
Edward Nelson¹
December 15, 2019

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

CONTENTS

Chapter 2: Monetary Policy and Public Policy Debates, 1973 to 1974
   a. Indexation
   b. Watergate
    a. Albert Rees
    b. Arthur Schlesinger

   a. The First Oil Shock
   b. Into the Floating Rate Era
    a. George Shultz
    b. William Nordhaus

Chapter 4: Debates on Monetary Policy and Macroeconomic Stabilization, 1975 to 1976
I. Events and Activities in Debates on Monetary Policy and Macroeconomic Stabilization, 1975–1976
II. Issues Related to Debates on Monetary Policy and Macroeconomic Stabilization, 1975–1976
   a. The Beginning of Monetary Targeting
III. Personalities Related to Debates on Monetary Targeting and Stabilization Policy, 1975–1976
    a. Arthur Okun
    b. Ronald Reagan
Chapter 6: Debates on Monetary Policy and Macroeconomic Stabilization, 1977 to 1978

I. Events and Activities Related to Developments in Monetary Control and Stabilization Policy, 1977–1978

II. Issues Related to Monetary Control and Stabilization Policy, 1977–1978
   a. Crowding Out and Ricardian Equivalence
   b. The Second Monetary Explosion

III. Personalities in Debates on Monetary Control and Stabilization Policy, 1977–1978
    a. G. William Miller
    b. Franco Modigliani
Conventions used in this book

The chapters in this book (those that cover blocks of years, i.e., Chapters 2–12) are divided into sections titled “Events and Activities,” “Issues,” and “Personalities” (with the latter two sections in turn broken into subsections). The “Events and Activities” section covers some of Friedman’s main engagements in economic debate over the years considered in the chapter; this section, however, omits those topics subsequently covered in the “Issues” and “Personalities” sections. The “Issues” section covers major policy or research issues in which Friedman was involved during the years in question. The “Personalities” section is of the same format as the “Issues” section, except that it is more closely focused on an individual with whom Friedman interacted (or to whom Friedman reacted) in the years covered in the chapter. In each case, no attempt is made to provide a complete picture of the work of the individual considered in the “Personalities” section. The aim of the discussion is, instead, to bring out the activities and work of Friedman that reflected his overlap of interests with the individual in question.

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

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

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

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

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

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

¹ An exception is made in the small number of cases, which include Friedman (1973c), in which there was no credited interviewer and in which Friedman was listed in the publication as the article’s author, even though the article was published in question-and-answer format.
I. EVENTS AND ACTIVITIES RELATED TO MONETARY POLICY AND PUBLIC POLICY DEBATES, 1973–1974


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

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

As it happened, just as the special issue saw print, one of Friedman’s sparring partners in the symposium—Don Patinkin—was on hand to attend Friedman’s workshop. Patinkin served at the University of Chicago during the Fall Quarter of 1972, as Ford Foundation Visiting Research Professor of Economics (Patinkin, 1973, p. 787). A graduate student who was also visiting the University of Chicago at this time, John Rutledge, himself attended the workshop and was struck by the stellar quality of Friedman and the other participants. “What I remember is that fall, him having just had his sixtieth birthday [on July 31]. And I think that he had heart surgery just after

---

1 Email: Edward.Nelson@frb.gov. The author is grateful to the interview subjects and George Tavlas for their generosity in providing useful information, as well as for comments from participants in a seminar at the University of California, Berkeley, at which some of the material in this chapter was presented. See the Introduction in Nelson (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.
that; not while I was there, but after I had gone. [In the Fall semester] it was that incredible group of people. This was the money and banking workshop that met Tuesday afternoons. And I was, of course, a kid. (*laughter*) And in that room, Friedman was at the head of the table. In terms of just some of the guys in the room, [there was] Don Patinkin, Robert Gordon, Stanley Fischer, Robert Barro…” Others Rutledge remembered among the attendees included Fischer Black, Don McCloskey (later Deirdre McCloskey), and Jeremy Siegel. “And there were several more, but it was all around one table… So it was an extraordinary roomful of people.”

One of the papers presented at the Fall 1972 workshop sessions was by two economists from the business school, John Gould and Charles Nelson (the latter a fixture at the workshop during the first half of the 1970s). The bottom line of this paper—titled “The Stochastic Structure of the Velocity of Money”—was that the time-series behavior of the log of U.S. M2 velocity resembled a random walk. The Gould-Nelson paper had been drafted during the summer of 1972 and sent to Friedman at that time. From Vermont, Friedman had composed a highly skeptical response in the form of a letter to Gould, which his secretary, Gloria Valentine, typed up in Chicago on August 7.2

It was against the background of this correspondence that, prior to the workshop presentation of the paper, Gould spotted Friedman and Fischer in a campus cafeteria. “It was during a summer period that some of that correspondence had gone on… Now it was the Fall quarter, and the university was kind of gearing up; Friedman was back and Fischer was around, and I happened to be having lunch… I happened to look over and see Friedman and Fischer having lunch together at another table. So, when we were finished with lunch, I went over to say “Hello, welcome back,” and everything else, and we actually fell into a discussion about the paper—about the velocity paper, and the possibility that velocity was a random walk.” When Fischer suggested that velocity could not be a random walk, Gould was surprised to find Friedman defend the other side of the argument—that velocity might be a random walk. “And so Friedman immediately turns on Fischer and says, ‘Oh no, there are quite a number of reasons it could be a random walk,’ and he proceeded to give the argument on the other side. And that was Friedman—the quintessence of Friedman: It was economics he was interested in, and he could go either side of the argument, and really get some insight out of each.” (John P. Gould, interview, March 20, 2015.)

---

2 A copy of the letter was kindly supplied to the author by John P. Gould and Charles Nelson.
Friedman’s thinking on velocity during 1972 was shaped by his finding with Schwartz—reported by them in that year’s NBER Annual Report—that twentieth-century movements in U.S. M2 velocity often mirrored those of broad-money velocity in the United Kingdom.\(^3\) Friedman and Schwartz took the similarity across the two economies of annual fluctuations in velocity as implying that the factors driving velocity in each country also moved together. They were therefore fortified in their view that velocity’s behavior was traceable to economic variables. Friedman articulated this interpretation of U.S. velocity behavior in his August 1972 correspondence with Gould, in which Friedman mainly accepted the premise—common at the time—that a finding that a macroeconomic series was a random walk made the series economically uninterpretable. However, even this letter allowed for the possibility that random-walk behavior of the logarithm of velocity could be consistent with it being an economically-meaningful series, provided that the economic drivers of velocity included random walks. This possibility was also likely what Friedman emphasized when he acknowledged the economic plausibility of velocity being a random walk in his Fall 1972 discussion with Gould and Fischer.

In retrospect, this discussion also points up the fact—noted by Stock (1988) in reference to the case of the consumption function—that Friedman grasped the essential concept of cointegration well before that concept was formalized by Engle and Granger (1987). In particular, Friedman realized that it was possible for the logarithm of M2 velocity to be a random walk, yet for the money demand relationship, expressed in levels form, to have long-run stability; that is, for the nonstationarity (stochastic trends) to cancel in the overall relationship linking log money (log M2), log prices (log \(P\)), log real income (log \(y\)) and other variables. More specifically, under the setting of a unitary income elasticity of real money demand—a parameterization that Friedman found empirically and analytically attractive by the early 1970s—the logs of real money and log real income were part of a cointegrating vector with coefficient 1.0 on log real income, and log velocity, log \(P +\) log \(y -\) log M2, could depend on an opportunity-cost variable (usually an interest rate) that was a random walk.\(^4\)

This possibility was also allowed for in the published version of Gould and Nelson (1974), which discussed how a random-walk process for the log of M2 velocity could be consistent with the existence of the monetary relations that Friedman and Schwartz espoused, while also considering

---

\(^3\) See Friedman and Schwartz (1972).

\(^4\) McCallum’s (1993) defense of the quantity theory of money considered another possibility—that financial innovation produces a difference-stationary money demand shock. On this interpretation, the absence of a cointegrating levels relationship between money, income, and opportunity-cost variables like interest rates is consistent with the quantity theory of money, which McCallum suggested should be thought of as primarily offering propositions concerning relationships between growth rates series.
situations in which a random-walk finding for velocity could be inconsistent with those relationships. Likewise, Anna Schwartz’s (1975, p. 154) consideration of Gould and Nelson’s (1974) findings highlighted an interpretation of Gould and Nelson’s results in which nonstationary velocity behavior occurred in the context of a stable money demand relationship (that is, a constant-parameter long-run money demand relationship with a stationary error term), so long as “the determinants of velocity… that we have isolated are also random walks.”

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

That said, in their work on money demand in the early 1970s Friedman and Schwartz still had the level of market interest rates appearing directly in the estimated function, rather than as a spread over the own rate (Schwartz, 1975, p. 147). Thus, though Friedman’s contention that M2 velocity was stationary was one he held both in the early 1970s and in Monetary Trends, his initial rejection of the alternative hypothesis of a random-walk velocity at least partly reflected the fact that—like many macroeconomists before the appearance of the Nelson and Plosser

---

6 Both in Schwartz (1975) and Friedman and Schwartz (1982), Friedman and Schwartz’s money demand function had nominal income growth as another driver of money demand. Provided that nominal income growth was mean-reverting, this term too could not be a source of nonstationarity in velocity. In particular, in the sample period covered by Gould and Nelson (1974), nominal income growth repeatedly returned to its mean and recorded a low rate in the final year of their sample, 1970. And, although nominal income growth was higher on average in the 1970s than in previous decades, it was only in the later years of the decade that it was high throughout; in contrast, the initial rise in nominal income growth in 1971–1973 was largely wound back in 1974–1975.
(1982) study—Friedman found it difficult to accept that short-term market interest rates were a random walk, at least when rates were measured at monthly, quarterly, or lower frequencies.

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

This retrospective analysis of the postwar period was therefore one that judged modern conditions as featuring covariance-stationary M2 velocity behavior. Ironically, however, having just arrived at this judgment, Friedman came to believe that a permanent shock to the velocity of U.S. M2 was in progress during 1973, one not unlike that seen at the start of the Korean War.

As will be seen below, against the background of surging inflation during 1973 and 1974, Friedman reaffirmed that aggregate demand behavior be put at the forefront of the analysis of inflation. But, for much of 1973, he was more well-disposed than usual to viewing the strength of aggregate demand, both nominal and real, as a reflection of forces other than those represented by monetary growth. For example, he initially viewed the increase in U.S. aggregate nominal spending in the first quarter of 1973 as substantially beyond what would be expected by monetary growth. In particular, Friedman was impressed by what seemed to be a sharp increase

---

7 Friedman (1973a, p. 7). In keeping with his practice since the late 1960s, Friedman’s 1973 analyses excluded large negotiable certificates of deposit from the definition of M2 (see, for example, Friedman and Schwartz, 1973, p. 50). The Federal Reserve Board also followed this practice, both for the M2 series it had published since 1971 and for its revised M2 definition in 1980. See Nelson (2018b, Chapter 14) for a detailed discussion.

8 Friedman (1973a, pp. 8–9).

9 Friedman’s assessment in 1973 of a stationary M2 velocity was related to Friedman and Schwartz’s revised analysis earlier in the decade of historical velocity data. As discussed in Nelson (2018b, Chapter 13), this assessment had the effect of leading them to regard the real income elasticity of U.S. M2 demand as approximately unity. They judged that their earlier estimates of the income elasticity had been biased upward by inclusion in their sample of the period in the late nineteenth century of rapid growth in U.S. commercial banking. Thus, while Gould and Nelson (1974) had been taking Friedman and Schwartz (1963a) as contending that M2 velocity since 1869 was trend-stationary, by the time the Gould-Nelson paper appeared Friedman and Schwartz had actually shifted to the position that velocity since 1903 was covariance stationary (and trend-stationary before 1903).

in M2 velocity and was prepared to ascribe some of the boom in aggregate demand in process
that year to an autonomous increase in velocity—in which households’ demand for money
shifted in response to a fear of shortages, as they had in 1950 (Instructional Dynamics
Economics Cassette Tape 119, April 25, 1973; see also Tape 120, May 11, 1973, and Tape 124,
July 4, 1973).\footnote{11}

But as more data accrued, it would turn out that such an appeal to factors driving velocity was
not necessary for the analysis of aggregate demand in 1973; indeed, by mid-1974, Friedman was
able to observe that the relationship between money (in particular, M2) per unit of output and
prices (another way of looking at the money/nominal income link) had been closer for the period
since 1962 than the average historical relationship between the two series (Newsweek, June 24,
1974).\footnote{12} The result that the relationship between money and the economy in 1973 was closer
than was thought at the time had a parallel with 1971–1972. In that earlier period, it was
nonmonetarists who had rashly declared money/income relationships to have shifted (see, for
example, Mitchell, 1972, as well as the discussion in Nelson, 2018b, Chapter 15). In 1973,
Friedman himself had been trying to explain what proved to be, at best, an ephemeral
discrepancy between the courses of money and nominal income.

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

\footnote{11 See also Nelson (2018a, Chapter 4).
12 David Meiselman published a similar chart to Friedman’s (covering 1960 to 1974) in the Wall Street Journal of
September 13, 1974—using the chart to support the position that 1973’s inflation predominantly reflected monetary
excess “rather than from declines in output stemming from such oft-cited events as the disappearance of anchovies
off the coast of Peru or the operations of the OPEC [Organization of Petroleum Exporting Countries] cartel.” (On
such nonmonetary factors and their use in explaining inflation, see the discussion below and in the next chapter.)}
through the end of 1972 generated “smoother short-period behavior” of velocity, while leaving
trend behavior unchanged.¹³

The Friedman and Friedman-Schwartz presentations of the leading-velocity concept crystalized
their earlier work on lags and included Friedman’s recent refinement of focusing on comparisons
of growth rates of money and nominal income when ascertaining lags.¹⁴ John Rutledge, who
gave a few presentations at the money workshop, recalled of his first presentation in the Fall
1972 semester, “after the workshop, everyone left and he [Friedman] stayed there with me. And
he was talking about the lags in monetary policy. And he had two rolls of tracing paper.” On
one roll, M2 growth was plotted; the second roll showed nominal GNP growth. “And we went
over to the window. We held up the tracing paper against the glass, so it was see-through with
the sunshine coming through it. And we moved the first roll of tracing paper left and right until
it looked like it was sitting right on top of the other. The M line was sitting right on top of the
GNP line—which was a way of measuring the average lag of monetary policy. And then he
said: ‘You know, this is really a pretty good way to do it, because you and I can see things that
sometimes regression programs miss.’” (John Rutledge, interview, November 14, 2014.)

Calculating leading velocity, instead of actual velocity is a way of transferring such matching-up
of money and income variations into velocity space. Specifically, the use of leading velocity has
the effect of moderating or even removing the spike (up or down) in velocity that is observed
when a money movement occurs that only later shows up in nominal spending.¹⁵ Indeed, for
1973—after account is also taken of post-1973 revisions and redefinitions of money and
income—most of the rise in velocity is absent from the leading-velocity series. With modern
data on nominal GDP and the modern M2 definition, the rise in velocity as ordinarily measured
is 3.9% in the four quarters to 1973, but the rise in leading M2 velocity for the same period is
less than 0.1%, with the four-quarter rise in nominal GDP of 11.1 percent to 1973:Q4 closely
matching the growth rate of M2 to 1973:Q2 of 11.1 percent.

¹³ See Friedman and Schwartz (1973, pp. 51–52).
¹⁴ For discussion of this refinement, see Nelson (2018b, Chapter 15, Section I).
¹⁵ See Nelson (2018a, Chapter 10) for more discussion of this concept, to which Friedman had alluded in 1970 (see
Nelson, 2018a, Chapter 6) and which had also been invoked in Wicksell (1935, p. 142). The concept was later used
in Federal Reserve Board staff research (Axilrod, 1983, pp. 4–6; Porter and Small, 1989, pp. 244–245) as well as in
Mankiw and Summers (1986, p. 420). The fact that Friedman first plotted a leading-velocity series in an outlet other
than Newsweek or a research journal may have slowed down recognition of his use of the concept. For, in a Federal
Reserve Board staff memo titled “Friedman’s Redefinition of Velocity,” from James Pierce to David Lindsey dated
June 19, 1975, Pierce recalled learning of Friedman’s use of the concept in conversation with Friedman “at the
Academic Consultants Meeting six months ago” (the meeting of December 12, 1974). (Federal Reserve Board
records.)
Over a longer period, the leading-velocity/current-velocity distinction diminishes in importance. Consequently, reliance on conventionally-measured velocity is sufficient to bring out the extent to which nominal income and money moved in line with one another over the period covered in this and the next few chapters. U.S. nominal GDP in the fourth quarter of 1976 was over 45 percent higher than its value in the fourth quarter of 1972; in contrast, in 1976:Q4 the ratio of nominal GDP to M2—that is, the standard velocity calculation—was only about 1.6 percent higher than its value in 1972:Q4.

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

Illness and recovery

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

In recording his biweekly economic commentary on December 15, 1972, Paul Samuelson observed: “The first order of business today is to wish good luck to my colleague, Professor Milton Friedman of the University of Chicago. According to what I read yesterday in the Washington Post and the New York Times and also this morning in the Boston Globe, Professor Friedman just today is to undergo open-heart surgery at the Mayo Clinic. It’s rare that an economist has in his own lifetime a significant influence on the history of ideas and on the passing scene of policy. But let me say it, since I have not always been in perfect agreement

---

16 See Laurent (1993, p. 11). The basis for money being expressed in real terms in the index lay in the fact that the leading indicators were primarily designed to project real economic activity, rather than nominal spending.
with Professor Friedman—that Milton Friedman has had this unusual influence. Not only has
been an adviser of presidents and to the advisers of presidents, but [also] he has influenced two
generations of students.” Samuelson added, in remarks that underlined the gravity of the
procedure facing Friedman: “I’ve been seeing quite a number of economists in the last two days,
and it’s been amazing what a groundswell of appreciation has been expressed in these last
hours.” (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 117,
December 15, 1972).17

As already indicated, Friedman’s operation was on December 15, 1972. In 1983, Friedman
would be quoted recalling: “They did a bypass operation, as a preventive measure, which has
been just incredibly successful. I have never had a problem since. Everything has been fine.”18
The initial aftermath of the surgery gave little indication that this outcome was likely. Marc
Nerlove recalled that “he later described the recovery from that to me, in some detail. It was
very hard to recover, but he obviously did—he lived many years longer without problems. But
[before that] he’d never been ill in his life.” (Marc Nerlove, interview, September 26, 2013.)
Anna Schwartz, in conversation with the author 35 years after Friedman’s surgery, noted that
Friedman’s condition in the initial weeks after the operation was so frail that those around him
had doubts about a full recovery. “Who would have thought, at that time, that Friedman would
live on to ninety-four?”19 Similarly, Rose Friedman’s correspondence with Arthur Burns in the
days following his operation expressed discouragement and distress at her husband’s weak
state.20

This initially precarious recovery during December 1972 was followed by a more robust
condition. Friedman was discharged from St. Mary’s Hospital on the morning of December 26
Friedman spent January 1973 convalescing in Palm Springs, California.21 The surgery had a
lasting effect on Friedman’s intellectual activity, by giving him a permanent interest—albeit as a
hobby rather than as a research topic—in the state of knowledge regarding treatment of heart
conditions (Newsweek, January 11, 1982). During 1973 he also had a surge of renewed interest
in shaking up the medical profession, devoting much of his first post-surgery cassette
commentary (taped from Palm Springs) to a restatement of the case against barriers to entry to

17 In addition to the newspapers Samuelson cited, Friedman’s operation received widespread coverage in other city
papers; among these, the Chicago Daily News made it a front-page item (December 13, 1972).
19 Schwartz’s remarks were made on May 27, 2008 (see Nelson, 2009a, p. 489).
20 December 1972 correspondence in the Arthur Burns papers, Ford Presidential Library.
21 See, for example, Friedman and Friedman (1998, p. 389).
medical practice and recalling *Income from Independent Professional Practice* in the process (Instructional Dynamics Economics Cassette No. 113, January 17, 1973).  

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

Due to his absence from the city of Chicago, Friedman would miss an awards ceremony at the Chicago Press Club on January 20, at which he would be named Chicagoan of the Year (*Chicago Tribune*, January 15, 1973). About a month later, however, the *Chicago Tribune* reported (February 14, 1973) that Friedman would be returning to Chicago that week. The Friedmans flew from Palm Springs to Chicago on Saturday, February 17. Consequently, by the end of February 1973, Friedman was back in the city of Chicago and again keeping close tabs on economic developments. Benjamin Eden, a University of Chicago graduate student over this

---

22 That is, Friedman and Kuznets (1945). Friedman also gave an interview on the subject in *Medical Economics* (April 16, 1973). A *Newsweek* column on drug regulation (January 8, 1973) was also on medical drug regulation, although it was probably written before Friedman’s surgery. His first column written after the surgery was likely that in *Newsweek*, January 29, 1973. Friedman also recorded two more cassette commentaries from Palm Springs: those of January 31 (Tape 114) and February 14 (Tape 115).

23 See Friedman and Friedman (1998, p. 422) as well as Chapter 5 below.


25 See Friedman (1973b).

26 In Friedman (1976b, p. iv).

27 See Friedman and Friedman (1998, p. 422). This return date was planned well in advance, being mentioned in Friedman’s letter to Arthur Burns (January 29, 1973). (Burns papers, Ford Presidential Library.)
period, recalled: “At the time, everybody was worried, but then he came back.” (Benjamin Eden, interview, March 14, 2014.)

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

Inflation erupts in the United States

At the end of 1972, in a report on the U.S. economic outlook, the Nixon Administration’s Council of Economic Advisers stated that “the prospects are good for another year combining rapid expansion and a reduced rate of inflation” (quoted in Chicago Daily News, December 27, 1972). In a similar vein, in an article published in March 1973, the Deputy Secretary of the Treasury, William E. Simon, could still claim success and express optimism concerning the administration’s previously-expressed objectives: “Over the past four years... we have succeeded in cutting inflation almost in half—from about 6 percent to almost 3 percent. We now have the lowest rate of inflation and the highest rate of real growth of any developed nation in the world... Our goals for 1973 are to continue to move toward full employment and to cut the rate of inflation to 2½ percent by the end of the year. These are ambitious goals—but attainable.”28 (Bankers Monthly, March 15, 1973.)

But by the time Simon’s article was published, major blemishes on the picture had materialized, and a Wall Street Journal front-page article observed that in a matter of weeks the U.S. economy had transformed from “a glistening Cinderella’s coach” to a “pumpkin” (Wall Street Journal, February 27, 1973). By then, the surge of the Consumer Price Index (CPI) in January 1973, shown in Figure 1, had been reported. The United States had entered what Modigliani (1978, p. 195) labeled “the three very disturbed years” following 1972.

Even by the time of his forced absence, Friedman was convinced that the U.S. economic developments demanded a course correction on the part of policymakers. In his interview before his operation (National Journal, January 13, 1973) he stated: “There is a difference in what is right when you’re in an expansion, and you have unused resources to employ, and what’s right otherwise.” He expressed much the same sentiment in a letter at the end of 1972—dated December 27, 1972, but written prior to his operation—to President Richard Nixon, with Federal Reserve Chairman Arthur Burns copied into the correspondence. What Friedman failed to

28 Quotation from page 16 of the article.
appreciate, however, was that economic overheating had already set in and that this excess demand was poised to spill over to inflation, even in the presence of mandatory price controls.

The end of the period of apparently benign behavior coincided closely with the end of calendar-year 1972. Friedman realized that the tranquility of 1972 was precarious. That had been a message of his December letter to Nixon. His letter had stressed the need for a prompt tightening in the settings of aggregate demand policy. Friedman affirmed this posture in January 1973, when he observed: “The Fed must be just about as distressed as I am over this explosion in money.” (Instructional Dynamics Economics Cassette Tape 113, January 17, 1973.)

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

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

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

---

29 A copy of the letter was also forwarded to Arthur Burns, and that copy is held in the Burns papers at the Ford Presidential Library. Notwithstanding the December 27 date on the letter, the accompanying cover letter from Rose Friedman to Burns confirmed that, as already noted, the letter was drafted prior to Friedman’s operation and that Friedman was not well enough to work further on the letter during the two weeks following his operation.

30 See also Friedman (1973a, p. 5).
When pressing in late 1972 and very early 1973 for a tightening of policy settings, Friedman premised his case on an acceptance that the authorities were satisfied to get 2–3 percent inflation and on the belief (in retrospect, very likely incorrect) that output remained slightly below potential. Thus, he saw the requisite tightening as one that lowered nominal income growth to 5 to 7 percent nominal income growth from mid-1973 onward. From this starting point, Friedman was caught off-guard by the sharp upturn in inflation and nominal income growth that quickly emerged at the start of 1973: as earlier noted, nominal income growth by the end of 1973 was well above 10 percent, and inflation itself was approaching double digits. A host of other economic indicators would also deteriorate in the course of that year, as discussed later.

Wage and price controls: intensification and strains

As recounted in detail in Chapter 15 of the previous volume, Friedman’s negative posture toward wage and price controls was shared by the Nixon Administration in its early years. But this attitude encountered considerable opposition in public discourse and was eventually eschewed by President Nixon himself. Nixon’s embrace of controls in August 1971 received wide support;
Friedman, who would acknowledge that Nixon’s move was backed by “a widespread national consensus,” was criticized for his continuing opposition to controls and to nonmonetary approaches to the analysis of inflation.\textsuperscript{33} For example, a column in the \textit{Chicago Daily News} at the end of August 1971 contended that Nixon’s switch occurred “with the nation on the verge of economic calamity,” and looked askance at Friedman’s reaction: “Undaunted and unashamed, Friedman now tells us that Mr. Nixon’s wage-price-rent freeze is a ‘lamentable mistake’ that ‘will end… in utter failure and the emergence into the open of the suppressed inflation.’” \textit{(Chicago Daily News, August 31, 1971.)} And in November 1972, as President Nixon approached reelection fifteen months into his controls program, economics columnist Hobart Rowen noted Friedman’s opposition and concluded: “So, controls have been tried—and they have worked…” \textit{(Boston Globe, November 5, 1972.)}

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

On January 11, 1973, President Nixon announced Phase III of his wage/price controls. This new setup was widely seen as amounting a major relaxation of the controls and as setting the stage for a transition to a largely voluntary incomes policy.\textsuperscript{34} Friedman himself took Nixon as having largely abandoned controls by taking this step and praised the president accordingly \textit{(Newsweek, January 29, 1973).} However, the fact that the CPI then registered such a large rise in January produced a backlash against Nixon’s relaxation of controls—prompting William Simon to acknowledge, in the aforementioned March 1973 article, that “Phase III has not been well received.”

When the analysis in Friedman’s January 29 \textit{Newsweek} column is viewed in the light of the January CPI data an subsequent data releases, it becomes evident that he had been underestimating the degree of excess spending—and corresponding inflationary momentum—already prevailing in the U.S. economy by the end of 1972. The column was, however, accurate in forcefully arguing that the basic course of inflation during 1973 would reflect the behavior of “total demand,” and not what was done by the U.S. authorities in the area of wage and price controls. In particular, Friedman stressed that those controls would not be able to contain

\textsuperscript{33} The Friedman quotation is from \textit{Fortune} (July 1974) (reprinted in Friedman, 1975a, p. 152).
\textsuperscript{34} The Associated Press report on Nixon’s announcement of Phase III said that it “scrapped most firm wage and price controls” \textit{(Arizona Republic, January 12, 1973).}
inflationary pressure created by great excessive demand. In the same vein, he would see much of
the inflation recorded during the Phase III period as reflecting the unveiling of price increases,
themselves largely driven by demand pressure, that were suppressed during the earlier, more
restrictive phases of price control. To him, the surge in U.S. inflation during 1973 confirmed
that controls had done nothing to remove the forces producing inflationary pressure. If anything,
he said, they had increased the scale of these forces, by encouraging the authorities to carry out
expansionary policies.

The magnitude of the inflation that appeared from 1973 onward would mean that the traditional
demarcation between wartime and peacetime experiences in the United States would need to be
reassessed. In 1970, Friedman had described the price rise during the late 1960s as the most
severe of “any peacetime period in American history,” while acknowledging the difficulty in
classifying it as a “peacetime” inflation in light of the ongoing U.S. involvement in the Vietnam
this classification issue moot, for the U.S. inflation rates recorded in 1973 and 1974 far exceeded
those seen in the late 1960s. And the 1973–1974 price rises were assuredly “peacetime”
inflation, occurring very largely after the U.S. left active combat in Vietnam in January 1973—
and certainly well after U.S. combat had peaked.35

In March 1973, Friedman engaged in his first overseas travel since his operation. In a lecture
given at the National Bank of Yugoslavia on March 20, Friedman harked back to the first famous
failure of price controls under Diocletian and drew parallels with the United States’ “attempts to
stop inflation by Phases I, II, or III.”36 Friedman realized, however, that his view that the controls
had only suppressed inflation and not removed them was not the popular perception. On this
occasion, he hedged his bets about whether U.S. policymakers yet grasped that controls were
only cosmetic: he noted that officials could embrace controls either to give the appearance of
fighting inflation or because they were “[b]elievers in ‘cost-push.’”37 In retrospect, as indicated
later in this chapter, it was the second motivation that remained the principal impetus for the use
But, irrespective of the motivation for controls, it was clear to Friedman, as he noted in April,

35 True, the early-1973 upsurge partly reflected unveiling of inflation that had been suppressed in the stringent
the United States being on a war footing. Defense spending peaked as a share of U.S. output in real terms in fiscal
year 1968 (Council of Economic Advisers, 2018, Table B–18, p. 553). This timing contrasts with the much later
36 Friedman (1973d, p. 6).
37 Friedman (1973d, pp. 5–6; quotation from p. 5).
that the advent of controls had encouraged “the miseducation of the public” about how to deal with inflation (Instructional Dynamics Economics Cassette Tape 119, April 25, 1973).

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

The June 1973 revision of the controls was variously dubbed “Phase IV” (for example, *Yorkshire Post*, June 15, 1973) and “Phase 3½” (for example, *Daily News* (New York), June 15, 1973). Friedman, in keeping with the administration’s own numbering system, reserved the “Phase IV” terminology for what would follow the new freeze (Instructional Dynamics Economics Cassette Tape 123, June 23, 1973). 38 He characterized the freeze as motivated by cost-push ideas about inflation (Instructional Dynamics Economics Cassette Tape 125, July 18, 1973). “It’s treating the façade of inflation rather [than] getting to the guts of it,” Friedman remarked when some details regarding the next, post-freeze, controls phase were announced (*Newsday*, July 19, 1973).

**Challenging cost-push views in 1973–1974**

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

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

In 1983, Friedman reflected: “It became increasingly clear that excessive money growth was a major culprit as inflation accelerated in the 1970s.” 39 However, the transition during which the acceptance that inflation was a monetary phenomenon became truly widespread among economists lasted over much of the 1970s, being very incomplete by 1973. And the acceptance

---

38 He also tentatively used the “Phase 3½” label for the new freeze: see his testimony of June 21, 1973, in Joint Economic Committee (1973, p. 132).
39 Friedman (1983a, p. 6).
among those in policy circles of this interpretation—reversing their renewed enthusiasm for cost-push views at the start of the decade—was, if anything, even slower, stretching into 1978 and 1979.

Friedman’s March 1973 lecture, while reaffirming his own stand that “what is called ‘cost-push’ is a symptom and not a cause of inflation,” acknowledged that this perspective had not prevailed; instead, there remained “wide disagreement about both the causes of inflation and the remedies for inflation.” Furthermore, within months of his lecture, the tightening of price controls under Nixon, alongside Arthur Burns’ continued support for incomes-policy approaches, pointed up the fact that the cost-push perspective still prevailed in U.S. official circles.

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

For Friedman, the ground had already been laid for a general price rise was provided by earlier years’ monetary policy. That being the case, the increase in food price rises simply became “the
form which a [general] price increase has taken which might have taken other forms.” (Instructional Dynamics Economics Cassette Tape 130, September 26, 1973.) As for the dollar’s devaluation, that, Friedman said, “must be ruled out entirely” as a source of inflation. With respect to viewing inflation in terms of nominal wage growth in excess of productivity growth, Friedman observed: “That’s arithmetically correct. Economically, it’s nonsense.” (Instructional Dynamics Economics Cassette Tape 127, August 15, 1973.) And, as discussed in Section III of the next chapter, quite aside from the analytical validity of tracing the behavior of inflation to variations in unit labor cost, Friedman was skeptical of the empirical reliability of the link between these two series.

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

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

---

44 Likewise, later, in Instructional Dynamics Economics Cassette Tape 168 (June 1975, Part 1), Friedman criticized the “basic idea that everybody has that costs determine prices.” See also Friedman and Friedman (1980, p. 262).

45 One interpretation of Friedman’s remark is that the causality he was talking about pertained to the contemporaneous causal ordering of variables, a la Sims (1980), and that he had in mind the notion that, in any given period, goods prices might be predetermined in relation to nominal wages. This interpretation of Friedman’s remark can be ruled out by the fact that the opposite position—i.e., that wages in period t are predetermined while prices can react to period-t shocks—is an important part of his aggregate supply theory (see Friedman, 1968a, pp. 9–10, and the analyses in Nelson, 2018a, Chapter 7; 2019a).

46 That the consumer-sovereignty view of relative price determination underlay Friedman’s characterization of the cost/price nexus is underscored by an observation on one of the cassettes already cited (Instructional Dynamics Economics Cassette Tape 125, July 18, 1973). He took the message of economic theory on this matter as that “things that cost absolutely nothing may still have very high prices, and that no matter how much you spend on
produced a relationship between real costs of producing each good and that good’s price. With regard to nominal costs, they, being nominal series, eventually had to stay in step with price-level trends—with those trends, in turn, flowing ultimately from monetary policy actions.

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

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

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

producing something that has no use or no value to anyone, its price will be zero.” (The reference to items that cost nothing to produce but command high prices may be primarily to fiat money. Friedman may also have had in mind aspects of human capital with which people are born, but which can generate high-price services. In Friedman, Porter, Gruen, and Stammer, 1981, p. 33, for example, he mentioned Frank Sinatra’s voice as an example of this phenomenon.)

47 This also reflects Friedman’s critique of older economic theories, prevalent especially before the advent of marginal-utility analysis, to the effect that products or inputs had an inherent value to them aside from that assigned by consumer preferences. For example, Friedman (1955, p. 902) characterized the development of marginal-utility analysis as allowing “demand to be assigned its proper role and the shackles of the cost of production or, even worse, labor theory of value to be overthrown.”
Accordingly, Friedman noted during 1973 that the “incredibly rapid rise in food prices has, I believe, meant a slower rise in other prices than would have occurred.” (Instructional Dynamics Economics Cassette Tape 130, September 26, 1973).48

The primary concession Friedman made was that over very brief periods—say, for three to four months—variations in the individual components of the price index do drive inflation, because a price increase concentrated in one sector could dominate the movement of the index. For example, and as discussed below, Friedman conceded that special factors had raised the 1973 U.S. inflation rate by 2 percentage points. But over longer horizons, as other prices in the aggregate index adjusted, the sector-specific price increase became a relative-price phenomenon only. Thus, in Friedman’s view, over horizons stretching from 6 months to 2–3 years, aggregate forces came into their own in the determination of inflation, as offsetting downward pressure on the inflation rate (Instructional Dynamics Economics Cassette Tape 130, September 26, 1973).

Renewing criticism of Chairman Arthur Burns

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

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

Friedman continued to be irked by Chairman Burns’ acceptance or promotion of a variety of nonprice mechanisms for organizing allocations in markets. Burns did make a move in the direction of financial deregulation when the Federal Reserve Board removed the interest-rate ceiling on large certificates of deposit in May 1973 (Meltzer, 2009b, p. 868). The same month, however, on May 22, Burns introduced a new, informal control over commercial banks when he

---

48 See also the discussion of the first oil shock in Section II of the next chapter.
wrote to Federal Reserve member banks urging them to limit lending (Meltzer, 2009b, p. 868). Friedman’s declared: “I may say I think it is a disgraceful letter,” adding that the request it relayed was “extralegal” because it did not rest on formal Federal Reserve powers (Instructional Dynamics Economics Cassette Tape 123, June 23, 1973).

Shortly afterwards, during a week in which Burns was on the program of an event in Scotland commemorating Adam Smith (see Burns, 1973a), Friedman remarked that this was “is in some ways amusing,” as Burns’ advocacy of incomes policy had led the United States away from the reliance on market mechanisms that Smith had advocated. However, Friedman added, it was “in some ways appropriate,” as Smith had happened to depart from market principles in his defense of taking state action against high interest rates or usury, and Burns’ role in recent years heading the interest-and-dividend-monitoring committee—which served as an adjunct to the Nixon wage/price controls—had made the Federal Reserve Chairman, in effect, a modern proponent of interest-rate ceilings (notwithstanding his recent relaxation of CD rate ceilings).49

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

Marginal reserve requirements were among the most distortionary of the Federal Reserve’s control instruments, as agents had strong incentives to find ways of bypassing the requirements.50 Furthermore, to Friedman, marginal reserve requirements made no substantial contribution to monetary control: open market operations could achieve the same aim as changes in reserve requirements, whether of the ordinary or marginal type.51 Furthermore, measures directed specifically at restricting banks’ issuance of wholesale deposits were not powers that

---

49 This Friedman observation was in Instructional Dynamics Economics Cassette Tape 122 (June 6, 1973).
50 Earlier, Goldenweiser (1949, p. 15) had put marginal reserve requirements in a favorable light but had noted that they had not been deployed in the United States to date. In 1952, when he and Friedman were co-contributors to a joint statement by U.S. economists’ on using monetary policy against inflation, Goldenweiser had been part of the majority opinion urging the introduction of marginal reserve requirements in the United States, while Friedman had dissented from this recommendation (see Samuelson and others, 1952, pp. 387, 390).
51 This point, already articulated at length in Friedman (1960, pp. 30–35), was something Friedman reiterated in mid-1973: “It’s worth emphasizing that there is absolutely nothing that can be done by changes in reserve requirements that cannot be done by open market operations.” (Instructional Dynamics Economics Cassette Tape 124, July 4, 1973.)
Friedman considered necessary, as he largely excluded banks’ wholesale liabilities from his preferred definition of money.\textsuperscript{52}

Friedman’s criticisms of Burns’ financial-regulation measures were, however, skirmishes when compared with his case against Burns’ monetary policy record and the FOMC’s strategy. In 1973, Burns’ conduct of Federal Reserve open market policy since 1970, and especially since 1971, was the subject of a renewed and intensified indictment from Friedman.\textsuperscript{53} Burns’ occasional protests notwithstanding, Friedman saw these actions as responsible for the rapid growth of the M1 and M2 monetary aggregates since 1971 and the outbreak of inflation in 1973. It was on this subject that Friedman was, in 1973–1974, involved in what proved to be his most prominent public confrontations with Burns.

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

In this session, Friedman declared: “Some of my best friends are at the Fed.” He added, alluding to Burns: “And that is a literal statement.” But he went on to say that the prior eighteen months’ monetary growth had been “decidedly too high” and that the Federal Reserve “must bear a great deal of responsibility for an economic climate which underlies the rapid price explosion in the first few months of this year.”\textsuperscript{54}

Having thus foreshadowed that he would resume hostilities with Burns, Friedman did so in earnest in August 1973. For the past year, Robert Barro had been a new colleague of his in the University of Chicago’s Department of Economics; and, apparently somewhat to Friedman’s irritation, Barro did not hesitate to remind him of the elation Friedman had expressed publicly

\textsuperscript{52} See Nelson (2018b, Chapter 14).
\textsuperscript{53} In addition to what follows, see the discussion in Nelson (2007, 2016).
\textsuperscript{54} From Friedman’s testimony of June 21, 1973, in Joint Economic Committee (1973, p. 130).
when Burns became Chairman in early 1970 (Jeremy Siegel, interview, September 17, 2013).\textsuperscript{55} Against this background, in a cassette commentary early in August Friedman said of Burns’ record (Instructional Dynamics Economics Cassette Tape 126, August 2, 1973): “I must confess that I am baffled, because I did not expect it.” He recalled that, in the knowledge of Burns’ record as an economist and official prior to 1970, he had “more or less settled back” upon Burns’ appointment as Chairman, expecting monetary growth to proceed henceforth at a rate that was noninflationary and fairly steady, only to find himself being “disappointed in both respects.”\textsuperscript{56}

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

Burns did not read Friedman’s column on a regular basis.\textsuperscript{57} Furthermore, he was not well disposed toward responding to Friedman’s criticisms; indeed, it had become rare for him to refer to Friedman publicly.\textsuperscript{58} But in September 1973, Burns was pressed in Congressional questioning

\textsuperscript{55} Barro joined the Department of Economics during the first half of the calendar 1972 (Barro, 1972, p. 598; American Economic Association, 1974, p. 21). Subsequently, he was, as already noted, a presence in the money workshop when Friedman resumed his hosting of that workshop in the second half of 1972.

\textsuperscript{56} The early–1970 praise for Burns was documented in detail in Nelson (2016; 2018b, Chapter 15). A more cautious note had been voiced by Friedman in December 1969, when he had stated (\textit{Los Angeles Times}, December 7, 1969) that there was a 50 percent chance that even with the switch to the Burns regime the average rate of inflation in the next five years will be same as previous five, because of overreaction to recession. However, even this remark had not envisioned the scenario actually realized—of inflation being much higher in 1970–1974 than in 1965–1969.

\textsuperscript{57} During 1972, Board staff provided a file of Friedman’s columns for the first half of the year after Burns expressed curiosity about their content. Later, Friedman tipped Burns off about his August 27, 1973 column, by sending him a pre-publication copy. (Information from Burns papers, Ford presidential library.)

\textsuperscript{58} This reticence contrasted with that of Burns’ fellow Board Governor, Jeffrey M. Bucher, who delivered a speech in April 1973 ostentatiously titled “Why Is Friedman Like Freud?” (Bucher, 1973). This was apparently the only speech by a Board Governor to mention Friedman in its title until Ben Bernanke’s (2002, 2004a) two speeches concerning Friedman. The content of Bucher’s speech was more standard than the title. The speech did contain a reaffirmation of the point, stressed by policymakers in 1970–1971 (see Nelson, 2018b, Chapter 15), that the FOMC’s 1970 change in procedures had not constituted an endorsement of monetarism. Bucher observed (p. 5): “But we do not regard the monetary aggregates as the only important star in the firmament. That is to say that I believe there is little, if any, tendency at the Federal Reserve Board to mistake the monetarist view of economic behavior as a viable substitute for the whole of economic thought.” (As stated, this was not a position from which Friedman would be likely to dissent, as he did not see monetarism as pertaining to all economic thought; in particular, he did not see monetarism as seeking to displace or challenge mainstream microeconomics.)
about the column and so submitted a written reply to it. This submission—which in the event did not mention Friedman at all—argued that “Federal Reserve policy has been to resist expansionary forces,” with reference being made to the fact that M1 growth had been below nominal GNP growth in 1972–1973.59

From the monetarist perspective, this did not constitute a satisfactory response: M1 growth had been high in absolute terms; the fact of a spread of nominal GDP growth above M1 growth was not evidence of easy monetary policy. Indeed, as Friedman had discussed in his Morgan Guaranty piece early in 1973, M1 velocity had exhibited an upward trend over the postwar period, as the community economized on currency and demand deposits, which did not bear interest. In the case of M1 growth, the problem as Friedman saw it was that in 1971–1973 it had been allowed to be 8 percent, instead of being kept at 4 percent; in the latter case, he contended, average nominal income growth would have been commensurately lower.60

In another respect, Burns was aided by the form of the question asked of him at the hearing, as the coverage of the question was confined to monetary policy in 1972–1973, while Friedman’s column had criticized overall monetary policy since 1970. Burns’ response stated: “If money supply growth had been somewhat slower in 1972, it is doubtful that price behavior in the first half of 1973 would have been appreciably better.”61 Even Friedman could agree with this observation, in light of his contention that monetary policy’s effects on inflation made themselves most felt with an 18–24 month lag. But, ahead of making this observation, Burns made a remark with which Friedman could not agree: “The recent rate of inflation we have been experiencing cannot be attributed in any significant degree to monetary policy.”62 Burns instead cited “special factors that have tended powerfully to raise prices this year,” including food price increases, bottlenecks in availability of industrial materials, and U.S. dollar devaluation.63

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

---

61 In Committee on Banking and Currency, House of Representatives (1973, p. 337).
63 In Committee on Banking and Currency, House of Representatives (1973, pp. 336–337); quotation from page 336.
Burns’ cost-push views figured prominently again in the next run-in with Friedman, which was also in the context of a reply to a member of a Congress, this time a senator. With Friedman’s criticisms of U.S. monetary policy gaining ground, Chairman Burns wrote a letter to Senator William Proxmire in November 1973 (subsequently published in Federal Reserve district banks’ research periodicals and in the Board’s *Federal Reserve Bulletin*: one such printing was Burns, 1973b) that apparently rejected the notion of Federal Reserve responsibility for inflation while also, seemingly, denying much of a linkage between Federal Reserve actions and monetary growth.

To the previous list of world economic developments, devaluation, and commodity price changes, Burns added environmental controls as a source of recent inflation.64 Burns’ analysis prompted Friedman himself to let fly in a rebuttal, which also took the form of a letter to Proxmire, in early 1974.65 This rebuttal made headlines (for example, *New York Times*, February 26, 1974) and would see print as an article in a number of Federal Reserve Bank bulletins, including the *Federal Reserve Bank of St. Louis Review*.66

In this rebuttal, Friedman seized on Burns’ (1973b, p. 21) statement, “The severe rate of inflation that we have experienced in 1973 cannot responsibly be attributed to monetary management...” “As written,” Friedman wrote acidly, “this sentence is unexceptionable. Delete the word ‘severe,’ and the sentence is indefensible.”67 Indeed, because he judged that the statement’s construction, by conveying a different impression from the actual content of the statement, made it “about as misleading a statement as anybody can make.” (Instructional Dynamics Economics Cassette Tape 135, December 4, 1973.)

---

64 On later occasions, along with the OPEC actions, other items Burns would add to the causes of inflation were U.S. grain sales to the Soviet Union (discussed in Chapter 5) and (in Burns’ testimony of August 6, 1974, in Joint Economic Committee, 1974b, p. 268) increases in public utility prices, which would “release new inflationary forces.”

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

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

67 Friedman (1974c, p. 20). Although Friedman did not note it, Burns’ use of the word “severe” made the Burns statement a more qualified one than Burns’ September *de facto* reply to Friedman. Thus, by Friedman’s lights, that earlier statement was indeed “indefensible.”
Commodity price increases and other one-time events, Friedman said, might explain why U.S. inflation in 1973 reached 8 percent instead of 6 percent, but not why inflation could reach 6 percent in the first place. Furthermore, the longer the period one considered, the less valid it was to invoke nonmonetary events to explain inflation. Consequently, “the Fed’s long-run policies have played a major role in producing our present inflation.” Friedman also discounted Burns’ stress on the looseness of the links between Federal Reserve actions and monetary growth, and between monetary growth and the course of total spending. As he often did, Friedman presumed that Burns and he largely shared the same analytical framework, especially regarding the determination of inflation. Friedman interpreted Burns’ verbal formulations as evasions that failed to face up to the implications of that framework.

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

This interpretation is underscored by the further analysis Burns provided when, once again, Congressional questioning forced the Chairman to offer a reply to Friedman’s letter in the week that it appeared. Burns appeared irritated that Friedman had written a reply to Burns’ letter to Proxmire; that letter, he told Proxmire, was in response to Proxmire’s questions; it was “not addressed to Mr. Friedman’s questions.” Burns added: “Mr. Friedman is a very dear friend of

---

68 Friedman’s concession that 2 percentage points to 1973’s inflation rate could be due to essentially nonmonetary factors appeared in both Instructional Dynamics Economics Cassette Tape 135 (December 4, 1973) and Friedman (1974d, p. 20). Later, Friedman and Schwartz (1982, p. 104) granted that, for the United States, 1.5 percentage points of (output-deflator) inflation resulted from the first oil shock of 1973–1974 (see the next chapter). But this assignment should be regarded as pertaining essentially to the calendar-1974 U.S. inflation rate; especially when annual-average data are used, it is the case that much of the OPEC oil shock’s direct impact on U.S. final-goods prices was in 1974 rather than 1973. These Friedman discussions of the 1973–1974 U.S. inflation may have been the basis for a later textbook’s generalization (without citation of a specific paper by Friedman or by other monetarists) that monetarists assigned 2 percentage points of inflation to the first oil shock (Collins and others, 1984, p. 214).

69 Friedman (1974d, p. 21).

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

71 Friedman occasionally noted that Burns had changed his position from the 1960s on inflation (Instructional Dynamics Economics Cassette Tape 126, August 2, 1973) yet, as noted in the text, Friedman’s (1974d) dissection of Burns’ analysis reads as though Friedman cannot quite believe that Burns’ views are so different from his own.
mine,” Burns added. “I don’t wish to engage in any debate with him or with any other economist.”\textsuperscript{72} Burns cited the “extraordinary” U.S. budget deficits from 1971 to 1973; these had had “an enormous influence on the rate of inflation in this country,” including, he implied, by forcing higher rates of monetary growth.\textsuperscript{73} Burns saw this as a key weakness of “Mr. Friedman’s very interesting letter to you, [in which] you will find not even one word about fiscal policy.”\textsuperscript{74}

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

In another area, Burns was more sanguine than Friedman. Burns quoted a sentence from Friedman’s letter, “There is literally no way to end inflation that will not involve a temporary, though perhaps fairly protracted, period of low economic growth and relatively high unemployment.”\textsuperscript{75} Burns countered: “I think that we can end inflation over the next 2 or 3 years without going through a period of heavy unemployment.”\textsuperscript{76} Although he was a gradualist with regard to how to carry out disinflation, Friedman did regard a period of slow expansion of economic activity as an inevitable part of a move to lower inflation. On his reasoning, a stretch of time in which output was below potential was required, in order to generate the requisite

\begin{itemize}
  \item\textsuperscript{72} From Burns’ February 26, 1974, testimony in Joint Economic Committee (1974a, p. 745). The \textit{New York Times} report (February 27, 1974) on the testimony instead rendered Burns as referring to Friedman as his “old friend.”
  \item\textsuperscript{73} From Burns’ testimony of February 26, 1974, in Joint Economic Committee (1974a, pp. 745–746).
  \item\textsuperscript{74} From Burns’ testimony of February 26, 1974, in Joint Economic Committee (1974a, p. 745). This remark was superficially similar to the criticism that Brunner and Meltzer’s (1972, p. 842) made of Friedman in the JPE symposium, for “neglecting fiscal policy.” However, when it came to the inflation process, their own assessment downplayed fiscal influences (for given monetary growth) and so was similar to Friedman’s and dissimilar to Burns’.
  \item\textsuperscript{75} Friedman (1974d, p. 23), quoted in Burns’ testimony of February 26, 1974, in Joint Economic Committee (1974a, p. 747).
  \item\textsuperscript{76} From Burns’ testimony of February 26, 1974, in Joint Economic Committee (1974a, p. 748).
\end{itemize}
disinflationary pressure. Burns, in contrast, saw a great deal of existing U.S. inflation as not originating in excess demand and hence as removable via nonmonetary measures.

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

Monetary/fiscal policy interactions and sources of high monetary growth

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

Chairman Burns himself in late 1973 and early 1974 publicly accepted that monetary policy had been too loose in 1972 (see Burns, 1973b, p. 21, and his testimony of February 26, 1974, in Joint

---

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

78 Friedman (1973b, p. 6).
Friedman was aware of both these statements and indeed quoted each of them (the latter in *Wall Street Journal*, August 21, 1975). But in the 1980s he and Rose Friedman would insist that they knew of any official statement from the Federal Reserve Board that admitted a policy error.\(^{79}\)

It is interesting to speculate about why Burns’ 1973–1974 statements were apparently not counted as being such admissions. One reason may be that Burns’ (1973b) admission was highly qualified: it referred to the rapid M1 growth of 1972, rather than to the high M2 growth (in both 1971 and 1972) on which Friedman focused. Furthermore, Burns (1973b, p. 21) suggested that the state of excessive policy ease was only something that “may” be evident “[i]n retrospect,” and so not amounting to a clear-cut policy error.\(^{80}\)

Another reason Friedman did not see Burns’ assessment as implying true contrition was, of course, because Burns attributed much of monetary growth, even when facilitated by Federal Reserve actions, to larger federal budget deficits. This assessment on Burns’ part was echoed by Robert Holland, who became a new Federal Reserve Board Governor in 1973 and attributed 1972’s high monetary growth largely to fiscal deficits (*Kansas City Times*, October 18, 1973).

For his own part, Friedman did see the expanded size of government since World War II as conducive to higher monetary growth. He cited this factor in an analysis in late 1974 (*Wall Street Journal*, November 23, 1974) and, earlier in the year, he suggested that legislated deficits had helped promote inflation.\(^{81}\) These analyses lined up with those he had given in prior decades: the notion that the need to finance an upward trend in government spending might lead to greater use of money creation had featured in his 1954 talk on the danger of a secular peacetime inflation and in his 1963 lecture on inflation.\(^{82}\) Friedman was, nevertheless, very skeptical of the near-automatic relationship between deficits and monetary growth posited by Burns. Friedman saw what relationship there was between deficits and money creation as reflecting acquiescence by the Federal Reserve, rather than any responsibility or obligation by the central bank to accommodate fiscal deficits.

---

\(^{79}\) Friedman and Friedman (1985, p. 94). See also *Newsweek*, July 24, 1978.

\(^{80}\) Similarly, in testimony of June 27, 1973 (Joint Economic Committee, 1973, p. 184) and February 26, 1974 (Joint Economic Committee, 1974a, p. 719), Burns said that judgments that a move to restriction should have started sooner, or that monetary policy should have been less expansive, were those he likely agreed with only “[i]n retrospect.” These statements did amount to qualified admissions of error, but they also implied that the best analysis available at the time did justify the policy actions taken.

\(^{81}\) Friedman (1974h, p. 20). See also Friedman and Friedman’s (1985, p. 107) observation that the “rising and variable rate of inflation is... itself primarily a result of the growing role of government...”

\(^{82}\) Friedman (1954a, 1963). See also Nelson (2018a) for discussion.
Furthermore, as already indicated, the scale of monetary growth in 1971 and 1972 exceeded what could plausibly be attributed to accommodation of fiscal policy. Accordingly, in 1974 Friedman would state that the present inflation owed a great deal to Federal Reserve behavior that was separate from President Nixon’s choices regarding economic policy (Instructional Dynamics Economics Cassette Tape 151, August 7, 1974).

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

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

---

83 Friedman and Friedman (1984, pp. 25–26; 1985, p. 31).
84 In Newsweek, August 5, 1974, Friedman gave the government-spending share as 39.1 percent in 1970. In Los Angeles Times (June 8, 1973) he gave the share as 43 percent.
But, as already indicated, although he saw fiscal restraint as an appropriate part of a disinflationary package, Friedman did believe that, even in the absence of fiscal restriction, it was possible and desirable for the Federal Reserve to achieve a policy that ended inflation. The FOMC had, of course, engaged in monetary restraint for stretches of time in the 1960s—and would again in 1973–1974—but Friedman cited aspects of the Federal Reserve’s reaction function that led monetary restriction to be abandoned too soon and for excessive monetary ease to be the predominant problem since 1960. One was already discussed above: the conviction by the Federal Reserve authorities—most notably, Burns—that, in large part, inflation control should appropriately be assigned to nonmonetary instruments.

Another problem was that, since the mid-1950s, it was rarely in doubt in the United States that monetary policy had sizable effects on output in the short run. Thus, as Friedman acknowledged, the skepticism among 1970s policymakers that aggregate demand restriction would reduce inflation was accompanied by their acceptance that such restriction would produce higher unemployment, at least in the short run.85 This meant that even if monetary restriction was seen to be a necessary part of reducing inflation, the prospect of the real costs of disinflation might discourage the authorities from enacting such restriction—and encourage them to overreact when recessions did occur.86 These obstacles motivated Friedman’s advocacy of indexation in 1973–1974—a campaign discussed in Section II below.

The reluctance to accept monetary restraint was reinforced by the United States’ commitment to a full-employment policy, whose pursuit Friedman saw as having the effect of pressuring the Federal Reserve to pursue expansionary policies.87 Thus he saw the New Economists of the 1960s, by making the full-employment goal even more central to U.S. economic management, as having helped “put us into the pickle we’re in now” (Instructional Dynamics Economics Cassette Tape 153, September 6, 1974).

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

85 See Friedman (1973b, p. 6).
86 See, for example, Friedman’s discussion in Fortune (July 1974), as reprinted in Friedman (1975a, pp. 151–152).
87 See, for example, his discussions in Friedman (1974h, p. 20; 1975a, p. 60) of the pressures on the Federal Reserve.
productivity growth that occurred around the end of 1973 (a slowdown discussed in later chapters of this book).

Once inflation burst out in 1973, some of those pointing to excess demand as the culprit did suggest that it was likely that the economy’s potential had been badly overstated. Friedman was not at the forefront of these voices. He did insist that the United States’ inflationary policy had had its counterpart a period of “high fever” in which output boomed above potential. But he was not among those analysts closely scrutinizing official potential-output estimates and suggesting improvements; and, as discussed later in this book, he was not ahead of the pack in noting the post-1973 growth slowdown. Nonetheless, as stressed by Orphanides and Williams (2013) and documented in detail in Nelson (2018a, Chapter 8), Friedman eschewed policy prescriptions that were geared to the levels of unemployment or output or to estimates of the full-employment values of these level series. Because, as Orphanides (2003) showed, output-gap measurement errors during the 1970s dominated the levels of real-time gap series but not their corresponding growth rates, Friedman’s policy prescriptions and monetary analysis during the decade were considerably less vulnerable to errors in estimates of potential output than were those made by many in economic commentary policy circles in the same period.

Interest rates

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

Some indications of more near-term prospects for interest rates were provided in a newspaper survey in early 1973. One of the forecasting firms surveyed—White, Weld and Company—suggested: “We believe that the rate of inflation has moved downward more than generally realized. Long-term interest rates will decline in 1973.” (Kansas City Star, January 14, 1973.)

88 Friedman (1974a, p. 15). Later in 1974, Friedman described the U.S. as experiencing excessive money (that is, nominal) demand (The Inflation Merry-Go-Round, PBS, October 7, 1974). This description may have been in recognition of the fact that much of the excess spending had by now been transmitted to inflation expectations and was accordingly making itself felt less than previously in a response of real output.
Actual interest rate behavior (shown in Figure 2), like that of inflation, defied these predictions. In December 1972, the federal funds rate averaged 5.33 percent, the three-month Treasury bill rate 5.07 percent, and the ten-year Treasury rate 6.36 percent. Not until the final months of 1976 would the two short-term rates reach levels as low as this, and in the intervening years—as part of what Friedman described (in Instructional Dynamics Economics Cassette Tape 183, January 1976, Part 1) as the “much greater variability of interest rates over the past few years”—the federal funds rate had stood at 12.92 percent and the Treasury bill rate near 9 percent, with both peaks being reached in the summer of 1974.

And longer-term interest rates, far from being well below their 1972 levels by 1977, never returned to 1972 levels over the rest of the decade. Contrary to Arthur Burns’ observation in October 1972 that there were “no basic economic reasons why there should be any strong upward pressure on these long-term interest rates” (quoted in Evening Sun, October 13, 1972), the ten-year Treasury rate rose during 1973 and peaked at an 8.04 percent monthly average in August-September 1974. The period through early April 1974 would lead a financial reporter to see “inflation and continually soaring interest rates [as] vying with each other as the [U.S.] market’s major concern” (Financial Times (London), April 13, 1974), while events later in the year would be remembered by a financial columnist as “those dismal days of late 1974 when interest rates were going through the roof and it looked as though the world was coming to an end” (Kansas City Star, October 17, 1979).

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

The upshot of the developments just described was that, after 1972, there was a drastic alteration in the time-series pattern of nominal variables. The U.S. inflation rate had already shifted, in the period from the mid-1960s to the early 1970s, from being a modestly serially correlated series to one with autocorrelations above 0.7 in annual data (Klein, 1975a, p. 134; 1975b, p. 467). But

---

91 It was this shift up that contributed to the failure to reject the natural rate hypothesis in Gordon (1972), a situation analyzed in Nelson (2018b, Chapter 14). Nelson and Schwert (1977)—a paper that would be cited by Friedman and Schwartz, 1982, p. 479)—argued for treating CPI inflation as difference-stationary even for the period January.
once data from the early 1970s were included, a unit root process became a good approximation of U.S. inflation behavior (King, Plosser, Stock, and Watson, 1991, p. 824). Indeed, for an unsettling stretch of time in the mid-1970s, it looked as though the behavior of inflation had shifted even beyond unit-root characteristics into a pattern that economists seldom need to consider in analyzing modern advanced economies: a strictly explosive process for inflation. McCallum (1994, p. 235) found that, as the four quarters of 1974 were added to the sample period for an autoregression for inflation, the coefficient sum on lagged inflation rose from 0.879 to 1.016—a point estimate implying an explosive time-series process for inflation.93

---

92 Levin and Piger (2004, p. 6) cited Nelson and Plosser (1982) as finding that “postwar U.S. inflation exhibits very high persistence, approaching that of a random-walk process.” In fact, however, Nelson and Plosser argued that inflation was stationary—perhaps not a surprising position on their part, as they used a long annual-data sample, ending in 1970, during which inflation showed a decided tendency to return to its mean. Specifically, Nelson and Plosser (1982, p. 148) found a first-order autocorrelation for consumer price inflation of 0.58 for 1860–1970 and a first-order autocorrelation for GNP deflator inflation of 0.43 for 1889–1970. In contrast, King, Plosser, Stock, and Watson (1991) found U.S. inflation was \( I(1) \) when a long postwar sample of 1954:Q1–1988:Q4 was used.

93 McCallum’s regression was for GNP deflator inflation. Data on CPI inflation tell the same story. For example, using the CPI inflation data in the 1975 Economic Report of the President (Council of Economic Advisers, 1975,
A parallel development took place with respect to short-term nominal interest rates. In a Ph.D. dissertation that came to Friedman’s attention, Shiller (1972, p. 49) observed that “the time series of short rates has shown no tendency to diverge…” 94 Within a couple of years, Shiller’s comment became obsolete: a first-order autoregression for the federal funds rate for the twenty-year sample period August 1954-July 1974 yields an autoregressive coefficient of 1.012, implying an explosive interest-rate process—the “tendency to diverge” that Shiller had seen as alien to interest-rate behavior in the United States.

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

In his April 1972 lectures in Israel, Friedman had observed: “By now, one lesson that Irving Fisher tried to spread some 75 years ago has been learned. There is an important difference between nominal and real interest rates.” 95 At the time, he could look back at the concurrent rises in interest rates and inflation in the 1960s and their falling-off together in 1970 and into 1971. Therefore, as of early 1972, the most recent evidence in favor of the Fisher effect had been the fact of jointly declining rates of interest and inflation. But these declines (which in any event were partly illusory in the case of inflation, being a reflection of wage and price controls) proved to be ephemeral. Friedman had warned in 1970 that if monetary restraint was not abandoned, “interest rates over the next decade will be decidedly lower than they are now” (Instructional Dynamics Economics Cassette Tape 45, February 26, 1970). But he had also cautioned that “the prudent man will still have to reckon with a very considerable probability that over the next 5 or 10 years the average rate of inflation will not be kept below 3 percent and in that case interest rates will not be as low as I have said.” (Instructional Dynamics Economics Cassette Tape 56, August 6, 1970.) This second scenario for the 1970s contemplated by Friedman in 1970 was the one realized.

---

Table C–48, p. 304), a first-order autoregression for the estimation period 1955–1974 delivers a coefficient on lagged inflation of 1.086.

94 Shiller’s dissertation, written at the Massachusetts Institute of Technology, was cited in Friedman and Schwartz (1982, p. 478).

95 Friedman (1973b, p. 60).
The years 1973 and 1974 saw both inflation and nominal interest rates reach new peaks, increasing the correlation between the two series. The correlation between nominal interest rates and inflation was so strikingly clear in light of this experience—as well as another stretch of simultaneously falling interest rates and inflation, emerging in 1975—that Friedman and Schwartz published a note in 1976 on the comovement of the commercial bill rate and CPI inflation.96

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

With regard to short-term interest rates, however, Friedman’s reference to the elusiveness of the liquidity effect was at cross-purposes with his continued acceptance that the Federal Reserve exerted considerable influence on short-term interest rates via that effect. Looking back on the experience of the early 1970s, Friedman would reconcile the two observations. The authorities had let short-term rates move up with expected inflation, but had done so slowly and incompletely, thereby keeping the real interest rate down and even promoting a decline in it. The Fisher effect helped greatly in understanding the contours of interest-rate behavior in the 1970s; but it by no means provided a complete account. The liquidity effect, through which easier monetary policy put downward pressure on nominal interest rates, still prevailed as a short-term feature in U.S. data, and the Federal Reserve had exploited its ability to generate liquidity effects as a means of keeping the short-term real interest rate negative for much of the period through 1980. As Friedman put it in 1976: “Adjusted for inflation, interest rates have been abnormally low.”97 This narrative was consistent with Evans’ (1984, p. 221) observation that, as “inflation became chronic by 1979 because interest rates had been kept below their natural rates in the 1960s and 1970s, interest rates had to rise for disinflation to occur.”98

96 See Friedman and Schwartz (1976).
97 Newsweek, August 23, 1976. The attributions made to Friedman in the preceding sentences are based on the material in this column and those in Newsweek, March 10, 1975, Newsweek, December 29, 1980, and Friedman (1983a, p. 3; 1984a, p. 27).
98 See also Dornbusch and Fischer (1978, p. 517). The notion that the FOMC was slow in allowing real interest rates to adjust was consistent with Clarida, Gali, and Gertler’s (2000) estimates of the reaction function for the federal funds rate. (However, contrary to the Friedman and Evans narratives, their theoretical discussion took for
The U.S. authorities’ pre-1979 actions likely did not reflect a wish on their part to generate the abnormally low values real interest rates of the kind that were realized in the 1970s. On the contrary, Arthur Burns stated in September 1974: “Interest rates throughout the world and throughout history have followed the rate of inflation, and for a very good reason.” Rather, they likely reflected a belief that nonmonetary measures could so reduce U.S. inflation that the nominal interest-rate choices made by the Federal Reserve would be consistent with positive and normal levels of real interest rates. In the event, the ineffectiveness, except for short periods, of nonmonetary devices against inflation meant that they were not reliable instruments for helping deliver normal real interest rates. As Figure 3—which depicts a measure of the short-term real interest rate—shows that the temporary intensification of price controls in 1973 gave rise to an ephemeral period of real interest rates of 4 percent, one rapidly succeeded by negative real interest rates. At the end of the 1970s, a renewed determination to use monetary policy to achieve disinflation, and corresponding disillusionment with nonmonetary devices for this purpose, created conditions for the real interest rate to be made positive on a sustained basis.

In the meantime, and in contrast to the new peaks attained by nominal interest rates in the 1973–1976 period, the real interest rate plunged from 1974 onward, most clearly so for short-term real rates, as shown in Figure 3. Indeed, this is a factor explaining why the Wallich prediction for interest rates quoted earlier was not as far off as one might expect. For although neither Wallich nor many others contemplated inflation being as high it was from 1972 to 1977, neither could they imagine real interest rates becoming so low. Longer-term interest rates could plausibly be regarded as still implying positive real yields, but only if, as of the mid-1970s, investors expected inflation to recede to well below 6 percent. Indeed, Wallich—having changed his role from commentator to policymaker, thanks to his recent switch from Newsweek columnist to Federal Reserve Board Governor—observed in April 1974 (Wallich, 1974, p. 9) that “investors do not expect inflation over the longer run to persist at rates comparable to those we have experienced in recent months.” In the same vein, Friedman would note in 1976, after inflation had come off its 1973–1975 highs: “The market is acting as if it expected inflation to decline still further over the coming years.” (Newsweek, August 23, 1976.) Alan Greenspan

---

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

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

In 1963, Friedman and Schwartz’s *Monetary History* had remarked on the tendency for defects in monetary tools—that is, in the operational or implementational aspects of monetary policy, as distinct from policymakers’ macroeconomic strategy and doctrinal framework for understanding the links between monetary actions and the economy—to enhance “mistakes in policy arising from erroneous analysis.”\(^{102}\) The Federal Reserve’s behavior in the 1970s would reinforce this observation. Doctrinal errors made the FOMC less well-disposed than it should have been to

---

\(^{101}\) Greenspan observed that, until the late 1970s, “the markets assumed that a noninflationary environment was always just beyond the horizon…. [E]ven during the 1973–74 price explosion, long-term U.S. government bond yields rarely exceeded 8½ percent. This implied a long-term inflation forecast of 6 percent or less. In fact, with double-digit inflation in 1974, the forecast of a 6 percent rate of inflation over a 15–20 year period meant that the market expected an early return to previous inflation levels and a rate of price increase perhaps quite below 6 percent in the outlying years.” (From Greenspan’s testimony of January 22, 1981, in Joint Economic Committee, 1981, p. 12.)

\(^{102}\) Friedman and Schwartz (1963a, p. 531).
deploy monetary policy against inflation. But even in periods like 1972–1973 when the Committee actually sought to restrain monetary growth, elements of their operating procedures hindered the achievement of the desired restraint.

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

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

What is more, the FOMC had already shown in the 1950s that it was perfectly capable of, in effect, managing short-term interest rates while at the same time framing its short-term decisions in terms of choices of some kind of reserves aggregate. A thoroughgoing move to a reserves aggregate as an instrument, in contrast, would mean eschewing interest-rate management. A major indication that no such move occurred was given in Chairman Burns’ speech of May 12, 1972, in which he characterized the recent Smithsonian agreement on international monetary arrangements as likely to help in “giving monetary authorities somewhat more scope to pursue different interest-rate policies” (Burns, 1972, p. 77). In mid-1973, Burns referred to the “federal funds rate, which we control in large part,” and he further noted: “While setting target ranges for RPD at each meeting, the Federal Open Market Committee also establishes ranges of tolerance for the federal funds rate…”

---

103 See Nelson (2018b, Chapter 15) for an analysis of Friedman’s discussions during 1970–1972 of the FOMC’s monetary-aggregate targets.
104 From Joint Economic Committee (1973, pp. 185, 193).
As discussed in the previous volume, as late as October 1972 Friedman was taking the FOMC’s adoption of RPD as a very serious change in operating procedures. He would eventually judge, however, that the FOMC made no material change in its procedures between 1952 and 1979.\textsuperscript{105} Consistent with this judgment, Friedman’s initial (early 1972) skepticism that RPD was such a change resurfaced in 1973. This was evident in the explanation he offered early in 1973 for why monetary conditions had been allowed to become so relaxed in 1972: “the Fed became worried about rising interest rates” (\textit{Los Angeles Times}, February 18, 1973).\textsuperscript{106} Indeed, as the switch to the RPD regime increasingly revealed itself not to be an authentic and major change in procedures, the acrimony that often characterized the debate on Federal Reserve operating procedures resurfaced—and became more public than ever on Friedman’s part.

Friedman concluded (Instructional Dynamics Economics Cassette Tape 127, August 29, 1973) that the Federal Reserve’s misses of its monetary-growth targets could not be explained wholly by technical problems in controlling money. As before, he maintained that the Federal Reserve could greatly improve monetary control if it focused on that goal using a \textit{bona fide} commitment to a reserves instrument. But, when it was faced with more stable short-run behavior of interest rates and more stable monetary growth, he believed that the Federal Reserve preferred the former. He had previously found that this was the case with Chairman Martin; interaction with Arthur Burns convinced him that this was also true of Martin’s successor (Instructional Dynamics Economics Cassette Tape 116, March 2, 1973).\textsuperscript{107}

The attempt to hold down interest rates also hindered monetary growth over long-term horizons, Friedman argued, because it promoted serial correlation in deviations of monetary growth from its target rate.\textsuperscript{108} Thus in a retrospective on the inflation observed earlier in the decade, Friedman would later note in 1978 that pressures “to keep interest rates low are a major reason for high monetary growth” (\textit{Newsweek}, April 24, 1978).\textsuperscript{109} Friedman emphasized that the authorities’

\textsuperscript{105} Friedman (1982a, p. 103).

\textsuperscript{106} See also Friedman’s remark (\textit{Newsweek}, August 6 1973) that the Federal Reserve Board had wished “to avoid the wrath of Congressman Patman.” Although Friedman did not spell it out on this occasion (or in another reference to Patman in Friedman, 1975a, p. 59), Patman (Chairman of the House of Representatives Committee on Banking and Currency Committee from 1963 to 1974) was a longtime critic of the Federal Reserve and in particular of policies that involved raising nominal interest rates (see Meltzer, 2009a, 2009b). Note, however, that, as stressed in Nelson (2018b, Chapter 15), nominal interest rates were allowed to rise somewhat in 1972, and Burns’ cost-push views allowed him to see price controls as helping to reduce the need to set nominal interest rates still higher.

\textsuperscript{107} Consistent with this characterization of his views, Burns himself would publicly say that “our research indicates that short-run fluctuations in the money supply have little or no significant impact on the economy” (Joint Economic Committee, 1973, p. 192).

\textsuperscript{108} See especially Friedman’s \textit{Newsweek} column of December 8, 1975, as well as his more detailed written remarks on the matter in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1975a, pp. 45–46).

\textsuperscript{109} See also Friedman (1977a, p. 24).
unwillingness to allow nominal interest rates to vary more, and to exceed inflation, was inhibiting not just short-run monetary control but also long-run price stability, and that only a period of disinflation could permanently deliver low nominal interest rates. Hence his recommendation: “The best way to hold rates down in the long run is for the Fed to raise them temporarily.” (Los Angeles Times, February 18, 1973).

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

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

Nominal income, output, and unemployment

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

Speaking in late 1971, Friedman had acknowledged that—notwithstanding his complaints about stop-go policies—the post-World War II period had, on the whole, been a great success for the United States in terms of aggregate economic stability (Instructional Dynamics Economics Cassette Tape 89, December 26, 1971). Boughton and Wicker (1975, p. 468) were similarly upbeat, observing: “Monetary policy has improved greatly since the end of World War II…” But the degree of economic fluctuations observed from 1973 onward meant that such judgments demanded reappraisal. U.S. output variations continued to be small in relation to those in the

---

interwar period, but they were markedly more intense than those typically observed for much of the period since the early 1950s. Figure 4 shows that aggregate demand instability, whether measured by growth in nominal GDP or in real GDP growth, rose sharply—alongside the increased rates of inflation (shown for the same period in Figure 5).

Like Friedman, the Federal Reserve saw an excess-demand situation as prevailing during 1973 (see, for example, Meltzer 2009b, pp. 823, 869 on the FOMC’s discussions). On inflation, too, although Chairman Burns in 1973 attributed far more of the rise in inflation to special factors than did Friedman, he granted that demand pressure had played a part.111 By the time of his major acknowledgment on this score in late 1973, Burns’ FOMC had tightened monetary policy considerably. Indeed, monetary policy tightening showed up in 1973 on a host of criteria: slower growth in the monetary base, M1 and M2; higher short-term nominal interest rates—with the federal funds rate continuing the rise that had started in 1972; and for a time, and as already noted, higher real interest rates.112

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

Back in 1971, Friedman himself had warned that an excessive monetary expansion in 1971–1972 might well lead to a subsequent monetary contraction and a recession.113 During the course of 1973, he believed he was seeing this prediction being realized. In August, he stated that the U.S. economy might already be in recession (Wall Street Journal, August 3, 1973). In September, more cautiously, he observed that U.S. real output had been slowing down for some months (Instructional Dynamics Economics Cassette Tape 130, September 26, 1973).114 The step-down in monetary growth that had taken place over the year so far led Friedman in October 1973 to

---

111 See the Burns (1973b) publicly-released letter discussed above.
112 Specifically, the quarterly average of the real interest rate is positive throughout 1973, then becomes negative for one quarter in 1974:Q1, and is continually negative from 1974:Q4 through 1978:Q1.
113 See the discussion in Nelson (2018b, Chapter 15).
114 The fact that the United States underwent a pronounced slowdown in 1973 has led some to argue that the recession of 1973–1975 should be dated as having begun early in 1973. McNees (1978) proposed March 1973, and Paul Samuelson (Newsweek, February 18, 1974) argued that the United States was in a growth recession ever since the spring of 1973. Dornbusch and Fischer (1978, p. 542) argued that the growth recession began in the second
quarter of 1973 and suggested that this was obscured at the time by the fact that initial real GNP estimates for that quarter took higher values than in later revisions, and by the continued placid behavior of the unemployment rate over the course of 1973. Of course, a growth recession is highly distinct from an actual recession, and the definition of a growth recession depends on the assumed growth rate of potential output.
predict a recession, one in which “we might be in the early stages” already (The Evening Bulletin (Philadelphia), October 12, 1973; see also Chicago Tribune, October 13, 1973).

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

Into early 1974, Federal Reserve officials articulated a diametrically opposed view on both the likelihood of and the need for recession. Board Governor Holland observed in October 1973 that there were no flashing warning signs of recession (Kansas City Star, October 18, 1973). As already noted, in his February 1974 statements in reaction to Friedman’s public letter, Chairman Burns took issue with Friedman’s position that a period of lower output was needed to get inflation down. And still another Board governor made one of the most outspoken public criticisms made by officials of reliance on monetary restriction to eliminate inflation, one directed specifically at Friedman. In what the Wall Street Journal called an “ unusually blunt counterattack against critics of the Fed’s policies,” Governor John Sheehan denied that inflation was primarily due to the Federal Reserve and that using monetary policy against inflation would generate 15 percent unemployment and a popular uprising: “Milton could go to his farm [that is, Friedman’s Vermont summer home] and sit this out, but when he comes back, he will find the cities burned down and the University of Chicago along with them.” (Wall Street Journal, March 29, 1974.)

A substantial rise in unemployment—to 9 percent in May 1975, from 4.6 percent in October 1973—was not avoided in the event. Indeed, as that increase in unemployment attests, the mid-1970s recession turned out to be severe; it also featured a 2.75 percent decline in real GDP on modern data. Friedman regarded contraction and unemployment of these magnitudes as unnecessarily severe and avoidable by alternative, smoother monetary policies. In particular, as will be discussed below, Friedman laid much of the blame for the severity on the recession on

---

115 Sheehan’s statement was subsequently quoted in Wall Street Journal, April 1, 1974, Schiff (1977, p. 9), and Bordo and Istrif (2018, p. 13).

116 However, with Sheehan’s declaration likely in his mind, Friedman would note that this rise in unemployment had not led to mass riots (Newsweek, August 4, 1975). His analysis of the unemployment rate in the 1970s is discussed in Chapters 4 and 6 below.
the Federal Reserve’s conduct of monetary policy during 1974, which did not turn out to be gradualist.

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

The National Bureau of Economic Research later dated the 1973–1975 recession as having begun in November 1973. This dating underscores the fact that the recession predictions that Friedman made in late 1973 amounted to another case (paralleling his 1971 prediction of a 1973 breakout in inflation) in which he should have adhered unwaveringly to a forecast. But—just as he had with his 1971 warnings of severe inflation in 1973—Friedman did waver. After a decline in the fourth quarter of 1973, real GNP grew in the first quarter of 1974, and in August 1974 Friedman said he did not see the Federal Reserve converting the slowdown into a serious recession; he added that there might well be a “minor recession” ahead, but he did not see the U.S. economy as currently being in recession (Instructional Dynamics Economics Cassette Tape 152, August 21, 1974). When a sharp economic downturn did emerge, Friedman took flak for having been too sanguine about the economy. For example, Washington Post economics columnist Hobart Rowen would (in Washington Post, October 15, 1976) point to Friedman’s remark at the White House’s general summit (of September 28, 1974) on inflation, “We are not, and I emphasize not, in danger of a major depression or even a severe recession,” as an example of the fallibility of Friedman’s analysis. Likewise, Gordon (1976, p. 55) quoted Friedman’s Newsweek column of September 23, 1974, which stated that “recent rates of monetary growth are not too low,” as an indication that Friedman had little inkling of the major economic downturn that was in process.

The fact of a recession became widely known and accepted around October 1974, and its severe character became clear later in the year. There is no doubt that, as Rowen and Gordon implied,

---

118 Romer and Romer (1994a, p. 27) noted that the Federal Reserve’s first major public acknowledgment of a protracted decline in economic activity was in the official record of the October 14 FOMC meeting.
the severity of the U.S. output decline in late 1974 caught Friedman off-guard. But it would be a mistake to suggest that this revealed a basic flaw in his monetary analysis of the time. Rather, two factors seem to have been important.

First, like many others, Friedman simply underestimated the extent to which inflation would take off in 1974. At a Business Council conference in October 1973, Friedman stated: “The consumer price index is much more likely to hit a 6 percent or 7 percent increase than to fall from this year’s 5.7 percent to the standard expectation of 5 percent in 1974.” (American Banker, October 15, 1973.) The consumer price index in fact rose by over 10 percent in 1974, resulting in Friedman’s acknowledgment that he and others had underpredicted inflation.\(^{119}\) In his case, this underprediction meant that his forecast of high nominal income growth (made on the basis of the behavior of high monetary growth) did not map into positive real output growth. As noted above, the nominal money/nominal income relationship exhibited very little disruption over the years from 1973 to 1976 as a whole, at least if an M2-type definition is used. However, \textit{within} the aggregate growth of nominal income, the amount of increase that took the form of inflation in 1974, as distinct from output growth, exceeded what Friedman had anticipated.\(^{120}\) This underestimate largely reflected the combination of the end of wage/price controls and the food and oil price shocks which, as discussed above as well as in the next chapter, Friedman granted had a short-term effect of measured inflation. It deserves emphasis that he saw their effects as mainly mattering for the time pattern of inflation, rather than for its average rate over a period of years. Indeed, in \textit{Newsweek} (April 24, 1978), Friedman pointed to the striking agreement in the United States during the 1970s between averages of the inflation rate and those of prior monetary growth, so he did not regard overall inflation performance as out of line with what had happened to the monetary aggregates (and particularly M2).

A second important factor underlying Friedman’s failure to foresee the severity of the approaching recession was the considerable further tightening of monetary policy that occurred during 1974. This tightening rendered his earlier comments on the economic outlook obsolete. Although, as already noted, the short-term real interest rate was very low in 1974–1975, Friedman maintained that monetary policy was tight on average over that period, particularly in

\(^{119}\) See his acknowledgments of this in Instructional Dynamics Economics Cassette Tape 159 (late November/early December 1974) and Friedman (1975c, p. 178), for example.

\(^{120}\) In Chicago Tribune, November 21, 1973, he had seen a 7-percent-plus inflation rate as being likely in 1974.
mid-1974 through early 1975. This interpretation requires there to have been considerable fluctuations in the natural real rate of interest that involved the natural rate taking unusually low values in mid-decade. But, as will be seen in the discussion that follows of monetary developments, it is an interpretation that is backed up by the weakness of monetary growth.\textsuperscript{121}

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

The table shows that monetary growth, after falling in 1973 and early 1974, declined still further over much of 1974. Friedman’s analysis of monetary growth behavior led him to identify a “sharp further tightening in mid-1974” (\textit{Wall Street Journal}, August 21, 1975), an analysis basically consistent with chronologies arising from accounts of Federal Reserve deliberations—with Romer and Romer (1989, p. 141) dating a notable monetary tightening to April 1974.\textsuperscript{122}

Even when speaking in early December 1974, Friedman called the recession “mild” (quoted in \textit{Pittsburgh Post Gazette}, December 6, 1974). What was needed, he reaffirmed at this time, was to “stick with the policy of monetary restraint for a two- or three-year period.” If that happened, benefits would eventually flow on both the price and output fronts: “We can kill inflation, get to

\textsuperscript{121} Some evidence that the fluctuations in the natural rate intensified during the 1970s and that the natural rate troughed in 1975 is provided in the estimate of Laubach and Williams (2003, Figure 1, p. 1066). In addition, at the time, the \textit{Journal of Commerce} (March 31, 1975) noted: “Today, everybody and his brother want their funds employed at short term. There is little interest in having funds tied up for seven to ten years.” Such a pattern of behavior would tend to reduce the short-term natural interest rate.

\textsuperscript{122} Like Friedman, Romer and Romer saw this tightening as coming on top of an earlier tightening in 1973 (see Romer and Romer, 1993, pp. 79–80). Bernanke, Gertler, and Watson (1997, p. 121) also stressed that U.S. monetary policy began tightening ahead of the 1974 downturn and before the oil-price surge of 1973–1974. They characterized it as a response by the Federal Reserve to the nonoil commodity price rise. However, it may be more appropriate to view the tightening as instead primarily resulting from the FOMC’s recognition in 1973 of an excess-demand situation, as noted above.
Table 1. Behavior of U.S. monetary growth, 1971 to 1975

<table>
<thead>
<tr>
<th>Period</th>
<th>Old M1</th>
<th>Old M2</th>
<th>Modern M1</th>
<th>Modern M2</th>
</tr>
</thead>
<tbody>
<tr>
<td>1971:Q1 through 1973:Q2</td>
<td>7.5</td>
<td>10.9</td>
<td>7.3</td>
<td>12.1</td>
</tr>
<tr>
<td>1973:Q3 through 1974:Q1</td>
<td>5.7</td>
<td>9.2</td>
<td>5.5</td>
<td>6.2</td>
</tr>
<tr>
<td>1974:Q2 through 1975:Q1</td>
<td>3.7</td>
<td>6.6</td>
<td>3.7</td>
<td>5.8</td>
</tr>
<tr>
<td>1975:Q2 through 1975:Q3</td>
<td>7.2</td>
<td>10.4</td>
<td>7.0</td>
<td>15.4</td>
</tr>
</tbody>
</table>

Source: In the case of modern M1 and M2, growth rates from quarterly averages of the monthly data in the Federal Reserve Bank of St. Louis’ FRED portal. The study of Lothian, Cassese, and Nowak (1983) is used for quarterly levels data on the old (pre-1980) M1 and M2 series.

Friedman had, however, already raised concern that, since June 1974, monetary growth had fallen too much (Instructional Dynamics Economics Cassette Tape 156, October 23, 1974). And, as the response of the economy started to emerge in earnest at the end of 1974, Friedman’s analysis of the mid-1970s downturn took a consistent complexion (Instructional Dynamics Economics Cassette Tape 160, December 19, 1974). In his narrative, the Federal Reserve had made a material monetary tightening in 1973 that was in the appropriate direction but should have been enacted much earlier. But, taking the leveling-off of interest rates as itself a sign that tightening had stopped, the central bank had continued and overdone the actual policy tightening. Specifically, according to this account, the FOMC had permitted a further decline in monetary growth because it did not lower the federal funds rate target rapidly enough during the course of 1974. It had thereby “deepened the recession” (Newsweek, March 10, 1975). A minor recession had turned into one that was large by postwar U.S. standards. And, as Friedman would indicate in his retrospectives on the period, the scale of this recession prompted an overreaction by the FOMC in favor of expansionary moves that ultimately reversed the decline in inflation that occurred in 1975–1976.

Friedman’s suggestion that monetary policy tightened in late 1974 seems valid. It does not appear appropriate to regard monetary policy as actually easing in the second half of 1974 after

---

123 Friedman (1975b, p. 707).
124 Again, see in particular his commentary in Wall Street Journal, August 21, 1975.
125 Among Friedman’s many statements of this point, see Friedman (1975d), his written and in-person testimony in November 1975 in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1975a), his Newsweek columns of, August 25, 1975, October 3, 1977, and February 19, 1979, and Instructional Dynamics Economics Cassette Tapes 163 (February 1975, Part 1) and 183 (January 1976, Part 1).
126 There is a parallel between monetary policy being tight in late 1974—notwithstanding real short-term interest rates being low—and Bernanke’s (1999, pp. 11–12) argument that the Bank of Japan’s monetary policy was tight.
the federal funds rate peak. Commentary at the time suggested otherwise: for example, a September 1974 press report stated that “the Federal Reserve shift actually began earlier this summer[,] with the Fed cautiously encouraging a gentle decline in the sensitive federal funds interest rate” (Kansas City Star, September 13, 1974), and an October 1974 report suggested that since mid-August, “in a series of moves, the central bank has signaled its somewhat easier stance on credit by steadily lowering the target range for fed funds” (Kansas City Star, October 21, 1974). But the behavior of monetary aggregates records a further tightening of the screw during the later part of 1974, and the course of economic activity in late 1974 is consistent with a reaction to the monetary tightening.

In particular, the fall in monetary growth was followed by a severe contraction in real output in the second half of 1974. On modern real GDP data, the output contraction in 1974:Q3 (of $-3.7\%$ at an annualized rate) is worse than that of 1974:Q4 (of $-1.5\%$ at an annualized percentage rate). The data at the time, however, concentrated the output contraction in the fourth quarter, and the headline of the Washington Star-News (January 16, 1975) in the wake of the release the Commerce Department’s initial data for 1974:Q4 came out summed up the dismal situation: “GNP Drops 9.1 Percent: Slump Deepens, Inflation Soars.”

This output contraction was associated with a decline in consumer spending that was hard to explain using what—despite some modifications under the influence of Friedman’s and others’ research contributions—were still largely old-style Keynesian consumption functions. Mishkin (1977, p. 126), for example, characterized the period as witnessing a collapse in consumption, and Blinder (1979, p. 57) referred to “the astounding $13.7$ billion decrease in consumer spending in 1974:[Q]4.” The shift to much lower rates of money creation during 1974 likely offers insight into the contraction in U.S. consumption observed late in the 1973–1975 recession. The squeeze on real money balances engendered by the monetary contraction may have put downward pressure on asset prices, as well lowered medium-term expected future income, and these two forces together may have promoted the decline in U.S. households’ spending.\footnote{Abel and Bernanke (1992, pp. 172–173) pointed out that consumption fell more in the 1973–1975 recession than in the 1981–1982 recession, and they appealed to the permanent-income effects of the first oil shock as the reason for the difference. This explanation for consumption behavior in the mid-1970s is complementary to the appeal to monetary factors given here. The permanent income hypothesis (either in Friedman’s form or in modern versions) implies that permanent real events matter for permanent income and consumption. But it also implies that monetary policy actions can, by affecting real interest rates relevant for spending, affect consumption for given expected future consumption or income, and it is also consistent with monetary policy having effects on the trajectory, though not the long-run level, of real permanent income.}

\footnote{Abel and Bernanke (1992, pp. 172–173) pointed out that consumption fell more in the 1973–1975 recession than in the 1981–1982 recession, and they appealed to the permanent-income effects of the first oil shock as the reason for the difference. This explanation for consumption behavior in the mid-1970s is complementary to the appeal to monetary factors given here. The permanent income hypothesis (either in Friedman’s form or in modern versions) implies that permanent real events matter for permanent income and consumption. But it also implies that monetary policy actions can, by affecting real interest rates relevant for spending, affect consumption for given expected future consumption or income, and it is also consistent with monetary policy having effects on the trajectory, though not the long-run level, of real permanent income.}
The fact that monetary growth continued to contract in late 1974 when nominal and real interest rates were declining raises the question of whether the Federal Reserve could, using open market purchases, actually have prevented the decline in money growth. The answer to this would seem to be that they could have. This supports Friedman’s characterization that monetary policy in this period featured avoidable protracted periods of too-low monetary growth as well as other periods of too-high growth in money. Short-term nominal interest rates, while off their peaks, remained well above—indeed, decidedly above—their lower bound of zero, so the option remained available to the authorities to stimulate monetary growth simply by engaging in larger conventional open market purchases. Indeed, policymakers at the time conceded that they were capable of raising monetary growth. They rejected this policy option because of the further decline in nominal interest rates it might generate, not on grounds of feasibility (Wallich, 1975, pp. 5–7).128

Nor is there good reason to doubt that open market purchases would have generated a prompt uptick in monetary growth. Although Federal Reserve officials sometimes wrote as though the money stock did not respond to open market operations until some quarters after short-term interest rates responded, there are grounds for believing that, during the 1970s, a substantial same-quarter response of monetary growth to open market operations.129 The studies cited in Chapter 14 of the previous volume pointed to that conclusion. So did the findings of Christiano, Eichenbaum, and Evans (2005, Figure 1) which showed that the response of M2 growth to Federal Reserve policy shocks followed a pattern that, while building over time, included an immediate—that is, same-quarter—reaction. Thus, the decline in monetary growth in late 1974 appears both undesirable and eminently avoidable, and Dornbusch and Fischer’s (1978, p. 543) observation seems appropriate: “there is little justification for the reduction of monetary growth in mid-1974.”

---

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

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

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

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

Friedman’s departure from the University of Chicago was also a looming prospect, notwithstanding his recovery from his 1972 operation. In a sign of what was to come, the

---

130 Some of Friedman’s newly-written research published during 1974 is considered in the next chapter.
Friedmans spent the lead-up to Christmas 1974 in San Francisco (Instructional Dynamics Economics Cassette Tape 160, December 19, 1974).  

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

Friedman’s principal function at the conference was to serve as one of the discussants of Tobin’s paper (coauthored with Willem Buiter). About three pages of Friedman’s 7½-page comment amounted to an amplification of the defense he had given in his 1972 *Journal of Political Economy* of comments he had made in a *Newsweek* column (January 23, 1967). As conference attendee Benjamin Friedman—himself soon to emerge as one of the leading critics of monetarism—recalled, Milton Friedman’s focus on this column’s material flowed from exchanges in the conference’s morning session. The conference, despite its sweeping title, focused almost entirely on the “monetary policy versus fiscal policy” debate that had occupied so much journal space in the 1960s. It was therefore geared toward a discussion of aggregate demand determination, rather than the broader Keynesian-monetarist debate.

The morning’s floor sessions had seen Tobin characterize the Milton Friedman view of fiscal policy. “Milton kept saying, ‘No, no, no, that’s not what I said, I didn’t ever say that,’” Benjamin Friedman recalled. “And Tobin got very upset, and we all went off for lunch, and then when we came back from lunch, Tobin had gone off and Xeroxed one of Milton’s *Newsweek* columns in which he had said that. And then there was a debate, part of which took the form of Milton saying, ‘Well, you can’t criticize me in an academic setting for something I said in *Newsweek!*’ You know, that sort of thing. So people thought of Milton as a very, as I say, elusive, deliberately elusive, debater.” (Benjamin Friedman, interview, May 30, 2013.)

---

131 This was ahead of the American Economic Association meetings a week later in the same city.
132 See Brunner (1976a, p. 27) for the date of the conference.
133 See Tobin and Buiter (1976) and Friedman (1976a). Much of Friedman’s (1976a) discussion was analyzed in Nelson (2018a, Chapter 5).
134 In the published version of Tobin and Buiter (1976, p. 274), the *Newsweek* column was quoted, albeit indirectly, by citing Friedman’s (1972a, p. 274) quotation from the column.
Friedman did, however, defend what he had said in the *Newsweek* column (which had argued that, for given monetary policy, a tax increase would only have a temporary and minor effect on inflation) and, as he had in his 1972 *JPE* reply to Tobin, elaborated on the modeling assumptions that would roughly justify the results predicted in the *Newsweek* column.

