Milton Friedman and the Road to Monetarism:
A Review Essay

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The objective of Ed Nelson's two-volume book, *Milton Friedman and Economic Debate in the United States, 1932-1972*, is to provide an account of Friedman's views in major monetary-policy debates during the period identified in the book's title. Nelson tells the story of the development of Friedman's monetary framework, from its Keynesian origins in the early-1940s, to its gradual absorption of monetary factors in the late-1940s, and, finally, to its monetarist character of the 1950s and after, through the windows of a selection debates that engaged Friedman. At the same time, Nelson places Friedman's monetary contributions within the context of the modern macroeconomics literature. In this essay, I consider doctrinal issues related to Nelson's account of the development of Friedman's monetarist framework.

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1. Introduction

Milton Friedman was the most influential economist in policy circles since John Maynard Keynes. Friedman almost single-handedly resuscitated the importance of monetary policy to academic and policy thinking while leaving his mark in such areas as the natural rate of unemployment and the long-run Phillips curve, the choice of exchange-rate regimes, the destabilizing effects of discretionary policies, the benefits of monetary-policy rules, the determinants of consumption, the demand for money, the effects of inflation expectations on nominal interest rates, and the narrative approach to monetary history. The objective of Ed Nelson's two-volume book, *Milton Friedman and Economic Debate in the United States, 1932-1972* (Chicago: University of Chicago Press, 2020), is to provide an account of Friedman's views in major monetary-policy debates during the period identified in the book's title although Nelson frequently draws on Friedman's works after 1972. The sweep of the book is astonishing, both in terms of the wealth of material presented and in terms of the author's extensive knowledge of the doctrinal and contemporary economics literature. Nelson has seemingly read everything that Friedman wrote during the latter's engagement with economics -- from the time of his graduate studies in the early-1930s to his death in 2006.

To provide some perspective of the book's immense compass, consider the following. The two volumes of the book contain a combined 15 chapters totaling 1,324 pages. The documentation of sources is impeccable: by my count, volume 1 contains 2,016 footnotes (164 pages); volume 2 contains 1,321 footnotes (115 pages). Volume 1 references 234 of Friedman's solo-authored works and 38 of his co-authored works; volume 2 references 232 of Friedman's solo-authored works and 35 of his co-authored works. These references do not include newspaper and magazine articles – including triweekly columns that Friedman wrote for *Newsweek* from 1966 to 1984 -- and electronic media items, which are listed separately. The bibliography of references of Volume 1 totals 98 pages; that of Volume 2 totals 93 pages. Nelson interviewed more than 250 people in preparing the book.

The end product is a work of the highest scholarship that will stand as the preeminent work on Friedman's monetary economics for both present and future generations. The book is not a biography although all the ingredients that comprise a biography are included in various places. Nor is the book a linear account of the

development of Friedman's monetary scholarship. The book tells the story of the development of Friedman's monetary thinking through the windows of a selection of debates that engaged Friedman. For scholars of both Friedman and the development of macroeconomics, this book is to be carefully studied although each chapter can be read separately from the others. The payoff will be a comprehensive understanding of the emergence of Friedman's views and their place in modern literature.

Most of the chapters of the book are divided into overlapping sections titled "Events and Activities," 'Issues," and "Personalities." The "Events and Activities" sections cover some of Friedman's main engagements in economic debates during the particular years covered in the chapter. The "Issues" sections cover major policy or research issues with which Friedman was involved during the years in question -- including those mentioned at the beginning of this review, the area of exchange rate regimes being the major exception. The "Personalities" sections focus on specific individuals -- 20 individuals in total -- with whom Friedman interacted (or to whom Friedman reacted) on the particular policy identified in the "Issues" section. The reader of the book will encounter some distinguished economists in the "Personalities" sections, including Alvin Hansen, Paul Samuelson, James Tobin, Robert Lucas, and Thomas Sargent. The chronological account of the book is interrupted by a five-chapter exposition of Friedman's macroeconomic and microeconomic frameworks and his views on policy rules.

A substantial body of the book shows how Friedman's monetary economics can be made consistent with present-day macroeconomics. In what follows, I mainly focus on doctrinal issues related to the development of Friedman's monetary thinking.

2. The Road to Monetarism

2.1 Education and Early Employment

Friedman earned a Bachelor of Arts degree in 1932, at Rutgers University, where he came under the influence of his teacher, Arthur Burns, with whom he would form a close, life-long personal friendship. That relationship was temporally ruptured in the 1970s over a dispute about monetary policy after Burns had become Chair of the Federal Reserve Board; I discuss the nature of that dispute below. After his graduation

from Rutgers, Nelson considers Friedman's professional career path over the remainder of that decade to have had "convoluted elements" (vol. 1, 27), reflecting Friedman's quick succession of moves from one institution to another. Friedman undertook graduate studies in economics at Chicago in the 1932-33 academic year, earning a Master of Arts (AM) degree in 1933. He then did graduate work at Columbia University in the 1933-34 academic year, where he was influenced by Harold Hotelling, the mathematical statistician and economic theorist. He returned to Chicago for the 1934-35 academic year as a research assistant to Henry Schultz, a pioneering econometrician. From 1935 to 1937, he was employed by the National Resources Committee in Washington D.C., constructing estimates of consumer spending. Beginning in 1937, he began an association (that lasted until 1981) with the National Bureau of Economic Research. Friedman's work at the NBER in the late-1930s entailed assisting Simons Kuznets in the latter's work on professional income: from 1937 until 1940, Friedman worked with Kuznets as both the latter's "salaried assistant ... at the National Bureau and, in effect, Ph.D. student of Kuznets" (vol. 1, 69). Based on their work together, Friedman and Kuznets co-authored the 1945 book, *Income from Independent Professional Practice*, which Friedman submitted as a doctoral thesis at Columbia. He was awarded a Ph.D. from Columbia in 1946. During the 1940-41 academic year, Friedman held a visiting position at the University of Wisconsin.

