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CHAPTER 10

MILTON FRIEDMAN'S MONETARY ECONOMICS

Theory and Empirics

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10.1 INTRODUCTION

"THERE is of course no sharp line between the empirical scientist and the theorist—we are dealing with a continuum, with mixtures in all proportions, not with a dichotomy." Milton Friedman wrote those words in an article on Wesley C. Mitchell, his mentor at the National Bureau of Economic Research (NBER) (Friedman 1950: 465). In the very next breath, Friedman, borrowing from Alfred Marshall, went on to say, "The most reckless and treacherous of all theorists is he who professes to let facts and figures speak for themselves" (ibid.). Then he added: "[And] . . . the most reckless and treacherous of all empirical workers is he who formulates theories to explain observations that are the product of careless and inaccurate empirical work" (ibid., 465–6).

Friedman did not say it in the article, but the motivation for these statements and for the piece on Mitchell more generally was almost certainly as a response to Tjalling C. Koopmans's characterization of Arthur Burns and Mitchell's *Measuring Business Cycles* as "measurement without theory" (Koopmans 1947). Friedman had worked with Burns and Mitchell. He had an intimate knowledge of their abilities. He often used the same tools of data analysis as they and he was interested in many of the same questions. He respected both men.

The Walrasian approach to theory and the formalist (and frequentist) approach to econometrics favored by model builders like Koopmans were things that Friedman found intellectually uncongenial and sterile. Indeed, the sentiments Friedman expressed in the article on Mitchell might easily have formed the basis of a response to some of the later critics of his work. For Friedman's own approach to economic analysis was influenced by Mitchell, and was otherwise quite different from what was

and is standard in much of economics.[1] That difference was a continual source of criticism.

On the level of theory, Friedman was Marshallian; on the level of empirics, he used methods akin to those used in the physical sciences, but with strong Bayesian overtones. I focus on three bodies of Friedman's work in particular in this chapter—his theory of the consumption function, his work on monetary dynamics and the Phillips curve, and his broader work in money/macro, in particular the historical work with Anna Jacobson Schwartz. His contributions in all three areas are among his most enduring intellectual legacies. I also draw on my own observations of and interactions with Friedman. My contact with Friedman began as a graduate student at Chicago in autumn 1967 and continued at Chicago through spring 1972, first in the courses he taught in monetary economics and macroeconomics, and later in the Workshop in Money and Banking when I was a dissertation student. Thereafter he and I had occasional direct contact and some indirect contact via our joint research interests.

10.2 Friedman's Approach to Price Theory and Empirical Research

For over three decades, the watchword in macroeconomics has been "microfoundations." This is as it should be. In principle, there should not be any divorce of macroeconomics from microeconomics, but the very fact that there has been this terminological dichotomy suggests that, in practice, such a divorce has existed.

As a graduate student in the late 1960s and early 1970s in Chicago, I had a quite different experience. Price theory—microeconomics—was, to use Marshall's phrase, a phrase oft repeated by Friedman, the "engine of analysis." This was true not only on the level of the individual, the firm, and the industry, but also on the level of the economy as a whole. On the macro level, the quantity theory of money, viewed as a play between the demand and supply of money, and growth theory, were the major tools. Both were heavily price-theoretic. To a Chicago-educated economist of my generation and the generations immediately preceding mine, therefore, microfoundations were always there.

One of the reasons this was so, I believe, is due to the role that Milton Friedman played as an intellectual leader at Chicago. Friedman was not only a monetary economist par excellence but, perhaps first and foremost, a price theorist par excellence. Price theory to Friedman was not a game to be played by clever individuals but a tool for understanding and making sense of the real world. I saw this in the two classes that I took with him—Economics 331: Money, and Economics 332: Macroeconomics,

[1] See Rockoff (2010) for an interesting and highly informative discussion of the influence of Mitchell and NBER methods on Friedman and Schwartz.

and, probably because the discussion was more freewheeling, even more so in meetings of the Workshop in Money and Banking.

Friedman summed up his views on price theory relatively early on in an article on William Jaffé's translation of Léon Walras's *Elements of Pure Economics* (Friedman 1955: 908–9):

> The large and substantial immediate rewards from Walras' concentration on form; the prestige and intellectual appeal of mathematics; the difficulty of making experiments in economics and the consequent laboriousness and seeming unproductiveness of substantive work devoted to filling in our analytical filing boxes—all these have combined to favor the Walrasian emphasis on form, to make it seem not only an essential part of a full-blown economic theory, but that economic theory itself. This conception—or misconception—of economic theory has helped to produce an economics that is far better equipped in respect of form than of substance. In consequence, the major work that needs now to be done is Marshallian rather than Walrasian in character—itself a tribute to Walras' impact.

He went on to conclude: "A person is not likely to be a good economist who does not have a firm command of Walrasian economics; equally, he is not likely to be a good economist if he knows nothing else" (ibid., 909).

