenspan's earlier one, pointed to monetary aggregates as a matter on which policymakers' position now differed materially from what had been expressed in Friedman's work. Indeed, Bernanke labeled Friedman's emphasis on monetary aggregates as a monetary policy indicator as the "only aspect" of his framework that had not survived.[342] Bernanke could limit this catalogue to one item because the Friedman list in 1970, being concerned with empirical regularities rather than policy prescriptions, had not included his statement of a preference for the monetary base over short-term interest rates as the policy instrument—an area in which the nonmonetarist position had clearly won the day. With respect to the other items on the list, however, Bernanke was struck by the extent to which Friedman's monetary positions had prevailed.

Bernanke wrapped up by saying, "one can hardly overstate the influence of Friedman's monetary framework on contemporary monetary theory and practice. He identified the key empirical facts and he provided us with broad policy recommendations, notably the emphasis on nominal stability, that have served us well. For these contributions, both policymakers and the public

[341] See also the discussion in Chapter 7 above.
[342] Bernanke (2004b, p. 207).

owe Milton Friedman an enormous debt."[343] At the end of Bernanke's presentation, Friedman was asked if he had any comments. "I have nothing to say but thank you," he replied.[344]

One aspect of Bernanke's 2003 speech that was little commented upon but that echoed a theme of his 2002 talk on deflation was that he noted Friedman's belief that monetary policy worked through multiple channels—including channels that continued to operate if the monetary authority had exhausted its ability to lower the short-term nominal interest rate. As Bernanke recalled, the 2002 deflation speech covered "the monetarist view of asset markets—sort of a Tobinesque view really—… [implying] a general equilibrium system with multiple assets that aren't perfect substitutes for each other, [so there is] the portfolio balance channel." (Ben Bernanke, interview, February 19, 2014.) Bernanke invoked Friedman's views at an October 2008 FOMC meeting, during the Global Financial Crisis, when expressing confidence in the ability of monetary policy to stimulate the economy via asset purchases: "I think the thrust of the elementary approach to quantitative easing is the old Milton Friedman idea—that changing the composition of money and other assets changes relative returns. So it's a way to bring down returns on other assets and create stimulus even if the policy rate is down to zero."[345]

From Greenspan to Bernanke

Alan Greenspan had been reappointed Federal Reserve Chairman in 2003, but term limits on Federal Reserve Board members compelled him to depart at the end of January 2006. In the leadup to his departure, much commentary on monetary policy centered on how the Federal Reserve strategy that had emerged in the 1980s and 1990s might be systematized. Friedman's case for rules had partly rested on his view that "[w]e've got to have a system… which doesn't depend on whether you happen to have the right man pushing the buttons at the right time."[346] The Bernanke-Mishkin research of the 1990s, being concerned with policy frameworks, had reflected a similar sentiment. Although they eschewed a fully rules-based framework—and, especially, did not subscribe to the money-based rules of the kind Friedman had propounded—their advocacy of a more formalized monetary policy framework in the United States had been

[343] Bernanke (2004b, p. 214).
[344] Transcript of Federal Reserve Bank of Dallas conference, session 5, October 24, 2003.
[345] From Bernanke's remarks of October 28, 2008, in Federal Open Market Committee (2008, p. 59). To go into more detail here would violate this book's confining itself principally to the span of Friedman's lifetime. A detailed discussion of the interaction between Friedman's views and the Federal Reserve's policy actions in 2007–2009 appears in Nelson (2013).
[346] *Free To Choose* (U.S. television version, debate portion), PBS, Episode 3, "Anatomy of Crisis," January 26, 1980, p. 8 of transcript.

based on the notion that principles regarding monetary policy formulation could be set out in a way that ensured considerable continuity across successive Federal Reserve chairs.

It was the theme of continuity with Greenspan that Bernanke stressed when, about six months after Bernanke had left the Federal Reserve Board to be chair of the Council of Economic Advisers, President Bush nominated him to return to the Board as chairman.[347] The nomination was announced at a joint Bernanke-Bush appearance at the White House on October 24, 2005—by chance, exactly two years after Bernanke had delivered the second of his two tributes to Friedman.[348]

Business Week contacted Friedman about the nomination and reported him as being strongly supportive of the Bernanke nomination. "I think he's an able man who has the right view about monetary policy," Friedman said, going on to imply that he hoped the Federal Reserve would offer less public advice about fiscal policy under Bernanke than had been the case previously (*Business Week*, November 7, 2005).

