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MONETARISTS AND KEYNESIANS ON
CENTRAL BANKING:
A STUDY OF A FAILED DEBATE

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Monetarists and Keynesians on Central Banking:

A Case Study of a Failed Debate

Thomas Mayer

The study of economics is justified more by the practical uses of economics than by its aesthetic value. Economists therefore feel obligated to offer policy advice on various issues on which they do not have compelling evidence. That is to the good. But they are tempted in these cases to lay claim to more certainty than is warranted, in part because consumers of economics, that is policymakers and students, want unequivocal answers. Moreover, their rivals, e.g., journalists and politicians are not at all reluctant to overstate the value of their wares. But exaggerating the degree of confidence that one's conclusions warrant can lead to a degradation of the debate within the profession as schools form that pay insufficient attention to what others are saying, and as battle lines are sharply drawn.

I will argue that this is what has happened with respect to the debate about whether central banks should undertake counter-cyclical policy or have the money stock grow at a stable rate. I do not claim that the participants intentionally overstate their cases or ignore what the other side is saying; they believe in what they say. But their motivation is not the issue. What matters is the outcome, an unwarranted claim to knowledge that has retarded understanding, in part by posing a false dichotomy. The methodological obstacle to resolving the debate has not been some subtle issue, such as economists not knowing their Popper, or adhering to an outdated philosophy of science, but a failure to stay within the bounds of rational conversation. It would be interesting to see if a similar tendency shows up in the natural sciences as they deal more with issues of environmental policy.

INTRODUCTION

The two sides to the debate are the monetarists, lead by Milton Friedman, Karl Brunner and Allan Meltzer who advocate that the money supply should grow at a fixed, or at least stable rate. and those who advocate counter-cyclical monetary policy. I will, rather loosely refer to the latter as "Keynesians".¹ I deal only with the question whether discretionary counter-cyclical policy (henceforth just called counter-cyclical policy) is feasible, and not with the entire issue of the quantity theory vs. Keynesian theory. It is quite possible that Keynesian theory is superior to the quantity theory; for example, that fiscal policy has a powerful effect on GDP, and accounts for as large proportion of the GDP variance than do changes in the money supply. and yet effective counter-cyclical policy cannot succeed because of lags, forecast errors and principal-agent problems. Conversely, there is nothing in the quantity theory that denies the efficacy of counter-cyclical monetary policy. This paper therefore deals only with part of the Keynesian-monetarist debate. I also do not take up the subsequent debate between the Keynesians and the New Classicals, since that involves quite different issues.

I start with 1947 when Milton Friedman published his critique of Abba Lerner's recommendations for stabilization policy. and essentially end forty years later, with just a brief look at the subsequent literature. Although the debate has continued beyond 1987, it has been substantially influenced by new factors: the breakdown in the stability of velocity in the early 1980s, the widespread acceptance of time-inconsistency as a major problem for monetary policy, and the consequent emphasis on feedback rules.

¹ Purely for convenience I will use the term "monetarists" to denote those who for the reasons given by Friedman and by Brunner and Meltzer reject the use of counter-cyclical policy. They should actually be called "strong monetarists", since one can be a monetarist without going that far. (See Friedman, 1983) Similarly, one can accept the essence of the General Theory, while opposing counter-cyclical policy. But the use of the term "Keynesian" for advocacy of counter-cyclical monetary policy can be defended by reference to Keynes' *A Tract on Monetary Reform* (1924)

Since the paper is long it may be useful to outline the various steps. I start by discussing the nature of the issue and the background in which it arose before turning to the monetarist case. One part of this case is that since monetary policy operates with long and variable lags, and since central banks have only a limited ability to forecast, effective counter-cyclical policy is not feasible. This requires looking at the length and variability of the lag, and at the Keynesians' answer that control theory shows that such a policy is feasible after all. I then turn to the other part of the monetarists' case, that even if effective stabilization policy were technically feasible, central banks would be unlikely to carry out such a policy. While monetarists have offered considerable, but not conclusive historical evidence on this issue, Keynesians have largely brushed it aside with disdain, perhaps on the grounds that monetarist views are driven by ideology. Subsequently I discuss the Keynesian criticisms of a fixed monetary growth-rate rule, and then turn to empirical tests, and to some recent developments that have changed this debate. This whole debate is hardly a model of McCloskey's (1985) "good conversation", and I ask what went wrong. The following two sections deal with the relation of this debate to some literature on philosophy of science, and suggest some reasons why the debate was not more productive.

WHAT IS THE QUESTION?

The issue is often formulated as "rules versus authorities", that is as a discretionary monetary policy versus a stable growth-rate rule for some monetary aggregate. Such a broad formulation is dubious. One should distinguish between at least three regimes, that is rules governing policy actions. One is a *strict* rule, that requires the central bank to generate a specific growth rate for a particular monetary aggregate, come what may. The second, a *moderate rule*, allows the central bank to change the monetary growth rate in response to supply shocks, or to perceived secular changes in velocity and in potential real growth, but not for the sake of

undertaking counter-cyclical policy. The third, is our current system of discretionary policy in which the central bank is free to vary the monetary growth rate for any purpose. Within these three regimes different variants are possible. For example, a central bank may be permitted to undertake counter-cyclical policy only in response to severe cycles, or else. decisions to respond to perceived secular changes in velocity and to supply shocks may require approval by the legislature. While Friedman (1960) has advocated the strict version of the monetary rule, Brunner (1984) has advocated a carefully circumscribed moderate version.

These three regimes should not be confounded. Evidence against the strict rule is not necessarily evidence against the moderate rule. For example it would have been a disaster had the Fed. adopted a strict monetary rule in 1980, because of the subsequent sharp fall in the secular trend of velocity. But that does not mean that the moderate rule, with its prohibition of counter-cyclical policy would have been bad. Similarly, evidence that the central bank does not possess the information required for effective *discretionary* counter-cyclical policy does not suffice to establish the case for a strict monetary growth-rate rule.

