
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.” 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.” 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. 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. See the Introduction in Nelson (2018a) for a full list of acknowledgments. The views expressed here are the author’s alone and should not be interpreted as those of the Federal Reserve or the Board of Governors.
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 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*. 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. 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.” 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). But Prescott never published the

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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).
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.)

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’ (1994b, 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 Prescott, interview, February 16, 2016.)

After the RBC literature had become well established, Kydland and Prescott actually had an exchange in print with Friedman. But the exchange, which appeared in the Journal of...
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.\footnote{See Friedman (1997b) 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.}

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 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.).\footnote{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; 1971g, p. 851), for example.} 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 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 major aspect of the RBC literature’s approach to empirical analysis was its use of the Hodrick-Prescott (1980) filter as a means of obtaining second moments of series, with these second moments intended to represent the variance/covariance patterns of the data at the business cycle frequency. The development of the Hodrick and Prescott’s filter arose separately 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. 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

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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 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

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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). 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, 1994b, 2004a) indicated 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.)

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*. 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.

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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 Chapter 2 of Book 1 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 (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).
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. 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.

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.

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. Nelson, 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).
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,

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21 See the next chapter.
Michael Hamburger in 1986, and Karl Brunner in 1989. An additional monetarist who passed away during the 1980s was Friedman’s former teacher Homer Jones, who died in 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.”

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

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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, 1985c) 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, 1986f, p. 329).

23 After Jones’ death, the New York Times took the unusual step of publishing a condensed version of Friedman’s 1976 Journal of Monetary Economics tribute to his former teacher (New York Times, March 23, 1986). This item was mentioned in Nelson (2018a, Chapter 9).

24 Friedman (1987c, p. 10).
reiterate his criticisms of Burns’ record. Friedman did this, for example, in a meeting with 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 an unrestrained course that he said gave rise to both economic instability and the deterioration of the thrift industry.

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.

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.

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25 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.
26 Friedman (1992c, pp. 251–252).
27 Friedman reaffirmed his high estimation of 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, 2018a, Chapter 8) and in an interview with Michael Parkin (Parkin, 1990, p. 100).
28 Friedman (1993b, p. 773).
Stigler, unlike Friedman, had retained an active University of Chicago affiliation, and as part of his duties at the university Stigler had been a coeditor of the *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. Friedman’s research on this matter developed into a piece issued in 1987 as a Hoover Institution working paper. 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.

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.

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

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29 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.
30 See Friedman (1987f).
31 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.
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 Reserve’s pegging of securities prices in the 1940s and early the 1950s.33

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.34 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

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33 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, attributing 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, when in other 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 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; however, this does not appear to have been the case for most of the pre-1951 period (see Chaurushiya and Kuttner, 2003, p. 8).

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.

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, a bumper-sized trade paperback collection of reprinted Friedman articles that the Hoover Institution issued in 1987 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.35

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, October 28, 1987; National Review, November 20, 1987). On a panel on Nightline in November 1987, in which Friedman featured with Robert

35 A Congressional submission (Friedman, 1958b) 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 (1970a) was reprinted in Monetarist Economics (Friedman, 1991c). 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.)
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 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*, 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.”36 In a joint Friedman-Samuelson television appearance in 1990, Samuelson observed that the 1987 stock-market crash had showed 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.”37

*Friedman and the later Reagan years*

A year almost to the day after the crash, Friedman was at the White House to receive the

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36 Both the Friedman and Schwartz remarks are from *Business Week*, April 18, 1988, p. 38.
(a) Growth rate of M2

Percent

(b) Interest rates, quarterly average

Percent

(c) Real and nominal GDP, four-quarter growth rates

Percent

(c) CPI and GDP deflator, four-quarter growth rates

Percent

Source: Federal Reserve Bank of St. Louis’ FRED portal. Quarterly averages taken of M2 and CPI.

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 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.38

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

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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, 2018b) that Friedman possessed a “technical mastery of his profession [that] is unchallenged,” 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. Friednman also felt negatively during this period about the administration for ratifying what he saw as a too-expansionary monetary policy. 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.

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. 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.)

