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## Milton Friedman's monetary economics and the quantity-theory tradition

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This article provides a selective review of Milton Friedman's contributions to monetary economics focusing on five areas in particular: the demand for money, the joint historical and empirical work with Anna J. Schwartz, the theoretical and empirical analyses of the Phillips Curve, monetary policy and monetary dynamics.

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### 1. Introduction

Milton Friedman taught at the University of Chicago from shortly after WWII to 1977 in an economics department that was unparalleled for its scholarly achievement. Nine Nobel prizing winning economists, including Friedman himself, graced the halls of that university at various points during Friedman's tenure there – six in the economics department, one in the committee on social thought, one in the school of business and one in the law school. Many of the non-Nobelists, had they been at any other major university probably would have been ranked among the brightest and most productive members of their faculties.

Milton Friedman was a, and perhaps *the*, major reason for the success of that group. He in many ways set the tone, not just as a researcher, but also as a colleague and teacher. He was an overwhelming intellectual force. On top of that he worked exceptionally hard.

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Friedman's long-time friend, colleague and fellow Nobel laureate, George J. Stigler, used to quip that while he – Stigler – just wanted to understand the world, Friedman wanted to change it. I repeated that story to Allan Meltzer several years ago. Allen gave me a quizzical look, and with several seconds delay, said “Well, Jim, George was wrong. Milton did change the world.” This is, I believe, certainly true both with regard to monetary economics and to how the economics profession and policymakers view it.

Friedman himself claimed that events had a lot to do with any success he had in that regard. He might have provided the analysis of what was wrong and why, he argued, but only when things actually went completely awry – as they did with inflation in the late 1970s – did policies change. Nevertheless they did change and along the general lines that Friedman had outlined. So too did economists' views, as Barro (2006) and De Long (2000) both argue and Nelson and Schwartz (2008) document in detail.

Friedman's early professional contributions, however, came not in the area of money-macro but in price theory (Friedman, 1935, 1949; Friedman and Kuznets, 1945) mathematical statistics (Friedman, 1937; Freeman et al., 1948) and combinations of the two (Friedman, 1953; Friedman and Savage, 1948).<sup>1</sup> His command of price theory was exceptional. So too was his intuition for statistics and its practical research applications.<sup>2</sup> The two informed everything he did in economics, including monetary economics.

Let me begin with a short list of Friedman's major scholarly contributions to that field. In the discussion that follows I will focus on four in particular: (1) the demand for money, both the initial theoretical paper (Friedman, 1956) in *Studies in the Quantity Theory of Money* (Friedman, Ed. 1956) and the later series of empirical articles, beginning with a 1959 article in the *Journal of Political Economy*; (2) the joint historical and empirical work with Anna J. Schwartz, most notably *A Monetary History of the United States* (Friedman and Schwartz, 1963a), but also their two related National Bureau books (Friedman and Schwartz, 1970, 1982) and their article on business cycles (Friedman and Schwartz, 1963b); (3) Friedman's theoretical and empirical analyses of the Phillips Curve – his presidential address to the American Economic Association and Nobel lecture; and (4) his other work on monetary policy, in particular the lectures that resulted in *A Program for Monetary Stability* (1965). These four, although subject to disputes of various sorts, were widely acknowledged as successes. In Stigler's (1976) terminology, they were “proper successes,” analyses that subsequently became part of how economists think about the issues in question. A fifth contribution in the area of monetary dynamics, which I will review somewhat more briefly, proved more controversial and in the end has been largely ignored.<sup>3</sup> In Stigler's terminology, it is, I believe, an “improper failure,” an analysis that was in fact highly insightful and, therefore, should have had an impact on economists' thinking but did not. I will omit entirely discussion of Friedman's “The Case for Flexible Exchange Rates” since that paper is dealt with extensively and insightfully by Russell S. Boyer (2009) and by Harris Dellas and George S. Tavlas (2009) in their papers in this issue.

## 2. The demand for money

Friedman's work on the demand for money began with “The Quantity Theory of Money: A Restatement” (1956) published as the lead essay in *Studies in the Quantity Theory of Money* (Friedman, Ed. 1956) a collection of papers derived from dissertations written by members of the Workshop in Money and Banking at Chicago. This essay is an exercise in capital theory and price theory more generally. Its purpose, as Friedman put it, was “to set down a particular ‘model’ of a quantity theory in

<sup>1</sup> For work authored by Friedman alone I only give the publication date as a reference.