The picture that emerges is that of a fledging, well-traveled economist who had received first-rate training in applied economics and statistical theory -- and had shown exceptional potential in those fields -- during a period of time that would see the Keynesian revolution sweep through the economics profession. By the mid-1940s, Friedman had become a theoretical statistician of the first rank, having published several important articles in that area. Together with L. J. Savage, in the late-1940s and early-1950s, he applied statistical analysis to the theory of choice under conditions of uncertainty. His work on statistical theory had a big payoff: in 1951, Friedman was awarded the John Bates Clark Medal for, as Nelson put it, having "built bridges between mathematical statistics and economics," and for having "used statistical theory to generalize the analysis of utility to a world of uncertainty" (vol. 1, 7). By 1951 Friedman had, reached what Nelson calls "the heights of the profession" (vol. 1, 176).

Amidst Friedman's rise in the 1940s, however, a transformation was taking place, slowly at first, in his research agenda. This transformation would initially have a

profound and negative impact on his professional reputation. As Nelson explains, after having been awarded the Clark Medal, Friedman's "standing in the profession was about to crash" (vol. 1, 176). What was the nature of that crash? Why did it happen? To shed light on these questions, we must take-up Nelson's account of Friedman's career path after he completed his one-year appointment at the University of Wisconsin.

2.2 The Initial Transformation

Friedman began working at the U.S. Treasury in 1941. According to Nelson, "prior to joining the Treasury, Friedman had largely accepted the theoretical contribution of the *General Theory*" (vol. 1, 88-89); and, Friedman "came to the US Treasury already having a Keynesian perspective" (vol. 1, 91). Nelson expresses the view that Friedman's employment at Treasury reinforced that perspective (vol. 1, 91-92).

The main issue that Friedman and his Treasury colleagues confronted in the early-1940s was to find a way to restrain aggregate demand in light of the large military build-up associated with the United States' entry into World War II, while, at the same time, facilitating the transfer of resources to the defense sector. The particular Keynesian perspective that Friedman and his Treasury co-workers adopted was that of Keynes's 1940 study, *How to Pay for the War*. That study addressed the excess demand conditions of a wartime economy from the perspective of the inflationary gap – that is, the excess of total nominal spending over the level that is consistent with price stability.

Nelson (vol. 1, 94) points-out that, under inflationary-gap analysis, fiscal deficits were "ipso facto a stimulant to aggregate demand." What mattered for inflation was the magnitude of the fiscal deficit. Moreover, "an accommodative monetary policy stance was *not* a key part of the sequence in which deficit spending generated an increase in nominal aggregate demand" (original italics, vol. 1, 94). Consequently, "lower deficit spending was crucial to restraining inflationary pressure in a way that monetary restraint was not" (vol. 1, 94). In the context of the inflationary-gap analysis, Friedman and his Treasury colleagues focused on ways of getting taxes to keep pace with the rises in government spending in order to contain inflation.

In May 1942, Friedman submitted a memorandum to the House Ways and Means Committee in which he dealt with ways to control inflation. To achieve that objective, Friedman argued that consumer spending would have to be restricted. The best way to

do that, he argued, was via income taxation. The other ways of avoiding inflation mentioned were through price controls and rationing, controls on consumer credit, a reduction in government spending, and selling war bonds to the public. Friedman, however, did not take a position on the effectiveness of price controls; as an employee of the Treasury, he would have been constrained in criticizing price controls. As Nelson (vol, 1, 96) points-out, Friedman did not view the reaction of monetary policy to deficit spending "as decisive in determining whether deficit spending raised the price level."

By the mid-1940s, however, Friedman had begun to alter -- if only modestly -- his views on Keynesian economics. Nelson (vol. 1, 121-22) notes that in a 1944 review of the book, *Saving, Investment, and National Income*, by Oscar Altman, Friedman put distance between himself and Keynesian income-expenditure theory. Friedman concluded that the book provided little evidence to support its main contention -- namely, that investment is the main dynamic variable that determines income and employment (vol. 1, 122). Similarly, in a statement -- which Nelson does not cite -- published in the *Congressional Record* on April 16, 1946, Friedman called for the elimination of price controls, imposed by the Office of Price Administration (OPA). He argued: "A major effect of OPA price controls has been to disguise rather than prevent price increases" (Friedman 1946, A2336). He also called on the control of the quantity of money to reduce inflation:

> We can and must take measures now to control the basic causes of inflation by limiting the supply of cash and bank deposits. This will require that Government collect as much and spend as little as possible; that we put the Federal Reserve System once again in control of the volume of cash and deposits by drastically raising reserve requirements and that we pin down the liquid assets in the hands of the public by a realistic debt policy, even if that means higher interest rates on the Federal debt (Friedman 1946, A2336).

After joining the economics faculty at the University of Chicago in the fall of 1946, Friedman's views on macroeconomic policies continued to change. Nelson singles-out the years 1948-51 as pivotal. He refers to Friedman's "epochal 1948-51 rethinking of monetary matters" (vol. 1, 40). Similarly, Nelson considers that the years 1948-51 marked "a shake-up in Friedman's thinking, especially with regard to monetary economics" (vol. 1, 32) and "from 1948 to 1951, he became a monetarist" (vol. 1, 295). The "shake-up" would initially diminish Friedman's standing in the economics

community because it marked Friedman's move away from Keynesian economics, with its de-emphasis on the role of money in the economy, and toward the adoption of the quantity theory of money (vol. 1, 32).