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39 See, for example, Friedman and Friedman (1985, p. 39).
40 See, for example, Friedman (1984i, p. 41). See also the previous chapter
41 See Friedman (1986e, 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.
42 See Friedman (1986e, p. 245).
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 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.43 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).44 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.”45

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 (Wall Street Journal, March 4, 1988). Trade policy was 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.”46 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.”47

44 In Stanford Review (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 to 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.
45 Friedman (1988c, p. 380).
46 Again, see Friedman (1988c, p. 380).
47 From Friedman’s July 28, 1988, remarks, in Friedman (1989c, p. 12).
Friedman’s disillusionment with Reagan’s record on matters concerning international trade had begun early. “I am a strong proponent of President Reagan’s general policies and he is a strong believer in free trade,” Friedman had said in March 1982. 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 believing that Reagan would hold the line against the protectionist movement (*Newsweek*, November 15, 1982). Friedman declared himself “severely disappointed,” and about a year later he listed a number of trade-restricting measures introduced as Reagan including “voluntary” quotas and higher tariffs on certain automobiles, as well as export subsidies in agriculture (*Wall Street Journal*, October 25, 1983). These measures came, Friedman complained, despite eloquent statements from Reagan about the benefits of free trade.

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.” 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 (*Lodi News-Sentinel*, 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

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48 Friedman (1982a, p. 43).
49 See also Friedman (1984i, p. 40).
50 See Friedman’s remarks in the aforementioned *Wall Street Journal* article of October 25, 1983. See also Friedman and Friedman (1985, pp. 124–125).
51 MacNeil/Lehrer News Hour, PBS, April 17, 1987, p. 4 of transcript.
kind receded in the 1990s with the anemic performance of Japan’s economy. Friedman would be quick to spot Japan’s entry during the early 1990s into a period of stagnation, with a *Wall Street Journal* article he wrote (October 23, 1992) citing the collapse in monetary growth in Japan as a factor.

**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.\(^{52}\)

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.) 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

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\(^{52}\) See Friedman’s remarks in *National Review*, June 30, 1989, and in Friedman (1992c, p. 251).
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, 2013a). Indeed in the wake of the S&L crisis, Friedman later noted that “a substantial equity cushion” gives banks “ample incentives to avoid excessive risk.”

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.

Friedman even ventured to state that, had his 100 percent reserves proposal in 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 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 (2018a, 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. 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 1991. 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.” Like many others, Friedman 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. 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 this event 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.

**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

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56 For his part, as we have seen, Friedman supported the Reagan defense buildup. He was 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 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 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 cited by Gray—inducing economic strain by obliging the USSR to increase its defense spending further—was a more clear-cut burden on the USSR, yet Schweizer’s narrative downplayed this factor in favor of the policy of blocking the Soviet Union’s access to trade.


58 See Friedman and Friedman (1988, p. 466).

59 See the discussion titled “George Herbert Walker Bush” in Section III of this chapter.
Choose producer Robert Chitester to film a new episode of *Free To Choose*, in which Friedman would take a victory lap of sorts in the Eastern European countries. 60 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. 61 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.) 62

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

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60 See Friedman and Friedman (1998, pp. 505–515).
61 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 left unchanged from the original (Friedman and Friedman, 1990, p. x).
62 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).
sold probably one-twentieth as many, five percent as many copies, namely *A Monetary History of the United States*, which I wrote jointly with Anna Schwartz…’63 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.64

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.65

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 1987.66 In October of the same year, he spoke of his...
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. 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, 1989a, p. 76). He stuck to that judgment, but the following years would see him revisit, in a piecemeal fashion, in 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. 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.

*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. 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. When the book appeared in 1992, however, the present author was

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67 See Friedman (1989a).
68 Friedman (1991d).
69 Friedman (1990b, 1990c, 1992e). 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, in effect duplicating what was already available in book form.
70 See Laidler (1993b) as well as his and Lars Christensen’s remarks in *The Market Monetarist*, July 1, 2013.
71 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.)
disappointed by the content of *Money Mischief*. That view did not change and when it was voiced to Anna Schwartz in 2009 shortly before her aforementioned stroke, she expressed a similar view, referring to the book as possessing a “catchy title” but not enough substance or ambition. (Conversation with Anna J. Schwartz, September 18, 2009.) 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*. 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. 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.

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

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72 On the “catchy title” assessment, Schwartz was in agreement with Laidler (1993b).
73 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. 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).
75 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.
Quantity of Money” and a concluding section based closely on Friedman’s 1987 New Palgrave entry (which, by 1992, was available in at least two paperback publications, as well as the hardback Palgrave version).76 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.77

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.78

76 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.

77 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.

78 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, 1984, 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
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.79 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” (*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

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79 The review was Williamson (1991). The book under review was Baumol, Blackman, and Wolff (1989).
would see things the same way as he. So, it was a relatively small issue [under discussion], but, 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.\textsuperscript{80} He had tried to pull away from the study of money with the release of \textit{Monetary 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.”\textsuperscript{81} But the beating that monetarism took in 1982–1986 had brought him back into the fray.\textsuperscript{82} In the early 1990s, however, with the disinflation of the previous decade locked in, \textit{Money Mischief} having reached completion, and Friedman’s refocus on M2 having seemingly 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” (\textit{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.