<sup>2</sup> Wallis (1980) and in an interview with Olkin (1991) discusses Friedman's contributions to statistics, crediting him among other things with the intuition behind sequential analysis.

<sup>3</sup> Friedman initially published two articles treating this question in the *Journal of Political Economy* (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 Brunner and Meltzer, 1972, Paul Davidson 1972, Don Patinkin and James Tobin, 1972, along with a lengthy reply by Friedman. The four critiques, along with Friedman's original articles and his reply to the critics were reprinted in a book edited by 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.

an attempt to convey the [Chicago] oral tradition.” It provided the theoretical backdrop for the other papers in the volume including Phillip Cagan’s celebrated article (Cagan, 1956) on the demand for money during hyperinflations, as well as for much of Friedman’s own later work.

In it, he laid out a demand for money 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 income and of what Friedman termed a “portmanteau variable,” a variable reflecting factors affecting the tastes and preferences of individuals and institutional factors like the payment practices of businesses. Transformed, this equation applied to velocity and hence could be used to express the usual quantity theory relation.

There are two key points to note with regard to Friedman’s “Restatement.” The first is its derivation in terms of price theory. To use a later term, Friedman’s formulation of the quantity theory had explicit “micro-foundations,” though in the Marshallian rather than Walrasian general equilibrium sense in which that term is now used. Contra Johnson’s (1971) subsequent rather strident critique, it was not simply a warmed-over Keynesian liquidity preference function.<sup>4</sup> Indeed, if there are echoes of Keynes in Friedman’s approach, there are even greater similarities to Fisher’s capital theory, but not of course Fisher’s particular version of the quantity theory. Friedman’s price theoretic approach to monetary theory is equally evident in his essay “The Optimum Quantity of Money” (1969) and in his article “Government Revenue from Inflation” (Friedman, 1971c) in which he derives the revenue-maximizing rate of inflation using the simple model of monopoly.

The second point to note with regard to Friedman’s “Restatement” is that by including a range of alternatives to holding money Friedman in effect widened the monetary transmission mechanism from the narrow money-to-bonds channel of the then reigning IS-LM approach to channels linking monetary changes and spending on a much broader scale – goods and services as well as other financial assets.

Friedman’s article “The Demand for Money: Some Theoretical and Empirical Results,” which appeared in the *Journal of Political Economy* in 1959, was one of the first empirical studies of money demand. It ushered in what eventually became a voluminous literature on the subject, much of it replicating and in various ways extending Friedman’s work and some of it taking issue with Friedman’s methods and conclusions.

In the article, Friedman used reference-cycle averages of annual data for the United States over the period 1870–1957 to estimate to a demand function for real per capita money, defined as M2 divided by population and the permanent (or long-run expected) price level, in terms of real per capita permanent income. He constructed both measures of permanent variables in the same way as in his monograph on consumption behavior. He then used this relation to compute annual estimates of velocity that he compared with the actual figures. Friedman’s estimates captured both the secular decline in velocity and its pro-cyclical pattern remarkably well. He went on to compare the cyclical patterns in the velocity errors with the cyclical patterns of various interest rates and the rate of change of prices – all measures of the opportunity cost of holding money. These results were largely, but not completely, negative.

These findings, Friedman stated, had implications – “for the theory of money, the study of business cycles and the conduct and possibilities of monetary policy.” He argued in particular that they supported an assets rather than a transactions approach to the demand for money and that they suggested that over the short run small changes in money supply would be capable of producing disproportionately large changes in nominal spending and hence income.

Friedman returned to the theme of money demand over the next two and a half decades in a series of papers (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 their related article (Friedman and Schwartz 1982).

<sup>4</sup> Johnson (1971) and Patinkin (1969, 1972) both accused Friedman of misrepresenting the Chicago oral tradition. Tavlas (1998) provides evidence that, as Friedman had argued, this is not so, that there was indeed a Chicago quantity theory tradition upon which Friedman built. See also Friedman’s reply to Patinkin in the article responding to criticisms of his article A Theoretical Framework for Monetary Analysis (Friedman, 1972).

