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Review of Milton Friedman and Anna J. Schwartz's 'A monetary history of the United States, 1867–1960'

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1.

A contribution to monetary economics reviewed again after 30 years – quite an occasion! Keynes's *General Theory* has certainly had reappraisals on many anniversaries, and perhaps Patinkin's *Money, Interest and Prices.* I cannot think of any others. Milton Friedman and Anna Schwartz's *A Monetary History of the United States* has become a classic. People are even beginning to quote from it out of context in support of views entirely different from any advanced in the book, echoing the compliment – if that is what it is – so often paid to Keynes.

Why do people still read and cite *A Monetary History*? One reason, certainly, is its beautiful time series on the money supply and its components, extended back to 1867, painstakingly documented and conveniently presented. Such a gift to the profession merits a long life, perhaps even immortality. But I think it is clear that *A Monetary History* is much more than a collection of useful time series. The book played an important – perhaps even decisive – role in the 1960s' debates over stabilization policy between Keynesians and monetarists. It organized nearly a century of U.S. macroeconomic evidence in a way that has had great influence on subsequent statistical and theoretical research. Perhaps most of all, *A Monetary History* served the purpose that any narrative history must serve: It told a coherent story of important events, and told it well.

2.

A Monetary History has a very simple structure. There is a brief introductory chapter, announcing the aim of providing an account of 'the stock of money in the United States' and of 'the reflex influence that the stock of money exerted on the course of events'. There follow eleven chronologically ordered chapters, each of which treats a subperiod of the 1867–1960 period covered by the book. In each of these chapters, the behavior of the money supply (M2) and of its proximate determinants is described. Contemporary movements in real income and the general price level are also described in each chapter. These facts are set out in a similar verbal and graphic format each time, and then the main economic and political events that determined their behavior are discussed in a straightforward narrative. Chapter 13 concludes with a brief summary of the empirical generalizations that emerge from the study. (Some of these generalizations might better have been announced in Chapter 1, as the organizing principles that underly the narrative.)

If the reader has not already anticipated it, he learns in this summing up that the history of the U.S. money stock and its effects on other variables is, for Friedman and Schwartz, a complete macroeconomic history of the United States over these nine decades. Every major depression and movement of prices and interest rates has been accounted for, every policy decision seen by the authors as important has been reviewed, and where policies have been found deficient, alternatives have been proposed and their likely consequences assessed. In place of a ninety-year period that in fact included many depressions and episodes of both deflation and inflation, one is given a vision of the way this portion of our history might have evolved, with stable prices and smoothly growing real output, and of the policies – well within the limits of the powers given to the monetary authority by the Federal Reserve Act of 1914 – that would have achieved this outcome.

A Monetary History constructs this vision through the consistent application to specific historical events of two simple principles. The first of these is the hypothesis of long-run monetary neutrality. It is implicit in Friedman and Schwartz's account that there is a trend path of real output, governed by forces that are not examined in the book, which has the property that neither its level nor its growth rate is affected by monetary policies. This secular path is stable: the economy returns to its trend behavior after displacements. The second central hypothesis is a short-run nonneutrality of money. Fluctuations in M2 induce spending fluctuations and these, in the face of nominal price rigidities, induce real output fluctuations. Again, no effort is made to elucidate or explain the nature of these price rigidities, except to say that they are transient (and so reconcilable with long-run neutrality). Little is said about the details of the economy's response to money changes, beyond the repeated insistence that monetary tightness and ease cannot be gauged by looking at interest rates. The empirical connection one observes is between M2 and nominal and real spending directly, with neither interest rates nor the composition of expenditures playing important roles.

All the aggregative positive and normative analysis in the book is a direct and simple consequence of these two principles. On the positive side, every depression is accounted for, as much as it can be, by prior and contemporary contractions in money. Of course, other sources of short-run instability are also active, and indeed many such possibilities are discussed in some detail, but monetary shocks form the consistent thread in the story, and it is an explicit conclusion that such shocks play the key role in all major fluctuations.

In arriving at this conclusion, no claim is made that M2 fluctuations are exogenous (a term never used in the book), although it is argued in specific instances that particular M2 movements cannot be seen as response to real events. On the contrary, a main theme of the book is the examination of the way governmental and private forces interact to determine the broad money supply. A few contractions are directly attributable to decisions by the monetary authority. Others are attributed to banking panics and flights to currency. The only consistent claim is that in every case the monetary authority *could* have prevented the contraction from occurring, either by avoiding its own mistake or by the timely offsetting of events in the private banking system, and that such action would have prevented or greatly mitigated the associated depression.