Much of Friedman's early published work actually was in price theory (Friedman 1935, 1949; Friedman and Kuznets 1945; Friedman and Stigler 1946) as well as in mathematical statistics (Friedman 1937; Freeman, et al. 1948), and in combinations of those two disciplines (Friedman and Savage 1948; Friedman 1953). Allen Wallis, in a paper (1980), and in an interview with Ingram Olkin (1991), discusses Friedman's contributions to statistics, crediting him, among other things, with the intuition behind sequential analysis. The Friedman and Kuznets book and Friedman and Savage article became classics. Friedman's work in money/macro came later.

Prior to my time in Chicago and not long after I left in spring 1972, Friedman taught the price theory sequence in the doctoral program. It is interesting to listen to and read what his former students had to say about those courses.

Gary Becker, in his "Milton Friedman as a Microeconomist," wrote that "[Friedman's] most famous course at Chicago was by far the two-quarter graduate sequence on price theory. Bob Lucas, I, Sherwin Rosen, Gene Fama, and legions of others who went through Chicago to study economics while he taught there remember that course as having a major impact on our approach to economics" (Becker 2007: 181). The course, Becker went on to say, was quite different from what was typical then and what is typical today.

In this connection, Sam Peltzman's (2013: 206) précis is particularly interesting:

> Along with perhaps 50 other wet-behind-the-ears new Chicago graduate students I trooped into my first class—Milton Friedman's Price Theory course. All of us eagerly awaited our inauguration into the deeper technical mysteries of our chosen profession. Instead we got an extended paraphrase of an essay entitled "I, Pencil," in which a humble pencil tells us of the herculean coordination problem required to

> get itself produced and distributed and of the virtues of markets in solving that problem. The technical level of the course did not subsequently rise very far above this. Friedman believed that the best economics consisted of applying some uncomplicated and basic insights to important real-world problems.

As Becker put it:

> Friedman did not draw a sharp distinction between micro and macro in terms of the tools of economics to be used. He went rather seamlessly from one to the other, as in his discussion of the returns on education, the consumption function, or the natural rate of unemployment . . . Closely related, [he] did not conceptually draw a distinction between partial and general equilibrium analysis. So-called partial equilibrium analysis of, say, the demand for cars took account of effects in other markets . . . This is what I call "empirically driven" general equilibrium analysis. (Becker 2007: 181)

In Friedman's schema, there was a strong interplay between theory and empirics. Theory was not something to be studied *in vacuo*. It had a strong empirical bent. Empirical investigation, for its part, needed to draw on theory to be meaningful. Friedman's 1950 article "Wesley C. Mitchell as an Economic Theorist" that I cited early in section 10.1 is especially revealing in this regard: "There is of course no sharp line between the empirical scientist and the theorist—we are dealing with a continuum, with mixtures in all proportions, not with a dichotomy" (Friedman 1950: 465). He then turned to Mitchell himself:

> [Mitchell's] empirical work is throughout shaped by a thorough knowledge of existing theory and directed toward the construction of a better theory. It is always analytical, never aridly descriptive. His theoretical work is throughout interwoven with his empirical work and made a part of an "analytic description" of the phenomena under study. (ibid., 466)

These statements are completely consonant with Friedman's own approach to economics and with the approach that he instilled in his students. He viewed scientific investigation, as he was wont to describe it, as "a series of successive approximations." Theory provided the backdrop for empirical investigation, with the results of such investigations in turn feeding back on the theory and leading to its refinement. Such a process is evident in Friedman's work on consumption, on the Phillips curve, and in monetary economics and macroeconomics more generally.

Friedman and Schwartz (1970: 91) provide a very clear statement on how they saw this process working out in practice:

> The problem is one that is common in scientific work. A preliminary decision—in this case, on the definition of money—must be made. Yet the decision can be made properly only on the basis of the research in which the preliminary decision is to be used. Strictly speaking, the "best" way to define money depends on the conclusions that we reach about how various monetary assets are related to one another and to other economic variables; yet we need to define "money" to proceed with our research. The solution, also common in scientific work, is successive approximations.

With regard to Mitchell's own approach to theory, Friedman (1950: 467) added:

> To Mitchell, economic theory was more than orthodox economic theory. It was a set of hypotheses explaining economic behavior in all its leading manifestations, and he was himself almost exclusively concerned with a part of economic theory that was largely outside the main stream of economic thought when he began his scientific work and that even today is least developed and least satisfactory—the dynamic adjustment of the economic system as a whole. Because we know so little about this part of economic theory, we tend to neglect it in thinking about economic theory, to use the term to cover what we have, rather than what we ought to have. This circumstance, I think, partly accounts for the widespread illusion that Mitchell was antitheoretical, or at least not concerned with "economic theory"; for Mitchell's work was consistently and almost exclusively devoted to the development of a theory of economic change.