Several important features of [Friedman's 1959](#) article have been missed by most commentators. The first concerns his choice of data. By using reference-cycle averages as his basic units of observation, Friedman was able to focus on positions of long-run equilibrium. He, therefore, was able to get around both the problem of monetary endogeneity and the partially related econometric problems inherent in modeling short-run monetary adjustment. The second concerns his approach to empirical analysis more generally. Friedman at heart was a Bayesian ([Pelloni, 1987](#)). He was aware of the potential for “con” in econometrics and in statistics more generally. He was chary of multiple regressions and the uses to which they often are put. 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 was wont to put it. The only true test, in Friedman's view, was replication using a different body of data.

We see almost all of this put into practice in Friedman's monograph *A Theory of the Consumption Function* ([Friedman, 1957](#)). There Friedman took an existing body of empirical results as his starting point, constructed a simple model of consumer behavior that was consistent with these results and 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 (p. x). Friedman concluded by outlining 11 additional tests that other researchers could use to test the implications of the theory. “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,” Arnold Zellner said rather admiringly in an interview in *Econometric Theory* ([Rossi, 1989](#) p. 297).

We also see most of this in practice in “The Demand for Money: Some Theoretical and Empirical Results.” The separate analyses of secular and cyclical behavior, the use of outside information from his consumption work to derive a permanent income measure, and the computation of yearly velocity estimates on the basis of the equation estimated from the cycle-average data are all examples.

Friedman with Schwartz in Chapters 6 and 7 of their *Monetary Trends* (1982) returned to the questions of money-demand and velocity behavior, using time-series data for the United Kingdom and the United States and very much the same methods as Friedman used in his earlier study including the use of longer term averages of the yearly data. Close to a decade later [Hendry and Ericsson \(1991\)](#) published an article criticizing that work and presenting alternative estimates of a money demand function for the United Kingdom alone to which [Friedman and Schwartz responded \(1991\)](#).

The interchange is instructive, for it highlights the difference between Friedman's approach to data analysis and what is standard practice in many, if not most, parts of the economics profession. Friedman later discussed the difference between the two approaches in an interview conducted by [Taylor \(2001, pp. 121–122\)](#), from which I quote:

Taylor: In recent years, you have had some debates with David Hendry about statistical issues relating to your empirical work on money. And that's related to the use of modern methods of statistics and time series. Could you describe your views about various approaches to time-series analysis? Where do you see some advantages and disadvantages?

Friedman: 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).

### 3. The monetary history and other NBER studies

Friedman and Schwartz's *A Monetary History of the United States, 1867–1960* first appeared in print 1963 and is still in print close to half century later. Chapter 7 of that work, *The Great Contraction*, was issued separately two years after the publication of the book and has recently been reissued in a second edition.

Over the next two decades Friedman and Schwartz followed up with two related monographs, *Monetary Statistics of the United States: Estimates, Sources, Methods* (1970), and *Monetary Trends in the United States and the United Kingdom: Their Relation to Income, Prices, and Interest Rates, 1867–1975* (1982), and a lengthy article on business cycles (1963b).

Economists' forecasts of economic conditions, as we know, often are rather wide of the mark. Economists' forecasts of the success or failure of ideas, one suspects, are doubly so. Nevertheless, soon after the release of the *Monetary History* a number of prominent economists wrote reviews of the book that were rather bold in their prognostications and that in retrospect turned out to be amazingly accurate.

"The book is clearly destined to be a classic, perhaps one of the few emerging in that role rather than growing into it," Alan H. Meltzer wrote in 1965. "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,'" Meltzer went on to add.

Robert Clower concluded his review article (Clower, 1964) 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 basically was right, but he should have said decades rather than "decade."

The future Nobel laureate James Tobin, if anything, was even more laudatory (Tobin, 1965) 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 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 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.

Rockoff (2000) in a retrospective review article called it the "most significant book in the field of economic history in the twentieth century." He went on to say, however, that

Perhaps the greatest mystery is not that the Friedman–Schwartz methodology was persuasive, but rather that despite the enormous impact of *A Monetary History*, few economists use its methodology. Typically, when an economist attempts to persuade other economists, the first step is to feed the numbers through the computer and in the process strip away the historical circumstances that adhere to them.