Furthermore, suppose there is strong empirical evidence that discretionary monetary policy has led to a better outcome than a strict monetary rule would have done. This does not suffice to make the case for counter-cyclical monetary policy, because the superiority of discretionary policy could be due entirely to its accommodation of secular changes in velocity. Conversely, if a monetary rule shows a better outcome, a policy of responding only to secular and not to cyclical changes in velocity might be better still.

In the vehemence of the debate such *refinements* have often been ignored. Monetarists have frequently written as though convincing evidence that central banks lack the information and the resolve needed for effective counter-cyclical policy suffices to establish the case for a stable monetary growth-rate rule, while Keynesians have often argued as though

the existence of secular changes in velocity and the occurrence of supply shocks provides a compelling case for counter-cyclical policy.

BACKGROUND

It is certainly not surprising that by, say 1946 counter-cyclical stabilization policy had become a central tenet of mainstream economics as taught in most leading American universities.

Although this was hardly a new contribution of the *General Theory* (see Keynes, 1924), the *General Theory* had provided such policy with a much more coherent theoretical justification, and the Great *Depression* had appeared to give it a practical justification.

Lerner's *Economics of Control* (1944) provides a paradigm of the "new economics".⁴

Monetary policy, having been freed from the constraint of the gold standard and the irrational constraints of "orthodox finance", *can* now be used entirely for domestic goals with international equilibrium being treated with benign neglect. Counter-cyclical fiscal policy no longer needs a special justification now that Keynes has decisively refuted the so-called "Treasury View" that government deficits crowd out private expenditures dollar for dollar, and that Lerner has shown that an internally held government debt is harmless.

Lerner could therefore set out the following welfare-maximizing rules for macro policy: if aggregate demand is insufficient then lower taxes, raise government expenditures, or lower interest rates. If aggregate demand is excessive do the opposite. The appropriate choice between these three tools depends on microeconomic considerations, that is on the marginal social products of consumption, government expenditures and investment.⁵ To finance its deficits the government should either print money or, if it wants interest rates to rise, borrow. It

⁴ In 1984 Scitovsky called it "the generally accepted framework within which many, perhaps most, economists think and argue about fiscal and monetary policy." (Scitovsky, 1984, p. 1561)

⁵ Lerner's book deals mainly with microeconomics, and sets out the social optimization rules for economic efficiency applicable to both capitalist and socialist economies.

all seemed very simple. except for the task of inducing politicians and the general public to abandon their irrational prejudice. Macropolicy has finally reached the age of enlightenment in which reason rather than tradition and prejudice govern. Indeed, it is likely that much of the enthusiasm for the Keynesian revolution resulted from its seeming ability to make economists extraordinarily useful. In the 1960s there was much debate about whether the business cycle still existed, or had been eliminated by modern macroeconomic policy.

Lerner was a theorist with little, if any interest in institutions.' He saw his role as setting out abstract economic principles, with the problems of implementation left to administrators. While other economists, particularly the institutionalists, held a less pure view of economics, it is still true that by modern standards mainstream macroeconomics at that time showed little concern with the problems that arise in implementing their policy recommendations. For example, in the 1960s many economists wanted to make fiscal policy more flexible by having Congress grant the president the power to adjust tax rates as economic conditions change. They could not understand why practical politicians scoffed at this "logical" step. Similarly, until the late 1950s almost nothing had been published on the lags of monetary policy.

It would be going too far to say that most macro-economists were unconcerned with *all* practical details. For example, they were willing to specify by how *much* interest rates should be changed. But they did not feel compelled to work through all the steps required to translate their general policy prescriptions into coherent advice in a world in which policymakers have imperfect information and serious principal/agent problems exist. At the time that did not seem an interesting set of problems. Insufficient attention to such problems was prevalent in microeconomics, too.

* Indeed, a main theme of his *Economics of Control* was that the issue of capitalism vs. socialism was of little relevance, that one can specify optimality rules, e.g. setting marginal social benefits equal to marginal social costs, that are the same for capitalist and socialist economies.

This deficiency of mainstream macroeconomics soon came under attack. In his review article of Lerner's *Economics of Control*, (Friedman 1947) mounted a powerful methodological critique by arguing that Lerner purported to give practical advice, when he actually did little more than tell the government that it should behave intelligently. To Friedman the question of whether his policy recommendations can be effectively implemented is not something that an economist should ignore.

To make ... [his recommendations for counter-cyclical policy into] a prescription to "produce full employment," Lerner must tell us how to know when there is "insufficient total demand," whether this insufficiency is a temporary deficiency in the process of being corrected or the beginning of an increasing deficiency. ... He must tell us how to know what medicine to use when a diagnosis has been made, how large a dose to give, and how long we may expect it to take for the medicine to be effective. The casual reader of Lerner's book -- or for that matter, of the majority of works on the control of the business cycle -- might suppose that these are simple questions. ... [T]hey are anything but simple. An easy answer to these difficulties is to say that ... errors in these actions are unimportant since they can be corrected quickly. ... This answer is, of course too easy, it conflicts with the hard fact that neither government action nor the effect of that action is instantaneous. ... Unfortunately, it is likely that the time lags are a substantial fraction of the duration of the cyclical movements. ... By the time an error is recognized and corrective action taken, the damage may be done, and the corrective action may itself turn into a further error. This prescription of Lerner's ... thus turns into an exhortation to do the right thing with no advice how to know what is the right thing to do. (Friedman, 1947, pp. 413-15, emphasis added.)

I have quoted this passage at length because it summarizes so well, about thirty years before Keynesian theory lost its hegemony, one of the two basic monetarist challenges to the prevailing Keynesian consensus on macro-policy, which are that due to the lag in its effects monetary policy is too inflexible, and second that central banks are often motivated by goals that conflict with stabilization.