This last statement, oddly enough, turned out to be rather prophetic. For Friedman too had a strong concern with the dynamic adjustment of the economic system as a whole. A good deal of his work on the subject met with a similar reaction to that accorded to Mitchell's. This is certainly the case with the two articles that Friedman published in the early 1970s on monetary dynamics (Friedman 1970, 1971a). These two articles also appeared in combined form in an NBER occasional paper (Friedman 1971b). The next year, the *Journal of Political Economy* published critiques of this work by Karl Brunner and Alan Meltzer (1972), Paul Davidson (1972), Don Patinkin (1972), and James Tobin (1972), along with a lengthy reply by Friedman (1972). The four critiques, along with Friedman's original articles and his reply to the critics, were reprinted in a 1974 book edited by Robert J. Gordon. Friedman and Schwartz (1982a: ch. 2) contains a later discussion of the adjustment issue with references to and discussions of work published in the intervening period.

The critics, for the most part, ignored Friedman's outline of the short-run adjustment process, which was a new contribution, and directed their comments to a host of other, lesser issues. Friedman in his reply said he was "baffled" by some of the points the critics raised. I have had very much the same feeling. The key question, I would have thought, was whether Friedman's characterization of the adjustment process was useful—whether it was accurate enough in its broad details that it could fruitfully serve as an engine of analysis and a building block for further empirical work. What makes the reception of that body of work even more difficult to fathom is that it had little in the way of competition at the time. The reigning IS-LM paradigm was all about comparative statics. Dynamic adjustment was not part of the picture at all.

Although Friedman did not use Bayesian econometric tools, his empirical approach was at heart Bayesian (Pelloni 1987). He was wary of multiple regressions and the uses to which they often are put. He was skeptical of multi-equation econometric models. He preferred instead to look at the data from a variety of perspectives and take the weight of the evidence as a whole. He viewed conventional hypothesis tests as devices that he could use "to calibrate [his] own internal probability calculator," as he more

than once phrased it in the Workshop in Money and Banking. The only true test, in Friedman's view, was replication using a different body of data.

Friedman traced his views on these issues to his experience as a member of staff of the Statistical Research Group (SRG) of Columbia University during World War II. In an appendix that Friedman wrote to "Alternative Approaches to Analyzing Economic Data" entitled "A Cautionary Tale about Multiple Regressions" (Friedman and Schwartz, 1991: 48–9) he tells a story that I had heard him relate in the Workshop in Money and Banking much earlier. As part of his research at the SRG, he constructed two new alloys for use in aircraft engines, based on a regression model that he estimated on data on existing alloys and their characteristics. The model predicted that the two new alloys would take several hundred hours to rupture at high temperatures. In actual fact, they did so in four hours and less when tested in the lab. Friedman wrote: "Ever since, I have been extremely skeptical of relying on projections from a multiple regression, however well it performs on the body of data from which it is derived; and the more complex the regression, the more skeptical I am" (Friedman and Schwartz, 1991: 49).

Friedman's approach to empirical analysis was distinctive in other ways too. We can get some inkling of this in an interview that John Taylor conducted with Friedman that was published in 2001. The interview is instructive, for it highlights the difference between Friedman's take on data analysis and what is standard practice in many parts of the economics profession.

Taylor asked Friedman to describe his views about the advantages and disadvantages of different approaches to time-series analysis. Friedman replied as follows:

> I think the major issue is how broad the evidence is on which you rest your case. Some of the modern approaches involve mining and exploring a single body of evidence all within itself. When you try to apply statistical tests of significance, you never know how many degrees of freedom you have because you're taking the best out of many tries. I believe that you have a more secure basis if, instead of relying on extremely sophisticated analysis of a small fixed body of data, you rely on cruder analysis of a much broader and wider body of data, which will include widely different circumstances. The natural experiments that come up over a wide range provide a source of evidence that is stronger and more reliable than any single very limited body of data. Let me put it another way. I don't believe that we can possibly understand enough about the economy as a whole to be able to predict or interpret small changes. The best we can hope for is to be able to understand significant larger changes. And, for that, you want a wide body of data and not a narrow body of data. If you have a complex model and then try to extrapolate outside of that model, it will not be very reliable. I learned that lesson very well while I was at the Statistical Research Group [during World War II]. (Friedman quoted in Taylor 2001: 121–2)

In a subsequent paper, which was actually a critique of the Taylor rule, Friedman made a similar point (Friedman 2010: 116): "Three or four estimated equations are crucial for the Taylor economic model," he said, "but the economy as a whole is determined by millions of equations. At most, we could hope to get a rough picture of it."

10.3 Consumption

We see Friedman's method of analysis laid bare in *A Theory of the Consumption Function* (Friedman 1957). He took an existing body of empirical results as his starting point. Friedman combined that with insights from his and Simon Kuznets's study of professional incomes, and with statistical theory and price theory, to construct a model of consumer behavior that was consistent with the existing results. He then confronted the model with a broad body of data, placing "a major emphasis on the consistency of results from different studies and [covering] lightly a wide range of evidence rather than [examining] intensively a few limited studies," as he put it in the preface to that work (Friedman 1957: x). In the penultimate chapter of *A Theory of the Consumption Function*, entitled "A Miscellany" (Friedman 1957: 200–19), Friedman outlined a series of additional tests that other researchers could use to test the implications of the theory.