The theoretical workhorses of *A Monetary History* were the quantity theory of money and price theory more generally. Friedman and Schwartz apply the quantity theory under a variety of institutional conditions – the floating dollar of the greenback era, the classical gold standard, the two World Wars, the varied arrangements of the interwar era and the post-WWII Bretton Woods regime of pegged exchange rates – and vary their analysis accordingly. It is a framework that Irving Fisher would have found congenial, but many contemporary economists found puzzling, if not distracting. For nowhere 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, in footnotes. Many of these are masterful as Russell Boyer in this issue of the *Journal* documents.

Friedman and Schwartz's methodology is a combination of historical narrative and careful analysis of the monetary and other economic data. They provide no tests of hypotheses in the formal statistical sense. Instead, they let history design the experiments, which they then use in quite ingenious ways to investigate the impact of money on prices and business conditions and to separate these monetary effects from other influences.

A propos of this approach, Ben S. Bernanke, in his "Remarks on Milton Friedman's Ninetieth Birthday," (Bernanke, 2002) 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 is plausibly unrelated to the current state of the economy."

A quick check on Google Scholar revealed over 2000 citations of the book, many of them rather recent. Schwartz (2004) commenting on the success of this work wrote:

I can think of two reasons for this unexpected occurrence. One reason is that the book has been a seedbed of ideas for research that began soon after its appearance and shows no sign of ending. The second reason for the book's longevity is that Milton Friedman has been tireless in informing a wider public than the research community of the main themes of the book.

I can think of two others. One is gains from trade – Friedman and Schwartz were perfect complements. The one by himself could have written a fine work on monetary economics informed by economic history. The other by herself could have written a detailed history informed by theory. Together they produced a *magnum opus*. A second is that the two were wordsmiths. The *Monetary History* is eminently readable. Economists like other people economize on their 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 of but few if any have read.

#### 4. The Phillips curve

The hallmarks of monetary policy in the 1960s and 1970s were an exclusive emphasis on nominal interest rates as indicators of the tightness or ease of monetary policy and the belief in an exploitable tradeoff between the rate of unemployment and the rate of inflation, the unadorned Phillips Curve.

The two were the subject of Friedman's critique in his 1968 presidential address to the American Economic Association "The Role of Monetary Policy." Both, Friedman argued, involved a failure to distinguish between nominal and real magnitudes – nominal and real interest rates on the one hand and nominal and real wages on the other – and between the short-run and long-run effects of monetary changes.

The key distinction in each instance was between the actual rate and the natural rate. Attempts to maintain the actual rate of unemployment below the natural rate – the rate consistent with long-run equilibrium in the labor market – might be successful in the short run, albeit at the expense of higher inflation. But, as expectations adjusted to the now higher inflation, unemployment would rise and eventually return to the natural rate. Continued over time, such a policy would lead not simply 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. Coupled with Phelps (1967) article on the Phillips Curve that reached very similar conclusions, "The Role of Monetary Policy" revolutionized macroeconomic thinking.

Friedman's conditional 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 "full-employment policies." In the United Kingdom, which started the process at an initially higher average rate of inflation, the peak in the average inflation rate, as Friedman's analysis implied, far exceeded that in the United States. We can see this clearly in Table 1.

The one departure from Friedman's theoretical account is that average rates of unemployment in both countries and elsewhere actually rose along with the average rates of inflation, rather than

**Table 1**

Quinquennial averages of monthly inflation rates: UK and US, 1955–1984.

Period	UK	US
1955–59	3.0	1.9
1960–64	3.0	1.2
1965–69	4.2	3.8
1970–74	10.2	6.4
1975–79	14.3	7.9
1980–84	8.1	6.3

Note: All figures are per cent per annum. 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.

remaining constant. The expectations-augmented Phillips Curve appeared to shift not just vertically as theory suggested, but upwards to the right.

Friedman in his Nobel Lecture (1977a) offered a “tentative hypothesis” with regard to why this was so. 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.