LAGS AND FORECAST ERRORS

Friedman returned to the question whether governments know enough to conduct an effective stabilization policy in his "The Effects of a Full-Employment Policy on Economic Stabilization: A

Formal Analysis", originally published in French in 1951 and reprinted in his Essays *in Positive Economics* (1953)⁵. In it he poses the following questions: "Under what conditions will counter-cyclical action succeed in its objective of reducing instability? Under what conditions will it actually increase instability? How does its effectiveness depend on the magnitude of action?" - (Friedman, 1953, p. 117) Thus Friedman goes much further than others towards posing the practical questions that have to be answered for stabilization policy to be implemented effectively. Yet he is almost apologetic for the abstraction of his analysis, writing:

The present note considers these questions on a highly formal level. its purpose is primarily to make clear that they are important and relevant questions; secondarily to indicate in general terms the considerations on which an answer in any particular case depends. (Friedman, '1953, p. 117)

Specifically Friedman interprets stabilization in a statistical framework as the minimization of the standard deviation of income. The variance of income if stabilization policy is undertaken is $\sigma_x^2 + \sigma_y^2 + 2R\sigma_x\sigma_y$ where σ_x^2 the variance of income in the absence of stabilization policy, σ_y is the size of the stabilization policy (measured by the change in income that it generates), and R is the correlation coefficient of σ_x^2 and σ_y^2 , that is the measure of how well timed the stabilization policy is. The optimal size of the stabilization policy equals $-R\sigma_x$, and the policy will be destabilizing if $\sigma_y/\sigma_x > -R$. This implies that it is far from obvious that the central bank can succeed in significantly reducing the magnitude of fluctuations. If it adopts a strong policy it may well be destabilizing, while a modest and cautious policy means that it cannot do much good. It is by no means unreasonable to expect even an only moderately ambitious central bank to destabilize income. Assume for example, that the central bank tries to offset, say one third of the standard deviation of income. It surely does not know

⁵ Five years earlier in a long footnote in his review article of Lerner's book Friedman (1947, pp. 414-15) had already stated the basic insight of this paper. The term "formal" in Friedman's title should, of course, be read in the context of the time, nowadays this paper would not be considered "formal".

with any accuracy by how much it has to change interest rates or the growth rate of reserves to set $\sigma_y/\sigma_x = 1/3$. And suppose it does change income by just this amount, its policy will be destabilizing if the change in income is badly timed, so that $R < -0.7$. But to time the policy better than that the central bank has to predict with sufficient accuracy the behavior of income in the absence of the policy, that is σ_x , as well as the distributed lag in the effect of its policy.

Does the central bank possess the required knowledge? Friedman does not provide any estimates of the relevant parameters, and hence cannot answer this question. Indeed, given the available data, there is no way in which he could have estimated them. But what he does show is that in the absence of supporting empirical evidence one cannot dismiss out of hand the hypothesis that a substantial stabilization policy is more likely to destabilize than to stabilize income. In other words, Friedman did not *refute* the Keynesian belief in the effectiveness of stabilization policy but he did show that this belief rested on an unsupported assumption, and conjectured that this assumption is invalid.

The severity of this problem was even greater at the time Friedman's paper appeared than it is now. The Federal Reserve did not make any explicit forecasts of GDP, indeed until well into the postwar period the Federal Reserve Board's staff was *forbidden* to make any forecast, and staff members could be fired for making one. To be sure, policymakers had to make implicit forecasts but they presumably did so primarily by projecting current conditions and trends into the future. Since at that time the Fed appeared to believe that monetary policy affects income with only a very short lag, this was not entirely unreasonable. But the Fed had no evidence that the lag of monetary policy was short, and apparently had not even studied this problem. Moreover, there is no reason to think that the Fed, or anyone else around the time, knew with any degree of accuracy by how much a given change in the interest rate or the money supply would change GDP.

Let us imagine that one were writing a rational reconstruction of a debate on monetary rules that obeyed the basic rules on which just about all methodologists -- except for the Austrians -- are in agreement. Economists would then have reacted in one of three ways. One is to follow up on Friedman's analysis by seeing when data on Fed forecasts became available if the empirical estimates of σ_y^2/σ_x^2 and R support Friedman's conjecture that a stabilization policy of significant size cannot succeed. The second is to present an alternative framework that allows one to test this conjecture. The third is to admit that we do not know whether counter-cyclical monetary policy can be a significant stabilizer. But that is not what happened.

Advocates of counter-cyclical stabilization policy essentially ignored both Friedman's review of Lerner and his 1953 paper. Thus, although most of Friedman's papers are massively cited, his review article of Lerner's book is cited only six times in the *Social Science Citation Index (SSCI)*, and none of these citations refer to Friedman's criticism of Lerner's stabilization policy. The sum-of-variances paper has been occasionally referred to, but with one exception the citations I found are not critical; mostly they are hat-tipping citations that merely mentioned the paper, or cited it approvingly, or else used it rather than criticized it. The one exception is a paper (Orr, 1960) pointing out that Friedman's framework applies only to a policy of stabilizing GDP around a given trend, and not to a policy that tries to reduce the gap between actual and potential GDP. Yet for much of the postwar period the emphasis was as much, if not more, on the latter than on the former. But Friedman made it clear that he was considering only counter-cyclical policy.⁶

There is, however, at least one paper that seems to criticize Friedman's conclusion, though without citing his paper. Buiter (1981) showed that a closed-loop system, that is a

⁶ Friedman, did not refer to this paper very often. Perhaps the reason is that it presents only a possibility argument, and that Friedman wanted to claim more than that.

system in which policy responds to the state of the economy, is superior to an open-loop system. This is formally correct if, as Buiter postulates, the authorities respond correctly. Buiter's results can readily be obtained within Friedman's framework. As long as R is negative there is some stabilization policy, however weak, that reduces the variance of income.

Hence, in a formal sense, Friedman's skepticism with respect to any counter-cyclical policy however small cannot be justified on the basis of lags and forecast errors alone. But this formal result does not necessarily have much practical significance. If R , though negative is very small in absolute terms, then even an ideal counter-cyclical policy would reduce fluctuations only very slightly. For example, suppose that $R = -0.2$. Then, on the strong assumption that the central bank estimates correctly the effect of its policy action on GDP, it could eliminate only 4 percent of the variance of GDP. But since it is uncertain about the effect of its policy on GDP it should aim at less than 4 percent, say 2 percent. The benefit of even such a small reduction in the GDP variance is for just about any economy greater than the cost of undertaking the required open market operations and the required research. It may therefore seem that Buiter has refuted Friedman's claim that because of lags and forecast errors counter-cyclical policy cannot be stabilizing at all. But Friedman has not made such a strong claim. He only claims that (a) lags and forecast errors severely limit the extent to which counter-cyclical policy can reduce fluctuations, and instead may cause such a policy to be destabilizing, and (b) that central banks usually pursue policies that are badly designed from the viewpoint of stabilization.