In *A Theory of the Consumption Function*, Friedman was able to reconcile the apparently conflicting evidence obtained with cross-section and time-series data. In so doing, he made the concept of permanent income and the importance of expectations and forward-looking behavior common coin in economics. He also showed the flaws in the Keynesian consumption function and the multiplier.

A Theory of the Consumption Function has been widely praised. Arnold Zellner, in the class that he taught in econometrics at Chicago that I took in 1970, described it as the best empirical study in economics. Zellner, in a later interview in *Econometric Theory*, stated: "It's the only work in economics, I think, in which an author has made a list of predictions and told others how to perform the analyses and predicted the outcomes" (Zellner quoted in Rossi 1989: 297). Alan Walters (1987: 424) lauded the book for its "incomparable amassing, organization and interpretation of the evidence." He went on to say: "One of the great contributions of this book was to give a new standard for empirical economics generally. Clearly this was how it should be done" (ibid.). Finally, in a 2004 retrospective review, Costas Meghir wrote that:

> Most important discoveries and insights are simple, economical, have important implications for a broad range of issues and withstand the test of time. Moreover, they generate large amounts of research, verifying it and refining it. This is exactly the case with Friedman's PIH [Permanent Income Hypothesis]. At the end of all this, the original idea has not only survived but has formed the basis for developing a coherent analysis of consumption and savings. As such it will always be remembered as a key turning point in the development of economic science. (Meghir 2004: F305)

Friedman himself claimed that: "*A Theory of the Consumption Function* . . . comes closer than anything else that I have written to adhering faithfully to the precepts of my essay on methodology. That is one, but by no means the only, reason why I have long regarded it as my best purely scientific contribution, though not the most influential" (Friedman and Friedman 1998: 222).

Two bodies of Friedman's other work that have indeed proved influential and have stood the test of time are his work on the Phillips curve and his coauthored work with Anna Jacobson Schwartz in monetary history, particularly *A Monetary History of the United States, 1867–1960*.

10.4 The Phillips Curve

Central bankers have traditionally used nominal interest rates both as indicators of the degree of monetary tightness or ease and as levers with which to manipulate policy. That was true in the 1960s and it is equally true today. To predict inflation, they have generally used Phillips curve-type relations. Again, that was true in the 1960s and it is also true today. What is different now, however, is that the distinctions between nominal and real magnitudes and between actual and expected values of variables are no longer simply being swept under the rug the way they had been in the past.

What changed things around was the work of Milton Friedman, in particular his Presidential Address to the American Economic Association (AEA) (Friedman 1968). In it he argued that attempts by central banks to peg either nominal interest rates or unemployment rates would only be effective for very limited periods. Both, Friedman argued, involved a failure to take account of differences between nominal and real magnitudes—nominal and real interest rates on the one hand, and nominal and real wages on the other—as well as a failure to take account of differences between the short-run and long-run effects of monetary changes.

The key distinction here was between actual rates and natural rates, the rates consistent with long-run equilibrium. Trying to maintain the actual rate of unemployment below the natural rate might be successful in the short run, albeit at the expense of higher inflation. But, as expectations began to adjust to that now higher inflation rate, unemployment would begin to rise and eventually return to the natural rate. Continued over time, such a policy would lead not just to higher, but to accelerating and, in the limit, explosive inflation, with no salutary effect on employment. A similar outcome could be expected, Friedman argued, from attempts by the monetary authorities to peg the nominal interest rate below its natural level, and for much the same reasons. As inflation rose and expectations began to adjust, nominal interest rates would begin to rise. Attempts to push them back down would necessarily involve higher rates of monetary expansion and higher inflation still.

In the class that I took from Friedman in 1968, and in a subsequent paper for the Institute of Economic Affairs (IEA) (Friedman 1975), Friedman fleshed out this analysis a good deal more in terms of price theory than in his AEA Presidential Address. He provided microfoundations for this bit of macroeconomic analysis in a way that was then largely lacking in macroeconomic analysis. Coupled with Edmund Phelps's (1967) article on the Phillips curve, which reached very similar conclusions

to Friedman's, macroeconomic thinking was completely altered by "The Role of Monetary Policy."

Well after the fact all of this seems quite simple. One might be tempted to ask what was the big deal? At the time, however, it was a big deal. But, it took time before Friedman's analysis gained widespread acceptance. The IS-LM model was close to being totally dominant in macroeconomics. It was and is a static model, and in its conventional form it ignores nominal and real distinctions. Friedman brought dynamics and forward-looking behavior back into the picture, along with the distinction between nominal and real magnitudes.