The "shake-up" saw Friedman embrace monetary economics as a primary field of interest. In 1948, he published the paper, "A Monetary and Fiscal Framework for Economic Stability" in the *American Economic Review*, in which he proposed a monetary rule for the first time. Under his particular rule, the stock of money would be increased when there was an increase in the budget deficit -- by the amount of the deficit. It would be decreased when there was a surplus in the budget – by the amount of the deficit. The budget would be balanced over the course of the business cycle or, alternatively, it would lead to a deficit that was sufficient to provide some specified secular increase in the quantity of money at a level of income corresponding to reasonably full employment (Friedman 1948a, 137). As Nelson (vol. 1, 141) points-out, the proposal required an estimate of the full-employment level of income as part of the process under which the cyclically-adjusted budget balance would be determined.

Thus, under the proposal, Friedman focused on the need to control the supply of money, using the government's fiscal position to do so. Nelson does not regard the proposal to be a monetarist one. He points-out that "the 1948 article's analysis is one that takes policy-induced changes in the quantity of money as having their effects on the economy via the fiscal-multiplier effect of deficit spending and not [as under Friedman's monetarist framework] via reactions of yields. The multiple-interest-rate channel of monetary-policy transmission that Friedman emphasized from the 1950s onwards is absent from this analysis" (vol. 1, 139). Nevertheless, Nelson views the 1948 paper as a move away from Keynesian analysis because it contained "an acknowledgement on Friedman's part that money-financed deficits had greater repercussions for spending than deficits financed by issuance of longer-term securities" (vol. 1, 139).

Nelson (vol. 1, 140-41) singles out two other features of the 1948 article that would play prominent roles in Friedman's subsequent work on policy rules. First, the article introduced Friedman's famous phrase "long and variable lags" into the economics literature. Second, Friedman noted that economic forecasters had established a poor

track record; therefore, it was preferable not to rely on forecasts in the area of policy formation.

On the basis of the above discussion, I believe that it is fair to argue that an *initial* “shake-up” in Friedman’s thinking on macroeconomics occurred between early-1940s and the publication of his 1948 *AER* article. What caused this initial “shake-up”? Part of the answer lies in one of Nelson’s “Personalities” sections -- namely, the one dealing with Chicago economist Henry Simons (vol. 1, 57-67).

While a student at Chicago in the early-1930s, Friedman did not take a course with Simons. Moreover, the two Chicagoans did not overlap in their teaching positions at Chicago -- Simons died unexpectedly a few months before Friedman began teaching at that institution in the fall semester of 1946. Nevertheless, the influence of Simons’s views on Friedman’s 1948 *AER* article was pervasive. By 1948, it was evident that Friedman had studied Simons’ policy views and had been heavily influenced by those views. As Nelson points-out regarding the influence of Simons on Friedman’s work: “A debt to Simons undoubtedly existed and was repeatedly acknowledged by Friedman” (vol. 1, 57). Friedman’s 1948 article contains numerous references to Simons’ views and cites three of Simons’ works. The similarities between Simons’ views and those of Friedman, 1948 vintage, are striking.[1]

- Both Simons and Friedman favored a policy rule under which the government’s fiscal position would be used to change the quantity of money with the aim of stabilizing the economy.
- Simons assigned a secondary role, at best, to open-market operations in producing changes in the quantity of money. Friedman favored the abolition of open-market operations.
- Both considered that fractional-reserve banking imparts an “inherent instability” to the financial system and was responsible for generating banking crises.

[1] For a discussion of Simons’s policy views, see Tavlas (2015).

- Both thought that the velocity of circulation of money is unstable.[2]
- Both Friedman and Simons advocated the imposition of 100 percent reserve requirements on demand deposits, and for the same reasons -- to reduce the frequency and severity of banking crises and to eliminate any slippages between actions by the monetary authorities on the monetary base and the behavior of aggregate commercial bank demand deposits.
- Both viewed short-term government securities and money balances as largely interchangeable assets.
- Simons and Friedman argued that the outstanding stock of government debt should be converted into consols.
- Both thought that Treasury bills should be eliminated.
- Both believed that that outstanding stock of consols should ultimately be eliminated, leaving the money stock as the only remaining government debt obligation.[3]
- Both were advocates of free markets.
- At a time that saw the overwhelming majority of American economists favor fixed or fixed-but-adjustable exchange rates, Simons and Friedman advocated flexible exchange rates.

Nelson shows that, beginning in the 1950s, Friedman would move away from Simons's policy playbook. According to Nelson, "Friedman's 1948 proposal ... proved to be the high-water mark of the agreement between his and Simons's framework" (vol. 1, 58). Friedman would drop his 1948 proposals that the issuance of Treasury bills be discontinued and that the existing stock be withdrawn from the market. Simons believed that a monetary rule could best be made effective in a highly-regulated financial system. Such a system would have prevented a wide range of financial transactions between borrowers and lenders and acted as a brake on the accumulation of capital. Friedman

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[2] Friedman's view that velocity is unstable was provided in an unpublished 1948 memorandum titled "Preliminary Plan for Completion of Data for Study of Monetary Factors in the Business Cycles" (Friedman 1948b).

[3] Nelson does not state that Simons favored the elimination of the outstanding stock of consols in the long term. For references to Simons's work in which he so-argued, see Tavlas (2023, chap. 5).