Given this account of observed depressions, the normative analysis is straightforward: The monetary authority has always had the ability to eliminate M2 instability, and it should have done so. In every instance, Friedman and Schwartz provide a detailed, operational account of how and when actions could have been taken that would have achieved this outcome. They do not discuss the possibility that monetary variability might have had a constructive role to play in offsetting nonmonetary sources of real instability. Whether this is because they believe that such active stabilization policies would be welfare-reducing, or that we do not have the knowledge to carry such policies out, or simply that they viewed this question as outside the scope of the study, they do not say.

3.

One level on which one can try to evaluate *A Monetary History* – and it is a level on which the book clearly invites a response – is to ask oneself whether one would follow its normative advice if one were in a position of monetary authority. On this level, I will say that I find the argument of *A Monetary History* wholly convincing. I think Friedman and Schwartz are right to focus on the avoidance of the really major macroeconomic disasters of the past as the main responsibility of current monetary policy. I find their diagnosis of the

1929–33 downturn persuasive and indeed, uncontested by serious alternative diagnoses, and remain deeply impressed with their success in explaining the remarkable events of these four years by applying the same principles they apply to lesser contractions. I do not believe our understanding of business cycle dynamics is adequate to guide any subtler monetary policy than the smoothing of the money supply (and disregard of interest rate movements) that Friedman and Schwartz argue would have avoided past disasters. 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.

These are my opinions on *A Monetary History* as a manual on the use of U.S. monetary history as a guide to macroeconomic policy-making. They are certainly opinions on which reasonable and competent economists may disagree: These are not issues resolved by theorems or hypothesis tests. To persuade me to change my opinions, however, a competitor to Friedman and Schwartz will need to apply his preferred principles to U.S. monetary history – certainly including the 1930s – and show that they yield an equally coherent analysis of past events and equally operational guidelines for policies likely to improve on past performance. This is a tall order.

A Monetary History is full of numbers, but there are many quantitative questions to which its model-free approach cannot provide answers. On the Great Contraction, for example, Friedman and Schwartz conclude (p. 301):

> Prevention or moderation of the decline in the stock of money, let alone the substitution of monetary expansion, would have reduced the contraction's severity and almost as certainly its duration. The contraction might still have been relatively severe. But it is hardly conceivable that money income could have declined by over one-half and prices by over one-third in the course of four years if there had been no decline in the stock of money.

This is not a verbal summary of tables describing the results of a numerical simulation; it *is* the simulation. Certainly Friedman and Schwartz are to be commended, not criticized, for the scholarly caution that marks this passage and the entire book. On the other hand, such conclusions obviously leave a good deal of room for disagreement over the sufficiency of smooth money growth as an antidepression policy.

One may be convinced by Friedman and Schwartz's account that it was well within the abilities of U.S. monetary authorities to prevent the occurence of contractions in the money supply, and that had this been done, depressions would have been much less severe. But by *how much* would the decline in real output to 1933 have been reduced had such a monetary policy been pursued? In general, what would the variance in real output growth have been over the 90-year period under study had money growth been smooth? What would the variance in real output growth have been over this period if resources had been allocated efficiently, in the face of unavoidable real shocks of various kinds? In order to conclude that smooth monetary policy is all the stabilization policy we need to have, we want to know the answers to quantitative questions like these.

There is, then, a second level on which the contribution of *A Monetary History* can be assessed. The book does not offer an explicit model of the economy, but its narrative account rests on the rigorous application of few simple economic principles. Are these principles useful as a starting point or guide to the development of a model that could provide answers to questions like those I have raised in the last paragraph? Or is it more promising to start from scratch, on some other basis? (Either answer to this question is obviously consistent with the opinion that familiarity with *A Monetary History* would come in handy in Washington.) Nothing in *A Monetary History* suggests that Friedman and Schwartz had any interest in explicit macroeconomic modeling, but I think it is clear that they viewed their work as providing a scientific foundation on which future economists could build [as they themselves did in Friedman and Schwartz (1982)]. The extent to which they succeeded in doing so has been controversial from the beginning.

4.