He also made a rather bold conditional forecast about the path that inflation and unemployment would take. As it turned out, that forecast all too soon was proven true by events. In both the UK and the USA, inflation ratcheted up on a longer-term average basis between the mid-1960s and the early 1980s, as the monetary authorities in both countries continually pursued "full-employment policies." In the UK, in which this process started earlier and at a somewhat higher average rate of inflation, the peak in the average inflation rate, as Friedman's analysis also implied, far exceeded that in the USA. We can see this clearly in the second and fourth columns of Table 10.1. The one departure from Friedman's theoretical account is that average rates of unemployment in both countries did not simply return to a relatively stable level, as might have been expected based on a slowly changing natural rate of unemployment, but rose noticeably along with the average rates of inflation. We see this in the third and fifth columns of Table 10.1. The expectations-augmented Phillips curve appeared to shift not just vertically but also upwards to the right.

In his Nobel lecture, Friedman offered a "tentative hypothesis" with regard to why this was so (Friedman 1977a). High inflation, he argued, generally goes hand in glove with more volatile inflation. This increased volatility, in turn, gives rise to increased uncertainty. He pointed to the Latin American experience documented by his

Table 10.1 Quinquennial averages of monthly inflation rates and unemployment rates UK and USA, 1955–84

Period	UK		USA	
	Inflation	Unemployment	Inflation	Unemployment
1955–9	3.0	1.6	1.9	5.0
1960–4	3.0	1.8	1.2	5.8
1965–9	4.2	2.0	3.8	3.8
1970–4	10.2	2.3	6.4	5.5
1975–9	14.3	3.8	7.9	7.0
1980–4	8.1	8.9	6.3	8.5

Note: All figures are per cent per annum. Inflation figures for the UK are for retail prices and for the USA for consumer prices.

Sources: Federal Reserve Bank of St. Louis and Bank of England.

colleagues Arnold Harberger and Larry Sjaastad as a case in point, and drew both on their analyses of growth and inflation in Latin America and Hayek's analysis of the informational role of prices (Hayek 1945) to provide a theoretical rationale for his empirical observations. "This uncertainty—or more precisely, the circumstances producing this uncertainty—leads to systematic departures from the conditions required for a vertical Phillips curve," Friedman argued (1977a: 465). Predicting both the longer-term drift in inflation and its shorter-term movements becomes much more difficult. The optimal length of contracts, therefore, shortens, and indexing becomes more prevalent, both of which reduce economic efficiency compared to a world where prices are more stable. More important, agents' ability to distinguish between changes in relative and absolute prices decreases. The role of the market-price system in coordinating economic activity and transmitting information becomes impeded, thus reducing economic efficiency further. Friedman opined that both of these factors very likely also had negative effects on employment.

Much of the commentary on Friedman's critique of the Phillips curve has stressed his use of adaptive expectations and juxtaposed this against the later rational expectations approach. Carl Walsh's short article, "Nobel Views on Inflation and Unemployment," is an example (Walsh 1997). Walsh writes:

> While Friedman also stressed that the real effects of changes in monetary policy would depend on whether they were anticipated or not, Lucas demonstrated the striking implications of assuming that individuals form their expectations rationally. Lucas abandoned Friedman's notion of a gradual adjustment of expectations based on past developments and instead stressed the forward-looking nature of expectations.

Friedman's thinking on the subject of expectations formation was, however, a good deal more nuanced than such a description suggests. This was clear early on in his discussion of permanent income in *A Theory of the Consumption Function*. It was also brought out forcefully in discussions in class and in the Workshop in Money and Banking. One episode in particular sticks in my mind. This took place in early 1970. One of my fellow graduate students was interested in reinvestigating the hyperinflation experience that Philip Cagan had studied in his classic article on the subject (Cagan 1956). The student wanted to apply a more flexible functional form to the data than Cagan's geometrically declining weighted average of past inflation rates. His object in doing so was to fit the endpoints, something Cagan's proxy was unable to do. Friedman was not encouraging. He told him that Maurice Allais had already done that (Allais 1966) and there would be no value added from another such attempt. He suggested instead that the student make use of outside information in modeling expectations, specifically information on government budget deficits. Such information, Friedman argued, must have been available in the newspapers and very likely would have been a major input in agents' forecasts of money growth and hence inflation. Friedman suggested he do this for one or a few of the episodes that Cagan had studied. The student came back with a revised proposal in the fall in which he drew on Muth's work,

but, as I recall, had no new data. For one reason or another, he never pursued the research further.

10.5 *A Monetary History* and Related Studies

Friedman and Schwartz's *A Monetary History of the United States, 1867–1960* (1963a) was published in 1963 and is still in print a half century later. Chapter 7 of that work, "The Great Contraction," was issued separately as a standalone volume two years after the publication of the book and was recently reissued in a second edition.

Friedman and Schwartz's original mandate was to study the role of money in business cycles. At the start of the project in 1948, Friedman, who had worked at the NBER with Simon Kuznets and also with Arthur Burns and Wesley Mitchell, was matched with Schwartz, who was a member of the Bureau research staff and had a good working knowledge of US monetary data. The *Monetary History* started out as a short background study to the statistical research that they had planned, but eventually it took on a life of its own.