"had no interest in imposing wide-ranging additional restrictions on the terms of private lending and borrowing" (vol. 1, 61). By the late-1950s, he de-emphasized the importance of 100 percent reserves.

What caused Friedman to subsequently reduce the emphasis that he had placed on the 100 percent reserves scheme? Nelson convincingly argues that there were two main reasons. First, although Friedman was concerned about the instabilities in the credit-creation process, in the early-1950s he realized that the possibility of a key instability that had contributed to banking crises in the 1930s and earlier -- namely, that associated with changes in the deposit-currency ratio -- had been greatly reduced by deposit insurance, introduced in 1934. Second, in the 1950s Friedman became convinced that open-market operations could offset changes in the deposit-currency ratio and, thus, achieve the goal of maintaining the money stock (vol. 1, 59).

The upshot of the above discussion is that Friedman began his engagement with monetary economics in 1948 highly influenced by the work of Simons, although he would, over time, substantially modify the Simons playbook. That playbook was by-and-large also advocated by Lloyd Mints, who taught the graduate course on money at Chicago until his retirement in 1953. It is also likely that Friedman had been influenced by Mints; Friedman and Mints interacted extensively in the late-1940s and early-1950s, with the result that there had been considerable cross-fertilization of their respective views (Dellas and Tavlas 2021). In the mid-1940s, Mints and Simons' views on money -- including the advocacy of rules, the use of the government's fiscal position to generate changes in the money supply, 100 percent reserve requirements, and flexible exchange rates -- became known in the profession as "the Mints-Simons program" (Hansen 1946, 73). There was a reason why Mints and Simons had been identified with a particular program: each of those policy positions was an outlier within the economics profession. Taken together, the program was viewed as downright ideocentric. It was for that reason that, in 1951, Harry Johnson wrote that Simons and Mints were the leading members of "the Chicago radical school" (Johnson 1951, 382). Yet in the early-1950s, a third name became identified with that program -- that of Milton Friedman. As I discuss below, the person who made that identification was Clark Warburton.

2.3 The Big Transformation

Nelson shows that Friedman's monetary economics took on their monetarist character in the early-1950s. The distinctive characteristic that marked Friedman's move to monetarism was his embrace of the quantity theory of money. As Nelson (vol. 1, 153), argues "by 1950 he had cast his lot with the quantity theorists," a move which greatly diminished his standing in the profession.

Essential elements of Friedman's monetarism included the following. (1) By the mid-1950s, Friedman had articulated a comprehensive empirical critique of the Fed policies in the late-1920s and early-1930s. (2) Friedman came to believe that open-market operations should be the instrument of choice for generating changes in the quantity of money. He abandoned his fiscal-based monetary rule in favor of a three-to-five percent money-growth rule. (3) He came to believe that a fractional-reserve banking system is not "inherently" unstable although, as mentioned, he continued to support the 100 percent reserves proposal. (4) He produced empirical evidence showing that the demand for money is a stable function of a few variables. (5) Contrary to his views in the 1940s, he came to believe that fiscal policy is an ineffective stabilization tool although he believed in the effectiveness of the automatic fiscal stabilizers throughout the 1950s. (6) He developed the permanent income hypothesis, which is an early example of individuals solving a dynamic model to engage in forward-looking behavior. (7) Beginning in the mid-1950s, in the Chicago Workshop in Money and Banking, which he supervised, Friedman compared the empirical performances of the quantity theory and the Keynesian income-expenditure theory using empirical methods, with the result that the former was shown to be far superior to the latter in predicting nominal income (proxied by nominal consumption expenditure). Nelson discusses each of these changes in Friedman's framework in detail.

As Nelson explains, not all of the above-listed changes took place within the three-years, 1948-51. For example, Friedman's first public espousal of a constant-money-supply growth rule did not take place until 1956. Nevertheless, as Nelson also explains, there was enough of a change in Friedman's views by the early-1950s to account for an epochal rethinking. Nelson (vol. 1, 32) writes: "his move to monetarism came in light of his study of empirical evidence, as well as his reconsideration of a large body of literature, including that of monetary economists, like Fisher and Pigou." What was the nature of the empirical evidence on which Friedman could draw that led to Friedman's rethinking about the role of money in the economy?

A good place to begin concerns fiscal policy. Nelson (vol. 1, 296) points-out that "By the early-1950s ... Friedman's rating of the effects of fiscal policy was much diminished. He now saw deficit spending per se as not exerting a great influence on aggregate demand. But he was redoubled in his conviction that monetary policy had strong effects." As Nelson (vol. 1, 321) explains, "after 1948 Friedman became skeptical about the idea that fiscal policy actually had much of a distinct impact on aggregate demand. The empirical basis for this skepticism began to see print even in the 1950s, with Friedman's 1952 article on wartime monetary relations a particularly notable example" (vol. 1, 319). The article to which Nelson refers is Friedman's "Price, Income, and Monetary Changes in Three Wartime Periods." Based on his ongoing work with Schwartz, Friedman presented the paper at the December 1951 meetings of the American Economic Association; it was published in the *AER* in May 1952. Friedman provided empirical evidence on the determinants of inflation during the Civil War, World War I, and World War II. He found that price behavior in all three episodes was proximately explained by the stock of money per unit of output. It could not be explained by any other variable examined, including measures of fiscal policy.