\textbf{II. ISSUES, 1987–1992}

\textbf{DRUGS}

As was discussed in previous chapters, in the late 1970s and throughout the 1980s the \textit{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, \textit{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 \textit{Journal}.

One particularly strong attack on Friedman came in October 1986 from in a “Viewpoint” op-ed by one of the \textit{Wall Street Journal}’s regular writers, Alexander Cockburn. The critique of monetarism that Cockburn laid out was similar to that in many other \textit{Journal} contributions, but

\begin{itemize}
  \item \textsuperscript{80} His final briefing was in November 1991 (Oppenheimer and Company, 1992; Rudolf Hauser, interview, June 22, 2012).
  \item \textsuperscript{81} From the preface, dated November 20, 1982, in Friedman (1983b, p. ix).
  \item \textsuperscript{82} 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.–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.
\end{itemize}
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 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.\(^83\) In this connection, he would allege that the U.S. welfare system provided incentives for single-parent families.\(^84\) 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.

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83 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).
84 See, for example, Playboy (February 1973, as reprinted in Friedman, 1975e, 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, 1965c, p. 890. Tobin, 1965c, 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.”) Likewise, while Friedman (in Friedman and Tobin, 1990, p. 78, Oppenheimer and Company, 1992, p. 11, and Friedman, 1993c, p. 13) cited Charles Murray’s (1984) work for its suggestion that welfare programs promoted poverty by discouraging two-parent families and encouraging one-parent families, Robert Gordon (2015, p. 57) favorably cited Murray’s (2012) work in connection with much the same point.
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 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. 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.

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

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85 In addition, both Friedman and his wife indicated their support for the pro-choice position in an interview later in 1984 (California, 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, 1989a). 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.)

86 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.
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 actually endorse drug legalization may well have been correct.87

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 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.88 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.89 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 1979.90 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.91 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

87 In an article that appeared after the first draft of this chapter was written and circulated, 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 he documents with evidence from 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.


91 For further such cases, see the discussion in Chapter 14 of Book 1.
smoking, and that deterring smoking through the official health warning was much more
effective.92

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.93

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, 1970.94 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

92 For Friedman’s remark see Proprietary Association (1979, p. 32). Nader’s rebuttal to Friedman’s remark
appeared on page 35.
93 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).
94 In a 1991 radio interview, Friedman would say he could not remember a time that he was not in favor of drug
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, 1947a 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.
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.95

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 same points.96 Friedman 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. For 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.) Friedman watched at close hand a 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.”97 “Drinking was

95 Friedman (1970h, p. 7).
96 Shortly ahead of the column, Friedman discussed his preference for drug legalization in Instructional Dynamics Economics Cassette Tape 95 (March 22, 1972).
97 Monday Conference, Australian Broadcasting Commission, April 14, 1975, p. 4 of transcript.
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.)98 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.99

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 1984.100 Although the 1972 Newsweek column was not cited, the Friedmans’ 1984 discussion was essentially a rewrite and expansion of the

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98 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.

99 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 would be to legalize marijuana and tax it.” (Friedman, 1982c, 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.)

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.101 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, his 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.”102

101 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, 1989b, p. 369).
102 This Week With David Brinkley, ABC, p. 5 of transcript.
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.\textsuperscript{103} The Drug Policy Foundation Press would publish in 1992 a book titled \textit{Friedman and Szasz on Liberty and Drugs}, containing some of Friedman’s public statements on drug legalization (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 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 (\textit{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.\textsuperscript{104} He left such work to economists like Jeffrey Miron, with whom Friedman was sometimes in touch on the drug issue. 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.\textsuperscript{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

\textsuperscript{103} 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. 

\textsuperscript{104} Friedman did advocate drug legalization in an economics journal, albeit in the letters section of the often-nontechnical \textit{Journal of Economic Perspectives} (Friedman, 1997c). 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.

\textsuperscript{105} For Miron’s research on drugs, see for example Miron and Zwiebel (1995). Friedman penned an endorsement and introduction to a later Miron study (see Friedman, 2005d, and Miron, 2004).
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.”\textsuperscript{106}

In its editorial condemning Friedman’s position on drugs, the \textit{Wall Street Journal} had suggested that he should celebrate the start of the 1990s not by drinking champagne but by taking hard drugs (\textit{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 whatsoever, any of these drugs whatsoever,” he had told that audience. “So I’m not speaking from personal experience.” (CSPAN, May 7, 1993.)