The match between Friedman and Schwartz was indeed fortuitous. The two proved to be near perfect complements. Friedman by himself might have written a fine work on monetary economics informed by economic history. Schwartz by herself might have written a detailed history informed by theory. Together they produced a *magnum opus*. What helped greatly is that the two also were first-rate wordsmiths. The *Monetary History* could easily have been a chore to read. It is anything but. We economists, like other people, economize on our time. A book of similar length, the principal focus of which was on detailed mathematical models and tables of econometric results, might have been a classic, but in the wry sense of the term—a book that everyone knows about, many have on their bookshelves, but few have read.

That has certainly not been so in the case of *A Monetary History*. In the five decades since its publication, it has had a considerable influence both on scholarly research and on the practical aspects of monetary economics, most notably, monetary policy. Hugh Rockoff (2000), in a retrospective review article, called it the "most significant book in the field of economic history in the twentieth century." A quick check on Google Scholar has revealed over 5,800 citations of the book, many of them quite recent. In his retrospective review, Robert E. Lucas, Jr. (1994: 8) wrote: "If I ever go to Washington for some reason other than viewing cherry blossoms I will pack my copy of *A Monetary History* and leave the rest of my library—well most of it—at home."

As things turned out Lucas never did go to Washington, but many of those who went evidently shared his sentiments. According to Randall S. Kroszner (2010: 1), a member of the Board of Governors of the Federal Reserve System from 2006 to early 2009 and chairman of its Committee on Supervision and Regulation of Banking Institutions

during the financial crisis, "Perhaps the single most important piece of economic research that provided guidance to Federal Reserve Board members during the crisis was Milton Friedman and Anna Schwartz's *Monetary History of the United States*, especially the sections related to the 'Great Contraction.'"[2] It is difficult to come up with the name of another book in economics, or any other social science, that has had an influence of that sort.

Over the next two decades, Friedman and Schwartz published two related monographs, *Monetary Statistics of the United States: Estimates, Sources, Methods* (Friedman and Schwartz 1970), and *Monetary Trends in the United States and the United Kingdom: Their Relation to Income, Prices, and Interest Rates, 1867–1975* (Friedman and Schwartz 1982a), as well as a lengthy article on business cycles (Friedman and Schwartz 1963b). Prior to the publication of *A Monetary History*, Friedman published two other related articles, "The Quantity Theory of Money: A Restatement" (Friedman 1956a) and "The Demand for Money: Some Theoretical and Empirical Results" (Friedman 1959).

The first of the two articles, "The Quantity Theory of Money: A Restatement," was the lead essay in *Studies in the Quantity Theory of Money* (Friedman 1956b), a collection of papers based on dissertations written by members of the Workshop in Money and Banking. The purpose of Friedman's essay, as he put it, was "to set down a particular 'model' of a quantity theory in an attempt to convey the flavour of the [Chicago] oral tradition" (ibid., 4). The essay provided a theoretical backdrop for the other papers in the volume, including Phillip Cagan's (1956) celebrated article on the demand for money during hyperinflations, as well as for much of Friedman's later research, including both the *Monetary History* and his other work with Schwartz.

In "The Quantity Theory of Money: A Restatement," Friedman posited a money demand function in which the real quantity of money demanded was a function of a vector of returns on alternatives to holding money (bonds, equities, physical goods, and human capital), of real wealth, and of what Friedman termed a "portmanteau variable," a variable reflecting factors affecting the tastes and preferences of individuals and institutional factors, such as the payment practices of businesses. It is, I believe, a good example of what Becker termed Friedman's "'empirically driven' general equilibrium analysis." Transformed, this equation applied to velocity and hence could be used to express the usual quantity theory relation.[3]

[2] See Edward Nelson (2013) and Lothian (2014) for discussions of the policies advocated by Friedman and Schwartz, and the extent to which they were followed during and after the recent (2008) crisis.

[3] Friedman followed up on this essay with a series of papers treating the topic of money demand. In his essay "The Optimum Quantity of Money" (Friedman 1969) and his article on the quantity theory in the *New Palgrave* (Friedman 1987) he developed the theory more fully. Friedman's 1959 article "The Demand for Money: Some Theoretical and Empirical Results," was one of the first empirical studies of money demand. Friedman pursued the question of money demand further in Friedman (1966, 1977b, 1988), and in a lengthy chapter six in his coauthored monograph with Schwartz, *Monetary Trends in the United States and the United Kingdom* (Friedman and Schwartz 1982a) and in a related article (Friedman and Schwartz 1982b).