Since it is most foolhardy to say that a certain paper has been explicitly cited critically only once, I will not do so. But I can say that if another explicit criticism of Friedman's sum-of-variances paper was published it was an outlier that did not play a meaningful role in

the debate.' And it is most unlikely that there is much of a literature that like Buiter (1981) responded to Friedman's paper without citing it.

The neglect of these two papers cannot be due to economists not being aware of them. Friedman's review article appeared in the *Journal of Political Economy*, hardly an obscure source, while his "Formal Analysis" essay appeared in a book that had high visibility, since it contains his much-cited essay on the methodology of positive economics. Indeed, rightly or wrongly, William White (1961, p. 142) referred to it as "well-known", and as "one of the most important sources of this skepticism about anticyclical *measures*." And the failure to respond to Friedman can hardly *be* attributed to a tendency to ignore the work of a young, unknown scholar, since by that time Friedman was already a leading economist who had won the AEA's John Bates Clark medal in 1951.⁹

A cynic might attribute it to the reluctance of the (at the time) mostly liberal economics profession to pay attention to papers written by a member of the Chicago school. But this explanation will not do. Friedman's other writings have garnered a vast number of citations

Moreover, Friedman was not the only one who contended that stabilization policy might very easily be destabilizing. Three years later A. W. Phillips (1957), who had formulated the economic stabilization problem in a control-theory setting, concluded that:

[I]f the lags in the real economic system are at all similar to those we have used in the models it is unlikely that the period needed to restore any desired equilibrium conditions after an economy has experienced a severe disturbance could be much less than two years, even assuming that the regulating authorities use the policy which is most appropriate to the real system of

⁹ I have two grounds for saying this. First, over the years I have followed this literature closely, and do not recollect such a paper. Second, I undertook an admittedly not exhaustive (but certainly exhausting) search of the SSCI for the years 1955-1974 when the paper, being new, is most likely to have evoked a critical response. I did not cover subsequent years because of the labor involved in tracking down references to this paper. The SSCI index lists a vast number of references to Friedman's *Essays in Positive Economics*, but only a small proportion of these relate to this particular essay.

¹ This medal is awarded once every two years to the economist under 40 who is judged to have made the greatest contribution to economics.

relationships existing in the economy, *As these relationships are not known quantitatively, it is unlikely that the policy applied will be the most appropriate one, it may well cause cyclical fluctuations rather than eliminate them.* ('Phillips, 1957. p. 276, emphasis added.)

Subsequently, William Baumol (1961) also using control theory analyzed the effects of stabilization policy, both with and without lags, in the setting of a multiplier-accelerator model. He described his results as: "... somewhat frightening. Plausible and reasonable contracyclical policies turn out to be capable of increasing the explosiveness and frequency of economic fluctuations. In fact, none of the possibilities examined proved to be entirely harmless in these respects." (Baumol, 1961, p. 24).

Friedman is not the only monetarist who questions the technical feasibility of successful stabilization policy. Karl Brunner (1980) presented his own variant of the infeasibility argument. He pointed out that long and variable lags **are** not the basic issue. Instead, the **basic** issue is whether the central bank **possesses** enough information about **the** future course of GDP and **about** the effects **of its** policy. But Brunner did not **present as elegant a** formulation of the problem as Friedman had done.

These papers had little if any effect on the prevailing consensus, as shown, for example, by the treatment of stabilization policy in elementary textbooks. One possibility is that they were not considered a major challenge, because although they established the possibility that counter-cyclical policy is destabilizing they did not provide any empirical evidence to support their conjecture that such policy actually is destabilizing. This possible explanation needs detailed discussion.

Empirical Implementation

In Friedman's formulation the parameters that determine whether macro policy is stabilizing are the size of policy-induced variance in GDP relative to the initial variance and the correlation of

these two variances. Assuming that the central bank is single-mindedly devoted to stabilization policy (an issue discussed below), this correlation depends on the accuracy with which it can forecast GDP and predict the effects of its policy.

One determinant of the ability to forecast GDP is the horizon of the forecast. Obviously, if the lag in the effect of monetary policy were a decade, then stabilization would be a hopeless task, while, if it were, a week, then a central bank that is well informed about current GDP should be an effective stabilizer. Not only would its estimate of the deviation of GDP from its target be accurate, but if it would make a mistake in estimating this deviation, or in estimating *the* effect of its policy on GDP it could readily reverse its policy.

Friedman and Schwartz therefore tried to estimate the lag of monetary policy (Friedman, 1958, 1961, Friedman and Schwartz, 1963a) by comparing the turning points in the growth rate of money with business cycle turning points. They found what at the time were considered long lags: for peacetime cycles a lag of sixteen months at peaks and twelve months at troughs. (Friedman, 1961) Using instead of turning points in the growth rate of money, the dates at which persistent changes in the money growth rate occurred, the lag is shorter, five months at the peaks and four months at trough. All of these are measures only of the "outside lag", that is the lag between the change in the growth rate of the money supply and cyclical turning points, thus excluding the lag between the time a change in monetary policy is needed and the time at which the Fed changes the money supply.⁹

Many other estimates made around the same time, or shortly thereafter, using various methods reached roughly similar results. So did a number of subsequent studies that measured the lag in monetary policy by seeing for how long an increase in the quantity of

Friedman and Meiselman (1963) also measured the lag by running lagged correlations between money and income. These suggest a three to four quarter lag. But Friedman and Meiselman's analysis is problematic, see Modigliani and Ando (1965) and DePrano and Mayer (1963)

money lowers the interest rate." Although a few studies of the lag found it to be much shorter than Friedman and Schwartz's estimate, this was not true for most.