At the time *A Monetary History* appeared, many macroeconomists believed that simulations of Keynesian macroeconometric models were capable of providing accurate, quantitative answers to questions about the effects of alternative stabilization policies, or that improved versions of these models would soon be able to do so. All of these models incorporated price rigidities of one sort or another, and so were consistent with the short-run nonneutrality of money that is at the center of Friedman and Schwartz's account. But monetary shocks were assigned no special importance by these models (or, as the authors of the models would have put it, by the data). Thus the Adelman and Adelman (1959) simulations of the early Klein–Goldberger (1955) model showed that income fluctuations in that model were almost entirely attributable to shocks to various components of private spending or, as we would say today, to preference and technology shocks. I have no doubt that this feature continued to obtain in all later Keynesian models.

Within the Keynesian tradition, then, the presumption was that an economy could drift into depression for all kinds of reasons. No emphasis was placed on identifying a single causal factor in depressions, and in any case there would be little hope of reducing the impact of changes in factors like 'consumer confidence' at their source. From this point of view, the appropriate stabilizing response did not depend crucially on the exact nature of the disturbance that set off a particular downturn: Massive open market operations would have been useful in 1930; so, too, would have been a large-scale program of public works.

When *A Monetary History* was published, in 1963, it did not stimulate a useful debate over the relative merits of these different approaches to stabilization policy. Friedman and Schwartz simply ignored contemporary econometric developments (although I take the reference on p. 102 to 'the absence of a tested theory of cyclical movements' as oblique criticism) and, in general, treated what they termed 'the Keynesian Revolution in academic economic thought' (p. 626) as a minor event, responsible mainly for a temporary lapse of attention to monetary policy. Keynesian model builders returned the compliment and ignored *A Monetary History*. [James Tobin's thoughtful (1965) review article is an exception, but Tobin accepted *A Monetary History* on its own terms, and avoided comparing Friedman and Schwartz's approach to that of contemporary model builders. His dissatisfaction with Friedman and Schwartz's treatment of the interest elasticity of money demand, for example, was shared by monetarists like Allan Meltzer and Karl Brunner, and did not raise more general issues of method that divided Keynesians and monetarists at that time.] At about the same time, of course, Friedman and Meiselman (1963) articulated their skepticism about models based on 'autonomous spending' shocks. Later, Friedman (1968) emphasized the inconsistency of these models with long-run monetary neutrality, and explained why he believed neutrality must obtain in any reasonable general equilibrium view of the long-run behavior of the economy. These direct attacks demanded (and got) a response from the opposition, but *A Monetary History* is content to stand on its own merits and leave it to others to draw comparisons with alternative approaches.

As everyone knows, the Keynesian macroeconometric models fell on hard times in the 1970s, when inflation exposed the deficiencies in their treatment of monetary neutrality. This research line has permanently altered our view of what macroeconomics can hope to achieve, but the models themselves now seem hopelessly crude and dated. As Fair (1992) observes, modern neo-Keynesians steer very clear of the Keynesian econometric tradition and of quantitative issues in general, contenting themselves with small-scale, qualitative models that illustrate various logical possibilities. The narrative approach taken by Friedman and Schwartz has proved more durable: In a two-volume collection of recent papers entitled *New Keynesian Economics* [Mankiw and Romer (1991)], Keynes's name does not appear in an index that contains 17 references to Friedman!

5.

In the 1970s, a number of explicit models were developed that were designed to reconcile the two neutrality principles on which Friedman and Schwartz built and to capture the central importance Friedman and Schwartz assigned to monetary instability. These models all assumed some form of nominal price rigidity, in order to obtain monetary non-neutrality in the short run, but did so in such a way that, using the principle of rational expectations, neutrality in the long run was preserved. All of these models were consistent, in a general way, with Friedman and Schwartz's accounts of depressions in the period they studied. Moreover, because of the long-run neutrality they embodied, all of them were consistent with the breakdown of empirical inflation–unemployment tradeoffs that occurred during the inflation of the 1970s. Thus it seemed that the principles underlying the analysis in *A Monetary History* could be used as the basis for econometric models that were as explicit as the Keynesian alternatives and empirically superior as well.

Though these rational expectations models all are consistent with the Friedman and Schwartz neutrality principles and with monetary shocks as the central factor in business cycles, not all of them carry the normative implication that the best monetary policy is perfectly smooth growth. This conclusion depends critically on the details of the way price rigidities are modeled. In the illustrative model of Lucas (1972), all exchange occurs in competitive markets and the *only* source of price rigidity is the limited information available to goods suppliers. In this context, smooth monetary policy leads to efficient resource allocation, even in the face of nonmonetary shocks. On the other hand, in models such as Fischer (1977), Phelps and Taylor (1977), Taylor (1979), and Mankiw (1985), in which the rigidity of prices is attributed to nominally set contracts or to costly price setting by firms, there is no presumption that simply removing monetary variability will result in a system that responds efficiently to other shocks. Though it is now clear that the two neutrality principles used by Friedman and Schwartz can be reconciled, the question of the appropriate conduct of monetary policy remains unresolved. I do not see how it can be resolved without better theories of price rigidity than we now have available to us.