Soon after the release of *A Monetary History* a number of prominent economists wrote review articles of the book, reviews that were both highly complementary and that, as things turned out, contained some amazingly accurate prognostications. "The book is clearly destined to be a classic, perhaps one of the few emerging in that role rather than growing into it," Alan Meltzer (1965: 404) wrote. He went on to say that "The reader cannot fail to be impressed by the size of the task to which the authors committed themselves, by the authors' ability to treat the broad sweep of a century of monetary history without being overcome by the mass of detail that they carefully examine, by the originality of the scholarship that is everywhere displayed, and by a host of other considerations, most of which are conveyed by the word 'classic.'" Robert Clower (1964: 380) concluded his review article with the statement: "The book offers an almost inexhaustible supply of worthwhile conjectures. I have no doubt that it . . . will be the focus of a major share of scholarly research on money and income during the coming decade." Clower was correct in one regard, but he should have said "decades" rather than simply "decade." Future Nobelist James Tobin (1965: 485) was even more laudatory than Meltzer and Clower: "This is one of those rare books that leave their mark on all future research on the subject," Tobin stated. The rest of Tobin's summation is very much worth repeating:

> I have not done justice to the scope of this book. History presents the theoretically minded scholar with one challenge after another. Here these are met with the brilliance and finesse one would expect. Examples are: the determination of the exchange rate and gold premium during the greenback era, the economics of the 1879 resumption; the silver question; balance-of-payments pressure and adjustments in the 1890s; FDR's gold purchase policy; the mechanics of Federal Reserve bond support policy during and after World War II. The reader is advised in no event to omit the footnotes, which contain many gems of monetary theory: on Gresham's law; purchasing power parity; the prohibition and regulation of interest on commercial bank deposits; the significance of the "free reserve" position of member banks; the monetary mechanics of shifts among currency, demand deposits, time deposits, and other thrift accounts. (ibid.)

The theoretical structure of *A Monetary History* revolved around the quantity theory and price theory more generally. It is a framework that Irving Fisher would have found congenial, but many contemporary economists found puzzling. For nowhere in the book is the theory spelled out all at once. It is delivered in bits and pieces as the need arises. Often, as Tobin pointed out in his review, it is in the footnotes.

The experimental design is in line with what Friedman had advocated elsewhere. The historical period under study spans close to a century. That long period helps mitigate the small sample problems that often plague empirical research. It encompasses 7 different monetary regimes, 22 business cycles—6 of them severe—and 5 inflation episodes.

In *A Monetary History*, Friedman and Schwartz combine historical narrative with careful analysis of the monetary and other economic data. They use no formal statistical tests of hypotheses. Instead, they let history design the experiments,

which they then use to investigate the impact of money on prices and business conditions and to separate these monetary effects from other influences. The fact that they had such a long sample period was a great plus in this latter regard. The variety of institutional conditions affecting the supply of money over that period helped them disentangle the influence of money on the economy from the reverse influence of economic conditions on money. The monetary regimes simply varied too much for there to have been a stable transmission mechanism from output to the supply of money.

In their summary to *A Monetary History* (1963: 688) Friedman and Schwartz pointed to three crucial experiments involving Federal Reserve tightening: January–June 1920, October 1931, and July 1936–January 1937. In each instance, the tightened policy was an exogenous move on the part of the Fed. In each instance, a sharp contraction in economic activity ensued. Apropos of this approach, Ben Bernanke (2002), in his "Remarks on Milton Friedman's Ninetieth Birthday," wrote: "The special genius of the *Monetary History* is the authors' use of what some today would call 'natural experiments'—in this context, episodes in which money moves for reasons that are plausibly unrelated to the current state of the economy."

In *A Monetary History*, Friedman and Schwartz examined the role of money in the 22 reference cycle contractions individually. In their article "Money and Business Cycles" they used NBER statistical techniques to study those cycles plus the 1960–61 contraction as a group. In the six severe contractions, they identified monetary shocks as the major force leading to declines in nominal income. The Great Depression, retitled by them the "Great Contraction" in light of the 33 percent fall in the stock of money during the course of that episode, was the classic case. In minor contractions, they concluded that, "while the evidence was far less strong, it is plausible to suppose that changes in the stock of money played an important independent role, though certainly the evidence for these minor movements does not rule out other interpretations" (Friedman and Schwartz 1963b: 63). Phillip Cagan (1965), in his NBER monograph *Determinants and Effects of Changes in the Stock of Money, 1875–1960*, presents evidence fully consistent with these conclusions.

This combination of findings—some largely positive, others negative—is one reason why Friedman was skeptical of countercyclical policy. A second was his findings of long and variable lags between changes in money, and in nominal income and its real and price components. Friedman's monetary policy proposals, for example, *A Program for Monetary Stability* (1960), in great part were derived from those two sets of findings.

The recent US recession of 2008 and the boom period that preceded it provided a natural experiment with which to test the Friedman–Schwartz explanation of the Great Depression. To do this I compared nominal-income and stock-price behavior with money-supply behavior in the USA in the recent recession and in the Great Depression (Lothian 2011). Prior to the cycle peaks, nominal income, stock prices, and money in the current episode in the USA increased in tandem. The same thing happened in the Great Depression in the USA. After the peaks, however, behavior was different in one key respect. Unlike the Great Depression in which the money supply plummeted,

the money supply in this latest episode continued to grow. So too did nominal income and, after a relatively short time, stock prices.[4]

10.6 Concluding Remarks: Friedman and the Workshop in Money and Banking

The Chicago workshop system was and is an apprenticeship system. Its goal is to provide dissertation students with an opportunity to present their work in progress and have it critiqued. Friedman's Workshop in Money and Banking was the prime example of how the system was supposed to work. It was, however, different from other Chicago workshops in several key respects. That fact, I later found out, was more widely recognized in Chicago than I had thought when I was a student.