However, Friedman and Schwartz's estimate of the lag was challenged by John Culbertson (1960) and by John Kareken and Robert Solow (1963). But, although Kareken and Solow claim that their data show a shorter lag than Friedman's, this is not so." Culbertson's conceptual criticism is more telling and led to a debate with Friedman (Culbertson, 1961, Friedman, 1961) in which the main issues were Friedman and Schwartz's comparison of the turning points in the *rate of change* of money with turning points in essentially the *level* of business activity, and also the direction of causality between money and income

This debate was superseded by the deeper criticism of William Brainard and James Tobin (1968) and Tobin (1970) which is embedded in their basic challenge to Friedman's inference of a causal relation from the timing of changes in money and in income. Brainard and Tobin simulated a small econometric model that exhibited many cases in which peaks in endogenous variables lead peaks in exogenous variables. Tobin then demonstrated this result for two theoretical models, one Keynesian and one monetarist. By showing that simple, and hence feasible procedures can give misleading results, Brainard and Tobin showed the difficulty of doing empirical work in this area. And inability to see exactly what inside the bowels of the Brainard-Tobin model produced their counter-intuitive conclusions perhaps further weakened the faith of economists in empirical work in this area.

For a survey of the earlier studies see (Mayer 1967). For an example of an approach that subsequently became widespread see Cagan and Gandolfi (1969).

Kareken and Solow estimate lags for various types of expenditures, but do not combine them into an overall lag. When one does so one obtains an estimate of the lag that **is not** shorter than Friedman's, **see** (Mayer 1967). In general, the Kareken-Solow paper appears to be somewhat incomplete, which might perhaps be explained by it being a commissioned paper (for the Commission on Money and Credit) that was written under a deadline.

One part of Friedman's reply to his critics contains a point that is central to his measurement of the lag. This is that his conclusion that money is causal is based much more on the historical evidence drawn from specific instances of changes in the money supply (see Friedman and Schwartz (1963b), than it is on the finding that turning points in money precede turning points in income. He therefore referred to the turning-point evidence as "by no means decisive." (Friedman, 1961, p. 449) But while his historical evidence may well be strong enough to support the quantity theory, it does not provide any justification for deriving the lag of monetary policy from a comparison of turning points in money and income. Here Friedman is relying on evidence that he himself does not consider decisive.

The controversy about Friedman's measurement of the lag sounds dated because nowadays the standard procedure for estimating the lag is to use an econometric model to simulate the effect of a change in monetary policy. As the examples given in the Appendix show, these simulations usually exhibit lags that are longer than those estimated by Friedman and Schwartz. This is not surprising since in such essentially Keynesian models much of the effect of monetary policy on expenditures usually comes through the long-term interest rate, and that rate is treated as adjusting only slowly to changes in the instruments that the central bank controls directly.

Suppose the econometric models are correct and the lag of monetary policy is as long as they show. Does that prevent stabilization policy from succeeding? ¹²The answer is not

In a paper that for some reason did not make the impact on the debate that it deserves Donald Tucker (1966) pointed out that the relevant lag is not measured correctly by the length of time it takes a dollar of new money to raise GDP. Suppose that it takes a long time for firms to increase investment when interest rates fall. If so, to equilibrate the money market interest rates have to fall further than they would do otherwise, thus stimulating additional investment early in the period, and reducing the weighted mean lag. This makes the story much more complicated with the timing of the effect of monetary policy now depending on lags in the money demand function as well as on lags in the expenditure functions. However Tucker's argument applies only

obvious, and raises five questions. first, what is the relation between the length of the lag and the central bank's forecast errors? Second, how does the length of the lag affect the feasibility of offsetting prior errors? Third, can the central bank estimate the length of the lag with sufficient precision? Fourth, does the lag vary substantially from case to case, so that knowledge of the *average* lag does not suffice? Fifth, can the central bank predict the effect of its actions on GDP closely enough?

At first glance it may seem obvious that the longer the lag, and hence the forecast horizon, the less accurate is the forecast. But that is not necessarily so. On the one hand, it is obviously easier to estimate what the gap between actual and desired GDP will be next month than what it will be, say five years from now, because the economy will not change much in one month. But that need not be so for less extreme comparisons. The variable that the central bank has to forecast is not the percentage change in GDP that will occur say five quarters from now, but what the gap between actual and desired GDP will be then. And the more quarters there are between the current quarter and the quarter being forecast, the greater is the chance that the errors made in estimates for the intervening quarters will cancel out. It is therefore not surprising that, as Stephen McNees (1988, p. 22) found, although forecast errors do tend to decline as the forecast period is reduced, at first "the improvement is quite gradual and summary error measures are virtually constant as the forecast horizon decreases. We know little more today about what will happen in a given quarter a year from now than we do about a quarter two years from now."

Another, and probably more serious problem that a long lag creates relates to the reversibility of the effects of monetary policy. Assume that only 10 percent of the effect on GDP occurs in the first quarter, and 50 percent occurs in the fourth quarter. Suppose now that

if the central bank sets a certain growth in the money supply (which is what monetarists recommend) and not if it sets the interest rate.

the central bank realizes that it has made a mistake and wants to reverse within the next quarter the effects of a restrictive policy that it adopted three quarters ago. It could, in principle, do so by initiating an expansionary policy that is five times as strong as the previous restrictive policy. Subsequently, it would then have to adopt an even stronger restrictive policy to offset the effects of the strong expansionary policy in later quarters. But no central bank is likely to do that. It is often uncertain about what its policy should be, and that makes it reluctant to undertake strong policies that might later appear to be wrong, if only because it has to be concerned about its reputation. Moreover, large policy shifts generate wide swings in interest rates, and central banks favor interest-rate stability. There is even some danger of instrument instability, that is an explosive increase in the variability of instrument settings, though that is not as likely a possibility as may appear at first glance. (See Holbrook, 1972.)