\textbf{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.”\textsuperscript{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 \textit{Newsday} profile of the Chicago School that appeared in October 1986, although ostensibly concerned with current state of the school, used Friedman as the lead into the discussion—a reflection of the fact that, as the article pointed out, “[the University of] Chicago really hasn’t had a publicly recognizable name for ten years, since full-time economist, part-time media star Milton Friedman left for the West Coast.” (\textit{Newsday}, October 19, 1986, p. 86.) The article 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.”

\textsuperscript{107} Friedman (1977f, p. 14).
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 motivated financially to perform a kind of arbitrage function for their company’s stock. That is, given their 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).

\(^{108}\) 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.)

\(^{109}\) 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).
“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, October 8, 1988).

By this time, the high-profile insider trading cases in London and New York had produced an 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. 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

111 January 22, 1976, testimony, in Committee on Banking, Currency and Housing (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.)

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.)
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.”

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). 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 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. 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

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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.)

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.”

III. PERSONALITIES, 1987–1992

ALAN GREENSPAN

In 1983, Friedman had acknowledged that Paul Volcker’s Federal Reserve had “brought inflation down.” 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.

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 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, February 23, 1987.)

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

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117 Business and Society Review (Spring 1972, p. 6; also excerpted in Friedman, 1975e, p. 240).
118 Friedman (1984i, p. 41).
119 See Friedman (1984a, p. 3) and the discussion in the previous chapter.
120 This attitude to Volcker had already characterized Friedman’s title for his 1985 paper (Friedman, 1985c). 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.
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 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 Chairman.

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

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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, November 5, 2006.
123 Friedman (1987g, p. 365).
124 Exceptions include Friedman’s remarks in Friedman and Kristol (1976, p. 41), Newsweek, October 16, 1978, and Friedman (1986c, p. 8). Friedman referred to Greenspan numerous times on his cassette commentary series, both
Chairman, Friedman referred to Greenspan as “one of my best friends.”\textsuperscript{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.)\textsuperscript{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.\textsuperscript{127}

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 (\textit{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.\textsuperscript{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 (\textit{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, 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).

\textsuperscript{125} Friedman (1987g, p. 365).

\textsuperscript{126} The Greenspan article in question was Greenspan (1959). Greenspan also recalled his early correspondence with Friedman in Greenspan (2004).

\textsuperscript{127} See, for example, Friedman (1986c, p. 8) as well as Chapter 14 of Book 1. The meeting of the Commission for an All-Volunteer Force on February 21, 1970, was the only occasion that Friedman and Greenspan visited the Nixon White House on the same day. (Information from Nixon Presidential Library, May 23, 2014.)

\textsuperscript{128} See, for example, Greenspan (1966) as well as Chapter 15 in Book 1.
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 members of the Presidential Economic Policy Advisory Board during Reagan’s presidency. In 1983, Greenspan had written of Friedman: “I’m a long and ardent admirer of his.” 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).

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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 (1984i, p. 21).
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 see the development of a consensus in the economics profession in favor of centering monetary policy conduct on the federal funds rate.

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

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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 (1988) 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, 1990a, 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.)
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 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

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132 See his testimony of September 25, 1974, in Committee on the Budget, House of Representatives (1974), for the quotation (p. 85) and the chart (p. 88).
paper), it was the long-run M2-and-prices relationship that was the focus of the study rather than
the specification of inflation dynamics.\footnote{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?”} 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.

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 growth rather than the level of money and was in practice concerned with inflation rather than the absolute price level.\footnote{Gorodnichenko and Shapiro (2007) characterize the Greenspan Federal Reserve as 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” which policymakers’ statements have in practice used when discussing inflation.} 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).\footnote{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).}

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 indexing 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.  

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. 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.” 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  

138 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 (1984j, p. 165) similarly noted that 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, April 18, 1981.) (See also Friedman, 1974c, 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 the issue of that indexing wouldn’t arise.” (From his February 24, 1982, Congressional testimony, in Committee on Finance, 1982, p. 105.)  

139 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.  

140 Donald Ratajczac on Wall Street Week, Maryland Public Television, July 14, 1989, p. 9 of transcript.
Association meetings, a talk that Friedman probably attended.\textsuperscript{141}

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 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.\textsuperscript{142}

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.\textsuperscript{143} 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.” (\textit{The Independent}, 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

\textsuperscript{141} 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.\textsuperscript{142} All numbers given in this paragraph are from Bernanke and Mishkin (1992, p. 191).\textsuperscript{143} Friedman (1987g, p. 365). See also Nelson (2018a, Chapter 8).
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 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 1991.144 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.”145

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) to 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 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. 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. The gradual approach had been manifested 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 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”

146 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/MDISCR1.txt.
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. In May 1992,
Friedman wrote that the United States had experienced “three years of stagnation” in the period
since mid-1989.148 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. 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
arguments with the 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 the time that it showed up in the unconditional behavior of velocity, the
Federal Reserve noticed this shift years ahead of Friedman, who was focusing on velocity.149

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149 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,
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 acknowledgement 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.”150 Consistent with this conclusion, Laurent (2000) and Whitesell (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 (for example, Wall Street Journal, February 28, 1992). M2 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.