In my view, although the results of the 1952 *AER* paper contributed to Friedman's emerging monetarism, that project could account for only a part of Friedman's changing views. For example, the results could not explain Friedman's switch to open-market operations conducted by the Fed as the preferred monetary instrument since the Fed did not exist in the Civil War and, once the Fed was established, it did not start experimenting with open-market operations until after World War I.[4]

What else, then, helped create the "shake-up" in Friedman's monetary thinking in 1951? To shed further light on this question, we must look to the work of Clark Warburton, another of the "Personalities" in Nelson's book. It has long been recognized that Warburton's views anticipated Friedman's. Recent research, published after Nelson finalized his text, suggests a *causal* connection. Lothian and Tavlas (2018) showed that throughout the course of 1951, Warburton and Friedman carried on a

[4] In 1953, Friedman published the English version of the paper "The Effects of a Full-Employment Policy on Economic Stability: A Formal Analysis," which had been published in French in 1951. Friedman showed that discretionary policies could add to the variance of income. The article supported the case for rules. Since Friedman had advocated a rule in 1948, I do not view that paper as having contributed to Friedman's change to monetarism, and neither does Nelson.

lengthy correspondence in which Warburton severely criticized Friedman's initial monetary framework. In Tavlas (2019), I showed that Warburton expanded his criticisms of Friedman's framework into two articles reviewing that framework, published in successive issues of the *Journal of Finance* in 1952 and 1953. Warburton began his 1952 article with the observation that Friedman's monetary framework bore a striking resemblance to the Simons-Mints platform: "It is similar to proposals for monetary reform developed during the past two decades by Professors Lloyd W. Mints and the late Henry C. Simons" (Warburton 1952, 328). In other words, Friedman's initial position in his engagement with monetary economics was that of the Chicago monetary tradition of the 1930s and 1940s.

In his 1951 correspondence with Friedman and in his *Journal of Finance* articles, Warburton's arguments included the following. (1) A monetary system in which banks' reserves are the liabilities of the central bank is not "inherently unstable," as had been argued by Simons, Mints, and Friedman. In such a system, the central bank can use open-market operations to offset shifts from deposits to currency and, thus, control the quantity of money by issuing its own liabilities. Simons, Mints, and Friedman had mistakenly analyzed the workings of a commodity-based monetary system in which the use of open-market operations is constrained by the central bank's holdings of commodity reserves. (2) The use of the government's fiscal position was far-too-unwieldy and unreliable as a mechanism with which to produce changes in the money supply. (3) Simons, Mints, and Friedman had failed to distinguish between the inherent characteristics of a monetary system and the way the system had been managed. The "inherent instability" that the Chicagoans had identified was, in fact, attributable to poor central-bank management, particularly in the early-1930s. (4) The Fed was responsible for initiating and exacerbating the Great Depression. (5) Friedman's policy rule was too complicated to be useful in practice. A far better rule was one under which the quantity of money increased by 4 percent a year. Earlier, in the 1940s, Warburton had compared the empirical performances of the quantity theory with an income-flow-based analysis of the Keynesian type, a comparison that presaged Friedman and Meiselman's work.

Nelson discusses the similarities between Warburton's views and Friedman's. With regard to Warburton's work comparing the Keynesian-type income-flow-based analysis with the quantity theory, Nelson writes: "Just as Friedman would argue subsequently, Warburton held that the money/spending relationship had proved more

resilient in wartime than the Keynesian consumption function" (vol. 1, 115). Moreover, Nelson points to the many similarities between Warburton's views and those of Friedman: "few cannot be struck by the extent to which Warburton anticipated Milton Friedman's work on matters relating to monetary policy. On the interpretation of the Great Depression, the advocacy of constant money growth, and several other matters, Warburton ... was taking a stand [in the 1940s] ... that Friedman would only take up after 1948" (vol. 1, 114).

Thus, Nelson, like previous writers, credits Warburton as having *anticipated* Friedman's views on money. On the basis of recent research, I believe that it is necessary to go beyond that characterization and conclude that Warburton *influenced* Friedman's views precisely at the time (1948 to 1951) that Friedman's views were undergoing an "epochal ... rethinking on monetary matters" (vol. 1, 40).

3. The Monetarist

As Nelson (vol. 1, 9) points-out, "Friedman wrote prolifically -- and yet he produced nothing that consolidated his views into a definitive statement." Friedman left "no monograph that could be regarded as a compendium of his monetary views." A central objective of Nelson's book is to present an encompassing picture of Friedman's monetary framework after Friedman became a monetarist. In doing so, Nelson presents a series of vignettes that describe and analyze Friedman's views on key issues relating to money. I provide an overview of several of those issues.

3.1 The Quantity of Money

Why did Friedman focus on the quantity of money, and not on the quantity of credit? Nelson explains that Friedman viewed "the demand function for credit as liable to exhibit considerable instability" (vol. 1, 62). Thus, a major reason for Friedman's advocacy of the 100 percent reserves system was that "it could automatically separate deposit creation from developments in the credit market" (vol. 1, 62). Friedman also thought that zero percent reserves could be a viable option. What was essential, in his view, was to make reserves uniform across deposits and keep them unchanged (vol. 1, 60). As long as the monetary authorities could conduct open-market operations, "even in a zero or fractional reserve-requirement regime, the option was open ... to carry out

open market operations on a scale sufficient to insulate the money stock from the volume of credit" (vol. 1, 62).