An additional problem is that the central bank does not have a good estimate of the length of the lag. It therefore does not really know at what future GDP gap it should aim in deciding its current policy stance. The distributed lags generated by various econometric models differ widely. The central bank has no way of knowing which lag estimate it should use because there is no way one can evaluate the accuracy of simulations, except for egregiously wrong ones.¹³

Even if the central bank would know which model's estimate of the distributed lag is correct, it would still face the problem that the models estimate only the mean lag. A central

¹³ The accuracy of a model's GDP forecasts does not provide a good indication of how accurately it simulates the effects of monetary policy. For example, model A may give a better GDP forecasts than model B because its consumption function is more accurate, and at the same time its money demand function may be worse than model B's, so that it gives a worse estimate of the effect of monetary policy. Moreover, as MENGES (1988) points out, past differences in the accuracy of various forecasters are often too small to be a reliable guide to their future performance.

bank deciding what to do at a particular time needs to know. not the average lag, but what the lag will be in this particular instance. There is no reason to assume that the variance of lags is small enough for the mean lag to be a reliable guide for specific policy decisions,

When Friedman and Schwartz (1963b) measured the lag of monetary policy by comparing turning points in the growth rate of money and cyclical turning points they found the lag to be highly variable, But, as already, discussed such a comparison of turning points is not a good measure of the lag (See also Mayer, 1967.) Better evidence on the variance of the lag comes from two papers that used variable-coefficient techniques. Cargill and Meyer (1978) estimated for the 1950s and 1960s the impact of monetary policy in the St. Louis model and also in a small Keynesian model, allowing the coefficients to vary over time. They found that both the length and the shape of the distributed lag of monetary policy varied significantly, depending upon when the policy was inaugurated. They argued that this provides one more reason to avoid "overly zealous" stabilization policies, though they left open the possibility that the differences in lags might be predictable. J. E Tanner (1979) considered this possibility in a paper using a simplified version of the St. Louis model. He found that the lag was substantially longer in the 1960s than in the 1950s, and that the length of lag strongly depends on the type of policy (with a restrictive policy having a longer lag than an expansionary policy). It also depends on the stage of the business cycle; in a recession policy is impotent for at least eight quarters. Moreover, the lag is longer for a tight policy than for a restrictive policy.

These findings suggest that the estimate of the distributed lag that an econometric model provides, i.e., the mean lag for its sample period, is not sufficient for effective policymaking. Admittedly, neither study is beyond criticism. Both use the St. Louis model, which has been heavily criticized (most effectively by Modigliani and Ando, 1976) and has performed very badly in recent years. To be sure, Cargill and Meyer also use a small Keynesian model. But that treats the money supply as an exogenous policy variable, even

though the Fed largely accommodates changes in the demand for money, so that this model may suffer from serious OLS bias. But if we reject the results of these two papers we have to admit that we do not know the variability of the lag at all, and that it may well be substantial."

One might therefore expect that the Cargill and Meyer and Tanner papers played a significant role in the debate about discretionary monetary policy. But they did not. Although the SSCI lists several citations for both papers, these citations are all in contexts other than the feasibility of discretionary policy.

It is not clear just how much of a problem the variability of the lags creates. Haskel Benishay (1971), using a control theory model in which the central bank estimates income by an autoregressive function, obtained the counter-intuitive result that the more variable the lag the greater is the optimal stabilization policy. However, from another control theory model, also using an autoregressive forecast of income, J. Phillip Cooper and Stanley Fischer (1972a) obtained the more intuitive result that the more variable the lag, the less is the pay-off from discretionary policy. In another paper (1972b) they showed that in a certain type of model it is the variability of the lag, and not its length that reduces the feasible degree of stabilization.

All in all, it is far from obvious whether the long lags shown by econometric models, and the variability of the lags shown by Cargill and Meyer and Tanner prevent an effective counter-cyclical policy altogether, or just reduce its effectiveness. To answer that question one would have to know, not just the length and variability of lag, but also how well the central bank can both forecast GNP, and predict by how much its policy actions change GDP. These questions attracted little, if any, attention in the context of the issue that Friedman raised. One

¹⁴ At certain times considerably more of the effect of monetary policy may occur in sectors that have a relatively short lag, such as consumption expenditures and residential construction, than at other times.

must therefore conclude that the empirical literature just discussed, while important for other purposes, does not answer Friedman's challenge.¹⁵ Instead another approach was tried.

Applying Control Theory

In the second half of the 1960s and in the early 1970s as econometric models matured, a sub-literature developed that tested stabilization rules for monetary policy derived from control theory. The results now obtained differed sharply from Baumol's earlier ones. A number of studies showed that if a particular econometric model, e.g., the Wharton model, the St. Louis model, or the Fed's FMP model, is valid, then feedback rules derived from control theory are superior to a constant monetary growth-rate rule. (See for instance, Cornwall, 1965, Lovell and Prescott, 1968; Cooper and Fischer, 1974; Cooper, 1974; Benjamin Friedman, 1975) Moreover, Craine, Havenner and Berry (1978) applying the Fed's FMP model to a short period, 1973-III to 1975-II, found that the policy the Fed had actually followed in that period was "slightly better", than a stable monetary growth-rate policy, which, in turn, performed better than the Cooper-Fischer and Bronfenbrenner feedback rules.

There was now finally some evidence for the Keynesian claim that, despite lags and forecast errors counter-cyclical policy can succeed. But there were three major problems. First, the results were model specific, as well as time specific. Since the impact multipliers for monetary policy differ substantially among various models, it is by no means clear how much credence the just discussed results deserve. Brunner (1980, p. 53) argues that: "There is

The only attempt during the period discussed to assess Friedman's challenge directly is, as far as I know, a paper (Mayer, 1958) that deals primarily with the length of the lag, but does plug some numbers into the sum-of-variances equation as more or less an after thought. It suggests that a small degree of cyclical stabilization is feasible. Subsequent to the period discussed here I made a more elaborate attempt to implement Friedman's analysis empirically (Mayer 1990b, Chapter 5), but that attempt has met with a quite negative reaction.

substantial evidence that the optimal control settings are not robust with respect to variations over the spectrum of models." Indeed, all the models may lack sufficient accuracy (see Brunner, 1983). Not until 1988, when Bennett McCallum applied feedback rules to a wider variety of models, was the problem of model specificity ameliorated to some extent, although McCallum's favorable results for his feedback rule are open to question. (See Benjamin Friedman, 1988.)