150 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 weak, it was signaling not only the weakness in M2 demand, but also the weakness in the economy.” (David Lindsey, interview, May 2, 2013.)
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. 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. 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.

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—pursued by Greenspan since 1987 and reaffirmed by the Chairman in July 1992—of shifting monetary policy settings toward consistent with price stability. 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.” 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.

151 See the discussion in Chapter 6 in Book 1.
152 See, for example, Feinman and Porter (1992) and Duca (1995).
153 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.
155 MacNeil/Lehrer News Hour, PBS, March 6, 1991, p. 7 of transcript
156 MacNeil/Lehrer News Hour, PBS, March 12, 1992, p. 10 of transcript.
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. 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. And the recession itself had been magnified by a major nonmonetary factor, in the form of the oil price rise that accompanied 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 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. But the 1990 oil shock then sent four-quarter CPI inflation to 6.3 percent for 1990:Q4, 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

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158 For example, MacNeil/Lehrer News Hour, PBS, March 6, 1991, p. 7 of transcript, and MacNeil/Lehrer News Hour, PBS, February 21, 1992, p. 5 of transcript.
159 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.
two.” 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.)

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 were about to become impossible for him to miss.

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.)

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161 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.
162 See the next chapter.
Friedman, of whom Stein was a longtime family friend, saw the film.\textsuperscript{163} 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: \textquotedblleft 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.\textquotedblright\textsuperscript{164}

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 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 \textit{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: \textquotedblleft It is very hard to be optimistic about the growth prospects for the 1990s, because the 1990s are

\textsuperscript{163} 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.)

\textsuperscript{164} Friedman (1989c, p. 13).
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.165 With 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.”166 Friedman expected Bush would indeed refuse to agree to 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

165 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 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 indicating a failure of President Reagan to achieve his goals. Part of the difference in this 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).
166 Friedman in Friedman and Tobin (1990, p. 76).
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.167 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 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.168

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167 Wall Street Week, Maryland Public Television, February 21, 1992, p. 6 of transcript; Frisko, 1992, p. 68; Forbes, August 17, 1992, p. 44.
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.”¹⁶⁹ 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 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.¹⁷⁰ 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

¹⁷⁰ Friedman and Friedman (1998, p. 392); see also Forbes, August 17, 1992, p. 44
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.\(^{171}\)

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.\(^{172}\) 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 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 (\textit{New York Times}, February 2, 1992; \textit{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).\(^{173}\) Having indicated his support for the war,

\(^{171}\) See the previous chapter.
\(^{173}\) 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 previous chapters of
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 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.174 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

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174 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).
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).175 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.176 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).177 During the campaign proper, in a television appearance in October 1992, Friedman voiced opposition to the Clinton campaign’s job-creation plan.178 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 indeed defeated Bush in the election the following November.

175 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.)
177 The letter was reported in Wall Street Journal, March 31, 1992. See also Rasche (1993c, p. 2) for a discussion of the letter.
Bibliography

Note: Items appear in this bibliography only if they were not cited in Nelson (2018a, 2018b). See the reference list of Nelson (2018a, 2018b) for bibliographical details on the items cited in the present chapter that are not listed below.

I. Newspaper articles, periodical articles, and electronic media items cited


Milton Friedman, “Prohibition and Drugs,” *Newsweek*, May 1, 1972, page 104.


Milton Friedman appearance on MacNeil/Lehrer News Hour, PBS, April 17, 1987; PBS transcript.


Milton Friedman appearance on *MacNeil/Lehrer News Hour*, PBS, August 27, 1990; PBS transcript.


Milton Friedman appearance on *MacNeil/Lehrer News Hour*, PBS, March 6, 1991; PBS transcript.


Milton Friedman appearance on *MacNeil/Lehrer News Hour*, PBS, February 21, 1992; PBS transcript.


*MacNeil/Lehrer News Hour*, PBS, October 21, 1992; PBS transcript.


Lars Christensen, “*Money Mischief*—A Major Inspiration for ‘Monetary Thinkers,’” *The Market Monetarist* blog entry, July 1, 2013.


**II. References**