Friedman’s defense of his *Newsweek* column at the Brown University conference reflected his conviction that, while his interventions in public-policy debates were made for a less technically-oriented audience than were his research contributions, both sets of contributions were based on the same economic framework. The public-policy writings lacked the methodical analysis expected of a research paper—“I would not defend in detail statements I might have made in more or less popular discussions,” Friedman would observe to the present author in 1991. But they typically included features that made the popular analyses line up with those Friedman made in his research papers. As Robert Gordon pointed out in the proceedings volume for the Brown University conference, Friedman’s “*Newsweek* pieces on fiscal policy contain almost exactly the same description [of the limited effect on nominal income on unmonetized fiscal actions]… as the journal articles” that Friedman published in the 1960s and 1970s. Benjamin Friedman, too, would note that Milton Friedman was among those academic economists who, when writing an article for the general public, made sure “it’s cleverly worded so that a professional peer who disagrees sees that there’s something there that’s actually an ‘out.’” (Benjamin Friedman, interview, May 30, 2013.) In his comment on Tobin and Buiter, Milton Friedman argued that the qualifications were a key part of the analysis: “such ‘hedges’ are highly relevant to both the theory that we find useful and the policy measures that we recommend.”

In particular, the proposition that unmonetized (or “pure”) fiscal actions little affected aggregate nominal income might be a useful approximation even if it did not hold exactly in the data.

The following month, Friedman attended the annual meetings of the American Economic Association, held in San Francisco on December 27–30, 1974. Friedman was a discussant at a conference session whose ostensible focus was the twenty-fifth anniversary of the “rediscovery of money” in the late 1940s and early 1950s. However, Friedman, like the other participants, focused on much more recent developments. Consumer-price data released in March 1974 showed that the twelve-month CPI inflation rate had reached 10 percent in February—the first occasion since 1948 on which the United States had had a double-digit rate (*Star-News*

---

136 Gordon (1976, p. 55). See also Nelson (2004) for a detailed comparison between Friedman’s *Newsweek* columns and his research writings.
137 Friedman (1976a, p. 312).
(Pasadena, California), March 22, 1974). The twelve-month rate had then stayed in double digits over 1974, with late-year readings at 12 percent. “Double-digit inflation and double-digit interest rates, not the elegance of theoretical reasoning or the overwhelming persuasiveness of serried masses of statistics massaged through modern computers, explain the rediscovery of money,” Friedman declared in the American Economic Association meeting session.138

Friedman’s contribution to the session affirmed his pride in being one of the early-1950s advocates of money’s importance. But it was only much more recently, as he saw it, that the economics profession had been reaching an adequate qualitative appreciation of the effects of monetary policy, particularly regarding inflation. Friedman saw this as the profession coming round, in large part, to his own view. A few weeks after Friedman’s San Francisco remarks, columnist William Safire wrote (New York Times, January 20, 1975): “Long ago, when even conservative economist Milton Friedman asserted, ‘we are all Keynesians now,’ the general theory put forward by Lord Keynes was, by and large, universally accepted.” But Friedman did not see the previous quarter-century that way at all. On the contrary, as he had written in 1968—when criticizing misinterpretations of the quotation from him that Safire would use—that there had been a long-running “fight in economics by some of us against entrenched Keynesianism.”139

Friedman took advantage of his remarks at the 1974 American Economic Association meetings to note one matter on which he had gained ground in the profession: attitudes to his monetary view of inflation. “The rediscovery reflects primarily the impact of experience. How can any teacher of elementary economics today stand in front of a class and begin his exposition of what is inelegantly called ‘macroeconomics’ with the statement: ‘throughout this course we shall treat the price level as fixed’?”140

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

INDEXATION

In 1973 and 1974, in personal appearances as well as in his Newsweek columns and other public writings—most notably an article in Fortune (July 1974)—Friedman made a prominent case for

138 Friedman (1975c, p. 176).
139 Friedman (1968b, p. 5).
140 Friedman (1975c, p. 176).
widespread indexation to inflation of contracts in the U.S. economy. This activity of Friedman’s and the debate it sparked is recounted and analyzed in detail in Nelson (2018c). The discussion of that debate in what follows is therefore limited to a summary and expansion of the main points.

For the public sector, Friedman called for tax schedules to be indexed. Such a move would prevent increases in average tax rates that arose when taxpayers had nominal personal and corporate incomes that merely kept in step with the price level. In Newsweek (May 18, 1974), Friedman pointed to various countries, most recently Canada, that had introduced tax indexation. Other countries would follow later in the 1970s, including Australia. However, tax indexation did not prove an altogether enduring reform—it would eventually be abolished in both Australia and Canada, for example—partly because governments, when returning inflation-induced revenue, preferred to do so via reductions in tax rates rather than by adjustments to income-tax rate thresholds. Indexation would, however, be introduced permanently into the United States federal income tax schedule in the 1980s.

Friedman also advocated the issuance of indexed long-term government bonds. His support for this measure went back to the 1940s. It was a reform that the United States would make in the 1990s although—as in other instances abroad—indexed bonds were issued in parallel with, rather than as a full replacement for, marketable nominal long-term Treasury securities.

The most controversial aspect of Friedman’s indexation proposals pertained to the private sector, for which he recommended widespread wage indexation. Four aspects of this proposal are worth noting.

First, Friedman did not wish indexation of nominal wages to the price level to preclude adjustments of the real wage to real shocks. Rather, he saw indexation as aiding the process in which real wages and relative prices adjusted appropriately to developments in the real economy. Indeed, he supervised in 1974–1976 dissertation research by his student, Jo Anna Gray, whose work underlined the need for real wages to adjust to real shocks (Gray, 1976a, 1976b). Nevertheless, Gray and others felt that Friedman’s public expositions of indexation gave insufficient emphasis to the ongoing need for appropriate readjustment of the relative-price structure in response to real shocks, including the then-new OPEC oil shock of 1973–1974.

Second, many misinterpreted Friedman’s advocacy of indexation as amounting to giving up hope that price stability would be achieved. Friedman, in contrast, viewed wage indexation both as a
means by which the private sector could protect itself against inflation when it was present and as creating conditions that made restoration of price stability more likely. He believed that disinflationary policies would be made more possible both because he tended to emphasize—indeed, overemphasize—incentives that governments had to choose to inflate (incentives that stemmed partly from non-indexation of incomes in the short run) and because he thought, as discussed presently, that indexation would limit the short-term damage to output and employment of a disinflationary policy. In a move that was likely intended to highlight Friedman’s view that indexation would promote disinflation, a rewrite of his *Fortune* article for a U.K. readership was subtitled *A Proposal for Escalator Clauses To Reduce the Costs of Ending Inflation*. Nevertheless, the perception that Friedman’s advocacy of indexation was a sign of acquiescence to permanent inflation died hard, and *Euromoney* magazine (April 1975, p. 21) asserted: “Even such an intrepid inflation fighter as Milton Friedman has practically thrown in the towel and advocated linking just about everything under the sun to the cost-of-living index, citing Brazil’s positive experience with ‘indexation.’”

Third, much initial reaction to Friedman’s proposals took him as advocating compulsory indexation of wages. But Friedman in fact insisted that such indexation be voluntary. This aspect of his recommendation was explicit in his *Fortune* piece, but he underlined it in a September 1974 talk in London, in which he declared: “What we ought to do is to encourage private indexing but not require it.” Likewise, in 1978 he stated of indexation clauses: “Such contractual arrangements should be on a strictly voluntary basis for private transactions but should be legislated for governmental transactions.” (*Newsweek*, May 29, 1978.) The misperception that he favored compulsory wage indexation likely stemmed from Friedman’s use of the example of Brazil. In Brazil, which Friedman had visited in late 1973, widespread indexation of private-sector contracts—an arrangement known as “monetary correction”—was indeed compulsory. During 1974, Friedman clarified that his advocacy of indexation did not hinge on the Brazilian case and that his belief in indexation predated his exposure to Brazil’s experience (Instructional Dynamics Economics Cassette Tape 149, June 26, 1974). Working at cross-purposes with this clarification, however, the publishers of the U.K. version of Friedman’s indexation article issued it as a pamphlet with the main title *Monetary Correction.*

---

141 See Friedman (1974b).
142 Friedman (1974c, p. 77).
143 See Boianovsky (2018) for a detailed analysis of Brazil’s experience and Friedman’s commentary on it.
144 Friedman (1974b); see also the subsequent U.S. reprint, Friedman (1974c).
Fourth, Friedman hoped that indexation would align the actual-inflation and expected-inflation terms in the expectational Phillips curve. Therefore, starting from high inflation, the interim costs in terms of lost output and employment of a tighter monetary policy, designed to restore price stability, would be reduced. As Friedman put it: “It is the delay in the adjustment of anticipations that prevents inflation from being halted without a temporary period of unemployment.” The research literature in the 1970s and 1980s offered only mixed support for this contention. On the one hand, results showed that a credible disinflation—one in which nominal contracts rapidly recognized the new, lower long-run inflation rate implied by a firmer monetary policy—would have little real cost. Such findings lent weight to the introduction of arrangements that allowed speedier adjustment of contracts to perceived developments in expected inflation. On the other hand, it was also pointed out that contracts that indexed wages to recent rates of inflation—which seemed to be how widespread indexation might be implemented in practice—would stretch out the reaction of inflation and real variables to monetary policy developments and to various real shocks, rather than producing a quick closure of the output gap in response to such developments. Friedman’s post-1974 writings indicated that he was taking heed of such results; in a 1977 paper, for example, he conceded that indexation need not be a reliable means of promptly aligning expected and actual inflation.

Giannoni and Woodford’s (2005) analysis of the effects of indexation in a modern New Keynesian model underlined the mixed implications of indexation. They found that indexation, when added to an environment of sticky prices, does reduce the distortion to relative prices arising from inflation; in so doing, indexation improves welfare. However, indexation means more persistence in the output gap and inflation. It therefore implies more persistent deviations than otherwise of aggregate output from its no-nominal-rigidities baseline.

WATERGATE

A passing remark by Rose Friedman in a dialogue with her husband for his cassette commentary series has an eerie quality in retrospect. In a session recorded on June 14, 1972, she indicated that the discussion would turn to longer-term topics “in view of the absence of really hot immediate issues” (Instructional Dynamics Economics Cassette Tape 101, June 14, 1972).

145 Friedman (1974g, p. 64).
146 Although these results came from the rational expectations literature, they lined up with Friedman’s (1969, p. 45) observation that “anticipated inflations or deflations involve no tradeoffs between inflation and employment.”
147 Friedman (1977a, p. 412). This contrasted with Friedman’s position on indexation (“escalator clauses”) in Friedman (1958, p. 183 of 1969 reprint).
Just three days later, the break-in at the Watergate hotel took place in Washington, D.C. The subsequent tracing-back of the burglars’ activities to the instructions of White House personnel gathered steam over the rest of 1972; in 1973 and 1974, this process transformed into what would be described on the back cover of Bob Woodward and Carl Bernstein’s book All the President’s Men as “THE BIGGEST STORY”—the Watergate scandal.

As of March 1973, the Watergate coverup was gaining headlines but President Nixon, having won his landslide reelection only four months earlier, was still in a strong political position. As noted in Section I, Nixon had felt able to relax the wage-price controls considerably (via Phase III) and it was against that background that Friedman offered the previously-quoted praise for the president in January 1973 in Newsweek. The same perspective underlay remarks Friedman wrote about two months later: “I honor the president’s courage and good sense…” (Newsweek, April 2, 1973). This picture of a firm drive on Nixon’s part to liberalize the controls system of his first term would, however, prove premature: six months later, Paul Samuelson (in Newsweek, September 24, 1973) would look back on the president’s “on-again, off-again support for controls” over the course of 1973.

By late April 1973, the economic picture had deteriorated with the price-level surge; furthermore, President Nixon’s political security had been greatly reduced by Watergate revelations that had implicated his closest domestic aides. At the end of the month, a financial columnist reported (Manchester Union Leader, April 29, 1973) that, in this environment, “[e]conomists at the giant commercial banks see pressures mounting for an almost-complete reversion to more rigid wage and price controls.” Friedman shared concerns about this eventuality. Events moved in this direction in early May, when Nixon tightened the Phase III price controls. Nixon’s new measures included a requirement that major firms give a month’s notice if they planned to set prices more than 1.5 percent above their January 10 levels, as well as an announcement of investigations into these firms’ pricing practices. Friedman commented that Nixon’s latest move was “plain public relations gimmickry, which would not have the slightest effect on the course of inflation.” He interpreted the president as endeavoring to relieve the public pressure on himself concerning inflation, at a time when Watergate strains were intensifying (Daily News (New York), May 3, 1973). Nixon’s adjustments to Phase III had indeed come just two days after he dismissed his senior advisers H.R. Haldeman and John Ehrlichman in the wake of the Watergate disclosures.

Friedman commented further on Watergate in June. By this time, Paul Samuelson had publicly observed that the Watergate revelations had “irretrievably marred” the president’s reputation
(Newsweek, June 11, 1973). In his own comments, Friedman stressed that he lacked any “special competence or knowledge on the political side,” adding: “I don’t know whether President Nixon will be forced to resign. I hope not.” But, Friedman went on, the “odds certainly are increasing that way [resignation] every day.” He judged that, in the face of such threats to his tenure, Nixon was now more likely to acquiesce to pressure for the federal government to undertake greater economic intervention (Instructional Dynamics Economics Cassette Tape 122, June 6, 1973).

That is indeed what happened, with the intensification of price controls announced by Nixon on June 13, 1973, with the new controls phase being, as noted above, unofficially labeled “Phase 3½” or “Phase IV.” Another name given for the new phase was “Freeze II” (see, for example, Blinder, 1979, p. 109). For, in common with the original August 1971 proclamation of controls, the June 1973 measures froze consumer prices. One of the distinguishing features of what Darby (1976, p. 144) called “the fiasco which was Freeze II” was that the freeze, announced as applying for sixty days, applied only to final-goods prices and not to wages (Daily News (New York), June 14, 1973). This aspect of the freeze gave Friedman some wry amusement. As already noted, he saw the new measures, like their predecessors, as driven by a cost-push view of inflation. But, unlike the 1971 freeze, the June 1973 freeze did not apply to wages; consequently, it was inconsistent even with the wage-push rationale often invoked in 1971 for the imposition of wage and price controls (Instructional Dynamics Economics Cassette Tape 123, June 23, 1973).

Friedman was away from the city of Chicago during the announcement of the June 1973 measures. In his absence, the Chicago Tribune contacted George Stigler for a local response. His reported reaction (Chicago Tribune, June 15, 1973) reflected Stigler’s own disillusionment with the Nixon Administration’s approach to economic policy. Stigler had been involved in the new administration’s policy formation by accepting an invitation to head a Task Force on Productivity and Competition, which reviewed antitrust policy and reported to President Nixon in February 1969. In 1973, however, Stigler remarked acidly on having had “the good luck to miss” the telecast of Nixon’s announcement of Freeze II. Of the new measures, Stigler remarked: “This is lousy. We’re just going back to the original [1971] freeze. The last time, we

---

148 In Freeze II, as well as the Phase IV program that followed in August 1973, there had not been a return to the stricter wage controls that prevailed in earlier years’ phases; instead, the liberalization of wage limits introduced in January 1973 under Phase III had been permitted to continue (Darby, 1976, p. 144). (In contrast, Schuettinger and Butler, 1979, p. 108, stated, in a brief summary of the June 1973 package, that it included a freeze on wages—a puzzling contention in view of the fact that, as Blinder, 1979, p. 109, stressed, it was the absence of a corresponding wage freeze made the new freeze historically anomalous.) In addition, agricultural prices were exempt from the June 1973 price freeze.

quit when the controls began to fall apart. Maybe this time, we’ll stay with them until their
dismal failure—and finally learn our lesson.”

As for Friedman himself, a few weeks before the announcement of the new price freeze, he (in Instructional Dynamics Economics Cassette Tape 121, May 24, 1973) had lamented the way the Nixon approach to inflation of recent years had both “miseducated the public and… introduced price control as a permanent weapon in the armory of economic policymakers.” When Freeze II was announced, Friedman lashed out. His remarks in New York City the day after the freeze, at the *Institutional Investor*-sponsored second trust management conference, marked his most decisive break with the Nixon Administration (*American Banker*, June 15, 1973). Nixon’s controls, he said, were the “worst mistake in American economic policy that has been made by an American president in the last 40 years” (*Oakland Tribune*, June 15, 1973), and they amounted to a policy that “has no end except ever-widening control over every facet of your life and my life” (*American Banker*, June 15, 1973.) “Once you start, where do you stop?,” Friedman asked. “…You go from price controls to allocation controls to production controls… to rationing.” He added that price controls had never worked over the previous two thousand years of history (*Evening Sun* (Baltimore), June 15, 1973).150 A month later, Friedman remarked that he saw U.S. price control continuing indefinitely and declared this situation a disaster (*Bennington Banner* (Vermont), July 19, 1973).

The new price freeze ended on August 11, whereupon a more flexible Phase IV (genuinely bearing the name that some had given to the preceding Freeze II) governed until the end of the controls period (Darby, 1976, p. 145). As mentioned above, wage-price controls had been initially relaxed considerably in very early 1973, only to be intensified with the midyear freeze. The continued controls regime did not prevent a significant rise in inflation; and by late in the year, as discussed in Section I, even Arthur Burns, who had so promoted the 1971 proclamation of controls, favored letting U.S. wage and price controls end. Public opinion also shifted against controls during 1973, partly in response to the increased occurrence of goods shortages (Darby, 1976, pp. 143–144; Brittan and Lilley, 1977, p. 145). The popular backlash against controls that George Stigler hoped would occur had indeed materialized. In January 1974, John Dunlop, Director of the Cost of Living Council, confirmed: “The notion of Phase V is not a viable notion.” (*Dallas Morning News*, January 17, 1974.)

---

150 Some studies that appeared in the 1970s suggested that the unsuccessful record of price controls actually stretched back forty centuries (Schuettinger and Butler, 1979—a book cited by Friedman and Friedman, 1980) or even fifty centuries (Schuettinger, 1976).
The 1970 law that permitted the controls was allowed to expire on April 30, 1974, and with it, the controls themselves lapsed (Darby, 1976, pp. 138, 145; Blinder, 1979, p. 110). Friedman’s distaste at the experience and his wary attitude toward the president were highlighted by his reaction to a Nixon economic statement three months after the end of controls: “I especially welcomed the news Nixon will avoid wage-price controls. I hope this time he sticks to it.” (Times-Bulletin (Van Wert, Ohio), July 27, 1974.)

The jaded tone in that remark likely reflected not only Friedman’s disappointment with Nixon’s 1971 U-turn but also the reality that, by late July 1974, there was abundant evidence that Nixon himself had misled the country had Watergate. The president’s situation had already deteriorated after the first eight months of 1973 to such an extent that the New York Times (September 5, 1973) asked Friedman whether he regretted publicly supporting Nixon in 1972.151 Friedman replied that he did not. “I do not condone the Watergate business. I think it’s disgraceful… But Watergate does not alter what a disastrous choice McGovern would have been.” Indeed, the fact that McGovern was the Democratic candidate in 1972 underscored for Friedman the lack of political imperative for Nixon’s imposition of controls and his other 1971 changes to domestic economic policy. Friedman reasoned that, insofar as these measures bought short-term political benefits, those benefits had been unnecessary for Nixon’s reelection, as George McGovern would likely have been the opposing party’s presidential nominee regardless of whether controls had been imposed, and McGovern’s candidacy itself ensured Nixon’s reelection (Instructional Dynamics Economics Cassette Tape 119, April 25, 1973).152 Furthermore, the longer-term economic costs of the New Economic Policy meant that the policy change had not been politically beneficial beyond the short run—an observation that Friedman was later pleased to see Nixon acknowledge in his presidential memoirs (see his column of Newsweek, October 16, 1978, referring to Nixon, 1978).

Even though Watergate often dominated economic developments in news coverage during 1973, Paul Samuelson observed in his column (Newsweek, September 24, 1973) that “inflation is the No. 1 political issue today—not Watergate, not détente with Russia and China and not even the rise in unemployment that now seems on its way.” Friedman shared this view, and during 1974 he expressed tentative hope that political pressure like that Nixon had faced in 1970–1971 for

---

151 Friedman and several other academics were asked in the article whether they regretted including their names in full-page New York Times advertisements in October 1972 endorsing Nixon. In contrast to Friedman, Phillip Cagan and Friedman’s University of Chicago colleague Lester Telser indicated that they regretted endorsing Nixon.

152 However, in Congressional testimony on June 21, 1973, Friedman indicated that it was not until September 1972 that McGovern was no longer regarded as having a serious chance of winning the election (see Joint Economic Committee, 1973, p. 145).
over-expansionary policies was becoming less likely to recur. However, he also recognized that an abatement of the inflation rate from double-digit levels, if it occurred in conditions of economic sluggishness, might revive pressure for an urgent stimulation of demand, in which case the improvement in inflation would largely be temporary (Newsweek, June 24, 1974). As discussed in later chapters, this scenario would be realized in 1975 and in the early years of the subsequent economic recovery. It had been made more likely, as discussed earlier and further in Section III below, by the fact that the United States’ experience of the mid-1970s did little to discourage a nonmonetary approach to inflation analysis among policymakers, even though it did rule out mandatory controls as a viable anti-inflation weapon.

As far as the economic repercussions of Watergate were concerned, Paul Samuelson observed in a column (Newsweek, August 16, 1974) that blaming economic problems on Watergate was widespread but erroneous. This judgment lined up with sentiments Friedman had expressed the previous year, when he had said that Watergate had had essentially no direct effect on economic activity (Instructional Dynamics Economics Cassette Tape 122, June 6, 1973).

As for indirect economic effects of Watergate, Friedman commented (in Instructional Dynamics Economics Cassette Tape 132, October 24, 1973) that the “only good thing I can see” from Watergate was that, as it so occupied Congress’ time, fewer laws would likely be passed in the short run. But, as already noted, he saw Nixon’s authority as weakened by Watergate, so the scandal tilted national policy in favor of the still-more interventionist economic policies favored by Nixon’s political opponents. Friedman ventured to suggest that this did not have to be the case: several months into the post-election Watergate revelations, Friedman contended that Nixon’s popularity could not go much lower—though it did—so “now is the time for him to do unpopular things.” (Instructional Dynamics Economics Cassette Tape 23, June 23, 1973). But Friedman realized that this was not how events were proceeding in practice. Indeed, he alluded
to the leadership vacuum left by Nixon’s predicament when, in complaining about the Federal Reserve’s performance, he observed in early 1974: “If it does not take the lead in imposing the temporarily unpopular measures required, who will?”

By early 1974, although the records of Nixon’s tape-recorded White House conversations remained unreleased, and the matter of whether the president had himself participated in the Watergate coverup was still a matter of conjecture, enough of the White House officials’ activities in relation to Watergate had come onto the public record for Friedman to indicate that “I’m not for a moment condoning any of those abuses” (Instructional Dynamics Economics Cassette Tape 138, January 16, 1974). What Friedman would call the “growing nightmare of Watergate” proceeded further during 1974, as Nixon’s personal culpability in the affair became clearer. White House conversation transcripts that Nixon released (doing so, as it happened, in the same week as the end of wage/price controls) were highly damaging to the president’s case. An effort by the House Judiciary Committee to obtain more detailed and more accurate transcriptions of key conversations led to a new set of releases in July; these contained further evidence of Nixon’s immersion in the coverup. In the first week of August, the president himself released a new transcript of a damning exchange from June 23, 1972—what came to be called the “smoking gun” conversation. In the wake of this disclosure, the Kansas City Star editorialized (August 7, 1974): “Mr. Nixon has lied repeatedly to the American people.”

Nixon resigned on August 9, 1974. That day, Friedman was quoted referring to Nixon’s “misdeeds” in Watergate (New York Times, August 9, 1974), and over two years later he reflected more generally (Chicago Tribune, November 28, 1976): “I think he [Nixon] is a very complicated person. He operates on different levels. I do not approve of many things he did.” In particular, he affirmed around the same time that Nixon’s behavior in Watergate was “so reprehensible” (Instructional Dynamics Economics Cassette Tape 202, November 1976, Part 1). But Friedman also expressed some qualms about the way Nixon had been treated: he felt that the pressure for the president to resign had been precipitate and that any departure by Nixon from office before his second term expired should have been via the process of impeachment by the House of Representatives and trial in the Senate (Instructional Dynamics Economics Cassette Tape 156, November 6, 1974). In addition—referring, as he had previously, to what he perceived as the misdeeds of former president, Lyndon Johnson—Friedman claimed that Nixon’s misconduct had not been greater than that of some of his predecessors.

---

158 The quotation is from Friedman (1975a, p. 41).
159 In Feldberg, Jowell, and Mulholland (1976, p. 43).
Although, on the whole, Friedman’s judgment was that Nixon had been “a terrible president” (quoted in Reason, June 1995, p. 33), he was also capable of giving a more itemized appraisal.

With respect to Nixon’s economic record, Friedman observed at the time of the president’s resignation (Instructional Dynamics Economics Cassette Tape 151, August 7, 1974) that Nixon’s international policy was good since 1971 and bad before 1971, with the reverse true for domestic policy. And for domestic policy, it remained the resort to wage/price controls that figured most heavily in Friedman’s assessment of Nixon’s economic legacy. Friedman was not always clear whether he believed Nixon’s move in 1971 reflected a genuine but drastic switch in thinking toward Keynesianism or simply constituted political expediency. Either way, he saw it as showing a lack of principle—or not sticking to one’s principles—on Nixon’s part. It undermined Friedman’s earlier belief that Nixon was principled in the area of domestic policy (expressed in Instructional Dynamics Economics Cassette Tape 17, March 1969). In the years after Nixon’s resignation, Friedman often drew parallels between the president’s economic-policy switch and Watergate, as they both exemplified the insufficient weight that Nixon had put on adherence to principle (Chicago Tribune, November 28, 1976; The Power of Choice, PBS, 2007). “I think the greatest disappointment to myself and many others has been what the Nixon regime turned into,” Friedman observed when Nixon had been out of office for about three years (Reason magazine, August 1977, p. 29).

And as between Watergate and wage-price controls, Friedman was emphatic about which of them he regarded as the more damaging of Nixon’s actions. In late 1974, Friedman observed (Miami Herald, November 24, 1974): “I hold that against him more than all that Watergate business. Watergate was trivial compared to the damage done by the imposition of wage and price controls.”160 “There never was a more clear, sharper, stronger statement of opposition to price and wage control than Mr. Nixon made for two years straight in 1969 and 1970,” Friedman observed in a 1977 television appearance, “and [then] he went back on all of us by putting in price and wage control[s]…”161 Later in the year, he repeated his view that the controls were “far more harmful than the much more publicized Watergate scandals,” and a quarter-century later, in 2002, he declared that the imposition of controls was the “worst decision he [Nixon] ever made.”162

---

160 For a similar remark, see Instructional Dynamics Economics Cassette Tape 157 (November 6, 1974).
III. PERSONALITIES IN MONETARY POLICY AND PUBLIC POLICY DEBATES, 1973–1974

ALBERT REES

Against a background of many discouraging developments, one change occurring in the 1973–1974 period that Friedman strongly applauded was the removal of U.S. wage and price controls. Their abolition was effective as of May 1974; three months later, Friedman took the ascension of Gerald Ford to the presidency as increasing the likelihood that controls had been ended permanently. From his interaction with Ford when Ford was a leading Congressman, he believed that Ford had economic principles. In addition, Friedman conjectured, Ford “will sacrifice those principles for political expediency less often than Mr. Nixon did. In retrospect, it’s obvious that Nixon’s biggest flaw was a willingness to sacrifice principle for political expediency. The impression I have of Jerry over the years is that he is less likely to do so.”

With regard to compulsory controls, Ford gave an early signal that reinforced Friedman’s confidence. Ford told a press conference on August 28: “I foresee no circumstances under which I can see the reimposition of wage and price controls… [I]t means wage and price controls are out, period.”

But, even in these early stages of its life, the Ford Administration was giving mixed signals about whether and to what extent incomes policy would figure in the federal government’s anti-inflation armory following the demise of formal controls. And, as we shall see, by the end of 1974, Friedman was expressing fears that Ford would eventually reimpose compulsory controls.

One of the manifestations of the Ford Administration’s ambivalence toward incomes policy was in the appointments to senior economic positions. Ford kept as his Secretary of the Treasury William Simon and he secured the confirmation of Alan Greenspan, Nixon’s nominee to be Chairman of the Council of Economic Advisers. Both these figures were strong opponents of incomes policy and remained so as members of the new administration. Furthermore, Greenspan in particular was an outspoken proponent of the monetary view of inflation, and in September 1974 he testified before the Joint Economic Committee that in “the longer term… the general
price level is essentially a financial phenomenon which largely reflects changes in unit money supply.”165

But, in strong contrast, Ford also brought into his economic team a major advocate of incomes policy and of cost-push views of inflation. Ironically, in view of the widespread tendency to regard the Chicago school as synonymous with Friedman’s monetarism, the new appointment—Albert Rees—had nearly two decades of experience at the University of Chicago and had been a departmental colleague of Friedman over that whole period.

Rees’ appointment was announced at a conference on inflation that the administration convened at the Hilton Hotel, Washington, D.C., on September 27–28, 1974. This summit was the culmination of a series of more specialized conferences on inflation held over the previous weeks. Two of these (on September 5 and 23) had been economists’ conferences; Friedman attended both of the economists’ conferences as well as the eventual summit, which contained a spectrum of participants of different specialties, as well as business and labor representatives. CEA Chairman Greenspan had hosted the first of these conferences at the White House, and President Ford himself had attended the first and final sessions of that conference. The final session consisted of summary statements to Ford from several of the leading attendees, including Friedman, Samuelson, John Kenneth Galbraith, former Secretary of the Treasury Shultz, and three past CEA chairs. In the session, Friedman observed: “There is one, and only one, way to cure the disease, to slow down the rate of increase of total dollar spending, and only the federal government can effect that cure.” Friedman concluded his remarks by directly addressing Ford, urging him “to tell the public the hard truth and to persuade the public that the sooner we bite the bullet and take the cure, the better.”166

Ford received the remarks politely: “Thank you very much, Milton.”167 But already Ford was making recourse to nonmonetary approaches to controlling inflation, and this trend would continue. Days before his resignation, President Nixon had sent legislation to Congress requesting the revival of the controls-era Cost of Living Council, and early in his presidency Ford renewed this request (National Journal Reports, August 17, 1974). The consequence of this proposal was the passing of a law creating the Council on Wage and Price Stability. At the September 28 proceeding of the general conference on inflation, Ford announced: “The Council on Wage and Price Stability, recently established by Congress at my request and with my deep

165 From Greenspan’s testimony of September 26, 1974, in Joint Economic Committee (1974c, p. 8).
appreciation, is another arm I will use in the fight on inflation. I have asked Dr. Albert Rees, a distinguished economist and professor of economics at Princeton, to direct the Council’s work. We are fortunate to have Dr. Rees [here] with us.”168

For most of his adult life, Rees, who was born in 1921, had known Friedman. They had first met in the mid-1940s. Rees had received a Ph.D. from the University of Chicago in 1950, his graduate studies having overlapped with Friedman’s early years as a member of the Department of Economics. Even prior to completing his doctorate, Rees was deeply embedded in the working of the department: he had been made an assistant professor in 1948 (American Economic Association, 1974, p. 332) and was less formally a departmental member still earlier—as evidenced by his retrospective description of Henry Simons, who died in 1946, as among his “former colleagues” (in Selden, 1975, p. 83). Rees was promoted to associate professor in 1954 and professor in 1961, before leaving for Princeton University in 1966 (American Economic Association, 1974, p. 332). After Rees’ departure from the University of Chicago, he and Friedman had kept in touch, and Rees was a participant in the conference held in Friedman’s honor at the University of Virginia in October 1972.

Friedman was a great admirer of Rees’ research in the area of the macroeconomics of labor markets—an attitude evidenced by Friedman’s commissioning of Rees’ (1962) book *The Economics of Trade Unions* for the Cambridge Economic Handbook series. Rees and Friedman had much common ground, their degree of accord being reflected in the label that Rees would receive of “Milton Friedman’s Secretary of Labor” among people who knew them both (see Nelson, 2018b). In particular, and contrary to the analysis of labor unions made by a range of economists, Friedman and Rees both articulated the view that the prevalence of organized labor did not make nominal wages an exogenous variable insensitive to economic slack. Rees was also a strong supporter of Friedman’s proposal for a negative income tax (see Selden, 1975, p. 183).

However, Friedman and Rees differed on an area highly relevant to Rees’ new post in the Ford Administration. Rees was very much a subscriber to a traditional, permanently downward-sloping Phillips curve. This led him to articulate the view in the 1960s that higher inflation was acceptable in exchange for getting unemployment down. Strikingly, as the natural rate hypothesis emerged in the late 1960s, Rees would reaffirm his belief in a long-run nonvertical Phillips curve. In 1969, in a consultant’s memorandum for the Federal Reserve he matter-of-

---

factly took the unemployment/inflation tradeoff as a permanent reality. As Rees explained in a lecture on “The Phillips Curve as a Menu for Policy Choice” that he gave in London in January 1970, he was aware of the challenge of the natural rate hypothesis—or, as he put it, “the ‘expectations’ approach, whose strongest opponents have been Professors Phelps and Friedman.” But, as he went on to explain, he did not find the hypothesis plausible: he argued instead that the long-run Phillips curve, though steeper than the short-run curve, was not vertical. In September 1974, Friedman himself gave a lecture in London on the Phillips curve; the footnotes to the published version of this talk cited Rees (1970)—an Economica article consisting of Rees’ lecture on the Phillips curve—as an example of the resistance that the natural rate hypothesis had faced. Rees’ opposition to the natural rate hypothesis had continued in the intervening years, and in 1973 he had again disputed Friedman’s contention that the long-run curve was vertical (see Nelson, 2018b).

Furthermore, like many believers in the original Phillips curve, Rees also took a “partial cost-push” view of inflation. That is, while Rees believed that excess demand made inflation higher than otherwise and economic slack made inflation lower than otherwise, he regarded inflation as having an inherent tendency to occur even when demand was not excessive. According to this view, at full employment there was a tendency for nominal wages and prices to rise, due to cost-push factors having a positive mean.

This perspective provided a rationale for direct government intervention in wage- and price-setting even in circumstances in which it was accepted that aggregate demand policies could influence inflation, as government intervention might improve the tradeoff (both short- and long-run) between inflation and unemployment. Rees’ sympathy to incomes policy was reflected in his taking, during his time in the United Kingdom in 1969–1970, a visiting position in the Wilson Government’s Prices and Incomes Board. When the new Heath Government abolished the Board, Friedman expressed delight at its demise in a letter to Rees—and gave Rees some friendly chiding about the time he had spent at the Board.  

169 See Nelson (2018b, Chapter 13).
172 See Friedman (1975f, p. 18; 1976b, p. 221).
173 That this was an important part of the view of inflation advanced by proponents of the Phillips curve during the 1950s and 1960s was emphasized, with documentation, in DiCecio and Nelson (2013), Nelson (2018a, Chapter 10), and Schwarzer (2018).
174 Letter to Rees of November 9, 1970, from Friedman’s papers in the Hoover Institution archives.
The satisfaction that Friedman could take at the turn of events in the United Kingdom proved fleeting. By late 1974, official incomes policies had been back in force in the United Kingdom for two years, while various instrumentalities designed to monitor and regulate price-setting had been instituted by the U.K. government. And in the United States, Rees himself was in charge of the latest prices- and incomes-monitoring board. It did not have mandatory control powers. But, as Friedman’s reactions to Arthur Burns’ statements over the previous few years had indicated, Friedman did not believe this was not a crucial distinction. Indeed, in his March 1973 lecture in Yugoslavia Friedman had remarked that “an ‘incomes policy’” against inflation simply meant “controls on wages and prices.”

And when his appointment was announced, Rees himself indicated that he expected his agency’s price and wage recommendations to be followed, even though they would not be compulsory edicts. He cited his own 1969–1970 experience with the U.K. Prices and Incomes Board, which likewise had lacked statutory enforcement powers, as providing a precedent: “By and large, its recommendations were widely accepted.” (The Daily Princetonian, September 30, 1974.)

Congressional testimony that Rees gave in February 1975 explaining the functions of the Council on Wage and Price Stability confirmed that intervention would be vigorous: “In the months ahead, we plan to continue an active voluntary wage-price policy.” Rees also indicated that he believed that there was widespread wage-push pressure that he had a remit to fight: “Most of the wage increases that might contribute substantially to inflation are those reached through collective bargaining.”

“Whip Inflation Now”

By the time of Rees’ testimony, one of the Ford Administration most-remembered, but also most desultory, initiatives against inflation had come and gone.

Notwithstanding the Federal Reserve’s tightening phase, and the presence of figures in the Ford Administration like Greenspan and Simon, Friedman remained irritated by the proliferation of false cures for inflation. Following his attendance of Ford’s general conference on inflation, Friedman lamented that the participants at the conference had offered a “bewildering variety of

175 Friedman (1973d, p. 6). See also Friedman (1976b, p. 233) for a similar remark.
177 From Rees’ testimony of February 5, 1975, in Committee on Banking, Housing and Urban Affairs, U.S. Senate (1975b, p. 15).
proposals for governmental action,” other than action on aggregate demand (Newsweek, October 14, 1974). Following the conference, President Ford himself gave further credence to the notion that inflation could and should be eliminated using measures other than aggregate demand restraint. He announced a WIN (“Whip Inflation Now”) campaign, in which members of the public were encouraged—via the wearing of WIN badges, ownership of other WIN memorabilia, or the signing of a WIN form—to identify themselves as inflation-fighters.

Friedman’s reaction to the WIN campaign was derisive. It was a “bunch of hogwash,” he remarked (Chicago Tribune, October 18, 1974).178 Friedman also described himself as “very much disappointed” that, having rejected controls, the Ford Administration could then advance irrelevancies like the WIN initiative (The Sunday Bulletin (Philadelphia) March 2, 1975).

WIN was not well received by the public, and Ford Administration officials themselves quickly lost enthusiasm for the WIN program. By February 1975, it was clear that the WIN campaign had essentially been abandoned (Washington Star-News, February 12, 1975). The “silly” WIN program, Friedman would note, had been “hastily buried” soon after its launch.179

Rees and administered price inflation

The demise of WIN did not, however, mean the end of nonmonetary measures against inflation by the Ford Administration.

In April 1975, Albert Rees appeared before Congress to recount the activities in which the Council on Wage and Price Stability had been engaged in since its creation the previous year. In the 1950s, when theories of administered price inflation were gaining ground, he and Friedman had been in the vanguard of the counter-movement. They had stressed that even firms and workers that treated their product’s price as a choice variable were ultimately beholden, in their choices, to the forces of demand and supply. But now Rees suggested that the possibility of administered inflation remained a live issue. “On April 14th, the Council had a conference on ‘Concentration, Administered Prices and Inflation,’” he recalled. “The conferees represented all sides of the debate on the question of the effect on prices of a small number of producers with market power… One near-consensus of the meeting was that these ‘administered prices’ did not necessarily react quickly to changes in supply and demand. The debate centered around the

---

178 See also Friedman (1974h, p. 2).
179 Friedman (1976c, p. 10).
questions of whether or not administered prices, over time, are in fact inflationary...”\(^{180}\) Rees also reaffirmed that the Council perceived wage-push forces that it would aim to counter: “We expect to become more active in monitoring wage negotiations, since we believe that wage increases will become a more important element in the inflationary process than they have been in the recent past...”\(^{181}\) The following June, Rees cited reasons the Council needed to monitor wage and price increases: “there are noncompetitive forces in the private sector, in corporations, in strong trade unions, and in some agricultural cooperatives, and I believe that all of these organizations with market power have to be watched.”\(^{182}\)

In contrast, at the same June hearing a former Friedman student articulated the opposite position. Phillip Cagan testified: “I believe that the only way to control inflation is to see to it that aggregate demand, reflected in the budget and monetary policy, grows at an even pace... I think the Council is an exercise in futility and is a waste of the taxpayers’ money.”\(^{183}\)

Friedman judged in October 1974 that there was a 50 percent chance that wage/price controls would be reintroduced by Ford before November 1976 (\textit{Chicago Tribune}, October 18, 1974). There remained encouraging signs that this would not happen: for example, Secretary of the Treasury Simon observed in October 1974: “I thought we had learned our lesson [from wage/price controls]. It doesn’t work.” (\textit{Salt Lake Tribune}, October 28, 1974.) But near the end of 1974 Friedman noted that controls seemed to be rating highly again in opinion polls—reflecting the fact, he suggested, that public understanding of inflation’s causes remained poor.\(^{184}\) He speculated that Ford might reinstitute compulsory controls: this time, he nominated August 1976 as a date by which controls might be revived.\(^{185}\)

\(^{180}\) From Rees’ written answer in Committee on Appropriations, U.S. Senate (1975, p. 1829).
\(^{181}\) From Rees’ submission for a hearing held on April 23, 1975, in Committee on Appropriations, United States Senate (1975, p. 1821).
\(^{182}\) From Rees’ testimony of June 17, 1975, in Committee on Banking, Currency and Housing, House of Representatives (1975a, p. 33).
\(^{183}\) From Cagan’s remarks on June 17, 1975, in Committee on Banking, Currency and Housing, House of Representatives (1975a, p. 33).
\(^{184}\) Friedman (1975b, p. 707). At other times in 1973–1974, Friedman gave formulations that seemed to imply that the public understood that inflation was a monetary phenomenon. For example, in Friedman (1973d, p. 6) he said that “despite their complaints, people like inflation.” By this, he meant that the expansionary policies that ultimately produce inflation initially had favorable effects on output and perceived real wages, and conversely that the public did not like the short-run real effects of disinflationary policies. These Friedman formulations elided the difference between public demands for overexpansionary policies, on the one hand, and public realization of or acquiescence in the inflationary results of those policies, on the other. When he did consider the latter more specifically, Friedman acknowledged that inflation \textit{per se} was “a policy that nobody wants” (see his June 21, 1973, testimony, in Joint Economic Committee, 1973, p. 135).
\(^{185}\) Friedman (1975b, p. 707). See also Friedman’s remarks in Instructional Dynamics Economics Cassette Tape 159 (late November/early December 1974).
Developments during December 1974 seemed consistent with Ford going in this direction. In that month, Rees, at the president’s request, urged that the steel industry rescind part of its announced price increases. Rees would testify the following June that the fact “we rolled back prices in the steel industry in December” was an achievement of his Council. But the attitude underlying this policy move—that the control of inflation required that the private sector not be free to make decisions on price-setting—had been the basis for the controls programs under Nixon. It also provided Friedman with an unhappy reminder of President John F. Kennedy’s 1962 intervention in the setting of steel prices: “It is really depressing in the extreme to see Mr. Ford proceeding on exactly the same path Mr. Kennedy proceeded on.” (Instructional Dynamics Economics Cassette Tape 160, December 19, 1974.)

ARTHUR SCHLESINGER

In the course of a talk he gave in mid-1982, Friedman took issue with recent proclamations on the U.S. economy by “Arthur Schlesinger... [who] made his reputation as a historian but, apparently, is not content with that modest state.” These remarks amounted to a return bout with Schlesinger. Friedman had previously challenged the eminent historian in an exchange in the U.S. press in late 1974.

Arthur Schlesinger had an arrangement with the Wall Street Journal under which he supplied occasional op-eds. It was in this capacity that he wrote an op-ed for the edition of October 23, 1974, titled “How About Taking Inflation Seriously?” Schlesinger acknowledged that his op-eds to date had “stayed a good distance away from economics,” but he declared that this practice now had to stop. His new op-ed turned out not just to be on economics, but to be directly concerned with a topic in Friedman’s area of specialty of economics. Schlesinger dealt with the causes of, and appropriate response to, the phenomenon of an economy being struck by “deepening recession as well as by spiraling recession.” In short—although he did not use the term in his op-ed—Schlesinger was concerned with “stagflation.”

The term “stagflation” had been used in U.K. policy debates by Conservative party politician Iain Macleod in 1965 (see Nelson and Nikolov, 2004). It had not caught on then, but after Macleod prominently reused the term shortly before his death in 1970, “stagflation” recurred in

---

186 From Rees’ testimony of June 17, 1975, in Committee on Banking, Currency and Housing, House of Representatives (1975a, p. 33).
187 Friedman (1982b, p. 54).
discussions of the United Kingdom’s economic situation. It was also picked up to a limited extent in the United States in 1970–1971. For example, it appeared in U.S. press reports on the U.K. economic situation (for example, New York Times, July 22 and September 6, 1970; Time, September 14, 1970) while the term was used in U.S. economic discussions in Fall 1970 by Herbert Stein (Manchester Union-Leader (New Hampshire), October 6, 1970) and Senator William Proxmire (New York Times, October 18, 1970). In 1972, the term “stagflation” subsequently appeared as an entry in a U.K. business dictionary (Davis, 1972, p. 176), while G. and C. Merriam Company in the United States indicated it would be including the word in the next major revision of the Merriam Webster dictionary (The Crescent-News (Defiance, Ohio), February 4, 1972; F.E. Compton Co., 1972, p. 532). In late 1972, with the U.S. economy expanding and recorded inflation lower than in earlier in the decade, an analyst drew a contrast with “the ‘stagflation’ policy of the early Nixon years” (Boston Globe, November 5, 1972).

The terminology became much more widely used when Paul Samuelson prominently used it in early 1973. In particular, the new edition of his textbook, appearing in March, not only used “stagflation” but gave it an index entry, and Samuelson also used the term in his Newsweek column of March 19, 1973. It may have been because he had just read this column that Friedman himself used the term in his lecture in Yugoslavia in March 1973. Over subsequent years, he would describe the terminology as “unlovely” and refer to “that miserable, unpleasant word,” apparently registering his distaste both for the word itself and the condition it summarized.

U.S. media commentary on stagflation’s causes and implications was abundant in 1973–1974. As Dornbusch and Fischer (1994, pp. 484–485) would recall, “during periods of stagflation, such as 1973–1974… there are articles in the newspapers that the laws of economics are not working as they should because inflation is high or rising even as output is falling.” Schlesinger’s Wall Street Journal piece was a perfect example of such an article, as it declared that the phenomenon

---

189 The column would be reproduced in Samuelson (1973b), as was a January 1973 essay in which Samuelson also used the word “stagflation” (see p. 218). For his textbook’s references to “stagflation,” see Samuelson (1973a, pp. 825, 829, 833). Samuelson’s other uses of the word around this time included his commentaries on his own Instructional Dynamics Economics Cassette series of January 12 and February 8, 1973 (Tapes 117 and 119 of the Samuelson series) as well as Samuelson (1973c, p. 226).
190 Friedman (1973d, p. 8).
191 On “unlovely,” see Friedman (1977c, p. 455) and Friedman and Schwartz (1982, p. 442); the “miserable…” remark appeared in Friedman (1983c, p. 48).
of stagflation was testament to the fact that “in the commanding heights prices are set, not by laws of supply and demand operating in a free market, but by private market power.”

Assertions such as Schlesinger’s appeared notwithstanding the fact that, as Dornbusch and Fischer, stagflation was reconcilable with demand/supply analysis. They noted two phenomena that provided such a reconciliation, both of them relevant to analysis of 1973–1974. First, there had been supply shocks, such as the first OPEC oil price increase, that lowered potential output. Second, for given potential output, stagflation was explicable using an expectations-augmented Phillips curve: An adjustment upward in inflation expectations implied a leftward shift of the aggregate supply curve, and so a deterioration in the inflation/unemployment combinations available to the economy in the short run.

The latter explanation of stagflation—in which the coexistence of inflation and above-normal unemployment is a part of the endogenous dynamics of the response of the two series to monetary policy actions—had, of course, been something Friedman had stressed when advancing the expectations-augmented Phillips curve in the 1960s.

Stagflation was difficult to reconcile with a traditional Phillips curve that lacked a prominent expectations term. Hence the phenomenon was puzzling to subscribers to that version of the Phillips curve. Paul Samuelson, for example, accepted that the recent boom of 1973 had generated inflation, but he added that “at this time, and as far that one can look ahead, the country definitely faces a problem of wage-push.” (Financial Times (London), August 6, 1974.) Similarly, during the 1973–1975 recession Albert Rees remarked that “it is unusual to have inflation and recession simultaneously.”

Friedman, in contrast, noted that the emerging inflation and unemployment outcomes were vindicating his own version of the Phillips curve. By early 1973, with the global experience of stagflation experienced to date, alongside developments in the research literature favoring the Friedman-Phelps perspective, Friedman was able to observe that “it is now being increasingly recognized” that there is “no tradeoff except as a very temporary phenomenon.”

In the months ahead of the appearance of Schlesinger’s op-ed, Friedman made several more public affirmations of the success of the natural rate hypothesis. In May 1974, he observed:

---

192 From Rees’ testimony of June 17, 1975, in Committee on Banking, Currency and Housing, House of Representatives (1975, p. 32).
193 Friedman (1973d, p. 7).
“Recent stagflation, etc.—in this country, in Britain, and elsewhere—has just put to rest the idea which Bill Phillips is his original article produced on the basis of evidence of a century in Britain.” Similarly, in his lecture in London in September 1974, Friedman noted that the “emergence of ‘stagflation’… [has] rendered somewhat ludicrous the confident statements that many economists had made about ‘tradeoffs,’ based on empirically-fitted Phillips curves.”

At the same time, however, a strand of Keynesian economics could legitimately argue that stagflation was reconcilable with Keynesianism, even without oil shocks. Thirlwall (2018, p. 14), for example, has observed that it is inaccurate to suppose that “Keynes’s macro-model cannot explain stagflation,” because Keynes’ analysis allowed for exogenous wage-push inflation that made both unemployment and inflation worse. Though the Phillips-curve literature had stressed the endogeneity of inflation, arguments that inflation was almost wholly cost-push had long been dominant in U.K. policy circles, and it was the consistency of stagflation with the cost-push view that had prompted Arthur Burns and other key figures in the United States to embrace cost-push views from 1970 onward.

Arthur Schlesinger took the cost-push explanation of inflation as accurate. Hence, Schlesinger argued, what economic analysis needed to do was go back to cost-push theories of inflation as espoused in the United States in past decades by figures such as Galbraith and Gardiner Means. Schlesinger made next to no distinction between the monetarist position, older Phillips-curve views, and the attitude of the Ford Administration. Rather, he seemingly took for granted that they all used a permanently-nonvertical Phillips curve as their understanding of inflation and as the basis for using aggregate demand restriction against inflation. He therefore provided little acknowledgment either of the fact that proponents of the natural rate hypothesis rejected a long-run nonvertical Phillips curve, or of the fact that policymakers like Burns had in recent years

---

194 Instructional Dynamics Economics Cassette Tape 145 (May 1, 1974). Similarly, in The Economist (September 28, 1974), Friedman referred to “A.W. Phillips’ incorrect interpretation.”

195 Friedman (1975f, p. 19; 1976b, p. 221). Friedman made a related remark, further using of the term “stagflation,” later in the lecture (1975f, p. 21; 1976b, p. 226). In this second passage of the lecture, Friedman took stagflation as so far having shifted general opinion among economists in favor of an expectations-augmented Phillips curve but not necessarily one that implied a long-run absence of a tradeoff (that is, one with a unitary coefficient on the expected-inflation term). This characterization, as discussed in Nelson (2018b, Chapter 14), may have inadequately recognized the degree of U.S. academic conversion to the natural rate hypothesis by 1974. However, the acceptance of a long-run vertical Phillips curve for the understanding of inflation/unemployment patterns remained uncommon in 1974 in U.S. media and policy circles, as well as in academic economics outside the United States.

196 Over the same period, cost-push views were also prevalent in much of continental Europe. For example, in June 1975, Jelle Zijlstra, head of The Netherlands’ central bank, stated: “Taking a longer view than just the past year, inflation has accelerated regardless of the economic weather… [T]he power of particular groups and interests to push incomes and prices up has exceeded the power of governments to stop them from doing so.” (Quoted in Kansas City Star, August 14, 1975.)
themselves embraced cost-push views. Indeed, as already noted, during the mid-1970s a Ford Administration policymaker, Albert Rees, buttressed official support for cost-push theories by promoting the administered-inflation idea that firm profit-push forces drove up inflation.

A key reason why Schlesinger likely saw little similarity between his own views and those of the Ford Administration likely lay in the fact that he favored, and officials did not, a return to widespread compulsory price controls. Much like Friedman did at the time, Schlesinger speculated that the Ford Administration might ultimately reimpose controls. But, in contrast to Friedman, Schlesinger saw such a return as desirable: “the time has come to substitute price-fixing in the public interest for price-fixing in the interest of private profit” (Wall Street Journal, October 30, 1974).

Friedman was galvanized by Schlesinger’s op-ed into writing a letter to the editor, published in the Wall Street Journal about three weeks after Schlesinger’s article appeared. “Since Mr. Schlesinger ventures into economics,” Friedman wrote, “perhaps I may venture into history.”

Friedman had long had serious doubts about historians’ coverage of U.S. economic development. Speaking at Rockford College, Illinois, on December 6, 1974, a couple of weeks after his reply to Schlesinger appeared, Friedman would recall that when he and Schwartz were writing their monetary history, “I read a great many of the general histories of the nineteenth century,” and he had found himself “simply appalled by the level of ignorance of economic matters that was displayed in those history books…”

This dissatisfaction extended to historians’ accounts of the twentieth century, as Friedman’s rebuttal to Schlesinger’s letter indicated. The historical record, Friedman argued in his Wall Street Journal letter, suggested that large firms’ power had not increased substantially over time. This observation buttressed a point Friedman had made eighteen months earlier, when the Nixon Administration had oriented price controls toward large firms. On that occasion, he had pointed out that this made little sense even from an arithmetic point of view, as the largest price increases were then coming from small firms and agricultural prices (Daily News (New York), May 3, 1973). By late 1974, with larger firms accounting arithmetically for more price rises, the idea of administered inflation had, on the surface, acquired enhanced plausibility. But, Friedman stressed in his letter, the United States had acquired a tendency for inflation in the postwar period even though it was not obvious that firm power had increased over that period. In contrast, he

---

197 Friedman (1976e, p. 6). See also Friedman (1983b, p. 65).
added, the economic role of government had increased over the same period, and that role was a more plausible source of the emergence of sustained inflation.198

Friedman concluded: “Mr. Schlesinger’s prescription for inflation—increased direct control over prices by the government—is like prescribing a bottle of Scotch as a cure for alcoholism.” (Wall Street Journal, November 23, 1974.)

This was a poignant analogy. Over 1973–1974, Friedman repeatedly used analogies linking inflation or inflationary booms with alcoholism.199 He would stick with this analogy in later years.200 It is likely that the appropriateness of the analogy came from experience with colleagues at the University of Chicago. Among many of the university’s economists, there was a culture of heavy drinking. Although not part of this scene, Friedman saw it at close hand. In time, Friedman’s advocacy of drug legalization would lead to accusations that he had a carefree attitude toward substance abuse.201 But in fact Friedman, who drank only sparingly, was all too aware of the self-destructiveness arising from alcoholism and other addictions. And his own heart problems in the early 1970s would only have magnified his awareness of the health dangers that could arise from undue alcohol consumption. When, in early 1974, while Friedman was attending a conference in Paris, fellow conference attendee Peter Jonson suggested they go over to a conference event at which drinks were “free,” Friedman replied: “Perhaps free for you, young man, but not for me.” (Peter Jonson, personal communication, December 28, 2018.)

As for the actual cure, Friedman did share the position of many traditional Phillips-curve proponents that it involved demand restriction and abnormally high unemployment. The distinguishing feature of his position was that this period of economic slack would only be temporary; the fall in inflation would, in contrast, be permanent. In a February 1974 radio broadcast, Friedman had noted that “given that we have started on an inflationary course, there is no way of going from that position to a position of no inflation without passing through a period of unemployment.”202 Furthermore, failing to restrict demand would not maintain the lowering of unemployment initially associated with an expansionary policy, as that policy had pushed

198 Recall from Section I that, although he was unsympathetic with ideas that monetary policy was forced to accommodate fiscal policy actions, Friedman saw monetary policy in the postwar period as having been inflationary in part by taking actions intended to stabilize the economy and interest rates.
199 See, for example, Friedman (1973c, p. 31; 1973d, p. 8; 1975b, p. 702) as well as the examples given in Section I of this chapter and in the discussion that follows.
200 Among many examples, see Friedman (1975e, p. 18; 1992, pp. 214–215), Feldberg, Jowell, and Mulholland (1976, p. 17), and Friedman and Friedman (1980, pp. 270–276). See also The Listener (London), April 24, 1980.
201 See Chapter 11 below on 1987–1992 for further discussion.
202 University of Chicago Magazine, August 1974, p. 13. See also Section I above.
unemployment below its natural rate. Rather, the expectations argument, as he stressed at the December 1974 American Economic Association meetings, implied not only that unemployment fell in the early stages of a shift to a more inflationary monetary policy regime, but rose later on as it returned to its natural rate—the “stagflation stage” of the dynamic adjustment of the economy to the new inflation rate.²⁰³

What a disinflationary policy could achieve was restoration of the original, lower, inflation rate, at the cost of experiencing an interim period in which unemployment rose above its natural rate. Once inflation expectations fell back to equality with the inflation rate, unemployment would revert to its natural rate. This was a point that Friedman, several weeks before his American Economic Association remarks, had illustrated by again deploying an analogy to drinking. “Inflation is like alcoholism. The good effects come first with the first few drinks, but the bad effects are felt the next morning with the hangover. If you go on the wagon, the bad effects from the attempt of a cure are felt first, but the good effects come later when the cure takes hold.” (Chicago Tribune, December 1, 1974.)

What Friedman would see soon after he made these remarks was that the “bad effects” of monetary restriction—higher unemployment—were being made worse than necessary by a lurch by the Federal Reserve in the direction of severe tightening. This shift in monetary policy would dominate Friedman’s commentary on the U.S. economy during much of 1975.

²⁰³ Friedman (1975c, p. 178).
“Saturday Signing Brings Peace!” declared the front-page headline of The Desert Sun for January 26, 1973. This was how the local newspaper of Palm Springs—the city in which Friedman was recuperating from his heart surgery—announced what was believed to be the end to the Vietnam War. The Desert Sun report quoted U.S. Secretary of State William Rogers saying of the ceasefire arrangement coming into force that weekend: “we hope that this does usher in a generation of peace.”

The ceasefire agreement—embedded in what were known as the Paris Accords—followed a period in which President Nixon had intensified U.S. forces’ bombing campaign against North Vietnam. That intensification may have played a role in Friedman telling Nixon in their December 1972 telephone conversation that the president was doing a fine job. For, prior to this conversation, in his occasional public comments on the topic, Friedman had suggested that air attacks should play a larger role in the United States’ prosecution of the war, and that undue emphasis had been put on troop-intensive ground combat.

This viewpoint was similar to that of Senator Barry Goldwater, the 1964 U.S. presidential candidate whom Friedman had served as an economic adviser. Goldwater—who was a far closer observer of defense and foreign-policy matters than Friedman was—had strongly approved of Nixon’s intensification of air attacks, and he saw the Paris Accords as flowing from that move (Arizona Republic, January 25, 1973). This judgment was reflected in Goldwater’s euphoric

---

1 Email: Edward.Nelson@frb.gov. The author is grateful to the interview subjects and George Tavlas for their generosity in providing useful information. 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.

2 See Nelson (2018a, Chapter 15). However, the Friedman/Nixon conversation preceded by several days the period regarded as featuring the heaviest intensification of U.S. air attacks.
reaction to the January announcement. “The ceasefire-and-peace agreement in Vietnam marks one of the most important victories the United States has ever scored over Communist aggression,” Goldwater contended. “…[I]t is a monumental achievement for the President of the United States, attesting at one and the same time to Richard Nixon’s vision, courage, and ability. What the accord means is that a strong president, willing to take bold action, literally snatched victory from the jaws of defeat…” (*Arizona Republic*, January 26, 1973.)³

However, even in the period of 1972–1973 when it was possible to believe, as Goldwater suggested, that the United States was achieving something approximating a military victory, Friedman implied that lasting damage to the United States’ domestic scene had been created by the Vietnam War. In October 1972, he had observed that the “history of the last ten years would have been wholly different … [without] this terrible bypass via Vietnam.” The war had “altered the intellectual atmosphere” in a way that, in Friedman’s estimation, derailed the momentum for free-market thinking that he had perceived when he wrote *Capitalism and Freedom* (Selden, 1975, p. 51). The war and its prosecution had generated antagonism toward U.S. government decisions from university students and academics—in part because of the conscription policy that Friedman himself strongly opposed—and had reduced the confidence by the U.S. populace in the rule of government.⁴ However, very much in contrast to Friedman’s attitude, the leading critique of the government that had developed in U.S. academia as a result of antiwar sentiment supported more, not less, public-sector intervention in the domestic economy.

In the event, the “peace” to which Goldwater and Rogers referred in their January 1973 remarks did not materialize. Fighting continued after the withdrawal of large-scale U.S. forces. In late April, Friedman remarked that it was doubtful whether one could refer to the Vietnam War as having ended, in view of the fact that the “whole thing is coming unstuck right now” (Instructional Dynamics Economics Cassette Tape 119, April 25, 1973). Some weeks later, Paul Samuelson astutely noted in his *Newsweek* column (of June 11, 1973) that it was not yet clear whether the January 1973 settlement would be regarded as a historic Nixon achievement. During the early Nixon years, Samuelson had expressed the judgment that the president realized he could not win the war (Instructional Dynamics Economics Cassette Tape 30 (Paul Samuelson series), July 1969). Consequently, Samuelson was skeptical that Nixon in 1972–1973 had so

---

³ The Goldwater commentaries reported in the *Arizona Republic* on January 25 and 26 consisted, respectively, of remarks in an interview he gave on January 24 (following Nixon’s national address, on the evening of January 23, announcing the agreement), and of a syndicated op-ed Goldwater subsequently penned on the matter.

⁴ For example, in his Congressional testimony of June 21, 1973, Friedman remarked that “the moral climate… suffered very greatly from the divisiveness over the Vietnam issue in the past ten years” (in Joint Economic Committee, 1973, p. 137).
turned the tide that he had secured a sustainable settlement that would preserve South Vietnam as a noncommunist state. South Vietnam went on to fall to Communist forces in April 1975. In light of this outcome, Milton and Rose Friedman would, years further on, have occasion to refer to the sequence of events that “condemned us to defeat in Vietnam.”

The fact that the Vietnam War outcome eventually came to be judged as ending in defeat for the United States opened up the question about whether the 1973 settlement was known by its signatories to be unsustainable. Answering this question is well beyond the scope of this book, but it is a question that, roughly speaking, has generated two diametrically opposed answers. On one view—prominently advanced by Nixon in his memoirs (1978, p. 889)—the 1973 settlement could have been preserved had the United States both provided military support and imposed economic sanctions when treaty violations occurred during the period from 1973 to 1975. On the other view, the U.S. leaders knew even in 1973 that the settlement would not be sustained and that South Vietnam’s defeat was therefore a near-inevitable future step.

Those followers of foreign policy to whom Friedman himself was close did not have a unanimous position on which of these interpretations was more valid. Among them, Friedman’s friend Ben Stein—a White House aide until Nixon’s departure in August 1974—was a proponent of the view that the 1973 settlement could have been sustained. This contrasted with the ultimate conclusion of Friedman’s first dissertation student, Warren Nutter. Nutter, who had served in a senior position in the U.S. Department of Defense during Nixon’s first term, initially defended the Paris Accords, describing them in a February 1973 television appearance as delivering “an honorable settlement.” But Nutter later came to doubt whether the administration had been sincere regarding its stated intention to act to enforce the Accords. He would become an outspoken public critic of Henry Kissinger, who was Nixon’s national security adviser at the time of the Paris Accords and who would succeed Rogers as U.S. Secretary of State in August 1973.

When looking back on the war, Milton and Rose Friedman would imply that it had been lost well before 1973. They judged that the diminution in U.S. public support for the fighting meant that

---

5 Friedman and Friedman (1985, p. 80).
6 See Garthoff (1994, pp. 293, 491). This second interpretation is partly supported by Dallek’s (2007, p. 454) subsequent analysis of White House conversations at the time of the settlement.
7 In Kaplan and others (1973, p. 61).
9 For further discussion, see Chapter 6.
the country was unwilling to allocate the resources needed for victory.\textsuperscript{10} They also believed that, as “the Vietnam War helped to undermine belief in the beneficence of government,” a long-term effect of the war, including America’s defeat, was to boost support for free-market views.\textsuperscript{11}

\textbf{Research output in 1973 and 1974}

With the U.S. withdrawal from Vietnam leading to the end of conscription in 1973, Friedman in 1973–1974 confined his writings on international matters primarily to the topics of other countries’ economic experiences and of international economic policy. Some of this activity was manifested in his research publications and projects over this period.

In the second half of 1973, Friedman’s book \textit{Money and Economic Development} (some of which was discussed in the previous chapter) appeared. Friedman gave the book a bit of advance publicity in his June 1973 Congressional testimony, telling members of the Joint Economic Committee that it would soon be released.\textsuperscript{12} In that testimony, Friedman’s Congressional testimony observed that in \textit{Money and Economic Development} he “recommended as probably the optimum policy under current conditions for a developing country that it peg its exchange rate to its major trading partner…”\textsuperscript{13} The book’s remarks, together with those Friedman made elsewhere, have been seized upon by critics of floating exchange rates—an early example being Bergsten (1975, p. 456), who declared that “further reflection has led even the most ardent supporters of flexible rates (e.g., Friedman) to admit that fixed rates were superior for some countries”—who have interpreted Friedman as seeing his case for floating exchange rates as having limited applicability.\textsuperscript{14} According to their interpretation, Friedman was a supporter of fixed exchange rates for small and highly open economies, or indifferent on those countries’ choice between fixed and floating rates (see, for example, Hanke, 2008).

But the remarks in the 1973 book, alongside Friedman’s other writings, do not sustain this interpretation. They suggest instead that he saw the countries for which fixed exchange rates

\begin{footnotesize}
\begin{enumerate}
\item Friedman and Friedman (1985, p. 80).
\item Friedman and Friedman (1988, p. 484).
\item See Friedman’s testimony of June 21, 1973, in Joint Economic Committee (1973, p. 115).
\item From Friedman’s testimony of June 21, 1973, in Joint Economic Committee (1973, p. 115).
\item Another early example was Charles Kindleberger, who, in a paper for the November 1974 Carnegie-Rochester Conference, remarked (on the basis of their 1973 Congressional testimony) that “Friedman and Meltzer believe that small countries should not have independent monetary policies, but tie to a larger unit and take their money supply and interest rates from it.” (Kindleberger, 1976, p. 72.)
\end{enumerate}
\end{footnotesize}
were preferable to floating rates as falling into a very narrow category.\textsuperscript{15} His remarks implied that fixed rates were likely to be valid only for some developing economies and for \textit{very} small city-state economies, such as pre-1997 Hong Kong.\textsuperscript{16}

Even in \textit{Money and Economic Development}, Friedman’s favorable remarks about non-floating regimes were primarily in reference to the case of a currency union of a developing economy with a larger economy, and not to fixed rates.\textsuperscript{17} “I regard a floating rate as second best for such a country,” Friedman stated. His basis for this belief that a floating rate gave a developing economy the leeway to take recourse to high inflation as a source of revenue, an opportunity it might well seize, especially if it lacked reliable means of obtaining regular forms of taxes on a larger scale. Large-scale recourse to the inflation tax, he believed, would lower the real output of the economy, both because of the adverse influence of inflation on economic activity and because higher revenues for the public sector would encourage public spending over private spending.\textsuperscript{18}

Consistent with his belief in the wide applicability of flexible rates, Friedman during visits to Australia in 1975 and 1981 urged that that country float its exchange rate. Furthermore, during his period (1988–2003) as Governor of the Reserve Bank of New Zealand, Don Brash wrote to Friedman, asking whether he believed fixed exchange rates, or even membership of a currency union, were appropriate for New Zealand—which under Brash had floating rate alongside an inflation-targeting domestic monetary policy strategy. Friedman, Brash recalled, “wrote back and said, ‘No, no, I don’t think you should do that at all….’… And he made it very clear that we should not depart from our current arrangements.” (Don Brash, interview, July 9, 2013.) Likewise, in 1998, Friedman characterized the economies of Australia and New Zealand, which had floating rates, as having weathered the Asian financial crisis better than those economies in the region that had fixed their exchange rates (\textit{The Times}, October 12, 1998).

\textsuperscript{15} See his testimony of June 21, 1973, in Joint Economic Committee (1973, pp. 127–128) and the discussion that follows.
\textsuperscript{16} For example, in September 1965 (as recorded in American Enterprise Institute, 1966, p. 109), Friedman stated: “I think it would be most unwise at the present moment, for example, for Hong King to give up its present fixed rates...” In 1983, Friedman would make informal contributions to the discussions that led to the introduction of Hong Kong’s currency board, based on a close link to the U.S. dollar (Friedman and Friedman, 1998, p. 326).
\textsuperscript{17} A confusion is that here, as in similar remarks in Friedman and Roosa (1967, p. 121), Friedman insisted on calling a fixed exchange rate between two currencies a “peg” and that a truly fixed rate came only with a currency union. In Friedman (1973d, p. 12), in contrast, he called a peg a “‘dirty’ fixed rates” system, with a currency union being “clean” fixed rates. See also Friedman (1982a, p. 99).
\textsuperscript{18} Friedman (1973b, p. 64). He included among the cost of inflations the distortions arising from attempts to suppress it via price and other direct controls: see Friedman (1974i, pp. 273–277).
Money and Economic Development also offered a seemingly strong empirical demonstration of the validity of the quantity theory of money. Friedman presented a scatter plot depicting country averages of monetary growth per unit of output and of inflation for forty countries. What emerged was a visually impressive cluster around a 45-degree-line. This result seemed to line up with the quantity theory’s notion of a one-to-one relationship with monetary growth per unit of output and inflation. Although cross-country-average plots of this nature had appeared occasionally in research studies and other commentaries up to this point, it was only after 1973 that evidence based on cross-country averages became very prominent in the literature on the quantity theory on money. Such evidence received particular impetus from McCandless and Weber (1995), a study that used cross-country-average data in plots and regressions alike.

However, the sort of cross-country evidence that Friedman presented in 1973, and that much later research emulated, was something of a trap. Crucially, it was a procedure neglected time-series patterns within each country. It is perfectly possible for it to be the case that, in each individual country, the time series of monetary growth and inflation are highly correlated—perhaps after allowance for lags—yet that no (or a weak) relationship between monetary growth and inflation appears in a cross-country scatter plot using averages of the two series over the sample period. This could be so even when allowance is made for different values of real output growth across countries, by transforming the monetary growth series to growth in money per unit of output. The reason for this shortcoming of cross-country evidence is that individual countries may have country-specific velocity trends, arising for example from different rates of technological progress in the banking and payments system of each country. Consequently, cross-country evidence is not a valid test of the quantity theory relationships because they impose a common velocity trend across countries (Nelson, 2003, pp. 1036–1037; McCallum and Nelson, 2011, p. 111).

By 1983, Friedman was sufficiently conscious of the empirical importance of this point that he was moved to observe that the monetary growth rate consistent with long-run price stability “will

\[\text{\textsuperscript{19}Friedman (1973b, pp. 17–18).} \]

\[\text{\textsuperscript{20}The cross-country evidence does appear impressive taken as a whole, as the chart in Friedman (1973b) and regressions in McCandless and Webber (1995) demonstrated. But this largely reflects the fact that the country-specific velocity trends described above tend to be numerically small compared with the overall movements of monetary growth and inflation, provided that countries with very high average inflation are included among the countries included. When attention is directed to low- or moderate-inflation countries, evidence is mixed on whether monetary growth and inflation have a strong (and near-unitary) relationship when cross-country averages are used. In this connection, De Grauwe and Polan (2005) took the looseness of the monetary-growth/inflation relationship in cross-country averages for such countries that inflation was not a monetary phenomenon. But this conclusion neglected the velocity issue described in the references noted in the text above.} \]
depend on the country and the circumstances,” and he also stressed his view that Japan’s M2 velocity in the postwar period had a downward trend that partly arose from developments in the country’s financial structure.21

1974 saw the publication of a brief Friedman article called “Monetary Policy in Developing Countries.” As would be expected from its title, this chapter overlapped heavily in content with the *Money and Economic Development* book. Indeed, it was largely an early version of some of the material in that book. Reflecting its earlier origin, the chapter gave Friedman’s affiliation as “University of Chicago and the University of Hawaii,” in recognition of the fact that it was largely written during his early-1972 spell in Honolulu.22

The chapter is particularly notable because of two aspects of its condensed account of Friedman’s monetary policy views. First, it opened by reaffirming that “monetary policy is a poor instrument for [active stabilization]… thanks to the length and variability of the lag in the effects of monetary policy [and] the limitations of our knowledge about the factors responsible for such lags.”23 This passage, alongside much other evidence, highlights the fact that Friedman’s emphasis on model uncertainty figured in his thinking about monetary policy in the 1970s; contrary to some interpretations, it was not merely an “old Friedman” view characteristic of his work only up to the mid-1960s.24 Second, a section in the chapter titled “The Rate of Money Creation” provided a rare post-1969 Friedman citation of his 1969 paper “The Optimum Quantity of Money.” Here, he characterized the 1969 analysis as consistent with recommending “a roughly stable or slowly declining level of final products”—and thereby greatly downplayed the perceived support of the 1969 analysis for a policy of deflation.25 Similarly, the following section in Friedman’s 1974 chapter, titled “Prescription,” endorsed policies of steady monetary growth that kept down, but did not eliminate the private sector’s opportunity cost of holding money balances. As with his bottom line in the 1969 paper, therefore, he favored price stability, not deflation.26 Friedman had progressed from a 1968 remark that the deflation rule “might be better yet” than gearing monetary growth toward price stability, to his remarks in print in 1969

---

21 Friedman (1983a, p. 4); see also his related remarks in *Newsweek*, September 4, 1978. For a discussion of Friedman and Schwartz’s increasing acceptance of the relationship between financial structure and velocity behavior, see Nelson (2018a, Chapter 8).
23 Friedman (1974i, p. 265).
24 See Nelson (2018a, Chapter 7) for an extensive discussion.
26 See Friedman (1974i, pp. 276–278); see also Friedman (1969, pp. 47–48) and the discussion in Nelson (2018a, Chapter 7).
and later that downplayed the desirability and empirical applicability of the deflation rule.\textsuperscript{27} Friedman and Schwartz reported in 1973 referred to “the draft of the manuscript we are readying on Monetary Trends in the United States and the United Kingdom.”\textsuperscript{28} Their work on this sequel to Monetary History and Monetary Statistics continued over 1973 and 1974, although progress continued to be hindered by the fact that Friedman was occupied with many other activities. Friedman did find time to give a presentation on the project’s work on the United Kingdom at a seminar at the University of Minnesota in October 1974 (see Nelson, 2018b, Chapter 15).

Also over 1973–1974, the Encyclopaedia Britannica—which, despite its name, was now being both published and organized principally from the city of Chicago—issued a new set of volumes. A Newsweek report (January 21, 1974) on the new edition noted that “a number of big names” had contributed new entries, including Friedman and Nobel Prize-winning chemist Linus Pauling (who, in 1976, would become a prominent critic of Friedman’s receipt of the economics Nobel).\textsuperscript{29} Although Friedman’s entry, titled “Money,” attributed authorship merely to “M Fr.,” this was hardly an unsigned article and, as the Newsweek coverage indicates, it was always recognized as Friedman’s work. Friedman himself acknowledged his authorship of the piece on many occasions—including in the Financial Times (January 6, 1977) and in the bibliography his office provided to Thygesen (1977, p. 96)—and in Martin (1983, p. 61), he recommended the entry (again identifying himself as its author) as an introductory treatment of monetary issues.

The entry’s discussion of the “monetarist” view (a term he used freely in the article) overlapped considerably with text from his published writings from earlier in the decade. A notable feature of the entry was its coverage of international issues. Friedman’s discussion treated the exchange-rate system in force in countries other than the United States as one of adjustable but pegged exchange rates, and the examples he cited of exchange-rate adjustment were from 1969.\textsuperscript{30} A lot had gone on between the entry being written and its 1974 publication.\textsuperscript{31} When he revised the entry for 1986 publication, Friedman was able to include a paragraph on the shift since 1971 from the “dollar standard” to a system of floating exchange rates.\textsuperscript{32} The latter stages of this shift are analyzed in Sections II and III of this chapter.

\textsuperscript{27} The quotation is from Friedman (1968a, p. 16).
\textsuperscript{28} Friedman and Schwartz (1973, p. 50).
\textsuperscript{29} See Chapter 5.
\textsuperscript{30} Friedman (1974f, pp. 351–352).
\textsuperscript{31} The most recent reference the entry cited was Friedman (1971a), and this citation may have been a late addition, as an earlier version of Friedman (1971a), namely Friedman (1970a), was cited on another page. See Friedman (1974f, pp. 355–356).
\textsuperscript{32} Friedman (1986, p. 325).
An article titled “A Bias in Current Measures of Economic Growth” in the March/April 1974 issue of the *Journal of Political Economy* proved to be the final article Friedman published in that journal over those years in which he was based at the University of Chicago. One prominent aspect of this article was its discussion of immigration, which Friedman numbered among the United States’ “great economic achievements.” That description was one of many discussions of immigration that appeared in Friedman’s writings and other public statements.

This is not what one might be inferred from the discussion of Friedman’s views on this matter in Ruger (2011, pp. 136–139). Ruger correctly notes that Friedman indicated over time that he favored free immigration in principle but not in conditions of a welfare state. However, Ruger’s earliest Friedman reference to immigration is from 1977, and Ruger implies that, other than in the same 1977 item, Friedman did not discuss the welfare state/migration linkage in public statements prior to 1998. In fact, however, Friedman discussed immigration on numerous occasions before 1977 and also gave multiple commentaries before 1998 on the immigration/welfare-state connection.

With regard to his support for immigration, there were many Friedman discussions that highlighted the benefits accrued to the United States from the free migration associated with pre-1914 arrangements—and the benefits to migrants of those arrangements. The 1974 *JPE* discussion was one example; another was his February 1973 *Playboy* interview, in which Friedman said of the period in question, “millions of penniless immigrants came in from abroad, with nothing but their hands, and enjoyed an enormous rise in their standard of living.” Ruger (2011, p. 136) claims that “immigration is basically ignored in his two major works on freedom” (that is, *Capitalism and Freedom* and *Free To Choose*). But in fact the Friedmans’ book version of *Free To Choose* opened with three paragraphs on the role of migrants in helping create the “economic miracle” of the United States. And in the television version of *Free To Choose*, Friedman, in the filmed portion of its first episode, likened modern-day immigrants to “the early settlers—they want to better their lot and are prepared to work hard to do so.”

33 Friedman (1974g, p. 432).
34 See Friedman (1975a, p. 8; 1983b, p. 19). Related Friedman remarks in, for example, Friedman (1976a, pp. 5–6; see also Friedman, 1983b, pp. 62–63), Friedman and Friedman (1980, p. 52), and Instructional Dynamics Economics Cassette Tapes 118 (April 13, 1973) and 214 (May 1977, Part 2). In addition, Friedman (1976b, pp. 162, 237) discussed pre-1914 immigration arrangements, in primarily a positive rather than a normative context, while Friedman (1962a, p. 96) had discussed how, in the nineteenth-century era of very high migration, public education likely was helpful, in promoting common values and a common language.
35 Friedman and Friedman (1980, p. 1).
In the same year as the broadcast of *Free To Choose*, Friedman noted: “I am in favor of freer migration as I am of freer trade.” However, a caveat to Friedman’s support for freer migration was that noted above: he regarded fully free migration as untenable in the modern world. The different welfare states of various nations were what made him stop short of advocating wholly free movement of individuals across countries. This was a fairly standard position of many libertarians. But it is not the case, as Ruger (2011) implies, that Friedman before 1998 hardly ever indicated that it was his own position. He articulated and endorsed the position on multiple occasions before 1998—not just in the 1977 talk that Ruger cites. In particular, Friedman explicitly did so in the 1980 *Free To Choose* television series.

Friedman would reaffirm this position on various occasions between 1980 and 1998. For example, in Hinshaw (1988, pp. 107–108), Friedman remarked: “Personally, I believe in free migration, but I believe that you can only have free migration effectively in a world in which you don’t have some other things that you do have in the present world; I mean that there’s a fundamental conflict between a welfare state and free migration. And so I have to choose a second-best at that point.” And in 1996, he stated: “In a welfare state you cannot have, unfortunately, free immigration. I’d like to have free immigration. I am the son of immigrants, of course... I don’t have any good way to answer this question... I can’t really come out and say that we should have completely free and open immigration.”

II. ISSUES RELATED TO DEBATES ON INTERNATIONAL ECONOMIC POLICY AND GEOPOLITICAL DEVELOPMENTS, 1973–1974

THE FIRST OIL SHOCK

Taping an economic commentary on October 4, 1973, Paul Samuelson proclaimed that the runup in commodity prices seen over the previous year had now gone into reverse. “Basic commodity

---

38 Ruger (2011, pp. 158, 198) cites the 1977 talk in question, “What Is America?,” as though it was unearthed by a 2008 blog. But, although not cited by Ruger as such, “What Is America?” was actually Episode 1 of *Milton Friedman Speaks*, a series of Friedman talks in the late 1970s put on limited videotape release in the early 1980s and available commercially in the twenty-first century as a DVD series. The talk in question was identified as being episode 1 of *Milton Friedman Speaks* by Friedman and Friedman (1998, p. 604) and was quoted and cited in Nelson (2009, p. 30). The relevant material on migration appeared early in the *Milton Friedman Speaks* episode in question (pp. 5–7 of transcript).
39 See *Free To Choose* (U.S. television version), PBS, debate portion of Episode 8, “Who Protects the Worker?,” March 1, 1980 (p. 8 of transcript). Reder (1982, pp. 30–31) also noted Friedman’s remarks on the matter in this debate.
40 CSPAN, April 18, 1996 (a talk by Friedman at Claremont College). See also Friedman’s remarks in *Forbes*, December 29, 1997, p. 55.
prices have in many cases declined substantially,” Samuelson remarked, and he conjectured that,
if a general index of these commodity prices existed, that index would register a peak during
August and a percent decline of about ten percent since then (Instructional Dynamics Economics

Just two days after Samuelson’s remarks, however, the die was cast for an event that would
tower over the commodity price increases observed in the first eight months of 1973. On
October 6, war broke out between Israel and an alliance of Egypt and Syria. Even in that day’s
newspapers, published before the war began, it was reported that the oil producers represented by
the Organization of Petroleum Exporting Countries (OPEC) were pushing for a 66 percent oil
price increase. Subsequently, however, as part of its response to the war, OPEC would preside
over the first oil shock—which would, in total, amount to a roughly fourfold increase in the

This shock set the oil price apart from other commodity prices. For, as Samuelson correctly
observed, by October 1973 certain commodity prices were retracing some of their increases from
the earlier year. Although they would show strength during 1974, prices for commodities other
than energy would record an absolute decline as the 1973–1975 recession proceeded. An
International Monetary Fund series on nonfuel commodity prices in industrial countries
registered an index value of 40.7 in 1972, 58.1 in 1973, and 75.8 in 1974, but a fall to 67.8 in
1975 (International Monetary Fund, 1986, p. 173). In contrast, the world oil price rose every
year, averaging $1.90 per barrel in 1972, $2.70 in 1973, $9.76 in 1974, and $10.72 in 1975
(International Monetary Fund, 1986, p. 171).

This behavior was testament to the fact that the oil price increase proved permanent—remaining
in force for years after the weeks-long 1973 Middle East war had ended. The second oil shock
of the late 1970s would then build on the permanent oil price increase of the first oil shock and
lead to an average oil price in 1980 of $28.67.

---

See the previous chapter.

A general index of commodity prices, the U.S. producer price index for all commodities (available in the Federal Reserve Bank of St. Louis’ FRED at https://fred.stlouisfed.org/series/PPIACO), registered an increase of only 4.2 percent in the year to August 1972 but an increase of 18.5 percent in the year to August 1973. Reflecting the softening to which Samuelson referred, the twelve-month rate of increase then fell to 15.4 percent in the year to November 1973, but, with the impetus of higher oil prices, it rose to 23.4 percent in the year to November 1974. The reported price is for Saudi Arabian oil. The spot price of West Texas Intermediate oil (available in the Federal Reserve Bank of St. Louis’ FRED portal at https://fred.stlouisfed.org/series/WTISPLC) shows a higher initial price as well as a less steep initial increase. Its annual averages are $3.56 in 1972, $3.87 in 1973, $10.37 in 1974, and $11.16 in 1975.
The 1973–1974 OPEC oil shock features very prominently in many narratives of U.S. macroeconomic developments in the first half of the 1970s. For Milton Friedman, however, the emphasis placed on the shock was largely misplaced. He did increasingly recognize the importance of the oil shock as a major event that altered the economy’s supply potential. But Friedman deplored the tendency for the OPEC shock to become a near-catch-all explanation for the deterioration in macroeconomic performance from the 1960s to the 1970s.

Friedman’s analysis of the oil shock can be partitioned into (i) his analysis of the origin of the shock, in particular the power and role of the cartel; (ii) what he regarded as the shock’s implications for output and potential output; (iii) his analysis of its effect on inflation; and (iv) his view on how U.S. energy policy responded to the shock. Items (i) to (iii) are analyzed in this section. Item (iv) is discussed in the discussion titled “William Nordhaus” in Section III below, as well as in later chapters.

Friedman’s analysis of the OPEC cartel and the origin of the oil shock

Increases in the price of energy and talk of an “energy crisis” had taken place over the course of 1973 even before OPEC’s October moves. Early in the third quarter of 1973, Friedman referred to what he called the “extraordinary bubble” that had developed in the oil price (Instructional Dynamics Economics Cassette Tape 125, July 18, 1973). The oil price increase that had been recorded was, he felt, of a piece with the rises in the prices of other commodities, including gold. In his view, these increases partly reflecting the world boom and other fundamental market conditions but considerably reinforced by “speculative fever” (Newsweek, September 17, 1973). However, even at this stage, Friedman did cite a role for OPEC in producing the increase. “At the moment,” he testified in June 1973, “the reason why the oil price is so high is because, with the assistance of unwise policies on the part of the United States, there has been formed an international selling cartel, the OPEC countries…”

The increases in energy prices that occurred before the fourth quarter of 1973 would, however, appear minor when judged against the steep rise imposed by OPEC in its actions from October 1973 onward. Friedman, like Samuelson, was caught off-guard by the increase: for example, in

---

44 For example, on May 17, 1973, the Hong Kong newspaper South China Morning Post editorialized that “fears of an energy crisis have made headlines in the American press for some months.” It added prophetically that “the world has up to now taken lightly the prospects of an oil famine on any major scale” and noted that “there is an increasing tendency among the Arab oil producers to exploit their stranglehold of the market as a political weapon against Israel and those countries which support her.”

June 1973 he had suggested that oil purchasers in Japan were being imprudent in making long-term contracts on the basis of the present oil price. In the event, of course, contracts that locked in pre-October prices proved highly beneficial to the purchasers.

The energy price increases associated with the first oil shock occurred against the background of continuing U.S. wage/price controls, including on retail gasoline prices. The controls did not prevent a sizable pass-through of the higher oil prices to prices at the pump. But a complete pass-through from world oil prices to U.S. retail prices was prevented by the general wage/price controls and then, in 1974–1981, by price controls applying specifically to retail gasoline prices.

The U.S. policy reaction to the higher oil price would generate trenchant criticism from Friedman for the rest of the 1970s. At the same time, however, his reaction to the OPEC measures helped establish his reputation as someone who downplayed the implications, in terms of both scale and duration, of the oil shock. The appropriate response, he said in November 1973, was to drop the price controls and “simply allow prices to rise… A 10 to 15 percent [energy] price rise will eliminate the shortage as a serious problem.” (Lima News, November 21, 1973.)

As the OPEC shock in fact led to an enduring manifold increase in energy prices, such proclamations appear wildly off-base with the benefit of hindsight. The late-1973 statements by Friedman are not, however, quite as indefensible as might be thought, because a distinction has to be made between the OPEC embargo and the OPEC price rise. The valid element of Friedman’s analysis was that the embargo—a sellers’ embargo, with OPEC ruling out export of its product to the United States and some other countries—that was superimposed on the oil price hike did not provide lasting obstacles to oil supplies in the United States. Oil could find its way to the United States not only from non-OPEC countries but also by rerouting of oil from countries (such as the United Kingdom) not subject to the embargo. Indeed, the embargo was lifted at the end of the first quarter of 1974 (Federal Reserve Bulletin, April 1974, p. 240), in part because OPEC recognized it had been ineffective in blocking oil supplies to the United States. Friedman was thus not wrong to suggest that a shortage—in the sense of unavailability of petroleum products to U.S. customers—might not be a major problem and might be eliminated by modest price adjustment.

---

46 Again, see Friedman’s testimony of June 21, 1973, in Joint Economic Committee (1973, p. 139).
47 Similar observations by Friedman appeared in Newsweek, November 19, 1973, and in Instructional Dynamics Economics Cassette Tape 134 (November 21, 1973).
But the short-lived nature of the OPEC embargo would only underscore the fact that a far more important aspect of OPEC’s measures was the price increase that the oil-exporting countries achieved via their restriction on amounts offered to the world market. It was on this price increase that Friedman’s record as a commentator and forecaster was truly checkered. He expected OPEC to crumble—and to do so quite quickly—and over the mid-1970s he accumulated a catalogue of failed predictions to this effect.

A sampling of predictions Friedman made in this vein in late 1973 through Spring 1975:

- In his June 1973 Congressional testimony, in response to the early OPEC price increases, Friedman said: “The cartel has fixed a price which is a multiple of the cost of producing oil in their countries. As a result, there is going to be tremendous pressure for that cartel to fall apart.” 48

- In November 1973, Friedman stated categorically (Instructional Dynamics Economics Cassette Tape 134, November 21, 1973): “I predict without fear of successful contradiction that within three years—no more—we’re going to have oil coming out of our ears, we’re going to have a collapse in the world price of oil, and I may say it couldn’t happen to a better bunch of people [OPEC].” 49

- In February 1974, he stated that the “oil price cannot stay at $10 a barrel; it will drop drastically within the next six or nine months” (University of Chicago Magazine, Autumn 1974, p. 12). 50

- In March 1974, Friedman said that—just as the surge of meat prices in early 1973 had now faded from the public’s memory—so in a year’s time the talk of the oil crisis would be a thing of the past. 51

---

49 Other statements from around this time in which Friedman also predicted oil coming “out of our ears” were in Philadelphia Evening Bulletin, November 9, 1973, and Chicago Tribune, November 21, 1973. The latter statement gave the timeframe over which OPEC would collapse as three to four years.
50 In Friedman, Teece, and Griffin (1982, p. 58), Friedman stated: “I was wrong about my prediction in 1974… [but] I did not predict imminent collapse but simply that the OPEC countries would not be able to maintain the cartel for long without specifying what ‘long’ was.” Although this characterization may be correct of Friedman’s 1974 Newsweek analysis, it does not apply to Friedman’s public remarks in 1973 and 1974 in toto. Although Friedman was not highly specific regarding the point in time at which the cartel would break down—in his January 1974 panel appearance (Long-Term Energy Solutions), for example, he remarked, “I don’t know when it’s going to fall apart”—his observations did specify a time-frame over which the cartel would break down.
51 From Friedman’s remarks of March 14, 1974 (Friedman, 1974k, p. 11).
• In his cassette commentary the following month (Instructional Dynamics Economics Cassette Tape 143, April 3, 1974), Friedman predicted that the approaching period would be one in which oil price “comes sharply down.”

• In September 1974 at the White House Economists’ Conference on Inflation, Friedman again predicted OPEC’s collapse (Council of Economic Advisers, 1974, p. 182).

• In an early-1975 column (Newsweek, February 17, 1975), Friedman predicted that President Ford would be able to include the collapse of OPEC among the favorable developments that had occurred in his tenure by the time he faced the November 1976 presidential election.52

• On Australian television in April 1975, Friedman reaffirmed that OPEC would break down—“we’re going to see that happen in the next year.” (Monday Conference, Australian Broadcasting Commission, April 14, 1975, p. 21 of transcript.)

• In Tokyo a week later, he predicted that this collapse would be accompanied by a fall in the oil price to below its real 1973 level (The Age (Melbourne), April 23, 1975).

And, even in early 1976, Friedman was maintaining that the “oil cartel will break pretty soon” (quoted in Feldberg, Jowell, and Mulholland, 1976, p. 46).

In mid-1979, with the original oil price increase of 1973–1974 in the process of being succeeded by a second oil shock, Paul Samuelson (Newsweek, July 2, 1979) obviously had Friedman in mind when he wrote: “It is only too easy to poke fun. Nor have economists failed to provide their own quota of laughs. Only remember those pundits who predicted the imminent collapse of the OPEC monopoly on the basis of no more than the syllogism that all past cartels have been mortal.” In light of remarks like Samuelson’s, the London Financial Times editorialized in late 1985 (December 14, 1985) that Friedman “has had to put up a great deal of ribbing from his academic colleagues over the years” about his predictions concerning OPEC. The ribbing extended beyond academia: in the mid-1970s, Friedman received a spoof award from the Promotion for Humor in International Affairs for his erroneous predictions concerning oil prices over the years.

52 Bruno and Sachs (1985, p. 195) cited this column and suggested that Friedman’s prediction was influenced by the “temporary and wholly reversed” non-oil commodity price boom of 1973–1975. However, as we have seen, Friedman’s predictions of OPEC’s collapse predated the winding-back of the non-oil commodity price boom—and, indeed, preceded the first oil shock period of October 1973 onward.
and the durability of OPEC (as he would recount in his columns in *Newsweek* of September 15, 1980, and March 21, 1983).

Friedman had indeed cited this property of cartels as one reason for his confidence that OPEC would collapse: “OPEC is a cartel, and cartels inevitably break up sooner or later,” Friedman had observed at the outset of the first oil shock (*The Evening Bulletin* (Philadelphia), November 9, 1973). On this occasion, he cited the source of the dissolution of a cartel the likelihood that there would be “a chiseler in the bunch” among the carter members. This sentiment echoed Friedman’s response to the oil price increases earlier in 1973: “There have been many international cartels… They last for a little while, but sooner or later there gets to be a chiseler, there is always one in every bunch.” Over a decade earlier, Friedman had also expressed this sentiment about cartels in *Capitalism and Freedom*.

But contrary to the characterization of them in Samuelson’s 1979 remarks, Friedman’s predictions had not been based wholly on a generalization about cartels but on what Friedman perceived to features of the world oil market. And for his initial reaction to the OPEC actions, Friedman had actually put heavy weight on the analysis of someone Samuelson acknowledged as an expert on the market: Samuelson’s Massachusetts Institute of Technology colleague Morris Adelman. Adelman’s analysis emphasized the absence of a physical shortage of oil supplies or reserves in the world, and Friedman relied heavily on this point. Adelman and Friedman also found fault with the United States policy actions for strengthening OPEC and making it an effective cartel. The overlaps and divergences of Friedman’s and Adelman’s analysis are discussed later in this chapter (in the course of the discussion titled “William Nordhaus” in Section III).

### Acknowledging a crisis

Over time, Friedman came to accept the gravity of the problem created by the first oil shock. This acknowledgment was exemplified by his acquiescence to the use of the terms “oil crisis” or “energy crisis.” In the first half of November 1973, Friedman tried to avoid referring to an

---

53 Likewise, on television three years later, Friedman observed, “That cartel has to break, like all other such cartels have broken in the past.” (*Wall Street Week*, Maryland Public Television, November 5, 1976, p. 15 of transcript.)