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As noted in Chapter 16, a journalist who profiled Friedman just before he turned age eighty was struck by his continuing energy. By the time Friedman reached ninety, characterizations like this had to be qualified, with Anna Schwartz observing in April 2003 that "Milton is active on things that interest him" and that he could be judged extremely active "for a ninety-year-old."[349] A little over three years on, Friedman, while noting that he was "extremely lucky to have lived as long as I have," indicated that he had slowed down further. "I feel tired a good deal of the time. I don't have the same ability to do active work." (*Rutgers* magazine, Fall 2006, p. 24.)[350]

As Schwartz alluded to in her observations on Friedman, monetary policy had largely lost priority among Friedman's interests after 1992. But his periodic resurgences of interest in the macroeconomic area included the final phase of his life in 2005–2006. Around the time when his *Journal of Economic Perspectives* paper appeared in print, Friedman attended another

[347] See nytimes.com, October 24, 2005.
[348] Not staying out of the news himself, Friedman had been quoted in a front-page story in the morning's *Wall Street Journal* on another topic (school choice).
[349] Anna Schwartz remarks at a dinner conversation, *History of Political Economy* conference, Duke University, April 26, 2003.
[350] Friedman also referred to his inability to recall names, but he had been complaining about this aspect of his memory for years.

Federal Reserve Bank of San Francisco conference, this one on productivity and held on November 17−19, 2005.[351] The conference brought one of Friedman's old University of Chicago colleagues, Robert Gordon, into town again, and the Friedmans had him as a guest at their apartment as well as taking him to dinner. Gordon was struck by the fact that Milton Friedman was still driving—and did so in a car that bore the license plate *MV PY*.[352] "And then he drives us himself down the hills of San Francisco. It made me think of that famous Steve McQueen movie, *Bullitt*," Gordon would recall. "And then he turned to us when we came into the restaurant, and the *maître d'* greets him effusively, we sit down… and then he turns to us and says, 'You know why that *maître d'* gives me that greeting? Because, one time, I brought Alan Greenspan here.' He was extremely sharp one year before he died. While, in some cosmic sense, he had obviously slowed down, he was still writing *Wall Street Journal* op-eds." (Robert Gordon, interview, March 21, 2013.) He was also writing research papers—specifically, the *Journal of Economic Perspectives* article, and, in 2006, the paper that he drafted on tradeoffs in monetary policy.[353]

As already indicated, Friedman made minor revisions to that tradeoffs paper in August 2006, just after turning 94. He also made a written public intervention in mid-August 2006 when, with Friedman's permission, Gregory Mankiw placed on his blog an exchange of messages that had just taken place between Friedman and himself, concerning Mankiw's recent paper on the state of macroeconomic research.[354]

In the three months starting in the latter half of August 2006, the world closed in on Friedman. Activities that had been feasible even earlier in the year now became more and more out of reach. In 1992, in one of the many interviews that he gave from his and Rose Friedman's high-

[351] Like the regular Federal Reserve Bank of San Francisco conference held the preceding March, this was one of its events that Friedman attended during Janet Yellen's tenure as the bank's president. Yellen had begun serving in this position in June 2004.

[352] Friedman's license plate choice is also noted by Taylor (2006). Gordon's website includes a photograph that he took of the Friedmans by their car, albeit with the license-plate inscription obscured by the reflection of the camera flash (see *https://gordon.economics.northwestern.edu/middle-period-1996-2010/san-francisco-fed-conference-on-productivity-november-17-19-2005-inunderground-garage-at-2750-taylor-in-sf-ian-dew-becker-with-milton-androse-friedman/*). Friedman was not the only monetarist to have the equation of exchange as a license plate. During this period, Laidler had a license plate based on the Cambridge $M = kPy$ version, while William Poole's license plate was *MV PT*. Friedman continued to drive until he lost his license in July 2006 after failing the vision test component of the renewal requirements. (This period, however, preceded the sharp worsening of Friedman's vision discussed presently.)

[353] Because Balke and Gordon (1986) was cited in Friedman's *Journal of Economic Perspectives* article, Gordon became the last individual, other than Friedman and Schwartz, whom Friedman ever cited in a journal article.