As Nelson (vol. 1, 242) points-out, Friedman's empirical work convinced him that "the money/output and money/prices relationships observed in the US data reflected the combination of a fairly stable money demand function and powerful effects of monetary-policy actions on total spending." Nelson also shows that Friedman thought that a "dichotomy" existed between movements in money and movements in bank credit. Unlike the demand function for money, "Friedman regarded the demand function for total credit as very unstable and not well understood" (vol. 1, 242). Several factors accounted for the unreliable relationship between bank credit and economic activity and prices. First, much of the provision of credit to the nonbank private sector was *not* intermediated through the banks -- for example, corporations often raised funds by issuing bonds. Second, the connection between bank deposits and bank credit was subject to slippage -- for example, the total volume of credit might be unchanged but deposits could increase because banks intermediated a larger share of the total amount of credit (vol. 1, 241-42).

3.2 The 1930s

An important reason for the profound influence of Friedman and Schwartz's *Monetary History* on professional thinking was its narrative about the Great Depression. Nelson describes the Friedman and Schwartz explanation of the origins of the Depression in terms "of the failure by the authorities to exercise control in the area that Friedman acknowledged as a legitimate field of government activity, namely, control of the money stock" (vol. 2, 37). Nelson argues that "the Federal Reserve's failure during much of the period 1929-33 consisted not of *initiating* the monetary contraction but of *failing to take steps to forestall the contraction*" in the face of a succession of banking panics in the early-1930s (original italics, vol. 2, 37). Those banking panics were characterized by a freezing-up of the credit market, large-scale deposit-to-currency conversions, and buildups of reserve balances by commercial banks, all of which produced money-supply contraction. The Fed not only failed to counter those effects through expansionary open-market operations but exacerbated the effects by raising its discount rate in the fall of 1931 following the departure of the pound sterling from the gold standard (vol. 2, 37).

Several points about Nelson's account of Friedman and Schwartz's critique of the Fed's performance are important to highlight. First, the Fed not only exhibited a passive posture with respect to the provision of bank reserves between 1929 and 1933, but exhibited a posture that was, in fact, what Nelson calls "perverse." Nelson (vol. 2, 39) points-out that total bank reserves declined during those years. In their *Monetary History*, Friedman and Schwartz referred to this decline in reserves but, according to Nelson, it was "probably not adequately emphasized" (vol. 2, 39). Second, during the Great Depression many Fed officials argued that policy was easy because nominal interest rates were low. In many of his works on the Depression, Friedman emphasized that policy was tight because *real* interest rates were high. This was one reason that Friedman thought that the money stock, and not nominal interest rates, was the appropriate indicator of monetary policy. This argument, however, was not highlighted in the *Monetary History*. Nelson writes: "one has to go to page 628 of the *Monetary* History for a statement to the effect that controlling interest rates, at the expense of interest in the money stock, had characterized the whole history of the Federal Reserve" (vol. 2, 42).

The third point concerns the reason for the Fed's inept behavior during the Great Depression. Friedman and Schwartz assigned a key role to the death of Benjamin Strong. Until his death in 1928, Strong had been the Governor of the Federal Reserve Bank of New York and the dominant figure in the Federal Reserve System. Nelson believes that the emphasis *Monetary History* assigned to Strong's absence for the policy errors of the early-1930s was misplaced: "The key role ascribed to Strong's death ... went against much of the spirit of Friedman's other work, which stressed the role that theoretical misperceptions played in producing policy mistakes" (vol. 2, 41). I return to this issue below.

Nelson shows that, although Friedman blamed the Fed for the severity of the Great Depression, he approved key banking reforms made by the Roosevelt administration in the early-1930s. These included the power given under the Emergency Banking Act of March 1933 to recapitalize the banking system, the introduction of deposit insurance in 1934, the latter, which, as mentioned, stabilized the currency-deposit ratio, and several actions with regard to international monetary arrangements in 1933-34 which loosened the link between gold and the dollar, thereby giving the Fed a greater scope to focus on the domestic economy.

Friedman's view of the supply-side reforms introduced by the Roosevelt Administration was a different matter: Friedman thought they had a negative effect on output, despite of a strong economic recovery that commenced in 1933, culminating in double-digit real output growth in 1936. Friedman attributed the recovery to a rapid rise of the money supply, beginning in 1933. Nelson (vol. 1, 46) raises the following question: could real output growth "have been improved on even if nominal income growth" had been unchanged? Nelson (vol. 1, 46) writes: "The debate on that question may be seen as a dispute regarding the implications for real growth of the supply-side measures of the Roosevelt administration" (vol. 1, 46). He compares the monetarist view of Friedman and Schwartz with the real business cycle interpretation and the New Keynesian view. The key results that emerge from this comparison are: (1) both the Friedman and Schwartz and the real-business-cycle accounts of the 1930s recovery conclude that real output would have been higher in the absence of the Roosevelt Administration's industrial policies (vol. 1, 51); (2) under the Friedman and Schwartz view, although nominal growth surged in 1934 and 1935, "a larger share of the observed nominal income growth could have taken the form of real growth and less the form of inflation ... if the US federal government's industrial policy had not pushed up the inflation component of spending growth" (vol. 1, 47); (3) like the real-business-cycle and the monetarist approaches, the New Keynesian account concludes that the industrial policies reduced potential output. However, because it was likely the case that expected path of the short-term interest rate was stable at a low level, the industrial-policy measures systematically raised the path of expected inflation, thereby stimulating demand and raising output.

