Milton Friedman's monetary economics: Theory and empirics

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October 2015

Forthcoming in Robert A. Cord and J. Daniel Hammond, eds. *Milton Friedman: Contributions to Economics and Public Policy*, Oxford University Press, 2016.

Abstract. In this chapter, I discuss Milton Friedmanὸs approach to economic analysis in the context of his scholarly work and his work as a teacher and mentor of dissertation students. To do so, I draw not only on his published writings, but also on biographical material and on my personal recollections. Friedman was a price theorist par excellence. He was also a sophisticated and highly competent empirical researcher. I discuss both of these attributes. In my discussion of his research, I focus on three areas in particular: the consumption function, the Phillips curve, and monetary history.

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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 (Friedman, 1950).

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." And then he added "[a]nd ... 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."

Friedman did not say it in the article, but the motivation for these statements and for the article on Mitchell more generally was almost certainly as a response to Tjalling C. Koopmans 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 Friedman found intellectually uncongenial and that he regarded as 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 Friedmanôs work in particular in this paper ï his theory of the consumption function, his work on monetary dynamics and the Phillips Curve, 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 as a dissertation student in the Workshop in Money and Banking. Thereafter he and I had occasional direct contact and some indirect contact via our joint research interests.

2. Friedman’s approach to price theory and empirical research

For over three decades, the watchword in macroeconomics has been "micro foundations." 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 nineteen sixties and early seventies in Chicago, I had a quite different experience. Price theory ï microeconomics ï was to use Marshallôs phrase, a

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

phrase oft repeated by Friedman, the ñengine of analysis.ò 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, micro foundations were always there.

One of the reasons this was so, I believe, is the role that Milton Friedman played as an intellectual leader at Chicago. Friedman not only was 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 ï money, Economics 331, and macroeconomics, Economics 332 and, probably because the discussion was more free wheeling, even more so in meetings of the Money and Banking Workshop.

Friedman summed up his view on price theory relatively early on in an article on William Jafféôs translation of Leon Walrasô *Elements of Pure Economics* (Friedman, 1955):

> 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 ï 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 ï or misconception ï 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 ï 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.

Much of Friedmanǿs early published work actually was in price theory (Friedman 1935, Friedman 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 1953; Friedman and Savage, 1948). Allen Wallis (1980) and in an interview with Ingram Olkin (1991) discusses Friedmanǿs 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. Friedmanǿs 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 ñMilton Friedman as Microeconomistò wrote (Becker, 2007): ñ[Friedmanǿs] 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.ò The course, Becker went on to say, was quite different from what was typical then and what is typical today.

In this connection, Sam Peltzmanǿs (2013) précis is particularly interesting:

> Along with perhaps 50 other wet-behind-the-ears new Chicago graduate students I trooped into my first classð Milton Friedmanǿs 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 ñI, Pencil,ò 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 he used. He went rather seamlessly from one to the other as in his discussion of the returns to education, the consumption function, or the natural rate of unemployment. é 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 óempirically drivenô general equilibrium analysis.ò

In Friedmanôs 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.

Friedmanôs article ñWesley C. Mitchell as an Economic Theoristò (Friedman, 1950) that I cited above is especially revealing in this regard. ñThere is of course no sharp line between the empirical scientist and the theorist ï we are dealing with a continuum, with mixtures in all proportions, not with a dichotomy,ò Friedman wrote (Friedman, 1950, p. 465).

He then turned to Mitchell himself (Friedman, 1950, p. 466):

> [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.ò

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

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

> The problem is one that is common in scientific work. A preliminary decision ï in this case, on the definition of money ï 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. (Friedman and Schwartz, 1970, p. 91)

With regard to Mitchellôs own approach to theory, Friedman (1950, p. 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 ï 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 Friedmanôs work on the subject, met with a similar reaction to that accorded to Mitchellôs work. This is certainly the case with the two articles that he published in the early 1970s on monetary dynamics (Friedman, 1970, 1971a). These two articles also appeared in combined form in a National Bureau 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 Friedmanôs original articles and his reply to the critics were reprinted in a book edited by Robert J. Gordon (1974). Friedman and Schwartz (1982, chapter 2) contains a later discussion of this issue with references to and discussions of work published in the intervening period.

The critics for the most part ignored Friedmanôs 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 ñbaffledò 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 Friedmanôs characterization of the adjustment process was useful ï 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 ñto calibrate [his] own internal probability calculator,ò as he put it. The only true test, in Friedmanôs view, was replication using a different body of data.

Friedmanôs approach to empirical analysis was distinctive in other ways too. We can get some inkling of that fact in an interview that John Taylor conducted with Friedman that was published in 2001. The interview is instructive, for it highlights the difference between Friedmanôs 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 (Taylor 2001, pp. 121-122):

> 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].

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

3. Consumption

We see Friedmanôs 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. He combined that with insights from his and Simon Kuznets' 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 ña 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,ò as he put it in the preface to that work (Friedman, 1957 p. x). Friedman concluded by outlining 11 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. And in so doing, 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 that I took in 1970 described it as the best empirical study in economics. Zellner in a later interview in *Econometric Theory* stated: ñIt'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.ò (Rossi, 1989, p. 297).