55 Friedman (1962a, p. 131).

“energy crisis”—maintaining that it was more accurate to refer to “shortages created by the [U.S.] government and exacerbated by the war in the Middle East” (*The Evening Bulletin* (Philadelphia) November 9, 1973). However, he used the term “fuel crisis” on a tape later in the month (Instructional Dynamics Economics Cassette Tape 134, November 21, 1973), while his *Newsweek* column of November 19 referred to the “current oil crisis” and his column of December 10 opened with a reference to “the energy and oil crisis.”

Nevertheless, Friedman’s continuing discomfort with the “crisis” label would be reflected in his remark (in *The Age*, April 23, 1975) that it would be a great mistake to view the world as facing a major energy crisis, as well as in his occasional subsequent indications that he regarded the “crisis” label as applying to the initial 1973–1974 disruptions, including the embargo, rather than describing a continuing state of affairs. But, as time went on, Friedman increasingly embraced both the terms “energy crisis” and “oil crisis.” For example, he titled a 1978 talk “The Energy Crisis,” using it to refer to the whole post-1973 period.

Later refinements and defenses of Friedman’s position

As his emphasis on the amleness of international oil supplies indicated, Friedman believed that a supplier restriction—that is, oil being withheld from sale on the world market—rather than problems of a global physical shortage of oil was what was behind the oil price increase. He would come to see the first oil shock of 1973–1974 as a milestone on the supply side of the oil market—marking the occasion when the OPEC cartel became effective for the first time. Friedman would trace the strength of OPEC in part to U.S. historical diplomatic decisions that had, as he saw it, ratified the creation of the cartel and strengthened its market power.

---

57 See Friedman (1977d, p. 17).
58 See, for example, Friedman (1975a, Chapter 15; 1977c, p. 463) for “oil crisis” and Friedman (1980, p. 56) (as well as *Newsweek*, July 14, 1975), for “energy crisis.”
60 Barsky and Kilian (2001) would later argue that the OPEC price increase was largely a validation of pressures on the demand side of the market. Friedman as sympathetic in other contexts, such as that of the labor market, to seeing apparent monopolistic power on prices as actually reflecting demand forces. But he did not see the first oil shock as produced by demand pressures. (He also discounted the role of aggregate demand in producing the oil price rise: he contended that the oil price rise was far too large in percentage terms for the bulk of it to be accounted for by monetary policy actions in the United States and other countries, and he declared attempts to do so statistically and economically unsound—see *Wall Street Journal*, August 28, 1978.) For a rebuttal to the Barsky-Kilian emphasis on demand for oil as a cause of the first oil shock, see Hamilton (2003).
61 See Friedman (1990, p. 51).
62 As well as the items cited above, see Friedman’s *Newsweek* columns of May 2, 1977, and June 18, 1979.
The resilience of the cartel and the associated energy-price increase would lead Friedman to modify and qualify his original position on the oil shock. Partially retracting his 1973–1975 predictions that OPEC would not have the discipline to restrict supply for a prolonged period (such as in The Age (Melbourne), April 23, 1975, and other items noted above), Friedman would grant that it had done exactly that. Indeed, by early 1975 Friedman was conceding that there would be some merit in imposing a tariff on imported oil to “hasten the disintegration of the OPEC cartel” (Newsweek, January 27, 1975), were it not for the fact that President Ford’s proposal for that tariff came as part of a package that included measures that would discourage domestic oil production (Newsweek, January 27, 1975; Instructional Dynamics Economics Cassette Tape 168, June 1975, Part 1).

For the most part, however, Friedman was willing to defend his initial economic analysis of the oil shock as sound (see Newsweek, September 15, 1980, and March 21, 1983). As his references in 1973 to a breakup occurring within three to four years attest, he did not see the pressures tending to undermine OPEC as acting instantaneously—although he was acknowledging by early 1975 that the forces were acting more slowly than he expected and that he had been wrong about the speed at which OPEC would collapse (Instructional Dynamics Economics Cassette Tape 165, February 1975, Part 3). Rather, he saw them as cumulating over time, in conformity with the property that short-run elasticities of demand and supply were much smaller than long-run elasticities. Thus, “the short-run demand curve facing the OPEC cartel was extremely inelastic,” but the long-run demand curve was not. In the case of the response to the oil shock, Friedman saw the forces making for the sending of appropriate price signals to both consumers and producers as having been greatly slowed down by highly counterproductive U.S. government policies regarding energy (see Section III below, as well as the following chapters).

Macroeconomic implications: the business cycle

In the mid-1980s, the Friedmans would cite the first oil shock as a factor that pushed up the unemployment rate. This interpretation of the oil shock was consistent with his original reaction in 1973, when he granted that the OPEC action increased the likelihood of recession (Instructional Dynamics Economics Cassette Tape 134, November 21, 1973). Subsequent evidence by Bernanke, Gertler, and Watson (1997, p. 121) supported Friedman’s notion that the

---

63 For example, in Friedman (1983d, p. 146).
64 See Friedman (1975a, p. 307) and Newsweek, March 31, 1975. See also Friedman (1990, p. 52).
65 The quotation is from the discussion in Friedman (1976b, p. 160), which referred to then-current 1975 events.
66 Friedman and Friedman (1985, p. 106). See also Friedman (1977c, p. 463).
recession of 1973–1975 likely would have occurred, due to tight monetary policy, even in the absence of the oil shock. Indeed, these authors suggested that the oil shock by itself would not have produced a recession had monetary policy not tightened in 1973–1974.\textsuperscript{67}

\textit{Macroeconomic implications: potential output}

On the economic-modeling front, the first oil shock concentrated the minds of economists in a way that made them bring variations in aggregate supply, or potential output, more routinely into modeling. Paul Samuelson (1974, p. 77) observed at the end of 1973 that “our Fisher-Keynes macro models do not tell us how to handle such a microeconomic restriction on supply and productivity.”\textsuperscript{68} The stage was therefore set for formal aggregate demand/aggregate supply analysis, as crystallized in macroeconomics texts late in the decade by Dornbusch and Fischer (1978) and Gordon (1978).

Ahead of this formalization, the oil shock was realized from an early stage as a force that reduced potential GDP. For example, testifying in early 1974, Chairman Arthur Burns, said that “our capacity to produce may actually decline in 1974, or at best rise at an abnormally low rate” and referred to the reduction in production capacity arising from “the sudden and very serious oil shortage.”\textsuperscript{69}

Notwithstanding this early reaction, official estimates of potential output in the mid-1970s were badly overestimated in the year. One problem was that the pre-oil shock, official estimates of the level of U.S. potential output were highly excessive, as sharp downward revisions in 1977 would make clear (Wonnacott and Wonnacott, 1979, p. 333). A further problem was that, although the 1973–1974 oil shock was recognized early on as a supply shock, it occurred roughly alongside a sharp slowdown in long-term productivity growth. So even estimates that incorporated major downward revisions of the estimated level of U.S. potential output soon implied serious overstatements of aggregate supply, because they missed the emerging decline in the growth rate of potential.

Friedman himself would note the supply-side effects of the oil shock: for example, his Nobel...
lecture included the observation that the oil crisis “directly disrupted the productive process.”

In his own quantifying of the importance of the oil shock for the behavior of potential output, Friedman was inclined to view it as a terms-of-trade shock—one that meant more output had to be shipped abroad rather than contribute to meeting U.S. aggregate demand. Accordingly, when he and Anna Schwartz constructed an estimate of the first oil shock on potential output, they arrived at a value of 1.5 percent, which corresponded to the extra share of U.S. aggregate output absorbed by the higher energy cost after the energy price increase.

In late 1973, before the extent of the oil price rise and the U.S. economy’s response to it were clear, Friedman had estimated the decline in potential output at about 0.4 percent. He was therefore appalled at what he viewed as a huge overreaction by the stock market to the oil shock. “The stock market has been saying a reduction of four-tenths of 1 percent [of GNP] over the next two or three years reduces the value of American enterprise by 10 percent,” Friedman observed. It makes no sense.” (The Evening Bulletin (Philadelphia), November 20, 1973, p. 22.)

Friedman’s assessment that the stock market had grossly overreacted to the oil shock remained even after his estimate of the decline in potential had been raised to 1.5 percent. Together with the behavior of the gold market, the stock market slump in the mid-1970s greatly enhanced his preexisting skepticism about the reliability of the links between economic fundamentals and the pricing of equities and commodities.

**Macroeconomic implications: the price level and inflation**

The 1.5 percent estimated decrease in potential output, as far as Friedman was concerned, also

---

70 Friedman (1977c, p. 463).
71 See, for example, Friedman and Friedman (1980, p. 263) and Instructional Dynamics Economics Cassette Tape 173 (August 1975, Part 1).
72 Friedman and Schwartz (1982, p. 104) did not source the 1.5 percent number, but Friedman mentioned a range of 1 to 1.5 percent in Instructional Dynamics Economics Cassette Tape 155 (October 10, 1974), and he described 1.5 percent as his estimate in Friedman (1977d, p. 17). The 1.5 percent estimate appeared also in other economists’ discussions of the OPEC shock in the mid-1970s. For example, at a March 1975 conference (as recorded in Hinshaw, 1977, p. 102), Wilson E. Schmidt (of Virginia Polytechnic) foreshadowed the Friedman-Schwartz position by stating: “A monetarist would say that there is only one real impact—namely, the adverse effect on the terms of trade. Between October 1973 and October 1974, the terms-of-trade cost for the industrial countries was only about 1.4 percent of real GNP, or something like that; the figure for the United States was 1.5 percent.” At the same conference, Robert Solomon considered related reasons for the 1.5 percent number (p. 102).
73 See also Friedman’s remarks on the excessive reaction of equity prices in Instructional Dynamics Economics Cassette Tape 135 (December 4, 1973) and Instructional Dynamics Economics Cassette Tape 137 (June 26, 1974). Friedman also discussed recent years’ equity price slump in Black Enterprise (June 1974), p. 114.
74 In retrospect, a major stock-market correction might seem justified by other factors occurring around this time, most notably the post-1973 productivity slowdown. However, it seems implausible to attribute to stock-market participants a sound understanding of this slowdown when policymakers and economic modelers only seem to have truly grasped its existence and scale at a later point—in the late 1970s and early 1980s.
answered the question of how much the OPEC shock raised the price level. By the time of the OPEC shock, he had already spent months refuting the idea that various commodity price increases, including those pertaining to meat, cereals, and anchovies, had played a large role in producing inflation. Friedman would reiterate these criticisms in December 1974, when he deplored blaming inflation on natural catastrophes (such as crop failures) that had lowered agricultural production. Indeed, his terms-of-trade-oriented view of commodity shocks’ macroeconomic implications led Friedman to suggest that increases in agricultural prices might be disinflationary for the United States beyond the short run: as the country was a net exporter of agricultural goods, agricultural price shocks meant the U.S. exports could pay for a higher volume of U.S. imports than previously (Instructional Dynamics Economic Cassette Tape 129, September 13, 1973; University of Chicago Round Table: The Nation’s Economy Out of Control, PBS, May 1, 1974).

The oil price increase fell into a somewhat different analytical category because, by 1973, the United States was a net oil importer. Therefore, the OPEC oil shock worsened the U.S. terms of trade and so, on Friedman’s logic, could make the country’s price level higher for a given stock of money. However, as already indicated, Friedman would rate the decline in potential output resulting from the oil shock as 1.5 percent: substantial, but not large in relation to ongoing inflation rates in the 1970s. Through this prism, it followed that the decline in potential output had tended to raise nominal money per unit of output by 1.5 percent and put up prices by the same amount. The oil shock in Friedman’s framework therefore had a permanent, but quite limited, long-run effect on the price level. This long-run effect had no implications beyond the very short run for the time derivative of the price level. As a corollary, the oil shock, Friedman

---

75 Friedman (1975b, p. 699).
76 Friedman also made this argument in a letter to Federal Reserve Chairman Burns on October 1, 1973 (Burns Papers, Ford Library). He made the further point in that letter that a country under a fixed exchange rate might not see its terms-of-trade improvement realized in a lower price level than otherwise, as that improvement might also be associated with a balance-of-payments surplus and so upward pressure on the country’s money stock. With floating rates newly prevalent, a terms-of-trade improvement was, he argued, now more likely to be a source of downward pressure, on net, on a country’s price level. The applicability of this analysis to the United States is limited by the insensitivity of U.S. monetary conditions to its balance-of-payments position even under fixed exchange rates; but the analysis is likely highly relevant for other countries. In this connection, it is notable that Gruen and Dwyer (1995) found that terms-of-trade improvements boosted inflation in Australia under fixed exchange rates, but not under floating.

77 An assessment based on the same principles was Karnosky (1976, p. 21). Upon finding that as the level of prices in 1974 exceeded the value implied by its estimated historical relationship with the money stock by 4.5 percent, Karnosky suggested that the decline in potential output for 1974 should be inferred as having been 4.5 percent. Karnosky’s high estimate of the potential-output decline might, however, have reflected a confounding of two nonmonetary influences on the price level: bona fide supply shocks (such as oil) that appreciably lowered potential output and relative-price shocks (food price shifts, for example) that influence the price level in the short run but may not have major ramifications for potential output (and, in Friedman’s analysis, might even have had a small positive effect on U.S. potential output).
would note on multiple occasions, “did not produce any longer-lasting effect on the rate of inflation.”

A passage on commodity shocks and inflation in a Friedman Newsweek column of June 1974 was much cited and quoted in later years. In this discussion, Friedman took issue with the notion that the energy shock meant an upward shift in the ongoing U.S. inflation rate. In challenging such cost-push interpretations, Friedman countered (Newsweek, June 24, 1974): “The special conditions that drove up the prices of oil and food required purchasers to spend more on them, leaving less to spend on other items. Did that not force other prices to go down or to rise less rapidly than otherwise? Why should the average level of all prices be affected significantly by changes in the prices of some things relative to others?” He immediately added: “Thanks to delays in adjustment, the rapid rises in oil and food prices may have temporarily raised the rate of inflation somewhat.” This qualification, however, did not undercut Friedman’s bottom line that, for given monetary growth, and taking potential output growth as unchanged by the oil shock, the oil shock could not raise the ongoing rate of inflation. Inflation remained a monetary phenomenon when oil shocks were part of the economic environment.

---

78 See Friedman and Friedman (1980, p. 263) and Friedman (1992, p. 204). This formulation left open the question of how to treat a situation in which a country’s terms of trade deteriorates but its volumes of exports and imports remain unchanged. Friedman (1974l, p. 14) himself seemed to recognize this ambiguity when he suggested that an increase in U.S. net exports might be a source of upward pressure on the price level, by reducing the goods available to U.S. residents for a given flow of U.S. aggregate nominal income. One clear-cut way of incorporating the idea that terms-of-trade shocks matter for the price level, without the reaction of net exports having much bearing on the matter, is to put imports into the production function as an intermediate good, in which case terms-of-trade shocks matter directly for the behavior of potential output.

79 See, for example, Argy (1985, p. 70) and Ball and Mankiw (1995, pp. 161–162). (In many of the citations over the past 25 years, the passage is slightly misquoted and the year and title of the column are given incorrectly.) Of course, this column, though providing a convenient capsulization of Friedman’s position, was one of many occasions on which Friedman stressed the notion that, for given monetary policy, a shock to one price tended to provoke downward pressure on other prices. For some examples, see Nelson (2018a) and the previous chapter.

80 This qualification indicates that the interpretation by Ball and Mankiw (1995, p. 162) of Friedman’s column as implicitly assuming a flexible-price view of the world is not appropriate. Indeed, although presented as a contrast to Friedman’s position on the oil price’s implications for inflation, their model can be regarded as a formalization of Friedman’s notion that the oil shock has a temporary influence on inflation. Note also that while Blinder and Rudd (2013, p. 153) take this qualification in the June 1974 Newsweek column as one that previous discussions of Friedman’s remarks neglect—and indeed state categorically, but quite incorrectly, that it “is never quoted”—the sentence in question was actually quoted in Humphrey’s (1976, p. 83) discussion of the column; this Humphrey article also appeared in the United States monetary literature (see Humphrey, 1977, p. 6; 1986, p. 22) and was included in the collection Federal Reserve Readings on Inflation (Federal Reserve Bank of New York, 1979). In addition, Nelson (2005) contained a quotation from and discussion of a passage in Friedman and Friedman (1985, pp. 83–84) that was essentially an elaboration of the points (including the qualifying sentence) expressed in the 1974 Friedman Newsweek column.

81 Among the occasions when Friedman stressed this point were his commentary in Instructional Dynamics Economics Cassette Tape 139 (February 4, 1974), in which he contrasted the situation in the “longer period, [when] these special factors ought to affect the relative prices of food and fuels,” in the short run, due to “inertia in other
This position contrasted with that held at the time by many academic economists and economic policymakers. The relative-price/aggregate-price distinction had, of course, been a key part of Friedman’s disputation with cost-push arguments over the decades, including in his exchange-rate essay in the 1950s.\footnote{See Friedman (1953, pp. 180–181).} It had also figured in his 1970–1972 arguments with Burns’ conversion to the cost-push view. However, that argument had largely pertained to wage-push explanations of inflation—and so it had centered on the endogeneity of wages and on the relationship between prices and costs, rather than on the relationship between particular prices and the general price level.\footnote{The rejection by exponents of the monetary view of inflation of both the wage-push and the special-factors perspectives on inflation was also emphasized around this time by Anna Schwartz (1973). She argued (p. 262) that “[n]o better response” to such perspectives had been given than by Irving Fisher (1911a), and she quoted him at length to this effect, as well as spotlighting (see Schwartz, 1973, p. 259) his 1911 debate with the University of Chicago’s James Laurence Laughlin on the matter (Laughlin, 1911; Fisher, 1911b). Friedman, after being alerted by Schwartz to this 1911 exchange, gave it prominence in Friedman (1972b). Much later, Friedman highlighted Laughlin’s cost-push views when he wrote the entry on Laughlin for the New Palgrave dictionary of economics (see Friedman, 1987c).} With the non-oil commodity shocks of early 1973, the last relationship became more prominent in the debate, and this became even more so with the unfolding of the OPEC oil shock from late 1973 onward.

After the OPEC shock, Burns—whose 1973–1974 public sparring with Friedman on the causes of inflation was discussed in the previous chapter—testified: “Rapidly rising prices of food and fuel, in fact, have accounted for a large part of our recent inflationary problem.”\footnote{From Burns’ February 26, 1974, testimony, in Joint Economic Committee (1974a, p. 720).} Burns, like many others, believed that the oil price shock was almost bound to leave a permanent mark on inflation due to the higher cost of living being embedded in wage contracts—a process that (according to cost-push views, with their emphasis on wage-price spirals) would lock in a higher rate of price inflation. This position underlay Burns’ remark that a “more fundamental factor affecting the course of inflation in 1974, however, may well be the course of wages and unit labor costs.”\footnote{From Burns’ February 26, 1974, testimony in Joint Economic Committee (1974a, p. 723).}

From the late 1970s, however, there emerged widespread acceptance of the monetary explanation for inflation in the economics profession and policymaking. This change in thinking led mainstream economic research and analysis in policymaking agencies to adopt frameworks consistent with Friedman’s position that oil shocks could not, in fact, give rise to ongoing inflation (absent monetary accommodation of the shocks—a topic discussed presently).

\footnotesize{prices,” the special factors led both to higher inflation and to a higher absolute price level than would prevail in a flexible-price environment.}
Nevertheless, the fact that oil-based explanations of inflation were so prevalent during the 1970s, combined with the continuing visceral appeal of accounts of inflation that refer to specific prices, has meant that even twenty-first century retrospectives on the 1970s inflation have often had to confront the energy-oriented explanations and explain their inferiority to monetary accounts. For example, in opening their classic paper, Clarida, Gali, and Gertler (2000, p. 474) felt obliged to make the point that “while jumps in the price of oil might help explain transitory periods of sharp increases in the general price level, it is not clear how they alone could explain persistent high inflation in the absence of an accommodating monetary policy.” This was a point already entrenched in textbook analysis (see, for example, Dornbusch and Fischer, 1994, pp. 234–235, 533–534), but the idea that higher oil prices were bound up with permanently higher inflation died hard.

The durability of oil-push ideas was also highlighted by Bernanke’s (2003b) remark, after he had articulated and endorsed the mainstream view that inflation in the 1970s was due to monetary forces: “You may have noticed that I have discussed the Great Inflation of the 1970s with an emphasis on Federal Reserve behavior but without mentioning oil prices.” He then spelled out why he had not mentioned them. That Bernanke included this explanation underlined the fact that, while the monetarist rejection of the cost-push view had gone from being a dissenting position in the mid-1970s to conventional wisdom among economists in later decades, even in the later period it still clashed with popular views of what drove inflation.

Appropriate monetary policy and the oil shock

Even among those in the mid-1970s who agreed that the monetary policy response was crucial in determining the implications of the oil shock for inflation, what monetary policy should do in response to the oil shock became an area of some discussion. Near-universal agreement, spanning Keynesians and monetarists, pertained to the notion that the authorities should not try to restore the pre-shock price level; that is, it should not reduce the money stock in step with the decline in potential output.86 However, some economists argued that the monetary growth rate might be temporarily increased, so that the relative-price adjustment set in motion by the oil shock could continue.

---

86 On the Keynesian side, James Tobin (according to Paul Samuelson in Financial Times (London), December 31, 1973) stressed the desirability of allowing the aggregate price level to rise on a one-time basis. Among others who endorsed this approach were John Flemming (as attributed to him by Samuel Brittan in Financial Times (London), September 9, 1976), Brittan (1983, p. 126), and Brunner and Meltzer (1993, p. 227). For a contrary position, in which it is suggested that “it would be undesirable to accommodate also the first-round effect on the price level,” see Svensson (2003, p. 3).
shock did not entail forcing down the absolute prices of non-energy-intensive products. Friedman saw a case for such a short-term adjustment of monetary growth at the level of principle. But he did not view the case as compelling, especially when judged against the need to contain inflation expectations in the face of the fillip that the initial price-level shock might give these expectations. Consequently, he remained in favor of a nonactivist monetary growth response.

An alternative policy to either a tightening or a temporary loosening of monetary policy in response to the oil shock was to raise monetary growth permanently in reaction to the oil shock. Despite the fact that it was not advanced by economists as the appropriate response, this scenario has become one of the most-discussed ones in the coverage of the monetary policy/oil shock/inflation nexus of the 1970s. One of the reasons for this focus is that monetary accommodation of oil shocks apparently squares the circle and reconciles oil-push and monetary explanations for inflation; as Friedman put it, an account in which “inflation is caused by the OPEC cartel may be correct if what is meant is that the OPEC cartel set in motion forces that induced” higher monetary growth.

Another reason for the focus on monetary accommodation lies in the fact that many observers seem to think that a change in the U.S. money stock trend in response to the first OPEC shock is what actually occurred. The fact that inflation’s surge and peak in the mid-1970s roughly coincided with the first oil shock may underlie this impression. The interpretation that monetary policy eased in response to the oil shocks became very widespread and appeared even in an early retrospective by Warren Nutter in 1976. It continued to be conventional wisdom thereafter, with an op-ed by Martin Feldstein in response to the 1990 oil price rise being titled “The Fed Should Not Accommodate Iraq” (Wall Street Journal, August 13, 1990), as though monetary accommodation of oil shocks had been the Federal Reserve’s past practice. Furthermore, the explanation for the Great Inflation offered by Chari, Christiano, and Eichenbaum (1998) was premised on the notion that the Federal Reserve raised monetary growth in response to the 1973–1974 oil shock.

87 For example, in April 1975 Michael Mussa noted (see Birnbaum and Laffer, 1976, p. 147) that “you might want to let the prices of those products that are going up in relative price to rise also in money prices, rather than trying to force down the nominal prices of other goods.” For a formalization of an argument of this kind, see Aoki (2003).
88 Friedman (1990, p. 16).
89 See Nutter’s testimony of February 17, 1976, in Committee on Finance, U.S. Senate (1976, p. 66).
These impressions run, however, against two absolutely fundamental aspects of Friedman’s account of the 1970s, which have been emphasized earlier in this chapter and which have both received support from subsequent empirical work.

The first aspect is a key fact of timing: the mid-1970s inflation in the United States did not reflect contemporaneous monetary ease but instead was largely a delayed reaction to the early 1970s monetary expansion. The U.S. monetary expansion of the early 1970s was so great and so closely related to subsequent inflation that it left comparatively little of the movement in the mean rate of inflation over the period to be attributed to the oil shock. The same was true of numerous other advanced countries, as shown in Darby and Lothian (1983) and Parkin (1980).

The second aspect of the actual 1970s record that deserves emphasis is that the Federal Reserve tightened monetary policy in 1973 and 1974—both before and after the oil shock. This pattern of behavior is at variance with narratives that argue that U.S. inflation and monetary policy developments can be understood in terms of policymaker accommodation of the first oil shock. The first oil shock was not, in fact, accommodated by monetary policy.

**INTO THE FLOATING-RATE ERA**

In his lecture in Yugoslavia on March 20, 1973, Friedman’s discussion of exchange-rate arrangements referred to “the new system that has emerged in the course of [the] past few weeks.” Over the previous seven weeks, the attempt to restore a version of the Bretton Woods system via the Smithsonian agreement had broken down. Canada and the United Kingdom already had floating exchange rates by 1973. On February 12, the yen was floated, while the U.S. dollar had another official devaluation, this time of 10 percent (Daily News (New York), February 13, 1973). Then on March 11–12, 1973, the dollar-centered fixed exchange rate system of the postwar period ended when six European countries started floating against the U.S. dollar (Daily News (New York), March 12, 1973; Dallas Morning News, March 13, 1973).

Friedman characterized the new system that had emerged as “‘dirty’ floating,” and he would continue to use that label when that system was still in force nearly thirteen years later (Wall Street Journal, December 18, 1985). But at the outset of the March 1973 breakdown of the organized fixed-rates system, he did not see widespread floating as likely to last, even in its

---

90 Friedman (1973d, p. 12).
“dirty” form (that is, featuring some attempts at exchange-rate management by central banks, which occasionally intervened in the foreign-exchange market to this end).

Instead, Friedman in the early months of floating reiterated his position that numerous countries would likely want to have balance-of-payments surpluses with the U.S. dollar. That is, he saw a continuing form of the “dollar standard,” in which accumulation of dollar exchange reserves was an aim of governments (Instructional Dynamics Economics Cassette Tape 120, May 11, 1973). Friedman noted that countries in the rest of the world might want to float or fix their exchange rate against the U.S. dollar. Either situation, he suggested, should be treated with equanimity: “If other countries want to peg [against] the dollar, that’s fine, it’s their business. If they want to float, that’s fine, it’s their business too.” What was important from the United States’ point of view, Friedman insisted, was that it should not have exchange controls nor make efforts to support other countries’ exchange-rate targets (Kansas City Star, March 15, 1973).

However, what emerged was actually far more like true floating; what management of the float occurred without restoration of widespread exchange-rate targets by the United States’ large trading partner for long periods against the U.S. dollar. In January 1974, Friedman noted that “one has to stress the fact that we are now in a regime of floating exchange rates, to all intents and purposes… And, obviously, that’s a highly desirable thing.”91 In 1986, while not completely discounting the possibility that the “dollar standard” label still applied to the modern world, Friedman acknowledged that “the world’s monetary system has consisted of a collection of national fiat currencies linked by ‘floating’ exchange rates set in the market.”92

Along with explicit foreign exchange intervention by central banks, there were a couple of other notable respects in which the early years of floating departed from Friedman’s prescription for the international monetary system. First, as discussed in Section III below, large-scale swap operations became and remained an important element of governments’ activities. Friedman disapproved of these operations, viewing them as a back-door attempt at exchange-rate management. Second, Whitman (1975, pp. 133–134) observed that in 1973–1974 several major countries engaged in a “form of indirect [foreign-exchange market] intervention by which countries may seek to avoid or reduce depreciation of their currencies,” namely, “by foreign (as opposed to domestic) borrowing by governmental or government-related authorities.” Although not opposed in principle to the notion of the public sector borrowing abroad, Friedman was

---

91 From Friedman’s remarks in the panel discussion on Long-Term Energy Crisis Solutions, held at the National Conference on Government Responses to the Energy Crisis, January 24, 1974.
92 Friedman (1986, p. 325).
opposed to such activity being carried out with the express intent of supporting the exchange rate. In a 1976 commentary, he acknowledged that government “borrowing abroad does of course tend to strengthen the currency,” but he indicated that this amounted to a situation in which the exchange rate was “artificially held up” (Instructional Dynamics Economics Cassette Tape 185, February 1976, Part 1).

Friedman’s role in bringing about the exchange-rate system

The fact that the United States and other major countries adopted floating exchange rates in the early 1970s raises the question of whether this came about to a significant degree because of the influence of Friedman’s advocacy, as reflected in his research and other statements. His 1953 paper remained his principal contribution on this matter, but he had consistently been a vocal advocate of floating rates in articles, testimony, and other public statements over the subsequent two decades.93

It was not in doubt that Friedman had had an important role in changing opinion among U.S. economists, particularly in academia, on floating. At the time of earlier problems with the Bretton Woods system, Paul Samuelson had been quoted saying (Chicago Daily News, March 28, 1968): “If you took a poll of experts in international economics, you would find 80 percent of them favoring floating exchange rates as a result of Friedman’s influence.”94 In the early 1970s, there were still some prominent U.S. economists in favor of a fixed-rate system—including Charles Kindleberger and Robert Mundell—but Friedman had the majority of U.S. academics interested on the matter on his side of the argument.

However, recognition that Friedman had prevailed in economists’ debate does not mean that the floating era came about because of the influence of his ideas. The general float occurred after the collapse of the 1971–1973 repairs to the Bretton Woods system. It did not arise out of a conscious embrace of floating on the part of policymakers; rather, these policymakers initially presented floating as a temporary measure (though some were privately enthusiastic about floating indefinitely—including George Shultz, as discussed in Section III below). The fact that floating was a default move rather than a concerted policy action in 1974 led Brunner and Meltzer (1976, p. 2) to observe: “The case against fixed exchange rates by Friedman [in his 1953

93 See Nelson (2009a, 2017, 2018b) for accounts and analysis of these post-1953 statements. The question of whether Friedman himself had a personal role in facilitating floating, via his various direct interactions with U.S. policymakers in the early 1970s, is deferred until the discussion of George Shultz in Section III below.
94 Friedman gave a similar estimate of the balance of economists’ opinion in Friedman and Roosa (1967, p. 133).
paper] was not successful for many years and seems to be more of a triumph for events over policymakers than for economists over events.” Later, Paul Craig Roberts (in *Wall Street Journal*, December 18, 1985) cast even greater doubt on the importance of Friedman’s views on floating when he confronted the notion that “the move to flexible exchange rates was a conscious policy decision, based on the power of contending ideas… It was no such thing.” Instead, Roberts suggested, “inflation forced us off the fixed-rate system.”

Friedman’s position on why floating became prevalent was, however, more nuanced than Roberts’.95 He acknowledged that policymakers had accepted a floating-rate system because fixed rates had broken down, rather than as a result of a systematic, orderly deliberation on the merits of fixed-rate versus floating-rate systems. But even in the early years of floating, Friedman allowed for considerable influence of the academic support for floating on how events had proceeded. In May 1974, he cited floating exchange rates as an example of a policy option whose viability had been strengthened by the fact that research and other writings had kept it going as an idea.96 In that discussion, he noted that his case for floating rates had been an argument against relying on exchange controls instead of floating as a means of securing monetary policy autonomy. Why, he asked, had those advocating the restoration of the fixed-rate system not advanced much stricter foreign exchange controls as a key part of their proposed new system? He suggested that this was because the distortionary effects of foreign exchange controls were now widely accepted (Instructional Dynamics Economics Cassette Tape 146, May 20, 1974). But, of course, this acceptance made a return to fixed rates less likely, as policymakers were loath to surrender national monetary autonomy.

Furthermore, Friedman declared in 1975 that the advent of floating had refuted “the arguments I used to hear 10 and 15 years ago about how it was utterly academic dreaming to suppose you could have a system of flexible exchange rates in the world. The truth is that it is the unrealistic academic who can look at the situation in its broadest context—and get away from the immediate policy position—who is the most realistic.”97

As the years passed and floating became the permanent system in many parts of the world, Friedman believed that the *retention* of floating rates after 1973 did reflect importantly the force

---

95 See Nelson (2009, 2017) for further discussion and documentation of Friedman’s position on this matter.
96 Although Burgin (2012, p. 221) relies heavily on Friedman and Friedman (1998) to document the point that Friedman saw this role for his academic work on floating rates, Friedman had actually frequently articulated this view in his earlier discussions of the shift to floating, including the 1974 and 1975 discussions cited in the text of this chapter and in Friedman (1984a, p. 52).
of the intellectual case for floating. The most important argument both in his 1953 article and in the post-1973 debate was that floating provided monetary policy autonomy and fixed rates (absent exchange controls) did not, and that argument proved decisive in the widespread acceptance of floating in many countries (including, by the mid-1990s, the United States, the United Kingdom, Canada, Australia, and New Zealand).

The decisiveness of the monetary-autonomy argument in promoting endurable support for floating exchange rates undermines suggestions (one reflected, for example, in the title of Leeson, 2003) that the prevalence of floating was a victory of free-market “ideology.” The case for floating rates cut across ideological divisions. Paul Samuelson’s support, discussed above, for flexible rates attests to this reality. So does the fact that as an economic adviser in the United Kingdom in the mid-1960s, hardline Keynesian Nicholas Kaldor had urged greater flexibility for the sterling exchange rate (for example, Dell, 1990, p. 315). The upshot was that, as Krugman (1989, p. 71) observed, “advocates of flexible rates may be traditional monetarists who want freedom to target their favorite aggregates or Keynesians who want to pursue activist stabilization policy.” And as discussed in the previous volume, Friedman stressed that the U.S. Republican party’s thinking was split on fixed versus floating rates, and its traditional inclination was to support a fixed-rates system.

The United States’ system of exchange controls (including such measures as the interest equalization tax) was removed in 1973–1974. This process started with an announcement in conjunction with the February 1973 dollar devaluation. At that time, Paul Samuelson remarked that he was “sure that the ideological antipathy towards those controls was an important part of the motivation” for abolishing them (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 120, February 22, 1973). Samuelson’s reasoning apparently was that abolition of exchange controls made devaluation more palatable to free-marketeers who were also fixed-rate advocates. Free-market sympathies well have been part of the motivation for the removal of controls. However, once the United States and other countries were no longer floating, the continuing relaxation of foreign exchange controls had a sound, non-ideological, rationale. As indicated above, Friedman had long argued that because countries were unwilling to subordinate macroeconomic management to exchange-rate or balance-of-payments

---

98 Similarly, Burgin (2012, p. 204) states that Friedman “abetted the unfolding collapse of the Bretton Woods system,” a characterization that (by the choice of the word “abetted”) implies that Friedman behaved reprehensively, as well as ideologically, in advocating floating rates.

99 A qualification is that in the 1970s and 1980s some Keynesian advocates of flexible rates, such as Kaldor and to some extent James Tobin, became more sympathetic to a return to forms of exchange-rate management.
considerations, adoption of fixed exchange rates in the modern era meant that one would institute exchange controls. A corollary was that with floating rates, one did not need exchange controls.

In Congressional testimony on June 26, 1973, Paul Volcker, Under Secretary of the Treasury for Monetary Affairs, endorsed the exchange-rate/exchange-controls link that Friedman had stressed when he observed: “we are certainly aiming at a [global] monetary system which is not dependent upon the widespread use of controls. We do want to get rid of the foreign direct investment controls. That is a major objective. And we are certainly willing to accept an implication that you need more flexible exchange rates as part of the bargain.”

The motivation for exchange controls in the pre-1973 system differed somewhat across the United States and other countries. Outside the United States, exchange controls helped deliver monetary policy autonomy alongside fixed exchange rates. In the United States, as Friedman stressed (see the previous volume and the discussion below), the authorities had been able to achieve monetary policy autonomy even when committed to an exchange-rate system, as it was straightforward for them to offset the effect of U.S. balance of payments deficits on American monetary conditions. But exchange controls did give U.S. policymakers a tool with which they could make the deficit lower than it would otherwise be.

The role of gold

A United Press International news report on official plans to return to a fixed exchange-rate system was headlined “IMF Experts OK Plan To Dump Gold Standard” (Detroit Free Press, January 16, 1974). The article therefore accepted the false premise that the Bretton Woods system had been a Gold Standard. As Friedman reiterated at a November 1974 University of Miami conference on the role of gold, the United States had not had gold standard under Bretton Woods or “anything even approaching” a true gold standard since the Federal Reserve was formed in 1914 (see Manne and Miller, 1975, p. 148).

Indeed, over time Friedman would see Bretton Woods as having been dissimilar to the Gold Standard for other countries as well. During the Bretton Woods era, he and others had largely accepted the narrative that non-U.S. members of the system did not possess a sufficiently broad exchange-control apparatus to secure appreciable monetary autonomy. Consequently, as far as monetary conditions in their domestic economies were concerned, member countries other than

---

100 In Joint Economic Committee (1973, p. 160).
the United States were portrayed as being beholden to U.S. monetary policy.\(^{101}\) Many commentaries on U.K economic policy, in particular, took for granted that the United Kingdom, at least until the 1967 sterling devaluation, had subordinated macroeconomic policy to maintenance of the exchange rate. For example, Marina Whitman, member of President Nixon’s Council of Economic Advisers, said on October 27, 1972 that the United Kingdom had “subordinate[d] its domestic economic goals to balance of payments considerations… throughout much of the period since World War II.”\(^{102}\)

Friedman himself subscribed to this interpretation at the time: for example, in September 1970 he remarked that the Bretton Woods system made the United Kingdom “a monetary satellite of Washington” (in *Financial Times* (London), September 18, 1970). But further study brought home to Friedman how much the U.K. authorities had been able to insulate U.K. monetary conditions, and in 1982 he and Anna Schwartz would refer to “obvious primacy of domestic considerations in the formulation of government economic policy in essentially all countries” after the Second World War.\(^{103}\) Correspondingly, in the early 2000s Friedman concurred that the United Kingdom had sterilized its balance-of-payments deficits during the Bretton Woods era.\(^{104}\)

That said, some countries clearly did ultimately find that the pre-1973 arrangements compromised their ability to carry out domestic stabilization objectives. Notably, the 1973 floats of the yen and the Swiss franc took place after both countries found that the many controls that each imposed on foreign investment were unable to withstand the vast capital inflows they were receiving.

It was, however, clear that the Bretton Woods system had, for the most part, not resembled the Gold Standard, especially for deficit countries. Nevertheless, the U.S. government had had had some form of commitments involving the gold market over most of the postwar period to date.\(^{105}\) During much of that same period, Friedman had been a critic of the role of commodity prices in monetary arrangements. Indeed, he noted in May 1973 (*Instructional Dynamics Economics*

\(^{101}\) This would remain a standard narrative. For example, Obstfeld and Rogoff (1996, p. 567) characterized the Bretton Woods system as one in which the “United States [was] setting the system’s monetary policy.”


\(^{103}\) Friedman and Schwartz (1982, p. 572).


\(^{105}\) Consequently, Friedman was willing to describe August 1971 as being “when the dollar was first cut loose from gold (*Newsweek*, January 30, 1978), even though he did not believe that the gold commitment had meaningfully constrained U.S. monetary policy.
Cassette Tape 121, May 24, 1973) that in an article in the early 1960s he had urged that the U.S. authorities abandon their activities in the gold market.\textsuperscript{106} He went on to observe that his opposition to commodity currencies predated the 1960s. Indeed, in his 1951 paper on “Commodity Reserve Currency,” Friedman had stated: “There is no reason to waste resources in piling up monetary stocks instead of adopting the essentially costless alternative of a fiat standard.”\textsuperscript{107} The U.S. authorities, he observed, had partly exited activities in the gold market simply by embracing selling arrangements that meant off-loading its gold at what became low prices—that is, $35 per ounce until August 1971 and $38 in 1972–1973 (Instructional Dynamics Economics Cassette Tape 121, May 24, 1973).\textsuperscript{108}

But the United States from 1973 onward was involved, initially through Under Secretary Volcker, in trying to work out a viable fixed exchange-rate system to displace the new floating-rate system, and this raised the question of whether it would involve gold in some way, even in a peripheral role like that gold played during the Bretton Woods era. Laffer (1982, p. 164) would later contend that Volcker was sympathetic to an exchange-rate system involving the convertibility of the U.S. dollar into gold. However, Volcker’s own statements both in and out of government office provide little support for this interpretation. In particular, in his June 1973 testimony he stated: “We believe the system of the future should not be dependent on gold—gold-based, in the sense that even the Bretton Woods system was.”\textsuperscript{109} As this statement indicates and as discussed further in Section III, Volcker was indeed interested in moving back to an organized fixed-rate system. But he was opposed to restoring even a minimal role for gold.

The post-1973 framework meant that, even with the longevity of the floating-rate system unresolved, gold was for practical purposes gold gone for good from U.S. and international monetary arrangements. The January 1974 press report mentioned above was a sign of this process, as it referred to a proposal that gold not be among the media used for official international-payments settlements in a proposed follow-on fixed exchange-rate system. Another sign was a change in U.S. law that allowed U.S. households to possess gold in appreciable quantities after December 31, 1974. The ban on gold ownership had been one of the means by which the United States implemented the peg of the gold price (abandoned in 1971). At the November 1974 conference on gold, Friedman observed that “it is perfectly clear that

\begin{footnotesize}
\begin{enumerate}
\item See Friedman (1961).
\item Friedman (1951a, p. 232).
\item The behavior of the world gold price after 1973 is considered further in Chapter 5.
\item Joint Economic Committee (1973, p. 159).
\end{enumerate}
\end{footnotesize}
once we permit private ownership of gold, there will be another nail in the coffin of an official monetary gold standard.“¹¹⁰

Verdicts on Friedman’s case for floating: two misconceptions

Almost as soon as floating rates became widespread, the results they were producing were said to contradict Friedman’s predictions about how flexible exchange rates would work. Ahead of discussing some of these critiques, the ground needs to be cleared by considering two arguments against Friedman’s case for floating rates that are invalid because they attribute to Friedman views that he did not hold.

First, imbalances in both the trade account and current account generally continued to be nonzero under floating rates, and this state of affairs was said to be inconsistent with Friedman’s promises regarding what floating rates would bring. Laffer (1973, p. 28) suggested of Friedman’s framework: “Trade must, however be balanced because of flexible rates…” Similarly, Gittins (1988, p. 126) contended that “the advantages of floating which had been promised by leading monetarists such as Milton Friedman” included that “current account imbalances… would disappear with floating rates.”

These critiques are invalid, as Friedman (accurately) suggested only that the overall balance of payments (the sum of the imbalances in the current and capital accounts) would be zero under floating rates—not that the individual account categories would be in balance. He believed that exchange-rate depreciations typically lower trade and current account deficits. But he did not claim that a floating rate would eliminate the current account deficit. Indeed, as discussed in later chapters and in Nelson (2017), Friedman regarded a nonzero current account balance as a normal state of affairs, and a deficit in the current account as desirable in many circumstances. Early in the floating-rate era, Friedman had occasion to touch on the matter (Instructional Dynamics Economic Cassette Tape 120, May 11, 1973), observing that if “we are able to import more than we export,” that was “not a problem. Quite the contrary. If foreign countries like Japan are willing to send us goods and services and take back… interest-bearing IOUs, we have no [cause for] complaint, they’re doing us a favor, they’re hurting themselves and helping us.”

Second, and remarkably, some economists have implicitly or explicitly grouped Friedman with those who advocated floating rates as a means of exploiting a permanently downward-sloping

¹¹⁰ Manne and Miller (1975, p. 167).
Phillips curve. For example, Argy (1992, p. 154), shortly after citing Friedman’s 1953 paper, attributed to floating rates’ proponents the position that “exchange rates allowed countries to choose their own combinations of inflation and unemployment.”

This attribution flies in the face, of course, of Friedman’s longstanding denial of a long-run inflation/unemployment tradeoff. It was not Friedman who tried to motivate floating by the alleged opportunities it offered to choose a permanent inflation/unemployment combination, but Harry Johnson. In the London *Times* (December 9, 1968), Johnson suggested that floating gave governments “the right to decide what combination of employment and inflation they prefer” and to achieve that combination, and he elaborated upon this argument in Johnson (1969). This argument, though intended to be an improvement and refinement of Friedman’s case for floating, would be discredited by the natural rate hypothesis. But Friedman’s case for floating emphatically did not appeal to that argument. Although, as late as 1974, some analysts were treating proponents of floating as arguing that it allowed a country to choose the average amount value of the unemployment rate in their economy (see Corden, 1974, p. 140), Friedman was insistent that central banks could not determine the unemployment rate in the long run under any exchange-rate regime.

*Early verdicts on floating rates*

Several prominent proceedings were held in 1973 and 1974 that tried to make verdicts on the floating-rate system. These included the June 1973 Congressional hearings on “How Well Are Fluctuating Exchange Rates Working?” (Joint Economic Committee, 1973), which received testimony from Friedman, Volcker, and numerous others; a December 1973 Federal Reserve Bank of Chicago workshop on international inflation that Friedman attended; and a May 1974 conference, held in Williamsburg, Virginia, titled “What Have We Learned from a Year of Greater Flexibility of Exchange Rates?” (see McKinnon, 1976, p. 113). No proceedings volume emerged from the May 1974 conference, but Friedman was an attendee (Instructional Dynamics Economics Cassette Tape 146, May 20, 1974).

In retrospect, these seem to have been absurdly premature occasions on which to make a judgment about floating rates. Alongside the obvious fact that the system had not been operating

---

111 He denied this tradeoff not only in the 1960s and later, but also at the time the Friedman (1953) article was written. See Nelson (2009, 2019b).


113 See, for example, Niehans (1984, p. 290) and Nelson (2009, p. 70; 2019b) for further discussion.
for long, there are two other compelling reasons for doubting that developments through May 1974 shed much light on floating. First, it was not yet clear that this was not simply a transitional arrangement before a follow-on fixed exchange-rate system—continuously under discussion in these years—was agreed upon. Indeed, Hansen and Hodrick (1983, pp. 120, 122) suggest that it was not until February 1976, when officials from various countries ratified floating, that it was clear that floating was the permanent new system. Their date seems too late—it will be argued in Chapter 6 below that by mid-1975 floating had received \textit{de facto} acceptance as the permanent system—but their point is valid with regard to 1973 and 1974. Second, the first real test of the floating-rate system—the impact of the first oil shock—did not crystallize into its final form until after March 1974, when it became a price increase applied to all countries instead of a price increase applied to some countries and a sellers’ boycott applied to other countries.

Friedman’s early evaluations of the post-Bretton Woods float were similar to his later ones. In his June 1973 Congressional testimony, his assessment was that the “past few months are a remarkable demonstration of the virtues of floating exchange rates,” with Friedman citing the fact that the foreign exchange market and international trade had proceeded in an orderly manner notwithstanding a background of surging commodity prices.\textsuperscript{114} At the December 1973 conference referred to above, Friedman remarked: “I think the floating rate system is fine.”\textsuperscript{115} In March 1974, describing the reaction of the foreign exchange market to the Middle East war, Friedman said that flexible exchange rates had worked the way he had predicted.\textsuperscript{116}

In contrast, Charles Kindleberger—in a paper for the November 1974 Carnegie-Rochester Conference, another proceeding devoted to the new floating-rate environment—claimed that the flexible-rate experience had confounded a prediction Friedman’s had made concerning exchange rates: specifically, that speculation in the foreign exchange market was stabilizing (Kindleberger, 1976, p. 62). However, the criterion that Kindleberger used to make that judgment was narrow: for him, the fact that exchange-rate variability had gone up since the float was sufficient.\textsuperscript{117}

\textsuperscript{114} From Friedman’s testimony of June 21, 1973, in Joint Economic Committee (1973, pp. 116–117; quotation from page 116).
\textsuperscript{115} Friedman (1974l, p. 16).
\textsuperscript{116} Friedman (1974k, p. 2)
\textsuperscript{117} Later, Fischer (1988a, p. 114) contended that the argument that “speculation would ensure substantial exchange rate stability” had been contradicted by the fact that the “the floating-exchange-rate system has seen major movements in exchange rates that have later been reversed.” However, more recently, research (such as Chari, Kehoe, and McGrattan, 2002) using optimizing business cycle models has found that these models could produce exchange-rate movements that were persistent but reversed. It is therefore not clear that the specific regularity concerning exchange rates that Fischer cited is in itself actually inconsistent with speculation being a stabilizing force.
Friedman, in his June 1973 testimony, cited instead the criterion of whether private speculation moved the exchange market toward an economically-justified value: “private speculators will have a strong incentive to step in and correct the obviously wrong exchange rate.”118 This position was itself sensitive to judgments about what exchange rate was justified by fundamentals. However, at the May 1974 conference mentioned earlier, Friedman did concede that stabilizing speculation had not yet emerged as a clear feature of the floating experience so far, and he expressed the hope that financial markets would develop instruments that helped promote this feature (Instructional Dynamics Economics Cassette Tape 146, May 20, 1974).

A more clear-cut matter on which Friedman’s predictions about floating were confounded pertained to capital flows. “It’s only with fixed exchange rates that you have these large short-term capital flows,” Friedman had declared late in the Bretton Woods period (Instructional Dynamics Economics Cassette Tape 74, May 20, 1971). That declaration was comprehensively refuted by the experience with floating, both in 1973–1974 and in later years.

But one could question Friedman’s position on speculation and reject his prediction concerning capital flows without denying the validity of the central, and for most observers, clinching argument in favor of floating: that it conferred monetary policy autonomy. Indeed, this argument’s strength was reinforced as, over the years, economists and policymakers came to accept his monetary diagnosis of the inflation process. In 1974, the International Monetary Fund’s Marcus Fleming had claimed that the Bretton Woods system broke down in part because “cost-push factors were becoming more important relative to demand-pull factors in the causation of inflation” (Fleming, 1974, p. 86). If this argument was valid, then inflation had become predominantly a nonmonetary phenomenon, and floating rates offered little extra scope to a country’s authorities for controlling inflation. In contrast, if monetary policy was decisive for inflation and cost-push forces only an ephemeral influence on inflation, then the authorities’ ability to steer monetary conditions in their country delivered the necessary and sufficient conditions for achieving price stability.

As already noted, the monetary-autonomy argument was of far more relevance to other countries than to the United States, which had monetary sovereignty even in pre-1973 conditions. Federal Reserve Chairman Burns said as much at the dawn of the floating-rate era when he remarked: “What happens to the discount rate happens [in] Washington and not anywhere else.” (Daily News (New York), March 17, 1973.)

At the start of 1972, Friedman was very much a critic of the settings of U.S. economic policy. To his great disapproval, the Nixon Administration had embraced fiscal expansion over the past year, instituted wage/price controls in August 1971, and revived fixed exchange rates via the Smithsonian agreement the following December. Friedman was also extremely unhappy with U.S. internal monetary developments, as reflected in critical correspondence (and a cooling of his personal relations) with Federal Reserve Chairman Arthur Burns and in a long line of public statements criticizing FOMC decisions under Burns, the latest being his column “Irresponsible Monetary Policy” (Newsweek, January 10, 1972).

Against this background, a Cabinet member in the Nixon Administration—George Shultz, then Director of the Office of Management and Budget—offered a strong public defense of Friedman. Friedman, Shultz said, had “one of the most extraordinary minds” that Shultz had come across and was, in addition, “a fellow of absolute integrity.” Shultz cautioned that as Friedman had studied monetary relations more “than perhaps anyone else in the world,” it would be an error just to categorize Friedman’s current dissent from U.S. economic policy as reflecting the viewpoint “of some nut” (Wall Street Journal, January 7, 1972).

This defense underlined the point that, even when Friedman was at odds with the Nixon Administration and the Federal Reserve, he maintained strong relations with his former University of Chicago colleague Shultz, who would become U.S. Secretary of the Treasury in June 1972.

Over the course of 1972 and into 1973, despite his continuing disagreement with the settings of U.S. economic policy, Friedman had a resumption of generally good relations with the Federal Reserve, which he praised in late 1972, and the Nixon Administration, whose reelection he supported. This rapprochement was, as discussed in the previous chapter, then shattered by the take-off in inflation in 1973 and the administration’s continuation of price controls.
It was during this time that Friedman, in a letter to Shultz of May 24, 1973, suggested that Shultz leave the administration. In the letter, Friedman expressed much the same sentiments that he made in his public commentary at the time (Instructional Dynamics Economics Cassette Tape 121, May 24, 1973) that the deterioration in U.S. inflation alongside the Watergate scandal would put pressure on President Nixon to tighten wage/price controls and increase public-sector intervention in the economy. In this commentary, he cited Shultz as a voice against wage/price controls but pointed to former Treasury Secretary John Connally as a contrary influence on the administration. In Nixon’s second-term lineup, Shultz had been given enhanced powers with an office in the White House on top of his Treasury position (Rochester Post-Bulletin (Minnesota), December 1, 1972), but Connally had returned to the administration as a counselor to President Nixon (Nixon, 1978, p. 908). Shultz did not follow Friedman’s suggestion; he remained Secretary of the Treasury throughout 1973.

Friedman’s fears in May 1973 that Shultz’s role in the administration would be marginalized initially seemed to be confirmed: although Shultz had said in that month that Watergate was not putting U.S. economic policy off-course (Evening Star and Daily News (Washington, D.C.), May 16, 1973), in June Nixon imposed “Freeze II,” discussed in the previous chapter. However, in subsequent months, though the economic news got still worse, the administration’s economic policy posture became more amenable to Shultz and Friedman in important respects. In the second half of the year (by which time Connally had left the administration), Shultz publicly expressed his unhappiness with the decision to impose Freeze II, while expressing optimism that wage/price controls would be completely gone by mid-1974 (Washington Star-News, October 28, 1973). This came to pass: Shultz departed from the Treasury on May 8, 1974; wage/price controls had been abolished a little over a week earlier.

As well as sharing an aversion to wage/price controls, Friedman and Shultz had similar instincts in other areas of domestic macroeconomic policy. Consistent with this shared outlook, Shultz did not voice dissent in February 1974 when a Congressional questioner described him as “George Shultz from the Friedman school of the economic restraint and the University of Chicago.” However, as discussed in the previous volume (Nelson, 2018b, Chapter 15), Shultz was not a monetarist, notwithstanding his sympathy with much of Friedman’s economic analysis. Shultz himself had noted in his January 1972 observations that he had “always described myself

119 The correspondence is in the Friedman papers, Hoover Institution archives.
120 From Senator William Proxmire’s remarks during Shultz’s appearance at a hearing of February 8, 1974, in Joint Economic Committee (1974d, p. 102).
as a friend of Milton Friedman—but not as a Friedmaniac” (Wall Street Journal, January 7, 1972).

One difference between them was on fiscal policy. Friedman frequently criticized Nixon Administration personnel (for example, in Instructional Dynamics Economics Cassette Tape 114, January 31, 1973) for believing that fiscal policy was important in its own right for the behavior of aggregate nominal demand, and not solely via its implications for monetary growth. Furthermore, Shultz believed in discretionary fiscal policy measures to stabilize aggregate demand, telling a hearing in early 1974 that the budget deficit in Nixon’s budget proposal “will provide a measure of support for the economy.” And, as discussed in the previous chapter, Shultz’s tenure at the Treasury saw very large increases in federal spending, although Shultz had limited influence on this total.

On inflation, too, Shultz agreed only partly with Friedman’s monetary diagnosis and was far more inclined than Friedman was to attribute the inflation occurring during Shultz’s years at the Treasury to nonmonetary forces. Friedman had warned Shultz (for example, in a letter of August 5, 1973) warning against overstating the role of special factors in 1973’s high inflation rate. Despite this injunction, in February 1974 Shultz testified that “what the world experienced last year was essentially a commodity price inflation.” He added: “Food prices alone may have been responsible for half the increases in consumer prices in the major industrial countries, and[.] toward the end of the year particularly, energy prices also contributed heavily.” Shultz traced the commodity price boom in good part to a global economic boom, but even then he emphasized the supply side as the factor driving price increases; furthermore, he expressed concern that the “cost-price screw” might be exacerbated if domestic wages and prices responded positively to energy-price increases. On this occasion, very little of Friedman’s position that it was the monetary climate that turned a commodity price increase into a general and ongoing price rise had a parallel in Shultz’s analysis.

In contrast, the area of international economic policy was one of much more widespread agreement between Friedman and Shultz. Some months after Shultz left office, Friedman noted that he had devoted his Newsweek column of April 1, 1974, to a “tribute to a remarkable man,

121 From Shultz’s testimony of February 8, 1974, in Joint Economic Committee (1974d, p. 87).
122 From Shultz’s testimony of February 8, 1974, in Joint Economic Committee (1974d, p. 63).
123 From Shultz’s testimony of February 8, 1974, in Joint Economic Committee (1974d, p. 64).
124 See Shultz’s discussion in his testimony of February 8, 1974 (Joint Economic Committee, 1974d, pp. 63–65 and 88); quotation from page 65.
George Pratt Shultz… a man of principle and an extraordinarily effective administrator and also not incidentally, an economist.”\textsuperscript{125} In that column, Friedman had characterized Shultz as the primary force among U.S. policymakers in the push for increased exchange-rate flexibility.

In the same column, Friedman suggested that the Federal Reserve had been an obstacle to the transition to a flexible-rate system, by, he claimed, urging intervention in the foreign exchange market and supporting the maintenance of exchange controls.\textsuperscript{126} This remark prompted, presumably at Burns’ instigation, a senior staff member of the Federal Reserve Board to write to Friedman (in a letter dated April 9, 1974) questioning Friedman’s statement and asking for evidence.\textsuperscript{127} As Friedman referred generally to the Federal Reserve and did not mention Burns by name, his column may have been referring principally to the Federal Reserve Bank of New York rather than the Board. The New York bank’s president, Alfred Hayes, was a critic of floating exchange rates.\textsuperscript{128}

As for Arthur Burns, he had been privately concerned about the liberalization of exchange-rate arrangements in 1971 (see Meltzer, 2009b, pp. 781–782), and in a public 1972 speech (Burns, 1972, p. 75), he had judged—without referring to Friedman by name—that “last fall’s floating rates did not conform to the model usually sketched in academic writings,” claiming that it had led to extra restrictions on trade and capital and increased uncertainty in the private sector about the economy and government policy.

However, Burns was far more upbeat about floating in his June 1973 Congressional testimony, stating that events since 1971 had increased his “degree of tolerance” of floating rates.\textsuperscript{129} And, as seen above, by 1974 the Federal Reserve Board took objection to suggestions that Burns was hostile to floating. However, Burns’ continued wariness about floating rates remained evident in subsequent years, such as in 1977 when he suggested that exchange-rate depreciation was a source of cost-push inflation. In addition, after he left office, Burns clashed with Friedman at a January 1980 conference about whether the floating-rate system had had the effect of increasing the vulnerability of the United States to international developments—with Burns arguing that it had done so (Feldstein, 1980, p. 95).\textsuperscript{130}

\textsuperscript{125} Friedman (1975a, p. 42).
\textsuperscript{126} Friedman (1974k, p. 3) made a similar claim.
\textsuperscript{127} The letter is in the Arthur Burns papers in the Ford Presidential Library.
\textsuperscript{128} Hayes stepped down as bank president on August 1, 1975 (https://www.newyorkfed.org/aboutthefed/AHayesbio.html) and shortly thereafter called for exchange-rate floating to end: see Daily News (New York), September 1, 1975.
\textsuperscript{129} From Burns’ testimony of June 27, 1973, in Joint Economic Committee (1973, p. 188).
\textsuperscript{130} Burns’ son, Joseph Burns, was, however, strongly in favor of floating (see Joseph Burns, 1979).
Friedman’s assessment in March 1974 was: “The free market in exchange rates owes more to George Shultz’s influence than to that of any other individual.”\textsuperscript{131} It would be going too far, however, to suggest that Shultz designed the floating-rate system that emerged in 1973 and that it consequently emerged as part of a Shultz plan for international economic policy.

A contrary impression may be gleaned by some discussions of proposals for international monetary reform that Shultz outlined in September 1972. Shultz (2017, p. 283) states that the 1972 plan “designed a floating exchange rate system in the clothing of a par value system” and Bordo (2018, p. 27) describes related Shultz plans of the period as aiming “to deliver exchange rate flexibility through the back door.” In response to interpretations that this was a plan that ushered in the floating-rate system, two things need to be said. First, the plan was a \textit{compromise} between fixed and floating rates, and it was therefore different from the floating-rate system that emerged in 1973. Second, it was a proposal that was well received but never implemented, being superseded by the crisis-induced widespread floating of early 1973.

It is also important not to overstate Friedman’s personal role in the 1972–1973 emergence of floating. Leeson (2003, p. 174) pointed to accounts that appeared after 1972 that suggested that Friedman had composed the speech Shultz gave in September 1972 outlining his plan and indicated that Friedman in 1999 had denied such close involvement. Suggestions that Friedman was the architect of Shultz’s plan were actually made as soon as Shultz announced it, with Arthur Laffer (who had returned to the University of Chicago after working in the Nixon Administration) contending that Shultz had consulted Friedman “every step of the way,” with the result that the “proposal is pure Friedman” (\textit{Newsweek}, October 9, 1972). Friedman immediately poured cold water on this suggestion, objecting that “Laffer grossly exaggerates the role which I personally played.” Friedman acknowledged talking to Shultz several times and meeting him in Washington, D.C. (Instructional Dynamics Economics Cassette Tape 108, October 5, 1972). In his recent account, Shultz (2017, p. 284) is magnanimous in describing Friedman as a “big contributor” as a consultant (albeit an unpaid and unofficial one) to Shultz in 1972 on international monetary reform. But Shultz also stressed other input he received on his proposal; and, as already indicated, the 1972 proposal was in fact never implemented.

\textsuperscript{131} Friedman (1974k, p. 3).
Once—contrary to policymakers’ plans—an authentic regime of floating rates materialized in 1973, the U.S. Treasury had in Shultz and his successor William Simon (1974–1977) a head who favored floating exchange rates. However, it would not be accurate to state, as Leeson (2003, p. 4) did, that it was a simple case that “Treasury advisers wished to bury Bretton Woods,” as this characterization overstates unanimity among senior Treasury figures on the matter. Official United States policy after the beginning of the March 1973 float was to work out a fixed-rate system that could succeed it. Paul Volcker was the Treasury official primarily assigned to work out such a system. Until he left office in 1974, it would seem that Volcker sincerely wanted to replace floating with a new system and was not simply going through the motions in seeking alternatives. Friedman himself in March 1974 hinted that Volcker was an opponent of floating. Likewise, around the time of his departure from the U.S. Treasury, Volcker remarked that the consensus his talks were establishing “certainly does not extend to maintaining a fully floating system permanently,” and so did not coincide with the “American academic view” that favored continuation of floating.

Even with his broad satisfaction that floating had been realized, one area in which Friedman was unable to gain ground with Shultz or other policymakers concerned swap agreements (short-term loans by central banks to each other). Friedman argued that the Federal Reserve should be prohibited by law from initiating swap agreements, which he likened to exchange-market intervention (Newsweek, April 23, 1973). This recommendation made no headway, and swap agreements proved to be a permanent feature of the floating-rate era.

Floating rates and the first oil shock

During the initial months following the first oil shock, Shultz commented: “There is no international financial arrangement which can offset the real effects of the oil price changes.” (Washington Star-News, February 15, 1974.)

---

132 See, for example, Shultz’s testimony of February 8, 1974, in Joint Economic Committee (1974d, p. 67).
133 Friedman (1974k, p. 2). The first half of 1974 saw U.S. foreign exchange controls wholly abolished. This was expedited from the original schedule under which controls would be phased out through December 1974 (Evening Sun (Baltimore), February 13, 1973). This was a reform that, in Friedman’s view, made the likelihood of a return of fixed exchange rates remote (Instructional Dynamics Economics Cassette Tape 139, February 4, 1974; Newsweek, April 1, 1974). However, as indicated by the June 1973 quotation from Paul Volcker given above, Volcker hoped to institute an exchange-rate system that reconciled liberal exchange controls with limits on exchange-rate flexibility.
134 From Volcker (1976, quotations from pages 17 and 15, respectively).
135 See also his testimony of June 21, 1973, in Joint Economic Committee (1973, p. 117)
136 Swap arrangements have been praised by such advocates of floating rates as John Taylor (see, for example, CSPAN, October 11, 2010).
The Secretary of the Treasury’s judgment lined up with a longstanding Friedman message regarding the properties of different exchange-rate arrangements—and of floating rates in particular. Friedman’s own analyses of the repercussions of the oil shock reaffirmed this message. As noted in Section II above, Friedman saw the increased cost of imported oil as bearing negatively on U.S. potential output, so it was a real shock to which the American economy had to adjust. But Friedman also stressed that the contour of the adverse reaction of potential output—its initial size and, especially, its magnitude and persistence beyond the period of the price increase—depended on U.S. policymakers’ response to the oil shock. Friedman quickly became highly critical of the actual policy response, as the discussion below of his 1974 panel appearance with William Nordhaus will underline. Ahead of that, it is worth dwelling further on the floating-rate/oil-shock connection.

It would be little exaggeration to say that the only positive aspects Friedman saw in the U.S. policy response to the oil shock were in the monetary area: first, U.S. monetary policy did not accommodate the shock (see Section II above), and second, floating rates made advanced economies’ adaptation to the shock better than it would otherwise be. With regard to the second aspect, Friedman would be laudatory of the role played by floating exchange rates as a shock absorber in the face of the OPEC price moves and the associated shift in international payments flows.\footnote{See Chapter 5.}

The distinction between shock-absorber and shock-nullifier is significant. Over the years, it is true, some commentators would attribute to Friedman the position that a floating-rate system would provide comprehensive insulation and so be a shock-nullifier: that is, it would cancel out the effects on the domestic economy of an oil shock. In this characterization of Friedman’s view, a floating rate could give an economy insulation with regard to all varieties of shocks originating from abroad, including real shocks such as the OPEC shock.\footnote{For example, Stanley Fischer suggested that the notion that “fiscal policies are transmitted internationally under flexible rates” contradicted the position taken by advocates of floating (Fischer, 1988a, p. 114) and also stated that the virtues of floating rates claimed by their proponents included the property that “flexible rates would insulate countries from foreign shocks” (Fischer, 1988b, p. 12). However, Fischer did not identify Friedman by name with either of these propositions. The association of these propositions with advocates of floating rates may have partly arisen from Johnson’s (1969, p. 12) assertion: “The fundamental argument for flexible exchange rates is that they would allow countries autonomy with respect to their use of monetary, fiscal, and other policy instruments.”}

However, as Friedman clearly indicated, what a floating-rate regime conferred to policymakers was more modest in character than this feature. In his advocacy of floating rates during the 1950s and 1960s, he had repeatedly stressed that a flexible exchange rate did not insulate an
economy from real international shocks. The property secured by floating rates was the
narrower but important property of allowing insulation of a country’s nominal money stock from
international factors. This put the country’s monetary authority, via its influence on the money
stock—and hence important asset prices in the home economy—in a commanding position to
influence the path of the nominal aggregate demand.

Consequently, from Friedman’s perspective, a float positioned a country’s monetary authorities
to set its inflation rate over time differently from that prevailing in the rest of the world. It also
permitted the authorities to design a monetary policy that was more stabilizing for real economic
activity than would be possible under a fixed-rate regime. In these circumstances, a float offered
the opportunity to facilitate an economy’s adjustment to a real shock, but it did not deliver
complete insulation from that shock.

This analysis implied that, for a country that possessed monetary autonomy—either that arising
from a float or, as seemed to be more pertinent to Friedman in the case of the United States,
although it did not have lasting implications for inflation, provided that monetary growth was not affected. But, as
Friedman granted that the oil shock affected the level of U.S. potential output, even his analysis
implied that this shock had long-run implications for real economic activity. And the longer the
higher oil price lasted, the longer-running the implications for the real economy.

In this area, Friedman found fault with U.S. policymakers—not principally with Shultz or Burns,
but with the officials and federal agencies delegated the most immediate responsibility for the
policy response to the oil shock.

WILLIAM NORDHAUS

Friedman’s critique of the policy response to the energy situation would span several years but at
an early stage would pit him in debate against Yale University’s William Nordhaus. Like

139 Along these lines, when referring in their Monetary History to the United States’ growing autonomy during the
nineteenth century with respect to monetary conditions, Friedman and Schwartz (1963a, p. 9) observed: “The
weakening of external links offered the possibility of insulating the domestic stock of money from external shocks.”
140 For example, in an appearance in Australia in April 1975, Friedman said of a float (Friedman, 1975g, p. 16):
“That doesn’t mean that what happens abroad doesn’t affect Australia, of course it does…” He gave the OPEC
price increase as an example before adding: “Changes that occur abroad will affect your real circumstances of the
economy, but nothing that happens abroad will determine your rate of inflation—that will be determined at home.”
141 See Nelson (2019b) for analysis of Friedman’s 1950s and 1960s writings on this point, especially Friedman
(1953) and Friedman and Roosa (1967).
Friedman, Nordhaus proved to be a future winner of the Nobel economics award. Unlike Friedman, however, in the 1970s Nordhaus was prepared to offer a partial defense of the national energy policies of which Friedman was so critical.

*Friedman’s free-market critique of U.S. energy policy*

Friedman’s indictment of the response of U.S. energy policy to the first oil shock—and, specifically, what he bluntly called the “stupid” federal government regulations on the pricing and allocation of oil and related products in the United States (*The Herald* (Chicago), March 18, 1974)—had discouraged U.S. oil production, held up consumers’ demand for petroleum products, and fortified OPEC in its efforts to make the 1973–1974 oil price increase permanent.

Friedman’s critique of the U.S. policy response to the oil shock was one he refined over time as he learned more about the energy market and as official policy itself evolved. But the overriding theme that he articulated over the years was consistent: he maintained that the free market was not being permitted to operate in response to the shock.

As already noted, Friedman had already pointed to U.S. government policy as having created energy shortages in the United States during 1973 even before the Middle East war; he had also discussed the oil market at length before 1973, as discussed presently. But once the first oil shock put the energy issue at center stage, the occasions on which Friedman articulated free-market recommendations in this area proliferated. Early instances included his October-November 1973 interventions already discussed in this chapter. Another was a talk on the energy crisis that Friedman gave at Princeton University on December 5, 1973, to a free-market student group, the Undergraduates for a Stable America.142

The following month, Friedman appeared at an event in Washington, D.C. on the matter: a National Conference on Government Responses to the Energy Crisis on January 24, 1974. The conference included a panel of economists: Friedman, MIT’s Morris Adelman, and Nordhaus. The panel did bring out major areas of agreement on the energy situation. But it also highlighted the fact that, even though Friedman’s emphasis on liberalization of the oil market became widely endorsed after U.S. energy price controls were finally removed in 1981, it was far from uniformly accepted among economists in the early years after the oil shock.

142 Information from Gloria Valentine, October 6, 2014.
In his Newsweek column of December 10, 1973, Friedman claimed that there was “wide agreement” that letting “the free market reign” was the most efficient way of responding to the oil shock. In fact, however, the period 1973–1974 saw considerable resistance even among economists to a market-based means of dealing with the oil problem. True, Kenneth Arrow and Friedman came close to putting out a joint statement in late 1973 favoring deregulation of U.S. retail gasoline prices. But the lack of consensus on the matter among leading economists was underscored by Paul Samuelson’s declaration in his own Newsweek column of November 26, 1973: “Gasoline and fuel oil should definitely be rationed.”

Samuelson’s position clearly rankled with Friedman. In teaching his Price Theory class at the University of Chicago in 1973/1974 Friedman would point to Samuelson’s Newsweek analysis of the energy situation as something that could be criticized as faulty positive economics (Charles Plosser, interview, April 2, 2015).

As it happened, the rationing of gasoline that Samuelson recommended was one intervention that was not implemented at the national level in the United States in the face of the oil crisis. In contrast to countries like the United Kingdom, the provision of petroleum to U.S. motorists was not subject to official physical rationing. The fact that rationing was not imposed may have been a factor in Friedman’s retrospective judgment (New York Times Magazine, December 27, 1987) that William Simon, the Nixon Administration’s “energy czar” in 1973–1974, made “a bad policy—price controls on oil—run relatively unbadly.” Nonetheless, as Friedman observed at the January 1974 panel on energy policy, it was not accurate to suggest that the United States had not responded to the oil price increase with rationing, as the federal government had imposed controls on allocation and production in the wholesale market for oil—controls that continued over the rest of the 1970s.

If Samuelson’s response to the oil crisis was discouraging to Friedman, he could take comfort from the posture taken by Samuelson’s MIT departmental colleague Morris Adelman, with whom Friedman and Nordhaus shared the stage at the January 1974 conference. In recalling the contributions of Adelman and Friedman to the debate on oil, Adelman’s longtime colleague Robert Solow noted: “I think that they were somewhat on the same side.” (Robert Solow, interview, December 2, 2013.) Notably, in comments made soon after the outbreak of the Middle East war, Friedman observed that the “person who has taken this position [on oil] most consistently—with a greater degree of expert knowledge than I have; mine is completely
superficial knowledge—is a fella by the name of Morris Adelman, who is a professor at MIT.” (Instructional Dynamics Economics Cassette Tape 131, October 10, 1973.) In particular, Friedman’s remark on that occasion that “we have enormous reserves of oil” echoed a longstanding Adelman point.143

More generally, Adelman’s and Friedman’s analysis had considerable overlap in stressing weaknesses in the underlying position of OPEC and in being critical of U.S. energy policy.144 In their January 1974 joint appearance, Adelman expressed numerous observations highly consistent with Friedman’s sentiments. Inter alia, Adelman remarked that “every fact we know about oil supply [suggests] it’s hugely elastic” and “one thing is certain: that, with current prices ruling, oil is going to be coming out from behind the woodwork all over the world.” Consequently, he proclaimed, OPEC “will undoubtedly crumble, as all cartels have done…”145

It is therefore not surprising that, during the 1970s, some commentators on the energy debate categorized Friedman and Adelman as members of the same camp. For example, a paper for a conference held on the third anniversary of the oil shock (Issawi, 1977, p. 91) observed scornfully that “many economists of the Morris Adelman-Milton Friedman school are still waiting for it [the oil price] to fall to its marginal cost of production, say 20–30 cents a barrel, which is like waiting for Godot…”

When appearing together with Friedman and Adelman at the January 1974 symposium, William Nordhaus himself alluded to the similarities between the Adelman and Friedman positions. When he spoke third in their joint appearance on the economists’ panel, Nordhaus remarked drily: “Well, we’ve heard several invigorating lectures today on the miracle of the price system. As the last panelist, it’s probably appropriate [for me] to turn to the long-run energy problem.”

But Friedman and Adelman were not interchangeable in their views on energy policy. Adelman was less inclined than Friedman to advocate a wholly noninterventionist policy response to the oil shock. In his January 1974 symposium contribution, Adelman suggested that OPEC might be

---

143 See, for example, Adelman (1972) and Wall Street Journal, February 9, 1973. Samuelson (in Newsweek, November 26, 1973) also referred to and endorsed Adelman’s finding that “known oil reserves are up, not down.”
144 Some evidence of Friedman’s analysis of the oil market itself having an influence on Adelman came in the fact that Friedman in his January 1974 appearance put the energy crisis in perspective by quoting at length from William Stanley Jevons’ (1865) fear—as it turned out, unrealized—regarding an approaching energy shortage (a use of Jevons’ work that he would repeat in Friedman, 1983d, pp. 141–142). Adelman (1975) would likewise open with this Jevons (1865) passage.
145 From Adelman’s remarks in the symposium Long-Term Energy Crisis Solutions, held at the National Conference on Government Responses to the Energy Crisis, January 24, 1974.
able to maintain discipline for years and that the U.S. government should take active steps to accelerate the group’s collapse. These suggestions prompted Friedman to imply that such activism was unnecessary: “I am much more optimistic than Morrie Adelman is about how rapidly the oil cartel is going to find that they are left holding the bag.” Also at the January 1974 symposium, Adelman advocated government financing of, or concessions for, research into alternative energy sources, while Friedman in the same discussion rejected both public funding and tax concessions for such research. Furthermore, between mid-1974 to mid-1979 (that is, after the embargo had been lifted but OPEC’s oil price increase of 1973–1974 remained), Adelman advocated U.S. government restrictions, in the form of quotas or tariffs, on importation of oil from OPEC countries as a means of squeezing the cartel.146 Friedman repeatedly opposed such trade restrictions.

Consequently, and especially in light of his high-profile remarks on OPEC, it was principally Friedman who was associated with free-market solutions to the energy crisis, and Friedman who took most of the criticism for having been excessively overoptimistic about an OPEC collapse.

And it was against Friedman rather than Adelman that William Nordhaus was primarily pitted in the January 1974 symposium. As well as representing sharply different positions on the appropriate role of the public sector in response to the oil shock, Nordhaus and Friedman were from different generations. Nordhaus was born in 1941, six years after Friedman started publishing in economics. This generation gap did not prevent Nordhaus—in the course of his lively exchange with Friedman—from remarking that Friedman reminded him of students he had who would not listen properly to what Nordhaus was saying to them.147

Friedman’s indictment of pre-1973 U.S. energy policy

Although Friedman could not claim to have the depth of knowledge of oil possessed by his fellow panelists in the January 1974 symposium—“I have much less expertise in that area than he does” Friedman noted when he followed Adelman on the panel—he had in fact written on the oil market for over a decade prior to the oil shock. In these earlier discussions, as in 1973, Friedman viewed the operation of the energy market as frustrated by market forces. For

---

146 See, for example, Adelman’s testimony (of January 29, 1975) in Committee on Foreign Relations, U.S. Senate (1975, p. 4) and Adelman’s remarks in Mitchell (1979, pp. 23–24). See also The Plain Dealer (Cleveland), February 1, 1975.

147 Other statements around this time by Nordhaus about his students were more flattering. Most notably, his 1973 study of U.S. energy resources acknowledged that “Paul Krugman [an undergraduate student at Yale University] provided research assistance extraordinary” (p. 529).
example, in *Capitalism and Freedom* he had stated that the United States should not use its
government to protect a group of oil producers.\(^{148}\) His *Newsweek* column of June 26, 1967 on the
oil market stated that U.S. oil producers, more than almost any producers, “rely so heavily on
special government favors.” Likewise, in early 1969, he described oil as a highly sheltered
product (*Instructional Dynamics Economics Cassette Tape 15, February 1969*). Indeed,
Friedman and Adelman were both invited to appear at a March 1969 hearings of the U.S. Senate
Committee on the Judiciary to make the case for much-reduced government intervention in the
oil market, but in the event only Adelman was available to testify.\(^{149}\)

A number of specific items were included in Friedman’s bill of charges in his indictment of U.S.
energy policy. Prior to its abolition in April 1973, he repeatedly criticized the quota on oil
imports; this had been imposed by President Eisenhower (citing national-security reasons) in
1959.\(^{150}\) Then controls on U.S. consumer prices on gasoline and crude oil had been imposed as
part of the economy-wide August 1971 wage/price controls. It is against this background that
when the first oil shock arrived, Friedman cited “government price-fixing and government
mismanagement” as setting up an energy problem even before the Middle East war (*Instructional
Dynamics Economics Cassette Tape 134, November 21, 1973*). This characterization contrasted
sharply with Nordhaus’ (1973, p. 530) contention, expressed at roughly the same time, that U.S.
“public policy [has] accepted a *laissez-faire* approach to resource pricing.”\(^{151}\)

Furthermore, when the Nixon general price controls were lifted at the end of April 1974, the
controls on prices of petroleum products remained in force (*The Evening Bulletin* (Philadelphia),
July 19, 1974).\(^{152}\) What is more, as already implied, the U.S. government reacted to the OPEC
shock by imposing a variety of new controls on prices and quantities in the U.S. wholesale
market for oil.\(^{153}\)

---

\(^{148}\) Friedman (1962a, p. 139).

\(^{149}\) See Friedman’s letter (dated February 26, 1969) in Committee on the Judiciary, U.S. Senate (1969, p. 6) as well
as Adelman’s testimony (of March 11, 1969) beginning on the same page. See also the mention in U.S. senators’
discussion (on April 1, 1969) of Friedman and Adelman as advocates of a freer market in oil (see page 305 of the
same volume).

\(^{150}\) On the details of the quota and its rationale, see Rieber (1961). On its abolition, see Bohi and Russell (1977, p. 2)
and Friedman (1975a, p. 310). Friedman’s criticisms of the quota included those in *Newsweek*, June 26, 1967,
policy in the 1950s that Friedman criticized was the introduction of controls on natural gas prices. See, for example,

\(^{151}\) Nordhaus’ (1973) paper was evidently completed in late January 1974 (see p. 556).

\(^{152}\) See also *Milton Friedman Speaks*, Episode 2, “Myths That Conceal Reality,” October 13, 1977 (pp. 30–31 of
transcript) and Friedman and Friedman (1980, p. 219).

\(^{153}\) Friedman’s critiques of these controls on the wholesale market (including the “old oil/new oil” distinction
introduced by the federal regulators) is analyzed in Chapter 5.
In his contribution to the January 1974 panel, however, William Nordhaus provided a defense of government intervention in the market. “While I generally agree with some of the panelists about the difficulty associated with unrealistic controls,” Nordhaus remarked, “I would not care to associate myself with their evangelical zeal for the free market’s virtues.” However, like Friedman, Nordhaus did warn against government policies that were premised on continuation of present oil prices; indeed, the likely outcome Nordhaus saw was not unlike that sketched by Friedman and Adelman, with demand and supply reactions expected to drive oil prices sharply lower by 1980.\(^{154}\)

This was a standard view at the time. Many disagreed with Friedman’s predictions of a quite-rapid OPEC breakdown. But the basic belief that the oil price would fall back by the end of the decade was widely shared—and contrasted sharply with the eventual outcome, which was a second oil shock in 1979–1980. For example, a few weeks after Nordhaus’ January 1974 remarks, Secretary of the Treasury Shultz urged his interlocutors: “ask yourself, is it likely that the price of oil will be higher in 1980 than it is now? And I think the answer to that is very, very clear: No, because of the great surges in supply that are coming from sources of oil and other energy…”\(^{155}\)

But what should be done in the interim? Nordhaus argued in his remarks that, during the period before oil prices fell back, federally-imposed price and quantity controls on oil “will smooth the transition from one regime to the next.” Nordhaus also saw a long-run role for the government in providing stockpiles of oil to protect against future oil shocks. He further insisted that the private sector would not do adequate research-and-development programs for alternative energy sources: “I think substantial federal involvement is called for,” Nordhaus contended. The private sector left to itself, he suggested, would devote insufficient resources to the high-risk investments that could generate technological advances. “These are areas where the theorems about the price system that Professor Friedman invokes simply do not apply.”

Those advocating a more interventionist policy response could also point out that the free-market positions that Friedman had advocated in the 1960s on oil jarred somewhat with later developments. For example, his June 1967 *Newsweek* column had criticized the national-

\(^{154}\) Nordhaus also stressed, as Friedman often did (see Friedman, 1983d) that, beyond the boycott period, physical unavailability of oil was not in prospect for the United States: oil would be available to the country in the amounts it demanded, the question was at what price. However, he parted company with Adelman’s and Friedman’s predictions about an eventual breakup of OPEC: “I guess I don’t share the faith in the powers of competitive forces to erode powerful monopoly positions.”

\(^{155}\) From Shultz’s remarks of February 8, 1974, in Joint Economic Committee (1974d, p. 75).
security basis for the oil cartel and had poured cold water on the notion of an oil embargo: “the Arabian countries themselves cannot refuse to sell for long.” In fairness, it needs reiterating that the 1973–1974 oil embargo itself did not last long, even though the oil price increase that accompanied it was permanent.\footnote{This (together with another hint that OPEC would soon collapse) was essentially the basis on which Friedman defended his 1967 analysis when he revisited that analysis in July 1974 (see Friedman, 1975a, p. 310).} But Friedman had in 1967 also not counted on the emergence of OPEC as a cohesive force, as his column had argued that the “world oil industry is highly competitive and far-flung and getting more so.”\footnote{Morris Adelman was in the similar position of being critical of pre-1973 energy policy but also surprised by the oil shock. On March 11, 1969, he had testified (Committee on the Judiciary, U.S. Senate, 1969, p. 7): “The basic fact about the world oil industry is that there will be ample supply at less than current prices for 15 or more years.” In remarks on January 29, 1975, Adelman acknowledged (Committee on Foreign Relations, U.S. Senate, 1975, p. 18): “I think there is no precedent for the current oil monopoly. Certainly the discrepancy between price and costs... is an order of magnitude greater than anything I have ever seen.”} Furthermore, his pre-1973 criticisms of U.S. energy policy—particularly the import quota and the “percentage depletion allowance,” a tax deduction for the U.S. oil industry—had made the case that the United States produced too much oil and imported too little, and that it should seek the lower petroleum prices available from freer trade.\footnote{See, in particular, his remarks in Chicago Daily News, July 29, 1970, and in Friedman (1970b, p. 437).} As Herbert Stein was able to point out in a televised debate with Friedman (\textit{University of Chicago Round Table: The Nation’s Economy Out of Control}, PBS, May 1, 1974), the pre-1973 control apparatus quite likely put the United States in a better position than otherwise to handle the immediate implications of the OPEC shock, by making the country less reliant on foreign sources of oil.\footnote{Similarly, Bohi and Russell (1977, p. 17) argued that the “quota and production controls [in force as of 1973]... saved the United States economy from making even more drastic adjustments to changes in the world price.”} For Friedman, however, a free market would have been preferable both before and after 1973 because the price system gave the U.S. economy more dexterity in reacting to world developments. It would have been desirable, he contended, for the United States to have benefited fully from the low world energy prices prevailing before 1973.\footnote{See Friedman (1983d, pp. 147–148).} And, in the wake of the OPEC shock, Friedman rejected the Nordhaus position that a public-sector role was needed in the storage of oil.\footnote{In his Congressional testimony of January 29, 1975, Adelman likewise endorsed the need for a federal agency to be created to stockpile oil (Committee on Foreign Relations, U.S. Senate, 1975, p. 18).} “All experience shows that men love lotteries,” Friedman said, so incentives existed for private-sector producers to store oil in anticipation of windfalls from future price surges.\footnote{From Friedman’s panel appearance of January 24, 1974. Friedman’s opposition to public-sector stockpiling programs echoed his discussion in Friedman (1954b) of similar proposals applied to the agricultural sector.} In addition, Friedman insisted that economically beneficial technological advance had predominantly come from the private sector. And, again in contrast to Nordhaus’
position, Friedman was adamant that the system of federal controls in force would not aid the United States’ adjustment to the OPEC price increase.

_Urging decontrol of retail prices_

From the onset of the first oil shock, Friedman especially stressed the need for decontrol of retail gasoline prices to send the correct price signal to consumers. He did, however, oppose going further on the discouragement of consumption by increasing federal taxes on gasoline. Friedman differed from Secretary Shultz on this score (Instructional Dynamics Economics Cassette Tape 134, November 21, 1973). A new gasoline tax, Friedman argued, would send imbalanced signals. The revenue arising from the tax would not go to suppliers, so the price rise generated by the tax would discourage demand without boosting energy production (Newsweek, May 2, 1977). Instead, he simply favored ending the official controls on energy prices. The resulting price signal, he believed, would result in a substantial shift of demand away from oil.

In time, a very large adjustment in demand would occur: growth in U.S. consumption of petroleum products moved from 4.5 percent per year in 1945–1973 to 0.5 percent per year on average in 1973–2006.

This shift showed little signs of emerging in the initial years following the oil shock. Friedman’s explanation for this included, as already noted, the likelihood that the sizable elasticity of demand for energy manifested itself only gradually. But this was only one part of the explanation, and it became less important over time. In 1978, Friedman cited the refusal of the federal authorities to permit the U.S. retail price of gasoline to reflect fully the global rise in oil prices as the central factor behind the absence of an appreciable U.S. drop in energy consumption in the years following 1973—a consumption pattern that contrasted with that in other countries. The price control, Friedman felt, prevented a sharp reaction of U.S. consumer demand, one that would have put greater pressure on the OPEC cartel.

And on the supply side, Friedman would note that, ultimately, the oil shock “led to a vast increase in oil produced outside of the OPEC countries” (MacNeil/Lehrer News Hour, PBS, April 17, 1987, p. 5 of transcript). But he felt that this supply reaction, as well as the

---

163 Matusow (1998, p. 266) also quoted what he described as a memorandum (actually, simply a letter—available in the Friedman archives at the Hoover Institution), dated November 16, 1973, in which Friedman laid out to Shultz his opposition to a federal gasoline tax.


165 See Friedman (1983d, pp. 146–147).

166 Similarly, Friedman (2000, p. ix) implied that OPEC was not able to protect itself against competition.

136
development of substitutes for oil, had been greatly slowed down by the U.S. energy policies of the 1970s.\textsuperscript{167}

As for William Nordhaus, his role in energy policy became an official one as a member of the Carter Administration’s Council of Economic Advisers in 1977–1979, with his focus being on energy policy issues. Once back outside the official sector, Nordhaus assessed successive U.S. administration’s response to the oil shocks and came out negatively. “I think the quality, not of the people, but of the actual decisions that have been made by the federal government over the last seven years is extremely poor,” Nordhaus observed in 1980, “and has actually exacerbated the problem.” Whereas in his 1974 panel appearance with Friedman, Nordhaus had cited insufficient stockpiling as a shortcoming of market mechanisms, his 1980 assessment was that “private institutions find it easier to deal with shortages, stockpiles, and allocations than do public institutions.”\textsuperscript{168} And, in contrast to his 1974 cautious endorsement of oil price controls, Nordhaus now stated that “decontrol of oil prices is the absolute precondition for rational behavior during emergencies.”\textsuperscript{169}

Looking back on Friedman’s stance on energy policy during the 1970s, Nordhaus would observe: “Basically his was a view that free markets are very powerful and regulation represents capture (which was not quite right on energy). He was right (more than I thought at the time) about the long run, but underestimated the lengths of the lags between prices and outputs. In one sense, the shale revolution today is the result of the deregulation that Milton Friedman pursued [and which was subsequently implemented] in 1981.” (William Nordhaus, personal communication, December 8, 2014.)

\textit{Nordhaus and debates on inflation and monetary policy in the 1970s}

Another activity alongside energy policy in which Nordhaus was highly engaged during the 1970s was the modeling of the behavior of aggregate U.S. prices and costs. His research output advanced criticism of the monetarist view of inflation—a criticism that drew a public response from Friedman.

\begin{flushleft}
\textsuperscript{167} See Chapter 5 and subsequent chapters below. \\
\textsuperscript{168} From Nordhaus’ testimony of November 11, 1980, in Committee on Finance, U.S. Senate (1980, p. 28). \\
\textsuperscript{169} From Nordhaus’ testimony of November 11, 1980, in Committee on Finance, U.S. Senate (1980, p. 29).
\end{flushleft}
The paper that spurred this response, Nordhaus (1972a), was prepared for the September 14–15, 1972 meeting of the Brookings Institution’s panel on economic activity. Nordhaus investigated the surge in nominal wage growth that had occurred across OECD countries in recent years. In judging the validity of this paper’s findings, a distinction needs to be drawn between its recording of an empirical regularity, on the one hand, and the conclusions that Nordhaus drew for monetarism from his analysis, on the other.

The regularity that there was a surge in nominal wage growth in the early 1970s across advanced economies became widely accepted, as was the label that Nordhaus gave to that regularity in the title of his paper: “The Worldwide Wage Explosion.” Nordhaus’ documentation of this regularity was an enduring contribution of the paper. Attesting to this, forty-five years after the paper’s publication the International Monetary Fund (2017, p. 143) referred to the “uptick [in labor’s share of income] around 1970 coinciding with the ‘worldwide wage explosion’ (Nordhaus 1972[a]).”

However, Nordhaus’ conclusion in the same paper about the merits of the monetary explanation of inflation has not proved enduring and is contrary to the modern consensus on the matter. Nordhaus (1972a) endeavored to use recent years’ international patterns of nominal wage growth to make conclusions about monetarism’s proposition about inflation. Nordhaus specifically cited, and took issue with, Friedman’s 1966 paper “What Price Guideposts?,” which had criticized wage-push views of inflation—and which had been one of the earliest instances in which Friedman used in print his dictum that “inflation is always and everywhere a monetary phenomenon”—a dictum Nordhaus (1972a) quoted.170

Nordhaus’ empirical approach was based on the contention that this dictum implied a unitary connection between nominal wage growth and monetary growth. And such a connection, Nordhaus suggested, was not present in the data: when estimating nominal-wage or nominal-wage-growth equations for seven countries, Nordhaus (1972a, pp. 437–439) found that nominal money was statistically insignificant in the case of all countries except the United States, and in the U.S. case money had a coefficient of 0.485 rather than being closer to 1.0.171

In 1977, Friedman responded to the Nordhaus’ (1972a) finding. Friedman’s first and primary point—that Nordhaus did “not even consider price inflation but only wage inflation” (The Times,

---

170 See Friedman (1966) and Nordhaus (1972a, p. 418).
171 Nordhaus also included productivity or its growth rate in the estimated specifications.
May 2, 1977)—is considered shortly. Before the discussion of that point, it is worth laying out the reasons why, even when one takes nominal wage growth as informative about inflation, Nordhaus’ (1972a) results did not amount to a convincing and valid finding against Friedman’s monetary view of inflation.

It should be said first that Nordhaus’ equation evaluating Friedman’s views, which regressed nominal wage growth directly on nominal monetary growth, constituted a reduced-form specification rather than an attempt to describe the structural equation for wage- or price-setting associated with Friedman’s analysis. The Nordhaus equation combined monetary policy’s influences on aggregate demand with the response of wages to the output gap, and it is the output-gap/wage link that would be the structural equation in Friedman’s work. Nordhaus (1972a), as well as his earlier analysis in Nordhaus’ (1972b), did not dispute the influence of the output gap on wage inflation, at least for the United States. More direct investigation of the relationship between resource gaps and wage inflation for the countries in Nordhaus’ (1972a) sample found that there was indeed a positive influence of such gaps (see Laidler, 1976b, and, for an extended sample, Grubb, David, Layard, and Symons, 1984).172

Once it is granted that aggregate demand matters for wage growth, the question then arises why Nordhaus found that monetary growth and wage growth were generally unrelated. Friedman’s contentions that monetary policy was the decisive aggregate-demand tool, and that monetary growth was a useful summary of how monetary policy mattered for aggregate demand, suggests that monetary growth should have been useful in predicting wage growth (especially if wage growth was a reasonable stand-in for price inflation). A likely resolution to the puzzle lies in the fact that Nordhaus (1972a) neglected the lag between monetary growth and inflation. He claimed to allow for “the customary six-month lag between monetary impulse and the change in income,” by putting six-months-earlier money rather than completely-current-year money on the right-hand-side of his nominal-wage equations (Nordhaus, 1972a, p. 437). But when Friedman wrote of a six-month lag, it was between monetary growth and growth in aggregate (real and nominal) real income. By 1972, Friedman saw the lag between monetary growth and inflation as much longer—about eighteen months to two years, on average.173 In contrast, by allowing only a

---

172 Grubb, Layard, and Symons (1984) estimated an augmented Phillips curve. Laidler (1976b), concentrating on pre-1970 data, did not include an expectations term, but he nevertheless found generally significant gap terms in equations for nominal wage growth. In Nordhaus (1972a, pp. 445–446), the expectational Phillips curve was found to perform badly for France, Germany, and the United Kingdom. In retrospect, however, this finding can be seen as a result of not adequately taking into account rises in the natural rate of unemployment in the late 1960s and early 1970s in the United Kingdom and in Continental Europe.