“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 (p.465). Predicting both the longer term drift in inflation and its shorter term movements became much more difficult. The optimal length of contracts, therefore, shortened, and indexing became more prevalent, both of which reduced economic efficiency relative to a world of more stable prices. More important, agents’ 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 became impeded, thus reducing economic efficiency further. Friedman opined that both of these factors very likely also had negative effects on employment. This article too spawned a small cottage industry of theoretical and empirical papers examining Friedman’s hypothesis.

## 5. Monetary policy

During the course of his career, Friedman’s monetary policy proposals became well known both to the economics profession and to the broader public.

Friedman’s published work on monetary policy spanned six decades. He wrote on the subject for scholarly journals, the financial press, the publications of a variety of think tanks, in books geared to general audiences and for congressional testimony. One of the best known and most widely read of his works on policy was his Friedman, 1960 monograph *A Program for Monetary Stability*.

Two things motivated these writings on policy. The first was his research findings, largely, though not exclusively, the findings of his historical work with Schwartz. The second was his long-standing belief – in part derived from his research – that a free society, in which government played a *de minimus* role was the best society.

In *A Program for Monetary Stability* Friedman made three proposals with respect to U.S. monetary policy: to switch from the pegged exchange rates of the Bretton-Woods System then in place to freely floating exchange rates, to impose 100 per cent required reserves on deposit-issuing banks, to direct the Federal Reserve to maintain a constant growth rate of the stock of money.

The first, the move to floating rates, has been effected, although exchange-market intervention has at times been implemented. The second, 100 per cent reserves, is now a dead letter, given the demonstrated ability of financial institutions to innovate to circumvent regulations that they deem onerous.<sup>5</sup> The third, the constant-growth rate rule has never been given serious attention by Federal Reserve policy makers, but of course remains an option.

<sup>5</sup> In later work, Friedman abandoned this proposal, tentatively advancing a proposal for free banking (Friedman and Schwartz, 1986).

In summing up his case for a monetary rule in *A Program for Monetary Stability* Friedman wrote:

Like other academicians, I am accustomed to being met with the refrain, ‘It’s all right in theory but it won’t work in practice.’ Aside from the questionable logic of the remark in general, in this instance almost the reverse of what is intended is true. There is little to be said in theory for the rule that money supply should grow at a constant rate. The case for it is entirely that it would work in practice. There are persuasive theoretical grounds for desiring to vary the rate of growth to offset other factors. The difficulty is that, in practice, we do not know when to do so and by how much. In practice, therefore, deviations from the simple rule have been destabilizing rather than the reverse.

The alternative, in which he expressed little faith, Friedman said, was to “let the Federal Reserve System do it.”

For most of the past two decades, the Federal Reserve “doing it” has appeared to work tolerably well. The Great Moderation may have had real causes, but monetary policy certainly did not upset it. Now, however, the situation has changed and overly expansive Fed policy that fueled the housing boom appears to be one of the reasons why.

On one view, the monetary excesses could have been avoided had the Fed simply adhered to Taylor rule as a guide, which they did not, as Taylor (2009) has documented. In any event, that is not really an answer since the Taylor Rule is a guide, rather than a rule in the proper sense of the word. Whether adherence to Friedman’s constant-growth-rate rule on the part of the Fed would have avoided the problem is a topic that ought to be investigated.

## 6. Dynamic adjustment

In partial response to criticisms of *A Monetary History*, Friedman wrote two articles (Friedman, 1970, 1971a) in which he set out a long-run quantity theory model, comparing it with a Keynesian model, and a generalized dynamic model of the short-run adjustment of inflation and nominal and real income growth to shocks that was consistent with the long-run quantity theory model. His object, as he put it in his response to the critical comments of five prominent economists, “was to outline a general approach that could suggest what empirical issues required study, an approach that could then be elaborated in further detail in connection with such empirical studies. I viewed the articles as supplementing, not replacing, my other writings, as another piece of a continuing endeavor, not as the final word.” (1972, p. 909).

What is interesting is that the critics – with the partial exception of Brunner and Meltzer (1972) – largely ignored Friedman’s outline of the short-run adjustment process, which was a new contribution, and directed their comments at a host of other issues, many of them pertaining to the history of economic thought. Friedman at several points in his reply said he was “baffled” by some of the points the critics raised. Rereading these articles, I have much the same feeling. The key question to be debated, 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 serve as a building block for further empirical work.