The second problem is that sturdy standby of skeptics, the Lucas critique. It *seems* likely that *the* temporary demise *that* these feedback models experienced in the late 1970s was due to the Lucas critique. Third, except for the Craine, Havenner and Berry paper, the control-model literature only showed that there is a particular discretionary monetary policy that will be stabilizing. It did not show that the policy the central bank is likely to adopt will also be stabilizing.

THE POLITICAL ECONOMY OF MONETARY POLICY

The discussion reviewed so far has treated the central bank as a perfectly obedient agent of a principal who has only one argument in the utility function, economic stabilization. It also assumed that the central bank acts in a technically competent manner, using modern economic theory instead of discredited doctrines. In doing so it ignored an important part of the monetarists' case against counter-cyclical policy. When Milton Friedman described the issues about stabilization policy on which he disagreed with Franco Modigliani, one of them was:

the assumption that if in fact you adopt a policy of accommodation [that is offsetting fluctuations in aggregate demand], Franco Modigliani will be twisting the dials. I have increasingly ... become impressed with the need for a positive science of politics, of political science. All of us ... have tended to follow the attitude: Well, now, what we need to do is to figure out the right thing. If only we can tell them what the right thing to do is, then there is no reason why able, well-meaning, well-intentioned people should not carry out those ideas. But we then discover over and over again, that well-intentioned, able people have passed laws, or have established institutions -- and lo and behold, they don't work the way able, well-intentioned people expected or believed they would work. And it isn't an accident that that happens. ... once you adopt a policy of

accommodating to changes [in aggregate demand] there will be all sorts of changes that ... [Modigliani] and I know should not be accommodated, with respect to which there will be enormous pressure to accommodate. ... *I have increasingly moved to the position that the real argument for a steady rate of monetary growth is at least as much political as it is economic* (Modigliani and Friedman. 1977, pp. 17-18, emphasis added.)¹⁶

Similarly, Karl Brunner (1981b, p. 37) wrote that it is wrong to assume that: 'a monetary authority will naturally pursue the optimal social benefit achievable with cleverly designed stabilization policies. ... They will have incentives to trade off degrees of achievable stabilization for political and personal benefits of various kinds.'

The Record of Monetary Policy¹⁷

One way to see whether the Fed actually tries to achieve counter-cyclical stabilization, and pursues this goal in a technically competent manner, is to look at the historical record. Thus Friedman (Friedman and Heller, 1969, pp. 77) wrote: "The reason I believe you would do better with a fixed [monetary] rule ... is because I have examined U.S. experience with discretionary monetary policy

Friedman and Schwartz (1963a) provided massive evidence on the pro-cyclical nature of Fed policy during the 1930s. (See also Elmus Wicker, 1966).¹⁸ But one might respond that the Fed has learned from this experience. It now has the high employment goal mandated by the 1946 Employment Act, and is not likely to repeat the mistakes it made in the 1930s. Friedman and Schwartz end their story in 1960, and in their discussion of the 1950s they do point out that the Fed has moved away from its prior adherence to the real bills doctrine towards emphasizing the growth-rate of the money stock. With the money stock then growing at a very stable rate Friedman and Schwartz did not have much criticism of the Fed for its

For a similar statement a few years later see Friedman (1983)

Because of my unfortunate ignorance about the history of central banking in other countries, I confine the discussion to *the* U.S.

¹⁷ Peter Temin's defense of *the* Fed does not seem convincing; see Mayer (1978)

policy in the 1950s. Indeed, Friedman (1960, p. 22) wrote: "Except for the sharp price rise in 1950-51, our monetary experience since 1948 has been admirable by previous standards."¹⁹ To be sure, he did claim that even in the 1950s the actual record of monetary policy is inferior to that which a stable growth rate rule would have provided (Friedman, 1960, pp. 93-94). But, as he himself pointed out, he had analyzed the record only in casual way that did not make explicit allowance for the lag of monetary policy (Friedman, 1960, p. 97).

Subsequent to the period discussed by Friedman the Fed generated or at least accommodated an inflation that between 1965 and 1982 saw the GDP deflator almost triple. To be sure, one might argue that this was not the fed's fault, that this policy was largely imposed on it by the intellectual and political currents of the time. But for the purpose of evaluating counter cyclical policy it does not matter whether the Fed was the source or merely the conduit for mistaken policies. A somewhat more plausible defense of the Fed is that much of this inflation resulted from a policy of accommodating supply shocks. Had these shocks not been accommodated they would have generated massive unemployment, so that the Fed was right in permitting high inflation.

Although Brunner and Meltzer devoted less attention to monetary history than Friedman they, too, have criticized the Fed's record, pointing out that in the postwar period the growth rate of money has been pro-cyclical (Brunner and Meltzer, 1983a, p. 97). But a pro-cyclical monetary growth rate is not necessarily inappropriate. One reason is that, given the lag of monetary policy it is *possible* that the monetary growth rate should rise faster during the expansion than during the recession. Another is that since early in a recession GDP is still above its trend-adjusted mean, while late in the expansion it is above its trend-adjusted mean,

However, twelve years later, in discussing a longer period, 1952 to October 1979, Friedman (1982) argued that the Fed began to pay increasing lip service to a monetary aggregates target, but that this did not change its procedures.

effective stabilization does not mean raising GDP throughout the recession and lowering it throughout the expansion. (See Argy 1979.)

Looking at the record of U.S. monetary policy therefore provides perhaps some evidence, but certainly not compelling evidence that counter-cyclical policy cannot succeed: learning takes place. Fed policy during the last few years is a great improvement over earlier policy. To make a strong case against counter-cyclical policy requires showing that there are systemic factors that inhibit effective policy-making by central banks.