173 See Nelson (2018a, Chapter 15).
six-month lag, Nordhaus was essentially evaluating a static relationship between monetary growth and nominal wage growth.

Consequently, Friedman was able to conclude that Nordhaus (1972a) “in no way contradicts” the proposition that monetary policy affects inflation with a roughly two-year lag. He also noted that Nordhaus’ results came from “data for a brief period (1956–1971)” and that longer samples would likely bring out more clearly the validity of the monetarist view of inflation (*The Times*, May 2, 1977). Consistent with Friedman’s emphasis on both the two-year lag and on the value of longer samples, Gordon (1981, p. 33) observed: “As further support for the monetarist case, the [wage] equations for the United Kingdom clearly indict the Bank of England as the major culprit for the 1974–76 British wage explosion.”

*Monetary policy and the markup*

All in all, then, Nordhaus’ (1972a, p. 436) contention that the empirical evidence contradicted the notion that “the recent wage explosion should be explicable on monetarist grounds” was not sound. Reasons have been given above why his rejection of Friedman’s proposition that “inflation is always and everywhere a monetary phenomenon” was unwarranted. But there was another crucial reason: Friedman’s proposition referred to price inflation, not wage inflation. And for Friedman, the connection between wage and price inflation was by no means as close, automatic, or obvious as Nordhaus’ and many other Keynesian analyses supposed.

To be sure, Nordhaus was correct to suggest that Friedman’s monetarist framework predicted that nominal wage growth was endogenous—and, in particular, was responsive to monetary policy actions. Furthermore, in Friedman’s framework expansions of aggregate demand do, other things equal, give rise to rates of increase in goods prices and in nominal wages (adjusted for productivity) that are equal to each other. But Friedman regarded the short-term relationship between inflation and wage growth as potentially very loose—in contrast to Nordhaus’ (1972a, p. 437) attribution to Friedman of the belief that real wage growth followed a smooth trend. And very importantly, Friedman saw monetary policy as exerting reliable influences on inflation even when wage growth was exhibiting short-run divergences from nominal wage growth. The way Friedman put it in April 1975 was that “the essence of an inflationary process is that both prices and costs go up”; “sometimes in the process one [costs]

---

174 See, for example, Friedman (1976b, pp. 222–223).
goes ahead and at other times [prices] go ahead.”175 When prices “go ahead,” in this account, they still reflect monetary influences.

One way of characterizing this line of argument is that—in contrast to static constant-markup models of the kind Nordhaus (1972a) used—Friedman thought of monetary policy as capable of exerting an influence on the markup of prices over marginal cost. In particular, excessive monetary growth could be manifested in higher inflation even if the monetary excess did not initially show up much in higher nominal wage growth: in response to monetary easings, markups could increase or rise at a higher rate. And restrictive monetary policy could reduce inflation or hold it down even when the restriction was not showing up in slower nominal wage growth: markups could fall or increase at a slower rate. An unlikely source of support for this Friedman position came during 1974 from James Tobin, who observed (Tobin, 1974, p. 228): “If the Fed were willing to starve the economy for liquidity, regardless of the consequences for real output and employment, presumably price indexes could be held down even when unit labor costs are rising…”

The relationship between prices and costs did have an important role in Friedman’s aggregate supply framework, with perceptions of prices in relation to labor costs being an inducement for suppliers to produce in that framework, and so being part of the behavior underlying the expectations-augmented Phillips curve for price inflation.176 This role underlies the fact that it is perfectly possible for narratives of price behavior that view prices in terms of a—possibly varying—markup over marginal costs to be consistent with an explanation of inflation in terms of monetary variables.177

Friedman, however, did not take this route in his empirical work. He was certainly familiar with analyses that focused on markup behavior, especially because of his exposure to the work of Wesley Mitchell.178 But, as discussed in the previous chapter, in practice Friedman did not find it fruitful to look to cost behavior for understanding the course of aggregate prices. As an

---

175 Friedman (1975h, p. 12).
176 Dimand (2018, p. 7) claims that Friedman’ (1968a) Phillips-curve theory “implies countercyclical movement of real wages.” But he provides so source in Friedman’s writings for this claim, and his characterization flies in the face of Friedman’s (1972a, p. 930) explicit discussion of the fact that his framework does not have this specific implication. He stressed that his framework, in which expected real wages matter for workers, can imply that actual real wages can have either a positive or negative relationship with employment.
177 See Nelson (2003, p. 1040) for further discussion.
178 Notably, Rotemberg and Woodford (1999, pp. 1053–1054) motivated their work on the markup by pointing to Wesley Mitchell’s emphasis on the regularity that costs rose more than prices late in expansions as profit margins narrowed, as well as the implications for aggregate economic fluctuations that Mitchell drew from this regularity. Friedman had discussed the same aspect of Mitchell’s thinking in Friedman (1950, pp. 482–483).
analytical matter, he undoubtedly saw a danger that taking such a posture would increase the danger of lapsing into analyses in which the inflation process was portrayed as featuring unidirectional causation from costs to prices. This was certainly how cost-push analysts viewed the process. Seeing things in costs-determine-prices terms was also a trap into which analysts who appropriately recognized the endogeneity of factor prices might also fall.179

And, as an empirical matter, Friedman saw the dynamic cost/price relationship as much looser than was usually claimed by those deploying a markup-oriented analytical framework.180 Friedman’s approach to inflation analysis concentrated on reduced-form relationships between monetary growth and inflation. This provided a way of avoiding structural modeling of the markup relationship, while also embedding his confidence that monetary policy actions showed through in price inflation under a variety of scenarios.

Gordon (1981, p. 4) attempted to encapsulate the monetarist position on inflation by observing that, according to that view, “‘wage-push’ by unions... may be able to influence the unemployment rate or the distribution of income, but not the inflation rate.” Although this characterization reflected the basic flavor of Friedman’s analysis in important respects, it needs to be qualified in two respects.

First, Friedman granted that the labor share of income, and conversely the markup are ultimately insensitive to monetary policy, thanks to the natural rate hypothesis. The markup, being a ratio of one nominal variable to another variable, was a real variable, and so its steady-state value was invariant to monetary actions.181 Therefore, while the markup could be subject to permanent shifts due to real factors, it was insensitive to monetary policy in the long run.

Second, for given values of the natural rates of output and unemployment, Friedman believed that wage-push, like other cost-push factors, was not a systematic source of upward pressure on prices and costs: apparent upward pressure on wages from wage-push in one sector of the labor

179 As will be seen below, this concern remains valid in modern times. New Keynesian analysis (in which costs and prices are properly viewed as jointly determined) has often been presented in shorthand form as implying that costs determine prices.
180 Rasche (1976, p. 307): observed: “My impression is that this assumption [a constant markup] is regarded by many monetarists as the ultimate ‘Keynesian’ heresy…” Not all the inferences Rasche associated with this assumption were correct (in particular, contrary to his discussion, a constant-markup view need not imply a wage-push view of inflation; and even monetarist accounts of inflation required a long-run proportionality between nominal wages and prices, ceteris paribus). But Rasche’s basic impression was well founded: in monetarist accounts, pressures on prices arising from monetary policy actions worked partly via the markup.
181 See Friedman (1970a, p. 211) and Friedman and Schwartz (1982, p. 50).
market would be offset by downward pressure in other sectors, though the downward pressure might be spread over a number of periods.\footnote{See Friedman (1951b, 1966) as well as the analysis in Nelson (2018a, Chapters 7 and 10).}

Friedman’s analysis can be reconciled with New Keynesian models’ perspective on the relationship between marginal costs and prices, though it differs from many narratives of New Keynesian analysis in the choice of what reactions should be emphasized. Standard New Keynesian analysis links the current price level to its lagged value and to the expected stream of current and future nominal marginal cost. This in turn implies (see Gali and Gertler, 1999, and Sbordone, 2002) a forward-looking expression for inflation dynamics that can be expressed either as \( \pi_t = \alpha \cdot rmct + \beta E_t \pi_t+1 \) or as \( \pi_t = \alpha \cdot E_t \sum_{i=0}^{\infty} \beta^i rmct+i \), where \( \alpha > 0 \), \( \beta \) is a discount factor near 1.0, \( rmct \) is the log of real marginal cost (which in the simplest case corresponds to the log of real unit labor cost), and where any constant terms on both sides are suppressed.\footnote{A shock term to the inflation equation is assumed absent in these equations. This simplification permits the analysis to focus on shocks to labor costs, rather than to prices for a given expected path of marginal cost.}

Usually, expositions of New Keynesian analysis take a positive shock to the current labor-cost term \( rmct \) as affecting the expected stream \( E_t \sum_{i=0}^{\infty} \beta^i rmct+i \) in the same direction and so as raising inflation. This perspective perhaps underlay Galí and Gertler’s (1999, p. 214) statement—not actually justified by their New Keynesian analysis, in which marginal cost and inflation are determined simultaneously—that “in our model, causation runs from marginal cost [to] inflation.”\footnote{That is, these series are simultaneously determined. The exogenous variables are the shocks in the model, and not either inflation or real (or nominal) marginal cost. Under such circumstances, description of causation running only one way cannot be valid, and a statement that real marginal cost causes inflation may be best interpreted as a statement that the reaction of marginal cost to various shocks (other than direct price shocks—which are assumed absent in the version of the New Keynesian Phillips curve given here) governs how inflation responds to those shocks.

The issue raised is similar to that brought up in some analyses of the inflation/output relation. As discussed in Nelson (2018a, Chapter 7; 2019a), in Friedman’s view of the transmission mechanism, as well as in modern New Keynesian models, inflation and output are simultaneously determined: higher demand puts upward pressure on the rate of increase in prices, but also higher prices (in relation to costs) stimulate production. In this setting, it would not be valid to describe causation between inflation and output as running in only one particular direction. It would also not be appropriate to infer (as in Modigliani, 1977, and Laidler, 1990, did, in an interpretation affirmed by Rivet, 2012) that Friedman’s various references to a prices-to-output channel signified a belief in flexible prices or a New Classical supply function or that these references were inconsistent with a belief in price stickiness.}
policy can hold inflation $\pi_t$ down by holding down expected-inflation $E_t \pi_{t+1}$ for the period in which (log) real marginal cost $rmc_t$ remains elevated. In terms of the $\pi_t = \alpha E \Sigma_i = 0 \beta rmc_{t+i}$ version of the equation, it amounts to saying that the monetary restriction in the presence of increased real marginal cost is able to stop the forward-looking sum $\alpha E \Sigma_i = 0 \beta rmc_{t+i}$ from rising. The insensitivity of this sum to shocks to current marginal cost shows that Friedman believed that transitory, and not just highly persistent, shocks to wages were important.185

Friedman’s suggestion that the markup rather than current labor cost might, on occasion, rise in the short and medium runs in response to monetary policy easings can likewise be put in New Keynesian terms. In terms of the $\pi_t = \alpha rmc_t + \beta E \pi_{t+1}$ expression, it means that monetary ease boosts $E_t \pi_{t+1}$ and hence $\pi_t$. Indeed, in the 1970s and 1980s Friedman repeatedly cited scenarios in which inflation rose in response to an increase in expected inflation before any reaction of wages.186 In terms of the $\pi_t = \alpha E \Sigma_i = 0 \beta rmc_{t+i}$ version of the equation, this amounts to saying that the monetary easing in the presence of unchanged current real marginal cost pushes up the term dated later than $t$ in the forward-looking sum, $\alpha E \Sigma_i = 0 \beta rmc_{t+i}$.

Friedman’s position that monetary policy can affect inflation in the face of considerable looseness in the short-run relationship between wage growth and inflation has gained considerable empirical support. Gowland (1983, p. 114) suggested that “prices rose less than costs” across economies in the mid-1970s because aggregate demand policies became more restrictive during those years, while Laidler (1982, p. 129) judged that “most studies show that the ratio of prices to money wages does seem to vary with market pressures.” It is also how officials have characterized how monetary policy affects inflation. For example, in Australia, Grenville (1995, p. 209) suggested that monetary tightenings “reduce [economic] activity and create an ‘output gap’… Inflation responds to this output gap, both directly and through the indirect effect on wages.”

The notion that nominal wage growth and price inflation might exhibit considerable looseness in their relationship has also received greater acceptance. For example, Obstfeld (2019, p. 2) states that the accrual of U.S. postwar data points to the conclusion that the inflation process has become “ever less dependent on domestic wage inflation.” Obstfeld also insists that this change

---

185 Carroll and Crawley (2017, pp. 84–85) argue that Friedman’s insistence on the presence of transitory shocks has been inadequately incorporated into the empirical analysis of consumption behavior. The same may be true in the analysis of marginal-cost behavior, particularly with regard to the connections between this behavior and the course of inflation and monetary policy.

186 See, for example, Friedman (1976b, p. 228) and Friedman and Schwartz (1982, p. 446).
does not compromise “a central bank’s ability to control the price level over the long term” (2019, p. 2).

Nordhaus and Cambridge University monetary economics

The public rebuttal, discussed above, that Friedman wrote to Nordhaus’ (1972a) challenge to his views on inflation came about because Nordhaus’ results had been seized upon by Nicholas Kaldor. In the U.K. press (The Times, April 6, 1977), Kaldor had cited Nordhaus’ work in support of his own position (already noted in Section II above) of hardline Keynesianism.

What might have made Kaldor think of Nordhaus’ work as being more supportive to his case than it actually was the fact that Nordhaus was one of the few U.S. macroeconomists of his generation to establish strong links with macroeconomists at Cambridge University—both Keynesians of an older generation like Kaldor and younger, but still hardline, U.K. Keynesians such as Wynne Godley. However, this interaction occurred against a background of Nordhaus’ continuing skepticism about the U.K. Keynesian position. Nordhaus recalled: “I visited Cambridge in 1970–71 and had many good friends [holding] the hard-core Keynesian position that money does not matter (such as Kaldor or Godley). I thought they were stuck in the 1930s and hadn’t learned the basic propositions of modern monetary economics.” (William Nordhaus, personal communication, December 8, 2014.)

During Nordhaus’ spell at Cambridge University, he started a project with Godley on the relationship between prices and costs in the United Kingdom. Early output on this research came in the form of Godley and Nordhaus (1972). But some indication of the divergent perspectives of Godley and Nordhaus is indicated by the fact that they were not able to finalize the ultimate output of their project—a book with Kenneth Coutts titled Industrial Pricing in the United Kingdom—until June 1977, after more than five years of drafting (Coutts, Godley, and Nordhaus, 1978, p. vii). As the authors put it, their main finding was that “industrial prices (relative to costs) are impervious to aggregate demand… The results indicate that, if demand affects industrial prices, it does so only through factor prices…”

This common stand among the authors on the insensitivity of the markup to aggregate demand contrasted with the dissension among them regarding whether wages were sensitive to the output gap. They disclosed this dissension in the book: “in the opinion of the two British authors [that

\[187\]Coutts, Godley, and Nordhaus (1978, p. 73).\]
is, Coutts and Godley], the connection between aggregate demand and the rate at which money wages change in the United Kingdom is at best a weak one” (Coutts, Godley, and Nordhaus, 1978, p. 73).

This passage highlighted the differences among monetarists like Friedman, U.S. Keynesians like Nordhaus, and U.K. Keynesians of the time like Godley. For Friedman, as noted above, aggregate demand affected both the markup and costs. For Nordhaus, it was the case that both in the United Kingdom and in the United States the markup did not respond to aggregate demand, but nominal wages did do so. U.K. Keynesians, in contrast, saw both the markup and nominal wages as insensitive to resource slack and inflation as a pure cost-push phenomenon. Godley expounded this position in work in the 1970s that challenged the monetarist view of inflation (Fetherston and Godley, 1978). Though labeled “New Cambridge,” this line of work by Godley reaffirmed the older Cambridge Keynesian position that inflation was insensitive to slack and therefore incapable of being controlled by monetary policy. Kaldor likely interpreted both the Godley-Nordhaus research on costs and Nordhaus’ (1972a) work on nominal wage growth as consistent with his own pure cost-push views.

Nordhaus, however, did not interpret his work this way and felt that Kaldor and Nordhaus were wasting their abilities by taking an unduly extreme anti-monetarist position. However, as already indicated, he also felt that Friedman’s monetarism was itself an undesirable extreme: “In one sense the Cambridge England and Friedman camps deserved each other.” (William Nordhaus, personal communication, December 8, 2014.)

Although he believed that monetary policy could affect nominal wage growth and inflation, Nordhaus felt that Friedman unduly discounted both the role of fiscal policy in affecting aggregate demand and the importance of cost-push factors affecting wages and prices. This perspective would be reflected in the twelfth edition (1985) of the Paul Samuelson economics

---

188 For his articulation of this position in the U.S. case, see Nordhaus and Shoven (1974, p. 16).
189 The book of Coutts, Godley, and Nordhaus (1978) book was itself reasonably neutral in its references to monetarism and to Friedman (other than referring to him simply by his surname in the index: see p. 145). The authors acknowledged (p. 65) the validity of Friedman’s (1975f) point that a markup equation could be consistent with expectational Phillips-curve explanations for inflation, and they also granted that a constant markup might be consistent with monetarist explanations for price inflation, provided that wages responded to monetary actions (p. 73).
190 The New Cambridge view was criticized by Hall (1978a) on this ground.
191 Nordhaus’ position was therefore close to the “partial cost-push” position of U.S. Keynesians described in Nelson (2018a, Chapter 10). Nordhaus’ belief in both aggregate demand policy and cost shocks as important factors affecting nominal wage growth was evident in Nordhaus (1972a, 1972b), and the weight he gave to fiscal policy,
textbook, for which Nordhaus was a coauthor. The inside front cover of the textbook depicted a “family tree of economics,” with monetarism (both its Friedman and 1970s rational-expectations variants) categorized as on a different branch from “Modern Mainstream Economics” (see Samuelson and Nordhaus, 1985).

monetary policy, and debt management as influences on aggregate demand was clear from Nordhaus and Wallich (1973).
I. EVENTS AND ACTIVITIES IN DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1975–1976

The behavior of U.S. macroeconomic data in the mid-1970s set the seal on what King and Watson (1994, p. 209) described as “the general increase in the difficulty of forecasting in the post-1970 period.” The restoration of more placid conditions during 1972—with a shift down in inflation and unemployment rates, together with a return to positive real short-term interest rates—had proved ephemeral. It had reflected the combination of a residue of prior—but abandoned—monetary restraint, real effects of the more-recent rapid monetary expansion, and the imposition of wage/price controls.

In contrast, the years immediately following 1972 saw multiple developments tending to magnify U.S. economic fluctuations: the abolition of controls, the prior period of monetary expansion leaving aftereffects of higher inflation and interest rates, and a shift to monetary restriction that bore adversely on aggregate output and unemployment. Together with developments in monetary aggregates and in the financial system that are discussed later in this chapter, the rough-and-tumble behavior of interest rates, output, inflation, and the unemployment rate after 1972 amply reinforced Friedman’s observation of early 1974: “This is a very unusual set of circumstances that we are facing now.” (Instructional Dynamics Economics Cassette Tape 139, February 4, 1974.)

These gyrations continued as the decade of the 1970s entered its second half. Against this backdrop, in 1975 a financial-advice columnist, when asked to comment on an investment that had started in 1968, remarked that seven years would normally be a sufficient time to reach a judgment on an investment. But that was not the case in this instance, he said, because “this has
been a ‘normal’ seven-year period in about the same way that the Grand Canyon is your ‘normal’
ditch.” (Washington Star (Washington, D.C.), April 1, 1975.)

Casting blame for the recession

As 1975 ended its first week, financial columnist Joseph Slevin told his readers that in
commentary on the current U.S. recession, they should expect to hear much on the theme that the
economic downturn was produced by the monetary authorities. “The alleged culprit is the
Federal Reserve System’s tight money policy,” Slevin wrote, “and more will be heard of the
charge in coming months.” Slevin noted the fact that a range of critics had concluded “that the
Federal Reserve has waited too long and done too little and that, thanks to the Federal Reserve
credit squeeze, the ongoing recession was ‘Made in Washington.’” (American Banker, January
7, 1975.)

Although Slevin’s column did not name him, Friedman was one of the prominent critics taking
this position—as a string of commentaries he would make in early 1975 would underline.
Federal Reserve actions, he said in a February 1975 television appearance, had put the United
States “into a more severe recession than we need to have.”

Slevin contrasted the diagnosis that Federal Reserve actions had produced the recession with the
position articulated by Ford Administration personnel “that the primary cause of the recession
was inflation and its slashing impact on the buying power of the public’s dollars.” Friedman was
receptive to a version of this explanation. “The recession is a result of the inflation; we mustn’t
suppose they are independent phenomena,” he remarked in the February 1975 television
interview. However, the narratives coming from officialdom gave the impression of an
exogenous increase in inflation occurring in the context of broadly appropriate rises in nominal
aggregate demand and being responsible for negative growth in real demand and output. In this
account, therefore, stabilization policy provided about the right course for nominal demand, and
it was events outside economic policy’s control produced the poor outcomes for output and
inflation. Friedman, in contrast, blamed policymakers for both inflation and recession.

Friedman did grant that special factors like food and fuel price increases had indeed raised
inflation to some degree in 1973 and 1974 (see the previous two chapters) and that these had had

---

2 Wall Street Week, Maryland Public Television, February 7, 1975, p. 15 of transcript.
3 Wall Street Week, Maryland Public Television, February 7, 1975, p. 15 of transcript.
the negative effect on purchasing power stressed by officials. He also acknowledged that the abolition of wage and price controls in 1974 had temporarily made both inflation and output worse: the controls’ removal, he noted about a decade later, “produced a rapid acceleration in inflation which was accompanied by a decline in real income.”

Friedman and Schwartz’s 1982 *Monetary Trends* analysis suggested that the lifting of controls amplified the recession but that output would still have declined during 1973–1975 in the absence of this event. In particular, using historical short-run relations between real and nominal variables, he and Schwartz transformed the recorded U.S. output patterns from 1971 to 1974 to adjusted series that could be interpreted as how the U.S. economy would have behaved if controls had never been imposed at any time, but aggregate demand policies had been the same as those historically observed. On their reckoning, controls had made the aggregate supply curve flatter than otherwise in 1972 and 1973 and steeper than otherwise in 1974, and that, under a controls-free scenario, U.S. aggregate output might have been flat instead of declining in 1974, though it would still have fallen in 1975.

But predominantly, of course, Friedman attributed the very high inflation of 1973–1975 to prior monetary excess rather than special factors. Consequently, even when stressing the likelihood of an inflation/recession link, he was disinclined to attribute the recession primarily to exogenous events. Furthermore, Friedman did not see the rise in U.S. inflation in 1973 as making a recession inevitable, in the sense that a recession was implied by the dynamics of the economy’s structure. On the contrary, his long-held position was instead that inflation or inflationary booms did not themselves necessarily give rise to recessions. Certainly, as the community accustomed

---

4 Friedman (1985, p. 57).
5 See Friedman and Schwartz (1982, p. 108). This is one interpretation of their adjustment. A different one is that the Nixon controls were purely cosmetic and merely distorted the recorded prices and output indices until controls were removed; that is, that the 1971–1974 U.S. controls experience was well characterized by Friedman and Schwartz’s (1963a, p. 558) summation of the World War II price controls, which involved suppressed increases in posted prices alongside de facto price adjustment “not recorded in the [official] indexes.”

Passages of Friedman and Schwartz (1982, especially p. 82, fn. 9) and their other writings seemed to apply this interpretation to other price-control periods and to the Nixon control program in particular. However, this characterization was likely dictated by a wish to provide a symmetric perspective on all the control episodes throughout the century of U.S. and U.K. data that *Monetary Trends* covered. The balance of Friedman’s writings on the Nixon controls did interpret them as having had, at least through sometime in 1973, a bona fide effect of making the short-run supply curve for U.S. goods and services flatter than otherwise: that is, as having given rise to genuine increases in output beyond those that would otherwise have not occurred, under the same setting of aggregate demand policies, and of conversely keeping down, for the duration of controls, actual inflation below its non-controls rate. That controls could have these properties under Nixon’s program is consistent with controls being more cosmetic in their effect on price indices during World War II. The wartime controls were associated with rationing and restrictions on production in the nondefense sector, and so the controlled prices in that era were not prices on which consumers or firms could make quantity decisions.

6 See, for example, Friedman (1964, p. 17; 1972a, p. 936).
itself to a higher rate of inflation, the initial output-boosting effects of a shift to a more inflationary policy would recede; but output need not fall (or take below-normal values) as part of this process.

In practice, however, Friedman did see recessions as following inflationary periods because there would be a public reaction against higher inflation and, partially recognizing that excess demand was a source of inflation, the authorities would rein in aggregate demand: “you can’t keep an inflation going indefinitely; people don’t want [it, so] you try to stop it.”\textsuperscript{7} It was this policy reaction, in particular the FOMC’s tightening beginning in the first half of 1973, to which Friedman attributed the recession that had begun in late 1973. As discussed in Chapter 2, he believed that the first year of the tightening was appropriate and of about the right magnitude; but he found fault with what he perceived as a too-severe further tightening in the middle quarters of 1974.

In a press interview given a week after his television appearance, Friedman observed that he had “just constructed a table [with] which I am going to soon blast the Fed” in his \textit{Newsweek} column (\textit{Journal of Commerce}, February 25, 1975, p. 3). This table duly appeared in Friedman’s bluntly-titled \textit{Newsweek} column of March 10, 1975, “What Is the Federal Reserve Doing?”\textsuperscript{8} After cataloguing the excessive monetary growth of 1971–1974, his column turned to the recent downturn in monetary growth. The period on which Friedman focused, July 1974 to January 1975, predated that of Congressionally-required targets for monetary growth (see Section II below). However, the FOMC already had published targets for M1 and M2 growth, as well for the federal funds rate: “Each month it publishes its [monetary] objectives,” he had noted in the February 1975 television interview.\textsuperscript{9} Drawing these from Federal Reserve publications, Friedman’s table and the accompanying discussion made a point that monetarists would articulate repeatedly in considering U.S. monetary policy in the 1970s: the Federal Reserve had systematic misses of its targets for monetary growth—M2 growth, for example, was never in its official tolerance range for the six months through early 1975 in the period—yet the federal funds rate targets were either met or missed by small amounts.\textsuperscript{10} The FOMC’s success in hitting the federal funds rate target had been “at the cost of achieving its monetary growth target.”

\textsuperscript{7} \textit{Wall Street Week}, Maryland Public Television, February 7, 1975, p. 16 of transcript.
\textsuperscript{8} In a separate \textit{Newsweek} item in the same issue (March 10, 1975b), Friedman was quoted as saying that the Federal Reserve’s policies were “terrible” (p. 60).
\textsuperscript{9} \textit{Wall Street Week}, Maryland Public Television, February 7, 1975, p. 17 of transcript.
As he was preparing the column, Friedman remarked: “In essence, what the Fed is telling us is that they want to stretch out the decline in interest rates over as long a period as possible, so that a rebound in rates can be avoided. But this means they must restrict the money supply to keep rates from falling. This, in turn, means that the recession will be stretched out over a longer period, and the rate of unemployment will climb higher than if they would let the money supply grow at their target rates.” (Journal of Commerce, February 25, 1975, p. 3.)

Friedman’s Newsweek column also noted that the Federal Reserve had recently been downplaying its scope to influence the money stock. But he argued that these denials should be discounted; and, as discussed in Chapter 2, in early 1975 Federal Reserve Board Governor Henry Wallich had in effect conceded that the FOMC could push up monetary growth if it were willing to permit the required federal funds rate fluctuation.11 Similarly, Chairman Burns remarked in February 1975 that the Federal Reserve could raise monetary growth and was now taking steps to do so “Forces have now been set in motion that will, I believe, soon result in a quicker pace of monetary and credit expansion.”12 Friedman, as discussed below, thought that the Federal Reserve exaggerated the amount of interest-rate variability that a more-stable pattern of monetary growth would entail. But he conceded that securing such a money-stock trajectory would indeed imply some additional degree of interest-rate variation in the short run.

Such variation would be desirable, Friedman maintained in early 1975, in order to pursue the open market operations necessary to get monetary growth going again. In the absence of a pickup in monetary growth, his column observed, there was a danger of “a major contraction” in output (Newsweek, March 10, 1975a).

Friedman’s column did not paint a flattering picture of the Federal Reserve’s leadership: but it refrained from criticizing Arthur Burns by name. In contrast, in his own Newsweek column a week earlier, Paul Samuelson named names. A few years later, in recalling how the “Fed’s performance during the recession led to a criticisms from a wide spectrum of economists,” Wonnacott and Wonnacott (1979, p. 320) noted that “Paul Samuelson wondered if the economy

---

11 Likewise, in Congressional testimony on March 13, 1975, Chairman Burns acknowledged that an injection of reserves under present circumstances would lead to an expansion of aggregate credit extended by commercial banks (Committee on the Budget, U.S. Senate, 1975, p. 842). He suggested that, under the circumstances then prevailing (in which commercial banks were hesitant about expanding their loan portfolios, as discussed in Section II below), investments in securities instead of private-sector loans would take a larger share than usual of the increase in commercial bank credit. Such an eventuality would not have prevented the money stock from expanding appreciably in response to the policy-induced increase in reserves; and, as discussed presently, Burns acknowledged this at around the same time as this testimony.

was headed into ‘A Burns Depression?’” Samuelson’s column of that title (March 3, 1975) took Burns to task for the tightness of monetary policy since mid-1974. On another occasion in early 1975, Samuelson speculated about a time when “Milton Friedman and Anna Schwartz come to write their history of the crimes of 1974 and 1975” (New York Times, February 26, 1975) should they write a new volume of their monetary history (which, of course, they never did; Friedman had said in 1964 that they had “no present plans for doing any further work on the qualitative historical evidence,” and, in the event, they never jointly resumed such work).13

Apart from his association of the recession with FOMC decisions, Samuelson’s basis for raising the prospect of a Burns depression was that he saw the recession as unlikely to bottom out before October/November 1975, and for national unemployment to peak above 10 percent during the recession or in its wake (Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 158, late February/early March 1975). “I submit that a sensible usage would be to define any recession that involves [unemployment of] more than 10 percent of the labor force as a depression,” Samuelson wrote (Newsweek, March 3, 1975, italics in original).14

It is unlikely that Friedman would have concurred with this definition. He had long been doubtful of the unemployment rate as a cyclical indicator; and, as discussed below, his doubts were reinforced in the 1975–1976 period by emerging changes in the relationship between the unemployment rate and indices of both employment and production. But, as already indicated, in the early months of 1975 both Friedman and Samuelson feared a further decline in U.S. aggregate output over the course of the year, with Samuelson observing, “there is still nothing to be optimistic about. The economy continues to slide, and the bottom is not yet in sight.”15

Friedman remarked in his appearance at Claremont College, California, on February 21, 1975: “As for the rate of unemployment, the odds right now are better than 50-50 that it will hit 10 percent soon—in the second or third quarter of this year.” He added that he saw “no reason to predict an end to the recession,” as a notable monetary expansion had yet to occur, and an economic turnaround would likely not occur until two to three quarters after that event (Journal of Commerce, February 25, 1975, p. 1; also in Participant, April 1975).

Recovery begins

As things turned out, recovery soon began after these statements of concern were made.

---

14 In the event, the unemployment rate stood above 10 percent in only one episode in the postwar period: September 1982 through June 1983. It reached, but did not exceed, 10 percent for one month (October) in 2009.
15 Instructional Dynamics Economics Cassette (Paul Samuelson series) Tape 158, late February/early March 1975.
Consequently, the unemployment rate did not reach the 10 percent-plus rate that Friedman and Samuelson feared it would reach in late 1975: instead, it peaked at 9 percent in May 1975 (Figure 1). The second quarter of 1975 likewise saw growth in aggregate output turn positive, and the NBER would date the trough of the 1973–1975 recession to March 1975. As of the end of the 1970s, U.S. quarterly national accounts data showed that the peak-to-trough fall in output (real GNP) had been 5.7 percent (B.M. Friedman, 1980, p. 12). Data revisions and redefinitions would moderate this decline greatly: modern real quarterly GDP data show an output fall of 3.1 percent over the course of the recession. However, in modern data on annual averages of real GDP, it remains the case that output fell in both 1974 and 1975—by −0.5 percent in 1974 and −0.2 percent in 1975—the only U.S. postwar recession other than that of 2007–2009 to include back-to-back declines in the annual-average data on total output.

Friedman, using M2, dated the trough in monetary growth to January or February 1975. So a pickup in monetary growth began only one or two months ahead of the beginning of the recovery—which, as Friedman acknowledged (Instructional Dynamics Economics Cassette Tapes 174, August 1975, Part 2, and 181, November 1975, Part 2), implied an unusually short lag. The modern series on M2—broader than the M2 definition that Friedman was using at the time—gives somewhat more indication of a standard lag length, as it implies that the quarterly annualized rate of growth of nominal money troughs in 1974:Q3 at 4.4 percent, before rising to 6.3 percent in 1974:Q4, 7.9 percent in 1975:Q1, and 15.5 percent in 1975:Q2. Thus, these data show a lag of two quarters from the monetary turnaround to the nominal-income turnaround.

Wallich (1975, pp. 4–5; 1977, p. 286) would offer a different interpretation of the course of U.S. monetary variables observed in 1974–1975. He argued that the episode was one in which interest rates outperformed monetary growth as an indicator. In particular, Wallich contended

---

16 See also the discussion titled “Arthur Okun” in Section III below.
17 Instructional Dynamics Economics Cassette Tape 170, June 1975, Part 3; Instructional Dynamics Economics Cassette Tapes 174, August 1975, Part 2, and 181, November 1975, Part 2; Newsweek, October 3, 1977. Brunner (175, p. 3) also cited February 1975 as the date for the trough in money (using M1, in contrast to Friedman’s focus on M2). Such dating is broadly consistent with Dornbusch and Fischer’s (1978, p. 537) observation that monetary policy was “quite restrictive until the second quarter of 1975.” Meltzer (1986, p. 450), in contrast, gave monetary growth as having a six-quarter lead over the 1975:Q1 economic trough. But this was a highly implausible and awkward dating procedure, as it required assigning the trough in monetary growth to a very early, pre-recession point (1973:Q3) and overlooking the late 1974/early 1975 monetary trough.
18 At the time (in Instructional Dynamics Economics Cassette Tapes 172, July 1975, Part 2, and 174, August 1975, Part 2), Friedman put the output trough at 1975:Q2 rather than 1975:Q1 because real GNP (on initial data) was flat in the second quarter—implying a still-short lag of about four months from monetary growth to output growth.
19 By 1975, lag estimates such as these had received such widespread acceptance that even Walter Heller could remark that “we know, from studies by Milton and others, it takes six to nine months” for monetary policy shifts to show up substantially in economic activity (March 12, 1975, testimony, in Joint Economic Committee, 1975c, p. 1040).
that nominal interest rates were highest in the first half of 1974 and the economic downturn was most severe in late 1974; and interest rates plunged, then troughed roughly six months ahead of the 1975 economic turnaround. Consequently, Wallich argued that, in this episode, interest rates provided the six-month lead over economic activity that Friedman frequently ascribed to monetary aggregates. Furthermore, noting that nominal interest rates and monetary growth were giving opposite signals during 1974 and early 1975, Wallich suggested that interest rates outperformed growth in money as an indicator of future economic activity over this period.

Wallich’s critique underscores the point that both nominal interest rates and money gave a “tight money” signal at some time during 1974—but at different parts of the year (in the first half for interest rates, in the second half for monetary growth). In addition, Wallich was correct to suggest, on the basis of the data available to him at the time, that the lead of monetary growth over the start of the 1975 economic recovery was well below six months. Wallich’s critique of

---

20 Thus, a later account by Joseph Slevin (American Banker, October 23, 1978a) could refer to “the excruciating tight-money squeeze” of 1974, without needing to be specific about whether monetary growth or interest rates were being used as the criterion for evaluating tightness. Friedman’s analysis of the 1974-1975 monetary squeeze was like that he gave of developments in 1966 the feature that Friedman initially doubted that there was a squeeze because in the initial part of the year interest rates rose but monetary growth did not undergo a severe decline.
money and his support for interest rates are, however, questionable in other respects. Although they rose in *nominal* terms, it is not obvious that interest rates rose in *real* terms in the first half of 1974; certainly they did not on an *ex post* basis. This is essentially why the Taylor (1993) rule prescription for the federal funds rate moves well above the actual federal funds rate in early 1974 and then stays there throughout the rest of the year.\(^{21}\) It is consequently not obvious that this period’s interest-rate behavior—which is discussed further below—accurately pointed to an intensification of the downturn in U.S. real economic activity. In contrast, monetary growth in early 1974, while lower than later in the year, did represent a step-down from prior rates (see Table 1). And this pattern is more pronounced for the modern definition of M2.

Finally, as noted above, if monetary growth is seen as particularly being a harbinger of future developments in *nominal* income growth, monetary growth (especially the movements in M2) did not obviously fail over the 1974–1975 period as an indicator of subsequent developments in income. The growth rates of the two series were in broad alignment over time—a fact discussed in Chapter 2 and amplified by Table 1 below. In addition, M2 growth had a lead over nominal GDP growth in this period—more clearly so in the case of the modern definition of M2 than for the 1970s M2 definition.\(^{22}\)

The factor making the short-run relationship between nominal money and real output somewhat disturbed over these years was not a break in the money/nominal income relationship but, instead, the behavior of inflation. Inflation’s sharp (further) rise in late 1974, along with its distinct decline the following year, made the short-run movements of nominal GDP and real GDP more disparate than usual, and some of the downturn/recovery pattern in 1974–1975 can be considered a reflection of the shifting shares of real GDP growth and inflation within nominal GDP growth. The 1975 upturn was another case of the phenomenon of money having a shorter

---

\(^{21}\) This is shown in Taylor (1999, p. 337) and in the final-data Taylor rule prescriptions for 1974 shown in Orphanides (2003, p. 649). Monetary policy was therefore consistently too loose throughout 1974 when judged in retrospect by the Taylor-rule criterion. In contrast, Friedman, using monetary growth, suggested, instead, that monetary policy in the second half of 1974 was too tight. The difference may reflect the fact that the Taylor rule allows only indirectly for variations in the natural real interest rate. Indeed, one motivation for a concentration on monetary growth as an index of monetary policy, rather than constructs based on observed interest rates, is that variations over time in the natural rate of interest are substantial. In addition, as already stressed in Chapter 2, Friedman’s complaint about the 1974 monetary policy shift partly rested on its suddenness (*Wall Street Journal*, August 21, 1975); the monetary growth rates to which the shift led were not below rates he likely regarded as slightly too high in the long run. In that respect his critique of 1974 policy is consistent with the Taylor rule’s message that policy throughout 1974 was too loose, but not with the notion that the degree of looseness was roughly the same over the year.

\(^{22}\) In monthly data, the trough in the twelve-month growth rate in nominal personal income trough is in July 1975; the 12-month growth rate trough for modern M1 is in February 1975, with that rate recorded again in April. The twelve-month growth rate of modern M2 troughs in January 1975, six months ahead of the nominal personal income trough.
Table 1. Developments in monetary and economic aggregates, 1974 and 1975

<table>
<thead>
<tr>
<th>Period</th>
<th>Nominal GDP</th>
<th>Real GDP</th>
<th>Old M1</th>
<th>Old M2</th>
<th>Modern M1</th>
<th>Modern M2</th>
</tr>
</thead>
<tbody>
<tr>
<td>1974:Q1</td>
<td>8.3</td>
<td>0.6</td>
<td>5.9</td>
<td>8.9</td>
<td>5.3</td>
<td>6.3</td>
</tr>
<tr>
<td>1974:Q2</td>
<td>8.2</td>
<td>−0.2</td>
<td>5.6</td>
<td>8.7</td>
<td>5.1</td>
<td>5.9</td>
</tr>
<tr>
<td>1974:Q3</td>
<td>8.8</td>
<td>−0.6</td>
<td>5.3</td>
<td>8.3</td>
<td>4.8</td>
<td>5.5</td>
</tr>
<tr>
<td>1974:Q4</td>
<td>8.4</td>
<td>−1.9</td>
<td>5.1</td>
<td>7.7</td>
<td>4.8</td>
<td>5.8</td>
</tr>
<tr>
<td>1975:Q1</td>
<td>8.4</td>
<td>−2.3</td>
<td>3.6</td>
<td>6.6</td>
<td>3.7</td>
<td>5.8</td>
</tr>
<tr>
<td>1975:Q2</td>
<td>8.0</td>
<td>−1.8</td>
<td>4.1</td>
<td>7.3</td>
<td>4.3</td>
<td>8.4</td>
</tr>
<tr>
<td>1975:Q3</td>
<td>9.6</td>
<td>0.8</td>
<td>4.8</td>
<td>8.3</td>
<td>5.3</td>
<td>11.2</td>
</tr>
<tr>
<td>1975:Q4</td>
<td>10.1</td>
<td>2.6</td>
<td>4.4</td>
<td>8.3</td>
<td>4.8</td>
<td>12.1</td>
</tr>
</tbody>
</table>

Source: In the case of real GDP, nominal GDP data are quarterly series in the Federal Reserve Bank of St. Louis’ FRED portal. In the case of modern M1 and M2, growth rates from quarterly averages of the monthly data in the FRED portal. Quarterly levels data in Lothian, Cassese, and Nowak (1983) are used to calculate the growth rates reported in the table for the old (pre-1980) M1 and M2 series.

lead over real than nominal income—a regularity that Friedman had already highlighted in the early 1970s as a finding emerging from his monetary studies. Correspondingly, another way of describing U.S. economic developments in the mid-1970s is that the period saw real monetary growth undergo a more severe squeeze in 1974 than that experienced by nominal money. Concerns about an overreaction

Friedman indicated in the early months of 1975 that he objected to the “over-restrictive policy” of the Federal Reserve not only because it was worsening the recession but also on the grounds that it would likely, in due course, trigger an overreaction of monetary policy in the opposite direction—a policy move that would see monetary growth permitted to move up into a range so high that inflation would eventually become worse (Newsweek, March 10, 1975a). Along these lines, he remarked on television that “the big danger now is... that in reacting to this recession, we’ll again overreact, pour the money in, print money like mad, and get off on another inflationary binge. I think that’s the real danger.”

23 See Nelson (2018a, Chapter 6).
24 Paul Samuelson focused on the tightness “of real money” in his Newsweek column of March 3, 1975, and in Samuelson and Temin (1976, p. 329).
25 Wall Street Week, Maryland Public Television, February 7, 1975, pp. 16–17 of transcript. See also the accounts in Oakland Tribune (January 13, 1975), Los Angeles Times (February 24, 1975), and Journal of Commerce (February 25, 1975, p. 3), of remarks Friedman made along the same lines in Indianapolis (in January) and Claremont, California (in February).
In expressing this fear, Friedman was repeating a theme he had laid out since the early 1950s and that he believed well described previous FOMC moves in 1962 (after the economic pause of that year), 1967 (in response to the mini-recession), and 1971 (after the seemingly-slow recovery from the 1969–1970 recession. Indeed, even in late 1974 Friedman had expressed fears of a Federal Reserve overreaction, and he had worried in December of that year that a surge in monetary growth in October 1974 was the beginning of a move to excessive rates. These early fears were temporarily superseded by the monetary data through early 1975, which suggested that monetary restriction was intensifying.

The turnaround in M2 growth in 1975, however, proved so sharp that over much of the year Friedman thought the Federal Reserve was overdoing stimulus and warned about the prospect that this would give rise in coming years to a resurgence of inflation. An early commentary to this effect was reported during a visit Friedman made in Japan in April 1975, when he argued that continuation of the present pace of monetary growth could lead to 20 percent inflation in 1977 (Omaha World Herald, April 23, 1975). In August, he again complained that monetary growth was excessive (Business Week, August 11, 1975), and in December he was reported as seeing inflation moving into a 12–15 percent range as early as the latter part of 1976 (Mexico Ledger (Missouri), December 5, 1975; Kansas City Times, December 5, 1975).

As Table 1 shows, monetary growth did pick up over 1975. Friedman’s resulting prediction (Omaha World-Herald, April 23, 1975) that a strong economic recovery would occur that year was vindicated and contrasted with various predictions from others in 1975 that the U.S. recovery would not be V-shaped. However, to Friedman’s surprise M2 growth settled down, albeit at double-digit annual growth rates, in the fourth quarter of the year, and in early 1976 he acknowledged that monetary growth had not been as high as he had been expecting (Instructional Dynamics Economics Cassette Tape 183, January 1976, Part 1). Indeed, as discussed in Section III below, Friedman saw the cooling-off in monetary growth in late 1975 that produced this more-moderate course for the twelve-month rate as bearing importantly on the outcome of the 1976 U.S. presidential election.

---

26 For these warnings, see Newsweek, November 4, 1974, and Friedman (1975c, p. 178).
27 These remarks were also reported on CBS Morning News (April 22, 1975, p. 21 of transcript) and in Time magazine, May 5, 1975, p. 70. See also Friedman’s remarks in Instructional Dynamics Economics Cassette Tape 167 (May 1975, Part 1).
28 For the predictions from economic commentators of a slow recovery, see, for example, Time magazine (May 5, 1975, p. 69) and Franco Modigliani’s remarks in Business Week (August 11, 1975), as well as the discussions in López-Salido and Nelson (2010) and in Section II below.
However, though Friedman’s warnings in 1975 of double-digit inflation in 1977 exaggerated the degree of easing by the FOMC, his relief in the first months of 1976 that monetary growth had largely been contained would prove premature. It would eventuate that his fears of an overreaction by the authorities in the wake of the 1973–1975 recession did not turn out to be far off. Monetary growth, measured by the behavior of the 1970s definition of M2, was higher in 1976 and 1977 than it had been in 1975. As this surge started to emerge in mid-1976, Friedman declared in his column: “We are currently in the midst of a monetary explosion…” (Newsweek, June 14, 1976). This became the second monetary explosion of the 1970s—an event discussed in detail in Chapter 6. And whereas, for the year to March 1976, M2 growth came in at approximately the midpoint of the FOMC’s 8½–10½ percent target band, for the year to 1976:Q4 M2 growth was 10.9 percent—outside the FOMC’s target range of 7½–10½ percent (Argy, Brennan, and Stevens, 1990, p. 54).29

Furthermore, although this was a period in which patterns in broad monetary aggregates were particularly good indicators of future behavior of inflation, the M2 growth data available in the mid-1970s understated the inflationary pressure in store for the U.S. economy for two reasons. First, thrift institutions’ deposit liabilities, which were outside the M2 definition of the time, were growing strongly, so M2 growth as measured in 1975 and in 1976–1977 (the latter period being that of the second monetary explosion) was consistently below that for modern M2 growth.30 Second—though would not become clear for several years—the post-1973 productivity slowdown had lowered the growth rate of potential GDP. The latter factor alone meant that the noninflationary rate of monetary growth after 1973 was about 1 percentage point lower than previously, moving down from about 3.5 or 4 percent in the 1960s to about 2.5 or 3 percent after 1973. This slowdown plays a large role in explaining why, as shown in Orphanides (2003, 2004) and discussed further in Section III, estimates of the output gap during 1975 reached double-digit negative values, even though modern data on the output gap—obtained using real GDP together with Congressional Budget Office estimates of potential as of 2019—the output gap got no deeper than minus 5 percent during 1975.31

29 Argy, Brennan, and Stevens (1990) gave M2 growth in the year to March 1976 as 9.6 percent; the Lothian, Cassese, and Nowak (1983) quarterly dataset implies a rate of 9.4 percent. Both sources register agreement on M2 growth being 10.9 percent in the year to 1976:Q4, and Bernanke and Mishkin (1992, p. 190) also gave this as the four-quarter rate for that period.

30 Simpson (1980, p. 103) showed that while new M2 growth in 1960–1979 as a whole only exceeded that of old M2 growth by 0.7 percentage points per annum on average, this excess rose to 1.5 percentage points for the period 1975:Q1–1979:Q4.

31 As discussed further in Section III below, the output gap using these modern estimates give a trough in the 1970s of the output gap of −4.7 percent, in 1975:Q2.
The 1975 income tax rebate

Friedman’s concern that the 1974–1975 recession would produce an overreaction in the form of a shift to excessively expansionary aggregate-demand policies had stemmed partly from the fact that many other leading economists had indeed been urging a major reversal of policy settings. In many cases, these recommendations went beyond winding back the late-1974 tightening of monetary policy on which Friedman focused. A notable illustration of this came at the White House economists’ summit—held in early September 1974, a time when Friedman’s position was still that monetary policy was not yet too tight. In describing the proceedings’ discussions, Arthur Okun characterized Friedman as an exception to the conference consensus that current monetary policy was in a setting of “extreme stringency.”

James Tobin, who was not among the conference attendees, shared the judgment that U.S. monetary policy had been too tight throughout the period since the first oil shock. On the occurrence of that shock, Tobin, as discussed in Chapter 3, had suggested that the U.S. authorities should accommodate its effect on the aggregate price level. He characterized the actual policy response by the Federal Reserve as not following this advice and, instead, going in the opposite direction: reacting in an overtly deflationary manner to the OPEC measure. By worsening the country’s terms of trade, the oil shock would have tended to reduce U.S. real income in any event, Tobin granted. But he contended that the restrictive monetary policy response had compounded the reduction in real income (see Tobin, 1977a, p. 57).

It was against this background that in April 1975 Tobin testified alongside Franco Modigliani to Congress’ Joint Economic Committee. At the hearing, both of them advocated not only easier monetary policy but, also, further fiscal expansion. Modigliani, believing the output gap was deep, urged that an aggressive stimulation of aggregate demand was necessary. However, he feared that instead the authorities would “take the Friedman approach—[which is,] use the next 10 years to get back to full employment.” Tobin, in articulating his own case for a loosening of the fiscal and monetary reins, argued that the “recession [was] generated by tight money policy,” while fiscal deficits would “not ‘crowd out’ private uses of credit or force interest rates to levels which prevent or impede economic recovery.”

34 From Modigliani’s testimony of April 24, 1975, in Joint Economic Committee (1975a, p. 34).
35 From Tobin’s testimony of April 24, 1975, in Joint Economic Committee (1975a, p. 36).
Friedman obviously concurred with the Tobin remark on monetary policy and the recession. Furthermore, by 1975, after several years of emphasizing the empirical relevance of crowding out, he was already moving towards Tobin’s position on the matter. By the early 1980s Friedman had become more skeptical about crowding out than was Tobin, and it would instead be Tobin who was stressing the empirical relevance of the pressure on interest rates arising from government borrowing (see Chapter 7 below). But consistently from the 1950s to the 2000s, Friedman opposed policy recommendations that were centered on fiscal activism, like those Tobin and Modigliani were advancing in 1975. Automatic stabilizers aside, Friedman was against altering taxes and government spending for countercyclical purposes. As for monetary policy, at the time of the Tobin/Modigliani Congressional appearance Friedman favored a resumption of moderate monetary growth, with this to be followed by a sequence of gradually declining rates, until the rate consistent with long-run price stability was reached.

As it happened, both monetary and fiscal settings had both in fact already shifted notably by the time the April 1975 Congressional hearing took place. With regard to monetary policy, early 1975 had seen a shift to a higher rate of monetary expansion, as already discussed. And in the area of fiscal policy, Congress passed in late March 1975 an income tax rebate that had been advanced by the Ford Administration as an anti-recession measure (Romer and Romer, 2010, p. 772).

The tax cut consisted primarily of a one-time transfer to households of part of their calendar-1974 tax payments. It was overtly advanced as temporary lowering of taxes rather than a permanent move to new tax arrangements (Poterba and Summers, 1987, p. 386; Romer and Romer, 2010, pp. 772–773). The payment was therefore essentially a windfall affecting the flows of households’ disposable income. The permanent income hypothesis has the implication that there would be little response of consumer spending to such a transitory disturbance to income flows. Indeed, it was on this point that Lydall (1958, p. 564) took Friedman’s *A Theory of the Consumption Function* to task, arguing: “Empirical evidence shows conclusively that people who receive windfalls consume more. Friedman’s attempts to get round this awkward fact are not convincing.”

But it would transpire that the 1975 tax rebate buttressed the permanent-income perspective on consumption behavior. Blinder (1981, p. 27) noted that the U.S. household saving rate surged during the rebate episode, “suggesting that little of the rebate was spent.”

---

36 Lydall’s (1958) review of Friedman (1957) is discussed further in Nelson (2018a, Chapter 5).
The evidence on the rebate’s effect on the behavior of consumption, as given by empirical estimates of the consumption function, largely confirmed the evidence suggested by the surge in the saving rate. Modigliani and Sterling (1986, pp. 1172–1173) found a significant positive effect of the tax rebate on consumption, but only when it was combined with the late-1960s tax surcharge to create a “transitory taxes” variable. Poterba and Summers (1987, p. 387) found that the 1975 rebate entered consumption functions with \( t \)-values less than 2 when considered individually. And significantly, Poterba and Summers found that the 1964 and 1981 tax cuts—which were permanent in character—generated far more clear-cut positive effects on U.S. consumer spending. Later, Campbell and Mankiw (1990, p. 271), using a specification that embedded both permanent-income mechanisms alongside more traditional Keynesian features in a general consumption function, found that the surge in disposable income in 1975:Q2 associated with the tax rebate produced a large residual in the estimated function and had little counterpart in U.S. household spending.

The absence of a sustained consumption response to the 1975 tax rebate disillusioned the Ford Administration’s economic team about such measures (see Taylor, 2012, pp. 54–56). The episode also likely made many economists more receptive to Friedman’s position that fiscal actions had little cyclical effect when not accommodated by monetary policy. Policy suggestions along the lines of the tax rebate continued to figure prominently, however—as evidenced by President Jimmy Carter’s tax-rebate proposal of early 1977 (see Chapter 6).

Friedman’s own commentary in *Newsweek* on employing a tax cut against the recession argued that it would be ineffective in stimulating the economy. In taking this line, however, he pointed to empirical evidence against fiscal policy, instead of invoking the permanent income hypothesis *per se*.

Irrespective of the specific argument Friedman relied on in downgrading fiscal policy, it became moot if the fiscal measure was monetized. In 1975, as on other occasions, Friedman noted that this conclusion about the ineffectiveness of a tax cut as a demand-stimulating measure would no longer hold if accommodated by monetary policy—though he added that the Federal Reserve could achieve much the same effect on aggregate nominal spending via purchases of the existing stock of government bonds outstanding, without any new fiscal stimulus (*Newsweek*, May 12, 1975). This repeated in print a point Friedman had stressed in his Claremont College appearance a couple of months earlier, when he remarked that “you don’t need a tax cut and a higher deficit to print money. You can provide the monetary growth without the tax cut.” (*Participant*, Spring 1975).
In his April 1975 proposals, Franco Modigliani had, in fact, suggested that the Federal Reserve should be required by Congress to accommodate the proposed move to a larger fiscal deficit and, specifically and relatedly, to enforce a ceiling on market rates on longer-term Treasury securities. But, in the event, although (as discussed above) monetary growth rose in 1975, this did not arise from conscious accommodation of the federal deficit (or from any target for long-term interest rates; these remained market-determined). The fiscal and monetary expansions were not closely related; and a very large spike in the U.S. budget deficit in 1975:Q2 did not give rise to a comparably outsized rate of monetary growth.

That spike in the deficit has, nevertheless, played a central role in some narratives of 1970s macroeconomic behavior that are based on the fiscal theory of the price level (FTPL). After Sims (2011, p. 49) expounded the FTPL by pointing to the Ford tax cut’s effect on primary fiscal deficits, Bianchi and Melosi (2013, pp. 30–32) claimed a central role for that tax cut in explaining U.S. inflation in the 1970s. However, Bianchi and Melosi’s exercise—which focused on the average values of inflation and deficits for the 1970s—proves to be unconvincing once it is examined in light of several details about that decade’s developments that they do not consider. The FTPL account requires fiscal deficits to generate higher aggregate nominal aggregate demand; but, as already indicated, aggregate nominal spending failed to respond appreciably to the 1975 tax cut. Furthermore, inflation was high in the first half of the 1970s—yet the tax cut did not occur until the start of the second half of the decade. Indeed, as Friedman remarked (Newsweek, February 23, 1981), the double-digit inflation of 1974 was accompanied by a low federal deficit in relation to national income.37

Nor can the high inflation of 1974 or preceding years plausibly be attributed to anticipation of the 1975 tax cut. The latter was not floated until January 1975 by President Ford—who in late 1974 had actually been proposing an increase in taxes (see Nelson, 2005). All in all, what Friedman called the “uneasy connection of deficits with inflation” (Newsweek, February 23, 1981) is well exemplified by the rebate episode. Indeed, over the course of 1975 and 1976—that is, just before, throughout, and for a year and a half after the distribution of the rebate—the U.S. inflation rate actually fell.

37 This contrast underlines the fact that, although monetary growth and deficits did indeed both rise during the first half of 1975, Bianchi and Melosi’s (2013, p. 32) implication that U.S. fiscal deficits for the 1970s as a whole were financed by money creation is likewise not supported by the detailed record of the behavior of monetary growth and deficits over that decade. Such a lack of relationship was in keeping with Friedman’s view of the choices of policy mix available for countries like the United States that had highly developed government securities markets. As discussed in Chapter 2, Friedman certainly believed that the growth of the public sector was a source of upward pressure on monetary growth over long periods, but he did not believe that budget deficits accounted for the rapid monetary growth in the early 1970s.
Inflation in 1975–1976: from new peak to local trough

With monetary growth having come down from its peak in 1972, and with output subsequently falling below potential, a partial disinflation took place. This began roughly at around the turn of the year from 1974 to 1975. Twelve-month CPI inflation peaked at 12.2 percent in November 1974, fell below 12 percent in January 1975, below 11 percent in March 1975, and below 10 percent in May 1975. Having returned to single digits, inflation continued to fall for another eighteen months. Early in the second quarter of 1976, Friedman predicted that inflation would fall to 4–5% by the end of the year (The Star (Johannesburg, South Africa), April 5, 1976). He was right: twelve-month CPI inflation stood at 5.0 percent in December 1976, while data on the GDP deflator show four-quarter changes of 5.1 percent in 1976:Q3 and 5.2 percent in 1976:Q3. The GNP deflator, used heavily at the time as a national price index, showed a similar but slightly lower rate of increase, passing slightly below 5 percent in late 1976.38

But Friedman believed that there was little prospect that inflation would fall further. On this, too, he was right: the inflation rate of 5 percent, or a little lower, in late 1976 proved to be a local trough, and all of the disinflation of 1975–1976 would be erased by the end of the decade.

As far as the trough in inflation was concerned, Friedman traced it to the fact that monetary growth had bottomed out two years earlier, during the squeeze of late 1974. But his pessimism about inflation was more deep-seated. He felt that a greater complacency was developing in U.S. public debate when it came to deviations from price stability. And he believed that policymakers, especially those at the Federal Reserve, lacked the analytical and policymaking mindset required to pursue and stick to policies consistent with achievement of price stability.

Fears of complacency

When the decline in inflation to 5 percent began to come about, Friedman voiced dissatisfaction that achieving inflation of that rate was now thought of “as doing a good job” (The Evening Bulletin (Philadelphia), November 18, 1976) or as “marvelous” (Instructional Dynamics Economics Cassette Tape 201, October 1976, Part 2). In 1970, he had observed that the populace a decade earlier has viewed 3 percent inflation as terrible, while now such a rate was

38 For example, Council of Economic Advisers (1977, p. 190) gave the provisional reading for four-quarter GNP deflator inflation in 1976:Q4 as 4.7 percent. In addition, data in International Monetary Fund (1981, p. 65) implied a twelve-month CPI inflation for December 1976 of 4.7 percent—a figure that may be lower than that implied by modern series partly because of rounding of the later vintages of price series.
regarded as “pretty good” (Instructional Dynamics Economics Cassette Tape 57, August 20, 1970). In that 1970 commentary, Friedman had expressed the hope that in four to five years—that is, in 1974–1975—3 percent inflation would be back to being regarded as terrible.\textsuperscript{39} In the mid-1970s, however, such an attitude had clearly failed to materialize.

Not only did Friedman regard people in the mid-1970s as being too complacent about achieving 5 percent inflation: he did not see the monetary regime as conducive to bringing inflation durably below that rate. What Klein (1975b, p. 137) assessed, on the basis of inflation data up to 1973, to be a “fundamental change in the underlying monetary framework,” seemed to be confirmed by the further rise in inflation in 1974 and its limited decline thereafter. Correspondingly, in 1975 Friedman described the 1970s as being part of an era of permanent inflation (Instructional Dynamics Economics Cassette Tape 165, February 1975, Part 3). Although, as discussed in Chapter 2, he did occasionally slip into formulations that implied that high inflation was intentionally generated by policymakers, his more careful elaborations stressed that the United States’ postwar inflation arose from the authorities’ miscalculations: “The Federal Reserve System clearly did not want to exacerbate inflation from 1971 to 1974,” he observed (\textit{Newsweek}, March 10, 1975). Consistent with this perspective, Romer and Romer (2002, 2004) provided documentary evidence that policymakers’ ultimate goal regarding inflation in the 1970s was a rate consistent with price stability—so the higher inflation in that decade did not reflect a change in policy goals since the 1950s or 1960s.

But, just as he saw recent years’ inflation as a by-product rather than an intended result of the policies followed, Friedman thought inflation could be a feature of future years because the monetary regime had not changed. In an October 1975 appearance in which he lamented the “series of roller-coaster, go-stop policies” that had characterized U.S. economic stewardship over the previous decade and more, Friedman said: “I feel we are still on this course and have more cycles to go through…” (\textit{Dallas Morning News}, October 17, 1975.)

In particular, Friedman did not feel that policymakers yet grasped the appropriate policy response needed for eliminating inflation. “You can’t stop inflation without unemployment and stagnation,” he had remarked earlier in the year. “All this business about consumers saving more and reusing paper plates to control inflation—that’s nonsense.” (\textit{Philadelphia Evening Bulletin}, March 2, 1975.) The methods being advocated at the policy level to were more sophisticated

\textsuperscript{39} By Friedman’s own assessment, however, 3 percent inflation was low enough that the economic incentives to introduce protections against inflation in contracts might not be significant (Instructional Dynamics Economics Cassette Tape 152, August 21, 1974; American Enterprise Institute, 1974, p. 51).
than the popular recommendation of paper-plate usage that Friedman cited. But, as discussed in Chapter 2, the termination, at the end of April 1974, of the United States’ wage and price controls was followed by a slew of recommendations from leading economists for post-control incomes policies. Walter Heller, for example, testified to the Joint Economic Committee in August 1974 in support of a wage/price monitoring agency, and he suggested that aggregate demand tightening might deliver higher unemployment but not lower inflation (Washington Star-News, August 1, 1974). And Heller (1976, pp. 51, 191) proposed that a tax cut could be used as a means of relieving wage-push inflationary pressure—an argument that financial columnist Sylvia Porter also used in making the case for the 1975 tax rebate (see Nelson, 2005). A short-lived uptick in inflation in the latter part of 1975 also saw renewed discussion of cost-push inflation, perhaps arising from the firm side rather than wages.

At the policy level, too, elements of a nonmonetary approach to inflation had been present in the Ford Administration from its inception—as discussed in Chapter 2—and survived over 1975–1976. It is true, as has already been indicated, that both CEA Chairman Alan Greenspan and Secretary of the Treasury William Simon were sympathetic with Friedman’s perspective toward inflation. Consistent with this, Simon testified to the Joint Economic Committee in February 1975 that what was required was “some margin of economic slack… for a period of years,” and he criticized the alternative approach, involving a very speedy elimination of the output gap (Washington Star-News, February 6, 1975). In addition, one member of the Ford Administration who had espoused cost-push views of inflation—Albert Rees—left public office at the end of July 1975 (Washington Post, March 9, 1979).

Crucially, however, throughout the Ford years Federal Reserve Chairman Arthur Burns remained firmly in the nonmonetary camp when it came to approaching inflation. Burns had argued in June 1973 that wage/price controls should be followed by a system in which a federal government wage-and-price-review board had a say on the economy’s major price changes and wage disputes. Similarly, in August 1974 Burns spoke out in favor of some federal-government direct management of private-sector pricing practices: in particular, he came out in favor of new wage/price guidelines (Washington Star-News, August 23, 1974). Statements like this generated new public rebukes of Burns from Friedman (see, for example, Instructional Dynamics Economics Cassette Tape 151, August 7, 1974). But Burns went on to make many similar

---

40 See also the discussion in Section III below of Arthur Okun’s tax-based incomes policy (TIP) proposals.
41 See Nelson (2005). This episode to some observers validated the concerns about “administered price inflation” which, as discussed in Chapter 2, were articulated by commentators and policy agencies during late 1974 and the first half of 1975.
remarks in 1975 and 1976—including his March 1976 observation that “if we try to rely solely on monetary and fiscal policies to achieve general price stability, I believe we are likely to fail… I am convinced that we will return to an incomes policy sooner or later” (quoted in DiCecio and Nelson, 2013, p. 409).

As this quotation indicates, Burns did not see the inflation benefits from economic slack that Friedman and Simon were postulating. His belief that a situation of output falling short of potential would not reduce upward pressure on prices mirrored his position that the 1970s surge in inflation would have taken place in much the way it did even if monetary policy had been tighter. Reflecting this difference in perspective, in the summer of 1975 Friedman criticized Chairman Burns for still failing to acknowledge sufficiently the Federal Reserve’s role, via its actions of the early 1970s, in producing the inflation surge of 1973–1974 (Wall Street Journal, August 21, 1975).

Interest rates in 1975–1976

It was, of course, in the context of monetary policy behavior that Friedman viewed the behavior of U.S. interest rates during the mid-1970s. Over this period, the Fisher effect was raising (and then lowering) nominal interest rates, and fluctuations in the natural real interest rate were creating further variations in the values of the short-term nominal interest rate consistent with price stability. In the face of these forces, the Federal Reserve could still choose to manage short-term nominal interest rates—and indeed it continued to do so, increasingly overtly, over 1975–1976 (with its pretense of managing a reserves aggregate abandoned in the latter year). But such management, if it went against the forces driving expected inflation and the real natural rate, could destabilize the economy. Friedman believed that the FOMC would, even when it had decided to follow a policy of managing interest rates, eventually have to adjust those rates in the direction of underlying economic forces, simply in order to avoid still greater economic instability.

U.S. short-term interest rates were low in real terms in the mid-1970s, especially in early 1975. In the first quarter of 1975, for example, the federal funds rate averaged 6.3 percent when the same quarter’s annualized increase in the CPI was over 8 percent. Barro and Xali-Martin (1990, pp. 17–18) have implied that real short-term interest rates in the United States and elsewhere in
1975–1977 were very likely *ex ante* positive even though they were negative in *ex post* terms.\textsuperscript{43} These authors’ discussion restricted the possibility of *ex ante* rates being negative to the case in which the natural real rate is negative—a situation they did not see as plausible except for extremely brief periods. Such a perspective, however, does not adequately allow for monetary policy as a powerful influence on *ex ante* real interest rates in the short run—an influence Friedman stressed in his work on the liquidity effect.\textsuperscript{44} Once monetary policy is acknowledged as a factor affecting real rates, there is no basis for believing that the *ex ante* short-term real interest rate cannot be (made) negative at the business-cycle frequency, even when the natural real rate is not negative. Friedman’s own writings on short-term interest rates took the private sector as having largely rational expectations when forming its short-term forecasts of inflation. He accordingly took the negative *ex post* short-term real rates in the 1970s as predominantly implying negative *ex ante* rates.\textsuperscript{45}

When these negative real rates coexisted with the take-off of monetary growth from early 1975, Friedman judged that the FOMC would have to let rates rise: “Short-term rates will go up practically no matter what the Fed does, even if it stands on its head.” (*Business Week*, August 11, 1975.) Although the news report that contained these remarks took them as implying that “Milton Friedman is one economist who believes the Fed cannot control interest rates,” it is clear from his many other statements during the period and at other times that Friedman did believe that the Federal Reserve could control interest rates (though he also believed it should not) but that its stabilization goals would eventually force it to change its target value for the federal funds rate.

But Friedman also believed that the FOMC of the post-Accord years was far too slow to make such changes. As discussed in Nelson (2018a, Chapter 8) and Chapter 2 above, Friedman’s critique of interest-rate policies involved the Wicksellian point that, when there was downward pressure on the natural real interest rate, central banks might not reduce the nominal interest rate fast enough, generating monetary restriction and forces promoting economic slowdown and recession.\textsuperscript{46} Such a scenario, of course, had figured in Friedman’s account of the 1974–1975

\textsuperscript{43} Brown and Santoni (1981) took an even stronger position—apparently denying that real *ex ante* short-term interest rates underwent an appreciable fall at all in 1974 and 1975. But they did not provide valid evidence that these rates did not in fact decline materially. In particular, their suggestion that real yields on some assets other than interest-bearing securities did not fall in the period is perfectly consistent (in a multi-asset world) with an outcome in 1974–1975 in which the real rates on interest-bearing securities fell and became negative.

\textsuperscript{44} See Nelson (2018a, Chapter 6).

\textsuperscript{45} See, for example, *Fortune*, July 1974, and *Newsweek*, June 28, 1982.

\textsuperscript{46} See the discussion of Friedman’s November 1975 Congressional testimony in Section II below, as well as the items cited in the discussion in Chapter 2 above.
deepening of the recession. This critique amounted also to an acknowledgment that the natural short-term real rate had been low during some months in the mid-1970s. Thus, although he felt that the FOMC had on the whole let real interest rates fall too low in the 1970s, Friedman also believed that, on occasion, it had not let real interest rates fall far enough.

In connection with longer-term rates, Friedman had observed in September 1973 (Instructional Dynamics Economic Cassette Tape 130, September 26, 1973): “I think inflation over the next ten years is almost sure to be higher than 4½ percent, it’s very hard to believe that it’s going to be that low.” Longer-term rates, he observed during the previous summer, “far from being abnormally high are, if anything, abnormally low,” as they appeared to presume a longer-term inflation rate below 5 percent (Instructional Dynamics Economics Cassette Tape 124, July 4, 1973). At this time (mid-1973), U.S. ten-year Treasury rates were about 7 percent. In August-September 1974, as we have seen, they crossed 8 percent. In 1975, longer-term interest rates failed to show the decided decline from the previous year’s values that was exhibited by short-term interest rates. Rather, longer-term rates hovered around 8 percent in both years. They were nevertheless, as noted in Chapter 2, roughly consistent with a market expectation that inflation would return to perhaps 2–3 percent beyond the immediately approaching years. In October 1975, Friedman noted this expectation and expressed doubt about its soundness. Markets might expect 2 percent inflation by the end of the decade, he said, but it was actually unlikely to happen (Dallas Morning News, October 17, 1975).

But though the level of short-term nominal interest rates, and to some extent that of longer-term rates, was inconsistent with the preservation of normal real returns, their fluctuations very closely tracked the course of inflation in 1973–1976—particularly so in the case of short-term rates. There is thus, as stressed in Chapter 2, no contradiction between the fact of the very low level of real rates in the mid-1970s and Friedman’s conviction over the same period that the Fisher effect was clearly visible in U.S. short-term interest rate data, the first time this had been the case on a sustained basis.

The relationship between inflation and interest rates had made an impression on Arthur Burns, too. “Inflation is the main reason for the high interest rates that we have,” Burns stated in an August 1975 television appearance, “and we’re not going to bring interest rates down significantly until we bring inflation under control.”47 Burns’ perspective on the relationship

---

47 From Arthur Burns’ appearance in Face the Nation, CBS, August 24, 1975, quoted in Banker’s Monthly, September 15, 1975, p. 2. See also Burns’ testimony of September 25, 1975, quoted in DiCecio and Nelson (2013,
between inflation and demand pressure had changed greatly since the 1950s and the 1960s; but with regard to interest rates and inflation, in the 1970s Burns continued to believe strongly in the Fisher effect.\textsuperscript{48} His change in view concerning the demand/inflation relationship put a new complexion onto his interpretation of the Fisher relationship (see DiCecio and Nelson, 2013, pp. 406–407). Burns’ changed posture meant that the task of “bring[ing] inflation under control” was one he saw as ideally assigned primarily to nonmonetary measures. On this view, low real interest rates alongside inflation did not mean that monetary policy should be tightened; instead, it implied that nonmonetary devices should be brought to bear more vigorously against inflation.

Unemployment benefits: the background

Back in July 1971—just ahead of the U.S. economy’s four-year odyssey of wage and price controls, an oil shock, double-digit-inflation, and multi-year recession—Friedman was making the case for restraint in aggregate demand. He was doing so in the face of widespread commentary that more stimulus was needed because the decline in the unemployment rate since the 1969–1970 recession had been too slow. Against this tide, Friedman remarked that a factor that he had not previously considered much in his audio commentaries, but whose importance “has been increasingly brought home to me” from conversations, deserved consideration in interpreting the data. The “much more liberal and widespread welfare provisions that we now have” and “much easier access to welfare, including food stamps and other things,” Friedman suggested, meant “a given unemployment percentage involves less pressure on the wage rate than it would have in earlier times”—that is to say, the natural rate of unemployment had increased (Instructional Dynamics Economics Cassette Tape 78, July 14, 1971).

During the summer of 1975, with unemployment only fractionally down from its May peak of 9 percent, Friedman pressed this point much more emphatically. Most notably, in his Newsweek column of August 4, 1975, Friedman commented that—although the recession had certainly created major job losses—“the high reported levels of unemployment are partly a statistical artifact reflecting expanding government programs to assist the unemployed.” Such programs, Friedman wrote, “have made unemployment more attractive [and] have also tended to increase the number of peoples who so report themselves,” rather than leaving the recorded labor force.

\textsuperscript{48} On Burns’ espousal of the Fisher effect in the 1950s and 1960s, see Nelson (2016; 2018a, Chapter 6).
In the period between 1971 and 1975, much of the running on this topic had been done by Martin Feldstein, a specialist in public finance and macroeconomics and a member of Harvard University’s Department of Economics since 1969. Feldstein had accepted that appointment in a situation in which he “had an offer from Chicago and from Harvard,” and he had met Friedman in a job-market visit to the University of Chicago (Martin Feldstein, interview, November 21, 2013). Then, in the 1970s, Feldstein produced a sizable number of research papers on the relationship between the unemployment-benefit arrangements and the U.S. unemployment rate. This work likely was a major basis for Friedman’s statement in 1980 that “in America… we have had some very careful studies” of the link between unemployment benefits and the unemployment rate.”49 The same work, along with a series of pioneering Feldstein papers on inflation and the taxation of capital, likely played a role in leading Friedman to predict in 1981 that at some point Feldstein would win the Nobel prize in economics (Wallechinsky, Wallace, and Wallace, 1981, p. 418).

The earliest Feldstein studies of unemployment benefits cited in Chetty’s (2008) discussion of that topic are Feldstein (1978) and Feldstein and Poterba (1984), while Krugman (1995, p. 73) stated that Feldstein’s interventions on the matter were “[a]s early as 1973.” In fact, however, Feldstein stepped into research and public debate on unemployment insurance earlier than these two discussions indicated—in a research study that underpinned Feldstein’s Congressional testimony of October 1972.

This testimony arose, Feldstein recalled, because “there was a debate going on between the Republican administration and Senator Proxmire, who was chairman of the Joint Economic Committee.” Senator William Proxmire, a Democrat representing Wisconsin, had some inclinations toward free-market policies: in early 1974, he would remark: “I’ve always been a Friedmanite or tried to be a Friedmanite. Or as much as one can be who doesn’t have a Ph.D. in economics. But I have great respect and admiration for Dr. Friedman and his principles, and I would agree that we should have as little government as possible; but the whole issue is how little government you can get away with.”50 As discussed further below (see “The Beginning of Monetary Targeting” in Section II below), Proxmire was also sympathetic with aspects of Friedman’s macroeconomic views too. Entering the U.S. Senate in 1957, Proxmire, had referred to Friedman’s monetary research during Congressional hearings in the early 1960s (Nelson,

49 Free To Choose (U.K. television version, debate portion), BBC2, episode “Created Equal,” March 1, 1980, p. 16 of transcript. A few years earlier, specific research that Friedman (1977a, p. 25) referred to on the matter was a then-new study by Clarkson and Meiners (1977).

50 From Proxmire’s remarks in the panel discussion, The Energy Crisis, January 24, 1974.
He pushed for monetary-aggregate targets during the late 1960s, challenged Arthur Burns in 1973 and 1974 using Friedman’s arguments regarding inflation, and then played a pivotal role in the formal introduction of monetary targeting in 1975.51

As far as stabilization policy was concerned, however, Proxmire favored more active government policies. Proxmire’s longtime advocacy of monetary targets did not really clash with this posture. As eventually legislated by Proxmire, monetary targets were seen as putting monetary growth down on a glide-path down to rates roughly consistent with real growth equaling its long-run potential rate. Subject to that endpoint, it was always possible to frame the numerical monetary targets to make them consistent with—possibly ambitious—targets for the level and growth rate of real output in the interim years.52 In keeping with Proxmire’s enthusiasm for forceful demand-management policies, Friedman in early 1974 would call the senator “one of my favorite interventionists.”53

In 1972, this attitude was manifested in Proxmire’s view toward the Nixon Administration’s economic goals. These were predicated on a full-employment unemployment rate of 4.5 percent, a value Proxmire considered insufficiently ambitious. “So,” Feldstein recalled, “he had the brilliant idea of saying, ‘What do we have to do to get the unemployment rate down to 2 percent?’ And he then commissioned Otto Eckstein to do a study about that, and Otto had recently set up Data Resources, a company. Otto was too busy to do the study, and so he turned to me and said, “Why don’t you do this?” So I did it, and one of the things I focused on in that study as a way of reducing the permanent rate of unemployment was to modify unemployment insurance. And Proxmire was very unhappy with all that. What he wanted was a study saying: More expansionary monetary and fiscal policy; increase demand. And [instead] here comes Marty saying well, no, you can integrate the minimum wage and welfare, you can change unemployment insurance benefits in a variety of supply-side policies.” (Martin Feldstein, interview, November 21, 2013.)

Feldstein testified to the Joint Economic Committee on October 17, 1972, as part of hearings that Proxmire later ostentatiously titled Reducing Unemployment to 2 Percent.54 Feldstein told

---

51 See Nelson (2018b, Chapter 13), Chapter 2 above, and Section II below.
52 This was certainly the case in early 1975, when there was wide agreement that there was slack in the economy and the monetary targets implied higher monetary growth than that observed in the immediate past.
53 From Friedman’s remarks in the panel discussion, The Energy Crisis, January 24, 1974.
54 In his study underlying this testimony, Feldstein (1973a, p. 3) indicated that he was sympathetic with the natural rate hypothesis and noted doubts about the pre-1972 research that had criticized the hypothesis. However, this was not his focus, as, even if the natural rate hypothesis did not hold, whether unemployment benefits tended to have a material effect on the unemployment rate was a policy concern.
Proxmire’s committee: “Decreasing the overall rate of unemployment requires not merely more jobs, but new incentives to encourage those who are out of work to seek employment more actively, and those who are employed to remain at work.” He added: “Unfortunately, the current system of unemployment compensation encourages excessive delays in returning to work. For many lower- and middle-income families, the combined effect of unemployment compensation and income taxes is to reduce greatly and often almost eliminate completely the cost of remaining unemployed for an additional one or two months.”

Unemployment benefits: Friedman’s critique

Feldstein therefore characterized the length and magnitude of unemployment as partly a response to the incentives offered by government and the market. He consequently postulated a link between the unemployment rate and unemployment benefits, while avoiding casting aspersions on benefit recipients. Previously, it had been frequently the case that those who cited unemployment benefits as a factor boosting the reported unemployment rate had been portrayed in political and popular discussions (and sometimes accurately so) as impugning the motives, and in particular questioning the work ethic, of those receiving governmental payments. For example, a remark by Ronald Reagan—made in the year in which he first ran for Governor of California—that unemployment insurance was a “prepaid vacation for freeloaders” came back to haunt him. The quotation was thrown back at Reagan in 1966 and 1980, respectively, by incumbents whom he challenged for office: Governor Edmund G. (Pat) Brown and President Jimmy Carter. During the 1980 presidential election campaign, in commenting on his early remark, Reagan qualified it and perhaps also backtracked, by saying that his comments had only meant to apply to a small number of benefit recipients (U.S. News and World Report, October 6, 1980, p. 60).

The stigma often attached to those receiving governmental welfare payments had been both a motivation for Friedman’s negative income tax idea and one obstacle he faced in gaining acceptance of the proposal. In this connection, a Time magazine (December 8, 1967) discussion of Friedman’s proposal had observed: “To many Americans, the idea of a guaranteed income

---

55 From Feldstein’s testimony of October 17, 1972, in Joint Economic Committee (1972, pp. 17, 23). Feldstein’s accompanying research study was published as Feldstein (1973a), and his testimony was reprinted as Feldstein (1973b). His follow-up research included Feldstein (1974a). Zell (1976) offered an opposing view.

56 An analysis of Reagan’s statements that appeared in the Press-Telegram (Long Beach, California), January 16, 1968, traced this remark to the Sacramento Bee of April 28, 1966. The quotation had, however, appeared in print slightly earlier—in a Los Angeles Times news report of April 21, 1966. Reagan had made the remark in a dinner speech on April 20.

57 See, for example, Redland Daily Facts (California), September 7, 1966, and Carter (1980).
smacks suspiciously of a dole to people who refuse to get a job.” However, in 1968, Friedman himself gave what an uncharacteristically unsympathetic discussion of the attitude taken by welfare recipients. There were members of “every economic or class in society,” Friedman said, who would gravitate toward welfare receipt if it was available because “they pay attention to immediate gratification and give little immediate thought to the future.”58

However, Friedman’s comments in the years before and after these remarks indicated that he did not view the bulk of benefit recipients this way. Rather, his position paralleled that articulated by Reagan in 1980 that those exploiting benefits without intent of seeking a job were a small minority of overall recipients. The position that unemployment benefit recipients did include a small portion of such individuals was actually voiced across the political spectrum in many countries. For example, Australia’s Labor Prime Minister Gough Whitlam remarked in the mid-1970s: “there is an irreducible minimum of bludgers. And I suppose this is the experience of all government welfare agencies…”59

In his discussions from the mid-1970s onward, Friedman showed signs of a wish to concentrate on economic incentives to engage in longer unemployment spells—and not to be perceived as attributing bad motives to the recipients. This posture was reflected in remarks he made that it was understandable and proper for those eligible to seek unemployment benefits. In a November 1976 television appearance, for example, he remarked that “a large fraction” of recorded unemployment “constitutes human beings taking advantage of very good arrangements that have been developed to help people who are unemployed. I don’t blame them for doing it. They’re foolish not to.”60 In the mid-1980s, Milton and Rose Friedman underlined that their emphasis on the link between governmental payments and unemployment was “not a criticism of the persons who receive unemployment benefit… So long as those programs are available, people are sensible to take advantage of them.”61

Furthermore, Friedman indicated that it was appropriate for the government to provide unemployment benefits and that, in principle, the existing levels of benefits levels could be appropriate. In April 1975, he remarked that “obviously[,] the more generous are the provisions… the more unemployed there will be. That doesn’t mean it isn’t desirable to make provision…”62

58 Friedman (1968c, pp. 25–26).
59 This Week, HSV7 (Australian television channel), September 21, 1975, p. 5 of transcript.
60 Wall Street Week, Maryland Public Television, November 5, 1976, p. 9 of transcript.
61 Friedman and Friedman (1985, p. 108).
62 Friedman (1975i, p. 28).
Like Feldstein, however, Friedman’s judgment was that in the United States unemployment benefits had become too high. Benefits were at a level that he believed would enlarge the number of people detached from the workforce by adding to those who would prefer to stay on welfare those marginally inclined to be a member of the workforce. Furthermore, it had become prevalent, in Friedman’s assessment, for it to be a rational action on a recipient’s part to stay on welfare, due to the financial penalties incurred, on net, in taking a job that was available. Consequently, in Friedman’s estimation, the U.S. situation was characterized by “governmental programs which encouraged a large number to become dependent.”63

In explaining how they had reached these judgments, both Friedman and Feldstein emphasized the interaction of welfare, wages, and marginal tax rates. Feldstein’s testimony, for example, suggested a situation of an 80 percent effective marginal tax rate for certain unemployed workers contemplating accepting a job offer in Massachusetts.64 In looking only at the benefits paid, Feldstein recalled, “I think people had the wrong idea. You know, people would say, ‘Look, the average unemployment benefit is only 20 percent of the average wage, so how can it make much of a difference?’ And what they forgot to take into account was that the average unemployed person had a much lower pre-unemployment wage, and potentially post-unemployment wage, than the average worker. And they also failed to take into account the taxation or non-taxation of unemployment insurance which, given marginal tax rates, especially for second earners, could be a big deal. So I busied myself explaining all that—making calculations and doing a bunch of econometric studies with micro data, which I think over time have convinced people that it really does have a big effect.” (Martin Feldstein, interview, November 21, 2013.)

In the same vein, the need for payments to the unemployed to be at levels that did not unduly deter accepting offers to work underlay Friedman’s negative income tax proposal and was also the basis for his criticism of Senator George McGovern’s 1972 election proposals for a negative income tax which, in Friedman’s views were too generous. However, as Friedman put it in April 1975, “I believe it is desirable to have a system under which anybody who is in distress and is assisted and enabled to have a minimum standard of life…”65

This mixture of attitudes on Friedman’s part toward unemployment-related benefits—that provision of some benefits was desirable; that those eligible for benefits were rational to apply for them; but that benefits had become too high and, in many cases, provided too great a

---

63 Speaking Freely, WNBC, May 4, 1969, p. 15 of transcript.
64 See his testimony of October 17, 1972, in Joint Economic Committee (1972, p. 17).
65 Friedman (1975i, pp. 28–29).
disincentive to seek or accept job opportunities that were available—were reflected in his numerous remarks—sometimes circumspect, sometimes acerbic—during 1975 and 1976 on the unemployment rate/benefits relationship.

There was a “question of the meaning of the unemployment figures,” Friedman observed in a July 1975 commentary (Instructional Dynamics Economics Cassette Tape 171, July 1975, Part 1). Recent increases in unemployment insurance had meant that, of late, for some workers, it was “in their self-interest—and I don’t blame them for doing this—to get themselves discharged rather than to leave voluntarily, and to collect unemployment insurance. In order to collect unemployment insurance, they must say that they are in the market looking for a job. The recorded number of unemployed is therefore higher than it otherwise would be.”

Later in the month (around the time his Newsweek column on the subject appeared), Friedman proclaimed that the “great expansion in government social and welfare policies have made unemployment a much more attractive status than it was before” (Instructional Dynamics Economics Cassette Tape 172, July 1975, Part 2). The following November, he remarked (Instructional Dynamics Economics Cassette Tape 181, November 1975, Part 2) Friedman remarked: “We have made it ... profitable for people to have the status of unemployment and therefore the numbers have tended to go up.” In September 1976, Friedman added that “in Britain and the United States we have made unemployment a very attractive situation. In my country, many a person can have as high an income in real terms by being employed... If there is a demand for unemployed people, as evidenced by a willingness to pay reasonable payments for being unemployed, then the supply of unemployed people will rise to meet it. Of course, there are many people who endure great hardships by being unemployed, but the numerical statistics [for total unemployment] are very misleading.” (The Times (London), September 13, 1976.)

Unemployment benefits: the U.K. angle

The inclusion by Friedman of the United Kingdom, rather than just the United States, in the remark just given likely reflected, in part, his awareness of the increasing attention that researchers on both sides of the Atlantic were giving to supply-side reasons for the upward trend since 1966 in the U.K. unemployment rate.

Emerging findings from Daniel Benjamin and Friedman’s former student Levis Kochin, ultimately published as Benjamin and Kochin (1979), argued that generous unemployment benefits were a major factor behind the United Kingdom’s interwar unemployment. That the
basic mechanism at work was not a new one was underscored when Friedman invited Kochin to present the Benjamin-Kochin study at the University of Chicago in 1976. It was now over twenty-five years since the spring 1951 quarter in which Friedman had launched his workshop on money and banking (see Horwich, 1964, p. 245). “We gave it at the money workshop,” Kochin recalled (interview, April 23, 2013). He noted with regard to this late-1976 gathering of the workshop: “It was very close to Friedman’s retirement. And it was an extraordinary session because all the faculty who were concerned with macro turned up.” In addition, Friedman himself would eventually cite Benjamin and Kochin (1979) in print.66

Although the paper concerned developments in 1921–1938, the Benjamin-Kochin paper was highly topical. Hamermesh (1980, p. 517) would observe: “Research on the effects of unemployment insurance (UI) on the duration of spells of unemployment has become a major growth industry in labor economics…”

The phenomenon for which Benjamin and Kochin provided evidence from U.K. interwar experience was increasingly widely regarded as relevant for modern conditions too. Feldstein had been one of those documenting this in the case of the United States. But, as Benjamin and Kochin (1982) would later stress, it was also the case that the rise in U.K. unemployment from its very low mid-1960s values (with a trough of 1.2 percent in February 1966) was widely believed as partly the consequence of the greater generosity of unemployment benefits.67 These were made by the U.K. government in the second half of 1966.68 Feldstein himself had pointed to the 1966 change in UI arrangements as prompting a breakdown between the relationship between the U.K. unemployment rate and job vacancies—a result that suggested that a rise in the natural unemployment rate from the higher benefit was one reason “British unemployment began rising dramatically” under the new arrangements.69

67 As well as Benjamin and Kochin’s (1982) discussion of this point, see their earlier exposition in The Banker, February 1979 (pp. 33, 36).
68 For the 1966 U.K. unemployment rate, see https://fred.stlouisfed.org/series/LMUNRTTGBM156S.
69 See Joint Economic Committee (1972, pp. 25, 27) and Feldstein (1973a, p. 47); the latter is also the source for the quotation.

Writing in late 1978, Dornbusch and Fischer (1980, p. 26) made the surprising statement: “U.S. studies have shown an increase in the natural rate of unemployment in the seventies, but we are unaware of such studies for the United Kingdom. This is an issue of considerable importance that has apparently not generated much discussion in Britain.” In fact, not only had prominent U.S. economists like Feldstein and Friedman highlighted the rise in the natural rate of unemployment in the United Kingdom in the 1970s, but by 1978 the matter had been highlighted in public discussion on many occasions, including by Samuel Brittan (1977, p. 191), who referred to the “steady reduction… of the incentive to work” because of improvements in unemployment benefits, and by the National Institute of Economic Research (1977) (which was skeptical—in retrospect, unduly so—about there being an appreciable rise in unemployment benefits. Some of the research literature had also by 1978 also suggested a rise in the natural rate of unemployment in the United Kingdom (see Nelson, 2019c, p. 29). Brittan (in Financial Times
Against this background, Friedman used the opportunity of a freewheeling 60 Minutes interview on the U.K. economy’s troubles to remark: “If you pay a man to be lazy, he’ll be lazy. If you pay a man to be energetic, he’ll be energetic…. Now, the arrangements adopted in Britain have been arrangements in which people are paid for not working very hard. The man who works hard is no better off than the man who is lazy. The man who is lazy or doesn’t work hard gets all these social benefits. If he earns a little more, if he’s hardworking, that’s taken away from him in taxes. What incentive is there for a man in Britain to be hardworking?” (60 Minutes, CBS, November 28, 1976, p. 14 of transcript.)

Unemployment benefits: the verdict

Although the U.S. unemployment rate from 1977 through mid-1982 was usually lower than the rates recorded in 1975 and 1976, it remained above levels typically seen in previous decades, and Friedman believed unemployment benefits were a significant factor underlying the increase in the rate. “I don’t blame people for collecting welfare checks,” he said of recipients in a February 1977 talk. “They are fools not to do it if they are entitled to it. I blame us for being so silly as to establish a welfare system which makes it more advantageous to collect welfare than to do productive work. I don’t blame them… I blame us.”70 Both in his Nobel lecture published in 1977 and in Newsweek (March 5, 1979), he reaffirmed that higher unemployment benefits had raised the U.S. unemployment rate in the previous decade.71 Friedman also cited the U.S. experience in a 1980 television appearance as supporting the proposition: “If you offer a higher price for being unemployed, you’ll have more unemployed.”72 Two years later, he remarked: “We have been paying people not to work.”73

As Feldstein’s 2013 remarks quoted above indicate, since the 1970s the position that unemployment benefits are a factor affecting the natural rate of unemployment has become widely accepted. In official circles, the U.S. Department of Labor commissioned an outside

---

70 Friedman (1977f, p. 17).
71 The Nobel lecture’s reference (Friedman, 1977c, p. 458) would be highlighted at the start of Hamermesh’s (1980) article on unemployment insurance.
73 Friedman (1982b, p. 57).
study in early 1975 that that year’s planned increases in unemployment benefits might add close to 1 percentage point to the unemployment rate (New York Times, February 25, 1975).74 This compares with Feldstein’s (1973a, p. 100) position that the U.S. unemployment rate was, in 1972, already at least 1.25 percent higher than otherwise because of disincentives from unemployment benefits. The two numbers were, taken together, broadly in accord with Friedman’s statement in 1980 (on the basis of his reading of others’ research) that “unemployment benefits account for something like one to two percentage points of our present unemployment level.”75

The position that benefits had added substantially to the unemployment rate in the course of the 1970s received endorsement from officialdom, in Federal Reserve Chairman G. William Miller remarks in July 1978 to a Congressional committee. Miller remarked that the full-employment rate of unemployment had been increased by “the effects of unemployment compensation and other institutional; factors on decisions regarding work.”76 Furthermore, the modern research literature assigns an important role to benefits in affecting the unemployment rate (see, for example, Hall, 2014), with particular stress placed by Pissarides (2013) and Hagedorn, Karahan, Manovskii, and Mitman (2016) on their importance in understanding the behavior of the U.S. unemployment rate in the early years of the recovery from the 2007–2009 recession.

The particular importance that Friedman assigned to benefits in understanding the level of the U.S. unemployment rate in the 1975–1976 period has also been supported by later researchers. For example, in 1977 Franco Modigliani suggested that the prolonging of unemployment insurance during 1975 amounted to “making it easy to be unemployed.”77 In a similar vein, Shimer (2010, p. 73) pointed to 1975 as the historical peak for the share of unemployed workers receiving unemployment benefits, while suggesting that the extensions of unemployment insurance that created this situation were an example of “well-intentioned government interventions [that] have kept unemployed workers from putting downward pressure on wages.”