Alan Walters (1987) 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."

In a retrospective review Costas Meghir wrote (Meghir, 2004):

> 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. (p. F305).

Friedman himself claimed that: ñ*A Theory of the Consumption Function* ... comes closer than anything else that I have written to adhering to the precepts of my essay on methodology. That is one, but by no means the only, that I have long regarded it as my best scientific contribution, though not the most influential.ò (Friedman and Friedman, 1998, p. 226).

Two bodies of Friedmanôs 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*.

4. The Phillips Curve

Central bankers traditionally have 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 generally have used Phillips-Curve type relations. Again that was true in the '60s 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 (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 ï nominal and real interest rates on the one hand and nominal and real wages on the other ï 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 1970 and in a subsequent article (Friedman, 1975), Friedman fleshed this analysis out a good deal more in terms of price theory than in his AEA address. He provided micro foundations for this bit of macroeconomic analysis in a way that was then largely lacking in macroeconomic analysis. Coupled with Edmund Phelps (1967) article on the Phillips Curve that reached very similar conclusions to Friedman's, "The Role of Monetary Policy" completely altered macroeconomic thinking.

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. And it took time before Friedman's analysis gained widespread acceptance. The IS-LM model was close to 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. And, as it turned our, that forecast all too soon was proven true by events. In both the United Kingdom and the United States, inflation ratcheted up on a longer-term average basis between the mid-1960s and early 1980s as the monetary authorities in both countries continually pursued ñfull-employment policies.ò

< Insert Table 1 here >

In the United Kingdom, in which process started earlier and at a somewhat higher average rate of inflation, the peak in the average inflation rate, as Friedmanõs analysis also implied, far exceeded that in the United States. We can see this clearly in the second and fourth columns of Table 1. The one departure from Friedmanõs 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 1. The expectations-augmented Phillips Curve appeared to shift not just vertically but upwards to the right.

In his Nobel Lecture, Friedman offered a ñtentative hypothesisò 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 colleagues Arnold Harberger and Larry Sjaastad as cases in point and drew both on their analyses of growth and inflation in Latin

America and Hayesôs analysis of the informational role of prices (Hayek, 1945) to provide a theoretical rationale for his empirical observations.

ñThis uncertainty ï or more precisely, the circumstances producing this uncertainty ï leads to systematic departures from the conditions required for a vertical Phillips curve," Friedman argued (Friedman, 1977a, p.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 reduces economic efficiency relative to a world of more stable prices. More important, agentsô ability to distinguish between changes in relative and absolute prices decreased. 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 Phillip's-Curve critique has stressed his use of adaptive expectations in this analysis and juxtaposed this against the later rational-expectations approach. Carl Walsh's short article ñNobel Views on Inflation and Unemploymentò 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 Friedmanôs 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 nuance 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 brought out forcefully in discussions in class and in the Money and Banking Workshop. 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. The student wanted to apply a more flexible functional form to the data than Cogan'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 dissertation further.

5. The Monetary History and Related Studies

Friedman and Schwartz's *A Monetary History of the United States, 1867-1960* was published in 1963 and is still in print a half century later. Chapter 7 of that work, *The Great Contraction*, was issued separately 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 Bureau 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 U.S. 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 ï 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, *A Monetary History* 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 ñmost significant book in the field of economic history in the twentieth century.ò A quick check on Google Scholar has revealed over 5800 citations of the book, many of them quite recent.

Robert E. Lucas, Jr. (1994) in his retrospective review of the book wrote: ñIf 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 ï well most of it ï at home.ò

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), 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, ñPerhaps the single most important piece of economic research that provided guidance to Federal Reserve Board members during the crisis was Milton Friedman and Anna Schwartzô Monetary History of the United States, especially the sections related to the óGreat Contraction.ôò[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 published two related monographs, *Monetary Statistics of the United States: Estimates, Sources, Methods* (Friedman with 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, 1982), and a lengthy article on business cycles (Friedman and Schwartz, 1963b). Prior to the publication of *A Monetary History*, Friedman published two other related articles, ñThe Quantity Theory of Money: A Restatementò (Friedman, 1956) and ñThe Demand for Money: Some Theoretical and Empirical Resultsò (Friedman, 1959).

The first of the two articles, ñThe Quantity Theory of Money: A Restatement,ò was the lead essay in *Studies in the Quantity Theory of Money* (Friedman, ed., 1956), 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 ñto set down a particular ómodelô of a quantity theory in an attempt to convey the [Chicago] oral tradition.ò The essay provided a theoretical

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

backdrop for the other papers in the volume including Phillip Caganôs (1956) celebrated article on the demand for money during hyperinflations, as well as for much of Friedmanôs later work, including both the *Monetary History* and his other work with Schwartz.

In ñThe Quantity Theory of Money: A Restatement,ò 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 ï bonds, equities, physical goods, and human capital ï of real wealth and of what Friedman termed a ñportmanteau variable,ò a variable reflecting factors affecting the tastes and preferences of individuals and institutional factors like the payment practices of businesses. It is, I believe, a good example of what Becker termed Friedmanôs ñempirically drivenôgeneral equilibrium analysis.ò Transformed, this equation applied to velocity and hence could be used to express the usual quantity theory relation.[3]

Soon after the release of *A Monetary History* a number of prominent economists wrote review articles of the book, reviews that were both highly complimentary and that as things turned out contained some amazingly accurate prognostications.