The new element introduced in these rational expectations models was the distinction between anticipated changes in money, predicted to be neutral, and unanticipated changes that were predicted to have real effects. Of course, the particular conditional expectation to be identified with 'anticipated' varies with the nature of the assumed price rigidity. The distinction adds little to Friedman and Schwartz's account of the 1867–1960 period in the U.S., where every large monetary contraction can reasonably be viewed as unanticipated, but its power in interpreting historical events received striking demonstrations in Sargent's (1986) studies of the disinflations that ended the European hyperinflations and the moderate French inflation of the 1920s. An unqualified association between monetary contractions (in the sense of reductions in the growth rate of money) and real activity would lead one to expect these disinflations to have been associated with major depressions. Sargent's analysis of the political context within which these contractions occurred shows that one can interpret them as anticipated, even though sudden and drastic, and hence reconcile their magnitude with the modesty of the real effects they induced.

Sims (1972) took a very different approach to the study of monetary influences on real activity, also explicitly in debt to Friedman and Schwartz, in his 'Money, Income, and Causality'. Rather than attempting to construct an *economic* model consistent with the principles applied in *A Monetary History*, Sims developed a purely statistical definition of *cause*, related to Granger (1969), in terms of lead–lag relations among variables. Sims's methods provide a test of the hypothesis that movements in money cause (in his sense) real output movements, estimates of a kind of dynamic money multiplier, and estimates of the fraction of output variance, by frequency, that can be accounted for by monetary instability. Sims also argues convincingly that lead–lag considerations play a very similar role, though not formalized in the same way, in Friedman and Schwartz's discussion of what they term the 'independence' of money changes.

More recently, Romer and Romer (1989) have drawn on Friedman and Schwartz's discussion of the independence of monetary changes in a related way, arguing for the use of historical evidence to establish that particular money movements – 'natural experiments' – did not occur in response to real events. They credit this method to Friedman and Schwartz, though they do not believe Friedman and Schwartz were successful in applying it, and they, too, argue convincingly that its roots can be traced to *A Monetary History*. For Romer and Romer, exogeneity is a property of a particular realization, while for Sims it is a property of a distribution: the two approaches are not the same. Friedman and Schwartz's discussion of independence is sufficiently unclear that both interpretations are defensible. So, too, is a third, which I prefer, which is that independence as Friedman and Schwartz use the term has nothing to do with statistical exogeneity, but means rather that whatever the sources of monetary contractions may have been, on average or in particular instances, the monetary authorities *could* have maintained M2 growth had they chosen to do so. It is independence in this sense that is, I think, conclusively defended by Friedman and Schwartz in detailed analysis of episode after episode.

I do not see any possibility of obtaining answers to normative questions of economic policy by atheoretical, purely statistical means. But the attempt to estimate the fraction of real variability (over a particular period) that can be attributed to monetary instability by atheoretical (Sims) or similar methods that use very little theory [e.g., Shapiro and Watson (1988)] is certainly worth pursuing, and success in this effort would obviously be immensely useful in guiding future theorizing. Certainly admirers of Friedman and Schwartz do not want to be drawn into arguments over whether theory or facts should come first!

6.

If the 1970s were a time of prosperity for the influence of *A Monetary History*, the 1980s must be viewed as at least a mild recession. With Kydland and Prescott's (1982) development of a purely real stochastic growth model that is operational enough to stand comparison to postwar U.S. time series, the role of monetary shocks has faded into the background of professional discussion. The idea that 'money doesn't matter', attributed (unfairly, I think) to Keynesians by Friedman and Schwartz, is now embraced even by many former monetarists. As a result, the last ten years have yielded little ostensible progress in our understanding of the appropriateness of different kinds of monetary policies. Kydland and Prescott showed, and much subsequent research has confirmed, that with the variance of productivity shocks matched to the variance of total factor productivity growth measured as in Solow (1957), such shocks can induce output variability of about the same magnitude as observed in the U.S. in the postwar period, as well as realistic behavior of other variables.