Publications and research projects

During 1975 and 1976, the drawn-out work on Friedman and Schwartz’s Monetary Trends book

75 Free To Choose (U.K. television version, debate portion), BBC2, episode “Created Equal,” March 1, 1980, p. 16 of transcript.
76 Miller (1978a, p. 645; p. 9 of typescript version).
77 From Modigliani’s remarks in a joint appearance with Friedman in Friedman and Modigliani (1977, p. 24).
A draft of the book manuscript’s chapter on the connections between the U.K. and U.S. economies was informally circulated in 1973.\textsuperscript{78} The same practice was followed with their draft chapter “Money and Interest Rates” in September 1976.\textsuperscript{79} They also published a short paper, drawing on findings related to that chapter, that dealt with the emergence of an inflation/interest rate relationship in the U.S. data since 1965.\textsuperscript{80} In addition, Schwartz reported on the book draft’s material on nominal-income adjustment and on money demand both at the Brown University conference on monetarism in November 1974 (see Schwartz, 1976) and in the \textit{Journal of Economic History} article discussed in Chapter 2 (Schwartz, 1975). But as of December 1976—the tenth anniversary of the completion of the first full draft of the book—finalization of \textit{Monetary Trends} remained only a remotely distant prospect.

During the 1975–1976 period, two new books by Friedman appeared in the United States. One of these, a revision and extension of 1962’s \textit{Price Theory} text, appeared in 1976. It is discussed in the next chapter. In 1975, a revised version of 1972’s \textit{An Economist’s Protest} appeared. Like the earlier book, it collected most of Friedman’s \textit{Newsweek} columns, this time through October 1974. The main form in which this new book appeared was as a hardback titled \textit{There’s No Such Thing As a Free Lunch} (not a phrase due to Friedman). Small print runs of paperback versions were also issued both under that title and as \textit{An Economist’s Protest}—the latter billed as a second edition of the 1972 collection.\textsuperscript{81} A few columns that had appeared in the 1972 collection were dropped from the \textit{Free Lunch} compendium, and the new collection also included some non-\textit{Newsweek} material—including some new chapter introductions and reprints of Friedman’s \textit{Playboy} interview, a September 1973 \textit{New York Times Magazine} piece on educational vouchers, and his 1974 \textit{Fortune} article on indexation.\textsuperscript{82}

---

\textsuperscript{78} This manuscript was cited by Hamburger (1977a, 1977b).
\textsuperscript{79} See Dwyer (1981).
\textsuperscript{80} See Friedman and Schwartz (1976), already discussed in Chapter 2. Although the authors do not appear to have regarded this as anything but a minor publication—simply a note recording ongoing finding—it would be referenced and quoted years later in a paper in a major journal: see Barsky and Summers (1988, p. 529).
\textsuperscript{81} See Friedman (1975a, 1975j). The 1975 collections were issued by different publishers: Open Court Publishing Company published Friedman (1975a), while Friedman (1975j) had the same publisher, Thomas Horton and Company, as that of the earlier (Friedman, 1972c) version of \textit{An Economist’s Protest}. The two collections differed also in the preamble Friedman provided: \textit{Free Lunch} had a four-page preface dated August 2, 1974; Friedman (1975j) had a short introduction dated June 19, 1974.
\textsuperscript{82} The dust jacket of \textit{There’s No Such Thing As a Free Lunch} highlighted the fact that the \textit{Playboy} interview appeared in the collection, stating “Includes the \textit{Playboy} interview” with the word \textit{“Playboy”} set apart in a different color. The reprint of the \textit{Playboy} interview in Friedman (1975a) (and in Friedman, 1983b) allowed the interview to be read by a wider audience. At least one economics book in the mid-1970s, however, cited the original \textit{Playboy} interview rather than its reprint, referring to a specific page number in the interview for good measure (see Rhoden, 1976, p. 322).
The year 1975 also saw new printings of some 1960s Friedman monetary writings. His paper “Money and Business Cycles” with Schwartz had been a contribution to a 1962 conference on monetary economics. The proceedings of this conference, which had seen print in 1963 in the *Review of Economics and Statistics*, now were reissued as a book titled *The State of Monetary Economics* (though *The State of Monetary Economics in 1963* would have been a more appropriate name).

Also in 1975, Fordham University Press reissued Friedman’s 1960 book *A Program for Monetary Stability*. Although Amacher and Sweeney (1980, p. 301) would cite it as a revised or new edition of Friedman’s book, the 1975 volume was actually a straight reprinting of the original 1960 monograph. But 1975 proved to be a good time to put *A Program for Monetary Stability* back into print. For, in that year, the key policy recommendation in the book—monetary targeting—appeared to have become official U.S. monetary policy strategy.

II. ISSUES RELATED TO DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1975–1976

THE BEGINNING OF MONETARY TARGETING

Friedman enthusiastically greeted the move in 1975 of U.S. monetary policy toward arrangements that seemed to be a stepping stone toward the constant monetary growth rule that he had long advocated.

This change came not from an initiative of the Federal Reserve, but from the U.S. Congress. Friedman had not had a high opinion of the economic understanding of the head of the U.S. House of Representative’s banking committee, Wright Patman. Like Friedman, Patman was a persistent critic of the Federal Reserve’s monetary policy. But, in Patman’s case, this criticism was based on a cheap-money perspective. Friedman was more favorably impressed with Patman’s successor, Henry Reuss, who also co-chaired the Joint Economic Committee with the aforementioned Senator William Proxmire (who had succeeded to the leadership of the Senate’s own banking committee). Both Reuss and—as indicated in the previous section—Proxmire were...

---

83 See Friedman and Schwartz (1963b).
84 See National Bureau of Economic Research (1975). However, some citations of the book, and of the articles reprinted therein, have conveyed the impression that the material in the book was actually new in 1975 (see, for example, Levrero, 2019).
receptive to Friedman’s focus on monetary growth as a criterion for evaluating Federal Reserve policy.85

As discussed in Nelson (2007, p. 156; 2018b, Chapter 13), there was already sufficient Congressional interest in this issue to lead the Joint Economic Committee to begin, in the late 1960s, advocacy of a 2 to 6 percent range for the growth in the (M1) money stock. Senator Proxmire had acknowledged in 1970 that this move “was on the basis, to some extent, on the advice of Dr. Friedman and others.”86 But both houses of Congress took things a step further with House Concurrent Resolution 133, requiring that the Federal Reserve report to Congress on its “objectives and plans” for monetary growth. This resolution—passed on March 24, 1975 (Lindsey, 2003, p. 25)—made targets for monetary aggregates a more formal part of the monetary policy process. The FOMC duly responding by announcing its targets for monetary growth for the year ahead.

This requirement that the FOMC specify monetary-growth targets for the year represented a major shift from its practice in the first half of the decade. Some retrospectives (such as Bernanke, 2006a) have instead pointed to 1974 as the year in which the Federal Reserve began monetary targets. Underlying this dating is the fact that the Record of Policy Actions for the FOMC’s January 1974 meeting (a document that was released publicly with a lag) recorded the Committee’s hoped-for monetary-growth rates (for M1 and M2: see Federal Open Market Committee, 1974, p. 9). However, Bernanke and Mishkin (1992, p. 190) instead dated the start of monetary targeting to 1975, and accounts released by the Federal Reserve such as Wallich and Keir (1979, p. 690) and Meulendyke (1998, p. 46) similarly associated the monetary-targeting era specifically with the period, starting in 1975, of publicly-stated annual growth targets.

These assignments seem appropriate, as it was only in 1975 that, following the Congressional resolution, the FOMC started regularly expressing its monetary-growth aims in terms of targets for the whole year ahead. In contrast, the targets made in FOMC meetings in 1974 had merely referred to the one-month ahead period before the next meeting. The 1974 targets were therefore a more numerically precise and regular version of the monetary-growth goals the FOMC had set.

85 As discussed in Nelson (2018b, Chapter 13), monetary targeting evidently appealed to Reuss (who supported from this policy from the late 1960s onward) for the contribution it could make to the stabilization of aggregate demand and for the oversight of U.S. monetary policy. He did not subscribe to Friedman’s monetary view of inflation. This was true, initially at least, of Proxmire, who had written in the New York Times (October 18, 1970) that “the present inflation is not a demand inflation, but a wage-price-push inflation.” However, the Proxmire quotation given in Section I above suggests that, by early 1974, he had likely moved closer to Friedman’s overall economic views.

itself since 1970. The FOMC’s 1973 policy directives, for example, had referred to hoped-for monetary growth rates (see Balbach and Jordan, 1974).

The fact that the pre-1975 announced targets did not express a monetary-growth objective for the FOMC longer than up to the next meeting was especially anomalous, as Federal Reserve officials frequently suggested that only over a period of several months could monetary-growth patterns reliably reflect FOMC meeting. Because the pre-1975 arrangements did not involve targets for monetary growth for a full year, Friedman—though fully aware of the existence of the numerical targets given during 1974 (see Section I)—did not see them as very revealing about the FOMC’s longer-term plans. Information on these instead had to be pieced together from various Federal Reserve publications. Thus in December 1974, when referring to “the tea leaves that emanate from the Federal Reserve,” Friedman asked: “why do they make us read tea leaves, instead of following the ancient wisdom of Simons and the new wisdom of Lucas and Sargent and telling us clearly and simply what they plan to do?”87 A couple of months later, he expressed concern that “they really haven’t changed their basic target at all, and all this talk [by the FOMC about monetary aggregates] is just window dressing for people like Senator Proxmire, Congressman Reuss and myself.” He added that he did believe that the FOMC was indeed giving more weight to monetary aggregates in its policy decisions than it had before 1970, but that the weight was still too low (Journal of Commerce, February 25, 1975, p. 3). It was against this background that Congress voted for Resolution 133. Friedman regarded this resolution as introducing explicit monetary targeting in the United States.88

**Monetary strategy**

The resolution was passed despite what Friedman (in Wall Street Journal, April 15, 1988) later called the “vigorous objections of the Fed.” In documenting the Federal Reserve’s opposition, Friedman would cite Burns’ (1975) testimony (to the Senate’s banking committee) of February 25, 1975.89 In this testimony, Burns urged that the resolution, which was still pending a vote, not be passed. Burns objected to the focus on monetary aggregates implied by the resolution’s requirement that the Federal Reserve specify target growth rates for money. But Burns’ objections went beyond the references to money. In contrast to later generations’ emphasis on central bank transparency, Burns took exception to the resolution’s requirement that the Federal Reserve lay out its intentions regarding monetary policy. Burns (1975, p. 154) remarked that

---

87 Friedman (1975c, p. 178).
89 See Friedman (1982a, p. 108).
“the Board objects to the last paragraph of [the resolution], which calls for semiannual reports to the Congress by the Federal Reserve of its plans for future monetary policy. Such a requirement could limit the flexibility of monetary policy in responding to unexpected developments, and it could undermine the capacity of the Federal Reserve to exercise its best judgment in adapting policies to changing circumstances.”

Burns was therefore—quite apart from his suspicion of monetary aggregates—critical of what today would be called “forward guidance.” For example, he believed that Federal Reserve policymakers should not talk about the future course of interest-rate policy or the likely behavior of interest rates. Along these lines, in early 1976 Burns stated (Joint Economic Committee, 1976, p. 377): “An announcement by the nation’s central bank of its intentions or expectations about interest rates would be subject to misinterpretation, could impair the effectiveness of monetary policy, and could have harmful consequences for the economy and the nation.”

By requiring the specification of targets for monetary growth in the year ahead, Resolution 133 appeared to reduce the Federal Reserve’s prerogatives in policymaking. But Friedman did not regard that as a bad thing. “I am opposed to an independent Federal Reserve,” he reaffirmed at the time of the resolution’s passage (Washington Star (Washington, D.C.), March 24, 1975). In any event, he elaborated, there was an important distinction between Congress setting an annual target for monetary growth, on the one hand, and the legislature giving the Federal Reserve detailed instructions about how it should act each quarter in pursuit of that target, on the other.

In this observation, Friedman was making a distinction similar to that between (possibly desirable) “instrument independence” and (likely undesirable) “target independence” of central banks, of the kind later outlined by Debelle and Fischer (1994). Burns (1975, p. 153) could point out that the Federal Reserve was subject to the Employment Act of 1946, which had been interpreted in practice as mandating both full-employment and price-stability goals. However, the resolution made the requirements on the Federal Reserve more specific, by requiring it to choose the target rate of monetary growth, subject to the requirement that the target rate be

---

90 For a similar comment that Burns gave in 1977, see Fischer (2017). See also Chapter 6 below.
91 Reaffirming, that is, his conclusion in Friedman (1962b; 1968b, p. 125).
92 See Friedman’s testimony of January 22, 1976, in Committee on Banking, Currency and Housing, U.S. House of Representatives (1976, p. 2183). In that testimony, Friedman reaffirmed (on pp. 2178, 2191) his view (expressed earlier in his March 1964 Congressional testimony: see Committee on Banking and Currency, U.S. House of Representatives, 1964, pp. 1143–1144, 1152; see also the discussion in Rotemberg, 2013) that Federal Reserve policy in line with Congressional opinion would have implied fewer major monetary policy mistakes at the cost of more frequent minor mistakes. The early 1930s experience, in which Congress was a source of pressure for more expansionary policy, figured heavily in this judgment.
93 That said, Friedman had very strong views about the appropriate choice and settings of monetary policy instruments, as discussed below.
consistent with long-run price stability, and to make that target public. For Friedman, the
requirement for a monetary growth target helped relieve a situation in which the Federal Reserve
was, as he had put it, “subject to no very clearly defined Congressional mandate.”

Friedman was, consequently, initially elated about Resolution 133. In *Newsweek* (June 2, 1975),
he hailed the Resolution’s linkage of the appropriate long-run monetary growth to longer-run
growth in real potential output, and its requirements for public statements by the Federal Reserve
concerning shorter-run actions, as comprising “perhaps the most important change since the
Banking Acts of the mid-1930s.” (*Newsweek*, June 2, 1975.)

But Friedman later retracted that assessment; in the early 1980s, he poured scorn on his initial
euphoria and indicated that he now viewed Resolution 133 as only a “noteworthy minor step[,]" rather than the major breakthrough that I had mistakenly interpreted it as being.”
The reason for this disillusionment are discussed presently.

*Monetary downturns and gradual disinflation*

On one metric—the avoidance of severe declines in monetary growth—official monetary
targeting apparently did have some effect on FOMC decisions. Wonnacott and Wonnacott
(1979, p. 320) suggested that the nosedive in monetary growth in the second half of 1974 was
what crystalized support for monetary targets in Congress. On the narrow criterion of avoiding
another episode like this, the imposition of monetary targets was successful: over the rest of the
1970s, dips in monetary growth were less protracted than in 1974, and the period saw no
recession.

But the existence of monetary targeting failed to lead policymakers to consolidate upon the
1975–1976 disinflation and, furthermore, did not prevent a renewed surge in monetary growth
and a return of inflation to double digits. Burns (1975) acknowledged that announced targets had
the value of putting the Federal Reserve in a better position to resist pressures to move to an
overexpansionary policy. What they did not stop, however, was the carrying-out over 1976 and
1977 of a monetary policy that *Burns* believed was anti-inflationary but that proved *in fact* to be
policy was achieving a middle ground between overly-tight and overly-loose postures and that

---

94 Friedman (1968b, p. 125).
95 Friedman (1982a, p. 108). See also Friedman and Friedman (1985, p. 108), a discussion that again quoted, and
criticized the verdict of, Friedman’s *Newsweek* column of June 2, 1975.
information from monetary-growth was receiving the right weight in the FOMC’s decisions; but—once again, just as in 1970–1972—monetary policy in 1976–1977 was in hindsight clearly far too loose and would have been closer to the appropriate stance if control of monetary aggregates had more firmly guided FOMC decisions.

**Friedman’s disillusionment with the practice of monetary targeting**

Indeed, as early as May 1976, Friedman confessed to being “sorely disappointed at the way in which [Resolution 133] has been converted into a meaningless exercise in public relations” (Instructional Dynamics Economics Cassette Tape 190, May 1976, Part 1). He would conclude that the targeting framework had been implemented in a way that had not led to the desired changes in monetary policy. Indeed, the fact that the Federal Reserve and other central banks found a way to reconcile monetary growth targets with policies that were contrary in substance to what Friedman recommended is likely the principal reason why in 2003 he stated that monetary targets had not been a success.96

In an earlier and elaborate statement of this judgment, Friedman had listed a number of aspects of monetary targeting in 1975–1979 that had “succeeded in rendering the [Congressional] requirement largely meaningless.” These included two aspects, each now considered in turn: the specification of several monetary targets at once (for M1, M2, and M3); and a shifting of the base for the monetary targets.97

**Multiple monetary targets**

Resolution 133 did not specify a definition of money, but in his Congressional appearance on the proposed resolution, Burns (1975, p. 153) noted that Senator Proxmire’s explanation of the resolution had suggested that it referred to M1. However, the resolution as passed remained nonspecific about the aggregate to be targeted. This lack of specificity affected how the FOMC responded to its new mandate. Proxmire would recall in 1987 that, though he had had M1 in mind in drafting the resolution, he had found that once it was passed, “Mr. Burns said, we will not only give you [targets for] M1, we will give you [targets for] M2 and M3.”98

---

97 See Friedman (1983a, p. 8; 1984a, p. 27). In these discussions, Friedman listed “specifying targets in terms of a range of growth rates, rather than dollar levels” and “shifting the base to which it applied its growth rates every quarter” as separate aspects of 1975–1979 monetary targets to which he objected. These two aspects can, however, be grouped under the heading of criticisms of base drift.
The Federal Reserve was always likely to have M1 among its targets. As the *Monetary History* had acknowledged, it was an M1-type aggregate that the Federal Reserve in the postwar period had regarded as “the” money supply, much more than M2, and M1 had featured in the Federal Reserve’s discussions of monetary aggregates in the early 1970s—to such an extent that part of Friedman’s critique of Burns (1973) was that Burns considered only M1 in evaluating whether monetary growth had been excessive in 1971–1972 (see Chapter 2 above). However, in its monthly non-mandated targets of 1974, the FOMC had targets for M2 as well. The Federal Reserve’s 1975–1979 public targets likewise referred to both M1 and M2, but they also added a target for M3.99

Friedman had long favored M2. And in the 1970s (but not later), the Federal Reserve Board essentially defined M3 as M2 plus thrift deposits. Consequently, M3 in the 1970s resembled a monetary series to which Friedman had on occasion referred favorably. In addition, as discussed below, Friedman was coming to view this M3 series as a more appropriate money supply concept than the existing M2 definition. It might, therefore, perhaps be expected that the FOMC’s setting of M2 and M3 targets alongside M1 targets would be something Friedman welcomed.

But this was not the case. Friedman argued that the use of targets for multiple aggregates was primarily “to obfuscate the issue and reduce accountability.”100 In this way, they were in Friedman’s view “a clever device by Arthur Burns to confuse the oversight operation of the House Banking Committee” and other legislative supervisors (*Boston Globe*, April 1, 1981, p. 45). Friedman maintained that the Federal Reserve should nominate a single aggregate as its monetary target and pursue open market operations in a manner intended to achieve that target. Although he preferred that M2 be the targeted aggregate, he also believed that an M1 target, with no other monetary targets pursued in tandem with it, was preferable to targeting a set of aggregates that included M2.101 Multiple targets, in his view, encouraged the illusion that the Federal Reserve could or should use a variety of control techniques (such as a complicated reserve requirement structure) in order to manage different monetary targets. Multiple targets also produced what Friedman called the “M1, M2 game” (Instructional Dynamics Economics

---

99 The resolution had referred to targets for both money and credit. Other than quoting the resolution’s text, Friedman largely ignored the reference to credit and, for the moment, the FOMC likewise did not specify a formal target for a credit total.
100 Friedman (1984a, p. 36).
101 See especially Friedman (1982a, p. 108; 1984a, p. 36). In correspondence of August 4, 1976 that he CC’d to Arthur Burns, Friedman implied that he preferred a single M1 target to the targeting of a set of aggregates that included M2 (Burns papers, Ford Presidential Library).
Cassette Tape 173, August 1975, Part 1), under which it became difficult to assess policy success when a target for one aggregate was missed while another was hit. Friedman accepted that different monetary aggregates had diverged from one another in the past, but he believed that the discrepancies between, say, M1 and M2 growth would recede if Regulation Q was removed and reserve requirements were streamlined. Reserve requirements, however, continued to differ across the demand deposits that were common to M1 and M2 and those in non-M1 deposits; and interest-rate ceilings on bank and thrift retail deposits remained in force throughout the 1970s, therefore serving as a potent force promoting divergences between the aggregates.

Base drift

What came to be called “base drift” in the Federal Reserve’s monetary targeting was something that Friedman identified as a problem as early as August 1975 (Wall Street Journal, August 21, 1975). Congress’ resolution had required the Federal Reserve to give a target for monetary growth for the coming twelve months. Friedman had regarded this as an important virtue of the resolution, and he contrasted it with the historical situation, in which the Federal Reserve had “never specified long-range numerical objectives” (Newsweek, June 2, 1975).

However, the FOMC implemented the resolution by setting targets for a quarter ahead. Argy, Brennan, and Stevens (1990, p. 48) noted that, in practice, this procedure did not mean the target rate was varied each quarter. Instead, during Burns’ final years in office (that is, the years of his tenure in which mandated monetary targeting was in force), the same target rate was kept over a calendar year. However, by expressing the target in quarter-ahead terms, the FOMC did, in effect, change the value of the money stock implied by the annual target. Its targeting practice

102 On Friedman’s wish to rationalize reserve requirements, see Friedman (1974d, p. 23) and his written submission for his testimony of November 6, 1975, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1975a, pp. 47–48). He distinguished these proposals from the Federal Reserve’s wish to extend reserve requirements to those commercial banks or thrifts that had opted to be outside the regulatory structure of the Federal Reserve System. Friedman felt that the Federal Reserve could easily offset by open market operations any effect on the money stock arising from nonmember bank activity (Friedman, 1974d, p. 22). The case he saw for imposing reserve requirements on these institutions was, he believed, valid only if this was part of an arrangement in which the same reserve requirement was imposed on all deposits in the aggregate targeted (Friedman, 1982a, p. 117). As discussed in Nelson (2018a, Chapter 4), he saw merit in that uniform reserve requirement being set to zero.

103 Bordo, Choudhri, and Schwartz (1990, p. 254) gave Poole (1976) as the earliest reference on base drift, while Newton (1983, p. 103) quoted September 1976 remarks on the subject from Karl Brunner (in particular, Brunner, 1976b, pp. 11–12). But the 1975 Friedman op-ed cited here (which referred to the base drift implicit in the FOMC’s July 1975 interpretation of its target) was an earlier example. Friedman did not, however, use the words “base drift” in that op-ed; he did use the term in Instructional Dynamics Economics Cassette Tape 208 (February 1977, Part 1), by which time the “base drift” terminology had been deployed in, for example, Wallich (1976, pp. 4, 5). Another early discussion of the concept was Cacy (1976a).
created a situation in which a miss in one quarter became inherited in the starting point (or base) for subsequent settings of the money-stock target. That is, during the Burns years, the actual money stock in the past quarter became the reference point from which target rates of growth for the period ahead were computed (see Walsh, 1987, pp. 5–6). In 1978, the legislative requirement pertaining to the monetary targets was tightened—requiring the FOMC to fix the base for its annual target as the end of the previous year (Walsh, 1987, p. 6). Even under this arrangement, base drift continued at a lower time frequency.

Instead of targets that were announced anew every quarter and expressed in growth rates, Friedman favored multi-year monetary targets, with gradually declining growth rates converted into a dollar target path for the monetary aggregate. That is, he wanted the money stock to be made trend-stationary. Instead, unit-root behavior of the money stock continued, and after roughly a decade of monetary targeting John Taylor was to note that “the money supply is a highly nonstationary series.”

However, Walsh (1986) would defend base drift as a way in which a regime of monetary targeting could appropriately take into account permanent shifts in money demand—that is, lasting changes in the relationship governing the absolute levels of real income, real money balances, and the opportunity cost of money. McCallum (1993) provided what was, in effect, an argument complementary to Walsh’s, when he contended that permanent shifts in money demand should be expected to occur continually and that the quantity theory of money should be regarded as, essentially, referring to the relationship between monetary growth and inflation—not between the corresponding levels series.

Much of Friedman’s framework fits in with McCallum’s characterization. We saw in Chapter 2 that Friedman did not deny that monetary velocity on occasion experienced permanent shifts. Furthermore, his policy recommendations rested primarily on the existence of reliable of growth-rate, rather than levels, relationships. There is therefore some tension between Friedman’s favoring of dollar levels targets for money and his longstanding practice of focusing on growth rates of money in monetary analysis and in policy prescriptions. However, Friedman did believe that (log) M2 velocity was stationary in the period since the 1950s, so he did see quantity-theory relationships in the United States as prevailing in levels and not just growth rates. In addition,
his increased insistence on levels-based targets for money also seems to have arisen from a concern that monetary-growth target misses would, in practice, be one-sided: an overshoot in period \( t \) might well be followed by an overshoot in period \( t+1 \).

As Wallich (1976) argued, concerns like this were not of great practical importance in the first year of monetary targeting, partly because some target misses, of mixed sign, averaged out across a number of quarters. However, persistent misses of the monetary target, and a corresponding effect of base drift on mean monetary growth, would become more important in the late 1970s.

**Federal Reserve operating procedures**

Neither the Federal Reserve’s use of multiple monetary targets, nor the fact that it allowed the targeted aggregates to accumulate base drift, was the main source of Friedman’s unhappiness with U.S. monetary targeting in practice. What was by far the greatest source of dissatisfaction for Friedman was, instead, the Federal Reserve’s operating procedures. He did not believe that the Federal Reserve had subordinated the management of the federal funds rate to the stabilization of monetary growth.

Indeed, any pretense on the part of the FOMC to targeting a reserves aggregate, and not to have the federal funds rate as its operating instrument, had been largely abandoned by 1975. Even though the Federal Reserve was formally adhering to a “reserves against private deposits” (RPD) targeting procedure until 1976, it was the case that formal records of FOMC meetings, which were regularly released publicly in the mid-1970s, provided the FOMC’s target band for the federal funds rate. Likewise, a November 1975 Federal Reserve Board staff memorandum (one rapidly made public, as discussed below) referred to the RPD regime in the past tense.

Informed outside commentary reflected the reality that the federal funds rate was the FOMC’s instrument, one example being a news item that referred to “the key federal funds interest rate, the rate that the Federal Reserve controls most directly” (*Washington Star-News*, January 5, 1975).\(^{106}\) In addition, to Friedman’s irritation, market commentary also often focused discussions of the Federal Reserve policy in terms of its implications for interest rates. For example, one market economist said (*Bankers Monthly*, March 15, 1974): “The Federal Reserve holds the key

---

\(^{106}\) Similarly, the *Journal of Commerce* (March 31, 1975) observed that it was a “direct result of Federal Reserve policy” that “money market rates… have reached new [recent] lows.”
to near-term interest rate movements.\footnote{107}

As noted in Nelson (2018b, Chapter 13), money supply theory—and, specifically, the area of central bank operating procedures—did not amount to a major research area for Friedman. It continued not to be in 1975–1976. What marked out the mid-1970s, however, was the degree to which Friedman became outspoken about the importance of operating procedures as a policy issue. This was especially the case in Congressional testimony he gave in November 1975.\footnote{108} It is worth dwelling on this matter, partly because it presaged the Federal Reserve’s temporary change in operating procedures in 1979–1982, but also because it was such a sensitive issue for Friedman.

It is this matter, of all those on which Friedman dueled with central bankers and central bank economists over the years, that involved the most contentious dispute. The debates on the Fisher effect, the natural rate hypothesis, monetary policy vs. fiscal policy, and cost-push vs. monetary views of inflation, though they were larger debates in terms of their implications for economic theory and policy, were far less likely to provoke the same degree of acerbity from Friedman as that on operating procedures. During 1975–1977, the Federal Reserve’s position on operating procedures magnified Friedman’s feeling of having been let down by Arthur Burns. And in later years, indeed for the rest of his life—including during the 1979–1982 regime, which he considered to be very far indeed from his own recommendations—the fact that the Federal Reserve did not switch to using a total-reserves or monetary base instrument continued to be a sore point with Friedman. In July 2002, Anna Schwartz told the present author that Friedman remained resentful and unhappy on the matter—a fact underscored by his continued advocacy, in the final decade of his life, of a monetary base or reserves instrument, both in printed discussions (for example, in Pringle, 2002, pp. 18–19, and in \textit{Wall Street Journal}, August 19, 2003) and in correspondence (including in 1999 with Bennett McCallum, in 1999–2006 with the present author, and in 2006 with Gregory Mankiw).

In the mid-1970s, Friedman regarded the Federal Reserve as still overly concerned with interest rates, as noted above. But the controversy on operating procedures transcended this issue and

\footnote{107} It is possible to defend these commentaries both because it is evident that the Federal Reserve was continuing to control the funds rate over this period and because, as stressed in Taylor (1999), regimes that do not involve an interest-rate instrument can be viewed in terms of the approximate interest-rate rule they imply.

\footnote{108} This testimony was for the Committee on Banking, Housing, and Urban Affairs, U.S. Senate’s (1975a) “Second Meeting on the Conduct of Monetary Policy.” The first meeting in this series had been on April 29 through May 1, 1975 (Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1975c). Friedman had not appeared at those first hearings. However, his former students, William Gibson and Beryl Sprinkel, and two of his former departmental colleagues, Carl Christ and William Dewald, had done so, as had Karl Brunner and Clark Warburton.
amounted to a disagreement on the matter of what procedure—when it was agreed that monetary-growth stabilization was the policy goal—was best for achieving that goal. “The major issue has shifted, I believe, from objectives to means,” Friedman testified in November 1975.109 As the title of a late-1975 Newsweek Friedman column put it, the question was one of “How To Hit the Money Target.”110

For Friedman, the fact that the putting of monetary targets on a more formal footing in 1975 was not followed by an abandonment of a federal funds rate instrument reflected conceptual errors on the part of the Federal Reserve, as well as what he regarded as an inertia that made it unamenable to a shake-up of its operations.111 In early February, several weeks before the passing of the resolution that mandated monetary targeting, Friedman suggested that the federal funds rate instrument was “something they [the FOMC] ought to forget about.”112

Similarly, in a newspaper interview given on February 21, 1975, Friedman remarked of current operating procedures: “It’s a technique that did make sense when they were trying to control interest rates. But it makes no sense whatsoever if they really want to control the money supply. The right way to control the money supply is to forget about the fed funds rate and just decide how much you want to add to the monetary base each month.” (Journal of Commerce, February 25, 1975, p. 3.)

To central bank economists taking the opposite position, Friedman’s arguments overstated what could be achieved by a change in operating procedures—and underestimated the financial market volatility that would be associated with adoption of a bank-reserves or monetary base instrument. And it deserves emphasis that it was indeed largely an argument between Friedman and central bank economists, rather than with academics. Tobin (1960) had come out in favor of an interest-rate instrument, but he did not contrast this with a reserves instrument, and much of his other research took the monetary base or reserves as the monetary policy instrument. Even papers that were explicitly concerned with the choice of monetary instrument (including Poole, 1970) had

---

109 From his testimony of November 6, 1975, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1975a, p. 42; see also p. 48 of his accompanying submission). Likewise, in Instructional Dynamics Economics Cassette Tape 179 (October 1975, Part 3), Friedman remarked that the problem was now not the Federal Reserve’s objectives but how to achieve those objectives.
110 Newsweek, December 8, 1975.
111 Many other monetarists took the same position as Friedman. One example is Anna Schwartz, who observed (in Hagerstown Herald (Maryland), March 1, 1975): “Nobody can set a price and then decide what quantity people are going to take at that price.”
112 Wall Street Week, Maryland Public Television, February 7, 1975, p. 18 of transcript.
typically taken for granted that the central bank could control the money stock perfectly if it desired and had cast the issue as the choice between targeting money and targeting (or managing) interest rates. That was not the issue at stake in the 1970s operating-procedures debate. This debate concerned whether, in the context of agreement that the central bank should target the money stock, the central bank should do so via a reserves instrument or, instead, a base instrument.

The academic debate on this matter had been very light. As a Federal Reserve Board staff memorandum—written in response to Friedman’s November 1975 Senate banking committee testimony—noted: “The issues involved are highly technical and detailed. While several of them have been widely debated, though not fully resolved, in scholarly journals and within the Federal Reserve System, others… have scarcely been examined by economists outside the Federal Reserve.”113 It was therefore not only not a major topic of Friedman’s research, but it was also not one of the principal areas on which he was engaged in debate (in print and in conferences) with other academic economists.

The debate Friedman conducted on the matter was largely outside journals, and it took the form of other public statements as well as interactions with Federal Reserve policymakers and their staff. In this connection, Friedman would over the decade from 1975 find himself at loggerheads with his former teacher Burns, Burns’ successors, and prominent Federal Reserve staff—including two former graduate students of his who now, as Board staff economists provided much of the analytical firepower against Friedman’s position on operating procedures. These two were Stephen Axilrod (at the Board since 1952) and David Lindsey (a Ph.D. student of Friedman’s who joined the Board from academia in July 1974). The Board staff memorandum prompted by Friedman’s November 1975 Congressional testimony was, however, unsigned.

This memorandum raised many of the issues that Axilrod and Lindsey would highlight in their later public-domain work and that would feature in the debate on operating procedures in both the United States and the United Kingdom in the late 1970s and early 1980s. The memorandum focused on the looseness of the reserves/money relationship in the short run, as well as the possible financial disruptions that could arise from a less-accommodative arrangements for the short-run provision of reserves to banks. In addition, the memorandum indicated continued reservations, like those Chairman Martin had expressed to Friedman in 1969, about the actual

113 Federal Reserve Board Staff (1976, p. 121).
desirability of the stabilization of monetary growth. \footnote{For discussion of these Friedman-Martin exchanges, see Friedman (1982a) and Nelson (2018a, Chapter 15).} “In any event,” the memorandum stated \citep[pp. 127]{FedReserveBoardStaff:1976}, “it is by no means obvious that it is economically desirable to severely limit shorter-run variations in money growth.” What is more, the memorandum also took a position that would recur constantly in arguments against a reserves-oriented operating regime: insofar as tighter reserves control delivered tighter monetary growth control, this would be associated with “short-run movements in interest rates of much larger dimensions than have occurred in the past.”

The memorandum was made available to the U.S. Senate’s Committee on Banking, Housing, and Urban Affairs and made public in 1976, and Friedman submitted a rejoinder to the Committee, declaring that the memorandum to consist of “vintage Federal Reservese.”\footnote{See Friedman (1976d, p. 130). “Federal Reservese” never caught on as a phrase. Instead, “Fedspeak” has become the prevalent term \citep[see][]{Bernanke:2004}.} Indeed, little in it would have surprised him, as it articulated the same arguments he had been hearing in his ongoing dialogue with the Federal Reserve—a long series of exchanges in what he would call \citep[Newsweek, July 24, 1978]{Newsweek:1978} “three decades of personal involvement with the [S]ystem.”

In his November 1975 testimony, Friedman had readily granted that for the 1974–1975 period, interest rates would indeed need to have fluctuated still further if closer control of monetary growth was to be achieved.\footnote{In addition, in his testimony of January 22, 1976, in \citep[p. 2182]{CommitteeBankingCurrencyHousing:1976}, Friedman said that the Federal Reserve “doesn’t adjust its federal funds target fast enough to keep pace with the market.” In this context, market meant market \textit{forces} rather than market interest rates; Federal Reserve policy actions could ensure that actual federal funds market interest rates conformed to the Federal Reserve’s target.)} But he regarded this extra variation as contributing to economic stability: the severity of the recession would have been reduced, as the 1974 collapse in monetary growth would have been avoided. Failure to let interest rates fall fast enough in 1974 had the unintended effect, Friedman contended, of “convert[ing] the minor recession of 1973 and 1974 into a major recession from 1974 to 1975.”\footnote{From his testimony of November 6, 1975, in \citep[p. 38]{CommitteeBankingUrbanAffairs:1975}.} Friedman went on to suggest that the FOMC subsequently reached a position of holding interest rates too low, thereby producing excessive monetary growth in mid-1975. He suggested that the growth of the money stock had subsequently softened as the Federal Reserve kept the federal funds rate up at a time when the private sector’s demand for credit was again subsiding, and that now (early November 1975) the FOMC was back to a situation of cutting rates, but not fast enough to keep monetary growth on target.\footnote{From his testimony of November 6, 1975, in \citep[pp. 38–39]{CommitteeBankingUrbanAffairs:1975}.}
Generally, however, Friedman felt that the Federal Reserve overstated the interest-rate volatility likely to occur under a regime that used reserves as the policy instrument and focused single-mindedly on stabilizing monetary growth. The analyses given by Federal Reserve officials and of their staff tended to treat all drivers of money demand other than short-term interest rates as fixed in the short run—an assumption that implied that there was a bivariate, inverse relationship between variability in monetary growth and the volatility of short-term nominal interest rates. Friedman conceded that greater control of monetary growth meant permitting greater interest-rate flexibility, especially on a day-to-day and week-to-week basis. But he believed that, beyond this time frequency, the monetary-growth variability/interest-rate volatility relationship became more complex, as other variables came into play. In particular, the interest-rate swings associated with a policy of stabilizing monetary growth would be moderated by the reaction of other endogenous variables that could move in the short run; these variables’ movements played a role in equating money supply and money demand. Such variables included expected future real income, expected future price levels, and the current prices of securities other than short-term assets.

In addition, as Friedman wrote in his rejoinder: “Note that in practice short-term rates have been highly variable. If the Fed has indeed deliberately accepted variability in monetary growth to avoid variability in rates, it has gotten very little in return. The end result has been neither stable monetary growth nor stable rates.”

Friedman criticized the “trust the Fed” tone of the staff memorandum. However, he later paid it, along with the numerous others that he had received from Federal Reserve Board staff over the years, the backhanded compliment that they read like “cleverly written briefs” by lawyers for the defense (Newsweek, July 24, 1978).

*The Burns/Friedman relationship in the era of monetary targeting*

Arthur Burns recognized the validity in principle of Friedman’s critique of Federal Reserve operating procedures. In 1976, for example, Burns observed that “efforts by the Federal Reserve to sustain particular interest rates could result in inappropriate rates of growth in bank reserves and money.” As Friedman saw it, however, when FOMC policymakers recognized that such a

---

119 See, for example, Friedman (1980, para. 18, p. 59; p. 56 of 1991 reprint).
120 See Nelson (2018a, Chapter 6) for further discussion of this Friedman position.
121 Friedman (1976d, p. 131).
122 Friedman (1976d, p. 130).
123 From Burns’ written answers in Joint Economic Committee (1976, p. 377).
situation was emerging, they changed their target value for the federal funds rate target, but too slowly. What he saw as flowing from this reaction process were stretches of time in which monetary growth and interest rates moved in the same direction, as the inadequate move in interest rates failed to halt the direction in which the monetary aggregates were going (Business Week, August 11, 1975).

This position amounted, of course, to a criticism of the soundness of the interest-rate decisions of Burns’ FOMC. However, for all his continuing criticisms of Federal Reserve policy, Friedman was (as indicated in Section I above) more subdued in late 1975 and throughout 1976 in his direct criticism of Arthur Burns. Friedman’s hard-hitting August 1975 Wall Street Journal op-ed on Burns’ “doubletalk” was not followed by similar columns in the Journal or Newsweek over the rest of the year or in 1976. In his vigorous criticisms of operating procedures, Friedman’s emphasis on “bureaucratic inertia” (in Newsweek, June 14, 1976, and in his November 1975 testimony in Committee on Banking, Housing, and Urban Affairs, U.S. Senate, 1975, p. 45) as the source of resistance to an abandonment of a federal funds rate target allowed Burns off the hook somewhat. In Congressional testimony in January 1976, Friedman remarked, “Dr. Burns is a former teacher of mine and a long-time friend, so I would certainly grant that I have been very much influenced [by him].” In a similar vein, the two corresponded in a friendly manner during the course of 1976, even before Friedman’s Nobel award in October of that year provided an occasion for another pleasant exchange.

Perhaps Friedman softened his direct criticism of Burns in 1976 because, although he was already convinced inflation would rise in coming years, he thought that the rise could be contained if monetary policy was moderate in the period ahead, and that he should provide backing for Burns against the many Keynesian economists calling for greater monetary expansion. Perhaps he also reasoned that a better relationship with Burns could help bring the Federal Reserve around to Friedman’s position on operating procedures. In any event, the relationship between the two of them was clearly away from its nadir of 1970–1974, and the foundations had been laid for the fuller restoration of their friendship that would occur in the late 1970s.

124 Also taking the edge off Friedman’s criticisms in his November 1975 testimony was the fact that that testimony pitted him against Paul Samuelson, who thus became, instead of Burns, the figure whom Friedman challenged on that occasion.
125 From Friedman’s testimony of January 22, 1976, in Committee on Banking, Currency and Housing (1976a, p. 2192).
126 See the correspondence over this period in the Arthur Burns papers, Gerald Ford Presidential Library.
In contrast, Anna Schwartz, after almost three decades as Burns’ employee, seemed to relish the fact that she could now be uninhibited in her criticism of Burns—as she was from 1973 onward, as a member of the Shadow Open Market Committee founded by Karl Brunner and Allan Meltzer. An indication of her views on Burns’ record to date as Federal Reserve Chairman was provided by her statement in an interview in early 1975: “We would not have had such a severe recession, and such a prolonged one, had the Fed been doing its job properly.” (Hagerstown Herald (Maryland), March 1, 1975.)

Friedman continued to interact with Burns directly from time to time in the mid-1970s. As well as keeping in touch through their occasional correspondence, the two had face-to-face encounters in this period, including in front of Federal Reserve Board staff at the meetings of the Board’s consultants panels. Friedman attended such meetings on June 22, 1973, December 12, 1974, June 24, 1975, and April 15, 1976. It was likely at the 1974 or 1975 meeting that one of the most-talked-about direct exchanges between Burns and Friedman occurred. Burns had developed excellent relationships with personnel in the Ford Administration (see, for example, Wells, 1994, pp. 145–146) and, according to Paul Samuelson, had acquired prestige as one of the most respected figures in Washington D.C., in light of Watergate and the fact that President Ford was still establishing himself as the nation’s leader (Newsweek, March 3, 1975). After the discussion of monetary policy had concluded, Burns said to the assembled consultants that it was well known that he had standing in government circles and asked if there was any matter on which the consultants thought he should engage with the administration. Friedman replied: “If I were you, Arthur, I’d stick to monetary policy.” (John Scadding, interview, January 22, 1992.)

The Bach Committee

Friedman’s participation at the Board’s consultants meeting of April 15, 1976, proved to be his final appearance on the panel in Burns’ tenure as Chairman. On this occasion, instead of discussing current monetary policy, Friedman’s role was primarily to participate in discussion of the forthcoming report by The Advisory Committee on Monetary Statistics (Bach and others, 1976).

This committee, although commissioned and published by the Federal Reserve Board and drawing on assistance from Board staff, consisted of academic economists; and Friedman, as discussed below, credited himself with suggesting such a committee in 1970. The committee was chaired by George Leland Bach (hence the committee being known as the Bach Committee), the longtime organizer of the Federal Reserve Board’s consultants panels. The other members of
the Bach Committee were Phillip Cagan, Clifford G. Hildreth, Franco Modigliani, and Arthur Okun.

The background of the report was dissatisfaction with the Federal Reserve’s data on monetary aggregates. The perennial issue of what to include in the various monetary definitions had been given added importance by the apparent speeding-up of financial innovations. In addition, the issue of accuracy of measurement of money had been brought to the fore by the fact that the Federal Reserve’s elevation in 1970 of monetary aggregates in its monetary policy discussions had been followed by a number of years in which monetary aggregate data appeared differently in retrospect from their initial estimates. In particular, the data for 1969 were revised upward in 1970, and those for the early 1970s were also revised upward substantially, on net.

A measure of the heightened interest in monetary aggregates was provided by the occasion of the first major revision, in December 1970. “A year ago it wouldn’t have happened,” one financial journalist noted (Christian Science Monitor, December 5, 1970). “Federal Reserve Board officials invite a dozen or so reporters to a background discussion on a revision of the money supply figures.”

Even before this revision, Friedman had recommended to Arthur Burns that the Board appoint an independent committee to study the collection of monetary data.127 A few years later, by which time the money growth data for 1971 and 1972 had also been subsequently revised up, Friedman remarked caustically: “It has long seemed to me little short of scandalous that the money supply figures should require such substantial and frequent revision.”128 The Bach Committee and Friedman’s participation in it was announced on January 31, 1974.129 Its report was published in mid-1976.130

Despite his many other activities at the time, Friedman played an active role as a member of the Bach Committee. His copious correspondence in connection with the subject of monetary statistics during the years of the committee’s existence underline the fact that—notwithstanding his drift over the previous three decades away from heavily technical work—there was still an

---

127 This was in November 26, 1970, correspondence to Burns, as cited in Friedman (1982a, p. 107) and alluded to in his Newsweek column of July 24, 1978.
130 The report was itself dated June 1976. However, the report was summarized in an article in the Federal Reserve Bulletin (May 1976) that also indicated that the report was already available to the public. Friedman gave the report’s date as “early 1976” (Friedman, 1982a, p. 107), and indeed it was likely essentially complete by the time Bach and Friedman discussed the report in the April 1976 closed-door meeting at the Federal Reserve Board.
element of the statistician in Friedman. His correspondence got deep “in the weeds” (about the mechanics of seasonal adjustment, in particular) and featured a considerable amount of handwritten formulas and tables on the subject. Indeed, although his own monetary research typically was most concerned with annual or still-lower-frequency data, the interaction of seasonality and monetary analysis continued to be an interest of Friedman’s even after the Bach Committee’s work was done. In the 1980s, this interest manifested itself in his agreeing to be a referee for the submitted version of Mankiw, Miron, and Weil (1987) (Jeffrey Miron, interview, June 20, 2013).

The main topic confronting the Bach Committee was, however, not seasonality, but the categories of financial assets that should appear in the definition of money. And here, notwithstanding Bach’s status as committee chairman, Friedman was something of a back-seat driver in the Committee. The final report has numerous examples of its writing style and many of his previously-expressed views about how to define money. Just as Friedman and Schwartz’s *Monetary Statistics* had recommended defining money primarily in terms of assets that the private sector regarded as largely equivalent, so did the Bach Committee. Thus, the committee recommended excluding negotiable certificates of deposit from the M2 definition on the grounds that, though formally classified as time deposits, these assets were *de facto* (bank-issued) commercial bills. The report also recommended that the Board publish an official monetary base series. The practice up to that point had been for the Board not to do so, with others—most notably the Federal Reserve Bank of St. Louis—constructing a monetary base series (and adjust the series for reserve requirements) from Board-published information.¹³¹

One recommendation the Bach Committee made had a counterpart in a change from the 1960s to the 1970s in Friedman’s own thinking. He and Schwartz had excluded thrift institutions’ deposits from M2. But, by the mid-1970s, Friedman indicated that he was now inclined to put them into an M2-type definition.¹³² Customers’ access to thrift accounts gave them considerable scope to withdraw funds on demand. Indeed, some monetary analysts felt that thrift accounts were more easily accessible on short notice—and so had greater similarity to demand deposits—than the retail time deposits included in M2.¹³³ A further key development in this area, occurred during the early 1970s, when thrift institutions in the New England area started offering Negotiable Orders of Withdrawal (NOW) accounts. New England’s commercial banks were

¹³¹ Jordan (1977, p. 125) stressed this recommendation and endorsed it, observing that “it is about time that the central bank of the U.S. also acknowledged the concept by publishing the data.”
¹³² See Friedman (1975d, p. 60) and Instructional Dynamics Economics Cassette Tape 144 (April 17, 1974), and his testimony of January 22, 1976, in Committee on Banking, Currency and Housing (1976a, p. 2182).
¹³³ See, for example, Charles Partee’s remarks in American Bankers Association (1979, p. 145).
subsequently permitted to issue such accounts, starting in January 1974 (Hafer, 1984, p. 21). The growth in popularity of these accounts received considerable coverage in the U.S. media in the mid-1970s (for example, Detroit Free Press, December 2, 1975). NOW accounts essentially amounted to interest-bearing checking deposits.

The Bach Committee came out in favor of redefining M1 and M2 in terms of asset characteristics, rather than in terms of whether the issuer was a bank or a thrift institution. Not only did this line up with the Friedman-Schwartz vision of defining money from a demand perspective, it also led the committee to recommend that most thrift deposits be included in M2.

The Federal Reserve Board’s official redefinition of monetary aggregates in 1979–1980 (Hafer, 1980; Simpson, 1980) largely reflected recommendations the Bach Committee had made with regard to the definition of money. In particular, the monetary base was added to the Board’s list of official monetary aggregates, the M1 and M2 definitions were defined in terms of class of asset rather than on a bank/nonbank distinction (so NOW-type thrift-issued liabilities became part of M1, and a larger category of thrift deposits came into M2), and the criterion for excluding certain commercial bank liabilities (like large certificates of deposit) from M2 was based on the notion that M2 should be a retail aggregate (see Whitesell and Collins, 1996, and Nelson, 2018b, Chapter 14).

It seemed, however, that Friedman was left unhappy at the end of the day. He acknowledged that the report had provided the impetus for the redefinition of money. But he viewed the main recommendations of the report as not having been adopted, mainly because its recommendations concerning seasonal adjustment (a matter on which, as already noted, Friedman spent a great deal of energy) were not implemented.134

The “missing money” controversy

Alongside this ongoing debate about money supply and monetary control, there was much dispute about money demand behavior in the years 1975–1976.

A celebrated study by Goldfeld (1973) estimated dynamic money demand functions on postwar U.S. quarterly data and reported that the estimated relationships were well behaved. However, this study was followed by a 1976 article by Goldfeld, titled “The Case of the Missing Money,”

134 Friedman (1982a, p. 107).
in which Goldfeld’s 1973 M1 demand equation was shown to have overpredicted the strength of real money balances since 1974. Goldfeld’s (1976) finding became a mainstay of intermediate macroeconomics textbook discussion during the 1980s (see, for example, Dornbusch and Fischer, 1981, pp. 234–237). The notion also became quite widely accepted among researchers and professionals that U.S. money demand stability had come to an end soon after 1973. Furthermore, a number of former students of Friedman’s, including Phillip Cagan, David Laidler, and John Scadding, largely endorsed the finding of a mid-1970s breakdown of the money demand function (see Judd and Scadding, 1982; Laidler, 1985; Cagan, 1987, p. 201).

Friedman himself was mentioned surprisingly little in this literature, and he was rarely called to comment on it himself.135 In a sense, he did not need to, because, though Goldfeld had reported a money demand function breakdown for M1, his 1976 study had actually affirmed the continuing constancy of an estimated M2 demand function.136 This finding was somewhat lost in the discussion of Goldfeld’s results, because policymakers and many academic economists tended, like Goldfeld, to part company from Friedman by focusing on M1 rather than M2 as the main monetary aggregate in the United States.137

For his part, Friedman put little weight on the findings of M1 demand instability. When asked in January 1977 whether the M1 or M2 demand function had shifted in recent years, Friedman replied: “Neither one. There is only a breakdown in the bad demand functions that people fit.”138 Friedman did not have confidence that money demand functions could be fitted to quarterly data, and hence he was not greatly worried when quarterly equations broke down. Stability of the long-run money demand function was what he regarded as the important criterion; fitted short-

---

135 Friedman and Schwartz (1982) cited Goldfeld (1973), but not Goldfeld (1976). The influence of the 1976 Goldfeld paper was felt, however, in Friedman’s (1987a, p. 9) acknowledgment of “anomalies that have arisen in econometric estimates of the short-run demand for money.”

136 Goldfeld’s (1973, p. 593) estimated income elasticity of M2 demand was very high—around 2—a finding that likely contributed to the low weight he gave to his M2 results. However, as the stability of M2 velocity in the mid-1970s attests, the result that M2 demand exhibited stability did not stem from setting the income elasticity appreciably above unity.

137 As well as Goldfeld (1976), see Judd and Scadding (1982, p. 1010) on the durability of the U.S. M2 demand function beyond 1973. Puzzlingly, however, a number of authors have cited Goldfeld (1976), Judd and Scadding (1982), or later contributions to the “missing money” literature as documenting instability in the M2 demand function—which this literature did not (for attributions of this kind, see, for example, MacDonald and Taylor, 1992, p. 195, and Rudebusch and Svensson, 2002, pp. 425–426). One study that did acknowledge that Goldfeld found M2 demand was stable was Blinder (1979, p. 188)—who, however, went on to make a seeming non sequitur by implying that, as policymakers were more interested in M1 than in M2 during the 1970s, the stability of M2 demand should not be deemed of interest for the purpose of studying the behavior of inflation over that period.

138 In Friedman and Modigliani (1977, p. 24).
run money demand functions could too easily break down because of the complexities involved in short-run economic dynamics.

The post-1976 money demand literature gradually confirmed Friedman’s position that M1 demand had not had serious instability problems during the 1970s—at least if relationships at a lower frequency than quarterly were the focus of attention. Indirect evidence that M1 demand was not behaving very badly in the mid-1970s came in the fact that, into the early 1980s, M1 growth continued to have good relationships with future rates of nominal income growth and inflation.\footnote{On the survival of the relationship between M1 growth and future nominal income growth over the 1970s, see, for example, Hafer (1980). (In contrast, Robert Solow was reported in *Kansas City Star*, January 27, 1980, claiming that the inflation and recession patterns observed in 1974 and 1975 were impossible to reconcile with a monetarist account.) The judgment that the nominal income growth/monetary growth and monetary growth/inflation relationship did not break down in the 1970s, irrespective of whether M1 or M2 is used, now appears to have become widely accepted (see especially B.M. Friedman and Kuttner, 1992).} Also notable was the regularity that M1 velocity seemed to exhibit a stable trend growth rate over the 1970s—one similar to the trend growth it had exhibited in the previous decade.\footnote{See, for example, Rasche (1987, 1990) and Poole (1988). Some of this evidence was based on estimates using the redefined M1 series (that is, the modern, post-1979, M1 definition). But the choice between old and new M1 had little bearing on Goldfeld-style demand equations, because old and new M1 growth behaved similarly through the mid-1970s. In particular, demand-for-money equations estimated on quarterly data using the Goldfeld (1973) specification continued to exhibit a break around 1974 even when the new definition of M1 was used (see Simpson and Porter, 1980, pp. 162–163, Hafer and Hein, 1982, and Goldfeld and Sichel, 1990). It is true, however, that the redefinition of M1 helped improve the stability of money demand (and the constancy of the trend in M1 velocity) in the second half of the 1970s, as that period witnessed major shifts to NOW accounts that clearly produced major discrepancies between the growth rates of the old and new M1 definitions (see Simpson, 1980).} But a more direct basis for questioning Goldfeld’s instability findings came from Hamburger (1977b, 1983). Hamburger argued that Goldfeld’s original (1973) specification was misspecified, because it implied an income elasticity for M1 demand well below unity—a result contrary to long-run money demand studies such as Chow (1966). Although treated skeptically by Judd and Scadding (1982, pp. 1007–1008, 1014), this critique would ultimately be strongly reinforced by long-run money demand studies of Lucas (1988), Hoffman and Rasche (1991), and Stock and Watson (1993). All these later studies found that long-run U.S. M1 demand equations with a unitary income elasticity exhibited parameter constancy as the estimation sample was extended through the 1970s and well into the 1980s.

Consequently, by the mid-1990s the professional consensus had shifted to the position that the long-run M1 demand function for the United States was stable over the decade of the 1970s, and that the shifts reported by Goldfeld’s reflected his use of a too-low income elasticity and an overambitious criterion of explaining quarter-to-quarter fluctuations in real money balances.
In the mid-1970s, however, the view that money demand had become unstable was widely accepted. Indeed, *Business Week* published an article (June 7, 1976), “Is Monetarism Dead?,” essentially on the strength of the money demand findings. Friedman was irritated by the omissions in the article; in particular, it overlooked the widely-acknowledged stability of M2 relationships. Furthermore, he complained that he had been interviewed at length for the article, only to be barely quoted in it (Instructional Dynamics Economics Cassette Tape 193, June 1976, Part 2).

Even before the *Business Week* and Goldfeld articles increased its prominence, Friedman was aware of what he called an “alleged decline in the demand for money” in 1975, as this postulated decline had been prominent in the Federal Reserve’s public discussions (Instructional Dynamics Economics Cassette Tape 190, May 1976, Part 1). Arthur Burns had been particularly outspoken on the matter. Burns saw the new reports of money demand instability as confirming his own long-held doubts about money/income relationships. Even in February 1975, when opposing monetary targets, Burns (1975, pp. 153, 154) had referred to the “fact that the public’s demands for currency, for demand deposits, for savings deposits, and for a host of other liquid assets are constantly changing,” while pointing to M1’s behavior in 1974 as confirming that “this concept of the money supply has lost much of its earlier significance.”

Federal Reserve Board staff studies of M1 demand would report the same sort of “missing money” phenomenon found by Goldfeld, with 1975 identified as a year in which real balances were especially short of predicted values (see Enzler, Johnson, and Paulus, 1976) and, in February 1976, Burns (1976, p. 121) would testify: “Increases in the turnover of money balances have been even larger than we at the Federal Reserve had anticipated.” Indeed, although Clark Warburton was very much on the same side as Friedman on the issue of monetary relationships, Burns took the opportunity of correspondence with Warburton to restate his own skepticism about velocity stability. “The experience during the current economic recovery clearly illustrates the importance of velocity,” Burns wrote. “The rate of turnover of M1 has shown an unusually large cyclical rise, in large part because [of] financial innovations and regulatory changes…”

---

141 The stability of the M2-income relationship in the economy over this period—discussed in Chapter 2 above—was highlighted in some discussions of the time, including in Fellner and Larkins (1976)—an article that appeared in the same issue of the *Brookings Papers on Economic Activity* as Goldfeld (1976). In addition, David Meiselman testified (in a hearing held on February 4, 1975) that M2 velocity had been “essentially constant since at least 1960” (see Committee on Banking, Currency and Housing, 1975b, p. 48).

142 Hamburger (1977b, p. 265) pointed to Paul Samuelson’s Congressional testimony of November 6, 1975 (given alongside Friedman) as another early case in which the claim was advanced that the demand function for M1 had exhibited a breakdown.

143 Burns letter to Clark Warburton, November 5, 1976, Federal Reserve Board records.
Parallel to the declarations of a shift in money demand were claims that the observed relationship between changes in the relationship between movements in money and in aggregate income. For example, Walter Heller (*Washington Post*, October 15, 1976) contended that “the monetarists have taken quite a beating” in 1976: nominal income growth and real income growth, he alleged, had exceeded the values that historical relationships with monetary growth would have implied. Likewise, Herbert Stein (1976, p. 73) argued that “the relation between the money supply and the national income has come completely unstuck.”

In light of the fact that M1 velocity continued to keep fairly close to a roughly 3½ percent growth line over this period, Burns’ emphasis on velocity instability seems overdone, even for M1. And, as Friedman emphasized when he addressed the Federal Reserve’s appeals to a fall in the demand for money, that appeal could hardly be justified when M2 was considered (Instructional Dynamics Economics Cassette Tape 190, May 1976, Part 1, and *Newsweek*, June 14, 1976).

Estimated equations linking nominal and real income growth to monetary growth also proved resilient over this period. For example, as discussed in Chapter 2 above, Feldstein and Stock (1994) found that the relationship between national income and (modern) M2 was constant over the 1970s. Furthermore, St. Louis-style equations linking nominal income growth to M1 (both the old and, especially, the revised definition) held up over the 1970s (Carlson, 1978; Hafer, 1980).

The likelihood that monetary growth, particularly M2 growth, remained a valid indicator over the 1970s, puts a new complexion on judgments on whether monetary policy was appropriate in 1976.

A notable example of such judgments was Dornbusch and Fischer’s (1978, p. 545) observation, with regard to monetary policy in 1975 and 1976: “It does seem that the Fed… came close to finding a path for the money stock that succeeded in financing the recovery, without putting pressure on the price level… The Fed’s careful steering of an anti-inflationary course seemed to be taking the economy slowly onto a desirable path.”

Although this judgment seems to hold up for FOMC policy during 1975, it in retrospect seems overly complacent regarding policy in 1976. Dornbusch and Fischer (1978) took money demand problems as clouding the signal from monetary growth in these years. Correspondingly, they interpreted the gentle decline in short-term nominal interest rates during 1976 as evidence of a
moderate monetary policy stance. In retrospect, it appears that the FOMC’s actions in 1976 were actually much more expansionary than the Committee intended and that Dornbusch and Fischer (1978) judged. A monetary policy in 1976 involving rising interest rates would have been more consistent with reconciling economic recovery and continued disinflation. Such a policy would have meant lower growth in M2 in 1976 than that observed, as well as slower growth in nominal income and real income over 1976 and 1977 than what was realized. But the policy would have helped avoid the excess demand and renewed inflation of the late 1970s.

Rudiger Dornbusch, who in the mid-1970s was emerging as a major member of the new generation of economists with the appearance of Dornbusch (1976) and other key studies, had been a University of Chicago student in the late 1960s and early 1970s, graduating in 1971. He briefly returned to the university as a member of the business school in 1974–1975, before joining Fischer at MIT (American Economic Association, 1981, p. 124). Dornbusch would always be a keen reader of Friedman’s work, but Dornbusch’s policy prescriptions in the late 1970s would, like the analysis of 1975–1976 developments in Dornbusch and Fischer (1978), diverge from Friedman’s. Dornbusch’s analysis during 1976–1978 would be predicated on the existence if a deep output gap that the authorities needed to eliminate by promoting quite large and rapid increases in household and business spending. Later, Dornbusch would come round to the perspective that Friedman had been articulating at the time: that, by mid-1976, the United States was back to the problem of creating too much—not too little—aggregate demand.

THE FINANCIAL CRISIS OF 1973–1975 AND ITS AFTERMATH

Somewhat lost in the accounts of the course of the U.S. economy in the mid-1970s that emphasize the outbreak of severe inflation and prolonged recession is the fact that the nation’s financial system was also in deep trouble in this period. Reflecting this aspect of the situation, study of U.S. financial developments led López-Salido and Nelson (2010) to characterize the United States as having its first financial crisis of the postwar period in 1973–1975.

The tone of discussions during this period of the state of the U.S. financial system is brought out by various contributions to a conference on bank structure held at the Federal Reserve Bank of Chicago in May 1975 (a time of the year when Friedman was out of town). The preface to the conference volume (Scheld, 1975) noted that “the program dealt with the perennial, but particularly timely, issue of capital adequacy as it relates to the soundness of the banking system.” The first chapter of the volume elaborated on the timeliness of the subject matter (Greenbaum and Taggart, 1975, p. 1): “Continued secular decline in recorded bank capital,
heightened economic instability, and recent failures of large banks have aroused renewed interest in... capital adequacy.” Another chapter (Sinkey, 1975, p. 85) opened with a reference to “recent happenings in the banking industry, namely the failures of United States National Bank of San Diego and Franklin National Bank of New York...” Still another chapter opened (Pany and Sherman, 1975, p. 226) by referring to the “unexpected failure and emergency merger of several large commercial banks.”

These developments occurred against the background of a squeeze in banks’ capital positions. As Greenspan (2001) would recount, large U.S. commercial banks’ equity-capital-to-assets percentage reached their postwar low of 4 percent in 1974. It was during this period that Friedman observed (Business Week, October 12, 1974) that “the only place where I do think we have an overleveraged position—where capital is really extraordinarily low—is in the banking industry.”

The financial strains on banks were also manifested in what Homan (1975, p. 265) described as “the failure since 1973 of three multi-billion dollar banks.” Here Homan’s count included the aforementioned two bank failures—specifically, of U.S. National Bank San Diego and Franklin National Bank—as well as the Security National Bank, whose formal failure was averted by absorption of the institution into another bank. Homan argued that these failures had “served to undermine the public's confidence in other banks and the system itself.”

Homan was writing in January 1975. By mid-1974, the situation was already at such a stage that one financial commentary (in Bankers Monthly, July 15, 1974) noted that banks were having problems rolling over their issuances of large certificates of deposit and commercial paper. This commentary added: “Financial community confidence probably is the lowest since the 1930s...” Friedman himself referred (in Instructional Dynamics Economics Cassette Tape 147, May 30, 1974) to the “great uneasiness and uncertainty about the character of the system.”

The assessments of Federal Reserve Chairmen in subsequent years underlined the seriousness of the mid-1970s financial crisis. Testifying in March 1977, Arthur Burns referred to the “public concern which arose in 1973 and [into] 1976 about banking,” and he added that “banks classified by the banking agency of our government [as being] in the ‘problem’ category... The number of such banks increased sharply in 1974 and 1975.”144 His successor, G. William Miller, noted of

---

144 From Burns’ testimony of March 10, 1977, in (p. 24, 37).
the banking situation in the four years from 1973, “That was a very traumatic period…” 145 And in 1979 remarks, Paul Volcker confirmed that the Federal Reserve had drawn up plans during 1974 for channeling funds into nonmember banks in the event that those institutions encountered “extraordinary liquidity problems.” 146

Consistent with these recollections, in 1975 the London Financial Times reported that the Federal Reserve was making contingency plans to provide large-scale assistance to the financial system in the event that the banking problems became still more elevated (Financial Times (London), March 7, 1975).

Policymaker concern was reflected also in the authorities’ regulatory and supervisory postures and in their pressing for changes in prudential practices. As the Chairman of Continental Illinois Corporation—an institution based in Chicago and one that would feature heavily in a later financial crisis—observed (Bankers Monthly, October 15, 1974): “For probably the first time in more than 40 years[,] the financial stability of the banking system has become a matter of concern not only in the investment community at large but to the regulatory authorities as well.” In this connection, one financial reporter also pointed to the fact that Burns “has been warning banks against excessive reliance on volatile short-term money [that is, wholesale deposit issuance] and has been urging them to acquire larger quantities of permanent capital.” These exhortations went in the same direction of bankers’ own inclination in the newly inhospitable financial environment, with Talley (1975, p. 125) observing that “even some bankers, including some very aggressive ones, nowadays seem to feel that bank capital ratios have fallen as far as they should go.”

The problems of U.S. financial institutions went beyond the commercial banks proper. Friedman observed (Instructional Dynamics Economic Cassette Tape 144, April 17, 1974): “I doubt that there is a savings and loan association or mutual savings bank in this country that is not technically bankrupt.” 147 Indeed, both Friedman and Alan Greenspan anticipated in 1974 that an official rescue of the thrift associations would soon be necessary. 148 This, however, did not in fact occur, as the thrifts survived on their own steam. An improvement in the thrift institutions’

---

146 From Volcker’s testimony of November 13, 1979, in Committee on Banking, Finance and Urban Affairs, House of Representatives (1980, p. 27).
147 In Washington Star-News (Washington, D.C.), July 15, 1974 (p. A–9), Friedman was more cautious, suggesting that “some” savings-and-loan institutions were “technically insolvent.”
condition, associated with the downturn in U.S. nominal interest rates from late 1974, meant that an official rescue of them was, for the moment, forestalled.

However, strains in the financial markets continued during 1975. These were amplified by the New York City municipal government’s financial problems—what Friedman called “the continuing Perils of Pauline of New York City” (Instructional Dynamics Economics Cassette Tape 181, November 1975, Part 2)—which came to a head in late 1975. Spreads shot up, and one commentator observed (Bankers Monthly, December 15, 1975, p. 2) that “banks and other investors have been much more selective in purchases of tax-exempts and non-government taxable debt obligations, resulting in yield differentials between prime and lower-rated issues that are near the widest levels in history.”

The overall picture therefore is of a financial crisis in 1973–1975—one that was not on the scale of 2007–2009 in its severity, except perhaps on the dimension of the rundown of banks’ capital margin, but one that nevertheless satisfies the characteristics of a financial crisis spelled out by Reinhart and Rogoff (2009)—and whose presence in the record calls into question the Reinhart-Rogoff position that the United States had no financial crisis in the postwar period until 1984.

As discussed in detail in López-Salido and Nelson (2010), the case for classifying the 1973–1975 period as one of financial crisis for the United States is reinforced by the fact that Reinhart and Rogoff classify the United Kingdom as having a financial crisis in 1973–1974. Yet this period saw no closures or official rescues of ordinary commercial banks in the United Kingdom, whereas the same period did see such activity in the United States, as noted above.

Most notable of these closures was the failure of the Franklin National Bank, which was declared insolvent in October 1974 after having been the twentieth-largest commercial bank in the United States at the beginning of that year (Wille, 1974, p. 1040) and which would be described by Arthur Burns as “such a large bank.”

In his own reaction to the situation, Friedman recognized, as indicated above, that banks had become overleveraged. The notion that a healthy bank capital margin was desirable was a persistent Friedman theme since the 1950s, and—in contrast to his hostility to many of the regulations, such as Regulation Q or variable reserve requirements, that had been justified

---

largely for monetary-control purposes—he was receptive to the notion that a minimal capital requirement be imposed on banks for prudential purposes.\textsuperscript{150}

Some of Friedman’s statements during the 1970s on the banks’ condition seemed, however, to play down the need for public-sector intervention. For example, while Franklin National was teetering, Friedman indicated that he thought that Franklin National should be allowed to close (Instructional Dynamics Economics Cassette Tape 150, July 24, 1974). He also said of the thrift institutions (\textit{Washington Star-News} (Washington, D.C.), July 15, 1974, p. A9): “I would let them go bust.” Around the same time, Friedman also speculated (Instructional Dynamics Economics Cassette Tape 148, June 11, 1974) that it might be desirable to have a few bank failures, but not on a crisis scale, in order to induce U.S. commercial banks to raise their capital ratios.

In an important respect, however, these statements about allowing institutions to fail obscured more than they revealed, owing to the ambiguity involved in talking about the “failure” (or “closure”) of a bank. By failure, Friedman in the 1970s seemed to have in mind, as did many others, letting the value of shareholders’ equity in the bank dwindle or disappear. This was in contrast to the early U.S. bank failures of the 1930s, when deposits were allowed to be marked down in value. That is, commercial bank failures in that earlier period were associated with part of the money stock being extinguished.

The example of Franklin National—the largest bank failure in the postwar period up to that time (Litan, 1994, p. 524)—provides a clear case in point. When it was formally declared insolvent, this occurred—as Friedman’s friend Walter Wriston, the Chairman of First National City Bank, noted—“without any loss to its depositors” (\textit{Washington Star} (Washington, D.C.), April 6, 1975, p. A1). Its deposit liabilities (both retail and wholesale) were redeemed in full. This was a development of a post-New Deal practice that Friedman had commented on during the 1950s—one in which officialdom’s handling of banks typically meant that depositors were unlikely to suffer losses even when a bank failed.\textsuperscript{151}

\textsuperscript{150}See Nelson (2013). Of course, Regulation Q had initially had a prudential rationale—one that Friedman and Schwartz (1963a, p. 444) rejected as stemming from a misinterpretation of the 1930s downturn—but, by the 1960s, it had evolved into a monetary-control technique (with its value for this purpose being criticized by Friedman, 1970d, and defended by Tobin, 1970a).

\textsuperscript{151}See Friedman’s October 1959 testimony in Joint Economic Committee (1959, p. 3031). See also Friedman and Schwartz (1963a, p. 437). Litan (1994, p. 524), like Friedman, noted that the U.S. regulators’ practice of arranging “mergers of failed banks instead of paying off their depositors and thus effectively guaranteed in full deposit accounts above the statutory insurance ceilings” dated to the 1950s. Litan saw this policy as having become explicit with the 1974 policymaker handling of Franklin National Bank. See also Tobin (1985a, p. 24).
Friedman was critical of the authorities, and the Federal Reserve in particular, for keeping Franklin National going by heavy discount-window loans during the first half of 1974, by which time its eventual closure seemed likely (Newsweek, July 15, 1974). He clearly would have preferred a more rapid winding-up of the bank, as already implied. But the other major part of the U.S. authorities’ response—the protection of depositors—was something Friedman largely took for granted and was a move that could be justified by his own edict that the money stock in the face of financial crises. Accordingly, when he observed (Washington Star-News (Washington, D.C.), July 15, 1974, p. A9) that “there won’t be a major financial collapse,” part of Friedman’s confidence doubtless stemmed from his presumption that operation of large-scale deposit protection—beyond that required by law—would help insulate the overall banking system from repercussions of Franklin’s problems. Indeed, a hint that Friedman did not truly advocate a laissez-faire-like response to the banking problems was provided in an observation that he might like to see market mechanisms be allowed to proceed, in the form of some institutions simply failing. Might but not would: If the market solution entailed losses to shareholders but not depositors, Friedman was not disturbed. If the market solution threatened full redemption of depositors’ funds, Friedman was much more congenial to public sector intervention. Nevertheless, some of Friedman’s statements over this period, while recognizing and partially endorsing the likelihood of a high degree of public-sector protection of the financial system, seemed too sanguine. Foremost among these was his remark, cited earlier, that it might be useful to have some bank failures to spur the system as a whole to boost its equity cushion. As noted, Friedman qualified this statement by affirming that large-scale failures would be undesirable. But his position seemed not to appreciate the degree of interconnectedness between financial institutions—particularly in the era of large wholesale deposit markets, markets that both connected banks to each other and provided major banks with large, but volatile, private-sector sources of funds—and the associated systemic risks associated with the ripple effects of a series of failures.

152 Friedman (1974c, p. 79).
153 Likewise, Friedman’s Washington Star-News statement that he would prefer that savings-and-loan institutions “go bust” was made in the context of comparison with a hypothetical alternative scenario in which a rescue operation protected the shareholders in these institutions from the danger of losing their investment (with this done by keeping the institutions going though government subsidies to thrifts’ payments of interest to depositors). It did not signify opposition to government intervention that wound down the institutions while protecting depositors. See Nelson (2013) and Edwards and Montes (2019) for other indications in Friedman’s statements that he favored protection of depositors and other creditors, but not shareholders, in the face of a financial emergency.
Likewise, in offering criticism for the Federal Reserve’s tactics concerning Franklin National Bank, Friedman overlooked the strategic success of the Federal Reserve and the other authorities in containing the scale of the systemic problems. The Franklin National Bank had experienced a run on its wholesale deposits, but a system-wide run on wholesale markets (such as would occur in 2007–2008) was most certainly avoided. A more major financial crisis was consequently forestalled, and a crisis-induced monetary contraction—that is, a decline in the deposit component of the aggregate money stock—prevented altogether.

The importance of ripple effects in the modern financial system was something Friedman seemed to have learned between the mid-1970s and the mid-1980s, when his commentaries on the Continental Illinois bank rescue and his subsequent remarks about savings and loans institutions’ problems indicated strong concerns about financial-system contagion (see Chapter 10 below).

U.S. commercial banks responded to their capital squeeze in the mid-1970s by a retrenchment of their loans. The state of affairs at the end of 1975 (Bankers Monthly, December 15, 1975, p. 2) was summarized by a banking commentator observed as one in which, following “[t]he trauma of 1973–75,” many banks, “confronted with rising provisions for loan losses, are emphasizing investment portfolio quality more than at any time since the Depression. This has been reflected in this year’s sizable buildup in bank holdings of U.S. Treasuries and federal agency issues.” The retrenchment thus largely took the form of a switch in banks’ assets from loans to securities. In addition, growth in managed liabilities became more subdued for a time.\(^{154}\)

The flipside of this was that monetary contraction was not part of the retrenchment process. One financial commentary suggested that banks were not moving to an expansion of their balance sheets in response to monetary policy’s injections of reserve balances because they were rebuilding their capital positions (The Plain Dealer (Cleveland), January 26, 1975). This commentary, however, seems to have been based on the misconception that the Federal Reserve had already swung to ease by January 1975. It actually only did so in that month and subsequently. As discussed in Section I, it was in early 1975 that the Federal Reserve moved to a decisively easier posture and injected reserves into the banking system at a more rapid rate than previously. In the wake of this, growth in both M1 and M2 did indeed pick up promptly and sharply. The Monetary History’s position that it was possible for the authorities to generate an

\(^{154}\) See Cacy (1976b, p. 7) and Beebe (1977, p. 22).
expansion of the money stock even in the face of softness in the market for bank loans was borne out, as it had previously been in 1933–1935.

The fact of rapid monetary expansion underlay Friedman’s confidence in a rapid rebound of the economy from the 1973–1975. “There has been an explosion in monetary growth in the last six months, and that means there is a great deal of steam in the boiler,” Friedman observed in the third quarter of 1975 (Business Week, September 29, 1975). He contrasted his prediction with those of Walter Heller, Arthur Okun, Franco Modigliani, and other prominent economists, who had seen the softness of specific sectors, as well as the banks’ caution about lending, as grounds for expecting that the recovery would be slow (Instructional Dynamics Economics Cassette Tape 174, August 1975, Part 2). And, as noted in Section I, the U.S. economic recovery that began in the first half of 1975 and continued thereafter was indeed of the V-shaped form that Friedman had observed as typical of the initial stage of economic expansions that came after severe recessions.155

III. PERSONALITIES IN DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1975–1976

ARTHUR OKUN

During the 1960s and 1970s, Friedman’s views were frequently juxtaposed against those of Arthur Okun. For example, economics columnist Hobart Rowen referred to “[i]ntellectual opposites like monetarist Milton Friedman and Keynesian Arthur M. Okun” (Oakland Tribune (California), July 27, 1977). But, although he was one of the leading U.S. Keynesians of the 1960s and 1970s, Okun—unlike James Tobin, Paul Samuelson, Robert Solow, Franco Modigliani, and Walter Heller—had little in the way of extended exchanges with Friedman in print. Indeed, when their names appeared on the same publication in 1976, it was not as adversaries, but as coauthors of the Bach Report.156

Nevertheless, Friedman’s and Okun’s separate written contributions during the 1960s and 1970s, together with their other public statements in these decades, clearly marked one another out as a

---

156 Later, their names also appeared alongside each other when both Friedman and Okun each provided endorsements for Gordon (1978) textbook. These endorsements were used in promotional material for the book. “One of the proudest things I achieved,” Gordon remarked, was that “the launch brochure for my textbook had a quote from both Milton Friedman and Arthur Okun, right on opposite sides. And that was, for me, like a symbol of how I bridged that gap and sort of steered my way down the middle.” (Robert Gordon, interview, March 21, 2013).
major exponent of an opposing line of thinking. However, as discussed below, over the same period Friedman and Okun absorbed concepts introduced in each other’s research into their own frameworks.

An exchange the two did have in print—one that almost evolved into a lengthier exchange between them—was in 1963. Okun was one of the discussants of Friedman and Schwartz’s “Money and Business Cycles” paper, presented at the 1962 conference on monetary economics that formed the basis for a 1963 issue of the *Review of Economics and Statistics* (and whose proceedings, as discussed in Section I above, would be reprinted as book in 1975). Okun (1963, p. 72) stated matters forthrightly: “I do not agree with the Friedman and Schwartz appraisal of the importance of money. I find their view of the world fascinating and stimulating, but I am not converted.” His comment focused on their estimates of the demand-for-money function and suggested that their case for money rested strongly on those estimates, which Okun found implausible.

In its manuscript form, Okun’s comment reached Anna Schwartz who (in a letter to Friedman of September 21, 1962) suggested that they write a short rebuttal to Okun’s comment. But Friedman was already on a long overseas trip when Schwartz made this suggestion, and the matter lapsed: Okun (1963) was published without an authors’ reply.

Perhaps it is just as well that Friedman and Schwartz did not publish a reply on that occasion. For, though they would certainly subsequently continue to hold the position that Okun (1963, p. 72) accurately attributed to them that “brings monetary policy to the fore and pushes fiscal policy into the background,” over the following years they would drop a couple of the specific elements of their “Money and Business Cycles” analysis with which Okun (1963) had found fault. In particular, they would continue to part company with Okun’s (1963) implication that money was an inferior good; but they would come to view the income elasticity of M2 demand as close to unity—in contrast to their 1963 analysis, which had put the elasticity at about 1.8. Friedman’s writings in the later 1960s and into the 1970s would, however, stress that his ranking of monetary policy over fiscal policy was compatible with a wide variety of values of the income and interest elasticities of money demand (see Nelson, 2018b, Chapter 13).

Friedman and Okun also came close to an exchange in print when, at the 1971 American Economic Association meetings, they both contributed papers on a December 28 session on the
topic “Have Fiscal and/or Monetary Policies Failed?” However, when the papers by both Friedman and Okun saw print in the *American Economic Review* a few months later, Friedman’s piece did not mention Okun, and Okun’s article did not refer to Friedman.

By this point—the early 1970s—Okun’s views concerning monetary policy had likely attained some appreciable, though largely unacknowledged, movement toward Friedman’s own. Certainly Friedman thought this had occurred. To be sure, Okun established himself over the 1960s as a leading advocate of major multiplier effects on aggregate demand of fiscal policy measures; and in this connection he had been criticized by Friedman for, in Friedman’s view, attributing national-income increases to the 1964 tax cut that should actually have been attributed to the U.S. monetary expansion that occurred alongside the tax cut. But, over the same period, Okun evidently also became more sympathetic to monetary views of the business cycle than he had been in 1963.

This posture was reflected in Okun’s behavior as head of the Council of Economic Advisers in late 1968 and early 1969, when he and colleagues put together the Johnson Administration’s final *Economic Report of the President*. One of Okun’s economist staff, Paul Wonnacott, was alarmed at a draft chapter on monetary policy by CEA member Warren Smith. “Warren Smith had a particular quirk—that he thought monetary policy didn’t matter—which I thought was bizarre,” recalled Wonnacott, who added that Smith’s chapter “was going to take Friedman on and show how absurd Friedman’s views were.” Wonnacott wrote a list of objections to Smith’s analysis and sent the list to Okun. “I was in to see Art on something, and when I brought the question of this chapter up, he just smiled. And that was the end of the chapter: It just got canned completely.” (Paul Wonnacott, interview, May 12, 2014.) The published version of the report had a section on monetary relationships (Council of Economic Advisers, 1969, pp. 89–93) that, while critical in key respects of Friedman’s findings (without mentioning him by name) and opposed to directing monetary policy toward maintaining constant monetary growth, acknowledged that money and GNP were reasonably closely related, on average. “Okun had a much more complex and reasonable position, in my opinion, than Smith did,” observed Wonnacott (Paul Wonnacott, interview, May 12, 2014).

Friedman did not, however, talk altogether kindly to Okun’s partial adoption of his views. In a 1970 talk in London, he suggested that his challenge to the Keynesian consensus had led to a

---

158 See Friedman (1972b) and Okun (1972).
159 See Nelson (2018b, Chapters 12–13) for further discussion.
“standard pattern” like that seen in other debates. His challenge, he argued, was first ignored, then followed by efforts to “make fun of him as an extremist, a foolish fellow...” Thereafter, he said, his Keynesian opponents had followed the practice of absorbing his own views while portraying him as “an extremist, one of those fellows who says only money matters...”\(^{160}\)

Keynesians’ reaction to Friedman’s monetary research did not proceed along quite these lines. Friedman had been portrayed by Keynesian critics as believing that only money matters even before much of the assimilation of his monetarist positions into Keynesian thinking had occurred. Most prominently, Okun (1963, p. 72) had characterized Friedman and Schwartz’s cycles paper as arguing “for the strong view that monetary changes fully explain observed business cycles.” Similarly, James Tobin gave a presentation at the American Bankers Association his review of their Monetary History (Tobin, 1965) by writing on the blackboard alternative positions on money—and associating with his own position the view that “MONEY MATTERS,” while attributing to Friedman and Schwartz the belief that “MONEY ALONE MATTERS” (see Samuelson, 1969, p. 7). And once he had accepted a greater role for monetary policy in U.S. economic fluctuations, Okun portrayed Friedman as having been refuted, not vindicated, by the intervening years’ research debate. Okun accomplished this by portraying the quantity theory of money (including Friedman’s version of that theory) as inseparable from a belief in a zero interest elasticity of money demand and by pointing to recent years’ studies that had confirmed that the demand for money was interest-sensitive.\(^{161}\)

As early as 1964, Friedman had objected to characterizations of him as believing that money drove output to the exclusion of other factors.\(^{162}\) In 1970, his frustrations with such attributions were, as indicated above, clearly evident in his lecture in London. By this time, the allegation that he believed only money mattered had become a mainstay also of Federal Reserve critiques of Friedman’s work. This fact was reflected in a July variant of the lecture he would give in London. In the July draft, he remarked: “Monetarists are often caricatured by their critics as saying that ‘only money matters.’ This is nonsense. I am myself so tired of being charged with this view that I have a standing offer of $100 to anyone (and double [that] for a member of the Federal Reserve Board) who can find a sentence in anything I have written that can reasonably be interpreted as saying that ‘only money matters.’”\(^{163}\)

---


\(^{161}\) See Okun (1970a) and the discussion in Nelson (2018b, Chapter 12).

\(^{162}\) Friedman (1964e, p. 20; p. 278 of 1969 reprint).

\(^{163}\) Friedman (1970e, p. 4). His grievance extended to Federal Reserve officials beyond Board members, and the same July draft took issue with the Federal Reserve Bank of New York’s president, Alfred Hayes, on the same lines...
These frustrations also came through in print in the same year’s “Theoretical Framework for Monetary Analysis.” That article observed: “Not only, say our critics, do we believe that money matters, we believe that money is all that matters.”\textsuperscript{164} What is more, on this occasion he named names when referring to the researchers who had so characterized him, for he immediately cited Okun (1963) and Tobin (1965). This passage taking exception to the Okun and Tobin characterizations was repeated in his and Schwartz’s book Monetary Trends—which appeared in 1982, two years after Okun’s premature death at age 51.\textsuperscript{165}

\textit{Okun and the natural rate hypothesis}

Okun had initially planned to return to Yale University when he left government service in 1969, but he ended up staying in Washington, D.C. and taking a position at the Brookings Institution. Okun acknowledged that the profile of the institution’s macroeconomists was overwhelmingly Keynesian.\textsuperscript{166} “Just as it’s difficult for Milton Friedman to get Keynesians at the University of Chicago, so it is difficult for us to get monetarists to come here,” he would remark (\textit{New York Times}, April 3, 1977).\textsuperscript{167}

The Brookings Institution nevertheless emerged as a major forum for discussions of the Keynesian-monetarist debates—largely thanks to the \textit{Brookings Papers on Economic Activity} journal that Okun coedited. The conferences underlying the successive issues of this journal featured the participation by critics of Keynesianism, including Alan Greenspan (before and after his 1974–1977 service as CEA head) and monetarist William Poole. “I was there, and David Fand was there, because Okun, particularly, wanted to have some representation from the other side,” Poole recalled (interview, April 30, 2013). Indeed, one upshot of the Brookings proceedings was another notable shift of Okun’s views in the direction of Friedman’s.

With disarming imprecision, Kareken (1978, p. 4) referred to this shift on Okun’s part when he observed: “Professor Friedman seems to have carried the day. Dr. Arthur Okun, former chair of the Council of Economic Advisers and continuing protagonist of Professor Friedman, wrote not too long ago (I have forgotten exactly where) that we are all accelerationists now.” Later, (p. 8). A year later, he again criticized President Hayes’ characterization of the monetarist position (Instructional Dynamics Economics Cassette Tape 78, July 14, 1971).

\textsuperscript{164} Friedman (1970a, p. 216).

\textsuperscript{165} Friedman and Schwartz (1982, p. 56).

\textsuperscript{166} An exception to this pattern was the institution’s employment of Friedman’s former doctoral student, William Gibson. Gibson served as a fellow at the Brookings Institution from 1973 to 1975 (American Economic Association, 1978, p. 156).

\textsuperscript{167} In fact, by the time Okun made this remark, Friedman was no longer located at the University of Chicago.
Okun’s former CEA staffer Paul Wonnacott provided greater precision when, in a textbook discussion, Wonnacott and Wonnacott (1979, p. 283) cited Okun (1975, p. 356) to document their statement: “In the words of Arthur Okun, ‘We are all accelerationists now.’”

The actual statement in Okun (1975) was: “Clearly, the short-term Phillips curve has shifted upward. In the sense of recognizing that shift, we are all accelerationists now (to reverse Friedman’s celebrated concession to Keynes).” On its own terms, this was not a very decisive acceptance of the natural rate hypothesis: even nonaccelerationist Phillips curves can imply a shifting tradeoff over time. But Okun’s discussion on the previous page (Okun, 1975, p. 355) made it clear that he did, indeed, see inflation developments in 1970 and into 1971 as vindicating the Friedman-Phelps Phillips curve and not merely the existence of increasing steepness of the Phillips curve. His highlighting of that period was also consistent with the _Brookings Papers_ studies of Robert Gordon, which had endorsed the natural rate hypothesis on the basis of the accrual of data from the early 1970s.168

Wonnacott and Wonnacott (1979, p. 283) observed that “the concession to Friedman” by Okun in 1975 was all the more notable in view of the fact that Okun was “an economist who had originally helped to popularize the Phillips curve in the United States.” Indeed, the aforementioned _Economic Report of the President_ that Okun oversaw in 1969 became more well known for plotting a downward-sloping Phillips curve for the United States. Likewise, in November 1969, ten months after leaving government service, Okun (1970b) had acknowledged but rejected Friedman and Phelps’ version of the Phillips curve (partly because he incorrectly took ever-rising inflation as an empirical prediction of their hypothesis) and endorsed a permanent inflation/unemployment tradeoff—a conclusion that had led his discussant to refer to “Okun’s tradeoff approach.” (Sametz, 1969, p. 185).169 Furthermore, as discussed in Nelson (2018b, Chapter 14) the 1970–1971 Brookings meetings that Okun organized came out against the natural rate hypothesis. So it was that Okun (1980, p. 166), in a talk delivered in Friedman’s presence a few months before Okun’s death, acknowledged that the breakdown of the 1954–1968 downward-sloping empirical Phillips curve reflected the validity of the Friedman-Phelps specification, and “most of the profession (including me) took too long to recognize that.”

---

168 See Gordon (1976) and the discussion in Nelson (2018b, Chapter 14).
169 For the Okun discussion of the Friedman-Phelps position, see Okun (1970b, pp. 23–24; pp. 15–16, 33–34 of 1983 reprint). Okun’s discussion had quoted Friedman’s (1963) statement that inflation historically had often been steady over time as inconsistent with the natural rate hypothesis. But the natural rate hypothesis did not actually predict that inflation would inevitably rise; rather, it implied that the amount of stimulus to real economic activity from a given rate of inflation would diminish as that inflation rate came to be expected.
However, Okun’s acceptance of the Friedman-Phelps specification proved ephemeral. Over the same mid-1970s period, Okun’s views on modern macroeconomic developments crystallized and apparently left him believing that, although the Friedman-Phelps Phillips curve nicely described the evolution of inflation/unemployment patterns through 1970, subsequent events had rendered Phillips-curve-based analysis—including its expectations-augmented, natural-rate-reverting version—obsolete.

Thus, Okun’s conversion to the accelerationist view turned out to be fleeting, as he moved to a much more hardline cost-push position and to rejection of aggregate demand restraint as a measure against inflation. This fact is brought out particularly by his numerous appearances before Congressional committees. Okun declared in 1977 testimony, in which he also contended that “excess supply cannot break the momentum of inflation and restore price stability.” In contrast to Okun’s November 1969 talk, which had contended that sufficiently tight aggregate demand policy was “a sure and completely reliable remedy” for inflation, and even his 1974 assessment that “demand deflation… can cure inflation ultimately,” his 1977 judgment was: “Any professional economist who respects the facts must conclude, regretfully, that our ‘momentum’ inflation cannot be brought under control by any reasonable fiscal-monetary strategy.” Similarly, the following year Okun proclaimed that “fiscal and monetary restraint… would not cure the inflation of 1978.”

These diagnoses were encapsulated in Okun’s (1979, p. 1) contention that “the chronic inflation of the seventies is a new and different phenomenon that cannot be diagnosed correctly with old theories or treated effectively with old prescriptions.”

Okun’s advocacy of cost-push views

The interpretation of Okun as moving over the 1970s ultimately to an embrace of hardline cost-push views contrasts with what is often inferred in discussions of his well-known analysis in

---

170 Okun was an extremely frequent expert witness at Congressional committees during the 1970s, and he once observed: “Some of the best seminars I have ever participated in, Mr. Chairman, have been in testimony before this committee [that is, the U.S. Senate’s Committee on the Budget].” (From Okun’s testimony of February 22, 1978, in Committee on the Budget, U.S. Senate, 1978, p. 145.)
Okun (1978a). For example, Modigliani (1986, p. 34) portrayed Okun as accepting the long-run-vertical expectations-augmented Phillips curve, albeit with a low (though nonzero) output-gap elasticity.\textsuperscript{174} If narratives along these lines are valid, the appropriate conclusion would be that, in the final years of his career, Okun stuck to the acceptance of the Friedman-Phelps Phillips curve that he had voiced in 1975.

Such a conclusion is, however, at variance with many statements Okun made in 1976–1980 that contradict an acceptance of a Phillips-curve framework. Indeed, in one of the final such statements—the aforementioned talk delivered in Friedman’s presence—Okun (1980, p. 166) portrayed empirical Phillips-curve analysis as in disarray: “Since 1970, the Phillips curve has been an unidentified flying object and has eluded all econometric efforts to nail it down.” Okun (1978a) is, by contrast, a fragile basis on which to conclude that Okun endorsed the expectational Phillips curve. For in the relevant portion of that paper he was summarizing estimates from other studies: as Gordon and King (1982, p. 206) observed, this aspect of the 1978 Okun paper was actually a “survey.” If, Okun (1978a) argued, others’ estimates of the accelerationist Phillips curve were valid, they implied that efforts at disinflation relying on demand restriction would produce an inordinate amount of lost output in the interim.

It would appear that the Okun (1978a) assessment of estimates of the inflation-dividend-from-slack was part of marshaling an overall position against demand restriction against inflation—a position he also backed with harder-line arguments. The latter, harder-line, arguments seem to be closer to Okun’s preferred ones. When he was describing his own central estimate of how the economy behaved, Okun implied that a given amount of slack would not lastingly reduce the inflation rate. That is, the reduction in inflation forthcoming from a period of slack would be nonexistent (and not merely small, as was the case the accelerationist-model estimates considered in Okun, 1978a). Evidently, Okun believed that if a Phillips curve did describe the inflation process at all levels of slack, the coefficient on expected inflation in that Phillips curve was unity (alongside a small slope on the unemployment gap or output gap, capturing the slack-to-inflation channel). But he implied that, in the period after 1970, U.S. inflation was not described by this process, and the actual process was instead asymmetric: excess demand could drive up inflation, but excess supply—negative levels of the output gap—did not matter for price changes after 1970.

\textsuperscript{174} Similarly, Taylor (1997, p. 279) suggested that in the 1970s in the United States the view that Friedman found himself arrayed against “was that it was hardly worth the high costs to reduce inflation, and this view was based on the expectations-augmented Phillips curve…,” while James Tobin (in \textit{New York Times}, November 11, 1979) seemingly used the Okun (1978a) summary of Phillips-curve estimates to assess the implications of demand restriction for U.S. inflation.
This perspective was reflected in numerous Okun statements in the later 1970s that indicated categorically that economic slack did not matter for inflation. One example: “despite the persistence of huge excess supplies, the basic inflation rate got stuck close to 6 percent, where it has essentially remained since mid-1975.” Another: “the facts of 1975 and 1976 should rebut the fantasies that inflation would disappear naturally in a slow recovery.” And still another: “we did have a double-size recession [that is, the 1973–1975 recession], and we gave a weak economy every possible chance to cure inflation, and it didn’t work.”