ñThe book is clearly destined to be a classic, perhaps one of the few emerging in that role rather than growing into it,ò Alan H. Meltzer (1965) wrote. Robert Clower (1964) concluded his review article with the statement: ñThe 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.ò Clower was correct in one

[3] Friedman followed up on this essay with a series of papers treating the topic of money demand. In his essay ñThe Optimum Quantity of Moneyò (Friedman, 1969) and his article on the quantity theory in the *New Palgrave* (Friedman, 1987) he developed the theory more fully. Friedmanôs (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 in his coauthored monograph with Schwartz *Monetary Trends in the United States and the United Kingdom* (Friedman and Schwartz, 1983) and in a related article (Friedman and Schwartz, 1982).

regard, but he should have said ñdecadesò rather than simply ñdecade.ò Future Nobelist James Tobin (Tobin, 1965) was even more laudatory than Meltzer and Clower. ñThis is one of those rare books that leave their mark on all future research on the subject,ò Tobin stated.

Meltzer went on to say that ñThe reader cannot fail to be impressed by the size of the task to which the authors committed themselves, by the authorsô 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 óclassic.ôò

The rest of Tobinôs 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 green-back era, the economics of the 1879 resumption; the silver question; balance- of-payments pressure and adjustments in the 1890's; 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.

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 plague empirical research. It encompasses seven different monetary regimes, 22 business cycles ï six of them severe ï and five 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 have 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.

Friedman and Schwartz in their summary to *A Monetary History* (1963, p. 688) pointed to three crucial experiments involving Federal Reserve tightening: January-June 1920, October 1931, 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.

A propos of this approach, Ben S. Bernanke (2002), in his ñRemarks on Milton Friedman's Ninetieth Birthday,ò wrote: ñThe special genius of the *Monetary History* is the authors' use of what some today would call ónatural experimentsôï in this context, episodes in which money moves for reasons that are plausibly unrelated to the current state of the economy.ò

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 National Bureau 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, re-titled by them "Great

Contraction" in light of the 33 per cent 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." 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 ï some largely positive others negative ï is one reason Friedman was skeptical of counter-cyclical 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. Friedmanôs monetary policy proposals e.g. *A Program for Monetary Stability* in great part were derived from those two sets of findings.

The recent U.S, recession 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 United States 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 United States increased in tandem. The same thing happened in the Great Depression in the United States. 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]

[4] Gerald Dwyer and I later extended the comparisons to include the Euro bloc, Japan and

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 Tolleyôs 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 ñWhatôs the matter, Jim?ò I replied, ñWell, you know, Larry, coming back to Chicago presenting a paper ... .ò He started laughing. ñJim, you were in the Money Workshop but itôs been over twenty years since Milton ran that. No problems with mine by comparison.ò I was relieved. ñYou mean you donôt start out with óWhatôs wrong with page one?ôò

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.

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the United Kingdom in addition to the United States in the recent recession (Dwyer and Lothian, 2012). We found much the same thing in these episodes as in the United States.

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 paperôs author a minute or two, as he would put it, ñto add anything to what you wrote, or to retract anything.ò 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 that regard. He wrote:

> Diplomacy was not my long suit. When I found what I regarded as errors or omissions in [Professor Schulzôs] 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 é 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, 51-52).

After Friedman gave his summary of the paper, he and the other workshop members would go through the paper page by page. The ñwhatôs wrong with page one?ò to which I alluded above was Friedmanôs typical opening line. ñAny comments on page two?ò 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: ñI think the difference between having a paper distributed and not is itôs easier for the speaker, but itôs less productive for both the speaker and the audience when it's not distributed ahead of time. é

[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"

Friedmanôs 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 ï their degrees of "moneyness" and " bondness" ï 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 data 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 a problem of derived demand ï 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, p. 75-76) ñAfter 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,ò Friedman wrote. ñAs I recall more than half a century later ï itself testimony to the impression it made on me,ò Friedman went on to say, ñ[Mitchell] said óThere 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 say the proposition clearly and unambiguously, you do not understand it.ô Friedman dutifully passed that lesson on to his students, myself included, and in no less uncertain terms.

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Table 1. Quinquennial averages of monthly inflation rates and unemployment rates United Kingdom and United States, 1955-1984

Period	UK		US	
	Inflation	Unemployment	Inflation	Unemployment
1955-59	3	1.6	1.9	5.0
1960-64	3	1.8	1.2	5.8
1965-69	4.2	2.0	3.8	3.8
1970-74	10.2	2.3	6.4	5.5
1975-79	14.3	3.8	7.9	7.0
1980-84	8.1	8.9	6.3	8.5

Note: All figures are percent per annum. Inflation figures for the United Kingdom are for retail prices and for the United States for consumer prices.
Sources: Federal Reserve Bank of St. Louis and Bank of England.