As it turns out, it has not served in that way, nor has it found its way into macroeconomic textbooks in discussions of the short-run adjustment of the economy to monetary (and other) shocks.<sup>6</sup> It has been replaced by general-equilibrium models, that despite their elegance, are ill-suited to the uncovering of empirical regularities that historically has been the stock and trade of monetary economists, not just Friedman or Friedman and Schwartz, but Irving Fisher, others writing in between Fisher and Friedman like Clark Warburton, and those writing much earlier David Hume and John Wheatley.

I regard this, as I stated earlier, as an “improper failure,” again to borrow Stigler’s terminology. One man’s meat, however, is another’s poison. Mine is a Marshallian view. The alternative, and now dominant, Walrasian view sees the situation differently.

<sup>6</sup> A lone exception is the now out of print intermediate text of Darby and Melvin (1986).



## 7. Conclusions

Milton Friedman viewed scientific investigation, as he described it, as “a series of successive approximations.” Theory formed the basis of empirical investigation with the results of such investigation in turn feeding back on, and then leading to refinement of the theory. Such a process, I believe is evident in Friedman’s work in monetary economics that I have just reviewed. Friedman began with a quantity theory model in the tradition of his teachers at Chicago, combined it with elements from the Cambridge cash-balance approach and, following in the footsteps of Irving Fisher, tested that model empirically. The end result was a coherent body of insights regarding both the long-run equilibrium properties of the monetary system and the short-run adjustment process whereby changes in the stock of money affected the economy. Money was certainly not the only thing that mattered in this view, but it mattered greatly for prices and had the potential, too often realized, for producing major cyclical fluctuations in business conditions.

One of the laboratories in which this process of successive approximations got worked out was the weekly Workshop in Money and Banking that Friedman ran at Chicago. It was a seminar-type setting but different from other seminars I have encountered. To begin with students had to be admitted to the workshop. The cost of entry was a passing grade in the difficult preliminary exam in money and banking. To remain in the workshop, a student was required to write a scholarly paper on some aspect of his or her dissertation work each year. There were no free riders.

Each week a different paper was presented, more often than not by a student, occasionally by a faculty member at Chicago or by someone from outside the university. The workshop format was always the same. The paper was distributed the week before so workshop participants could read it. At the start of the meeting, Friedman would give the paper’s author a minute or so, as he would put, “to add anything to what you wrote, or retract anything,” and then add his own short précis. Then the group went through the paper one page at a time.

“What’s wrong with page one?” Friedman would intone. “Any comments on page two?” Shaky empirical inferences, haphazard theorizing, and the occasionally silly statement were all grist for the mill and the mill wheel did indeed turn. Academic one upmanship, however, was not the game and attempts at it were not tolerated. Nor were there any knives in the back.

Friedman both verbally and several places in print (e.g., [Friedman and Friedman, 1998](#), p. 40), described himself and Abba Lerner referred to their sharing a certain “Talmudic cast of mind.” I thought of Friedman’s approach as being also very much in the Thomistic tradition – state your maintained hypothesis and its alternative in the clearest ways possible and then through logical argument – be it theoretical or empirical, or more likely, some combination of the two – arrive at a resolution.

Harry G. Johnson, a colleague of Friedman’s for a number of years, referred to the “tough environment” and “intellectual brutality” which he encountered at Chicago ([Moggridge, 2008](#), p. 205). That attitude might account for what I have always found curious, the lack of interaction between members of the Trade Workshop and the Money Workshop. Indeed it was particularly curious since during the time I was at Chicago (1967 to 1972), both the faculty and the students in the Trade Workshop were developing the monetary approaches to both the balance of payments and exchange rates, engines of analysis very much in the quantity-theory tradition and doubtless influenced by Friedman’s thinking as Boyer suggests.

For students, the Money Workshop was a first-rate apprenticeship. The message that came out of the workshop was clear. We had the chance to become masters in the field, but doing so required very hard work. In imparting that message and, more important, in teaching students how actually to do good research and in forcing us to explain what we had done clearly and simply, Milton Friedman was a teacher par excellence.

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