A motivating force taking Okun to this position seems to have been the phenomenon of stagflation. As stressed in Chapter 2’s discussion of stagflation, Friedman’s framework could account endogenously for stagflation via Phillips-curve dynamics, while pure cost-push theories could also provide an account for it (though by, essentially, making inflation exogenous). The implication was, as David Laidler stressed in a mid-1970s discussion (Financial Times (London), November 7, 1974), the monetarist position implied that stagflation was a temporary phenomenon, it was a permanent phenomenon according to the strict cost-push views associated with hardline Keynesianism. In particular, as Okun observed correctly, the accelerationist model implied that a recession permanently reduced inflation (see Okun and Perry, 1978, p. 213). Consequently, as Friedman noted repeatedly during the 1970s, a period of aggregate demand restriction would initially see high inflation and recession coexist, before giving way to a noninflationary or lower-inflation expansion. It was this prediction that Okun rejected, with Okun (1978b, p. 119) stating that his thinking in recent years had been driven by “the stubbornness of the inflation rate at high unemployment rates.”

Cost-push came to dominate Okun’s narrative of U.S. inflation developments. Even in the 1960s, when he adhered to the original Phillips-curve analysis, he believed that cost-push factors tended to exert a positive effect on the inflation rate. Indeed, his analysis in Okun (1970b) had criticized the Nixon Administration for abandoning wage-price guidelines, which he suggested could block “the substantial market power” of large firms and businesses” as holding up

---

175 In addition, his posthumously published treatise on inflation affirmed (see Okun, 1981, p. 239) that “all economists” had become accelerationists in the sense that they recognized that the Phillips-curve relationship prevailing in 1954–1969 U.S. data was gone, but, in connection with his own preferred framework, it also indicated that “the vanishing (or nonvanishing) tradeoff is not a part of this analysis” (1981, p. 243).
177 From Okun’s testimony of March 16, 1977, in Committee on the Budget, U.S. Senate (1977, p. 87).
179 See also Okun (1981, p. 242).
inflation. Correspondingly, in 1971 Okun praised the same administration’s adoption of wage and price controls and its rejection of Friedman’s opposition to such measures (see Nelson, 2018b, Chapter 15).

As the 1970s unfolded, however, Okun scaled up his estimates of cost-push shocks’ size, viewing the decade as characterized by “upward cost shocks” (Okun, 1978a, p. 352). True, he did acknowledge as contributors to inflation what in retrospect he judged to be the “serious mistakes of overstimulation that were made in the late 1960s and again in 1971–72.” But with the “prolonged period of excess demand” that this stimulation generated being over by the mid-1970s (Okun, 1978a, p. 352), Okun viewed developments in U.S. inflation in 1974–1980 very much in cost-push terms.

In Okun’s account, following the commodity price shocks of the first half of the 1970s, a “wage-price spiral became operative and continued to turn even when excess supply dominated the economy.” Okun described the situation that the United States faced as follows: “In many sectors of industry, prices are closely geared to direct costs plus a percentage markup, while wages follow a standard of equity relative to wages elsewhere or to the cost of living. With cost-oriented prices and equity-oriented wages, excess supply cannot break the momentum of inflation and restore price stability.”

This characterization predominated in Okun’s discussions of inflation by early 1976. After Okun’s presentation at a January 1976 congressional hearing, the committee’s chair noted that Okun, like Arthur Burns, was contending that inflation in the modern Western world was insensitive to supply and demand conditions.

Okun did acknowledge that agricultural prices were responsive to market conditions, including

---

180 See Okun (1970b, p. 26; p. 16 of 1983 reprint). See also his claim that the abandonment of guidelines produced a “very marked acceleration” of prices in key industries (1970b, p. 43; p. 27 of 1983 reprint).


182 In Okun (1974, p. 365), he specifically associated the “1973–74 inflation” with excess-demand pressures and indicated that these pressures were receding. Nevertheless, Okun’s assessment even in 1977 was that tight labor markets in 1973 “contributed very little to the upsurge in inflation” (Okun, 1977, p. 9).

183 From Okun’s testimony of February 5, 1979, in Committee on Ways and Means, House of Representatives (1979, p. 334).


185 See the proceedings of January 27, 1976, in Committee on the Budget, House of Representatives (1976, p. 64).
demand.\textsuperscript{186} But this concession did not really contradict his position that excess supply had ceased to be an influence on the aggregate inflation rate, because it was often supposed that commodity market conditions were not responsive to the \textit{level} of aggregate economic activity but only to its \textit{growth rate}.\textsuperscript{187} Consistent with this, Okun (1978a, p. 350) suggested that the \textit{change} in the output gap was a likely influence on inflation via a commodity-price channel, but that the \textit{level} of slack did not exert lasting downward pressure on inflation.\textsuperscript{188} This belief—in an influence on U.S. inflation of the growth rate, but not the level, of the gap—was also one adhered to during the 1970s by Federal Reserve Chairman Burns (see DiCecio and Nelson, 2013, pp. 410–411).

\textit{Developments in Okun’s advocacy of incomes policy}

Okun’s sympathy with cost-push views had a counterpart in his vigorous advocacy of incomes policies.

When the Nixon wage/price controls were expiring, Okun remarked (\textit{Kansas City Star}, April 30, 1974): “I think we’ll be out of the controls business for a while. Then, after its bad reputation wears off, we’ll be back into it. This has been the history of a lot of European countries.” Furthermore, Okun believed that a return to some form of national incomes policy was, in part, desirable: in April 1977 testified: “I think that countries that have adopted structural measures and incomes policies have found them worth having.”\textsuperscript{189}

In his April 1974 remarks, Okun had suggested that he would prefer some statutory control of wages and prices. However, he subsequently pursued proposals for incomes policies that, as he saw it, would generate private-sector adherence through incentives rather than compulsion. Okun specifically advanced a scheme that he labeled “real wage insurance” (see Okun 1974, pp. 369–371, as well as Okun, 1977, 1978a, 1979). This plan was also placed under the heading of what was called “tax-based incomes policy” (TIP).

\textsuperscript{186} See, for example, Okun’s remarks in \textit{Kansas City Star}, April 30, 1974, and his testimony of April 6, 1977, in Committee on Banking, Finance and Urban Affairs, House of Representatives (1977, p. 28), and his testimony of May 22, 1978, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1978a, p. 310).

\textsuperscript{187} Specifically, the belief was that commodity demand depended on the growth rate of aggregate economic activity.

\textsuperscript{188} This position implied that a recession, by itself, could reduce inflation, but that the disinflation would be wiped out if economic recovery was strong.

\textsuperscript{189} From Okun’s testimony of April 6, 1977, in Committee on Banking, Finance and Urban Affairs, House of Representatives (1977, p. 36).
Variants of TIP had, as Okun acknowledged, previously been advanced by both Sidney Weintraub (a longtime proponent of cost-push ideas in the United States) and Federal Reserve Board Governor Henry Wallich. In Okun’s version of TIP, firms and workers would be eligible to pay a smaller amount of federal income taxes if they agreed to adhere to limitations, laid out by the federal government, on wage and price increases. Okun argued that TIP was far preferable to demand restriction, which—as already indicated—he saw as ineffective against inflation: “The American public ought not to be satisfied with 5 percent or 6 percent inflation, but if we are going to have a serious effort to reduce that inflation rate, we have to adopt new policy strategies.”

When in 1978 Friedman was pressed to comment on TIP idea, he was withering: “The TIP plans are another form of government wage and price control, differing only by explicitly spelling out the penalties for violations. They would prove no less monstrosities than earlier price and wage controls.” He reiterated that the “one and only one cure for inflation” was demand restraint (Newsweek, May 29, 1978, p. 81). Around the same time as these remarks, Friedman likely expressed the same sentiment to Okun in person, when he served as discussant of Okun’s presentation “Current Monetary Policy” for the Federal Reserve Board’s consultants’ meeting of May 11, 1978.

Earlier in that year, Okun had publicly expressed indignation at what he considered the “groundless, indeed irresponsible, criticism that they [TIP arrangements] are a form of controls.” Okun suggested that as businesses and households could opt out of the TIP’s wage and price limitations, they were not controls. However, as discussed in Chapter 2, Friedman saw little distinction between formal controls and other types of incomes policies introduced by governments. Whether imposed by statute or introduced using carrot-and-stick methods, incomes policies aimed to allow the government to preempt the price- and wage-setting decisions of the private sector.

Unemployment, potential output estimates, and measures of resource slack in the 1970s

The 1970s are now seen as a period for which accelerationist-style Phillips provide a creditable performance. Indeed, even simple bivariate plots of unemployment and the output gap against

---

191 From Okun’s testimony of March 16, 1977, in Committee on the Budget, U.S. Senate (1977, p. 87).
192 Federal Reserve Board records.
the change in inflation look impressive for this period (see Blanchard, 1997, p. 344, and Roberts, 2004, Figure 1). Yet, it seems that at roughly the midpoint of this period, in 1975, even economists like Okun who were receptive to Phillips-curve approaches (including the accelerationist variant) then came to turn against it.

A less dramatic rejection of Phillips-curve mechanisms came from Paul Samuelson in September 1975. Samuelson remarked that while he believed that the recession was indeed a factor that had brought inflation back into single digits, he was now inclined to think that the “primary elements” of inflation’s behavior did not reflect a response to demand/supply factors but, instead, the influence of shocks to food and fuel prices, built-in momentum in wage contracts, and “the puzzling tendency of large corporations to administer their prices even in periods of weak demand” (*The National Observer*, September 20, 1975). Why were such cost-push explanations so in vogue during a period in which unemployment was high and inflation was falling?

One part of the explanation, already discussed in Section I, is that inflation exhibited a brief spike in the latter half of 1975 that seemingly defied the behavior of demand. However, the doubts about inflation’s sensitivity to the output gap continued after this short-lived rise in inflation. And for an understanding of the prevalent and long-lasting nature of these doubts, the vast differences between real-time and retrospective estimates of the output gap, as documented and stressed by Orphanides (2003, 2004), appear to figure crucially. They are especially relevant to the mid-1970s period covered in the present chapter. Orphanides presented real-time estimates of the output gap that exhibited a trough of around −15 percent in 1975. This number contrasts with revised estimates, as of 2019, that indicate that the actual trough of the U.S. output gap during the 1970s was −4.7 percent (in 1975:Q2).194

Taylor (2000) emphasized this trough in arguing that, during the mid-1970s, “serious economists,” including those in policy positions, did not take the gap estimate reported at the time seriously, notwithstanding the fact that it was prepared and issued by the U.S. government. It certainly seems to have been the case that the published official output-gap series underwent revisions at a slower rate than did policymakers’ own assessments of the gap. For example, even in 1975 the official figures were not indicating that the output gap became positive at any time in 1972–1974, yet by 1975 it was widely recognized by economists—including Okun, as indicated

---

194 From FRED series on U.S. real GDP and U.S. potential GDP.
above—that, during 1973, output had overshot its potential level. That is the most clearly valid aspect of Taylor’s position.

However, Taylor likely overstated leading economists’ and policymakers’ comprehension of the actual state of excess demand. A number of pieces of evidence indicate that leading economists actually believed that the output gap was in double digits during 1975, even if some of them did not suggest that it was quite as large as (negative) 15 percent.

Arthur Okun’s statements are particularly important in this context. In July 1975 Congressional testimony, Okun compared actual real GNP with various possible trajectories potential real GNP, each of these trajectories being based on different full-employment unemployment rates. Under 4.5 percent unemployment—the consensus full-employment rate of unemployment rate during this period (see Orphanides and Williams 2005)—Okun’s figure implied an output gap in 1975:Q2 of about −15 percent. In the same testimony, Okun called for demand stimulation to generate 8 percent real output growth over the year ahead.

Other leading Keynesian economists, as well as economists in U.S. policy circles, expressed similar views. For example, James Tobin described the U.S. output gap in the mid-1970s as in double digits, while Paul Samuelson called for 7 percent growth in real output in 1976 (Nelson, 2005). Congressional testimony by Walter Heller on January 28, 1975 (reprinted in Heller, 1976, p. 102) took the U.S. output gap as zero in 1973 and then widening to about 15 percent in 1975. Furthermore, the OECD-commissioned McCracken Report (McCracken and others, 1977, p. 82) used an estimate of the U.S. output gap that reached about −11 to −12 percent in the first half of 1975.

In his July 1975 testimony, Okun indicated that he did see the level of slack then prevailing as likely to reduce U.S. inflation. But it is easy to see why he soon shifted to a view that inflation was insensitive to the output gap when the gap was negative. Okun believed the 1975 output gap

---

195 See Wonnacott and Wonnacott (1979, p. 333). In contrast, the modern CBO output gap series (as of 2013) registers output above potential for beginning in 1972:Q1 and continuing through 1974:Q2.

196 See the hand-drawn chart in the written portion of Okun’s testimony of July 23, 1975, in Committee on Banking, Currency and Housing, House of Representatives (1975c, p. 78). The retrospective attempt to generate this chart in printed form in Pechman (1983, p. 475) did not appear to be numerically identical to the original figure but also implied an output gap of around the same magnitude.

197 From Okun’s testimony of July 23, 1975, in Committee on Banking, Currency and Housing, House of Representatives (1975c, p. 66).

198 His testimony of July 23, 1975, in Committee on Banking, Currency and Housing, House of Representatives, (1975c, p. 66), had said that an “encouraging aspect” of the situation was that slack would bear down on inflation.
was deeply negative and then saw the subsequent economic recovery was a disappointing one featuring a great amount of continuing resource slack. U.S. inflation fell in this recovery—but was higher than values often prevailing before 1973. Consequently, despite the decline in inflation, Okun came to see inflation and slack as disconnected in the United States.

Subsequent revisions to both output and potential output—most of them occurring long after Okun had moved over to the pure cost-push camp—did much to reconcile the behavior of inflation over the mid-1970s with that of the output gap. With regard to actual output, Okun’s July 1975 testimony had put the peak-to-trough decline in U.S. real GNP at 8 percent. Modern quarterly data on U.S. output (real GDP) instead put this decline at 3.1 percent. And, as already indicated, 2019-vintage Congressional Budget Office estimates of the output gap in 1975:Q2 suggest it was about −4.7 percent rather than a double-digit gap. They also imply the output gap had narrowed to about −2 percent in the first half of 1976.

In the mid-1970s, Milton Friedman had little in the way of special insights into looming revisions to U.S. output statistics. In addition, like Okun and so many others, he did not anticipate the major downward adjustments to historical estimates of potential output. Nevertheless, Friedman’s sense about the state of demand in the economy during 1975 was better than Okun’s.

The contrast between the perspectives of Okun and Friedman on this matter was brought out in their public comments during early 1975. As discussed in Section I, in early 1975 Paul Samuelson saw a danger of depression if the U.S. economic situation deteriorated over the course of that year. But Okun, in testimony on February 28, 1975, presented the prospect of depression as much more imminent: “It becomes ever more likely that the history books will record this episode as a depression.” Friedman, in contrast, gave no credence to such concerns: “There isn’t the slightest indication that the things which happened in the Great Depression are about to happen now.” (Tampa Times (Florida), March 11, 1975.)

Friedman’s implication that the output gap during 1975 was not as negative as Keynesian analysts were suggesting arose partly from his distrust of the unemployment rate as an indicator

---

199 See his written remarks in Committee on Banking, Currency and Housing, House of Representatives (1975c, p. 75). Similarly, shortly after the 1975 trough, financial commentator Sylvia Porter gave the estimated decline in real GNP during the 1973–1975 recession as being 7.7 percent (Detroit Free Press, June 5, 1975).

200 These calculations are based on data for the modern series in the Federal Reserve Bank of St. Louis’ FRED portal.

201 From the written portion of Okun’s testimony, in Joint Economic Committee (1975b, p. 912).
of unused resources. One aspect of his skepticism about unemployment statistics in this period has already been discussed in Section I: he believed that unemployment benefits had raised the natural rate of unemployment. On its own, this factor would narrow the output gap, by implying a lower level of potential output. But Friedman’s commentaries in 1975 also foreshadowed the upward revisions to actual real output that would further narrow the estimates of the output gap. Specifically, he implied that the demand for labor was stronger than the unemployment data suggested, because the rise in unemployment in the mid-1970s was accompanied by strength in the employment-to-population ratio, as teenagers and women entered the labor force in greater numbers (Newsweek, August 4, 1975). In July 1975, Friedman called for “looking not only at unemployment figures, but at employment figures.” With regard to the behavior of the latter set of data in recent years, he observed: “If you looked only at the employment figures... you might think we were in a boom.” (Instructional Dynamics Economics Cassette Tape 171, July 1975, Part 1.) The recession had seen the unemployment rate reach a (then) postwar peak. In contrast, Friedman noted, on the criterion of the employment ratio, the recession was the third-mildest of the postwar period (Instructional Dynamics Economics Cassette Tape 172, July 1975, Part 2).

What Friedman later in the year called the “value of looking at employment [ratio data] as opposed to unemployment” (Instructional Dynamics Economics Cassette Tape 181, November 1975, Part 2) was, in fact, something he had expounded for several years. As early as March 1970, Friedman was proclaiming the merits of the employment/population ratio as a measure of labor market conditions, writing on that subject to his longtime NBER colleague, Geoffrey Moore, who had become the Commissioner on Labor Statistics in the Nixon Administration.202 After leaving government service and returning to the NBER, Moore himself did much of the running on the issue, using contributions to the U.S. press to make the case for the employment ratio as a labor-market indicator (see, for example, Wall Street Journal, May 9, 1975, and Washington Post, July 25, 1975).

Friedman’s commentaries in the mid-1970s therefore provided multiple reasons for believing the output gap was narrower than Okun and others supposed. However, Friedman was not ahead of Okun in perceiving another reason why output-gap estimates in 1975 were exaggerated: the potential GDP slowdown, arising from the post-1973 step-down in long-run productivity growth. In a 1974 discussion of U.S. historical patterns, Friedman gave the long-term U.S. economic growth rate as 3 to 4 percent.203 But in a discussion the same year, when concentrating on the

---

202 Friedman’s letter to Moore (of March 9, 1970) on the issue was cited in an article on the employment ratio by Julius Shiskin, then Commissioner on Labor Statistics, in a December 1975 speech (see Shiskin, 1976, fn. 4).
203 Friedman (1974a, p. 13).
period since the early 1960s, he gave U.S. potential output growth as averaging “about 4 percent” (Newsweek, June 24, 1974). Friedman essentially reaffirmed a number close to 4 percent when, in November 1975, he described his 3 to 5 percent rule for M2 growth as meaning that monetary growth would “roughly match the rate of growth in our productive potential.” He continued to give rates of 3.5 percent or 4 percent for potential output into the late 1970s. This was not a very different pattern from that in Okun’s assessments. A longtime proponent of the 4 percent potential growth number, Okun reaffirmed that figure in July 1975 Congressional testimony. Along similar lines, Okun (1977, p. 10) gave potential output growth as 3.75 percent.

These assessments also lined up with official estimates. It was not until January 1979 that the Council of Economic Advisers marked down its estimate of U.S. potential output growth from 3.5 percent to 3 percent.

Okun’s law and the sacrifice ratio

The fact that Friedman’s estimates of potential output growth paralleled Okun’s reflected not only the fact that both economists failed to perceive the post-1973 slowdown, but also the reality that the measurement of productive capacity was not an area in which Friedman competed with Keynesians—and particularly with Okun, who had established himself as a leading researcher on the behavior of potential output.

In particular, a major reason why Friedman used the same potential-output growth estimates as Okun is that the latter economist had, in Okun (1962), done the pioneering quantitative and analytical work on constructing estimates of U.S. potential output, and Friedman largely conditioned on those findings. Certainly, Friedman had disagreements with the way Okun-style estimates of potential output had been put to use since the 1960s—for example, in forming the basis for policies aimed at targeting real variables, and in estimation of permanent-tradeoff Phillips curves. But he did not disagree with the notion of potential output and had used that concept (one that was already present in macroeconomics long before Okun’s work) himself for

---

204 From Friedman’s testimony of November 6, 1975, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1975a, p. 43).
205 See, for example, Instructional Dynamics Economics Cassette Tape 175 (September 1975, Part 1), Instructional Dynamics Economics Cassette Tape 198 (September 1976, Part 1), and Instructional Dynamics Economics Cassette Tape 208 (February 1977, Part 1).
206 See his testimony of July 23, 1975, in Committee on Banking, Currency and Housing, House of Representatives (1975c, pp. 67, 77).
many years in his discussions of aggregate supply (see Nelson, 2018a, Chapter 7). From this perspective, Okun’s work in constructing empirical potential-output estimates for the United States was an exercise to be welcomed.

The same was true of another innovation of Okun’s (1962) paper. As Okun recalled in 1978, “I have an article, [written] way back in 1961, which tried to link the relationship between unemployment and GNP. The result has held up remarkably well. It is called ‘Okun’s law’ by some of my friends.”207 One of the users of Okun’s law in the 1970s was Milton Friedman—bearing out B.M. Friedman and Wachter’s (1974, p. 167) observation that Keynesians and monetarists alike relied on some version of Okun’s law. The deployment of Okun’s law by both these camps reflected their shared position that aggregate demand principally drove cyclical fluctuations in output. Okun’s law was a numerical representation of one corollary of that position. It was a numerical representation of what Milton Friedman called the observed tendency for the “labor market to respond to changes in aggregate demand.”208

This is not to imply that the unemployment/output relationship was not clouded in the 1970s: indeed, at the White House conference on Inflation (of September 5, 1974) that Okun and Friedman both attended, Alan Greenspan remarked: “Everyone is ready to repeal Okun’s law…” However, Greenspan himself went on to defend the concept (Council of Economic Advisers, 1974, p. 20). And some of the structural changes in the labor market during the 1970s that Friedman stressed—such as the rise in the natural rate of employment and the increase in the employment/population ratio—did not necessarily fundamentally invalidate Okun’s law. For Okun’s law as expounded by Okun (1962) was a first-difference relationship, linking the percentage change in the output gap to the change in the unemployment gap.209 An empirical first-difference relationship of this type could be quite robust in the face of one-time shifts in the natural rate of unemployment that were not incorporated into the equation. Such shifts implied a large residual for the equation for a single period—not ongoing systematic errors. In light of the robustness of Okun’s law on this dimension, it is perhaps not surprising that Friedman used Okun’s law both in November 1970 (before he started pointing to likely increases in the U.S. natural rate of unemployment) and in November 1975 (by which time he was strongly

208 Friedman (1976b, p. 237).
209 It is true that Okun’s law was sometimes empirically implemented using the assumption that it applied in levels rather than only in first differences. See, for example, its use in the construction of output-gap series in OECD (1973, p. 8). But this should be seen as an attempted extension of Okun’s law rather than Okun’s law itself.
emphasizing recent increases in the U.S. natural rate). \(^{210}\)

Even in its first-difference form, however, Okun’s law—in the manner it was typically implemented in the mid-1970s—did prove to have systematic flaws. Two such flaws were very important. First, applications of Okun’s law were systematically astray in imposing a 4 percent potential growth rate in output when that value had become obsolete. Secondly, the coefficient, \(\kappa\), that translated output changes into unemployment changes in the Okun’s-law equation, \(\Delta(y_t - y_t^*) = \kappa(u_t - u_t^*)\), underwent a decline from about 3 to 2 after the early 1970s (see Blanchard, 1997, p. 363). \(^{211}\) Indeed, this decline in the Okun’s-law coefficient is believed to be one reason why the increase in the unemployment rate associated with the United States’ disinflation of the early 1980s proved to be less than the empirical accelerationist Phillips-curve estimates, surveyed by Okun (1978a), supposed (see Fischer, 1985, p. 57; B.M. Friedman, 1988, p. 65).

As Taylor (1992, p. 14) observed, “lower estimates of the costs of disinflation came to be more widely accepted,” and this was the case in the wake of the macroeconomic outcomes seen in the United States during the first seven years of the 1980s: a large increase in unemployment was associated with a dramatic decline in inflation, and the unemployment rate then declined in the course of a noninflationary economic recovery. In addition, as indicated above, the unemployment rate actually rose less in the course of the 1980s disinflation than was suggested by previous estimates.

This result did not, of course, overturn the fact that a temporarily negative output gap—of the kind Friedman in the 1970s insisted was an inevitable implication of a successful anti-inflation program—implied that unemployment would be unusually high (that is, \(u_t\) would be notably above its natural rate \(u_t^*\)) in a period of aggregate demand restriction. Therefore, Friedman accepted the validity of the concept of what came to be called the “sacrifice ratio” (Gordon and King, 1982, p. 206)—a temporary increase in unemployment or loss of output required to restore low inflation. Indeed, the fact that Friedman’s discussions of the Phillips curve were mainly in

\(^{210}\) See Instructional Dynamics Economics Cassette Tapes 60 (November 4, 1970) and 181 (November 1975, Part 2). (In both these applications, Friedman seemed to be using a multiplier of 2 to translate from unemployment-gap changes into output-gap changes, in contrast to Okun’s usual number of 3.) At roughly the same time as the first discussion, in a November 1970 talk in Chicago, Friedman (1971b, p. 62) in effect invoked Okun’s law (without giving a coefficient) when he remarked that real output growth had to exceed 4 percent for economic expansion to tend to reduce the unemployment rate.

\(^{211}\) These estimates provided retrospective validity to Friedman’s use of a lower Okun’s law multiplier in the discussions mentioned in the previous footnote.
terms of inflation and unemployment, rather than inflation and output, implied a sacrifice-ratio concept, expressed (as in Okun, 1978a) in units of the unemployment rate.212

Economists’ lack of knowledge of economic dynamics and uncertainty about estimates of the natural rate of unemployment continued to be reasons why Friedman was doubtful that a durable, structural Phillips-curve specification could be implemented empirically. To be sure, he maintained his position that the appropriate Phillips-curve specification was an expectational version (see the next chapter). But Friedman stopped short of publishing his own estimates of the expectations-based Phillips curve, partly because that would involve estimating a measure of economic slack. As was stressed in Chapter 2 above, the postulate that a short-run unemployment/inflation relationship existed was not something he doubted even in an era of stagflation. But, in light of the uncertainty associated with quantitative aspects of the short-run unemployment/inflation relationship, Friedman preferred to lay out a disinflation plan in terms of nominal variables alone.

When he was asked, during a visit to the United Kingdom in 1974, how much unemployment was needed to cure inflation, Friedman replied: “Nobody can answer that question and it’s like the question of ‘When did you last beat your wife?’”213 He was, however, forthright and consistent in his writings and statements in making the point that a period of high unemployment and economic slack was necessary to restore price stability. Hobart Rowen—the columnist noted earlier for contrasting Friedman and Okun—highlighted Friedman’s openness in acknowledging that his recommended monetary approach to reducing inflation would initially hit real economic activity.214

Friedman thus did not shirk from acknowledging, even before the major surge in the U.S. unemployment rate in late 1974, that disinflation would indeed entail a period of unemployment. “There is no way of slowing down inflation that will not involve a transitory increase in unemployment, and a transitory reduction in the rate of growth of output,” Friedman noted in September 1974. “But these costs are far less than the costs that will be incurred by permitting

212 Blanchard (2018, p. 98) observed that “while Friedman [1968a] referred to unemployment, he clearly had in mind output more generally,” so that the propositions about inflation and unemployment in Friedman (1968a) implied parallel propositions about inflation and output. Although Blanchard did not note it, Friedman actually made this connection explicit in his writings—for example, by using the natural-output concept in Friedman (1975c) and output-gap-based Phillips curves in Friedman (1970a) and Friedman and Schwartz (1982).

213 Newsday (U.K. television program), BBC2, September 20, 1974, p. 4 of transcript. Similar remarks that Friedman made in the same trip appeared in Friedman (1975f, p. 32) and in Irish Times (Dublin), September 18, 1974.

214 See Rowen’s discussion in Washington Post, October 15, 1976, which quoted Friedman (1973c) on the matter.
the disease of inflation to rage unchecked.”  (*The Guardian*, September 16, 1974.)

Friedman was also sporadically attentive to others’ estimates of the Phillips curve and the sacrifice ratio, as the citations of Robert Gordon’s 1970s work in his 1976 Nobel lecture would confirm. Prior to this, at the second Economists Conference on Inflation on September 23, 1974, Friedman expressed curiosity about the estimates reported at the meeting of the short-run unemployment implied by a disinflation (Council of Economic Advisers, 1974, p. 308).

Perhaps reflecting those estimates, Friedman in October 1974 went so far as to nominate values of 6.5 to 7 percent for the range of the U.S. unemployment rate that would need to be tolerated for a period of two to three years “in order to achieve the goal of reducing inflation” (*Chicago Tribune*, October 18, 1974). In March 1975, after the recession had emerged and become severe, he said that the unemployment rate had overshot the rate necessary for an orderly disinflation, and he added that 1975 and 1976 would be years of falling inflation and unemployment rates, higher productivity, and decreasing interest rates (which, generally speaking, they were). However, Friedman also expressed concern that the Federal Reserve would “step too hard on the accelerator” in 1975 as it had in 1971 (as discussed in Section I above, the feared repeat of excessive easing this did indeed occur, but primarily in 1976 rather than in 1975). As for what unemployment should be if the decline in inflation was to be maintained and consolidated upon, Friedman suggested that, beyond 1975, several years of unemployment at 5 to 6 percent would be required (*Tampa Times* (Florida), March 11, 1975.).

This was a revealing comment: Friedman clearly believed that such rates exceeded the natural rate. Yet, in retrospect, the baseline assessment is that the United States had a natural unemployment rate of about 6 percent over much of the 1970s (Orphanides and Williams, 2005, p. 1931). In this light, the vintage of Friedman’s statement—March 1975—is important. It predated two developments. First, he was taking 1975’s unemployment to be higher than it proved to be: he did not anticipate that economic recovery would begin almost immediately, in 1975:Q2 (a few months after the easing of monetary policy) instead of nearer to the end of the year. Second, Friedman did not foresee the degree of the rise in the natural rate of unemployment. The attention he drew to an uptick in the natural rate of unemployment arising from the enhancement of unemployment benefits would be prominent in his public statements later in the year and would follow developments that were still unfolding in March 1975. Friedman would, in particular, cite 1975:Q1 and 1975:Q2 as the period in which increases in unemployment insurance raised the U.S. unemployment rate (*Newsweek*, February 7, 1977; see
also Section I above). It was, therefore, not until the later 1970s that Friedman became more aware of the extent of the rise in the natural rate of unemployment that had occurred.

Belated though Friedman’s recognition of a higher natural unemployment rate was, it was well ahead of Arthur Okun, who in 1978 Congressional testimony declared that “the evidence today is that there is no more structural unemployment than there was in 1971.”

Friedman’s perspective on the inflation/unemployment relationship during the 1970s also appear more far-sighted, consistent over time, and modern than Okun’s. In 1969, Okun had remarked: “Just as we’d love to make omelets without breaking eggs, so we would love to correct our current price performance with no increase in unemployment. But no one in the world has a recipe for doing that.” Friedan himself closely echoed this sentiment nearly a decade later when, while stressing that “the economy would turn around” after an initial recession, he confirmed that a recession was indeed part of the remedy for inflation: “I don’t mean to say nobody’s going to get hurt. There’s no way to make an omelet without breaking some eggs.” (San Jose Mercury News, February 12, 1979.) In contrast, by the late 1970s Okun not only deprecated the influence of economic slack against inflation, he also believed that incomes policy allowed high inflation to be removed without any period of deficient aggregate demand: a complete disinflation could occur alongside “prosperity,” he insisted (Okun, 1978c, p. 284). And, starting from the situation of slack that Okun saw as prevailing after 1974, incomes policy could remove inflation, while aggregate demand stimulation could eliminate the negative output gap, with the stimulus modulated so that “the economy stays out of the zone of excess demand,” that is, the region of a positive output gap. A period of economic slack was an inevitable and unavoidable part of Friedman’s, but not of Okun’s, account of the disinflation process.

RONALD REAGAN

When President Gerald Ford took office, Friedman was well disposed toward him.
known each other since the days when Ford was a senior Congressman, through their shared connection to the American Enterprise Institute (Chicago Tribune, November 28, 1976). On the basis of that interaction, Friedman judged Ford “a splendid Congressman, a fine man…”218 Upon Ford becoming president in August 1974, Friedman said he expected Ford to “take the long view of things” (National Journal Reports, August 17, 1974, p. 1224).

In August 1975, on the anniversary of Ford’s ascendancy to the U.S. presidency, Friedman gave a generous assessment (Milwaukee Journal, August 11, 1975). In his judgment, Ford had “performed very well.” Friedman added: “There’s no doubt that he has encouraged an atmosphere of greater understanding of free enterprise in the maintenance of a free and productive society.”

Nevertheless, over the course of that first year of the Ford Administration, Friedman was discouraged by the president’s performance on numerous economic matters. And a little over six months after his August 1975 praise for Ford, Friedman was, in effect, calling for the president’s removal from office. He was doing so by writing supportively of Ronald Reagan, Ford’s challenger for the Republican party’s 1976 presidential nomination.

*Losing confidence in Ford*

Inflation was one of the areas in which President Ford’s conduct disappointed Friedman. Ford never reintroduced general wage and price controls or sought such control powers. But he nevertheless conveyed—in his own public statements, and in some of his actions—a largely nonmonetary perspective toward the sources and control of inflation. In this connection, Friedman’s unhappiness with Ford’s WIN program against inflation has already been noted in Chapter 2. Even after he had essentially dropped WIN, Ford irritated Friedman on the subject of inflation by failing to mention monetary policy when discussing inflation in an address to the nation (Instructional Dynamics Economics Cassette Tape 162, January 1975, Part 2).

Fiscal policy was another area in which Ford disappointed Friedman. Here one source of grievance was Ford’s confidence in fiscal policy as a macroeconomic-stabilization tool. When Richard Nixon was first elected in 1968, Friedman had proclaimed that, now, in the wake of the experience of the Johnson Administration’s income tax surcharge, “everybody believes” that the effect of a tax increase on inflation was negligible (Chicago Daily News, November 8, 1968, p.

Yet roughly six years later, President Ford announced a proposal in late 1974 to increase income taxes as an anti-inflation move.

As discussed in Section I of this chapter, clinching evidence of recession materialized in late 1974 and Ford dropped his tax-increase proposal and in early 1975 he proposed, and succeeded in getting enacted, an income tax rebate as a stimulus measure. Other than a change in sign, the action was similar to the 1968 tax surcharge, and the arguments Friedman applied against the effectiveness of that measure once more applied.\footnote{One difference between 1968 and 1975 was that the 1968 measure was designed to affect both aggregate demand and inflation, while the 1975 tax cut was intended specifically to affect aggregate demand. Another difference concerned Friedman’s respective postures toward the two tax measures. Though he regarded both as ineffective against aggregate demand, by 1975, he was becoming more inclined to invoke Ricardian equivalence, rather than crowding out, as the reason the measure would have little effect on aggregate demand. However, it would still be several years before his first reflex was not to appeal to crowding out, as Friedman would not really fully embrace the Ricardian equivalence view until the 1980s: see Nelson (2018b, Chapter 13) and Chapter 6 below. For the application of Ricardian-equivalence arguments to both the 1968 and 1975 temporary tax measures, see Kormendi and Meguire (1986).} As noted earlier, Friedman was dismissive of the effect a tax cut might have on aggregate demand. If anything, this argument applied more strongly to a rebate than to other types of tax reduction, and in February 1975, when the rebate was being proposed, Friedman lamented (\textit{Journal of Commerce}, March 4, 1975, p. 1; also quoted in \textit{Participant}, April 1975): “Today it is taken as an article of faith, as sure as two and two equal four, that deficits and tax reductions are a way to fight recession. I believe there is no theoretical or empirical evidence that this is a correct proposition.” Friedman’s retrospective judgment was similar: the 1975 rebate was “an absurd measure” (Instructional Dynamics Economics Cassette Tape 211, April 1977, Part 1).

Alongside his misgivings about individual fiscal initiatives of the Ford Administration, Friedman was dissatisfied with progress in containing the growth of the federal budget. One way in which this growth was manifested was in the size of the budget deficit. In April 1972, Friedman, describing fiscal policy as “highly expansionary” (Instructional Dynamics Economics Cassette Tape 96, April 5, 1972), had predicted that the United States was “heading toward [budget] deficits greater than at any time in peace both in absolute terms and as a percentage of national income” (\textit{Japan Times}, April 15, 1972). In October 1972 he had further predicted a “super gigantic” deficit for the 1973 fiscal year (\textit{American Banker}, October 9, 1972). But, in large part because of the economic boom and the surge in inflation, the U.S. budget deficit actually was lower as a share of output in fiscal 1973 than in the immediately prior years. In fiscal years 1975 and 1976, however, federal budget deficits (according to modern data) exceeded 3 percent of
GDP—the first time they had done so in the post-World War II period (Council of Economic Advisers, 2018, Table B–18).

Furthermore, to Friedman, the budget deficit remained an inadequate metric for representing the fiscal situation: it was the size of government spending that most concerned him. With regard to public expenditure, the picture had changed greatly since Nixon’s election in November 1968—an event that had led Friedman to predict “an absolute leveling off of the budget” (Chicago Daily News, November 8, 1968, p. 43). Matters had also altered drastically since March 1970, when the Nixon Administration economist Murray Weidenbaum (1970, p. 89) had laid out official projections indicating that real federal government purchases would fall by fourteen percent from 1969 to 1975 as the U.S. economy shifted to a post-Vietnam War footing. This decline in aggregate real federal purchases did not occur. Instead, growth in real nondefense spending swamped the decline that did take place in real U.S. defense expenditures. Friedman put this development into a longer-term perspective just before President Ford took office, by suggesting that a mechanical extrapolation of the trend since 1929 implied that total government spending (federal, state, and local), might exceed 50 percent of national income in 1988 (Newsweek, August 5, 1974).

Initially, Friedman hoped that Ford would create a change in course. He noted a possible contrast Ford might create with his predecessor (National Journal Reports, August 17, 1974, p. 1224): “While Nixon gave continuous lip service to cutting the budget, in point of fact he shifted back and forth.” But when, five months later, Ford laid out budget proposals, Friedman was discouraged: “Despite President Ford’s desirable and admirable emphasis on holding down government spending, even his [proposed] budget involves a very large increase in government spending.” (Instructional Dynamics Economics Cassette Tape 163, February 1975, Part 1).

Friedman did affirm, in commentaries later in the year, that he believed that Ford was sincere when expressing a desire to reduce taxes and diminish the role of government in the economy (Instructional Dynamics Economics Cassette Tape 176, September 1975, Part 2; Newsweek, October 27, 1975). Indeed, senior members of Ford’s economic team expressed sentiments on these matters that lined up with Friedman’s own. In particular, in April 1976 Ford’s Secretary of the Treasury William Simon, who had already expressed a reputation for being concerned about budget deficits, stressed the argument—already long emphasized by Friedman—that high public spending could be socially and economically damaging even when it was not associated with budget deficits (see Simon, 1978, pp. 10–13).
In the face of these expressions of intent, the actual Ford record on public spending was disappointing to Friedman. Of course, budgetary outcomes, as distinct from the administration’s budget proposals, reflected decisions by Congress, as well as the influence of U.S. economic performance. But Friedman was disappointed by what he saw as the president’s inadequate resistance to the expansion of public spending. Historical data on U.S. federal government outlays (that is, a total encompassing both purchases and transfer spending) show that these reached 20.6 percent of GDP in fiscal 1975 and 20.8 percent in fiscal 1976—higher than any values observed in any of the fiscal years 1947 through 1974 (Council of Economic Advisers, 2018, Table B–18).

When, in early 1976, Ford offered his proposals for the fiscal 1977 budget, Friedman concluded that, though they were being portrayed in media accounts as being tough, the appropriate characterization of them was different: on “any reasonable measure, Ford’s budget is huge…” (Newsweek, February 9, 1976.) By this point, Friedman’s criticism of the president had intensified sharply, and he was very amenable to the prospect of having Ford displaced on the Republican presidential ticket by Ronald Reagan.

Early Friedman/Reagan interactions

Although Friedman and Ronald Reagan had both played public roles in the Barry Goldwater presidential campaign of 1964, they did not meet until early 1967 (shortly after Reagan had been sworn in as Governor of California), during the months in which Friedman had a visiting position at the University of California, Los Angeles. Friedman would, however, later note that he might have influenced Reagan earlier than their first meeting: “we happen to know that Reagan read Capitalism and Freedom before I ever met him” (quoted in Roberts, 2006).

In addition to meeting Reagan, Friedman had occasion during this sabbatical to make his first endorsement of Reagan’s policy initiatives. Friedman was reported in the national media as supporting Reagan’s efforts to increase fees for University of California tuition (Fortune, June 1, 1967). Friedman applauded what he saw as a move away from subsidizing middle-class households, and he spoke in favor of the fee increases in a public debate with UCLA’s economics professor Michael Intriligator.220

---

With regard to his first meeting with Reagan, Friedman said, “I was very, very favorably impressed. I thought he was a very serious, thoughtful person. He is interested in the principles, and he has reached them by thinking them through and by reading.” Describing these and later meetings, Friedman observed, “I have found Reagan will discuss and understand complicated ideas.”\(^{221}\) Reagan’s grasp of, and—as Friedman saw it—his largely correct posture toward, key economic issues, were what Friedman emphasized. This contrasted with one feature that many critics, along with some supporters, stressed as a key characteristic of Reagan both before and during his presidency: his poor command of, and likely lack of interest in, detail.

In the early 1970s, Friedman related Reagan’s virtues to his junior colleague and office neighbor Stanley Fischer. “He taught me how smart Reagan was... He called him correctly. I mean, [Reagan] wasn’t a genius, but he had a very consistent worldview, and he seemed to have a lot of common sense. And that takes you far.” (Stanley Fischer, interview, August 30, 2013.)

*Interactions during Reagan’s second term as governor*

Friedman and Reagan made a couple of joint television appearances in Reagan’s second term (that is, 1971 to 1975) as Governor of California. On the first of these, in an appearance at the start of 1972 on William Buckley’s public-television program *Firing Line*, Friedman was distressed to find that Reagan was defending Nixon’s August 1971 New Economic Policy as an appropriate response to the danger of a severe recession. Friedman’s reaction was: “You’ve been snowed, Governor.”\(^{222}\)

In the same broadcast, however, Friedman also praised Reagan as an exception to the tendency of leaders of both major U.S. political parties to favor growth in the public sector. It was Reagan’s position on this matter that led to the second Friedman/Reagan joint television appearance.

On this occasion, in late 1973, they appeared on another PBS debate program, *The Advocates*. This time, in a program taped in Hollywood, they acted as a team—both arguing in favor of the referendum Reagan was holding on a measure to cap the growth of (state-level) government spending.\(^{223}\) Although they stood side-by-side on the program, this joint television appearance actually had less on-air interaction between Friedman and Reagan than their 1972 *Firing Line*

\(^{221}\) *Boston Globe*, April 3, 1983, p. 43. In this interview, Friedman mistakenly recalled his UCLA visit and meeting with Reagan as taking place in 1970, and this incorrect date was reused in Rayack (1987, p. 1).

\(^{222}\) *Firing Line*, PBS, January 5, 1972, p. 17 of transcript.

\(^{223}\) The appearance was taped on October 29, 1973 (*Pasadena Star-News* (California), October 30, 1973) and was broadcast on November 1, 1973.
panel did. Because of the mock-legal format of the program, each of them was “cross-examined” by a hostile interlocutor about the matter under debate. The program therefore did not really give rise to televised exchanges between Friedman and Reagan.

But, as Friedman acknowledged on the program, he had served on the task force that led to the Reagan ballot initiative under discussion—Proposition 1. And that initiative itself is noteworthy because of its intersection with Friedman’s developing views about how to limit the growth of the public sector.

Friedman had concluded that California’s constitutional limitations on borrowing had not served as an adequate curb on the individual totals of taxes and spending (Instructional Dynamics Economics Cassette Tape 115, February 14, 1973). In addition, he felt that while the population was becoming concerned about the scale of public spending, the way in which appropriations were decided upon meant that U.S. legislatures were not sufficiently receptive to this concern. To Friedman, each proposed individual government spending program might not encounter much opposition—either because it was generally popular, or because few people had a strong incentive to oppose it, as the financing of the program (notably taxes) and any negative externalities that the program generated were community-wide, while the benefits it produced tended to be concentrated. Yet this process might generate an aggregate level of public spending programs that exceeded what the populace would choose if it had an opportunity to choose that aggregate directly. Friedman had applied this argument to some degree in previous years, but it was one he articulated with increased emphasis during the 1970s. Later still, during the Reagan presidency, it became a central theme of the Friedmans’ book *Tyranny of the Status Quo.*

Friedman articulated the matter in his *Advocates* appearance as follows: “There is a fundamental defect in our present democratic structure. That defect arises from the fact that the legislature, the government, approves individual programs and that nobody takes a look at the whole… The problem is that if you handle each item of expenditure separately, the sum of the pieces tends to be larger than we would want to spend if we looked at it as a whole. The great virtue… of this

---


225 That is, Friedman and Friedman (1984, 1985).
proposal [Proposition 1] is that it gives the people for the first time an opportunity to look at the whole pie and decide how much of their income they want government to spend.”

Galvanized by this reasoning, Friedman devoted a considerable portion of his time in the decade from 1973 to 1982 to the promotion of constitutional-reform initiatives at the state and federal level that sought to put a cap on the overall budget. California’s Proposition 1 ballot was an early example. In addition to his help in preparing Proposition 1 and promoting it in his television appearance with Reagan, Friedman spoke in its favor in other media appearances and in the promotional literature for the initiative.

“I think Reagan quite liked Friedman,” observed Arthur Laffer (interview, June 10, 2014), who remarked that Friedman’s interaction with Reagan really got going “with Prop. 1 in California, which I thought was a mistake at the time. And not that I didn’t want spending [restraint] as they did, but I’d much rather do tax limits than spending limits, because they’re far more successful. Everyone loves government spending; they just hate paying for it. So if you go against government spending, you usually lose.” This proved to be the case with Prop. 1, which was heavily defeated. Friedman had been optimistic about the likelihood of a victory, believing that the general public would see that a limit on aggregate spending meant a limit on taxes. Indeed, Friedman would retrospectively refer to Prop. 1 as a tax-limiting initiative.

The proposition had indeed included tax limits and cuts among its provisions. However, its most prominent element, on which public debate had been centered, was, of course, its cap on the state government’s spending as a share of state (personal) income. And the governor’s advocacy of constitutional limits on spending would subsequently lead Friedman to attribute to Reagan a “pioneering role” (Newsweek, November 10, 1980).

The electorate’s rejection of Prop. 1 took place after the campaign against the proposition pointed to possible implications of the expenditure ceiling for individual state government programs. This line of criticism was buttressed by the actual text of the ballot paper for Proposition 1. Opponents of the measure succeeded in having a statement put on the ballot paper

---

226 The Advocates, PBS, November 1, 1973, p. 7 of transcript.
227 See, for example, the radio interview transcribed in Manion Forum, October 28, 1973, and the interview Friedman gave in a promotional booklet (Government of California, 1973). The latter item was excerpted in Los Angeles Times, June 8, 1973, some months before voting day.
228 Friedman learned of the defeat while recording his regular audio commentaries. Instructional Dynamics Economic Cassette Tape 133 (November 7, 1973) featured a break in recording in order for Friedman to hear news of the referendum defeat.
229 Friedman (1975a, p. 41).
to the effect that cutbacks in public services would flow from enactment of as the proposed amendment—an inclusion Reagan objected to, and about which he would express anger during his appearance on The Advocates. As for Friedman, the possibility of reductions in government services had been raised in The Advocates, when the cross-examination cited his recommendation (in Capitalism and Freedom) that the U.S. government should sell off national parks. Friedman dismissed the citation of Capitalism and Freedom as a red herring for the existing discussion, as the ceilings implied by Prop. 1 did not imply reductions from the existing level of government spending. He himself wanted public spending reduced, he explained, but the measure under discussion implied restraint but not absolute reductions in state government expenditure.230

Notwithstanding the defeat of Prop. 1, Friedman continued to see the introduction of restrictions on the government’s taxing and spending powers as the way ahead for the United States (Newsweek, June 23, 1975). At the federal level, one route, apart from constitutional change, that he saw as a way in which the population could make its views felt concerning restriction of the overall budget was through the executive branch. As the only positions subject to choice by the entire voting population, the U.S. president and vice president were, Friedman felt, the only posts in government on which public dissatisfaction with the overall size of government spending and taxes could systematically exert more weight than could the popularity of individual spending programs.231

Ronald Reagan was appealing to Friedman from this perspective both because he was known to have presidential ambitions (having had a short-lived participation in the 1968 Republican nomination contest) and because he shared many of Friedman’s views about the role of government. In November 1975, Reagan—who the previous January had ceased being California’s governor—had announced that he would be challenging Ford for the Republican nomination for the presidency at the 1976 election. Well ahead of that announcement, press coverage noting the possibility of a Reagan candidacy had pointed to Friedman as an influence on Reagan’s economic thinking (see, for example, Dallas Morning News, February 18, 1975).

With regard to inflation, Reagan’s position paralleled Friedman’s in blaming inflation on aggregate demand policy rather than on cost-push factors. But Reagan in the 1970s articulated a far more mechanical view of the connection between fiscal deficits and inflation than Friedman

---

230 The Advocates, PBS, November 1, 1973, p. 11 of transcript.
231 See Friedman and Friedman (1985, p. 64) and Friedman (1987b, p. 220).
could countenance (see Nelson, 2005). This partly reflected a presumption that the occurrence of federal deficits led to monetization of the deficits—a sequence whose empirical applicability Friedman had come to doubt for the United States. Even state government deficits—for which there was even less basis to suspect a link with money creation—drew a negative reaction from Reagan: in his Advocates appearance, he described financing California’s government spending by borrowing as a “fraudulent” practice.\(^\text{232}\) Reagan’s strong expressed aversion to government debt issuance stretched right into the period when he started running large deficits himself as U.S. president. For example, a few weeks into his presidency, Reagan declared that “we cannot continue on the path of ever bigger deficits and just endlessly running up debt... We’ve gone along for decades now with this belief that we could spend more than we take in...”\(^\text{233}\)

_Friedman lashes out at Ford_

It was energy policy (discussed further in the next chapter) that proved to be the final straw in Friedman’s evaluation of President Ford. This subject had already generated some sharp criticism by Friedman of the Ford Administration when, in early 1975, it had been proposed to tax oil and reroute the resulting revenue to households in the manner of a negative income tax (NIT). Friedman’s reacted viscerally. He was already opposed to increases in gasoline or oil taxes as a response to the oil shock (see the previous chapter); now, he was incensed at seeing it combined with a version of the NIT. Friedman labeled the latter aspect of the Ford proposal as “a bastard version [of the NIT] that trivializes a good idea” (Newsweek, January 27, 1975).\(^\text{234}\)

Less than a year later, and several weeks after the Reagan challenge was launched, Ford took an action in the energy area that Friedman would later identify marking the point “when he lost me.”\(^\text{235}\) This was Ford’s signing of an energy bill that continued price controls on oil. In one of the regular commentaries he was giving for CBS Morning News during this period, Friedman lashed out at Ford’s “demonstrated capacity for waffling” and his ratification of a law that

---

\(^\text{232}\) _The Advocates_, PBS, November 1, 1973, p. 5 of transcript.

\(^\text{233}\) _A Day with President Reagan: NBC News Special_, NBC, February 13, 1981.

\(^\text{234}\) Friedman might have been expected to be pleased with the introduction later in 1975 of the federal government’s Earned Income Tax Credit (EITC), under which low-income households received top-ups of their labor income via the tax system. But he was not. Becker (2007, p. 185), Robert Barro (in _Business Week_, July 13, 1998), and Baumol and Blinder (1982, p. 659) all cited the EITC, which continues to this day, as an implementation of Friedman’s negative income tax (NIT) proposal. However, Friedman’s objection to measures like the EITC was that, in contrast to his own NIT proposal, they were introduced as a supplement to, rather than a replacement of, existing welfare programs. Friedman wanted the negative income tax to supplant the existing welfare system—not to be the basis for an additional element in that system. Consequently, after 1975 Friedman continued to regard his NIT proposal as never having been implemented (see Nelson, 2018b, Chapter 13).

departed so brazenly from free-market principles.\textsuperscript{236} Friedman underlined this criticism in a *Newsweek* column (January 19, 1976), titled “Rising Above Principle,” which concluded that “the president’s behavior widens still further the gap between his rhetoric and his actions... His actions are weak and vacillating.” Friedman would later judge that Ford’s signing of the energy-price control extension as a “disastrous economic decision.” The move, Friedman suggested, had hurt Ford both in the primary and national election because it conflicted with Ford’s previous statements and suggested an unwillingness on the president’s part to “stick to his principles” (*Saturday Evening Post*, May/June 1977, p. 20).

Friedman made various interventions during 1976 that were supportive of Reagan’s candidacy. One of these was an appearance in February 1976 at the third annual Conservative Political Action Conference, in Washington D.C. At the event, Friedman supported Reagan’s calls for federal spending reductions (*The Detroit News*, February 15, 1976). However, the specific item under discussion—a $90 billion cut in federal spending that Reagan had floated—had damaged Reagan’s campaign against Ford and contributed to that campaign’s near-collapse in February 1976 (see Cannon, 2003, pp. 409–411). Reagan’s campaign, however, regained momentum from late February onward, as he focused on tax cuts and, especially, foreign policy (Garthoff, 1994, p. 604; Cannon, 2003, pp. 420–424).

Friedman used his *Newsweek* column (March 1, 1976) to defend Reagan against the charge of hypocrisy with regard to public spending. Friedman granted that government spending in the state of California had more than doubled during Reagan’s tenure as governor. But this had been a smaller increase than that in several other U.S. major states, such as New York state, and the resulting contrast showed, Friedman believed, that Reagan had “successfully coped with tremendous pressure” for a larger public sector.\textsuperscript{237} This argument would prove useful again in Friedman’s retrospectives on Reagan’s national administration in the 1980s. For it would transpire that President Reagan’s tenure would see general restraint in the growth in—but not a lower absolute level of—federal spending and regulations.

During Reagan’s revitalized primary contest with President Ford, Friedman remarked that “I am very sympathetic” to the Reagan candidacy (*Chicago Tribune*, May 23, 1976): he explained that

\textsuperscript{236} *CBS Morning News*, January 9, 1976, pp. 23–24 of transcript.
\textsuperscript{237} The Governor of New York with whom Friedman contrasted Reagan was Nelson Rockefeller. Rockefeller had been the major competitor against Barry Goldwater for the 1964 Republican nomination. Ford and Congress had chosen Rockefeller to be vice president after Ford became president, but Ford announced in late 1975 that Rockefeller would not be his running mate in the 1976 election (and so would leave office in January 1977, even if Ford won the election).
this was because “he is a major believer in a smaller central government and in promoting free enterprise.” Friedman further contended that Reagan’s proposal to reduce the functions of the federal government aligned with Adam Smith’s vision of the public sector’s responsibilities. And when comparisons were made with the very unsuccessful Goldwater run, Friedman defended Reagan’s presidential bid. Friedman contended both that Reagan was a more appealing candidate than Goldwater had been and that U.S. voters in 1976 were more congenial to a small-government platform than they had been in 1964 (Christian Science Monitor, August 26, 1976).

By the time these last remarks had appeared, the Republican convention had taken place, and Reagan had narrowly lost the Republican party’s nomination to President Ford. It was Ford who would face the Democrats’ candidate, Jimmy Carter, at the November 1976 election.

The wrath of Ford

President Ford seemed to register his displeasure with Friedman’s decision to support Reagan’s challenge by apparently snubbing him on a couple of occasions during 1976. The first occasion was connected to an official visit to the United States by Australia’s Prime Minister, Malcolm Fraser, in July 1976. As of 1976, Fraser was associated both with an endorsement of the assignment of monetary policy to the task of inflation control and with a belief in reducing the role of government in the economy—though, over time, his words and actions created great distance from both these positions. In his early period as prime minister, Fraser was often described as having been influenced in his thinking by the writings of both Friedman and Ayn Rand. In 1970, when the United Kingdom’s Edward Heath, also associated at that time with free-market views, had visited the White House, Friedman had attended the state dinner for Heath, at President Nixon’s invitation. But when President Ford hosted Fraser for a state dinner on July 27, 1976, Rand was a guest, but Friedman was notably absent. As Friedman would later note, in the case of Rose Friedman and himself, the 1970 event for Prime Minister Heath proved to be “our first and last White House dinner.”

---

238 Friedman (1977h, p. 8).
239 See Friedman and Friedman (1998, pp. 390–391) and Nelson (2017) for further discussion of Friedman’s meetings with Heath.
241 Friedman and Friedman (1998, p. 390). Friedman went on to state (p. 630, endnote 15) that he had turned down some later invitations; he did not, of course, refer to dinners he would like to have attended but to which he received no invitation.
The guest list for the 1976 White House dinner for Prime Minister Fraser had two notable consequences from Friedman’s perspective. First, he never did meet Rand, who died in 1982 (Reason, June 1995, pp. 35–36). Second, he did not meet Fraser until a visit to Australia in April 1981—when his encounter with the prime minister led Friedman to judge him one of the rudest people he had ever met (Maurice Newman, interview, September 18, 2013).242

The next notable snub of Friedman on President Ford’s part occurred after Friedman won the Nobel award in economics on October 14, 1976. About two months had passed since Ford had seen off the Reagan challenge and received the Republican nomination, but “President Ford did not forget that [i.e., Friedman’s support for Reagan], and he never sent a congratulatory letter to Professor Friedman” when he received the Nobel, recalled Friedman’s secretary Gloria Valentine. Indeed, Friedman’s office heard “not a peep” from the president (Gloria Valentine, interview, December 5, 2013). In contrast, as discussed in the next chapter, former president Nixon phoned Valentine, and then Friedman, on the day of the award, while at the end of 1976 President-elect Carter rang Friedman with his own congratulations. As for now-former presidential candidate Ronald Reagan: On October 18, 1976, in recognition of the Nobel award announcement of the previous week, he made two recordings about Friedman’s views for his series of regular radio commentaries (Skinner, Anderson, and Anderson, 2001, p. 510).

_Ford versus Carter_

“Personally I prefer Mr. Ford to Mr. Carter, quite obviously,” Friedman observed in his final cassette commentary before the election (Instructional Dynamics Economics Cassette Tape 201, October 1976, Part 2). In a network television appearance around the same time, Friedman indicated that while he felt that Ford would—if he won a new term as president—likely only contain the size of the U.S. government rather than scale it back, he preferred Ford to Carter because the latter proposed to expand the public sector.243

A slowdown in monetary growth had occurred in late 1975—one that, as noted earlier in this chapter, Friedman attributed to the FOMC keeping up its federal funds rate target in the face of a weak credit market.244 Friedman warned after the first quarter of 1976 that, although U.S.

---

242 See also Friedman and Friedman (1998, pp. 431–432) on Friedman’s account of his meeting with Fraser.
243 _Meet the Press_, NBC, October 24, 1976, p. 3 of transcript.
244 Friedman also highlighted the slowdown in monetary growth in the second half of 1975 in his _Newsweek_ columns (December 8, 1975; June 14, 1976) and Instructional Dynamics Economics Cassette Tape 184 (January 1976, Part 2). The reduction is masked in the four-quarter growth rates reported in Table 1 above, but is evident in quarterly annualized rates. On this criterion, M2 growth using the old definition slowed down from 10.5 percent to 6.6 percent from 1975:Q3 to 1975:Q4; for the modern M2 definition, the corresponding slowdown was from 15.4 percent to 10 percent. The monthly data on modern M2 also indicate that the August-December 1975 period saw
monetary growth had recently picked up, the prior monetary slowdown could interrupt the nation’s economic expansion (*The Star* (Johannesburg, South Africa), April 5, 1976). Indeed, Friedman’s later verdict on 1976 was that “the [monetary] slowdown in late 1975 produced the economic pause in the second half of 1976 that played such a prominent role in the Ford-Carter election battle” (*Newsweek*, October 3, 1977). Some thirty years after the 1976 presidential election, Ford’s former press secretary expressed the judgment that a slight rise in the U.S. unemployment rate before the election—a rise associated with the economic pause—was a decisive factor in President Ford’s narrow loss to Jimmy Carter in the November 4 national vote (*USA Today*, December 27, 2006).

In the aftermath of the presidential election, *Time* magazine (November 15, 1976) noted that Ronald Reagan “will be 69 in 1980—which may be too old to try again.” *Newsweek*’s analysis (November 15, 1976b) of Reagan’s prospects was even blunter: “Only the truest of believers believe Reagan could be the GOP nominee in 1980—when he will be 69.” In his own post-election analysis, Friedman seemed to share *Newsweek*’s negative assessment, speaking as though Reagan was finished as a political candidate (Instructional Dynamics Economics Cassette Tape 202, November 1976, Part 1).

However, far from being politically finished as of November 1976, Reagan would dominate the 1980s. Indeed, a full decade on from November 1976, on November 21, 1986, President Ronald Reagan would be receiving Friedman in the White House, for what would turn out to be the last of the meetings of Reagan’s Presidential Economic Policy Advisory Board.

---

distinctly slower monetary growth than the rates recorded in the immediately prior and later periods. Friedman attributed the weakness in the credit market, which in turn triggered an unintended move to monetary restriction, to a glut of supplies of funds to U.S. short-term securities markets, *vis a vis* longer-term markets, in the wake of the New York City financial crisis (see the discussion earlier in this chapter as well as his testimony of November 6, 1975, in Committee on Banking, Housing and Urban Affairs, U.S. Senate, 1975a, pp. 67–68, and his testimony of January 22, 1976, in Committee on Banking, Currency and Housing, U.S. House of Representatives, pp. 2182–2183).

245 The unemployment rate in the third quarter of 1976 (7.7 percent) was fractionally higher on average than in the previous quarter (7.6 percent).

246 This view was widely shared at the time, and retrospective statements to the contrary should be treated with suspicion. For example, Cannon’s (2003, p. 436) declaration that Reagan came out of the 1976 Republican convention as “presumptive front-runner” for the 1980 nomination was at variance with Cannon’s (2000, p. 253) own statement that Reagan’s 1976 convention speech was widely seen at the time as the finale to his political career. It also overlooked the fact that, in light of the closeness of the 1976 presidential election result, the prospect of a 1980 Ford-Carter rematch was taken very seriously in the late 1970s. Gerald Ford kept a high profile well into 1979 finding fault with the Carter Administration (see, for example, *Marshall Evening Chronicle* (Michigan), August 10, 1979), and Carter would later observe (*The View*, ABC, September 20, 2010) that Ford “spent a lot of the time when I was president criticizing me.”
I. EVENTS AND ACTIVITIES IN DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1977–1978

A preamble to a March 1977 newspaper interview with Milton Friedman described him as “now on leave” from the University of Chicago (U.S. News and World Report, March 7, 1977). And in the March/April 1977 issue of Society—a public-affairs periodical published by Friedman’s undergraduate alma mater, Rutgers University—he described himself as a citizen and taxpayer of Chicago.\(^2\)

These characterizations were accurate, but they did not tell the whole story. When completing his tax returns around the time the Society article appeared, Friedman would certainly have fallen into the category of a citizen of Chicago, with his local and state tax liabilities reflecting his residence in that city during calendar 1976. And, in early 1977, he was, indeed, on leave from the Department of Economics of the University of Chicago. But it was also true that he was not coming back to the department. In fact, the situation in March 1977 was that Friedman had already left both the University of Chicago and the city of Chicago for good.

Milton and Rose Friedman relocated permanently to San Francisco at the very start of 1977—in so doing, missing one of the city of Chicago’s worst winter months in recent history. They arrived in their new home city on Monday, January 4, and the following day Friedman started his three-month spell as a visiting scholar at the Federal Reserve Bank of San Francisco (San Francisco Examiner, January 5, 1977).\(^3\)

---

1 Email: Edward.Nelson@frb.gov. The views expressed here are the author’s alone and should not be interpreted as those of the Federal Reserve or the Board of Governors. The author is grateful to the interview subjects for their generosity in providing useful information, as well as for comments from participants in a seminar (November 2013) at the University of California, Berkeley, at which some of the material in this chapter was presented. See the Introduction in Nelson (2018a) for a full list of acknowledgments. The author regrets to note that, in the period since the research for this chapter was carried out, Kenneth Arrow, David Backus, Gary Becker, Charles Brunie, Allan Meltzer, and Paul Volcker, whose interviews with the author are drawn upon below, have passed away.
2 Friedman (1977g, p. 89). In addition, in Newsweek (March 21, 1977), Friedman described Senator Adlai Stevenson (D—IL) as one of his state’s senators.
3 The press coverage in connection with the formal announcement of Friedman’s retirement from the University of Chicago indicated that his visiting position at the Federal Reserve Bank of San Francisco would last about six
**Visiting the Federal Reserve Bank of San Francisco**

Friedman’s spell as a visiting scholar at the Federal Reserve Bank of San Francisco bridged much of the gap between his thirty years at the University of Chicago and what, to his surprise, would prove to be an almost-30-year stretch at the Hoover Institution, starting in fall 1977. In a public statement around the time of Friedman’s visitor appointment, the San Francisco bank’s president, John J. Balles, observed: “Milton Friedman has altered the course of economic thinking and continues to contribute [to] and influence our fundamental judgments concerning the theory and practical applications of economics.” (*American Banker*, January 18, 1977.)

Friedman’s visit had arisen from an invitation that Michael Keran extended after Keran became research director of San Francisco’s reserve bank in 1973. When seeing Friedman in the city of Chicago in the mid-1970s, Keran recalled, “I said, ‘We have a visiting-scholar program at the San Francisco Fed. Would you be interested in participating?’ And, to my surprise, he said, ‘Yes.’” (Michael Keran, interview, March 7, 2013.)

Keran had, from a distance, been a student of Friedman’s during the 1960s, having been guided into his dissertation research topic (the study of money and the business cycle in Japan) by Friedman when they met in Japan in 1963, and having then completed his doctoral dissertation at the University of Minnesota, with long-distance feedback from Friedman.4 Prior to being hired as the Federal Reserve Bank of San Francisco’s research director, Keran had been at the St. Louis bank’s research department during the period in which the latter institution’s monetarist research took off. Keran was hired to raise the San Francisco’s research department’s profile; but he also had in mind making its research sensibility more monetarist. “When I left St. Louis, I said, “I’m hoping to go to convert St. Francis to the views of St. Louis.”” (Michael Keran, interview, March 7, 2013.) Already by 1975, Friedman had applauded the stamp that Keran had put on San Francisco’s reserve bank by increasing the monetary-research content of its monthly review (Instructional Dynamics Economics Cassette Tape 175, September 1975, Part 1). Having

---

4 See Friedman and Friedman (1998, pp. 325–326). The dissertation, completed at the University of Minnesota in 1966, was in effect confirmed as work in the money-workshop tradition via its inclusion, in revised form (see Keran, 1970), in a University of Chicago Press book collection, alongside several Friedman-supervised University of Chicago Ph.D. dissertations (see Meiselman, 1970).
Friedman work temporarily among the bank’s economists dramatically underlined the bank’s shift of interest toward monetarism.

According to Keran, the rapport Friedman developed with the Federal Reserve Bank of San Francisco’s research-department staff was “incredibly good. I’d take him around when he first came, to introduce him to the staff and the research assistants. And [over time] he extracted more information about them, especially the research assistants, than I knew. He was very good at making people feel at ease, asking them a lot of questions about themselves, and generally making himself very agreeable. That was a skill I envied.” (Michael Keran, interview, March 7, 2013.)

Keran also recalled Friedman being “inundated” with mail during his stay, in the wake of the publicity of the Nobel prize and ceremony just before Friedman’s move to San Francisco. Though he had not yet formally moved to Stanford University’s Hoover Institution, that was already Friedman’s official postal address for business correspondence. His secretary Gloria Valentine, who had started employment at the institution, would bring Friedman’s voluminous mail to downtown San Francisco for his inspection.

Research activities

The research project that primarily occupied Friedman during his time as a visiting scholar was an investigation of the properties of the monetary base. Although he had a longstanding preference for an M2-type definition of money, Friedman saw attractions in the monetary base as an alternative monetary concept. Most obviously, monetary targeting that was focused on the base could sidestep the monetary-control issues raised by targeting an aggregate that included bank deposits.

A further possible reason for preferring the base was empirical. In their Monetary Statistics, Friedman and Schwartz had noted from their impressions of other countries’ experiences that some of the most severe recorded velocity problems had arisen from the behavior of bank deposits in the wake of financial innovation (including the extension of interest payments on deposits, as well as the creation of deposit substitutes) and changes in banking and monetary regulations. They suggested the stock of currency as an aggregate that could be used for monetary analysis in such circumstances, as currency series tended to be insulated from those
institutional changes that bore specifically on the behavior of deposits. In follow-up work during the early years of the 1970s, Friedman’s graduate student James Lothian had offered the monetary base as another aggregate that could be more robust to financial innovation than was the case for monetary totals that consisted of the sum of currency and commercial bank deposits (Lothian, 1973, 1976).

Militating against these arguments was experience that suggested that M1- or M2-type aggregates might be more reliable than currency or the base in terms of their relationship to aggregate economic activity. This experience included the Great Contraction of 1929–1933. The rise in both the currency/deposit ratio and the reserve/deposit ratio (for the M1 and M2 definitions of money alike) during the banking panics of those years had made currency and the base misleading indicators for a time. Furthermore, as the 1976 Bach Report had noted, the payment of interest on reserves, if enacted in the future, would amplify the difference between currency and bank reserves and render it more difficult to predict nominal income on the basis of the behavior of their sum. More routinely, reserve-requirement changes could make the base a misleading indicator of monetary conditions if the base series was not adjusted for those changes.

It was this last consideration that Friedman investigated during his early 1977 researches. As he and Anna Schwartz later recalled (Wall Street Journal, December 20, 1993), their Monetary History “attributed the depth of the 1929–33 depression and the occurrence of the later 1937–38 recession to Fed mismanagement.” The 1937–1938 recession, being preceded by the Federal Reserve Board’s 1936–1937 moves to raise reserve requirements, had highlighted for them the importance in practice of reserve-requirement changes as a factor bearing on the quantity of money.

For the postwar period, the same consideration applied in a different way. The Federal Reserve had, as Friedman observed, followed an interest-rate stabilization policy that meant that reserve-requirement changes were generally not allowed to have a powerful, 1930s-style, effect on the money stock. Rather, the Federal Reserve routinely offset its reserve-requirement changes with open market purchases or sales. The fact that it did so, however, only underlined the differences between the base and the adjusted base (that is, the monetary base adjusted for changes for

---

6 Friedman had referred to Lothian’s work-in-progress in his consultant’s memorandum to the Federal Reserve Board of June 9, 1971 (Friedman, 1971c, p. 10).
7 See Nelson (2018a, Chapters 2 and 8) for further discussion.
reserve requirements). The Federal Reserve Bank of St. Louis had, since the 1960s, provided a series on the adjusted base. In his 1977 investigations, Friedman compared the predictive performance, for variations in nominal income, of changes in the unadjusted monetary base with those of changes in the adjusted base.\(^8\) He found that the adjusted monetary base performed far better. Friedman took this finding as suggesting that, in the case of the United States, one could not bypass the money-measurement problem simply by relying on high-powered money. Leaving aside the option, aired by Friedman and Schwartz, of treating currency as the monetary aggregate of interest, this finding implied that either the adjusted base or a currency-plus-deposits aggregate was needed as the measure of money.

Completion of this research project became a casualty of Friedman’s busy schedule and waning research ambitions. A research article never came out of the project. Despite the enthusiasm of a coauthor at the Federal Reserve Bank of San Francisco, who worked on a draft of a paper that would lay out the project’s findings, the writing-up of the project was never finished. The only major glimpse of the project that Friedman subsequently provided was his recollection of the project’s results, which he gave in passing in the course of a floor discussion during an NBER Festschrift for Anna Schwartz, held in New York City in October 1987 (see Bordo, 1989, pp. 76–77).

The year 1977 saw a number of research publications by Friedman, including multiple issuances of his Nobel lecture (discussed in the next chapter) and a written record of his January 1977 debate with Franco Modigliani, published as a special issue of the *Federal Reserve Bank of San Francisco Review* (see Section III below). But, after 1977, it became clear that he had put his days of prolific research output behind him. The year 1978 became the first year since 1945 in which no article by Friedman appeared in an economic-research journal.\(^9\)

Gary Becker firmly associated the change in Friedman’s research output to his relocation. “Well, he didn’t do much research after he left Chicago. If he had stayed at Chicago, he would have continued to do research. I mean, you can’t stay at Chicago without doing research; otherwise, you’d better leave. The reasons he left were for Rose, and so on, who didn’t like Chicago and wanted to live in San Francisco. And yes, he basically stopped serious research at that point; he may have finished up some stuff, [but] he basically stopped doing serious

\(^8\) In Instructional Dynamics Economics Cassette Tape 206 (January 1977), the first cassette commentary recorded from San Francisco, Friedman referred to this exercise as a current research project.

research.” (Gary Becker, interview, December 13, 2013.)

By moving physically west to California, Friedman had left the metaphorical Wild West that was the University of Chicago economics world. At the Hoover Institution, he did on rare occasions hijack a seminar that he was attending (see the next chapter for an example). But Friedman’s routine of hosting and attending freewheeling research workshops that could be disruptive for presenters, and a gladiatorial experience for attendees, was now behind him. So too were occasions such as that in 1962 when, as an audience member, he had turned a lecture at the University of Chicago by Harold Wilson into an unscheduled debate between himself and the future U.K. prime minister. *Fortune* magazine (March 19, 1984, p. 21) would note, “Friedman says he still misses the intellectual climate—but not the weather—at the University of Chicago.”

Thomas Sargent contrasted Friedman’s new permanent location of the Hoover Institution (which is discussed in more detail below) with competitive economics departments like the University of Chicago’s. “Hoover is much more decentralized and, you know, relaxed. And it’s not an intense research place. Some people like that and thrive on it, and some don’t.” (Thomas Sargent, interview, January 24, 2014.)

In some respects, Friedman did indeed thrive in this environment. As subsequent chapters will detail, his Hoover Institution years, particularly through about 1992, saw Friedman engage in prodigious writing and continuing activity in public policy. On these dimensions, Friedman remained a vibrant force past age 65, which he reached on July 31, 1977. But he did not have the engagement in research that would later characterize leading economists like Becker or James Heckman, who would, unlike Friedman, stay at the University of Chicago when they themselves reached their mid-sixties.

Influence in universities

Although Friedman was drifting away from participation in research, and was becoming a much less common sight at economic-research conferences, his impact on the profession was far from over. To be sure, his influence was not felt primarily through self-identified monetarist economists. There never really was a broad-based Friedman school in academia, located across major universities and represented by senior figures at those universities, of the kind there was for Keynesian economics. Instead, during his University of Chicago years, Friedman’s

---

10 For other reasons for Friedman’s move away from research, see the discussion in Chapters 8 to 12 below.
following had a ragtag nature to it, with many of the senior economists sympathetic to monetarism not to be found at major universities but at smaller ones, as well as at Federal Reserve Banks and private-sector financial institutions.

And in 1977, although Friedman had just finished over two decades of overseeing graduate students working in the monetary field, it was rare to find such students of Friedman in prominent places at the “top twenty” U.S. universities. Certainly, there were a few cases. Notably, Phillip Cagan was an economics professor at Columbia University. But Cagan did not make an impact with his research of the 1970s and 1980s comparable with that he had generated with his research of the 1950s and 1960s. And Cagan tended, in any event, to take a much more softly-softly approach to argumentation than Friedman did. “He likes to be provocative,” Cagan observed to the present author with regard to Friedman (Phillip Cagan, interview, January 13, 1992), making clear—by the way in which he said this—that that was not Cagan’s own approach. Two Friedman students who had graduated in the early 1970s, Michael Darby and Benjamin Klein, were by late in the decade both situated at the University of California, Los Angeles—“one of the best regarded state universities in the country,” the Friedmans would have occasion to note—and would become senior figures at the university, as well as among the most-cited researchers among his former doctoral students. Darby, already well established in microeconomics, would remain prominent in monetary economics through the late 1980s, but Klein was moving away permanently from the monetary field by 1978.

In the late 1970s and the early 1980s, Friedman’s monetary work made itself known to the next generation of economists. But it did not do so primarily via the promulgation of that research by his former doctoral students. Instead, others whom he had not taught or supervised would fulfill this function, through an activity from which he had now withdrawn: the teaching of graduate students in economics at U.S. universities. The positions Friedman took did end up becoming the subject of discussion for the next generation of economists, as his views on monetary matters would have a prominent place in graduate courses around the country. Furthermore, much of his work was presented favorably.

A particularly celebrated example of Friedman’s success in influencing the thinking of the next generation took shape in the mid- to late 1970s at the Massachusetts Institute of Technology. Friedman’s longtime sparring partners at that institution—Modigliani, Paul Samuelson, and

---

11 The quotation is from Friedman and Friedman (1980, p. 176).
12 The interaction with, and posture toward, Friedman in Harvard University economics circles is considered in the next chapter.
Robert Solow—remained vibrant presences. Indeed, by 1977, all three of them were maintaining a higher level of activity in the research sphere than Friedman. But, in large part, they had handed over teaching responsibilities for the core MIT graduate economics programs to a younger cohort in their department. In particular, Stanley Fischer, Friedman’s 1969–1973 University of Chicago colleague, taught MIT’s graduate monetary-economics course.

In 1969, Friedman had impertinently offered a generalization: What was received wisdom about monetary policy at the University of Chicago during one year became received wisdom at Cambridge, USA—but with a lag of five years (Instructional Dynamics Economics Cassette Tape 17, March 1969). Via Fischer and other new MIT professors like Rudiger Dornbusch, that rule of thumb became even more true from 1973 onward, as they turned prominent elements of Friedman’s research output in the 1960s and through 1972 into part of the MIT economics mainstream. Through Dornbusch and Fischer, Friedman’s positions—on the importance of the real/nominal interest rate distinction, the centrality of monetary policy for the behavior of aggregate demand and inflation, and cautions about overstating the power of fiscal policy—permeated MIT teaching and research.

Most strikingly, the natural rate hypothesis, which had challenged the Samuelson-Solow position on the inflation/unemployment relationship and which had given rise to Solow’s (1969) counterattack against Friedman, was now treated as a valid critique of the original Phillips curve. Fischer (1979, p. 172) referred to the task of “trying to improve macroeconometric models… [to take] account of misspecifications, such as those pointed out by Friedman (1968[a]) and Phelps (1967) when they discovered the role of expectations in the Phillips curve.” And Dornbusch (1980, p. 162) referred to “the vantage point of modern Phelps-Friedman macroeconomics.” In the face of perspectives such as these from his newer colleagues, Solow (1982, p. 24) would acknowledge that “most macroeconomists” now accepted the Friedman-Phelps position on the Phillips curve, although he went on to confirm his own continued resistance to it.

Also, as Dornbusch and Fischer saw things, “a major stage in the acceptance of monetarism occurred when Friedman and Schwartz published their Monetary History of the United States.”

“One of the big changes” on the teaching side at MIT, recalled Frederic Mishkin (interview, June 18, 2013), “was that Stan Fischer came when I was entering graduate school, and Stan was a huge fan of Milton Friedman’s. [Though] that didn’t mean he agreed with all of his policy

---

13 Dornbusch had seemed receptive to a permanently-sloping Phillips curve as recently as Dornbusch (1973, p. 894).
prescriptions. So, to give you an example: When I was taking a course, my first course from Stan, he said that if you’re serious about being a monetary economist, you have to read Friedman and Schwartz by your bedside every night: you have to have it [the Monetary History] at your bedside, and read it every night. And, in fact, I did. So, I think, with the advent of Stan coming on the faculty, that the appreciation of Friedman’s work went up just tremendously… And, in fact, there was a big change in the kind of economics that was taught at MIT because of Stan’s coming. It moved away from the strict Keynesian, Modigliani-Solow-type discussion of macroeconomics to ones where rational expectations was brought into the classroom [and] the emphasis was on long-run issues; and Monetary History was viewed as one of the classic works that had ever been written in monetary economics, and so forth. So there was really a sea change.”

A somewhat later MIT graduate student, Christina Romer recalled at a memorial event for Anna Schwartz in April 2013: “I remember vividly one meeting of our second-year monetary economics graduate class at MIT. The professor, Stanley Fischer, started the class by asking, “How do we know money matters?”… Finally, Stan shut down the discussion [and said]: ‘Because Friedman and Schwartz showed us.’”15

“What happened is Stan, who was at Chicago maybe a little longer than I was, then went back to MIT, where he had gotten his Ph.D.,” recalled Richard Zecher (interview, September 3, 2013).16 “… He took a lot of the things he learned in Chicago, and he went back to MIT, and he sort of ‘MIT’d’ them.” By “MIT’ing” the concepts, Zecher explained, he meant that Fischer “gave them a different slant; but they were some of the same basic ideas. And then he trained all these guys—all these guys that are running central banks now; all trained by Stan… Maybe we could say Milton’s had a tremendous impact on the heads of central banks, through Stan Fischer at MIT.” (Richard Zecher, interview, September 3, 2013.)17

15 C.D. Romer (2013, p. 3). See also Ball and Mankiw (1994, p. 128) for a related recollection.
16 Zecher in fact had a University of Chicago affiliation for a longer span of years (1968 to 1973) than did Fischer. But Zecher spent part of that time away from the university, taking a visiting position in Australia.
17 Since Zecher wrote these words, some of the individuals who were MIT graduate students over the years that overlapped with Fischer’s time at MIT have stepped down or retired. For example, Ben Bernanke left the Federal Reserve Chair position in 2014, Charles Bean ended his period as Deputy Governor of the Bank of England in 2014, Mario Draghi’s tenure as ECB president ended in 2019, and David Wilcox retired as chief economist for the Federal Open Market Committee in 2019. However, the same span of years saw Philip Lowe move up from Deputy Governor to Governor of the Reserve Bank of Australia (in 2016)—a move that also saw Guy Debelle appointed Deputy Governor after having been an assistant governor—and Stacey Tevlin become chief economist for the Federal Open Market Committee (in 2019). These individuals had also been MIT graduate students during the Fischer era.
Fischer’s own characterization of matters was that his period at the University of Chicago “enabled me to combine MIT’s analytics with the policy relevance that Milton Friedman typified” (quoted in Blanchard, 2005, p. 249). In the decade to 1978—which for Fischer comprised roughly six years at MIT and four at the University of Chicago—he had had ample reason to become extremely familiar with Friedman’s other positions on monetary matters: that is, those extending beyond the Phillips-curve work and the Friedman-Schwartz historical research. Fischer (2017b, p. 2) would describe his earliest publications in monetary economics as “research [that] was carried out when monetarism was gaining credibility in the profession.”

In this early period of Fischer’s career, Friedman—who would later state that “Stanley Fisher is a good friend of mine, and I have a great deal of respect for his abilities” (Moneyline, CNN, January 23, 1998)—would be a major source of feedback on Fischer’s work on policy rules with Phillip Cooper, during the period when both Cooper and Fischer were located at the University of Chicago (see Nelson, 2018a, Chapter 8). Fischer sat in on Friedman’s graduate money course (Blanchard, 2005, p. 249), and both Cooper and Fischer were regular attendees of Friedman’s money workshop. Upon moving to MIT, Fischer would develop MIT’s own money workshop (Blanchard, 2005, p. 250).

Finally, in his research using rational expectations models, Fischer produced results that were a closer heir to the Friedman tradition than the Lucas-Sargent-Wallace-Barro body of work could claim to be. This was because Fischer saw systematic monetary policy, and not just one-period monetary policy shocks, as mattering for output fluctuations (Fischer, 1977a; 1980, p. 213).

In light of the posture toward monetary issues that Fischer revealed in his writings, McCallum (1998, p. 309) would ask if there was anything that “differentiates Stanley Fischer from monetarists?” The label of “monetarist” was, however, was one Fischer rejected as a description of himself. And, indeed, Fischer did not agree with Friedman on many details of monetary analysis, while also parting company with Friedman on the desirability of the constant-monetary-growth rule. Furthermore, Fischer believed, with qualifications, in the validity of fiscal-multiplier analysis, while Friedman did not.18 Rudiger Dornbusch was likewise in favor of much

---

18 Fischer also parted company with Friedman on the lineage that Friedman (1956) saw between his restatement of the quantity theory of money and earlier University of Chicago research on money. As a graduate student at MIT, Fischer was research assistant for Don Patinkin’s work that critiqued Friedman’s contention, and in the resulting paper Fischer was thanked in the opening footnote, for reading older University of Chicago Ph.D. dissertations on money (see Patinkin, 1969, p. 46). Fischer recalled that Patinkin, in assigning him that task, “really made me work for that footnote. It was a pleasure, incidentally; and there just wasn’t a whole lot [in the theses] that you could possibly describe as what Friedman said was a Chicago tradition. Now, it wasn’t clear to me why it was so important for him to have the Chicago tradition. You know, otherwise, he could have said, ‘I invented something,’ which he may well have done.” (Stanley Fischer, interview, August 30, 2013.)
more active and, in practice, stimulative demand policies in the 1970s than the policies espoused by Friedman (see Section II below).\textsuperscript{19} It remains the case, however, that, in forming their positions on macroeconomics, both Dornbusch and Fischer had assimilated many of Friedman’s ideas on the effects of monetary policy on the economy.

\textit{Influence on textbooks}

A shift similar to that observed at MIT was also occurring elsewhere in the later 1970s. It was felt not only in the content of graduate teaching, but also in undergraduate textbooks. One example of the latter was an intermediate undergraduate textbook on monetary economics published in 1977 titled \textit{Money, the Price Level and Interest Rates: An Introduction to Monetary Theory}, by Gail E. Makinen, an associate professor at Wayne State University. The text’s Chapter Nine was titled “The Monetary Theory of Milton Friedman.” Makinen (1977, p. 232) began the chapter, “That a chapter in a textbook is devoted exclusively to an examination and explanation of the monetary theory identified with a single individual stands as a measure of his influence and importance.”

However—paralleling the case of graduate teaching, discussed above—MIT provided the most notable example of undergraduate textbooks that felt the stamp of Friedman. The first edition of Dornbusch and Fischer’s intermediate undergraduate textbook \textit{Macroeconomics} appeared in 1978. The macroeconomic modeling in the latter part of the book, particularly Part III, was developed partly from Fischer’s MIT graduate-course lecture notes, rather than from his undergraduate class (Richard Anderson, interview, November 14, 2013). The many respects in which the book as a whole reflected the influence of, and underlined the importance of, Friedman’s macroeconomics have been discussed in Nelson and Schwartz (2008, p. 848) and Nelson (2018a, Chapter 1).\textsuperscript{20} As the authors themselves put it in their text (Dornbusch and

\begin{footnotesize}
\textsuperscript{19} Miles (1979, p. 601) described Dornbusch as an international monetarist, and Fischer (1977b, p. 60) even referred to the “simplest monetarist model, due to Dornbusch (1973)…” However, these descriptions were written in an era when contributions to the literature on the monetary approach to the balance of payments were routinely described as monetarist. These contributions gave an important role to the money demand function in the analysis and made the price level endogenous. They were therefore closer to Friedman’s monetarism than to the Keynesian open-economy models of previous years. But they tended to consider issues different from those that were the focus of most of Friedman’s monetarist research, and they reached conclusions that often did not coincide with Friedman’s own positions on open-economy matters.

\textsuperscript{20} As detailed in Nelson (2018b, Chapter 15), the authors were critical of Friedman’s discussion of lags in the effects of monetary policy, and they implied that he had not been consistent on the matter. Similarly, in an interview for this book, Fischer suggested that in Friedman’s popular writings “there was always some damn lag that allowed you to say that everything—inflation, or growth, or whatever—was caused by money, but [one was] never sure that they were the same lags each time.” (Stanley Fischer, interview, August 30, 2013.) It is suggested in Nelson (2018b, Chapter 15), however, that the Dornbusch-Fischer (1978, p. 526) criticism of Friedman along this lines may have
\end{footnotesize}
Fischer, 1978, p. 520): “Much of the analysis of this book would, a few years ago, have been considered monetarist.” In this respect and others, the new textbook’s analysis veered sharply from prior texts: Fischer recalled that, “certainly, what was in our book was new when it came out” (Stanley Fischer, interview, August 30, 2013).

The textbook was replete with mentions of Friedman. In discussing why this was the case, Fischer observed: “Milton was very active in a lot of debate on policy, and particularly pushing the money-supply view. And it was in the newspapers all the time. It was very prominent. Milton was one of the most prominent public economists. [Here] I don’t mean public-economics economists; I mean, people whom the public knew about.” In contrast, with regard to Paul Samuelson, Fischer observed, “Paul was not particularly interested in macroeconomics, as opposed to all the other things he was interested in. Paul Samuelson could have gotten the Nobel Prize in five or six areas; and Milton couldn’t have.” Fischer suggested that, instead, the question to ask with regard to the textbook was: “Why was there more Friedman than Modigliani?” The reason for this, Fischer said, was that “Modigliani was never as prominent in the public’s view. I think Modigliani in many ways was as important an economist as Milton. But not for public policy.” (Stanley Fischer, interview, August 30, 2013.)

Another undergraduate macroeconomics textbook, released the same year as Dornbusch and Fischer’s and in practice the major competitor against it, was by another former Friedman colleague, Robert Gordon. Gordon (1978), also titled Macroeconomics, incorporated into its analysis the modifications to the standard macroeconomic model that were associated with Friedman: the permanent income hypothesis, the natural rate hypothesis, the real/nominal rate distinction, and heavy qualifications about the effectiveness of fiscal policy. Although Gordon was not a monetarist—and, indeed, would become extremely vehement during the 1980s in his public criticisms of particular monetarist ideas and recommendations—he consulted Friedman by correspondence on his 1978 edition’s coverage of monetarism; and, as has been noted in Chapter 4, Friedman provided a public endorsement of that edition.

James Tobin’s ongoing resistance

At Yale University, in contrast, James Tobin was a continuing source of resistance to the notion overlooked the fact that in the early 1970s he explicitly changed his position on the amount of time it took for monetary policy to affect inflation, and that he subsequently stuck to this new, longer estimate.

In any event, the textbook’s coverage of inflation became more monetarist over time, with the fourth edition stating (Dornbusch and Fischer, 1987, p. 638): “The answer to the question [of] whether inflation is a monetary phenomenon in the long run is yes.”
that Friedman’s analysis should be incorporated into mainstream teaching and modeling. A recollection by Gregory Mankiw concerning Matthew Shapiro underlines this fact. At graduate school in the early 1980s, Mankiw recalled, “Matthew was a co-student with me at MIT. [As an undergraduate,] Matthew had gone to Yale, where he had worked under Tobin. He was Tobin’s research assistant, I believe. And then he came to MIT, where he used to take courses with me, and we took courses from Stan Fischer. And I remember him saying this, years ago: One of the things that really struck him was a difference in attitude toward Milton Friedman between MIT and Yale at the time. When he was at Yale, he kind of grew up thinking that this guy Milton Friedman was some sort of crank at the University of Chicago; and then he went to MIT, and he found people speak about Friedman with a sort of reverence.” Thus, in classes that Shapiro took, the manner in which Friedman was characterized underwent a transformation: over “a period of a year, this guy had gone from being a crank to being one of the most revered members of the profession.” (Gregory Mankiw, interview, September 24, 2013.)

Shapiro himself recalled that he was “trained as an undergrad in the Keynesian-monetarist controversies… Milton Friedman was treated adversarially, as having quite a different view from the James Tobin Yale Keynesian perspective.” (Matthew Shapiro, interview, November 14, 2013). Shapiro was an undergraduate during the time when Tobin’s remarks at a 1975 conference appeared in print—remarks in which he declared (Tobin, 1977b, pp. 763–764) that “monetarism is a religion, whose widespread following is a significant economic fact in its own right, whether or not its doctrines are true.”

Though he wrote generously about Friedman when the latter received his Nobel prize (The Economist, October 23, 1976), Tobin soon resumed hostilities. Barry Eichengreen, a graduate student at Yale University in the 1974–1979 period, remarked of Tobin’s graduate monetary economics course: “As late as 1977/78, which must have been the second time I took the course… Friedman was in ‘the empty chair,’ metaphorically.” That is, Friedman was an absent figure, against whom a good deal of the teaching was directed.21 Eichengreen recalled that Tobin was “teaching a lot about points like ‘Post hoc ergo propter hoc?’—that came from 1970, so it was relatively recent [Tobin, 1970b]. He was concerned to show that there was no mechanical relationship between money and price or money and output; that there was of financial assets of which money was [only] one. And he presented Friedman as kind of the counterpoint to that. So what people were learning in other places in the second half of the 1970s, like rational

21 Eichengreen’s parallel here was with Clint Eastwood’s use of an empty chair as though it was occupied by President Barack Obama when Eastwood laid out a critique of President Obama in a speech at the 2012 Republican party convention.
expectations, came up in passing in the course—but was much less important than these debates about does money matter; how much does money matter; how much do we have to simplify the financial sector in order to figure out how much money matters.” (Barry Eichengreen, interview, April 3, 2014.)

Another graduate student of the period, David Backus, concurred: “Friedman, I think that’s true—he was the guy in the empty chair.” (David Backus, interview, April 16, 2014.) However, Backus stressed that he found that Tobin was reticent when it came to comparing his views directly with those of Friedman: “I don’t recall getting much insight into Friedman—or Lucas, Sargent, et al. Looking back, it was a pretty isolated world. We saw work by these guys, but didn't know what to make of it.” (Davis Backus, personal communication, April 6, 2014.)

Backus indicated that, rather than from courses or from reading Tobin’s written exchanges with Friedman, “most of what I got out of Tobin was just talking to him and Brainard; they were extremely generous with their time.” Tobin had a research program that he was following,” and “I would say what I lacked as a Ph.D. student was any sense of what this other stuff was… We [students] were just missing that line of thought. We just didn’t understand what it was about.” (David Backus, interview, April 16, 2014.)

Tobin’s reticence on this score was evident in a discussion by Tobin and de Macedo (1983, p. 7) that took issue with the idea that “the commodity [goods] price level is an asset price, in the sense that its reciprocal is the real value of money.” This was clearly a follow-up to the exchange that Friedman and Tobin had had in the November 1974 conference on monetarism. At that event, Friedman had contrasted his own position that the price of money was the inverse of the price level, with the “Keynesian or central banker” stand that the price of money was the interest rate. This had provoked a reply from Tobin that Friedman’s contrast reflected a superficial attitude to capital theory.22 But the Tobin-de Maceo paper did not reference the earlier exchange or, indeed, mention Friedman at all.

Another echo of that Friedman/Tobin exchange on the interest rate occurred in Fischer’s MIT graduate class. Shapiro recalled: “Stan Fischer was teaching; I guess it was the second year monetary course that Greg and I were sitting on as first-year students. And I can’t remember exactly how Stan framed the question, but he basically asked, “What’s the price of money?” And somehow, he made clear that the answers were either 1/P or the nominal interest rate. And he took a poll, and I was sitting in the front row, so I didn’t see what everyone else was saying.

22 See Friedman (1976a, p. 316) and Tobin (1976, p. 335).
So, apparently everyone in the class raised their hand for $1/P$ except me, (laughter) who, being a student of James Tobin, said $R$. Which kind of sums it [the Friedman/Tobin divide] up on theoretical points.” (Matthew Shapiro, interview, November 14, 2013.)