[354] See *Greg Mankiw's Blog*, August 16, 2006. The paper under discussion was published as Mankiw (2006).

rise apartment, he had gestured at the view of San Francisco and remarked, "I have a very good life. Why shouldn't I be cheerful?" (*Forbes*, August 17, 1992, p. 45.) He then subsequently bounced back after various health setbacks that took place from 1994 to 1997, and the Friedmans were still living in the San Francisco apartment in the fall of 2006—prompting the title of an interview-profile "The View from Up There," along with a description in the profile of Friedman's surprisingly brisk walking around the apartment (*Rutgers* magazine, Fall 2006, p. 23). But by November 2006, Rose Friedman had determined that, in view of her husband's precarious health, they would need to leave their Russian Hill residence of nearly thirty years and move to an assisted-living community. Milton Friedman disliked the idea strongly but acquiesced, and arrangements for the move went ahead. In the event, he never lived to see the day of the move.

Rose Friedman's judgment that they had to leave their longtime home had flowed from the steep decline in Friedman's health that started in the late summer of 2006.[355] A key factor was his eyesight. He had long used a computer screen that had blown-up type to compensate for poor vision (*Los Angeles Times*, November 17, 2006, p. A27). But, during the second half of 2006, his optical capacity worsened sharply further. After banging his head hard in the bathtub, Friedman complained of blurry vision. Even aside, however, from the effects of this accident, it had already been determined by medical tests that an age-related, irreversible, and comprehensive erosion of his eyesight was in process. In August 2006, subsequent to the diagnosis of macular degeneration, but prior to his later steep deterioration in eyesight, Friedman had mentioned the diagnosis when two of the Friedmans' friends, Michael Mork and his son Peter, visited them at their apartment. Despite Friedman's reference to his deteriorating sight, his vision was serviceable at this time, and he went through, and commented on, a set of time-series graphs that Michael and Peter Mork had brought with them for the discussion (Michael Mork, personal communication, May 18, 2023).

Friedman could, in the early part of the fall of 2006, see well enough to compose by longhand an op-ed, published in the *Wall Street Journal* on October 6, in which he expressed concern that Hong Kong's traditional free-market economic policy would be dropped.[356] But on November

[355] This and the next paragraph draw in part on information that the author received in personal communication with Anna Schwartz in 2006 and with Gloria Valentine in 2013–2014.

[356] Rule of Hong Kong had been transferred from the United Kingdom to China nine years earlier. At the time of the changeover, Friedman had published a *Wall Street Journal* op-ed (July 8, 1997) titled "If Only the United States Were As Free As Hong Kong." The content of the op-ed made clear that the article should have had the title "Economically" before "Free." Absent that word, the title of the op-ed was senselessly provocative, as well as

14, he complained to his secretary Gloria Valentine that his eyesight had deteriorated so much that he now could hardly see. On November 16, 2006, Milton Friedman died of heart failure.

Later that day, Federal Reserve Chairman Bernanke issued a press statement, which was partly a paraphrase of his 2002 talk at the University of Chicago:

"Among economic scholars, Milton Friedman had no peer. The direct and indirect influences of his thinking on contemporary monetary economics would be difficult to overstate. Just as important, in his humane and engaging way, Milton conveyed to millions an understanding of the economic benefits of free, competitive markets, as well as the close connection that economic freedoms bear to other types of liberty. He will be sorely missed."[357]

Also on that day, John Taylor wrote: "I had seen Milton quite a bit in the last year, and I will miss him a lot. We will all miss the constant outflow of fresh ideas which he put forth with enthusiasm and kindness. What an inspiration."[358]

Anna Schwartz was in Washington, D.C., on the day of Friedman's death, but the following day she was back at her office in the New York City division of the NBER. Having worked at the NBER continuously since 1941, she had seen the New York office location shift several times. It was now situated within the City University of New York, in the same building as the university's economics department. A longtime member of that department, Alvin Marty, had been part of the economics scene in the Greater Chicago area during the late 1950s and early 1960s, and from that vantage point had seen the Friedman shakeup of monetary economics at close hand. By 2006, he had known both Friedman and Schwartz for over four decades. On the day after Friedman's death, Marty stopped by at the open door of Anna Schwartz's office. On seeing him, Schwartz expected Marty to talk to her about Friedman's death. To her surprise, however, Marty said nothing at all. Before moving along, he exchanged looks with Schwartz for a long spell. But he could not bring himself to speak about the passing of the era that both of them had lived through.

giving an inaccurate picture of the content of Friedman's piece—which was concerned with the good economic record of Hong Kong under U.K. rule.

[357] Federal Reserve Board website item, issued on November 16, 2006 (Bernanke, 2006b).

[358] John Taylor, email to the author, November 16, 2006.

On the same day, Schwartz wrote with regard to Friedman, “his death is a great loss to me personally, how much more so to Rose, and the world at large.”[359]

[359] Anna J. Schwartz, email to the author, November 17, 2006.

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