Viewed as positive theory, real business cycle models do not offer a serious alternative to Friedman and Schwartz's monetary account of the early 1930s. The Solow (1957) residuals for the years 1928 through 1933 were: 0.020, − 0.043, 0.024, 0.023, 0.011, 0.072! There is no real business cycle model that can map these shocks into anything like the 40% decline in real output and employment that occured between 1929 and 1933 (nor, indeed, does anyone claim that there is). Even if there were, imagine trying to rewrite the Great Contraction chapter of *A Monetary History* with shocks of this kind playing the role Friedman and Schwartz assign to monetary contractions. What technological or psychological events could have induced such behavior in a large, diversified economy? How could such events have gone unremarked at the time, and remain invisible even to hindsight? It is surely no accident that no one has attempted to apply real business cycle theory to the 90-year period that Friedman and Schwartz studied.

In Kydland and Prescott's original model, and in many (though not all) of its descendants, the equilibrium allocation coincides with the optimal allocation: Fluctuations generated by the model represent an efficient response to unavoidable shocks to productivity. One may thus think of the model not as a positive theory suited to all historical time periods but as a normative benchmark providing a good approximation to events when monetary policy is conducted well and a bad approximation when it is not. Viewed in this way, the theory's relative success in accounting for postwar experience can be interpreted as evidence that postwar monetary policy has resulted in near-efficient behavior, not as evidence that money doesn't matter.

Indeed, the discipline of real business cycle theory has made it more difficult to defend real alternatives to a monetary account of the 1930s than it was 30 years ago. It would be a term-paper-size exercise, for example, to work out the possible effects of the 1930 Smoot–Hawley Tariff in a suitably adapted real business cycle model. By now, we have accumulated enough quantitative experience with such models to be sure that the aggregate effects of such a policy (in an economy with a 5% foreign trade sector before the Act and perhaps a percentage point less after) would be trivial.

Whatever one's views on the potential of real business cycle theory as positive economics, it has taken normative discussion in macroeconomics to a new level, where the efficiency of fluctuating time paths of real variables can be assessed in the same terms we routinely apply to welfare analysis in other areas of economics. Once one *states* the question of efficiency the way Kydland and Prescott did, it is evident that the perfect smoothing of real output growth is not a sensible objective of policy, and that attempts to attain it would entail large welfare costs. (Indeed, with hindsight one wonders why this question was not raised in the context of the old Keynesian models, in which fluctuations are largely driven by shocks to private spending.) Beyond this qualitative observation, it appears that *quantitatively* efficient output fluctuations are of the same order of magnitude as observed fluctuations in the postwar period.

Of course, research on the cyclical role of money has also continued in the last decade. The models in Taylor's (1993) recent monograph capture the effects of monetary forces in an operational, quantitative way. McCallum's (1988, 1990) analyses of base control rules, while not based on any specific economic model, are grounded in a sophisticated understanding of what is useful in recent theoretical research. Models in the style of Kydland and Prescott are now being adapted to the study of nonneutral monetary influences, though it is far from clear how this might best be done and to what extent such modifications will improve empirical performance. The reward from success in this enterprise is very high, since these models admit meaningful normative comparisons of alternative monetary policy rules in a way that earlier models did not. The prospects for success depend, I think, on our willingness to leave the placid and familiar world of postwar quarterly time series and test our ideas against the events of the interwar period.

7.

A Monetary History of the United States is a remarkable and durable achievement of historical and economic scholarship. Friedman and Schwartz used a few basic economic principles to organize nine decades of tremendously varied economic history into a coherent picture, in which the main events become understandable as the effects of identifiable causes. It is a picture that is consistent with our instinct that the depression of the 1930s was an event that should not have happened, a preventable disaster. The role of the Federal Reserve System, the institution that was created to prevent such disasters and that had ample power to do so, is described in enough detail that one can see how disaster can follow from arrangements that grant wide discretion to well-intentioned managers, secure in their business-world sophistication, ignorant of economics and of economic history.

This thirtieth anniversary review has focused on subsequent research that seems to me to have the promise of sharpening the picture provided by

A Monetary History to the point where questions passed over or given only qualitative answers by Friedman and Schwartz might be answered quantitatively with some reliability. This focus has taken me far into what Tobin (1965) called 'the parochial disputes of monetary theorists'. That is what I get paid to do but, as was Tobin, I find myself relieved to agree with Friedman and Schwartz that we already know enough, and knew enough in 1963, to avoid the major policy mistakes of the interwar period. Whatever may be the influence of *A Monetary History of the United States* on future research, it will stand as the classic statement of these important lessons from our past.

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