Several decades after I had left the university, Larry Sjaastad, who also was one of my teachers, invited me back to present a paper in his and George Tolley's Workshop in Public Policy. Prior to presenting the paper, Larry and I were having lunch and talking in the faculty club. At one point I guess I became a bit preoccupied. Larry looked at me with the barest trace of a grin. Then the grin gradually spread across his face. He said something like "What's the matter, Jim?" I replied, "Well, you know, Larry, coming back to Chicago presenting a paper . . ." He started laughing. "Jim, you were in the Money Workshop but it's been over twenty years since Milton ran that. No problems with mine by comparison." I was relieved. "You mean you don't start out with 'What's wrong with page one?'"

Unlike the other workshops, Friedman's workshop was not open to all-comers. The price of membership for students was a passing grade on the preliminary exam in money and an agreement to present a paper every year. Faculty could attend without paying the membership fee and junior faculty specializing in money/macro generally did attend. So also did Gary Becker in the two years I was a member.

Unlike the other workshops, the author of a paper never actually presented it. Papers were distributed a week in advance so that workshop members could read them and prepare their comments. At the start of each workshop meeting, Friedman would give the paper's author a minute or two, as he would put it, "to add anything to what you wrote, or to retract anything." He would then give his own short précis of the paper. It could be withering. Friedman was a caring man, but he did not pull any intellectual punches. His reminiscences of his early career at Chicago as a graduate student research assistant to the econometrician Henry Schultz and his interactions with Schultz are interesting in this regard. Friedman wrote:

[4] Gerald Dwyer and I later extended the comparisons to include the Euro bloc, Japan, and the UK (Dwyer and Lothian 2012). We found much the same thing in these episodes as in the USA.

> Diplomacy was not my long suit. When I found what I regarded as errors or omissions in [Schulz's] manuscript I pointed them out to him bluntly. I did not realize how rare it is for a senior and established academic to accept readily and with a thoroughly open mind such unvarnished criticism from a youngster . . . In later years, I discovered that openness to criticism, if not unique to Chicago, is much rarer elsewhere. It was and remains one of the chief characteristics that has made the University of Chicago such a powerful center of scientific innovation. (Friedman and Friedman 1998: 51–2)

After Friedman gave his summary of the paper, he and the other workshop members would go through the paper page by page. The "what's wrong with page one?" to which I alluded above was Friedman's typical opening line. "Any comments on page two?" It was a grueling process for the paper giver, but looking back on it I think it provided excellent training.

What were the discussions like? In many ways they were similar to what you get in any seminar situation, but much more to the point and not at all superficial. Gary Becker, in a recorded conversation with Friedman (Liberty Fund 2003), summed it up very well:

> I think the difference between having a paper distributed and not is it's easier for the speaker, but it's less productive for both the speaker and the audience when it's not distributed ahead of time . . . [G]iving workshops you learn a lot. You don't come away feeling you did that well but when you think about it you got a lot out of it.

Friedman's comments typically dealt with both the theory and the empirics. Writing was also fair game. My own dissertation and a subsequent article derived from it (Lothian 1973, 1976) focused on the question of how to define money when the characteristics of deposits—their degrees of "moneyness" and "bondness"—differed over time or space. The empirical criterion that I used to answer this question was stability in demand. The topic itself was an outgrowth of discussions with Friedman on how to define money when the characteristics of deposits were altered in environments of rising inflation and regulation of the interest rates payable on deposits. The statistics that I used consisted of annual data for 40 countries during the 15-year period 1952 to 1967. Friedman made several important points in the course of the workshop discussions of my work. The first was theoretical, to approach the problem as one of derived demand—Friedman used Marshall's example of knife handles and blades. The second was empirical and centered around the use of analysis of variance and covariance and their regression analogues in analyzing the panel data set that I had assembled.

Friedman attributed his concerns about writing to a lesson Mitchell had imparted to him early on (Friedman and Friedman 1998: 75): "After reading my draft of a proposed bulletin on our early results, Mitchell came into my office and gave me a dressing down about the quality of the exposition," Friedman wrote. "As I recall more than half a century later—itself testimony to the deep impression it made on me," Friedman went on to say,

> [Mitchell] said "There is some excuse for Simon [Kuznets] if he doesn't write clearly. After all, English was not his native language and he did not learn it until

> his late teens. But there is none for you. English is your native tongue. People often excuse bad writing by saying that they know what they mean, and simply have difficulty expressing it. That is nonsense. If you cannot state a proposition clearly and unambiguously, you do not understand it."

Friedman dutifully passed that lesson on to his students, myself included, and in no less uncertain terms.

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