James Tobin’s tendency to avoid mentioning Friedman by name when articulating his critique of monetarism would again display itself in an article that appeared in the *Southern Economic Journal* in early 1978. The article was based on an address that Tobin had given in Atlanta on October 18, 1976 (a few days after Friedman’s Nobel award had been announced). Tobin (1978a, p. 421) opened this address by indicating he would be concerned with the transmission mechanism—“the process by which monetary policies are transmitted into changes in expenditures for Gross National Product.” In the subsequent exposition, he proclaimed that “[n]aïve calculations of Fisherian real rates of interest are very unreliable indicators of financial incentives for real investment” (Tobin, 1978a, p. 426). This was a theme that had been a basis for Tobin’s criticisms of Friedman going back to 1966 (see Nelson, 2018b, Chapters 12 and 13). The quantitative implications of this criticism had been amplified in the 1970s—a period in which real interest rates on securities were persistently negative, while measures of $q$ from the stock market suggested instead an extremely high real cost for U.S. businesses in raising funds for capital spending. So Tobin was concerned with making the argument that the economically-crucial real interest rates were high, during a time when Friedman was contending, in contrast, that the cost of borrowing was low in real terms. Yet Tobin’s 1978 discussion of this matter made no mention of Friedman.

In the same address, Tobin made a different but also anti-monetarist point that “inside money is... more powerful stuff than outside money” with regard to the ramifications of monetary developments for aggregate demand (Tobin, 1978a, p. 434). Tobin was therefore maintaining a position he had taken in his review (Tobin, 1965) of the *Monetary History*. On that occasion, Tobin had argued that loan creation by commercial banks was more significant for aggregate demand than a commercial-bank or central-bank purchase of bonds because the loan’s existence could directly give rise to flows, which might not otherwise occur, of private-sector spending on goods and services. Both Friedman and Schwartz had responded to this line of argument in their writings since 1965.23 But Tobin’s 1978 paper did not cite these writings and, in common with the rest of the article, did not mention Friedman at all. Friedman and Schwartz nevertheless recognized the paper as a critique of their own position on money and, in revising their *Monetary Trends* in the years ahead of its 1982 publication, would take the opportunity to include what

---

23 See Friedman (1972a, p. 922) and Schwartz (1969, p. 9; p. 177 of 1987 reprint).
was, in effect, a short reply to Tobin (1978a). Friedman and Schwartz noted that “Tobin does not refer to any empirical evidence to support such an interpretation of his conclusions,” and they argued that Tobin’s theoretical outline did not adequately distinguish between predictions for aggregate spending and predictions concerning the composition of spending.24

Tobin was less reticent during the late 1970s in naming Milton Friedman on those occasions when Tobin participated in economic debate in the public square. One instance in which naming Friedman proved unavoidable was Tobin’s April 1977 joint appearance with Friedman on television. That debate is discussed later in this chapter. Another instance came in a New York Times op-ed by Tobin (November 20, 1977) that argued that Arthur Burns should not be reappointed head of the Federal Reserve Board. Whereas Tobin (1977b, p. 763) had made a general reference to “monetarist criticism from economists” as a factor framing recent years’ Federal Reserve policy decisions, his op-ed was more specific that it was “the influence of Milton Friedman and other monetarists” that had led to the Federal Open Market Committee’s monetary policy organized in terms of monetary targets. Tobin argued that, partly as a consequence, Burns had pursued too restrictive a monetary policy and should not be given a third term as Federal Reserve Chairman.

Tobin’s piece did acknowledge, with considerable understatement, that Friedman himself was not “entirely satisfied” with the evolution of monetary policy under Burns. And in another article during the same period, Tobin (1977c) granted that Friedman did not think monetary policy was tight (and, indeed, had been warning of late that it was too loose). Tobin cited Friedman’s Newsweek column of October 3, 1977, to this effect. From the column, Tobin inferred that Friedman must believe the current rate of unemployment of about 7 percent was at or below the natural level. As it happens, modern estimates of potential output, constructed by the Congressional Budget Office, imply that the U.S. output gap actually was positive in 1977:Q3, so if this was Friedman’s position, it was one supported in retrospect by the data. It is more likely, however, that Friedman—as of late 1977—simply believed that enough monetary stimulus was in the pipeline to produce an undershooting by unemployment of its natural rate in the period ahead. Indeed, U.S. unemployment kept declining well into 1979.25

24 Friedman and Schwartz (1982, p. 32). Elsewhere in Monetary Trends, however, Friedman and Schwartz (1982, p. 37) did make wholly positive remarks about another 1978 Tobin discussion (published as Tobin, 1980a), in which Tobin had taken issue with the emerging literature seeking microfoundations of money-holding in overlapping-generations models. They endorsed Tobin’s position that the stock of money in such models served as a vehicle for household saving but did not have the more distinctive characteristics usually associated with monetary assets. McCallum (1983) endorsed Tobin’s analysis for the same reason.
25 In discussing the early period of the Carter Administration, Karl Brunner and Allan Meltzer would portray it as one in which monetary acceleration spilled over into inflation despite the existence of continuing slack (Wall Street
In Tobin’s book *Asset Accumulation and Economic Activity*, which appeared in 1980 but was based on lectures given in January and March 1978 (Tobin, 1980b, p. vii), he did bring to an end at least one longstanding disagreement with Friedman. Tobin’s opposition to the natural rate hypothesis had come to be perceived as being behind the times, and his retrogression in some of his analysis, such as Tobin and Buiter (1976), to the simple assumption of a constant price level came in for particular criticism (see Calvo, 1985, p. 95). In the early-1978 lectures Tobin indicated that he now accepted the natural rate hypothesis (see especially Tobin, 1980b, p. 39). This aspect of the lectures would be much noted upon when they appeared in book form (see, for example, Lucas, 1981, p. 560; Grossman, 1982, p. 136).

Tobin’s acceptance of the Friedman-Phelps modification of the Phillips curve made itself felt also in his subsequent usage of the terminology given in the accelerationist and natural-rate approaches to the full-employment unemployment rate. Thus Tobin (1983, pp. 512–513) referred to the nonaccelerating-inflation rate of unemployment (NAIRU), and Tobin (1985b, p. 606) referred to the natural rate of unemployment. These analytical concessions did not, however, bring Tobin much closer at all to Friedman’s perspective on inflation as far as practical policy prescriptions were concerned. Tobin continued to associate much of U.S. inflation with factors other than demand conditions: indeed, as of late 1977 he viewed the U.S. economy as featuring 17 percent excess capacity (Tobin, 1977c). Tobin perceived demand restriction as an inefficient and weak, and largely unnecessary, means of removing inflation—whose appropriate remedy he continued to see as, instead, largely lying in incomes policy.

Tobin’s book of lectures also signaled that he was redirecting much of his energy in research debates away from criticism of Friedman and toward challenging rational expectations macroeconomics—whose arguments against activist stabilization policy and questioning of fiscal policy led Tobin (1980b, pp. 21, 36) to give it the label of “Mark II Monetarism.” Consequently, when Robert Lucas published “Tobin and Monetarism: A Review Article,” that highly-critical article focused primarily on the book’s criticisms of optimizing rational-expectations models and of Ricardian equivalence (see Lucas, 1981). Lucas’ book review did, however, pay tribute to the Friedman-Phelps development Phillips-curve analysis. And it blasted the empirical record of Keynesian economics, in a passage that Friedman would quote favorably in print in 1987.26

---

26 See Friedman (1987a, p. 13).
Tobin’s disagreement with Friedman extended to microeconomic and public-policy issues. When others stressed the similarities of his and Friedman’s welfare-reform proposals, Tobin would pour cold water on the notion of a convergence of views (see Nelson, 2018b, Chapter 13). Likewise, in the area of income taxation, Tobin stressed his differences with Friedman at a May 1976 conference. In the course of the conference proceedings, a Friedman *Newsweek* column (April 12, 1976) of the previous month—in which he had proposed that tax revenue and economic efficiency could be boosted by a large cut in income tax rates implemented in conjunction with a tightening of tax deductions—became a focus of discussion. In the resulting exchanges, Tobin concurred that the system of deductions needed overhaul but added: “If Friedman’s choice—of either reduced rates or less revenue—were the only choice you offered me, I would support his proposal. I do not, however, believe that it is the only choice available.” Friedman’s proposal, Tobin suggested, would, in effect, “reward the people who have, by seeking and using all kinds of loopholes.”

Friedman was not an attendee of this conference. But it took place at a poignant location: the Hoover Institution, which Friedman would formally join about fifteen months after the conference took place. For, as indicated earlier in this chapter, in 1977 Friedman’s years at the Hoover Institution began.

*Friedman at the Hoover Institution*

Biographical material supplied by Friedman over the years frequently gave his affiliation with the Hoover Institution in 1976—and sometimes specifically dated it to December of that year. But this dating was not accurate. Soon after his relocation to California, Friedman did give a talk at the Hoover Institution in January 1977 on the topic of his receipt of the Nobel award in economics. But this contribution had been part of his participation in a conference. Friedman’s employment proper at the Hoover Institution did not begin at that time, and he was not located at the Hoover Institution in the subsequent eight months. His position as Senior Research Fellow

---

27 From Tobin’s remarks in Campbell (1977, p. 174).
28 For example, the biographical sketch in Friedman (1976f) incorrectly implied that he taught at the University of Chicago while holding his Senior Research Fellow post at the Hoover Institution, and that in Friedman (1977i, p. 11) gave him as starting his Hoover Institution post in December 1976. Friedman’s entry in Europa Publications Limited (1986, p. 822) described him as “Sr. Research Fellow, Hoover Inst. of Stanford Univ., 1976–.”
29 See Friedman (1977j).
30 During his time at the Federal Reserve Bank of San Francisco, Friedman reluctantly agreed to pay one visit to the Hoover Institution, to see his new office. “I had to coax Professor Friedman to come down and see his new office at Hoover. The inducement was to see what a very nice office he had, far better than his University of Chicago office. He came to see the office just before he went to Vermont for the spring and summer [1977].” (Gloria Valentine, personal communication, October 4, 2014.)
at the Hoover Institution formally began only in the first (that is, the fall) semester of the 1977/1978 academic year. This would be a post he would hold for the rest of his life.

Friedman received his own spacious office in the Hoover Institution’s building, with his University of Chicago secretary of the previous five years, Gloria Valentine, continuing as his secretary throughout Friedman’s Hoover Institution era and being located in the outer office. Next along from Friedman’s office, Rose Friedman also had her own permanent office (Straits Times (Singapore), October 18, 1980; Gloria Valentine, interview, April 1, 2013).

From the beginning, however, Friedman intended the Hoover Institution to be a base of operations, rather than a place at which he could be expected to be found on a five-day-a-week basis. Gloria Valentine recalled (interview, April 1, 2013): “There were sometimes meetings going on or seminars that he wanted to attend, and he would go to those; and he usually tried to get to the office once a week.” The fact that Milton and Rose Friedman had moved into an apartment in San Francisco was significant here. Gary Becker (interview, December 13, 2013) noted with regard to Friedman’s physical presence at the Hoover Institution that “he was not around a lot, because he lived in San Francisco, and if you know that area, San Francisco’s not so close to Hoover: it’s like a 45-minute to an hour drive, depending on traffic. So he was not there on a daily basis—not at all.”

But though Friedman was not as constant a presence at the Hoover Institution as his new affiliation might have suggested, it was certainly true that his move to Northern California’s Bay Area was permanent. It signified a break from Friedman’s former attitude which, Michael Keran recalled (interview, March 7, 2013), was “that this is Lotus Land and you’ll never get any real work done if you come to the West Coast.” Richard Muth, who had served as an associate professor at University of Chicago’s business school in 1959–1964, had been a professor of economics at Stanford University since 1970 (Blaug and Sturges, 1983, p. 279). Muth recalled of Friedman’s own move, “it was funny because, before that time, whenever he saw me, when I used to go to Chicago but I’d gone to Stanford, he would ask: “How are things in Lotus Land?” So, when he first moved out here, I asked him the same question.” (Richard Muth, interview, May 20, 2015.)

31 The Wall Street Journal (July 17, 1978, p. 27) referred to the “Hoover Institution in Palo Alto” as being “nearby” the Friedmans’ apartment in San Francisco and suggested that Friedman’s commute was short. In fact, the Hoover Institution was not close to the Friedmans’ apartment (nor, incidentally, was the Hoover Institution located in Palo Alto, though it was near that city), and the commute was therefore not short, even for an automobile driver who exceeded the legal speed limits (as Friedman often did).
The fact that Muth and Friedman were once more affiliated with the same university reflected the fact that Friedman’s move to the Hoover Institution implied a move to Stanford University as well. In a form letter sent to Friedman’s correspondents notifying them of his change of address, his new address was given as “The Hoover Institution[,] Stanford University[,] Stanford, CA. 94305.”32 And when Friedman’s January 1977 talk on his Nobel award was published by the Hoover Institution, the booklet’s cover identified that body as “Hoover Institution • Stanford University.” Indeed, a wire report on the announcement of Friedman’s appointment had highlighted the Stanford University part of his new affiliation, as the story was headlined: “Friedman To Take Post At Stanford” (Christian Science Monitor, November 4, 1976).

These descriptions were totally accurate. Hoover Institution was indeed part of Stanford University, and, consistent with this, Friedman was issued a university identification card. Nevertheless, it deserves underscoring that, particularly in the era of the 1970s, the Hoover Institution was perceived as somewhat separate from the university. And Friedman’s appointment was not a joint one. Unlike such subsequent leading macroeconomists at the university like Michael Boskin, Robert Hall, and John Taylor, he did not have a dual Department of Economics/Hoover Institution affiliation. Indeed, in a sharp reaction to a hostile profile of him that appeared in the magazine Human Behavior, Friedman included among his list of the article’s errors the description of himself as “a professor of economics at Stanford.” “I have no formal connection with the Department of Economics at Stanford,” Friedman pointed out (Human Behavior, March 1979, p. 10). It was accurate to call him Professor Friedman, but only because he held an emeritus professorship at the University of Chicago: “an inactive and uncompensated” professorship, he noted. He did not owe the title to his Stanford University affiliation, as the Hoover Institution did not confer professorships.33

Stanford University’s Department of Economics members of the late 1970s and the 1980s, like Paul Evans (a member of the department in 1976–1984) and Kenneth Arrow (who rejoined Stanford University in 1979), did not remember Friedman attending the department’s seminars

33 In addition, although Friedman sometimes made statements (see, for example, Glasgow Herald, October 20, 1980, and Australian Business Monthly, October 1993, p. 54) liable to create the impression that his work still involved grading papers or giving classes, neither of these was part of his duties after 1976 (aside from guest lectures at Stanford University, in the case of teaching). (The Chicago Tribune of April 2, 1978a, likewise said of Friedman that “he now teaches at Stanford University,” which he did not.)
or striking up much of a relationship with the overall department (Paul Evans, interview, February 26, 2013; Kenneth Arrow, interview, December 7, 2013). Indeed, as of the late 1970s, Friedman and the Department of Economics at Stanford University had had a background that was not very conducive to a close relationship: “he had a checkered history with them,” Gary Becker recalled (interview, December 13, 2013). In particular, when the department had sought to hire Friedman in the mid-1960s, it had turned down Friedman’s request that an offer be made to Becker as well. This inaction had helped sour Friedman’s attitude to the offer, which he declined.34

Indeed, it warrants emphasis that, when Friedman joined the Hoover Institution, its relationship with the rest of the university when Friedman joined it was far different from that prevailing in the first two decades of the twenty-first century. In 1977 and 1978, it would still be some time before a host of senior members of the Department of Economics held joint affiliations with the Hoover Institution or when, as now, graduate students from the economics department are a regular sight in the Hoover Institution’s corridors.

In the 1960s and 1970s, and into the 1980s, the Hoover Institution was much more self-contained, for the most part lacking extensive interaction with the broader university. This tendency was reinforced by the fact that many in the regular social-science departments of the university viewed the institution with suspicion, because ideological persuasion figured explicitly in the basis for its existence (Wall Street Journal, June 15, 1984). To be sure, the tensions between the Hoover Institution and the wider university were more severe for social sciences other than economics: for example, the Hoover Institution’s foreign-policy and national-security specialists typically took a more straightforwardly “hawkish” posture on United States/Soviet Union relations than that usually found among international-affairs experts in the political-science and history departments of Stanford University and elsewhere. But the Department of Economics’ membership and Hoover Institution’s economists also tended to have something of an arms-length relationship until the 1980s.

The relationship between the university and the Hoover Institution improved after the 1970s. Even in 1987, however, Friedman described the relationship between the overall university and the institution as one of “armed neutrality”—and did not, himself, stimulate good relations by adding that he would like the Hoover Institution to have “nothing to do with Stanford” (Stanford Review, November 1987). The more convivial relationship between the university and the

34 For more details, see Nelson (2018b, Chapter 12).
institution, especially between its economists, that developed in later years make such statements now seem very jarring.

Notwithstanding the tension between Hoover Institution and the university in Friedman’s first decade or so there, the number of economists with joint Hoover Institution/Department of Economics affiliations gave Friedman some degree of connection with the department over time. So did the presence of old friends in the department like Richard Muth: “I saw as much of him during that period, I think, as I ever had.” (Richard Muth, interview, May 20, 2015.) And through such means as their visiting the Hoover Institution and their attending conferences that it held, Friedman developed extensive interactions with some more junior economists whose permanent Stanford University affiliations were not with the Hoover Institution. Notable among these members of the younger generation was Ben Bernanke—whose regular affiliation from 1979 to 1985 was with Stanford University’s business school, but who visited the Hoover Institution in the 1982/1983 year. The upshot of these interactions was that, by the time the Friedmans wrote their memoirs, Milton Friedman could cite the proximity to the Stanford University economics department as an advantage of his Hoover Institution affiliation.35

Friedman’s attendance of Department of Economics seminars remained extremely rare, however. Thomas Sargent was based at Hoover Institution for part or most of each year from 1985 to 2002 and was, in addition, closely related to Stanford University’s economics department, sometimes with a formal affiliation.36 With regard to Friedman’s attendance at the economics department’s seminars, Sargent observed, “I think I saw him at one seminar.” However, Sargent noted that, additionally, “we had seminars at Hoover; he came to some of those.” (Thomas Sargent, interview, January 24, 2014.)37 Indeed, though Friedman himself moved further toward public policy and away from research in his Hoover Institution years—especially after his and Anna Schwartz’s completion of their final book, Monetary Trends—he continued to supply comments on much of the monetary research sent to him by correspondents. All told, Friedman’s presence at the institution contributed to a shift noted by Michael Boskin, in which the Hoover Institution’s contribution to research had improved over the previous decade, from being in a poor state in 1974 (Wall Street Journal, June 15, 1984).

37 John Maynard Keynes’ life played a role in giving rise to an instance in which Friedman involved himself in the economics seminars of Stanford University’s economics department. For the occasion in October 1991 when Keynes’ biographer Robert Skidelsky was visiting the Hoover Institution, Friedman contacted Stanford University’s economics department to encourage the scheduling of an additional Skidelsky talk, in its own seminar series (Gloria Valentine, interview, April 1, 2013; Lord (Robert) Skidelsky, interview, November 26, 2013).
During the period in which he accustomed himself to the new environment in 1977 and 1978, Friedman’s appearances at the Hoover Institution were somewhat more frequent than would later be the case. An incentive to come into the office in that period lay in the fact that, during the 1977/1978 academic year, the Hoover Institution had a large amount of visiting researchers who shared a University of Chicago or monetary-economics background with Friedman. Among these were Karl Brunner and Allan Meltzer, as well as Robert Barro and Michael Darby.38


With regard to the national scene, at the start of 1977 Friedman entertained some hopes about the economic policy likely to be pursued by Jimmy Carter, who would take office as U.S. president on January 20. As noted in Chapter 4, during the 1976 presidential election campaign Friedman had indicated he preferred Ford to Carter (and Ronald Reagan to either of them). Furthermore, Carter’s principal economic adviser in that campaign had been Lawrence Klein, the pioneering Keynesian model-builder and past sparring partner of Friedman. Under Klein—and in contrast to Friedman’s own perception of the appropriate prescription for the U.S. economy—Carter’s presidential campaign had put considerable emphasis on the need for economic stimulus. Klein had specifically indicated: “We feel the Fed has been miserly in dealing out money. There should be a more expansionary policy.” (The Sun (Baltimore), July 25, 1976, p. K9.)

Shortly after the election, Friedman had observed in a television interview: “I believe that the best thing for this country will be if Mr. Carter can rise above his advisers, including Larry Klein, who I like and respect but don’t agree with.”39 Around the same time, Friedman was under the impression that Arthur Okun would likely figure among the administration’s economic personnel: he welcomed this, on the grounds that Okun was “much less extreme [and] much more balanced” than other Keynesians such as Lawrence Klein (Instructional Dynamics Economics Cassette Tape 202, November 1976, Part 1). In the event, Okun did not join the administration, while Klein did not take an official post either.

38 Brunner (1978, p. 649) stated: “The first draft of the paper was prepared [during Brunner’s period] as a visitor at the Hoover Institution (Stanford University) during the winter 1977–8.” Meltzer was also at the Hoover Institution in this year (Allan Meltzer, interview, April 21, 2013; Committee on Banking, Finance, and Urban Affairs, 1978c, pp. 107–108). For Barro’s and Darby’s positions as Hoover Institution in National Fellow in 1977/1978, see Barro (1978, p. 569), as well as their respective online CVs: https://scholar.harvard.edu/files/barro/files/vita_0518.pdf?m=1525874087 and https://www.anderson.ucla.edu/Documents/areas/fac/strategy/MICHAEL%20R.%20DARBY%20CV.pdf.
39 Wall Street Week, Maryland Public Television, November 5, 1976, p. 11 of transcript. The transcript of this appearance was largely reproduced at the time in the American Banker (November 22, 1976). In addition, the host of Wall Street Week, Louis Rukeyser, used his syndicated newspaper column to summarise Friedman’s position on Carter and his advisers (see The Evening Bulletin (Philadelphia), November 15, 1976).
Friedman would point to someone other than himself whose economic prescriptions Carter should heed. This was the U.K. Prime Minister James Callaghan, who had declared in a Labour party conference speech of September 28, 1976: “We used to think that you could just spend your way out of a recession and increase employment by cutting taxes and boosting Government spending. I tell you, in all candor, that that option no longer exists, and that insofar as it ever did exist, it only worked by injecting bigger doses of inflation into the economy, followed by higher levels of unemployment as the next step.” Friedman quoted this statement many times (see Nelson, 2009, p. 84), including in his December 1976 Nobel lecture.40

The notion that Callaghan had provided words for Carter to live by informed the title of Friedman’s *Newsweek* column of December 6, 1976, titled “To Jimmy from James.”41 In that column, Friedman provided what turned out to be a prophetic characterization of what would ensue if the United States in 1977 pursued expansionary economic measures: “If Mr. Carter tries to put his advisers’ policies into effect and succeeds in doing so—including getting the Federal Reserve System to speed up substantially the rate of monetary growth—there might be a sudden spurt in the economy and a quick reduction in unemployment. However, these good results would be temporary. By 1978 or 1979, inflation would be back in double digits and wage and price controls would be in place or in contemplation. By 1980 at the latest, unemployment would be rising sharply. As Machiavelli might say: what a way to face the 1980 election!”

In a talk he gave in Chicago during this period, Friedman suggested that the U.S. economy would face “deep and real trouble” unless Carter proved to have “the ability to rise above his campaign promises” (*Chicago Tribune*, November 18, 1976).

Friedman was to be disappointed by the fact that the Carter Administration actually pressed ahead during 1977 with advocacy of stimulus to aggregate demand. Although the administration did abandon its early proposal for a tax rebate (see Section II below), its economic policy continued to be oriented toward prompt elimination of the perceived high level of economic slack in the economy.

Another economic move on Carter’s part was the announcement, on April 15, 1977, of a variety of measures intended to reduce inflation from 6 percent to 4 percent by the end of 1979 (*Boston*

---

40 See Friedman (1977c, p. 460).

41 The title carried the incorrect implication that the U.K. prime minister was more formal in his rendering of his first name than was President-elect Carter. In fact, Prime Minister Callaghan was widely referred to in his home country as Jim Callaghan. (Friedman would belatedly refer to Callaghan as “Jim Callaghan” in Friedman, 1984b, p. 29.)
As this package consisted of a series of measures intended to alter specific prices and involved no monetary policy action, it might have been expected to provoke a wholly negative reaction from Friedman. But Friedman’s response to the package—which he gave in the course of a debate on April 18 with James Tobin on public television’s MacNeil/Lehrer Report—was disarmingly benign. Friedman noted that the package was, in essence, directed at relative prices and could not be expected to achieve its stated aim of reducing inflation. But, evidently wearing his “public choice theory” hat (see the next chapter), Friedman took the president’s team as understanding the monetary nature of inflation but as obliged to announce an inflation package for public-relations purposes. The president had not included monetary measures in his package, Friedman said, but that signified a recognition that the executive branch lacked authority over monetary policy. Furthermore, Friedman stressed, the fact that Carter had not proposed broad wage and price controls was to be applauded. Accordingly, Friedman observed: “I want to commend President Carter.” In addition, Friedman gave a backhanded compliment to the inflation package, which was “excellent because it tries to little to do so little of the wrong things.”

In making this judgment, Friedman took something for granted that he often did in analyzing Arthur Burns’ statements. This was that policymakers ascribed the same degree of importance to monetary policy in inflation’s determination that he did. As has already been indicated in previous chapters, by the mid-1970s, Friedman regarded the monetary view of inflation as so strongly confirmed that he tended to assume that others accepted that view. For example, in April 1978, when confronted with a recent analysis of inflation by Michael Blumenthal, U.S. Secretary of the Treasury, that seemed to line up with wage-push views of inflation, Friedman proclaimed that “Secretary Blumenthal knows as well as you and I do that inflation does not come from trade unions.”

Even by this time, however, considerable evidence had accumulated in support of the conclusion that the Carter Administration did not adhere to a monetary view of inflation. In April 1978, after a year in which inflation had failed to fall below 6 percent, President Carter—who once described inflation as the result of “attitudes and habits” (quoted in Kemp, 1979, p. 97)—gave a new major speech on inflation. The president’s emphasis continued to be on a nonmonetary

---

43 And as he would again—for example, in Newsweek, October 3, 1977, when Friedman discussed a recent speech by Chairman Burns.
approach to disinflation. Carter stated: “Reducing the inflation rate will not be easy, and it will not come overnight. There are no easy answers. We will not solve inflation by increasing unemployment. We will not impose wage and price controls. We will work with measures that avoid both extremes.” (Kansas City Star, April 11, 1978, p. 2A.)

The “measures” that Carter actually advanced in this talk consisted of wage guidelines, alongside an indication that firms should keep price increases in line with the cost increases implied by the wage guidelines. This package, of course, amounted to a revival of a Kennedy-Johnson-era measure against inflation. A year earlier, in his television appearance alongside Friedman, Tobin had lamented Carter’s failure to introduce guidelines in his April 1977 inflation announcement. As Carter later would, Tobin had portrayed guidelines as an alternative to a strategy of demand restriction: “guideposts, or some other direct attempt to reduce the inflation rate and to break the hard-core momentum of the wage/price spiral that we’ve inherited from the past, would be preferable to the solution of keeping the economy stagnant, with… a lot of excess capacity for a prolonged period of time.” In reply, Friedman disputed Tobin’s contention that guideposts had worked in the United States in the 1960s. Friedman went on to provide a challenge: Could Tobin nominate another historical instance in which incomes policy had, he judged, been effective? Tobin declined to accept the challenge, prompting Friedman to remark that “you’re evading my question, Jim. I want to know if you can cite a single example of a successful wage-and-price guideline policy, other than the one that you were involved in.” Tobin insisted that the 1960s U.S. precedent was sufficient: “That’s the one I want to copy.”

By April 1978, however, Carter’s own embrace of guidelines was motivated by the perceived success of a different example: recent years’ wage-oriented incomes policy in the United Kingdom. Ironically, therefore, Carter did adhere to Friedman’s suggestion that he should pattern his approach on that of Prime Minister James Callaghan. But instead of taking a leaf from Callaghan’s monetarist-inspired September 1976 speech on aggregate demand management, Carter was emulating Callaghan’s actual practice of taking a nonmonetary approach to the control of inflation. Judgments concerning the success of Callaghan’s incomes policy would be revised drastically when U.K. nominal wage growth and inflation surged in early 1979. But, as of April 1978, a look at the U.K. experience might have suggested that the decline in inflation in the past couple of years was testament to the value of incomes policies.

Friedman’s reaction to the administration’s April 1978 measures was pointed. “President

---

Carter’s [anti-]inflationary package is like Hamlet without the Prince of Denmark… Inflation is not caused by trade unions, business interest[s], consumers, or oil sheiks… Inflation is a disease that has been around 1,000 years and, in all that period, only one medicine to cure inflation has been found: to hold down the rate of monetary growth and hold down governmental spending.” (Manhattan Mercury (Kansas), April 27, 1978, p. A6.) The president’s package, he later added, was “a reworked ball of nonsense” (Bluefield Daily Telegraph (West Virginia), May 3, 1978). In September 1978, Friedman would express frustration at Carter’s lack of reference to monetary policy on the occasions when the president discussed inflation—a tendency that Friedman said set Carter apart from every other “high government official in any Western country.”

This tendency continued the following month. In October 1978, with U.S. CPI inflation approaching 9 percent, Carter announced yet another anti-inflation package, in which he broadened his wage/price guideline proposals. The deterioration in inflation performance over the previous eighteen months was reflected in the fact that, whereas in April 1977 Carter had sought to get inflation down to 4 percent in 1979, now he merely hoped to bring the inflation rate in 1979 back down to about 6 to 6.5 percent (Financial Times (London), October 25, 1978).

The devices in Carter’s October 1978 package advanced to achieve this goal included voluntary wage guidelines, voluntary price guidelines, and a “real wage insurance” scheme (Omaha World-Herald, October 25, 1978). The last of these proposals, which the administration continued to promote into 1979 (see Committee on Ways and Means, House of Representatives, 1979, and Romer and Romer, 2002, p. 62), would provide tax relief, for those employees who adhered to the wage guidelines, if realized inflation proved to be high in relation to the guidelines. It was therefore a variant of the “tax-based incomes policy” idea of which Friedman was so derisive (see Chapter 4 above). As it required Congressional approval that was not forthcoming, the real-wage-insurance idea was not enacted (Biven, 2002, p. 189). However, the Carter administration continued its wage-price guidelines beyond 1978.

II. ISSUES RELATED TO DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1977–1978

CROWDING OUT AND RICARDIAN EQUIVALENCE

Underlying the Carter Administration’s approach to inflation in 1977–1978 was the view that

---

high inflation did not reflect a problem of excessive aggregate demand, nor did the cure for inflation lie in restriction of aggregate demand. Indeed, as the administration saw things in these years—particularly during 1977—the U.S. economy had too little, not too much, aggregate demand. This attitude shaped the administration’s early moves in the area of fiscal policy.

As of late 1976, Friedman seemed confident that Carter would discard his pre-election commitments to introduce stimulative measures. Friedman thought he observed in Carter’s post-election press conference the start of a “gradual withdrawal from his pre-election promises” to stimulate the economy—a process that he believed would see a “neatly-staged retreat from those positions” (Instructional Dynamics Economics Cassette Tape 203, November 1976, Part 2). As for the policies that Carter should actually follow, Friedman stated: “The advice that I would give to Jimmy Carter is exactly the advice I would give to Gerald Ford, [and] exactly the advice that I would give to anybody else: What this country needs is less government spending, not more; what we need is to let people spend more of their own money instead of first sending it to Washington, taking a commission off and then sending a small portion back...” (Donahue, NBC, November 24, 1976.) In particular, Friedman denied the need for stimulus, arguing that the economy was “pretty strong… [W]e’re in a good expansion.”48

On taking office, however, the Carter Administration proceeded with plans to stimulate the economy. In a discussion of the history of U.S. fiscal measures designed to affect aggregate demand, Romer and Romer (2010, p. 764) conclude: “Tax actions… were common in the early postwar era but virtually disappeared after 1975.” This is an accurate description of implemented tax measures in the quarter-century after 1975. However, it is notable that, early in its existence, the Carter Administration attempted to enact a demand-stimulating tax measure. Shortly after taking office, Carter proposed implementing a $50-per-household tax rebate. This rebate (actually a transfer that would go even to those who lacked any tax liability) was proposed to be put into effect in the second quarter of 1977 (New Republic, May 7, 1977, p. 11).

Friedman had poured scorn in his first Newsweek column for 1977 on “the implicit assumption of so much current talk… that a tax cut or an increase in government spending is ‘stimulative’” (Newsweek, January 10, 1977, p. 59). When the rebate proposal was formally advanced a few weeks later, Friedman was, not surprisingly, strongly negative in his reaction (Los Angeles

---

48 Wall Street Week, Maryland Public Television, November 5, 1976 (American Banker, November 22, 1976, p. 6).
This negative reaction had two branches: one that pertained to a case in which the tax rebate was monetized; the other that referred to the likely effects of the rebate if it turned out to be a “pure” fiscal policy measure—that is, if it was not accompanied by extra money creation.

As far as the monetary side of the tax proposal was concerned, it is important to bear in mind that—at this stage of his understanding of U.S. historical regularities—Friedman still regarded the typical pattern as one in which budget deficits were accommodated by monetary policy (Financial Times (London), January 6, 1977; St. Louis Globe-Democrat, December 16, 1977; Newsweek, April 24, 1978). This background made him think it likely that a rebate would prompt an increase in the U.S. money stock. That this was not an unreasonable expectation, whatever the historical evidence said, was underlined by the fact that Federal Reserve Chairman Arthur Burns—although he was critical of the rebate proposal—indicated that the Federal Reserve might allow the money stock to rise, on a one-time basis, with the tax rebate (Daily News (New York), March 23, 1977). Furthermore, Burns had confirmed that he intended to follow policies consistent with Carter’s announced goals of 6 percent real GNP growth in 1977 together with an unemployment rate of around 6 to 6.5 percent by year-end, compared with the value of about 7.5 to 8 percent believed to be prevailing when Carter was elected (Omaha World-Herald, November 24, 1976; Los Angeles Times, February 4, 1977). In the event that it was monetized, Friedman said, the tax rebate would be inflationary.

For the case in which the federal borrowing arising from the rebate was not monetized, Friedman saw the measure as unlikely to be effective in stimulating aggregate demand. This was, of course, the same position that he had taken on previous instances when the federal government had attempted countercyclical fiscal measures—including the Ford Administration’s tax rebate in 1975.

The way in which Friedman in 1977 articulated his skepticism about fiscal policy is notable, in light of what has subsequently been said of his views. In a February 2010 blog entry, J. Bradford DeLong declared that while “Friedman was a critic of fiscal policy,” it was a violation of “Milton Friedman’s model” to argue that the ineffectiveness of fiscal policy reflected the notion

---

49 He briefly covered the rebate plan in his Newsweek column (April 11, 1977). Friedman also criticized the plan on the daytime talk program Dinah! (Dinah Shore show) on March 30, 1977.
that attempts at fiscal stimulus amounted to “taking money from one place and giving it to another place.” This confident statement by DeLong should be juxtaposed against Friedman’s actual words, given in response to the Carter rebate proposal: “How can the government stimulate the economy by taking money out of one pocket of the public and putting it into another pocket?” (*U.S. News and World Report*, March 7, 1977.)

**Crowding out**

What were the economic mechanisms that Friedman saw as working against the effectiveness of fiscal-stimulus measures like a tax rebate? The answer to this question was something on which his views were shifting in the late 1970s. The empirical evidence had long convinced Friedman that fiscal measures on their own were, by and large, ineffective in influencing aggregate demand. But his conception of the fundamental reasons why this was the case was changing over this period—from one emphasizing crowding out, to one stressing Ricardian equivalence.

“Crowding out” summarized the process in which fiscal actions that boosted disposable income or public spending had their effect on aggregate demand largely offset by interest-rate-raising effects of the accompanying increase in government borrowing and the resulting downward pressure on interest-elastic private spending. By 1977, crowding out was a well-established part of public discourse on macroeconomic management in the United States. For example, Snellings (1976, p. 6) noted: “Some well-informed observers fear the ‘crowding out’ of private borrowers that was so much-talked-about last year.” Much of the credit for the popularization of the concept went to President Ford’s Secretary of the Treasury, William Simon, who had triggered media and Congressional discussions of crowding out by suggesting in 1975 that it was a likely implication of actual U.S. federal deficits.

Ahead of Simon, however, Friedman had promoted the notion of crowding out for many years in research and public-policy discussions. For example, in November 1968, he observed: “The state of the government budget has a considerable effect on interest rates. If the federal

---

government runs a large deficit, that means the government has to borrow in the market, which raises the demand for loanable funds and so tends to raise interest rates."\(^{53}\) It was against the background of such statements that Benjamin Friedman had occasion in 1978 to refer to “the ‘portfolio crowding out’ emphasized by Milton Friedman.”\(^{54}\)

In discussing the most recent calls for fiscal activism, Friedman’s *Newsweek* column of January 10, 1977, cited the failure of the 1968 tax increase to stop inflation as evidence of the unreliability of fiscal measures in affecting aggregate demand. That 1968 episode had, indeed, been a considerable watershed in creating support among economists for Friedman’s skepticism about fiscal policy. But, although retrospectives tended to point to the temporary character of the fiscal measure as the reason for its apparent failure, Friedman himself had in 1968 had appealed to reverse-crowding-out, rather than his own permanent income hypothesis, as the basis for predicting that the tax increase would fail to brake aggregate demand.\(^{55}\)

Just as in his analysis of the 1968 tax surcharge, Friedman’s critiques of President Carter’s proposed tax rebate largely cast its likely ineffectiveness in terms of crowding-out mechanisms. He granted that the rebate would, if enacted, lead to “extra expenditures by consumers” (*U.S. News and World Report*, March 7, 1977). But the associated government borrowing, he contended, would lower aggregate U.S. saving (*Newsweek*, April 11, 1977) and reduce U.S. businesses’ investment expenditures (Instructional Dynamics Economics Cassette Tape 206, January 1977, Part 1).

**The permanent-income/Ricardian equivalence alternative**

In 1977, just as in 1968, it fell to a Keynesian, Robert Eisner, rather than to Friedman, to emphasize the permanent income hypothesis, rather than crowding out, as the reason for the likely ineffectiveness of the proposed fiscal measure. On February 7, 1977, in testifying about the rebate proposal to the House of Representatives’ Committee on Ways and Means, Eisner observed: “I have to recall to you the theory of Milton Friedman, who points out in his permanent income hypothesis that people’s spending is most influenced by the long run, or

\(^{53}\) From his remarks in Friedman and Heller (1969, p. 50).

\(^{54}\) B.M. Friedman (1978, p. 598).

\(^{55}\) In particular, he did so in his 1967 discussions of the matter, which are cited and discussed in Nelson (2018b, Chapter 13). In addition, in Friedman and Heller (1969, p. 50), he said that the decline in interest rates following the introduction of the 1968 tax surcharge was “precisely what we had predicted and what our analysis leads us to predict.”
permanent, income, and not by temporary income changes.”56 Eisner noted that the 1968 tax surcharge had been associated with a reduction in the saving ratio and the 1975 tax rebate by a surge in the ratio—patterns that suggested that consumers rode out the changes in taxes.57

Buttressing Eisner’s position was something that Friedman acknowledged on television in April 1977: the fact that “interest rates are relatively low,” as they were in nominal terms (compared with the recent past) and especially real terms (compared with most of twentieth-century U.S. history)—in the mid-1970s. He acknowledged that this fact created a problem for the use of crowding-out story as the explanation for why the fiscal deficits already recorded in the mid-1970s might not have been stimulative, on net, for private aggregate demand.58

Thus, as between portfolio crowding out and the permanent income hypothesis, the latter seemed to have considerably more appeal by the late 1970s in understanding the limitations of taxes as a demand-management tool. Furthermore, in 1978, the permanent income hypothesis was given a modern cast by Robert Hall’s (1978b) derivation of the consumption function in an infinite-horizon model. Although Hall treated real interest rates as constant in his analysis, research of around the same vintage by Boskin (1978) provided evidence that saving (and so consumption) significantly depended on the real interest rate—a position that Friedman had long advocated.59

Thus, this period saw the basic ingredients of the New Keynesian IS function—a permanent-income infinite-horizon consumer model and interest-elastic consumption (as well as investment) expenditures—laid out in research studies. This approach to modeling private behavior affirms that measures such as tax rebates will fail to affect aggregate demand. And it does so because of the permanent income hypothesis, and in particular a particular corollary of that hypothesis: what in the late 1970s came to be known as Ricardian equivalence.

Moving toward acceptance of Ricardian equivalence

The puzzle is why, as the architect of the permanent income hypothesis, Friedman himself did not in 1977 deploy the hypothesis when he criticized the Carter rebate proposal. It was not for

56 In Committee on Ways and Means (1977, p. 498).
57 Similarly, Franco Modigliani, in testimony at another hearing the same day (see Joint Economic Committee, 1977, p. 158) suggested that the 1975 tax cut had primarily been saved. However, as discussed in Chapter 4, in his later research, such as Modigliani and Sterling (1986), he went back to a more traditionally Keynesian position that the 1968 and 1975 tax changes had indeed sizably affected household spending.
59 On the last point, see Nelson (2018a, Chapter 5).
lack of understanding of the implications of the permanent income hypothesis for temporary tax measures. Those implications—which were embedded in Ricardian equivalence—had been spelled out by Barro (1974). Barro had presented this formal outline of Ricardian equivalence at Friedman’s money workshop at the University of Chicago in 1973. In the course of the discussion of Barro’s paper, Friedman eventually sided with Barro on Ricardian equivalence against the skeptical reaction expressed by other participants in the workshop (Barro, 2007, p. 133).

Furthermore, even before Barro’s formalization of the idea, the notion of Ricardian equivalence had run through many discussions of fiscal policy. In 1977, the concept’s venerable status in economics analysis was underlined by O’Driscoll’s (1977) pinpointing an exposition of the hypothesis made by David Ricardo. But even in the quarter-century to 1974, Ricardian equivalence had been a recurring feature of theoretical discussions of the effects of fiscal policy. Barro (1974, p. 1096) had himself offered a number of quotations to this effect. In addition, Modigliani (1964a, p. 483) had referred to the hypothesis as a familiar one: he had observed that it was a widespread proposition that in conditions of certainty, perfect credit markets, and infinite horizons, “tax finance and loan finance must have identical effects,” and in particular the “consumption pattern would be the same in both cases.” And Patinkin (1967, p. 11) suggested that “whether government interest-bearing securities… [are] a net asset of the private sector” was an issue “increasingly discussed in recent years.”

In addition, and notably, an extended analysis of pre-1974 discussions of Ricardian equivalence by Elmendorf and Mankiw (1999, pp. 1643–1644) drew attention to a passage in the second edition of Patinkin’s Money, Interest and Prices (see Patinkin, 1965, p. 289) that implied that Patinkin had first learned of the idea of Ricardian equivalence indirectly from Friedman (via Patinkin talking to Carl Christ, who had attributed the idea to Friedman).60

60 Elmendorf and Mankiw very strongly implied that Patinkin must have learned of the hypothesis during the year in which he, Carl Christ, and Friedman were all located at the University of Chicago—that is, the 1946/1947 academic year. However, the evidence cited by them gives little support for this speculation, for the Patinkin passage in question was not in the original, 1956, edition of Patinkin’s Money, Interest, and Prices; rather, it first appeared in the 1965 edition (with Beard and Johnson, 1974, specifically noting it as an addition). Carl Christ was an associate professor at the University of Chicago in 1955–1961 (American Economic Association, 1981, p. 96), so there were abundant opportunities for Christ to talk about fiscal policy with Friedman after 1946, and for the conversation that Patinkin heard about to take place. The fact that Patinkin (1967, p. 11) associated the discussion of the Ricardian-equivalence idea specifically with Mundell (1960) reinforces the likelihood that Patinkin did not perceive Friedman as having floated the notion at a much earlier point than the early 1960s.
Friedman had also recognized the Ricardian-equivalence scenario in print when, in 1970’s *Monetary Statistics*, he and Schwartz had noted that government debt would not count as part of individuals’ (net) real wealth if “individuals are regarded as treating the obligation to pay taxes to finance interest payments on government debt as a liability.”

By the early 1970s, Friedman was, in fact, including Ricardian-equivalence mechanisms in his list of reasons for expecting little response of total spending to arise from fiscal stimulus. He did this most notably in his contribution to the 1972 *Journal of Political Economy* symposium on monetary theory. It was largely on the basis of this 1972 contribution that Roley (1981, p. 22) suggested that “Milton Friedman also appears to rely on the ultrarationality assumption [that is, Ricardian equivalence] in describing the ineffectiveness of bond-financed fiscal policy.” But as he had indicated in a 1970 discussion (Instructional Dynamics Economics Cassette Tape 61, November 18, 1970), the actual outcome Friedman anticipated in the wake of a tax-cut-induced budget deficit was a mixture of an interest-rate rise and Ricardian reactions (in which, in effect, the tax cut proceeds were used to buy bonds) and Friedman’s 1972 article was consistent with this, as it had still counted downward pressure on interest rates as one effect of a tax increase—so it was not actually, at this point, ruling out crowding-out channels entirely. Similarly, Tobin (1976, p. 335) was jumping the gun somewhat when he pointed to a Friedman *Newsweek* column of January 27, 1975, as having made Ricardian equivalence “a central argument” against fiscal policy as a stabilization device. For a subsequent Friedman *Newsweek* column (May 12, 1975), though acknowledging as a special case that in which new bond issuance by the federal government “can be regarded as advance receipts for future taxes!,” affirmed that a tax cut would in practice tend to raise interest rates, as it would not be fully saved.

What seems to have been a stumbling block preventing Friedman’s putting Ricardian equivalence front and center, both in these discussions and again in his 1977 critique of the rebate, is that he was not quite ready to accept an infinite-horizon approximation as the baseline assumption for the analysis of the private sector’s reaction to fiscal measures. As of 1977, he was still inclined to view the evidence as balanced in favor of the presumption that U.S. households did not fully offset changes in public-sector saving with their own saving decisions. He was likely broadly sympathetic at this stage with the work in the area by Martin Feldstein.

Feldstein had argued that Social Security taxes reduced U.S. private capital formation (see Feldstein, 1974b—work Friedman mentioned in Instructional Dynamics Economics Cassette

---

62 Correspondingly, Benjamin Friedman (1978) had specifically cited Friedman (1972a) as having advanced the crowding-out position.
Tape 174, August 1975, Part 2) and who had debated Barro on that issue.  

Even in early 1977, Friedman was, however, exhibiting further moves in the direction of empirical acceptance of Ricardian equivalence. This was evident when he stated that a bond-financed tax cut raised future interest payments of the government, but without specifically claiming that a tax cut pushed up interest rates (Newsweek, January 17, 1977; U.S. News and World Report, March 7, 1977).

In a short span of years after 1978, Friedman would move to, essentially, complete acceptance of the Ricardian equivalence proposition as the baseline hypothesis. Friedman had a vigorous informal debate with Robert Hall on the consumption function in conversation at the Hoover Institution during 1978/1979 (Levis Kochin, interview, April 23, 2013). Not only would he become persuaded by the analytical arguments put forward by Barro and Hall; but also, he came to be further convinced that the real-interest-rate/budget-deficit relationship in the United States in the 1970s had not, in fact, borne out the predictions implied by the crowding-out story that he and others had expounded (see Chapter 9 below).

In sum, although doubts concerning the power of fiscal policy were a perennial feature of Friedman’s analysis from the early 1950s onward, it was only after the 1970s that the permanent income hypothesis really achieved pride of place as the basis for his doubts. He would then believe, as he had before, that moving money from one pocket to another was not stimulative. But he would come to see this experiment in terms of shifting funds between a pocket in the present and a pocket in the future.

President Carter’s abandonment of the rebate proposal

President Carter withdrew the tax rebate proposal in mid-April 1977. Dornbusch and Fischer

63 See Barro and Feldstein (1978). Friedman’s sympathy with the Feldstein results did not signify a rejection of rational expectations. Indeed, Feldstein pointed out (especially in Feldstein and Pellechio, 1979, p. 362) that he was resting his theoretical basis for rejecting Ricardian equivalence not on consumers being irrational but, instead, on the empirical importance of deviations from an infinite-horizon framework. (In the case of Social Security specifically, the opposite, Barro, position was that households—having infinite horizons—would make saving plans that offset the effects on nationwide saving, and on the allocation of resources across generations, associated with payroll taxes and other Social Security arrangements. This behavior, in turn, would lead capital formation to be unaffected by Social Security arrangements. See especially Barro and Feldstein, 1978, and Boskin and Kotlikoff, 1985.) In time, however, Friedman would become more comfortable with the infinite-horizon baseline, and so he moved away from the Feldstein side to the Barro side of the Ricardian-equivalence debate.

64 Kochin was a National Fellow at the Hoover Institution in 1978/1979: see https://econ.washington.edu/file/2154/download?token=ZwqbbCcm.
(1978, p. 546) observed that the abandonment of the rebate idea was partly because of good readings on U.S. economic activity. They added, however, that a consideration weighing against the rebate was “the question of whether transitory tax cuts or rebates are really effective in stimulating aggregate demand.” Indeed, another textbook account—Gordon (1978, p. 487)—maintained that the rebate would likely have been saved. Gordon therefore largely endorsed the Ricardian-equivalence account, in this case. However, notwithstanding the respect with which the economics profession, by this stage, already treated the permanent income hypothesis, there remained widespread adherence to an old-Keynesian belief that the rebate would have been powerful. For example, in the period preceding the plan’s withdrawal, the Wharton econometric model’s forecast had predicted that the rebate would boost consumption and output in the second and third quarters of the year (Daily News (New York), March 25, 1977). And, after the plan’s withdrawal, Tobin (1978b, p. 20) lamented the opposition the rebate had received and claimed that the “rebate in 1975 had been instrumental in launching the recovery.” Robert Solow, while conceding that doubts about the effectiveness of rebates had grown in the economics profession, suggested that “a $50 rebate might be spent rather quickly” (New Republic, May 7, 1977, p. 13.)

Solow criticized the “infirmity of purpose” behind the rebate’s withdrawal (New Republic, May 7, 1977, p. 13). Indeed, when she came to office as Prime Minister of the United Kingdom two years later, Margaret Thatcher viewed Carter’s reversal on the rebate as a lesson on what not to do, on the grounds that backing down from a publicly-stated position amounted to a lack of resolve (Young, 1991, p. 129). In 1977, however, Friedman offered a more favorable interpretation of the president’s move than that embedded in these assessments. “I think it speaks extraordinarily well for President Carter’s capacity to adjust his plans to circumstances,” Friedman remarked shortly after the rebate’s cancellation. “It’s very, very hard for any of us to admit that we’re wrong, and to withdraw from a position in which we have gone.” (Instructional Dynamics Economics Cassette Tape 211, April 1977, Part 1.)

Subsequently, it became rare for Friedman to speak so highly of President Carter. The April 1977 praise arose partly because Friedman took Carter’s move as an acceptance that stimulation of aggregate demand was no longer appropriate. It would, however, become clear that this was not the case. In his commentary on the rebate’s withdrawal, Solow implied that demand-stimulating measures were still needed (New Republic, May 7, 1977, p. 13). The Carter Administration shared this view, and it continued to support expansionary measures after April 1977, in both the fiscal sphere and the monetary sphere.
THE SECOND MONETARY EXPLOSION

As recounted in Chapter 4, in the year to December 1976, CPI inflation in the United States was slightly below 5 percent. This rate, the lowest since 1973, implied a decline in inflation of over seven percentage points from the peak in late 1974. That considerable achievement set the tone for much economic commentary in late 1976 and into 1977—in which it was suggested that, although no further decline might be in prospect, the reduction in inflation seen during 1975 and 1976 would, essentially, be maintained. For example, one financial commentator wrote in late 1976 (Bankers Monthly (November 15, 1976, p. 2): “Inflation is likely to remain in the 5½ percent range over the coming year.” Once, in early 1977, inflation had shot back above 6 percent, the same commentator remained sanguine, observing (Bankers Monthly, July 1977, p. 2): “Price inflation has slowed predictably from its unsustainable, weather-induced first-quarter pace. Consumer prices should advance at a rate no faster than the 5½–6 percent official estimate in the second half of this year.” Over in Forbes magazine, an analysis of inflation had recently been very optimistic about the prospects for inflation, with the article concluding: “Inflation, where is thy sting?” (Forbes, May 1, 1977.)

A dissenter from this consensus was Milton Friedman. He was far less optimistic. To Friedman, it seemed that a sharp and lasting rise in U.S. inflation was likely to occur in the coming years. In mid-1976, he proclaimed that the decline in the U.S. inflation rate had either come to an end or very shortly would (Instructional Dynamics Economics Cassette Tape 192, June 1976, Part 1). Speaking about six months later, in early December 1976, Friedman predicted that inflation would trough around February 1977 (Instructional Dynamics Economics Cassette Tape 204, December 1976, Part 1).

Friedman went further, making quantitative predictions. In that early December 1976 commentary, for example, he asked, “What will be the rise in the CPI over the next expansion? … [T]he prediction I came out to was an average rate of rise over the next expansion of 7 to 9 percent.” He specified this prediction as pertaining to the period from February 1977 to early or mid-1979. A month later, Friedman observed (San Francisco Examiner, January 5, 1977): “I’m hesitant to give any number [but] the probability is for 7 or 8 percent at the end of 1977 or in 1978.” On television in mid-April 1977, Friedman said of inflation, “I expect it’s going to step up in the next year or two to 7 percent or 8 percent.”

---

Around October 1977, with the accrual of several months’ monetary data having given Friedman the material with which to extend his forecast, Friedman predicted that the overall average rate of inflation from mid-1977 to mid-1979, he suggested, would average 7 to 9 percent. He added that it was “now too late to do much about the rate of inflation over the next couple of years,” as it was going to reflect monetary policy actions that had already been taken.66

Friedman had also specifically indicated that a return of inflation to the double-digit range was in prospect for 1979 and 1980 (Newsweek, October 3, 1977).

In February 1978, Friedman predicted that the peak of inflation in 1979 or 1980 could well exceed 12 percent and would come in the wake of rates of 7 to 9 percent in 1978.67 Friedman elaborated in April 1978 that double-digit M2 growth during 1977 had led him to judge that “inflation from February 1977 to October 1979 will average something like 7 to 10 percent… [and] no sustained reduction in inflation can be expected before mid- or late 1979.” (Newsweek, April 24, 1978.). The following month, in an appearance at the Economic Club of Detroit, Friedman made other specific predictions. The United Press International report on this speech opened by observing: “Economist Milton Friedman says the U.S. economy will fall into a major recession by 1980 after a period of double-digit inflation next year.” The article quoted him specifically saying that inflation “probably will be eight or nine percent by the end of the year—and may be into double digits in 1979” (Kenosha News (Wisconsin), May 19, 1978).

And Friedman would be vindicated. These various predictions—which differed in the vantage point at which they were made, numerical specificity of the forecast, and the time frame to which the forecast referred—would have in common the property that they proved accurate. Friedman’s pessimistic picture of how inflation would evolve over the rest of the 1970s would indeed be realized.

Having crossed above 6 percent in February 1977, the twelve-month CPI inflation rate did not go below 6 percent again until August 1982. In May 1978, it moved above 7 percent, in September 1978 above 8 percent, at the end of 1978 it hit 9 percent, and during 1979 it passed through the 10, 11, and 12 percent barriers, moving above 13 percent at the end of the decade. In terms of averages, Friedman’s prediction of 7 to 9 percent inflation, which Friedman had predicted first for February 1977 to mid-1979 and later for 1978 alone, was met for both periods: the average

---

66 Friedman (1977a, p. 23).
quarterly annualized inflation rate was 8.6 percent for 1977:Q1–1979:Q2 and 8.9 percent for 1978. In contrast, as 1978 approached professional forecasters’ consensus prediction for that year’s inflation rate was about 6 percent (U.S. News and World Report, August 7, 1978, p. 17; Wall Street Journal, October 1, 1979).

Long-term interest rates

Friedman’s accurate prediction that the late 1970s would see a sharp rise in inflation and, indeed, a renewed period of double-digit inflation contrasted sharply with the assessments of inflation prospects underlying trading in the bond market. In the final quarter of 1976, the ten-year Treasury bond rate averaged 7.2 percent. Financial commentator William Hummer contended during this period (Bankers Monthly, November 15, 1976, p. 2) that “medium-term and long-term yields could stay in their current range or decline a bit further.” Several months later, Hummer’s forecast seemed to have panned out: he observed (Bankers Monthly, July 15, 1977, p. 2) that “as summer arrived, bonds were trading near their highest price levels in 3¼ years…” The inflation expectations embedded in these bond rates were correspondingly placid, being in mid-single-digits (see Levin and Taylor, 2013, p. 222). Therefore, the behavior of longer-term Treasury securities reinforced the consensus that what inflation trouble lay ahead would be considerably less severe than that seen earlier in the decade.

Friedman observed in late 1976 (Instructional Dynamics Economics Cassette Tape 204, December 1976, Part 1): “To stick my neck out, I find it hard to believe that long-term bond yields will not rise this coming year, and yet, as I say, I was wrong on that a year ago and maybe I’m wrong now.” Longer-term interest rates were indeed about 80 basis points higher in December 1977 than they were in December 1976—but this increase was concentrated in the final quarter of the year. In fact, before October 1977, every month of the year had recorded a bond rate lower on average than in the corresponding month in 1976. As he explained in his December 1976 commentary, Friedman’s basis for believing that rates would increase had been that the late-1976 values were justified only if the inflation rate in the next several years stayed around 4 to 6 percent, while “monetary growth for the past few years does not justify a rate anything like that low.”

Yet even with the late-1977 increases in the bond rate, it is clear that the markets did expect inflation far below that actually seen in the late 1970s. The monthly average for the rate on ten-year Treasury bonds did not cross 8 percent until February 1978 and did not pass 9 percent until December 1978. See Figure 1.
Figure 1. Ten-year Treasury bond rate and twelve-month CPI inflation rate, January 1973 to December 1979. Source: Federal Reserve Bank of St. Louis’ FRED portal.

*The monetary basis for Friedman’s predictions*

Why did Friedman forecast inflation so differently from (and, as it turned out, better than) others in these years? The answer is that the behavior of M2 growth over this period provided a good guide to future inflation developments, at a time when other approaches fell short. He traced the second round of double-digit inflation, that in 1979–1980, to the second monetary explosion of 1976–1977.

As far as Friedman was concerned, the path of future inflation was provided by the pattern of monetary growth, and by the roughly two-year lag from monetary growth to inflation that he had found to characterize the historical U.S. data. As was discussed in Chapter 4, after being surprised by how restrained monetary growth was over 1975, Friedman had sounded the alarm in 1976 about a surge in monetary growth. Late in that year, he was drawing parallels between the high monetary growth in 1976 and the rapid rate of expansion in the early 1970s that had preceded the 1973–1974 outbreak of U.S. inflation (Instructional Dynamics Economics Cassette Tape 190, May 1976, Part 1). When high monetary growth continued, Friedman was prompted
to observe at the start of 1977 that there had been “unduly expansive [monetary] policy for the last six months” (San Francisco Examiner, January 5, 1977).

The evolution of the second monetary explosion in the 1970s is depicted in Table 1, both for the old definition of M2 used in the 1970s and the modern M2 definition adopted in the United States in 1980.

<table>
<thead>
<tr>
<th>Quarter of year</th>
<th>Q1</th>
<th>Q2</th>
<th>Q3</th>
<th>Q4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Year</td>
<td>Old M2</td>
<td>New M2</td>
<td>Old M2</td>
<td>New M2</td>
</tr>
<tr>
<td>1976</td>
<td>10.5</td>
<td>13.0</td>
<td>10.0</td>
<td>12.7</td>
</tr>
<tr>
<td></td>
<td>[13.3]</td>
<td>[12.6]</td>
<td>[11.1]</td>
<td>[15.3]</td>
</tr>
<tr>
<td>1977</td>
<td>10.9</td>
<td>13.7</td>
<td>9.0</td>
<td>11.2</td>
</tr>
<tr>
<td></td>
<td>[14.3]</td>
<td>[11.2]</td>
<td>[9.5]</td>
<td>[8.5]</td>
</tr>
<tr>
<td>1978</td>
<td>7.0</td>
<td>7.5</td>
<td>8.4</td>
<td>7.5</td>
</tr>
<tr>
<td></td>
<td>[7.7]</td>
<td>[7.7]</td>
<td>[7.7]</td>
<td>[7.7]</td>
</tr>
</tbody>
</table>

Source: Simpson (1980, Table A2, p. 113) for both old and new M2. The square brackets give calculations using FRED data on new M2.

It was on the basis of this monetary explosion that Friedman made his prediction of a renewed, late-decade surge in U.S. inflation.68 Friedman’s initial predictions in 1976 and 1977 of inflation of 7 to 9 percent in the years ahead were evidently arrived at by from subtracting about 3½ or 4 percent average real growth from the 10.5 to 12 percent rate of monetary growth prevailing since mid-1975. But as, during late 1977 and the first part of 1978, he moved into predictions for 1979, he made a somewhat less mechanical adjustment. He reasoned that the inflation of the 1970s had imparted a slight upward trend into M2 velocity, so that a percentage point or two had to be added to inflation projections that were based on (old) M2 growth (Newsweek, August 20, 1979; Charles H. Brunie, interview, July 15, 2013). That factor partly accounted for why, in early 1978, Friedman was offering a 12 percent inflation prediction for mid-1979—even though he was not predicting still-higher monetary growth than that observed so far.

68 Other judgments on this period that have stressed 1976 as the year in which the resurgence of U.S. inflation originated included Brunner (1980, p. 26), who contended that the Federal Reserve launched new round of inflation via its actions in 1976, and Romer and Romer (2004, p. 140), who saw monetary policy in the late Burns period as very loose: they argued, on the basis of the behavior of real short-term interest rates, that “at the end of 1976, policy became dramatically more expansionary.”
Continuing along this line of argument, Friedman predicted at a briefing to the Oppenheimer and Company financial group in mid-1978 that inflation would likely peak in the fourth quarter of 1979, and that “it would be a miracle if inflation peaked below 10 percent, and 10%–12% or 10%–13% would be more likely” (quoted in Wall Street Journal, October 1, 1979).

Friedman was on fairly solid ground predicting an extra couple of points of inflation over that suggested by the patterns of M2 growth. From his participation in the Bach Committee (see Chapter 4) and his related deliberations with the Federal Reserve, Friedman knew that the traditional, commercial-bank-centered M2 definition was on the way out and would be replaced by a broader M2 series that included thrift institutions’ deposit liabilities. Although that new M2 series was not yet published in 1978, its behavior could largely be gleaned from the behavior of what in 1978 was officially called M3 (and was published by the Federal Reserve Board). Friedman would therefore have known, from M3’s behavior, that the new M2 concept had consistently been growing a couple of points more per year in 1976 and 1977 than had the old (and, before 1980, still official) M2 series. Until the most recent years, a mean-reverting velocity had been a good approximation for the old M2 aggregate, so monetary growth (minus output growth) and inflation behaved similarly, on average: see Chapter 2 above, as well as Friedman’s observations in Newsweek, April 24, 1978. It could be conjectured that the new M2 series would inherit this historical property of a roughly stable velocity. That being the case, it followed that old M2 velocity had acquired an upward trend of 2 percentage points or so. Consequently, a reduced-form approach of the kind Friedman used, based on past monetary growth and a simple velocity assumption, Friedman had been able to forecast successfully the United States’ second bout of double-digit inflation in 1979–1980.

*Cost-push views in officialdom*

“What’s needed is not the knowledge; it’s the will.” With this statement in February 1978 Friedman displayed his tendency, already mentioned in this chapter, to view policymakers as sharing his own, monetary, perspective on inflation’s causes. Starting from this premise of a common economic model with policymakers, he attributed the monetary authorities’ generation of inflation, and their failure, so far, to stick to plans to disinflate, to a lack of resolve and to

---

69 It was subsequently shown to possess this property (specifically, stationary behavior) by Engle and Granger (1987), Small and Porter (1989), Rasche (1990), and Hallman, Porter, and Small (1991).

70 This upward trend could be interpreted as a shift by households away from holding bank deposits in the wake of the higher inflation of the 1970s. The fact that new M2 velocity was stationary could then be interpreted as reflecting the fact that the flight away from bank deposits was in large part toward thrift accounts, as well as to the money-fund accounts (included in the modern M2).

acquiescence to outside pressures. However, for the late Burns years of 1976 to 1978, as for the prior six years, the evidence is strong that Friedman was incorrect: lack of knowledge was a problem, after all. In particular, Burns continued to adhere to a nonmonetary view of inflation and, insofar as he thought that monetary policy mattered for inflation, he mistakenly believed that the Federal Open Market Committee (FOMC) was providing anti-inflationary pressure.

An indication that these were Burns’ positions is provided by the transcript of the June 1977 FOMC meeting, which includes an exchange between Burns and the president of the Federal Reserve Bank of Boston, Frank Morris. The subject of the exchange was the long-term bond rates. As discussed above, bond rates were quite low. By way of explanation for this state of affairs, Morris remarked: “I think there is an expectation that we are not going to have a sharply rising trend in interest rates. That also shows up in the Treasury bill futures markets. The Treasury bill futures yields are going down, reflecting an anticipation of that.” Chairman Burns asked Morris: “You think this behavior of long-term interest rates reflects, in part, confidence in the Federal Reserve’s policies?” Morris replied: “I think so, yes.” “Well, that’s been my own judgment, but it’s a hard thing to be sure of,” Burns concluded.72 Although Burns was cautious in his interpretation of what was driving bond rates, he clearly believed that it was appropriate to be confident that FOMC policy was noninflationary.

Potential problems on the inflation front, as Burns saw it, arose from elements other than in the monetary sphere. In fact, although Burns was at odds publicly with the Carter administration over much of 1977, an emphasis on cost-push factors was common ground between himself and the Carter administration team. Meltzer (2009b, p. 919) suggested the contrary, by juxtaposing the views of Carter’s advisers against Burns’ statement (on July 29, 1977) that inflation cannot go on without “monetary nourishment.”73 Burns’ full remark, however, was: “For our part, we at the Federal Reserve know that inflation ultimately cannot proceed without monetary nourishment.” This was an acknowledgment that monetary restraint was necessary for long-run price stability, but it did not imply that such restraint was sufficient for obtaining this condition, and it was consistent with nonmonetary factors impeding the achievement of price stability.74

---

73 Meltzer cited this statement as being from the “Carter papers, Jimmy Carter Library.” Actually, there is nothing unique to the Jimmy Carter Library when it comes to the availability of this statement. It was from Burns’ opening testimony at a Congressional hearing and was printed in the Federal Reserve Bulletin at the time. For this published version of the passage in question, see Burns (1977a, p. 728).
74 More specifically, if cost-push factors had a positive mean in the long run, as adherents to cost-push views of inflation maintained, monetary growth rates that would otherwise be consistent with price stability would be associated with continuing inflation.
Furthermore, the statement was predicated on the—in retrospect, badly-informed—notion that monetary policy was already, in 1977, contributing to the reduction of inflation.

In line with his cost-push perspective, Burns regarded the revival of inflation in 1977 as occurring despite a continuing considerable amount of the slack in the economy (see Nelson, 2005, and DiCecio and Nelson, 2013, pp. 406, 411). Impressed by the speed of the national economic expansion in the late-1976/early-1977 period, Burns had opposed Carter’s tax-rebate idea (Los Angeles Times, February 4, 1977; Daily News (New York), March 23, 1977). But, as noted, he had signed up to a 6 percent real growth for 1977, and he had endorsed the administration’s proposal to bring the unemployment rate down from by about 1.5 percentage points from the end of 1976 to the end of 1977. Viewed in this light and in conjunction with Burns’ other statements, the concern that Burns voiced during 1977 about stimulus boosting the inflation rate indicated a speed-limit view of inflation dynamics—and did not signify agreement with Friedman’s judgment that the United States was poised to cross into excess demand territory (see DiCecio and Nelson, 2013, pp. 410–412).

*Phillips-curve approaches*

It was noted in Section I that, by the late 1970s, the Friedman-Phelps work on the Phillips curve had been accepted by many economists. These economists eschewed pure cost-push views of price behavior and instead saw inflation in terms of an expectational Phillips curve relationship, with a sizable response of inflation to the output gap. Those economists who did model inflation using such a Phillips curve were capturing in their projections and policy analysis the aggregate-demand-to-inflation connection that Friedman was using in the reduced-form, monetary-growth-based, inflation forecasts he gave during 1977 and 1978. In principle, therefore, they could, like him, have predicted the double-digit inflation of the end of the decade. In practice, this did not happen.

A key reason for this, discussed further below, is that, in the late 1970s, output-gap estimates continued to be heavily mismeasured—rendering unreliable inflation projections that were based

---

75 In addition, like Friedman, Burns approvingly quoted U.K. Prime Minister Callaghan’s criticism of past inflationary policies. He did so, for example, at an FOMC meeting held a couple of weeks after Friedman quoted in his column (see Federal Open Market Committee, 1976, p. 40). The context in which Burns recited this quotation suggested that he felt that monetary policy was exercising restraint and that the fault in current stabilization policy lay in the danger was that fiscal policy would become overstimulative. Similarly, Burns (1977c, p. 4) portrayed Callaghan’s statement as an indictment of past approaches—“policies for stimulating employment on which we have relied in the past”—from which he distinguished the modern-day formulation monetary policy in the United States and elsewhere.
on the output gap. Whereas a cost-push approach would be in error by giving a zero weight to the output gap when a sizable weight was warranted, a Phillips-curve approach might appropriately give a sizable weight to the output gap; but, in the late 1970s, such an approach might use an output-gap estimate that suggested a deeply-negative gap, when it was actually close to zero or even positive. That this was not merely a hypothetical problem in the late 1970s is indicated by Orphanides’ (2003, p. 645) Figure 2, in which an official output-gap estimate of the time is always negative in 1977 and 1978, while the revised “final” output gap is nearly always positive in those years.

But in principle, as stressed in earlier chapters, take-offs in inflation such as that observed in 1977 could have led modelers to revise their output-gap estimates promptly and get their inflation projections back on track. However, this process did not occur. The flawed output-gap estimates were, in 1977 and 1978, largely retained. The rise in inflation was, for the moment, primarily attributed to temporary factors; and the chance to project accurately the double-digit inflation of 1979 was missed.

Indeed, even Rudiger Dornbusch who—as described in Section I of this chapter—had done much, with Stanley Fischer, to integrate monetarist ideas about economic behavior into mainstream economics teaching, was inclined to view the output gap as sizable, and monetary ease as appropriate in 1977 and 1978. In Congressional testimony of March 7, 1978, Dornbusch stated: “The first priority then should be continued expansion in aggregate demand.”76 To this end, he called for reductions in interest rates: “Interest rates should not be allowed to rise further and, indeed, a rollback is desirable.”77 Dornbusch saw the stimulus he recommended, along with other measures he was urging, as consistent with keeping inflation at 6 percent.78

Dornbusch and others—including presumably the bond traders who set their years-ahead inflation projections so low during 1977 and into 1978—may have been misled into thinking that inflation in 1977 was overstated by the cold winter weather of early 1977 and by the year’s exchange-rate decline, rather than perceiving that the rise in inflation was overwhelmingly due to overly-easy monetary policy.

76 Committee on Banking, Finance, and Urban Affairs, House of Representatives (1978b, p. 42).
77 From Dornbusch’s written submission in Committee on Banking, Finance, and Urban Affairs, House of Representatives (1978b, p. 53).
A fundamental reassessment among policymakers, commentators, and market participants of output-gap estimates and inflation expectations would not occur until 1979. With this reassessment would occur a rethinking of whether monetary policy was, as Friedman had insisted, been much too loose in 1976–1978. Dornbusch himself was in the vanguard of this rethinking. An early glimpse into his revised thinking came in the fact that, in contrast to Dornbusch’s 1978 assessment that interest rates had risen too much already, Dornbusch and Fischer (1979, p. 5) would observe that, during the economic recovery that had started in 1975, interest rates had taken a long time, by historical standards, to pick up. Later, Dornbusch (1993, p. 332), in looking at the double-digit inflation of 1979, observed, “Arthur Burns… was responsible for that situation.” In so doing, Dornbusch lined himself up with the Friedman interpretation that the double-digit inflation in 1979 originated in overly-loose monetary policy in the late Burns years.79

It is not clear that Phillips-curve-based forecasts, even when using revised data, would have done quite as well as Friedman did in predicting the double-digit inflation of 1979. For example, Stock and Watson’s (1999, p. 323) Figure 4 suggested that such projections implied that inflation would be well below 10 percent in 1979 and into 1980. This miss perhaps reflects intractable problems with measuring the output gap or the unemployment-rate gap and in pinning down those gaps’ dynamic relationship with inflation. Certainly, however, Philips curves using the revised gap series would have accurately predicted a sharp rise in inflation.80 Once again, therefore, the theme emerges of major problems with measuring the output gap—and how Friedman was able to insulate his predictions from these problems.

Output-gap estimates and official views

By 1977, there was considerable realization that the output gap had been overestimated in the past. In the case of the United Kingdom, for example, Laidler (1976c, p. 87) observed that “the use of macroeconomic policies to maintain levels of employment too high to be viable in the long run has been an important source of inflationary pressure.” What was less appreciated, in both the United Kingdom and the United States, was that such overestimation remained a major practical problem in the late 1970s.

---

79 A drastic reassessment of the appropriate posture of monetary policy in the late 1970s would also be evident in Dornbusch’s analysis of the United Kingdom. Whereas Dornbusch and Fischer (1980, p. 25) estimated that the U.K. output gap was −9.6 percent in 1977, Dornbusch would later assess that the U.K. output gap was zero in that year (Financial Times (London), July 24, 1985).
80 Reflecting this, the simplest accelerationist Phillips curves do well in explaining U.S. inflation’s behavior during the 1970s (see the references discussed in Chapter 4).
Instead, as of 1977, it seemed that the authorities had now put their house in order and appropriately revised their estimates of potential output. The Carter Administration’s ambitious economic growth target for 1977, and the perception of a large amount of slack that motivated the target, might have been even stronger, had not estimates of potential output not been substantially revised down. Downward adjustments to estimates of potential GNP had led the Council of Economic Advisers in January 1977 to issue revised estimates of the output gap in recent years, with the result that the output gap during the trough in 1975 was reduced (that is, made less negative) by about 4 percentage points (Orphanides, 2003, p. 655). Likewise, the Federal Reserve Board in early 1977 substantially lowered the estimate of potential production that underlay its estimates of capacity utilization (Financial Times (London), January 14, 1977). It turned out, however, that the scale of the change in the aggregate-supply picture that had occurred in recent years was so great that, even with these revisions, the economy’s sustainable-output path remained substantially overestimated.

A crucial problem here was that, as yet unbeknownst to policymakers, from 1973 onward the potential growth rate of the economy had dropped from close to 4 percent to about 2.5 percent. In contrast, policymakers in 1977 continued to work on the assumption of trend growth close to its pre-1973 rate: Arthur Burns, for example, during 1977 gave potential output growth as 3.5 percent (Burns, 1977b, p. 361). The belated recognition of this productivity-growth slowdown is considered further in Chapters 8 and 9.

The difficulties in estimating the output gap arising from the changing potential-growth picture were compounded by large revisions to actual output data. Another factor that continued to cloud the picture of the true output gap was, of course, the rise during the 1970s in the natural rate of unemployment. This matter is discussed further in the discussion titled “Franco Modigliani” in Section III below.

The combination of inaccurate initial output data and inadequately-revised potential-output estimates meant that output-gap mismeasurement continued to be a serious problem in 1977. Though at the time perceived as being about 7 percent (Orphanides, 2003, p. 645), modern estimates of the output gap in 1977:Q1, Carter’s first quarter in office, suggest that output was less than 2 percent below potential by that point. Furthermore, the gap was narrowing rapidly, and it was positive in each quarter from 1978:Q2 through the end of the decade.

---

81 See Orphanides (2003), as well as the discussion in Chapter 4 above of the revisions to the decline in output during the 1973–1975 recession.
In marked contrast to these subsequent assessments, James Tobin, in his April 1977 television debate with Friedman, had declared that the United States had inflation “not because there’s too much money chasing too few goods. Quite the contrary. We have excess capacity, and a large amount of unemployment.” Similarly, in officialdom, Carter’s Chairman of the Council of Economic Advisers, Charles Schultz, remarked in September 1977: “Ample resources are available to permit further expansion…” (Daily News (New York), September 14, 1977.) In the same month, the Federal Reserve Board’s Governor Charles Partee testified that “sizable unused resources exist in this and other economies” (Partee, 1977, p. 891; also quoted in American Banker, October 3, 1977, p. 15).

As stressed in Chapter 4, Friedman was not ahead of the pack in recognizing the shift in the behavior of potential output. In 1978, for example, his estimate of noninflationary monetary growth seemed to be predicated on potential growth of about 4 percent (Newsweek, April 24, 1978). Only in 1979 did he seem to embrace a potential growth rate of about 3 percent (American Banker, June 12, 1979, p. 3)—by which time the Carter Administration had brought down its official estimates of potential growth to such a rate. Nor did Friedman and others on his side of the debate on stabilization policy show much sign of detecting how badly the output statistics themselves had been underestimated: Lucas and Sargent (1978, p. 49), for example, referred to the United States as having had a “depression” in 1973–1975. Friedman, as we have seen in Chapter 4, did sound warnings early on about the rise in the full-employment unemployment rate, but this was only one factor behind the exaggerated output gap estimates of the 1970s.

The monetarist side nonetheless became well known for eschewing output-gap estimates in their policy prescriptions and macroeconomic analysis (see Wonnacott and Wonnacott, 1979, pp. 331–334, for an early discussion). This characterization was justified, on two dimensions.

First and most obviously, they stressed policies, notably the constant monetary-growth-rule, that did not respond to estimates of the level of the output or unemployment gaps.

The second respect in which monetarists in the late 1970s distanced themselves from output-gap

---

83 The best sign Friedman gave during this period of a recognition of a secular productivity-growth slowdown was to point to the robust growth of employment since 1975 as an indication that the U.S. economy had had a good recovery from the 1973–1975 recession. (Oakland Tribune (California), January 27, 1977).
84 In addition to what follows, see the discussion in Nelson (2018a, Chapter 8).
estimates was that their view of the inflation process suggested a picture of the excess-demand situation that was at variance with the extant output gap estimates. This was a point on which some members of the monetarist school were explicit during 1977 and 1978. The economic analysts at Citibank, for example, noted that official estimates of slack could not produce inflation rates of the kind observed in recent years and concluded (Citibank Monthly Economic Letter, October 1977, p. 4): “This indicates that either there is less slack in the economy than the conventional indexes would have us believe[,] or that the existence of slack has less of a damping influence on the inflation rate than it used to.” The monetarist position was squarely in line with the first of these interpretations, with David Laidler (1978b) challenging a prominent report to the OECD by McCracken and others (1977), which had reported mid-1970s troughs of the output gap as being in double-digit percentages for the United States and other major countries. Laidler pointed out that the inflation of the 1970s suggested a condition of excessive demand—not the chronic excess supply that had been the report’s characterization of the decade. For his part, Friedman’s emphasis on excess demand had been implicit in his predictions from late 1976 of a sharp rise in inflation over the rest of the decade.

Friedman, therefore, by no means denied the output gap/inflation link in his own analysis. On the contrary, the reduced-form monetary-growth/inflation link in his framework arose as an outgrowth of the structural dependence of inflation on excess demand. Consequently, as indicated above, Friedman’s prediction that rapid monetary growth would be followed by inflation embedded the notion that, along the way to producing inflation, monetary ease would generate positive output gaps. But his lack of confidence in estimates of these structural relationships, and in empirical measures of resource gaps, led him to the shortcut of direct monetary-growth/inflation relationships. This reduced-form approach served him especially well during the late 1970s.

Other reduced-form forecasts

We have seen, therefore, that Friedman’s inflation predictions did well because they captured the relationship between aggregate demand pressure and inflation without using unreliable estimates of slack to predict inflation. Could econometric models have done as well as he did, if they had forecast using available macroeconomic data and not used gap estimates? It is difficult to know the answer to this question definitively, as this was a time when other reduced-form approaches to forecasting inflation were hard to come by. Autoregressive integrated moving average (ARIMA), or Box-Jenkins, time-series forecasting methods were available and had become widely used but, by construction, these approaches projected inflation based on its own past
behavior—and so they supported the notion that the moderate inflation pattern of 1976 would continue. The vector autoregression (VAR) approach to forecasting was just getting started: Sims’ (1980) pioneering article on the subject appeared in working-paper form in 1977 (see Sargent, 1979) and applications of the VAR approach were only beginning to appear in print in 1977–1978—for example, Sargent (1978).

One reduced-form approach that likely would have done about as well as Friedman’s forecasts of inflation using monetary growth was not prevalent at the time. This would be to predict inflation using nominal income growth. Friedman took for granted that rapid nominal income growth would occur in the course of monetary ease being transmitted into inflation—and that, with the different lags involved in the reaction of output growth and inflation to monetary policy, nominal income growth would most likely take off ahead of inflation. This regularity certainly was observed again in the late 1970s. Jerry Jordan noted in 1978 (American Banker, October 23, 1978) that nominal income growth in the prior three years had averaged over 11 percent, “the highest rate for any three-year period in over 30 years.” This pattern is confirmed by modern data on nominal GDP growth. A three-year average of annualized quarterly nominal income growth (see Figure 2) shows that, in the late 1970s, aggregate demand growth reached a new plateau that was steeply higher even than the previous high of the mid-1970s. The three-year averaging process also shows also how ephemeral the restrictive policies that led to the 1973–1975 recession proved to be.

Thus the pattern of nominal income growth, like that of M2 growth, provided an early warning of the late 1970s resurgence of inflation. Because nominal income growth typically lagged monetary growth over this period, however, monetary growth, and increases in M2 in particular, provided a signal about the rise in inflation that was ahead of the signal provided by nominal income growth.

In light of episodes like the late 1970s, Gordon (1985, p. 49) offered the generalization: “Inflation in the long run is always and everywhere an adjusted-nominal-GNP phenomenon.” Friedman would largely endorse this notion, himself remarking in 1985, “Inflation tends to depend on the average rate of growth of nominal income… over a considerable period.” However, Friedman regarded tracing inflation to monetary growth as a more fundamental exercise. Furthermore, nominal GDP growth can be very high and not lead to inflation, as

---

85 Gordon defined “adjusted nominal GNP” as nominal GNP growth minus the long-run rate of potential output growth.
86 Friedman (1985, p. 52).
occurred in 1983–1984. In this later period, rapid nominal GDP growth was noninflationary because the economy was absorbing considerable resource slack. That is, the mid-1980s featured a state of the economy that many nonmonetarist economists, largely erroneously, had perceived as prevailing during 1977–1978: rapid growth in aggregate spending that was noninflationary, as it was justified by the amount of slack in the economy.

**Monetary targets and the federal funds rate**

The failure of the Federal Reserve to meet its monetary targets, particularly by the criterion of M2, was what for Friedman lay behind the monetary explosion that had generated faster growth in nominal spending and the revival of inflation in the United States.

In late 1976, Friedman lamented the fact that the Federal Reserve’s announced intention of a gradual slowdown in monetary growth had not been manifested in its actual performance (Instructional Dynamics Economics Cassette Tape 204, December 1976, Part 1). A little later (in Instructional Dynamics Economics Cassette Tape 208, February 1977, Part 1), after noting that Burns had adjusted down the target range, Friedman vented: “But all he’s changing are the targets—not what’s actually happening!” Similarly, Friedman observed late in 1977 that “while the targets are going down, actual monetary growth is going up” (*St. Louis Globe-Democrat*, December 7, 1977). He summed matters up in September 1978 with the observation that the
“targets for monetary growth set by the Fed have consistently declined in the past two years in
order, the Fed says, to foster a gradual reduction in inflation, but actual monetary growth has
proceeded along a rising, not a declining[,] trend.”87

These characterizations were borne out by the fact that the Federal Reserve’s M2 target range
had stepped down from 8½–10½ percent growth in 1975 to 7½–10½ percent in 1976, but the
outcomes over the target periods were 9.6 percent and 10.9 percent, respectively (Argy, Brennan,
and Stevens, 1990, p. 54).88 Monetary growth was then 9.8 percent in 1977.89 Furthermore, the
pattern for M3—which as suggested above, Friedman by this stage likely considered a more
meaningful monetary aggregate, as it signaled the behavior of the in-progress redefined M2
series—was considerably worse: the Federal Reserve overshot its targets in 1975, 1976 and
1977, and monetary growth was above 11.5 percent in every year (Argy, Brennan, and Stevens,
1990, p. 54; Bernanke and Mishkin, 1992, p. 190).90 Monetary growth on the M2 measure did,
however, decline substantially in 1978 (see Table 1 above and the discussion titled “G. William
Miller” in the next section).

Friedman also expressed exasperation at Burns’ statement that monetary growth had not been
excessive in 1976 (Instructional Dynamics Economics Cassette Tape 208, February 1977, Part
1). The FOMC did raise the federal funds rate in 1977, by about 200 basis points in total: it
averaged 4.65 percent in December 1976, 6.56 percent in December 1977. Furthermore,
testifying in mid-year about the rising funds-rate profile, Burns attributed the increase to FOMC
moves to restrain monetary growth. But the Chairman also pointed to the stability of long-term
bond rates as implying that the FOMC tightening had been sufficient (Burns, 1977a, p. 724).
Indeed, the fact that long-term rates were quite low likely reflected not only unrealistically
optimistic inflation expectations on the part of markets, but also downward pressure on real
interest rates from still-easy monetary policy. In 1975, Burns had implied that monetary policy
had a negligible effect on (real and nominal) long-term rates in the short run. By February 1977,
he was willing to concede that long-term rates responded to movements in the federal funds rate

87 Friedman (1978c, p. R–182). In this assessment, “the past two years” may have been a reference to 1976 and
1977.
88 Bernanke and Mishkin (1992, Table 1, p. 190) recorded a similar rise. The data for old M2 in Simpson (1980,
Table A2, p. 113) show a sharper move up, from 8.4 percent in 1975 to 10.9 percent in 1976.
89 See Argy, Brennan, and Stevens (1990, p. 54) and Simpson (1980, Table A2, p. 113). (In Bernanke and Mishkin,
1992, p. 190, the reported M2 growth for 1977 had an evident typographical error, with 9.8 percent being incorrectly
reported as 3.8 percent.)
90 The pattern for M1 behavior differed from that of the broader aggregates. But, from mid-1976, M1, too, was
largely characterized by growth in excess of the FOMC targets (Paulus, 1980, pp. 103–104). Indeed, during 1977,
when M2 growth was high but formally within the Federal Reserve’s generous target range, M1 growth was in
excess of its own target band (American Banker, October 3, 1977, p. 3).
“to a degree” (*American Banker*, February 10, 1977), but even in the FOMC meeting the following month, Burns insisted “there’s only one interest rate over which we have a high degree of influence, and that is the federal funds rate.”

A different outlook on the Federal Reserve’s short-run influence on interest rates might have led Burns to view low long-term rates in mid-1977 as a symptom of monetary policy being too loose.

The failure of the monetary-growth targets to be met, together with the Federal Reserve’s continuation of an operating procedure centered on setting the federal funds rate, led Friedman to conclude in September 1978 that “the major effect of monetarism has been on the language in which policy is expressed rather than on the content.”

He would have been reinforced in this view by the observation of the aforementioned Federal Reserve Bank of Boston’s president, Frank Morris, a couple of months earlier (*Wall Street Journal*, July 17, 1978, p. 1): “Most of us have learned a great deal from Friedman and the monetarists, but few of us accept the entire body of monetarist doctrine. I believe that monetarism may have peaked and that it now may be on the wane.”

*Monetary targeting and the law*

Notwithstanding his dissatisfaction with Federal Reserve policy over this period, Friedman could at least point to some further legislative successes for his own approach to monetary policy. The requirement that the Federal Reserve state and pursue monetary growth targets, already initiated by Congress in 1975, was put on more formal footing with the passage of an amendment to the Federal Reserve Act, signed by President Carter in November 1977. Friedman later noted that this legislative move had, in essence, made a monetary-growth rule part of United States law.

As Friedman acknowledged, however, the new statute essentially repeated the wording of Congress’ 1975 reporting requirements for the Federal Reserve’s monetary targets. It followed that the 1977 legislation amounted to a continuation of arrangements that Friedman had already concluded had not led to a material change in the substance of U.S. monetary policy. Influenced by the analysis of a former student, Robert Weintraub, who worked as a staffer for the

---

91 From Burns’ remarks at the FOMC meeting of March 15, 1977 (Federal Open Market Committee, 1977b, p. 31).
93 The legislation, as signed and passed, is available at [https://fraser.stlouisfed.org/title/1040](https://fraser.stlouisfed.org/title/1040). In the legislation’s text, the amendments to the Federal Reserve Act it contained were separately and officially termed the Federal Reserve Reform Act, and that terminology would later be used by Federal Reserve Chairs (see, for example, Miller, 1978b, p. 185 [p. 1 of typescript version]; Bernanke, 2013, p. 7).
94 See Friedman (1995, p. 175). Friedman also briefly referred to this 1977 legislation in Friedman (1982a, p. 107) and Friedman and Friedman (1985, p. 162).
Congressional supervisors of the Federal Reserve, Friedman became persuaded that nothing
short of a constitutional amendment imposing a monetary rule was needed to make the rule
stick. This idea was in harmony with Friedman’s strong, if notably uncritical, interest in the
public-choice literature. The idea led to the Friedmans’ drastic, and hardly practical, proposal in
1980 (in the book version of Free To Choose) of a constitutional amendment that would
consecrate a constant-monetary-growth rule for the United States—one that would apply,
initially at least, to the monetary base.

Other Congressional initiatives

Another legislative change in the 1977–1978 period that had a bearing on the Federal Reserve’s
targets was the Full Employment and Balanced Growth of 1978, also known as the Humphrey-
Hawkins Act, which became U.S. law in late October 1978. The Humphrey-Hawkins
legislation further formalized the reporting process for the Federal Reserve’s monetary targeting;
and it made the announced target ranges pertain to the whole year, rather than being routinely
readjustable at every FOMC meeting.

Other aspects of the legislation also had implications for monetary policy. But exactly what
these implications were was a matter of differing interpretation, both at the time and
subsequently. The Humphrey-Hawkins Act established goals for the federal government for full
employment. In itself, this was not a new statutory requirement for either the federal
government generally or the Federal Reserve specifically. The latter institution already had an
assigned goal of maximum employment alongside a price-stability goal. The act, as passed,
was merely a statement of goals (and, in view of existing legislation on the books, really just a

95 See Friedman’s (1995, p. 175) discussion, which cited Weintraub (1978) in support.
96 See Friedman and Friedman (1980, p. 308) as well as Friedman and Friedman (1985, p. 99). (On Friedman’s
growing interest in monetary-base-oriented rules during the 1980s, see Chapter 10 below.) Friedman (1985, p. 61)
himself acknowledged that a constitutional amendment to impose constant monetary growth was “highly unlikely.”
97 The bill was passed in its final form by Congress on October 15, 1978, and approved by President Carter on
October 27, 1978 (see https://fraser.stlouisfed.org/title/1034 and https://www.presidency.ucsb.edu/documents/acts-
approved-the-president-week-ending-friday-96). DeLong (1997, p. 271) gave a date of 1977 as when the act was
passed and signed; this date was not correct.
98 See Chapter 4 above, as well as Paulus (1980, p. 103) and G. William Miller’s testimony of February 21, 1979, in
Committee on Banking, Finance, and Urban Affairs, House of Representatives (1979, p. 8).
99 It did so through the aforementioned amendment to the Federal Reserve Act in 1977: see, for example, Bernanke
(2006b). (However, the implication in the same Bernanke speech that the Federal Reserve did not have a legislated
price-stability goal until 1977 is at variance with how many pre-1977 policymakers, as well as numerous
commentators including Friedman, interpreted existing statutory goals. For a discussion, see Nelson, 2018b,
Chapter 12.)
reaffirmation of goals). However, it did so with some numerical specificity, however, calling for a 4 percent unemployment rate to be achieved by the 1980s (as well as inflation below 3 percent). In view of this, some observers, both then and later, saw the Humphrey-Hawkins Act as setting employment goals inconsistent with price stability. DeLong (1997, p. 271), for example, viewed the Act as symptomatic of the policies that produced the 1970s inflation and as helping to encourage the latter (a characterization somewhat difficult to square with the fact that, as discussed in the next section and in Chapter 8, monetary policy tightened in both 1978 and 1979).

Closer to the time of the law’s passing, Blinder (1983, p. 70) had reached a similar conclusion to DeLong. Blinder judged that the “Humphrey-Hawkins type definition of high employment” involved an unemployment goal far below the natural rate and so would be inflationary if used as the basis for monetary and fiscal policy settings. Later, however (and when he was himself a policymaker), Blinder (1994, p. 336), in essence, withdrew this criticism. He instead cast U.S. law as specifying an economic-stabilization goal for the Federal Reserve—but one that did not oblige the authorities to set a target for unemployment that was below the natural or full-employment rate of unemployment.

In the event, the Federal Reserve’s own posture over the years from 1977–1978 onward was similar to that of Blinder (1994). The interpretation on which FOMC policymakers put the Humphrey-Hawkins and the other laws giving it an employment mandate was that they set an unemployment-rate goal but one that should evolve as policymakers’ estimates of the natural rate improved. In addition, both in the years leading up to the act and after its passing, the Federal Reserve leadership was vocal on the point that the full-employment unemployment rate was higher than it had been in the past.

---

100 DeLong (1997, p. 271) incorrectly took the act as introducing a dual mandate of price stability and full employment for the first time.


102 The other laws included the Employment Act of 1946 and the aforementioned Federal Reform Act of 1977, both of which specified a “maximum employment” objective. On the fact that the maximum-employment remit has been interpreted by officialdom as referring to a long-run sustainable employment concept, see, for example, Blinder (1997, p. 4). Blinder added that “the FOMC has never officially adopted the natural rate Phillips curve as part of its intellectual framework.” This situation changed in 2012. However, even in a framework not based on the natural rate hypothesis, policymakers may regard it as desirable, on price-stability and other grounds, to avoid economic overheating and may interpret maximum-employment goal as something below a physical maximum. These positions were in fact those that prevailed in policy circles in the United States throughout most of the postwar period. For example, Karl Bopp, Federal Reserve Bank of Philadelphia, cautioned against “policies of overfull employment and inflation” and saw monetary policy as appropriately directed at promoting “maximum sustainable use of available resources” (Bopp, 1970, pp. 15, 16).

103 For example, like Friedman and others, both Chairman Burns and his successor cited higher unemployment benefits as a factor pushing up the full-employment unemployment rate. Burns’ own way of articulating this point
nonmonetary policies should be responsible for lowering that rate and that it was not appropriate for monetary policy to try to attain below-normal unemployment rates.

For his part, Friedman had vehemently opposed early versions of the Humphrey-Hawkins bill. These drafts had actually required the federal government to engage in large-scale public-sector hiring, in the event of the unemployment rate reaching values perceived as excessive, in order to attain the bill’s specified unemployment target. Friedman had spoken out strongly against this measure in his Newsweek columns—both in 1974, when it was being mooted in Congress, and in 1976, when it was specifically associated with the Humphrey-Hawkins bill (Newsweek, September 2, 1974; Newsweek, August 2, 1976).