3.3 The Transmission Mechanism

In his 1948 *AER* article, Friedman adhered to the belief of a direct relationship between changes in the money supply and changes in spending that operated through the real balance effect under which changes in money produced changes in wealth. During the 1950s, however, Friedman downplayed the empirical significance of the real balance effect. Instead of working through a wealth channel, Friedman came to the view that monetary policy operated by changing a wide spectrum of interest rates -- explicit and implicit rates. Nelson (vol. 1, 217-22) calls this the multiple-yield view of monetary transmission. Why, then, did Friedman focus on the quantity of money and not on interest rates in his discussions of monetary policy? One reason, as Nelson (vol. 1, 244)

points-out, is the following: “although it is interest rates and not money that appear in the IS equation, the money stock provides a convenient summary, or sufficient statistic, concerning these yields.” Friedman did not ascribe to the view that the transmission effects of monetary policy could be adequately captured in one or several interest rates. Another reason, mentioned in the above discussion on the stance of monetary policy during the Great Depression, was that Friedman’s incorporation of the Fisher effect into his monetary framework led to the conclusion that nominal interest rates are not a reliable indicator of the stance of monetary policy.

4. The Great Inflation and Friedman’s Public Choice View of Policy Making

On February 1, 1970, Friedman’s former teacher and close friend, Arthur Burns, became Fed Chairman. Friedman was delighted. Nelson (vol. 2, 322) quotes what Friedman wrote in a *Newsweek* column on February 2, 1970: “My close friend and former teacher Arthur Burns is not just another chairman. He is the right man in the right place at the right time.” Friedman added that Burns was the first Fed Chairman to have “the right qualifications for that post.” As Nelson documents, Friedman believed that “Burns subscribed to much of monetarist theory,” including the natural-rate hypothesis (vol. 2, 322). Friedman, however, would soon become disappointed with Burns. Nelson states: “The Federal Reserve under Burns eventually permitted very rapid monetary growth, so rapid in fact that double-digit inflation emerged. Indeed, once lags between monetary-policy actions and inflation are taken into account, *both* the mid-1970s and 1979-80 bouts of double-digit inflation can largely be attributed to ... Burns’ tenure” (italics in original, vol. 2, 324). Beginning in 1970, Burns advocated a cost-push theory of inflation and wage-price guidelines as a way of speeding up the response of inflation to demand restraint. By early 1971, Burns had come to argue that fiscal and monetary policies were not sufficient to control inflation; he began to favor compulsory wage-price controls set by the government, policies that Friedman viewed as ineffectual and detrimental to political freedom (vol. 2, 320-21).

Friedman was angered by the change in Burns’s policy position. Nelson convincingly shows that Friedman’s criticisms of price-wage controls in the 1970s and his argument that inflation could not be reduced by any means other than through monetary restraint, were, along with Friedman’s 1967 *AEA* presidential address

(Friedman 1968), in which he argued that the long-run Phillips curve is vertical at the natural rate of unemployment when inflation is fully expected, the major factors in converting a major part of the profession to the view that monetary policy is important.

Why did Burns change his policy position so radically in the early-1970s? Nelson (vol. 1, 324) argues that "A large part of the reason is that Burns changed sharply his economic theory of inflation soon after becoming [Fed] chairman," switching from a monetary theory of inflation to a cost-push theory. Nelson documents that the changes in Burns's theory of inflation and policy response to inflation led Friedman to send Burns a long, handwritten letter in May 1970, critical of Burns' statements on incomes policy (vol. 2, 324). According to Nelson, the *Philadelphia Inquirer* reported that "Burns was shaken by the letter" (vol. 2, 324). The switch in Burns's theoretical and policy positions led to a rupture in his relationship with Friedman that lasted at least for several years. As Nelson reports, even as late as 1978, by which time the relations between Friedman and Burns had improved, Friedman commented as follows in a *Wall Street Journal* article: "What I find hard to forgive is not so much what he did in monetary policy, but coming out for wage and price controls. I'II never understand why he did that" (vol. 2, 330). But, if Burns changed his policy position in the early-1970s because his economic theory of inflation changed, why did he change his theory? I discuss this issue below.

Before I do so, it is necessary to address a change that Nelson shows took place in Friedman's views around the time that the changes in Burns' theoretical and policy positions came about. The specific change in Friedman's views, I will argue, bore a direct relationship to the change in Burns's position on inflation. Nelson does not address that change in conjunction with his discussion of Burns, the latter which is a subject covered in volume 2 of Nelson's book. The specific change in Friedman's view to which I now turn is covered in volume 1.

Nelson writes: "Friedman's articles and statements concerning monetary policy from the mid-1970s onward heavily reflected his admiration for, and acceptance of, the analysis of the decisions of the political system and bureaucracies contained in the 'public-choice' economic-research literature" (vol. 1, 321). Whereas in his critiques of the Fed's historical performance in his works prior to the mid-1970s Friedman had "focused heavily on flaws in the Federal Reserve's analytic framework as a source of

historical policy errors," from the mid-1970s Friedman came to the view that Fed officials operated on the basis of prestige-seeking and accountability-avoiding objectives (vol. 1, 323). Nelson (vol. 1, 322-23) quotes from a 1987 letter from Friedman to Stanley Fisher in which Friedman commented on the policy makers' loss function.

> From revealed preference, I suspect that by far and away the two most important variables in [the Fed's] loss function are avoiding accountability on the one hand and achieving public prestige on the other. A loss function that contains those two elements as its main argument(s) will I believe come far closer to rationalizing the behavior of the Federal Reserve over the past 73 years.

Nelson shows that, not only had Friedman come to believe in the mid-1970s that Fed policy makers operated on the basis of prestige-seeking and accountability-avoiding objectives, but he came to the view that, in striving to achieve these objectives, the Fed chair was heavily reliant on the views of the Fed's staff -- to such an extent, in fact, that "the identity of the Federal Reserve chair made little difference to the conduct and objectives of US monetary policy" (vol. 1, 323).