Friedman’s Newsweek analyses considered two cases. In the first case, the extra public expenditure involved in the new federal hiring would not be monetized by the Federal Reserve. In such an instance, the hiring “would not reduce unemployment but simply add to government employment,” a circumstance that would make “us all poorer” because the public employment would direct resources less efficiently than the market system (Newsweek, August 2, 1976). Contentions that public hiring added to overall output and employment, Friedman suggested in his 1974 column, were based on a “neglect of indirect effects” (Newsweek, September 2, 1974). His 1976 column had zeroed in on the “folly” in the economic analysis guiding Senator Hubert Humphrey. Humphrey—who, during his spell away from the U.S. Senate in the 1960s, had been on the 1964 and 1968 presidential tickets, and so had been on the opposite side to Friedman in those campaigns—was moved to reply in Newsweek’s letters pages to Friedman’s 1976 column. In that response (Newsweek, October 11, 1976, p. 8), Humphrey cited the existence of economic slack and implied that the slack would not be absorbed unless the public sector grew: “The more than 7 million workers now unemployed attest to the failure of the private economy to absorb these workers.” This reply would have grated with Friedman both in its implication that measured unemployment invariably recorded economic slack and that additions to government spending, for an unchanged monetary policy, provided a means of adding to overall economic activity.

The second case considered in Friedman’s two Newsweek columns was that in which the fiscal expansion associated with public-employment measures was accommodated by Federal Reserve...
monetary easing. Friedman added that, in this case, new government demand would not be completely at the expense of “private demand[,] and so could create new jobs” (*Newsweek*, September 2, 1974). But he stressed that the end result would be to add to inflation, not to real economic activity.

Although Humphrey’s bill contained price-stability as well as employment goals, and in his 1976 reply to Friedman he had affirmed that the “choice is not… between full employment and inflation,” it was clear that Humphrey did not share Friedman’s perspective on the demand/inflation linkage. Rather, in Humphrey’s assessment (*Family Weekly*, July 3, 1977): “In recent years inflation has been the result of OPEC pricing policy, poor harvests, underutilization of machinery and equipment, high interest rates[,] and noncompetitive pricing by big business.” Like so many who took a nonmonetary perspective on inflation, Humphrey saw stagflation as a vindication of cost-push views: “we have seen that as unemployment went up so did prices, and as inflation came down [after 1974] so did unemployment” (*Family Weekly*, July 3, 1977).

However, the version of the Humphrey-Hawkins bill that went into law—nine months after Humphrey’s death—was considerably watered-down and lacked the public-employment provisions that Friedman had criticized. Friedman himself quickly became reconciled to the Humphrey-Hawkins Act, devoting a *Newsweek* column to the subject of “Implementing Humphrey-Hawkins” (*Newsweek*, March 5, 1979). In his 1976 rebuttal, Humphrey had complained: “Nowhere in his article does Professor Friedman even indicate that he supports full employment as a goal.” In fact, Friedman had a lengthy record of endorsing full employment as a policy goal (see Nelson, 2008; 2018a, Chapter 8). His objection was instead to activist full-employment policies, which he proposed to replace with rules that would promote conditions of full employment without targeting output or the unemployment rate directly.

In his 1979 column, Friedman squared the low numerical unemployment target value in the new law with his own natural-rate framework by treating Humphrey-Hawkins as an injunction to lower the unemployment rate through improvement of the supply side of the economy, alongside fiscal and monetary rules that “would provide a more stable economic environment” (*Newsweek*, March 5, 1979).

*Federal Reserve transcripts*

Still another area of monetary policy in which legislators were active in the later 1970s was that of Federal Open Market Committee policy communications.
As of the mid-1970s, the Federal Reserve adhered to the following practice with regard to FOMC meetings: In addition to regularly releasing, after a multi-week interval, minutes of the monthly meetings, it published transcript-like lengthy records (called the Memoranda of Discussion) of the meetings.\(^{104}\) During the Burns era, these transcript documents were issued with a roughly five-year lag (so, for example, those for the year 1966 were available by early 1972).\(^{105}\)

In April 1975, however, Representative Wright Patman—who, the previous January, had been dislodged by Henry Reuss as chair of the House of Representatives’ Committee on Banking, Currency, and Housing, but who still possessed oversight powers as chair of its subcommittee on monetary policy—formally requested that Chairman Burns hand over the as-yet-unreleased transcripts for meetings from 1971 to 1974.\(^{106}\) Burns declined, on the grounds that a multi-year lag in the release of the transcripts was needed to foster a mood of candor and give-and-take in FOMC meetings (see Lindsey, 2003, pp. xii–xvii).

In the face of such Congressional pressure for early release of the transcripts, as well as a separate move being made in the courts to compel expedited release of a variety of FOMC documents (such as the meeting minutes), the FOMC announced in March 1976 that it would stop preparing the Memoranda of Discussion (Lindsey, 2003, p. xi).\(^{107}\) By making this move, it seemingly signified that it would not keep transcripts or audio recordings for its meetings from mid-1976 onward (\textit{Wall Street Journal}, December 20, 1993; Lindsey, 2003, pp. 8–10).

It was against that background that, in 1976–1977, Stephen Neal, the new chair of the U.S. House of Representatives’ subcommittee on monetary policy (Patman had died in March 1976), solicited responses from experts, and held hearings, on the subject of maintaining and making public the FOMC meeting transcripts.\(^{108}\) Friedman’s own contribution was a letter to Neal dated

\(^{104}\) These publications described the meeting’s exchanges in the third person. But they provided detailed, chronological accounts of individual participants’ contributions to those exchanges. Their transcript-like quality was reflected in Warburton’s (1976) characterization of them as “mechanically reported verbatim records.”

\(^{105}\) See Federal Open Market Committee (1971, pp. 90–93).

\(^{106}\) Patman apparently did not apparently request the 1970 transcripts, whose public release was due within a year.

\(^{107}\) On the legal actions made against the Federal Reserve at the time, see \textit{Washington Star} (Washington, D.C.), November 25, 1977, Goodfriend (1986, pp. 65–78), and Lindsey (2003, pp. 4–24), as well as the capsule account by Friedman and Schwartz in the \textit{Wall Street Journal} of December 20, 1993.

\(^{108}\) The published volume to result from these inquiries was \textit{Maintaining and Making Public Minutes of Federal Reserve Meetings}. This was a misleading title, as the FOMC already did continue to maintain minutes (formally titled the Record of Policy Actions and Minutes of Actions) for its meeting, and these documents continued to be regularly released from the mid-1970s onward without interruption. The Neal investigation, like the Patman request, had been concerned with whether transcripts (\textit{a.k.a.} the Memoranda of Discussion), not the minutes, should be maintained (and be released in an expedited manner). Friedman himself, like Burns and many others, referred to
October 2, 1976. This letter remarked that Neal’s questionnaire “touched a very sensitive nerve in my particular case. When Anna Schwartz and I were writing our Monetary History of the United States we were denied access to the minutes of the Open Market Committee by the Federal Reserve System at that time.”109 (Here, by “minutes” Friedman meant not the minutes, but instead what were, as noted above, known by the 1970s as the Memoranda of Discussion—that is, the published versions of the meeting transcripts.) Friedman recalled how he and Schwartz, in writing their History’s account of monetary policy in the 1930s, had been able to compensate for the absence of these documents for FOMC meetings (as well as the meetings of the predecessor committees to the FOMC) by using former Federal Reserve Bank of New York president George Harrison’s publicly-available papers. These papers, deposited with Columbia University, had included internal Federal Reserve documents, including detailed policy-meeting memoranda.

In his letter, Friedman made the point that the start of the Federal Reserve’s practice of releasing same-year FOMC minutes had begun in 1967, while its practice of releasing FOMC transcripts after a five-year lag had begun in 1964. Prior to these changes, the minutes for the FOMC meeting in a given year had been released only in the following year’s Federal Reserve Board Annual Report, while transcripts had had no official release at all. Friedman suggested that the likely catalyst for these mid-1960s increases in FOMC disclosures was the release of the Friedman-Schwartz Monetary History. The implication was that the Friedman-Schwartz study provided impetus for enhanced FOMC meeting documentation: both because their work had highlighted the limited amount of releases the Federal Reserve had made to date on FOMC deliberations; and also because the Federal Reserve hoped that issuance of its historical records would prompt other researchers to produce rival studies that disputed the Monetary History’s mostly-negative verdict on the Federal Reserve’s historical monetary policy performance.110

Other accounts have pointed to different reasons why the FOMC started to release these documents in the 1960s.111 Whatever the reasons, however, it remains the case that Chairman

---

110 See also Wall Street Journal, December 20, 1993.
111 See Nelson (2018b) on these alternative accounts.
Burns was being accurate when he remarked in his 1975 response to Patman that, as things stood, the FOMC regularly issued a “copious body of information” concerning its decisions, separate from that in the transcripts.\(^{112}\)

True, the Federal Reserve had, by 1975, acquired a reputation for obfuscation that was now hard for it to shake. For example, *Business Week* (April 28, 1975) had editorialized that the “Fed itself is sticking to the traditional policy of saying little, and what it says comes out in circumlocutions that require translation and interpretation.” But, in fact, the FOMC minutes were not vacuous. They contained material information about the FOMC’s thinking, and the Federal Reserve Board staff of the time prided themselves on keeping accurate records of the meeting.\(^{113}\) Furthermore, these releases could be taken in conjunction with the large number of speeches by Burns and other FOMC policymakers, the Congressional testimony of Burns and other Board members, as well as other material in the *Federal Reserve Bulletin* and in the Board’s annual reports. The sum of these items provided contained ample material for ascertaining policymakers’ doctrine and strategy and the motivation for FOMC decisions during the 1970s—and they were used for this purpose by researchers such as Poole (1979) and Romer and Romer (1989, 2002).

From the mid-1970s onward, Friedman was heavily influenced by the public-choice literature (see Nelson, 2018a, Chapter 8, as well as the next chapter). This perspective made him extremely disdainful from about 1975 on when it came to much of the Federal Reserve’s public descriptions of its policies. With regard to strategy and doctrine, Friedman would, as already indicated, contend that the monetary policymakers understood the monetary nature of inflation. Correspondingly, he implied that their public appeals to cost-push explanations amounted simply to disinformation.\(^{114}\)

But it seems that, instead, Friedman was under a mistaken impression when he supposed in the later 1970s that Burns and other policymakers no longer believed cost-push theories. Their actual rejection of such theories did not occur until a little after the Burns era. With regard to individual policy decisions, Friedman would lament in 1988 that FOMC policy documents told him the policy actions take but not “the policy that produced these actions” (*Wall Street Journal*, April 15, 1988). This conclusion, however, was flawed by a failure to consider a wider body of

\(^{112}\) From Burns’ letter of June 3, 1975, to Patman, as reproduced in Lindsey (2003, p. xvi).

\(^{113}\) For example, Robert Holland, at different times a staff member and governor of the Federal Reserve Board, wrote that as “secretary of the Federal Open Market Committee during the year 1972, [I] was responsible for an accurate rendition of what took place at its meeting.” (*New York Times*, September 1, 1974.)

\(^{114}\) A similar position was taken later by Meltzer (2009b).
official policy materials the Federal Reserve had put on the public record, including the policy minutes. Romer and Romer (1994b, p. 81) found that these materials were informative about “the purpose of the policy” underlying FOMC behavior in 1987–1988. This was the very period for which Friedman in 1988 had complained Federal Reserve public documents were unrevealing about the consideration driving monetary policy.115

In contrast to his jaded post-1974 attitude toward FOMC communications, Friedman was more on track earlier when, in 1970–1974, he indicated that he understood the thinking behind Federal Reserve decisions but that he disagreed with that thinking. And with regard to the reaction function, in the late 1960s and, in the early 1970s, both he and Samuelson indicated they saw systematic aspects in the Federal Reserve’s reaction function; again, in Friedman’s case the key objection was disagreement with the reaction function followed.116

Specifically, before 1975 Friedman’s reservations stemmed not from a notion that the Federal Reserve’s reaction function could not be ascertained, but instead on the fact that the reaction function followed did not give rise to a predictable pattern for monetary-growth; in addition (though relatedly), he objected to the absence of an articulated, announced long-term plan by the Federal Reserve. He favored a different instrument (reserves or the monetary base), with a changed implied reaction of interest rates to the state of the economy than prevailing previously, alongside public announcement of target rates of monetary growth. And once the Federal Reserve adopted formal monetary targets in 1975, neither the instrument choice nor the reaction function seemed to Friedman best suited to achieve those targets.117

Rather than positing a relationship between transcripts and disclosure of the reaction function, Friedman’s 1976 letter focused on the transcripts’ historical value. He called for the Federal

---

115 See the coverage of 1987-1988 monetary policy on pp. 81–82 of Romer and Romer’s (1994b) section, “Monetary Policy Since 1987.” Their analysis drew on the Record of Policy Actions (the then-name for the FOMC meeting minutes), which had been reprinted in the same Federal Reserve Board (1988) Annual Report that Friedman had criticized as vacuous.

Friedman and Schwartz’s own work had demonstrated that the public documents of the Federal Reserve were revealing about the reasons for policy decisions. This is brought out by the fact that for a quotation from the Federal Reserve regarding the reason for the 1936 decision to increase reserve requirements, Dewald (1975, p. 154) cited “Federal Reserve technical memorandum quoted in Milton Friedman and Anna J. Schwartz, A Monetary History of the United States...” The quotation (indeed used by Friedman and Schwartz, 1963a, p. 526) actually came from the Federal Reserve Board’s annual report, not an internal memorandum.

116 For documentation of these points, see Nelson (2018a, Chapter 8; 2018b, Chapter 15) and Chapter 2 above.

117 In this later period, Friedman layered onto his technical objections to the Federal Reserve’s reaction function criticisms that reflected his acceptance of the public-choice literature’s arguments. For example, he argued that central-banking and commercial-banking interests were opposed to reforms of monetary-control procedures, and that the political environment was a force working against implementation of a noninflationary monetary policy.
Reserve’s creation of Memoranda of Discussion to be resumed and for their release to be two years, rather than five years, after the meetings.\textsuperscript{118} This two-year suggestion implied he likely had some sympathy with the Burns position that a too-rapid release of the transcripts would limit the candor of the meeting discussions. It is also notable that, even during his post-1974 era of enhanced cynicism regarding the reliability of public statements by the Federal Reserve, Friedman did not himself argue that rapid (that is, a same-year, and perhaps near-immediate) transcript release was necessary for clarity about the reaction function. In contrast, in his own letter to Neal, Robert Lucas (1976a) argued that immediate release of the FOMC transcripts was necessary, in order for the FOMC’s reaction function to be known by the public.

The Burns FOMC was not really very secretive about its reaction function, as it did relate policy-rate decisions to the state of the economy. It is true that, as already noted in Chapter 4, Burns did not want to disclose his intentions or the FOMC’s and tensions regarding future interest rates. But the disinclination to engage in discussion about interest-rate prospects (that is, about the interest-rate values that are most likely in future periods) is, in principle, separate from transparency about the reaction function (that is, about the mapping between current monetary policy decisions and the state of the economy). And, as it turned out, the FOMC transcripts for the later Burns years did not turn out to be particularly forthcoming on policymakers’ assessments interest-rate prospects. They were instead marked by much the same tendency to avoid discussion of the future rate decisions as that Burns exhibited in public. As Burns said on the record in 1977, “I won’t talk about the future. I don’t know what the future will be.” (\textit{Wall Street Journal}, October 7, 1977, p. 33.)

In the Burns era, as in other periods, the transcripts supplemented and fleshed out information on the FOMC’s decisionmaking. But they were not a unique or even essential source for grasping the thrust of Committee thinking. Rather, they supplemented voluminous material that was in the public record far earlier than the transcripts. Indeed, the FOMC transcripts for the second half of the 1970s, as eventually released, verified that parts of the public record that Friedman and subsequent writers sometimes dismissed as mere disinformation for public consumption—such as the Federal Reserve’s continued appeal to cost-push explanations for inflation, and Arthur Burns’ contention that monetary policy was not overexpansive in 1976 and 1977—actually reflected the positions genuinely taken by policymakers, as they also articulated them in their internal deliberations.

\textsuperscript{118} Friedman (1976g, p. 202).
The largest missing element among the information released publicly by the Federal Reserve in the 1970s consisted of high-frequency releases about FOMC decisions. The actual policy decision made at any FOMC meeting was typically not subject to a formal public disclosure until the release of the meeting minutes some weeks later (although financial observers were usually able to glean the essence of the decision well ahead of that time, by observing the open market operations taken by the Federal Reserve after the meeting). Friedman’s 1976 letter had called for ending this situation, by having the FOMC’s policy directive be made public immediately after the FOMC meeting.\footnote{Friedman (1976g, p. 202).} The problem of the absence of immediate postmeeting official information was not resolved until 1994, when, under Alan Greenspan, the FOMC started the practice of releasing a policy-decision statement immediately after each meeting.

Although this gap in information prevailed throughout his tenure, in other respects Arthur Burns seemed to take to heart—in his later years as Federal Reserve Chairman—the criticism that the Federal Reserve was insufficiently transparent. A few weeks after Business Week (April 28, 1975) editorialized that “Arthur Burns should stop playing a man of mystery,” the Washington Post (May 22, 1975) published an article titled “Arthur Burns Goes Public,” which highlighted his recent agreement to give a television interview for NBC’s Meet the Press. Burns followed this with other public media engagements, including an appearance of ABC’s own Sunday morning news show, Issues and Answers (January 18, 1976), a long interview with U.S. News and World Report (May 17, 1976), and press conferences in November and December 1976 (Washington Star (Washington, D.C.), November 25, 1976, and December 6, 1976).\footnote{It was earlier reported (Washington Post, May 22, 1975) that Burns was considering having a press conference every six weeks as a permanent arrangement—a proposal with which he did not ultimately proceed.}

Furthermore, with regard to the FOMC, Burns in 1975 and 1976 presided over moves to more-rapid release of the meeting minutes. Under his tenure, the lag between FOMC meetings and meeting minutes’ public disclosure went down from the ninety days introduced in 1967, to 45 days, then to about a month—so that, by the time of new FOMC meeting, the minutes for the previous meeting were already publicly available (Washington Star (Washington, D.C.), November 25, 1977, p. A–6; Lindsey, 2003, pp. xi, 14–17).

These efforts at greater transparency would, however, be enormously overshadowed by a countervailing action on Burns’ part, taken in 1976 but only brought into the open in 1993, which would put paid to any prospect that he would be remembered as being forthcoming about monetary policy. As noted above, in 1976 the FOMC strongly implied, via its announcement that it would discontinue its Memoranda of Discussion, that it would no longer be keeping
meeting transcripts. In fact, this was an untruthful impression. The Federal Reserve continued to make, and keep, transcripts of FOMC meetings from 1976 for internal purposes. Indeed, Burns took his own copies of the transcripts from his tenure with him when he left the Federal Reserve in 1978, and these were included in the papers he subsequently deposited with the Gerald Ford presidential library (see Lindsey, 2003, p. 13). Friedman and Schwartz were among those criticizing the Federal Reserve when, in 1993, the fact of the withholding from the public of the existence of the transcripts became widely known (Wall Street Journal, December 20, 1993). The practice of releasing transcripts five years after an FOMC meeting was subsequently restored, and the Federal Reserve also published the meeting transcripts from the 1976–1993 period.

The end of the Burns era

Burns’ second term as Federal Reserve Chairman was due to expire at the end of January 1978. When Jimmy Carter was elected, Friedman speculated that, if Carter chose not to reappoint Burns, the most likely nominee for the Chair position was Arthur Okun (Instructional Dynamics Economics Cassette, Tape 202, November 1976, Part 1). However, a year later, Okun publicly ruled himself out of consideration for the position (Washington Post, November 18, 1977).

By this time, Friedman had actually come to the view that Carter would likely decide to reappoint Burns, despite the public airing of several disagreements between the president and the Federal Reserve leadership. Having Burns at the head of monetary policy, Friedman reasoned, meant that the administration could be critical of the Federal Reserve, rather than being

---

121 Similarly, Chairman G. William Miller later stated: “Those Memoranda of Discussion, of course, have been suspended recently; I hope that they will be reinstated, so that there will be a historical base for analyzing decisions.” However, he also said that he was still “learning about” the FOMC meeting process, and he may not have known that meeting discussions were still being transcribed—and that what had been suspended was (only) the preparation of a public version of the transcripts. (The quotations are from Miller’s testimony of April 10, 1978, testimony, in Committee on Banking, Finance, and Urban Affairs, 1978b, p. 127.)

122 This usage appears to have been limited to the creation of the public minutes, which then became the main record of the meeting consulted, rather than for regular referencing in place of the minutes. Consistent with this, Federal Reserve Chairman G. William Miller said to a Congressional questioner on April 10, 1978: “the minutes that I have [of the February 1978 FOMC meeting] are the same that you have” (in Committee on Banking, Finance, and Urban Affairs, 1978b, p. 125).

123 The 1970s Memoranda of Discussion up to 1976 that had not been released by 1976 had already been released on the five-year-lag schedule. For example, writing in 1978, Poole (1979, pp. 478–479) used the Memoranda of Discussion through 1972 (though referring to them as “FOMC Minutes”) used the Memoranda of Discussion through 1972, while in the same proceeding Jordan (1979, p. 497) noted that the coming years would see the public release of the Memoranda of Discussion for 1973, 1974, and 1975. (Those prepared for early 1976 were released as well.)
associated with its policies. In mid-December 1977, Friedman assessed that the “chances are good” that Burns would be reappointed (St. Louis Globe-Democrat, December 16, 1977). But on December 28, 1977, Carter announced that he would be nominating a new candidate, G. William Miller, for Federal Reserve Chairman (Dallas Morning News, December 29, 1977).

Friedman expressed doubt that Carter’s decision would have much bearing on events. “On very few occasions in the past,” he remarked in reaction to the Miller nomination, “has the name of the man who is chairman of the Fed made much difference.” (New York Times, December 30, 1977, p. D3.) “Policy is unlikely to change,” Friedman observed (Journal of Commerce (New York), December 30, 1977). In particular, he had already indicated that he expected inflationary policies to continue whether Burns was reappointed or not.

Shaping Friedman’s judgment was the tenure of Arthur Burns. Speaking before the Miller nomination was announced, Friedman alluded to his 1969–1970 euphoria over Burns’ initial nomination and appointment (see Nelson, 2018a, Chapter 15) when he noted that “the name of the man who is Chairman of the Federal Reserve Board makes far less difference than at one time I thought it did.”

For the moment, the perception of Burns among news and financial commentators lined up with the characterization given in a Wall Street Journal article (May 4, 1977) that “Mr. Burns is perhaps the nation’s best-known inflation fighter.” In contrast, shortly before the announcement that Burns would not be reappointed, Friedman would describe him as having presided over “a Fed which is promoting inflation” (St. Louis Globe-Democrat, December 7, 1977). Friedman saw the esteem in which Burns was held by others as par for the course, and he would later declare that “[n]o major institution in the United States has so poor a record of performance over so long a period yet so high a public reputation as the Federal Reserve.” However, with regard to Burns specifically, Friedman’s verdict on the Chairman’s record would, in the years after 1977, become much more widely accepted. For example, in 1987 the Financial Times judged

---

124 Friedman offered this scenario both in Instructional Dynamics Economics Cassette Tape 208 (February 1977, Part 1) and a talk at the Commonwealth Club of San Francisco on November 11, 1977 (see Friedman, 1977k, p. 494).
125 Friedman made a similar remark in Friedman (1982a, p. 103).
126 See Friedman (1977k, p. 494) and St. Louis Globe-Democrat, December 7, 1977.
127 Instructional Dynamics Economics Cassette Tape 215 (January 1978; recorded in December 1977).
that Burns had “enjoyed starry-eyed respect on Wall Street for longer than his track record probably justified.”

Contributing greatly to these retrospective judgments—as well as buttressing the negative verdict on Burns that Friedman was voicing during 1977—was monetary policy’s behavior during Burns’ final year or so as Federal Reserve Chairman. In an assessment written a little while before Burns left office, Friedman stated: “Monetary policy during 1977 was not in my opinion conducted effectively.” The new monetary explosion, Friedman said, meant “we have, for the fourth time in fifteen years, paid the cost of a recession to stem inflation and then thrown away the prize by starting off on a new inflationary path.”

The same letter criticized what Friedman called the Federal Reserve’s “futile attempt to control interest rates.” Indeed, Friedman saw the Federal Reserve’s reliance on the federal funds rate as a major factor behind the resurgence in monetary growth. Although Burns’ FOMC had raised the federal funds rate during 1977, Friedman believed that the Federal Reserve had been too slow in doing so and had thereby permitted rapid monetary growth.

Such a pattern largely repeated what had been observed earlier in Burns’ tenure. For example, a research article published in 1977 on the Federal Reserve’s federal funds rate reaction function found, on the basis of a sample period of December 1970 to December 1974, that “the Federal Reserve has been willing to move its operating instrument by only relatively small magnitudes, on average, in response to undesired growth in money” (DeRosa and Stern, 1977, p. 218). This regularity was prevalent not only in the first half of the 1970s but over almost the whole decade. In the 1980s, Friedman would note that the two years after formal monetary targets were introduced in 1975 did not lead the Federal Reserve to abandon stabilizing interest rates in the short run. The retrospective judgment of David Lindsey, Friedman’s former student and a Federal Reserve Board staffer in the later Burns years, affirmed that this stabilization pattern had been at the expense of longer-run monetary and macroeconomic stability. “The results for inflation in the 1970s suggested that the FOMC had acted ‘too little too late,’” Lindsey remarked in a 2003 Federal Reserve Board staff memorandum.

---

129 This judgment appeared in an editorial of May 26, 1987, a month before Burns’ death.
130 Friedman (1978d, p. 156).
131 Jerry Jordan likewise judged (in American Banker, March 24, 1983, p. 10) that the FOMC “resist[ed] rising market interest rates in 1977 and 1978 by rapid injections of money and credit.” It happens, as discussed below, that this characterization applies more to 1977 than to 1978, at least if M2 (rather than M1, the aggregate that Jordan tended to prefer at the time) is used as the measure of money.
132 See Friedman (1982a, p. 108) and Friedman and Friedman (1985, p. 95).
133 Lindsey (2003, p. x).
It was a severe disappointment to Friedman that Burns’ FOMC did not genuinely change the Federal Reserve’s operating procedures and had, instead, maintained the federal funds rate as its operating instrument. Burns opposed a move to a reserves-control system to the end: in a September 1977 letter, the Chairman maintained that “technical adjustments in our procedures… would not improve matters” (quoted in American Banker, October 3, 1977, p. 3). To Friedman, this position was symptomatic of Burns’ having become a “captive of and spokesman for the bureaucracy he supposedly commands” (Newsweek, July 24, 1978).

Indeed, the issue of monetary control ranked near the topic of incomes policy as a source of strain between Friedman and Burns. One of the senior staff at the Federal Reserve Board of the time, and a former Friedman student, Stephen Axilrod, observed of Burns and Friedman, “Their very close friendship, I think, kind of broke up because they didn’t agree on the money supply… Milton became really ticked off. I can’t remember all the letters I wrote to him, over Burns’ signature, on this subject, but there were a lot of them—maybe three; and, on this subject, three is a lot.”\(^{134}\) Axilrod, however, traced Burns’ resistance to changes in monetary-control arrangements less to the staff influence than to the fact that “Burns, essentially, did not believe at all in money.” (Stephen Axilrod, interview, April 24, 2013.)

Paul Volcker, who observed the Burns/Friedman relationship first from the vantage point of the U.S. Treasury (from 1971 to 1974) and then (from August 1975) as president of the Federal Reserve Bank of New York, observed that “they were deadly enemies. Though it was funny, because Friedman was a student of Burns. But Burns was very unhappy with [Friedman’s outlook]… I’m trying to reconstruct a personal relationship insofar as I knew it, and I didn’t know it all that much. But I did know that, intellectually, they’d become enemies.” (Paul Volcker, interview, October 16, 2013.)

During 1977, Burns did affirm the desirability of slowing the growth in the money stock over time, as already indicated. However, in response to a statement by Allan Meltzer, who had said, “The Fed simply tried too long to hold down short-term interest rates,” Burns defended compromising between stability in short-term interest rates and achievement of the monetary

---

\(^{134}\) In addition to direct correspondence with Friedman about monetary-control issues, Burns had to write or commission Federal Reserve replies to Congress’ expressions of interest in Friedman’s proposals in this area. Chapter 4 above gave one such example: the exchange stemming from Friedman’s November 1975 Congressional testimony. An earlier example occurred in 1971, when Burns wrote a letter (dated May 3) to William Proxmire, who had asked for a response to Friedman’s Newsweek column (May 3, 1971), “Money Explodes.” As was his custom by this point, Burns made minimal reference to Friedman: he mentioned him fleetingly at the start of his three-page, single-spaced letter (see Burns, 1971b).
targets. He maintained that, in seeking a reconciliation between these goals, “I’m a quasi-monetarist” (Wall Street Journal, October 7, 1977).

With the announcement of Burns’ departure, Friedman seemed relieved that he would no longer have to criticize Burns on current monetary policy matters. He opined that Burns could “render greater service” as a private citizen than as a policymaker (Journal of Commerce (New York), December 30, 1977). He looked forward to Burns having more time to participate in public discourse on nonmonetary economic issues, such as the role of the public sector, on which Friedman felt Burns had spoken eloquently while Chairman (Newsweek, January 9, 1978a).135

Friedman’s relationship with Burns, already considerably improved by 1978 over its state earlier in the decade, would subsequently move back toward its previous state of amicability. Gloria Valentine (interview, December 5, 2013) noted that in her initial years as Friedman’s secretary Burns was “angry with Professor Friedman about his criticism of the Fed,” and she observed that, after leaving office as Federal Reserve Chair, “Arthur Burns came and visited Hoover, and he and Friedman talked; it was as if they had never had a problem.” Burns and Friedman would work together on Republican candidate Ronald Reagan’s economic team during the 1980 presidential election campaign (see Chapter 9).

In these circumstances of restored relations, Friedman was reluctant after 1978 either to criticize Burns’ record or to recount in detail his previous criticisms of Burns—though he occasionally did both of these things.136 This self-denying ordinance was likely reinforced by the reality that, once appointed U.S. Ambassador to the Federal Republic of Germany in mid-1981, Burns was in no position to respond publicly to criticism, as well as by the fact that Friedman largely supported the Reagan Administration foreign policy that Burns had been assigned to help pursue. When, however, the subject of monetary policy in the 1970s came up in his research or policy writings, Friedman repeated his earlier criticisms, and he would remain willing to name Burns on occasion in that context.137

---

135 Friedman also had some favorable things to say regarding Burns’ statements concerning monetary policy. See the discussion titled “G. William Miller” in the next section.
136 Silber (2012, p. 150) notes that Friedman did not mention Burns in his Newsweek column of February 19, 1979, which criticized the monetary policy of the 1970s. However, most inaccurately, Silber (pp. 150, 151) suggests that Friedman’s columns never held Burns accountable for the inflation of the 1970s, a claim that overlooks many of Friedman’s columns (and other commentaries) during Burns’ tenure. (The earliest Friedman Newsweek column that Silber considers was written after Burns’ successor had been named.) For many counterexamples, see Nelson (2013; 2016; 2018a, Chapter 5).
137 For example, Friedman named Burns (as well as Martin and Miller) in his indictment of monetary policy since the early 1960s, for example in the Free To Choose television series (U.S. version, Episode 9, “How to Cure
A glimmer of praise for monetary policy in the late phase of Burns’ tenure was discernible from a remark Friedman made in a Newsweek column in 1981. Although his 1978 article on Burns’ imminent departure had observed that “the actual rates of monetary growth, as determined by the Fed with Arthur Burns as chairman, have risen rather than decreased” in recent years (Newsweek, January 9, 1978a, p. 53), Friedman would subsequently grant, in his Newsweek of column of June 15, 1981, that monetary growth had begun to slow in late 1977.

This finding was based on the modern (post-1979) definition of M2. However, Friedman had earlier made a similar judgment on the basis of the old definition of M2 and had indicated that monetary growth started coming down in October or November 1977 (Newsweek, April 24, 1978; The Register (Orange County, California), December 23, 1979, p. E11). This result is also evident in Table 1 above. That table shows that, in the case of both old and new M2, monetary growth at annualized quarterly rates moved out of double digits during 1977. For the twelve-month growth rate, the shift out of double digits was somewhat later: in January 1978, new M2 was 9.8 percent above its level a year earlier—high, but the first single-digit rate recorded since June 1975.

Later studies by Sims and Zha (2006) and Bianchi, Lettau, and Ludvigson (2018), utilizing modern econometric techniques and variously drawing on monetary-growth and interest-rate data, have affirmed Friedman’s position that the switch away from an inflationary monetary policy began in late 1977—earlier than the oft-cited October 1979 date.

Thus, at the tailend of Burns’ tenure, the Federal Reserve had begun to end the second monetary explosion. This policy tightening would continue under Burns’ successor.

III. PERSONALITIES IN DEBATES ON MONETARY POLICY AND MACROECONOMIC STABILIZATION, 1977–1978

G. WILLIAM MILLER

It seemed to some observers that G. William Miller had come from nowhere to be in charge of U.S. monetary policy: the first Newsweek issue to appear following his nomination described

---


138 In the same vein, Friedman in the Chicago Tribune (April 2, 1978, p. A7) gave the period of very rapid monetary growth as the year 1976 and early 1977.
him as an “unknown quantity” (*Newsweek*, January 9, 1978b). But Miller had, in fact, established a profile on national economic issues for some years. The statements that Miller (as head of Textron, Inc., based in Providence, Rhode Island) made in these contributions did indicate a few areas of agreement with Friedman on economic issues. For example, Friedman had long been an advocate of relaxation of travel and nonmilitary trade with the USSR and China (see Chapter 9). So too was Miller: he had made headlines in 1966 when he obtained permission from the State Department to visit China (*The Detroit News*, July 13, 1966), and in 1970 Miller arranged a visit the USSR to encourage commercial relations (*Tampa Tribune*, May 5, 1970). But there were also areas of major disagreement, as discussed below.

Miller, who had been head of Textron since 1960, remarked in 1974: “I love the job and have lots of fun doing it. One problem, though: I don’t see how I can get a promotion.” (*Boston Globe*, February 4, 1974.)139 President Carter would remedy this problem by nominating Miller to a leading position in national economic policy, and Miller was confirmed by the U.S. Senate as the new Federal Reserve Chair in March 1978.

**Miller and macroeconomics: 1969 to March 1978**

Miller did not have Burns’ background of formal economics training, and he did not claim otherwise. “I cannot profess to have the qualifications to step into Dr. Burns’ shoes,” Miller remarked alongside Carter and Burns at the press conference announcing his nomination (*Dallas Morning News*, December 29, 1977). A couple of months later, in a Congressional hearing held about a month after Miller was sworn in, a legislator began haltingly, “Dr. Burns—Dr. Miller,” promoting Miller’s reply, “Neither Dr. Burns nor Dr. Miller is here!”140

Miller was, however, not new to monetary policy, as he had been serving for many years on the board of directors of the Federal Reserve Bank of Boston. This was not a *bona fide* policymaking position. It was nevertheless a senior (though part-time) role—one that involved receiving briefings on the economy and monetary policy, as well as consultations with the bank president (who, being an FOMC meeting participant, actually was a policymaker). Duesenberry (1983, p. 126) contended that, notwithstanding Miller’s success in the private sector, it was really

---

139 Although he was head of Textron throughout the 1960–1978 period, the formal positions Miller occupied in the firm varied somewhat over time. He was chief executive officer 1968–1978, president 1960–1974, and chair 1974–1978. He had joined the firm in 1956 and been its vice president from 1957 to 1960 (Europa Publications Limited, 1986, p. 1095).

his Federal Reserve Bank of Boston service that put him in the running for the Federal Reserve Chair position. Duesenberry also suggested that, during this service, Miller had been schooled in Federal Reserve System practices by the bank’s president, Frank Morris. As already indicated, Morris believed in the late 1970s that the tide was turning away from Friedman’s ideas; consequently, it is likely that the impression conveyed to Miller of Friedman’s economics from Morris’ schooling was far from favorable.

In his capacity as a director, Miller also attended some of the Federal Reserve Bank of Boston’s research events. As early as June 1971, for example, Miller was an attendee of the bank’s conference on consumer spending and monetary policy. This event attracted not only Keynesian luminaries like Duesenberry, Modigliani, Tobin, and Garder Ackley, but also Friedman’s former students Phillip Cagan, David Meiselman, Richard Selden, and Beryl Sprinkel, as well as his former teacher, Homer Jones.141

Furthermore, as a board member, Miller interacted with Robert Solow, who overlapped with him as a director. Solow recalled, “I was the only economist on that board, other than Frank Morris.” Consequently, Solow recalled, “I took it on myself to explain economics and to explain the role of the staff—the research staff of the Fed, and their findings, and all of that—to the rest of the board... So that was the role I played.” With regard to his impressions of Miller from these interactions, Solow observed, “I thought he was very sharp, and very smart. I don’t think he understood economics very well... But in terms of IQ… [and] just pursuing a line of thought, an argument, I thought he was pretty impressive.” (Robert Solow, interview, July 7, 2014.).142

During the leadup to 1978, Miller made a fair number of contributions to public discussions of economic stabilization policy and of price-setting. These statements indicated that, however unhappy Solow was with the quality of Miller’s economic analysis, that analysis had even more substantial differences with Friedman’s perspective on economics.

An early example is provided by a statement Miller made in early 1969. Although he himself was the head of a conglomerate, he called for an inquiry into conglomerate mergers (The Detroit News, January 31, 1969). This move was perhaps an endorsement of the view—prevalent at the time—that increased concentration of industries was among the factors driving high inflation. The notion that Miller subscribed to cost-push views of inflation is reinforced by his comments

he gave in a *Business Week* guest op-ed in 1974 (an article earlier discussed by Romer and Romer, 2004, pp. 155–156). In this piece, Miller did not specifically claim what he called “controlling the aggregates—the supply of money and net federal spending” could not reduce inflation. But he contended that there were alternative means of achieving disinflation that did not entail short-run output losses. In addition, although Miller’s analysis criticized direct controls on wages and prices, it proposed a mixture of control and decontrol on other dimensions: for example, for firms, Miller proposed introducing more tax incentives and lower regulation, but he suggested introducing selective controls on consumers and removing, for two or three years, labor unions’ option to go on strike, in favor of a compulsory arbitration system (*Business Week*, October 5, 1974).

Miller’s economic views also got a measure of national exposure via a speech on the macroeconomic scene that he delivered in Pittsburgh in January 1977. The periodical, *Vital Speeches of the Day*, decided to publish the speech in its March 1977 issue. In the same issue, *Vital Speeches* printed a talk, “The Future of Capitalism; The Intellectual and the Businessman,” given at Pepperdine University, Los Angeles, California, on February 9, 1977. The author of this speech was Milton Friedman.143

Another occasion over this period that saw Miller acquire a high profile in economic-policy discussions was when he testified in February 1977 to the U.S. House of Representatives’ Committee on Ways and Means. Miller did so during the committee’s hearings on the new administration’s tax proposals.144 In this testimony, he spoke in favor of the Carter Administration’s proposed increase in the investment tax credit, but he added his own favored measure of region-specific rapid depreciation allowances for business investment. The contrast with Friedman’s stand on this matter is notable. By this time, Friedman had warmed to the idea of an investment tax credit, in light of the apparent logjam facing other ways of reducing corporate taxes (Instructional Dynamics Economics Cassette Tape 197 August 1976, Part 1, and Tape 207, January 1977, Part 2). But the specific arguments Miller invoked for his tax-credit package were not ones on which Friedman looked favorably. Miller stated: “I prefer not to reduce corporate taxes generally, because then the reduction can simply go into the corporation’s

---

143 See Friedman (1977l) and Miller (1977). Miller (1977) was previously analyzed by Romer and Romer (2004) and is touched on below.

144 See Committee on Ways and Means, House of Representatives (1977). When Miller’s nomination was announced, the *Irish Times* (Dublin) (December 30, 1977) referred to a “scouring of recent economic pronouncements by Mr. Miller” as having unearthed “a speech in Pittsburgh last January” (Miller, 1977), but it did not mention this testimony. (Romer and Romer, 2004, also cited and analyzed Miller, 1977, but not the 1977 Congressional testimony.) The testimony may have become little noticed because Miller was identified only as William Miller, not G. William Miller, in the hearings volume.
coffers and stay there.”\textsuperscript{145} In contrast, Friedman \textit{did} want general corporate tax reductions, while Friedman’s emphasis on monetary policy and aggregate demand made him unsympathetic to the view that whether a corporation saved or invested the proceeds from a tax cut was decisive for the path of aggregate demand.

Likewise, Friedman would have been unsympathetic with Miller’s suggestion, in the February 1977 testimony, that the prominence of investment spending in the rise of U.S. aggregate demand during 1961–1965 was a major factor in allowing the economic expansion of those years to be noninflationary. The “established merits of capital spending as a means of creating jobs without unleashing inflation,” Miller contended, made it appropriate to “suggest that there was a correlation among the economic growth, full employment, price stability[,] and expanded fixed investment.”\textsuperscript{146} In contrast, Friedman had stressed on numerous occasions (and would continue to do so) that forces that added to potential output, including capital spending, were typically minor factors in determining the course of inflation compared with monetary growth.

At his January 1978 confirmation hearings nearly a year later, Miller’s portrayal of what monetary policy could do paralleled Burns’. Monetary restraint, Miller said, was a necessary part of controlling inflation, so “over time, working on inflation will mean lower [growth in] money aggregates.” However, he maintained that such monetary restraint on its own could not be expected, even by 1985, to restore price stability—it was “a mistaken hope to believe the Federal Reserve on its own could accomplish this, because the economic environment in which we live is made up of many factors.”\textsuperscript{147} Like Burns, Miller did not regard monetary restriction as sufficient to remove inflation.

In sum, although—shortly before Miller was formally confirmed as chairman—President Carter described Miller as having an “excellent understanding of economics in every respect,” there was plenty in Miller’s economic analysis that was inconsistent with an emphasis on monetary policy as an influence on aggregate demand and—especially—on inflation.\textsuperscript{148} His positions were not very different from those of his predecessor or of many, perhaps most, academic economists at the time.\textsuperscript{149} But they were very far indeed from Friedman’s.

\textsuperscript{145} From Miller’s testimony of February 7, 1977, in Committee on Ways and Means, House of Representatives (1977, p. 370).
\textsuperscript{146} From Miller’s written submission in Committee on Ways and Means, House of Representatives (1977, p. 332).
\textsuperscript{147} From Miller’s testimony of January 24, 1978, in Committee on Banking, Housing, and Urban Affairs, U.S. Senate (1978b, p. 43).
\textsuperscript{148} The quotation is from Carter (1978), which records remarks the president made on March 3, 1978.
\textsuperscript{149} Romer and Romer (2004, p. 155) argued that the 5 to 5.5 percent natural unemployment rate they inferred from Miller’s (1977) speech as “much lower” than that espoused by the Federal Reserve, and specifically Arthur Burns, in
Miller on inflation control: 1978

Miller’s first Congressional testimony as Chairman was on March 9, 1978 (the day after his swearing-in). His opening testimony affirmed that the FOMC intended its strategy of lowering monetary growth over time—policy settings that would “prove consistent with… a gradual winding down of inflation over the longer run.” But he included among “policies beyond the province of the Federal Reserve” included “this nation’s ability to find a way to reduce the upward wage-price pressures that continue to plague our economy.”

In statements given later in the year, Miller continued to characterize inflation’s causes—and, to a large degree, its control—as being outside the sphere of monetary policy. For example, when speaking at an October 1978 conference on productivity, Miller (1978c, p. 21) stated: “Monetary policy can be used to restrain inflation, but without coordination with other economic policies, it offers substantial dangers of its own.” He contended that recent years had seen “a cycle of intense upward pressure on costs and prices,” as U.S. workers tried to maintain their real incomes, and he argued that dollar depreciation had further pushed up inflation. Earlier in the year, he had said of President Carter’s wage guidelines: “If businesses do not cooperate … then we will have high rates of inflation.”

These statements, it should be stressed, did not amount to a dismissal of the importance of ending inflation. Miller actually stressed this as an imperative. For example, in April 1978, he testified: “I am in favor of making inflation our No. 1 priority in terms of domestic policy.”

that period. However, a rate of about 5 percent for the full-employment unemployment rate was a common view in officialdom in 1977. As discussed in DiCecio and Nelson (2013), Romer and Romer’s analysis of Burns’ 1977 statements took him as believing that the economy was on the verge of overshooting potential output in 1977—when actually, as noted above, it seems that Burns’ concerns in that year were about too-rapid growth creating inflation through bottleneck mechanisms. Like others, Burns in 1977 believed that notable slack existed.

Miller (1978b, p. 188; p. 9 of typescript version).
Miller (1978b, p. 189; p. 12 of typescript version).

From Miller’s testimony of April 10, 1978, in Committee on Banking, Finance, and Urban Affairs (1978b, p. 135). Similarly, a little earlier in the Chicago Tribune (April 2, 1978a (Section 2, p. 8), Miller had stated: “I hope that we have the courage to make inflation our highest priority for domestic economic policy right now.”

Romer and Romer (2004, p. 155) referred favorably to previous retrospectives that had suggested that “it was apparent before Miller was appointed that he was primarily concerned about employment and growth and, hence, would run [an] inflationary policy.” However, as discussed below, neither the very important Romer-Romer analyses of Miller’s tenure as Federal Reserve Chair, nor other detailed accounts including that provided below, support the notion that Miller pursued deliberately inflationary policies or even that he loosened monetary policy. Furthermore, as both the quotation just given and those below indicate, Miller was not a subscriber to the position...
Rather, the above-quoted statements amounted to a different assessment on Miller’s part from that of Friedman regarding the issues of how inflation arose and the appropriate means of removing it.

**Friedman on Miller**

On May 11, 1978, Friedman met Miller in Washington, D.C., when the Federal Reserve Board governors conferred with their panel of consultants. Friedman had been attending these consultants’ meetings periodically since the panels were launched in 1965. This would, however, be the final such meeting Friedman attended. Friedman had become disillusioned with the meetings.

One reason Friedman was critical of these “meetings of so-called academic consultants” as a basis for a dialogue with the Federal Reserve concerned the role that the staff economists were assigned in them. Only Federal Reserve Board policymakers, and not the Board staff, interacted with the consultants during the formal proceedings of these events. The fact that, as Friedman put it, “the many Federal Reserve personnel [attending] sat around the sides of the Board Room, where the meeting was invariably held, without participating,” made these occasions more stilted than the various meetings in which Friedman had participated at the Federal Reserve Banks of Chicago, St. Louis, and (most recently) San Francisco. At these district-bank events, both policymakers (the bank presidents) and research staff had been Friedman’s interlocutors.

A deeper reservation Friedman had about the Federal Reserve Board panel meetings pertained to whether the events really amounted to meaningful consultations. This reservation had made him reluctant to continue accepting invitations to join the panels. Although he had attended one Board consultants’ meeting a year from 1971 to 1975, and he then appeared at the April 1976 meeting that rolled out the findings of the monetary-statistics committee (on which he had served), Friedman was absent from the subsequent panels that convened in October 1976,

---

that inflationary policies were a successful means of achieving a boost to the economy, and he put stress on the need to remove inflation.

The quotations in this paragraph are from Friedman (1982a, p. 105). The information he conveyed here was not correct in two respects. First, some of the later consultants’ meetings, including the May 1978 meeting, were held in Dining Room E in the Federal Reserve Board’s Martin Building (which was opened in 1974), instead of the Board Room in the Board’s Eccles Building. (The Board Room would have been the more memorable location, as consultants would be seated at the same long table used for FOMC meetings.) Second, by the late 1970s, the panel convened at these meetings was formally referred to as the Economic Consultants rather than academic consultants, as an early innovation in Arthur Burns’ tenure as Board Chairman had been to bring in nonacademic private-sector economists as panel members.

However, after making this observation to Axilrod, Friedman did accept the invitation to the May 1978 event, the first consultants’ meeting to be held under Miller. As of then, and for some time beyond, Friedman held out hope that Miller would be more receptive than Burns had been to the operational and strategic changes in monetary policy that Friedman was recommending. Nevertheless, by the time the May 1978 panel convened, Friedman’s negative perspective on the consultants’ meetings had firmed. Friedman later complained that “the choice of the particular consultants invited to attend seemed designed to guarantee offsetting and contradictory advice, leaving the Fed free to pursue its own devices.”156 The May 1978 meeting would not have struck him as an exception to this pattern.157 This meeting of the Board of Governors and the economic consultants had two sessions: one in which a monetarist (William Poole) analyzed monetary control, with Paul Samuelson as a discussant; and another in which a Keynesian (Arthur Okun) gave a presentation on current monetary policy, with Milton Friedman as his discussant.158

As he concluded his discussion of Okun’s presentation, Friedman “said he was never coming again,” recalled David Lindsey, the former Friedman student who, on that day, was among the many staff economists in the audience for the meeting.159 “He welcomed Miller as the new chairman and said, ‘You know, I’ve been coming to these for a long time and giving advice, and they never listen to me, and I’m not coming again.’ And Paul Samuelson said, in response, ‘I consider it an honor to be invited to these [meetings]; and I’m happy to give my opinion, whether you take it or not, and as long as you keep asking me, I’ll keep coming back.’ So, Samuelson, who I disagreed with mostly, was quite generous and cordial whereas Friedman [on this occasion] was a little bit of a jerk, I thought.” Friedman did stay for the rest of the meeting, as

156 Friedman (1982a, p. 105).
157 The information that follows is based on Federal Reserve Board records.
158 Although known, thanks to Poole (1970), for having provided theoretical support for the interest rate as a monetary policy instrument, William Poole in the late 1970s and early 1980s was an advocate of a reserves-based operating procedure: he judged in Poole (1979, p. 479) that “Burns’ failure to reform Federal Reserve operating procedures… was responsible for many of the policy mistakes of the era.” Consequently, he would have advocated a position at the consultants’ meeting one opposite to that of his discussant, Samuelson. As for Arthur Okun, he had criticized the increases in the federal funds rate in Burns’ final year in office (Washington Post, November 18, 1977) so, on this and other grounds, the position he voiced at the discussants’ meeting would have differed greatly from that of his discussant, Friedman.
159 Those staff members did not include Janet Yellen, who was an economist at the Federal Reserve Board in 1977–1978 (see https://www.brookings.edu/experts/janet-l-yellen/). Her absence from the attendee list suggests that, by May 1978, she had already left for the United Kingdom to start a new position at the London School of Economics.
well as the subsequent luncheon with the staff and the Board governors; so “he didn’t get up and stomp out; but it was virtually like that.” (David Lindsey, interview, May 2, 2013.)

Despite the tone he had conveyed with his appearance at this meeting, Friedman in 1978 saw reasons for optimism about Miller’s ascension. Ahead of the May meeting, he had said to Stephen Axilrod that policymakers “might pay attention now” to monetarists’ suggestions now that Miller was Chairman (Stephen Axilrod, interview, April 24, 2013). And a week or so before the consultants’ meeting, Friedman had given a negative retrospective on Arthur Burns’ performance as Federal Reserve Chairman and added, “Let’s hope it will be different with Miller.”

Asked later in the month to provide an assessment of Miller, Friedman said, “His statements to date are excellent. However, the real test is performance, and it is too early to judge that.”

It is evident from this last remark that Friedman had been left with a favorable impression by some of Miller’s statements since his nomination—perhaps those in which Miller had indicated that the Federal Reserve could not permanently reduce nominal interest rates or the unemployment rate through money creation. Also likely to have struck Friedman favorably was the fact that Miller made remarks about the need for sustained monetary restraint in pursuit of the goal of price stability. In taking this position, Miller mirrored Burns, of whom Friedman had said (St. Louis Globe-Democrat, December 7, 1977): “Burns’ statements are excellent.”

As indicated above, however, Miller’s statements prior to joining the Federal Reserve indicated that he was also well-disposed toward cost-push views of inflation. Furthermore, Miller’s

---

161 Newsweek, May 29, 1978. As these remarks were presented by Newsweek as part of a joint question-and-answer session with Paul Samuelson for the magazine, they may have been given when both the economist-columnists were in Washington, D.C., for the Federal Reserve Board consultants’ meeting held, as noted above, on May 11, 1978.
162 On monetary growth and unemployment, for example, Miller had testified on March 9, 1978 (the day after his swearing-in) that “[m]acroeconomic policies will not be able to produce the reduced level of unemployment that all of us seek as a national goal without unleashing a degree of inflation that would be self-defeating. In fact, we could unleash inflationary forces that would bring us right back to high unemployment. So I think you are absolutely correct in saying that if we rely upon macroeconomic policies, we are going to have enormous difficulties and perhaps not achieve our unemployment goal.” He was responding to a question that referred to a stated administration goal of a rate of 4 to 4.4 percent unemployment by 1983. (Committee on Banking, Finance, and Urban Affairs, U.S. House of Representatives, 1978b, pp. 118–119.) Miller’s warnings against the dangers of holding nominal interest rates down included page 153 of the same testimony and his remarks of June 29, 1978, in Joint Economic Committee (1978, pp. 126–127).
163 For example, Burns had said (Family Weekly, July 3, 1977): “Too often in the past we have lacked the courage to stay long enough on a monetary and fiscal path that will lead to noninflationary economic growth. We cannot afford to backslide once again.” Even when making this admonition, however, Burns did not suggest that removing inflation would require a period of recession or slow growth, as Burns envisioned the appropriate policies as “sustaining the expansion now in progress” and, indeed, “significantly reducing the high level of unemployment.”
statements as Federal Reserve Chairman would make clear that he continued to hold a nonmonetary view of inflation.\textsuperscript{164} One statement by Miller, in July 1978, that “monetary policy cannot do the job alone,” would particularly draw Friedman’s ire: Friedman was still quoting it in the 1980s.\textsuperscript{165} That this statement would have been anathema to Friedman was certain; but the statement amounted, in essence, to a repeat of remarks Miller had made at his January 1978 confirmation hearings and his March 1978 first Congressional testimony as Chairman (both quoted above). Friedman may have largely overlooked these early statements and may have worked on the presumption that Miller’s views concerning inflation and monetary policy were close to his own. But Miller’s subsequent affirmation as Federal Reserve Chairman of a nonmonetary perspective toward inflation would have led Friedman to abandon that presumption. In September 1978, Friedman complained that in the face of rising inflation, “discussion about appropriate policy tends to concentrate either on income policies or fiscal policy.”\textsuperscript{166}

Another area of disappointment for Friedman was that of monetary policy operation. At the time when Miller was nominated, Friedman said that Miller’s first act, once confirmed, should be to establish a committee to investigate Federal Reserve operating procedures, with a view to a major reform: “its techniques for controlling money creation are obsolete” (\textit{New York Times}, December 30, 1977, p. D3). Several months into Miller’s tenure, Friedman still hoped that Miller’s background in the business world might incline him to be innovative with regard to monetary policy procedures—and in particular to initiate a move from reliance on a federal funds rate instrument to the use of a bank-reserves instrument (\textit{Newsweek}, July 24, 1978). The Miller FOMC, however, retained the federal funds rate as its operating instrument.

\textit{A “long-range program”}

In 1977 and 1978, Friedman saw another example having been recorded of a pattern he had observed on several previous occasions since 1962: the United States abandoning demand restraint. Against this backdrop, Friedman laid increased stress on the need for a multi-year program of disinflation. In an April 1977 television appearance, for example, he stated: “I think the most effective cure for inflation is a slow, steady attempt to bring it down over a period of four or five years.”\textsuperscript{167} Around the same time, he noted the advantages of “a steady course on the

\begin{itemize}
\item\textsuperscript{164} On this matter, as well as the items noted here, see Romer and Romer (2002, pp. 31–32) and Nelson (2005).
\item\textsuperscript{165} See \textit{Newsweek}, May 2, 1983, and Friedman (1984a, p. 55).
\item\textsuperscript{166} Friedman (1978c, p. R–185).
\item\textsuperscript{167} \textit{MacNeil/Lehrer Report}, PBS, April 18, 1977 (\textit{American Banker}, April 21, 1977 p. 6).
\end{itemize}
path of government which can be known in advance by people in the marketplace” (*Saturday Evening Post*, May/June 1977, p. 16).

A year later, Friedman gave a specific version of this proposal: a *Newsweek* column (of April 24, 1978) called for steps down in M2 growth each year, accompanied by restrictions on federal government spending that would limit the adjustment to the monetary restriction required of the private sector.168

On May 9, 1978, Chairman Miller replied to a request by Representative Dawson Mathis (D-GA) to react. Miller wrote: “In the last section of his article Dr. Friedman asserts that ‘We need a long-term program dedicated to eliminating inflation.’ I agree wholeheartedly.”169

This was true, in general terms: in the previous month, for example, Miller had observed that a “high priority should be the implementation of a strong anti-inflation program.”170 The difference was great with Friedman on specifics, however. This was brought out by Miller’s next observation, “Monetary policy has a critical role to play, but it cannot alone bear the whole burden of combating inflation.”

In an interview a couple of months later (*U.S. News and World Report*, August 7, 1978), Miller reiterated and elaborated on his opposition to Friedman’s proposed long-range plan. Miller argued that the “Federal Reserve is limited to what it can do about inflation” (p. 18) and cited factors that he thought were introducing more inflation, including a Social Security tax increase legislated for January 1, 1979, which “gets right into prices and is very inflationary” (p. 19). Miller said of Friedman’s plan (p. 19): “I don’t think it would work. It’s a nice, neat way to set the dial and go home, but I don’t think the world is made up that way. I don’t think that’s the way monetary policy works.” Rather, “monetary policy has to take account of oil boycotts and wheat deals and famines and wage settlements.”

---

168 Similarly, in a speech given in Scotland a few days after the *Newsweek* article appeared, Friedman (1978e, p. 7) observed: “What is needed is for the authorities to announce long in advance, and by that I mean years in advance, what is going to be the course of the monetary aggregates…” Such gradualist programs, of course, had been made by Friedman in some form for years. Stanley Fischer—who, as Section I discussed, participated in Friedman’s money workshop for several years starting in 1969/1970—observed (Fischer, 1981, p. 35): “Advocates of gradualism have argued since 1970 that the best way out of inflation is for the growth rate of the money stock to be reduced over a period of four to five years to the noninflationary range of 3 to 4 percent.” (Fischer added that there had since been “Fed acceptance of the argument.” As indicated presently, however, the Federal Reserve’s leadership, although it was agreeable to the notion of a gradual slowdown in monetary growth, did not endorse convergence to a completely noninflationary rate of the kind associated with M2 growth of about 4 percent.)


170 *Chicago Tribune*, April 2, 1978b (Section 2, p. 8).
Miller added in this interview, “What Dr. Friedman suggests is in a sense workable, but I think the volatility of interest rates and the impact on the real economy would be so disruptive as to be counterproductive.” Friedman’s Newsweek column proposal, however, had aimed to reduce the impact on the real economy of disinflation by reducing monetary growth in stages—by 1 percentage point per year—as well as by limiting the burden of adjustment on the private sector through restraint on federal spending. And as it happened, however, the monetary growth rate for 1978 for M2 that Friedman had recommended in his column—8 percent—was not very different from the actual M2 growth generated for the year under Miller.

However, Friedman’s proposal had featured a continuing downward path for monetary growth from 1978 to 1982, and the endpoint of this proposed path had drawn criticism from Miller in his May rebuttal. In that letter, Miller argued that “the 4 percent rate of growth in M2 that Dr. Friedman sets as a goal for 1982 would be the lowest rate of growth in that aggregate since 1960, except for the ‘credit crunch’ year of 1969.”\(^{171}\) The comparison with 1969 was not entirely appropriate, because 1969’s 4 percent monetary growth rate had been attained suddenly, rather than as part of a multi-year phased reduction. A 4 percent growth rate acquired in a gradualist manner could be expected to involve less of a crunch than a sudden shift to that rate.

The fact that Miller objected even to a gradual achievement of this 4 percent rate suggests a possible parting of company with Friedman on the appropriate definition of price stability. Friedman and the Federal Reserve probably did not differ too much in their assessments of longer-term M2 velocity behavior. It was widely accepted that M2 velocity had been roughly trendless since the late 1950s at least through the mid-1970s (see the discussion in Section II above), though there was some doubt about whether this was the case in the most recent years.\(^{172}\)

If Miller took potential output growth to be about 3.5 percent, and this assumption is combined with a constant or gently rising longer-run pattern for M2 velocity, Miller’s objection to 4 percent M2 growth might have reflected a judgment that the appropriate longer-run inflation objective was about 2 percent rather than around zero. Such a judgment would be consistent with Romer and Romer’s (2002) position that the Federal Reserve’s (and U.S. government’s) economic objectives have not changed appreciably over the decades.

\(^{172}\) As noted above, however, it is likely that both Friedman and the Federal Reserve viewed the M2 concept that was likely to have a stable velocity in the future as being the prospective redefined, and broader, M2 series, rather than the existing reported M2 series.
Friedman’s ultimate target of 4 percent monetary growth, in contrast, reflected his preference for roughly zero inflation. Evidently, Miller opposed a goal of a monetary growth rate consistent with zero inflation, even if that rate was approached gradually—perhaps because he believed that a 1 or 2 percent inflation rate was more conducive to orderly relative-price adjustment and so to macroeconomic stability.  

Friedman was invited by Congressman Mathis to write a rejoinder to Miller’s letter. In this riposte, dated June 8, Friedman remarked that he “had hoped that a new chairman who as a businessman has had to face facts and correct error” might involve a more open-minded response to criticism, but that he did not see evidence of this in Miller’s letter. “Unfortunately, Chairman Miller’s comments on my proposal for an announced five-year policy of monetary deceleration are strictly in the Federal Reserve tradition of blandly dismissing all criticism by undocumented assertion.” Friedman argued that Miller’s rejection of rules-based policy came despite the “highly defective” historical record produced under a U.S. monetary policy not based on rules. “I need not repeat the litany of failure documented fully in Anna Schwartz’s and my Monetary History of the United States, 1867–1960, nor remind you of the Federal Reserve’s contribution to both inflation and recession in the period since that covered in our book—including the credit crunch of 1969 that Chairman Miller refers to[,] as well as that of 1966.”

The blame-game and Miller

The choice, discussed above, between zero and 2 percent inflation had become a distinctly hypothetical one in the late 1970s. Inflationary pressures were again becoming severe, and the inflation rate shot up in 1978 (Table 2). Policymakers were caught off-guard by this development. Secretary of the Treasury Blumenthal had said in the summer of 1977 (American Banker, August 2, 1977): “We have a little too much inflation… Inflation is now coming down gradually.” And in remarks in March 1978, President Carter gave the inflation rate for the second half of 1977 as 4 to 4.5 percent, assessed the current underlying inflation rate as 6 to 6.5 percent, and reaffirmed his goal is getting inflation materially below 5 percent (Carter, 1978). Speaking in August (by which time inflation had risen substantially), Carter said (Business Week, August 21, 1978, p. 100) that “it has been running up at a rapid rate caused by unanticipated food price increases” but suggested that “the inflation rate curve [will] top out at the end of this year

173 If this was the basis for the rejection of a zero inflation target, it was in keeping with arguments in Tobin (1972) and that Friedman had considered in Friedman (1958, p. 252; p. 182 of 1969 reprint) (and expressed sympathy with at some earlier point: see Rees, 1970, p. 237), although he felt that other considerations outweighed this argument.

and then perhaps start to go down.” In the same vein, Chairman Miller stated at roughly the same time (*Iron Age*, August 21, 1978, p. 20) that “both the consumer price index and the GNP implicit deflator should show some improvement in the second half of the year.” Instead, both deteriorated (see Table 2 for the modern data), and both series would not peak until 1980.

The poor inflation record in 1978 and 1979, and Miller’s departure in 1979 from the Federal Reserve for the U.S. Treasury—at a time of adverse sentiment about the economy in financial markets and the U.S. community—would permanently damage his reputation. Miller was also vulnerable to a loss of standing because he came to the Chairman position lacking deep Congressional support. In contrast, Burns was held in high regard by Republican members of Congress—*Newsweek* (January 9, 1978b) had observed that Carter had “sacked a conservative icon” by not reappointing Burns—and by many Democrats. Burns’ champions saw him as vigilant against inflation, with a letter titled “Why Burns Should Stay” by Senators John Sparkman (D–AL) and Jacob Javits (R–NY) pointing to low longer-term interest rates as a demonstration of Burns’ success in containing inflation expectations (*Wall Street Journal*, December 22, 1977). In contrast, as we have seen, Friedman thought Burns had stirred up a surge in inflation, and Friedman’s 1978 criticism of G. William Miller amounted essentially to the position that the new chairman was not making a daring break from the Burns regime.

By the mid-1980s, with Paul Volcker having made a success of the job of Federal Reserve Chairman, the negative verdict on Miller was so entrenched that even a labor-economics textbook included a problem-question that gave a 1979 quotation from Burns and asked the reader to try to make a “coherent statement” of the logic behind “Miller’s assertion” (Ehrenberg and Smith, 1985, p. 331).
Although Miller was destined to be blamed for making inflation worse and not responding effectively to it, neither the stance of monetary policy during his tenure, nor the direction in which he took FOMC decisions, really justifies this verdict.

First, with regard to the overall stance of monetary policy under Miller: Although writers on monetary policy often follow Friedman in taking inflation in year $t$ as largely determined by events in years $t-1$ and earlier, it seems that this practice becomes rarer when it comes to discussing the behavior of inflation under Chairman Miller. Very often, the fact that inflation was high and rising under Miller is taken as evidence that Miller’s monetary policy was inflationary (examples include DeLong, 1997, pp. 271–272, and Levin and Taylor, 2013, p. 238).

An alternative interpretation is, however, available and is consistent with Friedman’s commentaries. Under this interpretation, the second peak of inflation in the 1970s reflected the aftereffects of the easy monetary policy of 1976 and 1977. Indeed, as documented in Section II, the surge in inflation that occurred from 1977 onward was predicted successfully by Friedman on the basis of data up to 1977. On this interpretation, the inflation behavior from 1978 to 1980 was largely set in place by monetary developments before 1978, and so it cannot be viewed as indicative of an easy monetary policy stance in those years.

Second, on the direction in which monetary policy went under Miller: The fact that monetary policy tightened under Chairman Volcker is sometimes taken as having the corollary that it did not tighten under Miller (see, for example, Goldfeld, 1990, p. 200, and Rotemberg, 2013). In fact, as will now be discussed, monetary policy did tighten notably under Miller.

_The Miller tightening_

That monetary policy tightened appreciably under Miller was stressed by Friedman at the time, but not often by later analysts (exceptions include Romer and Romer, 1989, 1990, and Nelson, 2005). However, M2 growth visibly fell in 1978—see Table 1 above—and, in later years, Miller would emphasize his success in hitting M2 targets and the fact that most of his tenure witnessed a firming of policy (see Biven, 2002, p. 143). Furthermore, the federal funds rate rose over his period in office, and the real short-term interest rate was positive for a considerable part of his tenure (see Figure 3). In addition, narrative evidence of a Miller tightening underlay the inclusion in the “Romer dates” of an FOMC tightening episode in August 1978 (see Romer and Romer, 1989, p. 136; 1990, p. 161).
The weight of the evidence therefore suggests that monetary policy moved decidedly toward tightening under Miller. This may seem discordant with the fact that Miller’s views on monetary policy and inflation were quite similar to Burns’. As Miller, like Burns, doubted that inflation would respond to a negative output gap and he emphasized nonmonetary influences on inflation, he would seem *a priori* unlikely to undertake a monetary policy tightening. But he did so. Why?

One explanation—that Miller tightened under pressure from the administration—can be ruled out. DeLong (1997) highlighted Miller’s tenure as one in which the administration urged the Federal Reserve to adopt a tighter monetary policy. But such a situation describes only a fleeting period during 1979 when the administration and Miller disagreed about the timing of monetary policy firming (see Chapter 8).

During the Federal Reserve’s tightening in 1978, in contrast, the administration spoke out against tightening. For example, President Carter stated in an August interview (*Business Week*, August 21, 1978, p. 102): “The Federal Reserve Board doesn’t consult with me before it takes any action. I deplore the rapid increases in interest rates that have occurred this year... My hope is
that for the rest of this year we will not have any additional increase in interest rates. So whatever the Board can do to prevent increases in short-term rates would certainly be constructive.” The Federal Reserve raised the federal funds rate further shortly after Carter made this statement.175

As for Friedman, his reckoning of monetary policy stressed the political environment as a factor shaping overall monetary policy stance—but not specific administration pressures to loosen monetary policy. He held the Federal Reserve, not Carter, responsible for monetary policy, though he faulted Carter for espousing a nonmonetary account of inflation’s sources.

An unforced error on Miller’s part that contributed both to the lasting perception that he was both an ineffective Chairman and inclined to loosen policy occurred when, at a meeting on June 30, 1978, of the Federal Reserve Board, he not only voted against a discount-rate increase but was also defeated by other Board governors in the vote (Wall Street Journal, July 21, 1978). Paul Volcker—being Federal Reserve Bank of New York president rather than a Board governor at the time—was not a participant in the Board vote but was disconcerted by it: “he got outvoted at one point; and I don’t think he realized the symbolic, or whatever, significance of that. You know, people thought he wasn’t very important because [they judged that] you got outvoted and therefore didn’t have much influence. I think he made a mistake in the way he handled that, but I think the criticism—it wasn’t so much criticism, but the [overall] feeling—that he was a weak chairman was a little bit unfair.” (Paul Volcker, interview, October 16, 2013.)

This episode contributed to what the Financial Times (August 3, 1978) reported as “questions… raised about Mr. Miller’s judgment,” due to his “appearing to take a softer line on… the outlook for interest rates than much of Wall Street believes desirable.” It deserves underscoring, however, that the FOMC under Miller did raise the federal funds rate, both before and after this discount-rate vote.176 Notwithstanding that reality, retellings or inaccurate recollections of the discount-rate-vote episode likely contributed to the post-1979 misconceptions that Miller either loosened monetary policy during his tenure or that he suffered defeats in FOMC votes as Chairman.177

175 This produced the Washington Post headline “Fed Moves To Raise Funds Rate” (August 29, 1978).
176 Indeed, in midyear a commentary had observed that Miller “has overseen a sharp tightening of short-term interest rates” (Kansas City Star, June 21, 1978), and Time magazine’s cover, titled “The Inflation Fighter: Federal Reserve Chairman G. William Miller,” for its July 17 edition, portrayed Miller as a gunfighter entering a saloon.
177 Miller explained his vote as reflecting a view that the discount-rate increase was excessive in relation to the increases in market interest rates that the FOMC had so far generated: see Miller’s testimony of July 28, 1978, in Committee on Banking, Finance, and Urban Affairs, U.S. House of Representatives (1978a, p. 37).
The main reasons for the Federal Reserve tightening in 1978, notwithstanding its nonmonetary view of inflation and administration pressure not to tighten, are likely twofold. First, although voicing skepticism that economic slack (excess supply) could reduce inflationary pressure, Miller, like Burns before him, did accept that excess demand could add to inflationary pressure, and so he was anxious to tighten by an amount that prevented output from overshooting its potential value: “we are up to a capacity point where I am worried,” he remarked in August (Iron Age, August 21, 1978, p. 20). Earlier in the summer, he explained that, as he saw it, the emergence of excess demand would superimpose new inflationary forces upon those already present from the cost side, so that “we are at a point where we must be very careful not to trigger demand-pull inflation along with our present cost-push inflation.”\(^{178}\) With the perception that the gap was closing, the warranted rate of both real and nominal income growth declined, so Miller concurred—notwithstanding his opposition to an explicit multi-year proposal of the kind Friedman had laid down—that “[w]e need to set the general target of squeezing down the monetary aggregates” (Iron Age, August 21, 1978, p. 20).

A second key factor that made the Federal Reserve well disposed toward policy tightening in 1978 was the depreciation of the U.S. dollar exchange rate, discussed in the next chapter. Though the FOMC’s tightening on this occasion was partly based on a rationale that Friedman deplored (that is, import-price-push views of inflation), it helped produce policy settings he preferred (that is, a further move away from the second monetary explosion of 1976–1977).

The tightening of monetary policy during 1978 was not a drastic squeeze: as Citibank Monthly Economic Letter observed (November 1978, p. 3): “We haven’t yet had the sort of credit squeeze that pushes interest rates far above their usual relationship to inflation.” But it was a material firming, and Friedman recognized, and stressed, the fact that monetary policy had tightened during 1978 under Miller. Indeed, as will be discussed in Chapter 8, by early 1979 Friedman was worried that Miller may have already tightened too much.

**FRANCO MODIGLIANI**

The beginning of this chapter discussed how Friedman began his time living in California with a three-month spell as a scholar-in-residence at the Federal Reserve Bank of San Francisco. One of the key events that the San Francisco bank convened during this stay was a debate on the

evening of January 26, 1977, between Friedman and Franco Modigliani.179

The bank’s research director at the time, Michael Keran, recalled of the event: “We organized that. I was the moderator, if you will; those two people needed one… We had to hire a large conference room outside the Fed [building] to handle all the people who wanted to come to hear it.” (Michael Keran, interview, March 7, 2013).

Modigliani had recently completed a year as president of the American Economic Association and, the previous September, had delivered his presidential address at the association’s annual meeting. That address (Modigliani, 1977), which has been discussed at various points earlier in this book, had been notable for containing an acknowledgment—at the time, almost alone among major Keynesians born before 1930—of the likely validity of the Friedman-Phelps critique of the Phillips curve. In this connection, Modigliani (1977) was the first in the list of references that McCallum (1982, pp. 7–8) gave when documenting the point that “a number of influential researchers in the Keynesian tradition have in recent years expressed agreement with the NRH [natural rate hypothesis].”180

Indeed, of the four major university-based Keynesians against whom Friedman was often pitted—Modigliani, Samuelson, Solow, and Tobin—Modigliani was the one who gave the earliest signs of acceptance of the natural rate hypothesis. His address was given in 1976, while Tobin and Samuelson did not provide their own endorsements of the hypothesis until 1978–1980, and Solow remained a skeptic.181 Because of this and other material in the Modigliani presidential address, Friedman was able to cite it, in a paper he wrote for the September 1978 Mont Pelerin Society meeting, as a demonstration of the ground that Keynesians were giving to his position.182

179 See Friedman (1977d) and Friedman and Modigliani (1977). (For the specific date of the event, see Oakland Tribune (California), January 27, 1977.) Dimand (2018, p. 10) incorrectly implies that the Friedman/Modigliani January 1977 event was not examined in the research literature on Friedman until 2016. In fact, however, Nelson (2007)—which appears to be, by a considerable margin, the most-cited research article specifically about Friedman to appear since his death—not only discussed this event but also updated the empirical evidence that Friedman offered in his exchange with Modigliani. Subsequent pre-2016 discussions of the Friedman-Modigliani event include Nelson and Schwartz (2008) and Nelson (2008, 2009); earlier, it was cited in Mayer’s (1998) analysis of aspects of the Keynesian/monetarist debate.

180 In addition, although Modigliani (1977) was mostly critical of the rational expectations literature, McCallum (1979, p. 66) perceived in the address an acknowledgment of the Lucas (1976b) critique.

181 See Nelson (2018b, Chapter 13) and the discussion earlier in the present chapter.

However, Modigliani’s acceptance of the natural rate hypothesis was highly qualified. In contrast to subscribers to the Friedman-Phelps Phillips curve, Modigliani seemed very enamored of the position, prevalent both in policy circles and among Keynesian economists, that the response of the output gap to economic slack had become negligible. In 1975, for example, he stated: “There is little evidence that increasing unemployment to 9 percent from 6 percent gives anything in the way of reducing inflation.” He also stated that measures that raised the unemployment rate in the short run constituted an “unacceptable” way to fight inflation (Kansas City Star, February 25, 1975).

That is, like Arthur Okun—another Keynesian economist who had sometimes been portrayed as a convert to accelerationist or natural-rate ideas—Modigliani seemed to believe that if a Phillips curve still described inflation dynamics, that Phillips curve was of the Friedman-Phelps kind; but that, most likely, a Phillips-curve relationship, in the sense of a responsiveness of inflation to the output gap, did not actually hold until full employment was reached.\(^{183}\) Similarly, like Okun, Modigliani attributed inflation in the 1970s largely to cost-push causes. For example, Modigliani (1977, p. 7) contended that inflation after 1973 “was driven primarily by an exogenous price shock rather than by excess demand.”\(^{184}\) And he voiced support for a variant of Okun’s idea of a tax-based incomes policy to fight inflation.\(^{185}\)

Most of the cost-push aspects of Modigliani’s ideas were, however, absent, from Modigliani’s presidential address. The address instead focused on revisiting the Keynesian/monetarist debate on policy rules in light of the experience of the United States in the first half of the 1970s. The January 1977 event in San Francisco consisted of a re-presentation by Modigliani of this presidential address, followed by Friedman as a discussant, with a back-and-forth exchange between them completing the session.

*The January 1977 debate*

“I was prepared to meet him,” Modigliani recalled in August 1982 of his debate with Friedman, adding that he went into the debate aware that Friedman was “tough and very fast and dangerous” (quoted in Klamer, 1983, p. 120). Furthermore, by the mid-1970s Modigliani regretted the time and effort he had put into the “AM/FM” debates in the 1960s (Charles

\(^{183}\) On Okun’s views, see Chapter 4 above.

\(^{184}\) In this vein, Modigliani held (in Friedman and Modigliani, 1977, p. 21) that “anybody who looks at the evidence must conclude that what happened in 1974 is primarily an explosion of prices, due to the impact of food and oil.”

\(^{185}\) See Okun and Perry (1978, p. 282).
Steindel, personal communication, September 9, 2015). These debates had evidently not led to the clear-cut and widely-acknowledged victory over Friedman that Modigliani had hoped for. Nor, for that matter, had Modigliani’s (1964b) critique of Friedman’s view on rules; despite being published in the Journal of Political Economy, this article had neither generated a written exchange with Friedman nor prevented increased interest in the constant-monetary-growth rule. Instead, what prevailed by 1977—as Modigliani put it on July 30 that year—was that economists were living in “these days of rampant monetarism.”

Modigliani’s (1977) address contained an extensive critique of the constant-monetary-growth rule, not only on grounds of principle but also via the deployment of what Modigliani argued was empirical evidence that the rule did not perform well in the United States. Like many Keynesians of the time, Modigliani perceived a greater conversion by U.S. policymakers (before 1979) to Friedman’s prescriptions than had actually occurred. But Modigliani (1977, p. 11) went further in this direction by implying that the constant-monetary-growth rule had been instituted informally under Arthur Burns. Modigliani held that U.S. monetary growth had been stable from 1971 to 1974. He contended that this had resulted in outcomes inconsistent with monetarists’ predictions: the stability of monetary growth in the 1970s had been associated with sizable swings in aggregate output. The same was true of another period of stable monetary growth that Modigliani cited: that from 1953 to 1957 (see Modigliani, 1977, pp. 11–12).

In the comment on Modigliani (1977) that he delivered during the January 1977 debate, Friedman stressed that monetary growth had not been stable in the 1971–1974 period when judged by his preferred aggregate, M2. Modigliani’s demonstration had used M1. On this basis, he challenged Friedman at the Federal Reserve Bank of San Francisco event, with reference to 1971–1974: “I defy you to find any other period in which, for a period of that length, you get that low level of variation.”

---

186 Charles Steindel was a graduate student at MIT in 1973–1977 (Charles Steindel, interview, December 3, 2015), and Modigliani was his coauthor (see Modigliani and Steindel, 1977) and thesis advisor. Steindel discussed the AM/FM (Ando-Modigliani versus Friedman-Meiselman) debate with Modigliani after coming across the debate during his graduate-student years. On that debate, see Nelson (2018b, Chapter 12).

187 From the remarks published as Modigliani (1979, p. 330).

188 See Friedman (1977d, pp. 15–16). For 1953–1957, Friedman applied the same argument, combined with the point that the start of the FOMC’s bills-only policy in 1953 had meant that the effective money supply was more variable over that period than was the measured money stock.

189 From Modigliani’s remark in Friedman and Modigliani (1977, p. 20).
After consulting the data, Friedman did just that. Whether judged by M1 or M2, monetary growth had been more stable in the early- to mid-1960s—when the economy displayed considerable stability—than in 1971–1974. Modigliani conceded this point.

A second concession from Modigliani during the seminar was his acknowledgment that Friedman had not—as Modigliani had claimed in his presidential address—rested his natural-rate-hypothesis exposition in 1968 on the assumption of competitive and atomistic labor and goods markets.190

Notwithstanding his concessions of these two points in the course of his exchange with Friedman, Modigliani maintained the two original claims in the published version of his presidential address, printed in the *American Economic Review* in March.

Just as the January 1977 debate saw Modigliani voice more concessions to Friedman than those he actually made in print, at that event he also qualified his criticisms of the constant-monetary-growth rule. “One of Modigliani’s points,” Keran recalled, “was that Friedman’s [constant-monetary-growth rule] approach would have been great if he were in Italy; but that, here in the United States, it’s too crude a thing to work; that, if you have a lot of inflation and a lot of disorganization in the markets, the monetarist approach might work well, but not in the subtle environment of the U.S. economy... I remember that comment quite strikingly.” (Michael Keran, interview, March 7, 2013.) “Unlike Milton, I happen to think there are some things the [U.S.] government can do very well,” Modigliani observed eighteen months after the January 1977 debate (*Wall Street Journal*, July 17, 1978, p. 27).

With regard to that debate, Keran judged that Friedman was the winner on the day. “In terms of Modigliani, I can still see the flash in his eye when he wanted to counter a point that Milton made. So he had a lot of passion. Milton was very analytical and calm, cool, and collected. And so, I think—I might be biased on this, but that was my impression—that Milton came across stronger.” (Michael Keran, interview, March 7, 2013.) However, Modigliani was satisfied with his own performance: “I think that I did fine” (Klamer, 1983, p. 120).

190 For Modigliani’s acknowledgment that Friedman did not base his 1967 presidential address on perfect competition, see Friedman and Modigliani (1977, p. 19). (Modigliani went on to argue that because agents respond to price signals in Friedman’s model, it was *de facto* a perfect competition model; but see Chapter 8 of Nelson, 2018a, as well as Chapter 2 above, for critical analysis and refutation of this argument.) For Modigliani’s acknowledgment that the early to middle 1960s provided a counterexample to his position that the 1970s (and 1950s) provided the most-stable periods of monetary growth, see the footnote written by Modigliani that appeared in Friedman (1977d, p. 16), as well as Modigliani (1986, p. 37).
Debating resource slack and stabilization policy

In the afternoon ahead of their evening debate, Friedman and Modigliani met the local media to discuss the national economic situation (*Oakland Tribune* (California), January 27, 1977). This appearance revealed disagreement about the current policy prescription. Friedman had already recommended to the authorities that they “[l]et the recovery proceed… at a moderate pace” (*Newsweek*, December 6, 1976) and exercise fiscal and monetary restraint, rather than increase stimulus. Indeed, as discussed in Sections I and II above, in the late-1976/early-1977 period he had already concluded that policy settings were already too loose, and that the stance of monetary policy was promoting a resurgence of inflation.

Modigliani, in contrast, recommended stimulus. His recommendation, it is true, was tempered by an upgraded estimate of the full-employment unemployment rate. He had already granted in his presidential address that more-generous unemployment benefits had raised the baseline unemployment rate.¹⁹¹ Five years earlier, he noted at the media appearance, his nominated full-employment goal would have corresponded to a rate of 4.5 to 5 percent. “[But] now, I’m around 6 percent. I am responding to the changed composition of the figures.” (*Oakland Tribune* (California), January 27, 1977.)

To be more precise: Modigliani’s estimate of the full-employment unemployment rate was probably about 5.8 percent. This value can be inferred from the fact that, in the same January 1977 media event, he gave the amount by which current unemployment exceeded that rate as 2 percentage points. Modigliani’s assessment of the value of the full-employment unemployment-rate was therefore higher than that being used by the Carter Administration, which accepted the value of 4.9 percent that it had recently inherited from the Ford Administration (see Biven, 2002, p. 202). Modigliani’s estimate was also close to the value of around 6 percent that would be judged in retrospect to be the appropriate estimate of the natural rate of unemployment in the late 1970s. But, notwithstanding his fairly realistic estimate, Modigliani recommended an amount of fiscal and monetary expansion that was inappropriate. His recommendation was unintentionally overstimulative because, as already indicated, it rested on then-prevailing estimates of potential economic growth. These estimates suggested, erroneously, that output needed to grow by more than 3.5 percent a year just to make headway into the amount of resource slack.

¹⁹¹ See Modigliani (1977) and the discussion in Chapter 4 above.
Friedman, in contrast, cast doubt on the 2 percent unemployment-gap estimate that Modigliani cited. With regard to unemployment-rate targets, he had already remarked in a November 1976 television appearance that “we are only led down a false trail by trying to believe that, somehow or other, a powerful government can pick a number out of the air—like 3 percent or 4 percent or 5 percent—and produce policies which will lead to this result.”192 Although Friedman regarded the 6 percent rate cited by Modigliani as more realistic, he did not want to center the formulation of economic policy on any numerical estimate of the full-employment unemployment rate. He had said in November, “we ought not to regard the [unemployment] numbers which are generated in that way [by the statistical agencies] as calling for massive policies which can do more harm than good.”193 With regard to the amount of resource slack, Friedman stated in the January 1977 media appearance: “I question whether the margin is big enough to make it possible for the government to pour out money without threatening another surge of inflation.”

Indeed, as already indicated, by early 1977 Friedman believed at this point that an inflation surge was already in motion. Modigliani told reporters at his joint appearance with Friedman that it was feasible for President Carter to preside over declines in both the inflation rate and the unemployment rate during his presidency. Friedman, in contrast, gave the reporters his estimate of a rate of 7 to 9 percent inflation for the next couple of years (Oakland Tribune (California), January 27, 1977). As has been stressed in this chapter, this was a prediction that was vindicated.

**Revisiting the 1977 debate**

About a decade after their 1977 exchange, Friedman and Modigliani both had occasion to revisit it in print. Modigliani’s book *The Debate Over Stabilization Policy*, based on lectures given in 1977, was belatedly published in 1986. The book alluded to the Federal Reserve Bank of San Francisco debate with Friedman, which had taken place not long before the lectures.194 However, little of Friedman’s work since 1970 was mentioned and, like Tobin, Modigliani was now concentrating his fire on the rational-expectations critiques of activist stabilization policies, rather than on Friedman’s writings.

---

Modigliani (1986) did, however, indicate (as had Modigliani, 1977) that the monetarist
movement had made valid criticisms of the early vintages of Keynesianism. Modigliani had
himself long set himself apart from these variants of Keynesianism. Indeed, Friedman had noted
in the San Francisco debate in 1977 that “I’ve always thought that Franco, insofar as you use
these terms, has always been a monetarist, in very important ways.” Friedman had particularly
pointed to Modigliani’s (1944) paper, which—at a time when Friedman himself was only
beginning to shake off extreme Keynesian views—had relegated instances in which monetary
policy was ineffective in altering nominal income to a special case, allowing an important role
for the money stock in other circumstances. Of course, once he became a monetarist, Friedman
would go further than this and would reject the empirical relevance of the liquidity-trap case.

In his New Palgrave dictionary entry on the quantity theory of money, which appeared a decade
after Modigliani (1977) was published, Friedman paid Modigliani the compliment of quoting
that presidential address—specifically, its observation that there were “no serious analytical
disagreements between leading monetarists and leading nonmonetarists.” But Friedman
immediately added that “there remain important differences on an empirical level.” Even this
qualification understated the divide between Friedman and Modigliani, however. For one thing,
different empirical judgments can imply different analytical frameworks: Modigliani’s insistence
in 1977 on the prominence of cost-push shocks as drivers of inflation, for example, amounted to
a rejection of Friedman’s monetary view of inflation. For another, by the 1980s Modigliani was
apparently not well disposed toward seeing his differences with Friedman as capable of being
resolved empirically, as he claimed that Friedman “has a mission and seems to be willing to
sacrifice some intellectual honesty for that” (Klamer, 1983, p. 120). Modigliani did not provide
any specifics underlying this claim. His remark may have reflected the disdain that leading
Keynesians often had for Friedman’s popular writings, which they felt were oversimplified.

Though its final version saw print in 1987, Friedman’s New Palgrave entry was initially drafted
in late 1985. That period also saw the fulfillment of a prediction that he had made concerning
Franco Modigliani. In 1981, Friedman had predicted that Modigliani would win a Nobel award
in economics for his work in consumption. This came to pass in October 1985.

---

195 Friedman (1977d, p. 12).
197 Friedman (1987a, p. 13).
198 See Chapter 2 above.
Bibliography

This bibliography consists of two parts: a chronological listing of the media items (pieces in sound, television, newspaper and magazine sources) that have been cited in this study, and a reference list, in alphabetical order, consisting of the research papers and books that have been cited.

I. Newspaper articles and other media items cited (chronological listing)


“Happy Talk on Money—Except the Yen,” *Newsweek*, October 9, 1972, pages 75–76.


Senator James L. Buckley, Milton Friedman, and Senator William Proxmire panel discussion on *The Energy Crisis*, held at the National Conference on Government Responses to the Energy Crisis, January 24, 1974; audiocassette.

Morris A. Adelman, Milton Friedman, and William D. Nordhaus panel discussion on *Long-Term Energy Crisis Solutions*, held at the National Conference on Government Responses to the Energy Crisis, January 24, 1974; audiocassette.


“Economist Warns Against ‘Overreaction,’” *The Herald* (Illinois), March 18, 1974, Section 3, page I.


Appearance by Robert Eisner, Milton Friedman, and Herbert Stein on *University of Chicago Round Table: The Nation’s Economy Out of Control*, PBS, May 1, 1974.
“Economic Outlook: Continued Slowdown and Inflation” (interview with Milton Friedman),


Milton Friedman, “Perspective on Inflation,” *Newsweek*, June 24, 1974, page 73.


19, 1974, page 41.

Associated Press, “Economic Talk Reaction Mixed,” *Times-Bulletin* (Van Wert, Ohio), Saturday,

D.C.), August 1, 1974, page A–12.


Paul A. Samuelson, “America’s Flirtation With Two-Digit Inflation,” *Financial Times* (London),
August 6, 1974, page 12.

Editorial, “The Time Has Come for Mr. Nixon To Leave the White House,” *Kansas City Times*,
August 7, 1974, page 40.

August 9, 1974, pages 45 and 50.


Milton Friedman appearance on Newsday (BBC2 television program), September 20, 1974; BBC transcript.


Milton Friedman and Walter Heller appearance on Inflation Merry Go Round, PBS, October 7, 1974.


Hobart Rowen, “Ford Plans May Hurt Economy,” *The Plain Dealer* (Cleveland), February 1, 1975, page 9–C.


Milton Friedman appearance on *Wall Street Week*, Maryland Public Television, February 7, 1975; transcript.


Soma Golden, “More Jobless Aid Seen Raising Rate: Moves To Broaden Benefits Likely To Add 0.5 to 0.9 Point, Study Asserts,” *New York Times*, February 25, 1975, page 45.


Milton Friedman appearance on *Monday Conference*, Australian Broadcasting Commission, April 14, 1975; transcript.


*CBS Morning News*, April 22, 1975; transcript.


Gough Whitlam (Prime Minister of Australia) appearance on *This Week*, HSV7 (Australian television), September 21, 1975. Transcript available at [https://pmtranscripts.pmc.gov.au](https://pmtranscripts.pmc.gov.au).


Associated Press, “Friedman Sees 12% Inflation Rate for ’76,” Mexico Ledger (Missouri), December 5, 1975, page 2.

Repps B. Hudson, “Friedman Thinks Inflation Will Resurge,” Kansas City Times, December 5, 1975, page 16C.


Milton Friedman, “Are These Monetary Swings Necessary?,” Newsweek, June 14, 1976, page 80.


Milton Friedman appearance on *Meet the Press*, NBC, October 24, 1976, NBC transcript.


Milton Friedman appearance on *Wall Street Week*, Maryland Public Television, November 5, 1976; transcript.


Milton Friedman appearance on Donahue, NBC, November 24, 1976 (Chicago broadcast date; syndication broadcast dates included December 1, 1976).


60 Minutes, CBS, November 28, 1976; CBS transcript.


362


Milton Friedman appearance on *Dinah!* (Dinah Shore syndicated television talk show), broadcast March 30, 1977.


“How Inflationary Was the Steel Settlement?,” Forbes, May 1, 1977, page 97.


366


Free To Choose (U.S. television version), PBS, Episode 1, “The Power of the Market,” broadcast dates in U.S. areas include January 12, 1980; transcript available online on Free To Choose website.


Free To Choose (U.K. television version, debate portion), BBC2, episode “Created Equal,” broadcast March 1, 1980; BBC transcript.

Free To Choose (U.S. television version), PBS, Episode 8, “Who Protects the Worker?,” broadcast dates in U.S. areas include March 1, 1980; transcript available online on Free To Choose website.

Free To Choose (U.S. television version), PBS, Episode 9, “How To Cure Inflation,” broadcast dates in U.S. areas include March 7, 1980; transcript available online on Free To Choose website.


“And Friedman Chose a Rose…,” Straits Times (Singapore), October 18, 1980, Section 2, page 1.


*A Conversation with Milton Friedman*, 2002 interview with Milton Friedman conducted by Gary S. Becker; issued commercially as a DVD by the Liberty Fund in 2003.


Bill Nichols and Tom Vanden Brook, “Tributes Pour In for Ford,” *USA Today*, December 27, 2006, pages 1A–2A.


II. References


390


Government Printing Office. Available at
https://babel.hathitrust.org/cgi/pt?id=pur1.32754077530800&view=1up&seq=3.

Committee on Ways and Means, House of Representatives (1978). The President’s 1978 Tax Reduc


October 12. Available at


Friedman, Milton (1975a). *There’s No Such Thing As a Free Lunch*. LaSalle, IL: Open Court Publishing Co.


Friedman, Milton (1975d). “Statement of Milton Friedman, Professor of Economics, University of Chicago.” In Committee on Banking, Housing and Urban Affairs, U.S. Senate, *Monetary Policy Oversight: Hearings Before the Committee on Banking, Housing and Urban Affairs, United States Senate, Ninety-Fourth Congress, First Session, on S. Con. Res. 18, Referring to the


Friedman, Milton (1975g). Address to the National Press Club of Australia. Canberra, Australia, April 9.


Friedman, Milton (1976d). “Rejoinder by Milton Friedman to ‘Federal Reserve Staff Comments on Prof. Friedman’s Statement Before Senate Committee on Banking, Housing, and Urban Affairs (Nov. 6, 1975).’” In Committee on Banking, Housing, and Urban Affairs, U.S. Senate, Third Meeting on the Conduct of Monetary Policy: Hearings, May 3, 4, and 5, 1976,


Sargent, Thomas J. (1978). “Rational Expectations, Econometric Exogeneity, and


Bank Structure and Competition, May 1 and 2, 1975*. Chicago: Federal Reserve Bank of
Chicago. p. v.

Freedom Books.

In Michael Walker (ed.), *The Illusion of Wage and Price Control*. Vancouver, British Columbia:
The Fraser Institute. 57–97.

Schuettinger, Robert L., and Eamonn Butler (1979). *Forty Centuries of Wage and Price


Schwartz, Anna J. (1973). “Secular Price Change in Historical Perspective,” *Journal of Money,

Schwartz, Anna J. (1975). “Monetary Trends in the United States and the United Kingdom,

*Monetarism*. Amsterdam: North-Holland. 43–49.