Nelson dismisses Friedman's public-choice perspective of monetary-policy decision making as follows: "The public-choice perspective also sheds little light on the reason for the inflation of the 1970s, because (in seeing the inflation as a conscious policy decision) it attributed to policy makers of the 1970s a view of the inflation process that they did not have" (vol. 1, 324).

Several comments are warranted. First, in his work in the 1950s and 1960s, Friedman left open the possibility that, under a discretionary regime, the monetary authorities were susceptible to political pressures. Thus, one argument that he made in support of monetary-policy rules was that discretionary policies "meant continual exposure and reliance of the authorities to political and economic pressures and to the deceptive effects of short-lived tides of events and opinions" (Friedman 1960, 85). Second, as mentioned above, in their *Monetary History*, Friedman and Schwartz attributed the Fed's poor performance during the Great Depression to the absence of an individual -- Benjamin Strong – from the scene, and not exclusively to flaws in the Fed's analytic framework. Friedman and Schwartz (1963, 692) wrote:

> We are inclined to believe that the particular course of action followed by the Reserve System owed less to the climate of opinion – though it was certainly a

> necessary condition – than to a sequence of more or less accidental events and the running conflict for power within the System. Benjamin Strong's death in 1928 unleashed an active phase of conflict which dominated policy throughout 1929, producing a deadlock between the Board and the New York Bank – acting as leader of all the Banks – about the proper policy to adopt in face of the stock market boom. The result was a policy that, in our view, was too easy to break the bull market and too tight to permit vigorous business expansion.

Friedman and Schwartz (1963, 692) went on to argue that, if Strong had lived another year, the depression would have ended in 1930.

Third, I do not believe that it is a coincidence that Friedman's public-choice perspective on the Fed's decision making was fortified at the same time that he witnessed the abrupt change in the Burns's policy position. Friedman saw first-hand his long-time mentor and friend, Burns, undergo an abrupt shift from a monetarist view of inflation to advocacy of price and wage controls. Friedman surely thought deeply about the reasons leading to that sudden change in Burns's thinking.

What were those reasons? Upon assuming the position of Fed chair in 1970, Burns had been outside of the economic research arena for some 20 years. In those years, macroeconomic policy research had undergone a profound change: large-scale Keynesian macroeconometric models had become the dominant tool used in policy circles. Those models were built with the aim of simulating the effects of alternate policies on key economic variables. The fact that the typical models of the early-1970s contained hundreds of equations added to their complexity and mystique. In the early-1970s, the Fed was a bastion of large-scale model construction and use. The main academic institutions that constructed those models were the University of Pennsylvania and MIT. The model used at the Fed was known by the abbreviation, MPS model, reflecting the academic affiliations of its key developers -- Franco Modigliani (MIT) and Albert Ando (University of Pennsylvania), and the organization (Social Science Research Council) through which Federal Reserve support for the project was channeled. To those who were not familiar with those models but who needed policy advice, including the decision makers at the Fed, there was strong pressure to rely on the guidance of the staff that operated those models.

The fourth comment, which supports the preceding remarks, has to do with my experience. I began my professional career in the late-1970s at the State Department, which is located across the street from the Fed's Board of Governors. My first work

assignment was to learn to operate and build large-scale Keynesian macroeconometric models. I became well-acquainted with a number of modelers and other economists who worked at the Fed. Many, perhaps most, of those modelers had Ph.D.s from MIT, Yale, and Penn.[5] The models used by the Fed in the 1970s provided a very different policy perspective than the monetarist view of the world. Even in the late-1970s, versions of those models continued to show a long-run Phillips curve trade-off, while expansionary fiscal policies, unaccompanied by monetary accommodation, had long-run effects on output.[6] The early-1970s Keynesian models showed that monetary policy had weak effects compared with fiscal policy. In describing the structure of the MPS model as of the mid-1970s, Ando and Modigliani compared that structure to that of an economy on a "golden-age growth path." Ando and Modigliani (1975, 539) wrote:

> Fiscal policies, by influencing the savings-income ratio, by inserting a wedge between the rates paid by borrowers and received by lenders, by determining the size of government debts, and by a number of other means, will have important impacts on characteristics of the long-run behavior of the economy. Monetary policies, on the other hand, will not have very substantial impacts other than to determine the level of wages and prices and, perhaprs, if one believed that the Phillips curve retained its importance in the long run, the level of unemployment. With a few minor exceptions, these statements all apply to the MPS model.

A typical result of those Keynesian macroeconometric models was that expansionary monetary policy *lowered* inflation -- because lower interest rates reduced the cost of capital, a reduction which fed into price equations. For someone untrained in macroeconometric modeling, and who had been away from academic research for 20 years, Burns was susceptible to the views and advice of his staff.[7] Friedman saw this happen first-hand. He had good reason to believe that the Fed chair had been influenced by the views of the Fed staff. Friedman had good reason to adopt a public choice perspective of monetary-policy decision making.

[5] Several of these economists became my collaborators and life-long friends.

[6] These characteristics were part of the Fed's multicountry model. A book describing the model was published in 1984. See Stevens *et al.* (1984). I reviewed the book in Tavlas (1984).

[7] For a recent discussion of the role of the Fed staff on decision-making, see Kuvvet (2022).

5. Conclusion

Friedman never provided a fully-articulated statement of his monetary framework. Nelson does just that and much more, narrating a comprehensive doctrinal account of the development of Friedman's framework while placing Friedman's monetary contributions within the context of the modern literature. It is fair to say that Nelson has produced the definitive study of Friedman's monetary economics. The book is a remarkable achievement.

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