The Monetarist View of Central Bank Behavior

What factors could prevent central banks from being as effective counter-cyclical stabilizers as their ability to forecast allows them to be? One possibility is that they are not free to determine their own policies. A second is that they tend to adopt policies that serve their own interests. Third, their policies may be sub-optimal due to some of the cognitive failures that organization theorists have discussed in their analyses of other organizations, for example, the problem of group-think. Finally, even otherwise efficient policies can have *bar.* effects due to a coordination problem between the central bank and the public.²⁰ Monetarists have raised all of these issues.

Friedman (1960, 1968) has argued that central banks are not free agents and that in case of serious conflicts with government policy, the government can get its way. Brunner (1981a, p. 69), too, sees the central bank as placing the interests of its masters ahead of its stabilization task: "stabilization policies rank comparatively low among the interest of the clientele [of central banks] compared to the potential gains to be expected from allocative arrangements frequently pursued under the guise of monetary policy." He (Brunner, 1983)

²⁰ These four categories do not, of course, present distinct divisions in the sense that a particular characteristic of central-bank behavior can fall into only one of them.

suggests that such a wish to accommodate their clients explains why central banks prefer to target interest rates.

Here are some concerns raised by other monetarists. David Meiselman (1986) finds evidence of a political business cycle after 1960. David Fand (1986) cites specific instances in which the Fed has succumbed to political influence. More generally, Robert Weintraub (1978) argues that presidents get the monetary policy they want, and shows that major changes in monetary policy were associated with the election of a president who had different views on monetary policy. He also shows that whenever presidents with sharply different views on monetary policy were elected monetary policy changed. Although these conclusions are not fully supported by events subsequent to Weintraub's paper his examples still make an impressive case.²¹

Robert Hetzel (1986) argues that a significant function of monetary policy is to redistribute income by controlling interest rates in accordance with the wishes of politically powerful groups who could threaten the Fed's independence. He does not claim that this necessarily happens frequently. He claims only that at times this consideration does influence monetary policy. To maintain its freedom to respond to such pressures when this is necessary, the Fed is unwilling to set monetary targets it will adhere to. Hetzel argues that there is therefore a flaw in the basic Keynesian case for stabilization policy. This basic case is that due to long-term contracts the private sector is slow to adjust to changes in aggregate demand, while the public sector can respond quickly. But, says Hetzel (1986), the importance that the

²¹ In 1979 a sharp change in monetary policy occurred without a change in presidents. And the same was true in 1982. A political scientist, not identified with monetarism, John Woolley (1984, p. 111) reaches the more moderate verdict that: "Rather than conclude that presidents generally get the monetary policy they want, it would be more accurate to say that only infrequently are presidents extremely unhappy with the monetary policy they get."

political system attaches to the distributional consequences of monetary policy eliminates much of the seeming flexibility of monetary policy.

William Poole (1986) looks beyond the influence of politicians and argues that the public's pressure distorts monetary policy. The public looks at only the currently pressing problem, and wants the fed to focus on that. In periods of high unemployment it demands expansionary policies, and in periods of high inflation contractionary policies. Because of the lag in monetary policy such pressures push the Fed towards destabilizing policies. (See also Lombra, 1988.)

Many monetarists reject a popular solution to the problem of political pressures giving central banks constitutionally guaranteed independence. They argue central banks would then have even more freedom than now to follow their self-interest, and that this is incompatible with effective stabilization policy. Friedman places much emphasis on central banks not having a bottom line, so that the public cannot readily evaluate their performance.

To show that the Fed follows its own interests Friedman (1982, 1986) gives several examples of Fed behavior that he believes can best be explained that way. One is its reluctance to adopt serious monetary targets in place of money-market-conditions targets. Friedman attributes this to monetary targets providing greater accountability. Another is the reluctance to adopt a fixed monetary growth-rate rule, something that would greatly reduce the importance and status of Fed policymakers. The third is the Fed's inflationary bias, which Friedman (1982), following Mark Toma, attributes to the Fed's gain from seigniorage. The fourth is the Fed's concern about keeping member banks from leaving the Federal Reserve System, because having many member banks enhances its prestige, power, importance and lobbying clout.²² Still another example is the Fed's open market churning, that is, its undertaking a great volume of open market operations that are soon reversed. Friedman

²² State chartered banks have a choice whether to be member banks.

attributes this to the Fed's wish to be seen as important. An additional example is the delay in releasing the Federal Open Market Committee's Directive on open market operations. He argued that by thus keeping financial markets in the dark the Fed enjoys a sense of importance, and also creates well-paid, private-sector job opportunities as Fed watchers for its staff.²³

Moreover, both Brunner (1980) and Friedman (1986) raise a basic question. Why assume that, although agents in the private sector are driven by self-interest, in the public sector agents do not try to maximize their own welfare, but conscientiously do their principal's bidding?

As Brunner (1984) points out, the heterogeneity of the central bank's principals and the ambiguity of their interests provide a central bank with the opportunity to attend to its own interests. Examples of self-interested policies that Brunner (1983) cites are the Fed's preference for complex instruments, its concern about retaining member banks, and its attempt to curb dissent within its own ranks. William Poole (1986) argues that Fed officials are convinced that the Fed's independence is essential. To protect this independence they may therefore sometimes adopt policies that they know are not appropriate for stabilizing GDP.

Despite the usual reluctance of mainstream economists to consider irrational behavior, monetarists devote substantial attention to the Fed's cognitive errors. In their *Monetary History* Friedman and Schwartz (1963) discuss in much detail the damage done by the Fed's adherence to the discredited real bills doctrine. Perhaps this stubborn adherence results from a defect that Friedman (1986, p. 188) attributes to central banks: that "an independent central bank will almost inevitably give undue emphasis to the point of view of bankers," which, he believes, induces then to confuse money and credit. Moreover, central banks are myopic. "If

²³ In 1994 the Fed greatly reduced this uncertainty by releasing at each FOMC meeting its target for the federal funds rate.

each case is considered on its merits, the wrong decision is likely to be made in a large fraction of the cases because decision-makers are examining only a limited area and are not taking into account the cumulative consequences of the policy as a whole." (Friedman, 1968, p. 192) Beyond this, the absence of a bottom line makes it hard for a central bank to learn from experience, and thus fosters bureaucratic inertia. Examples of such inertia are adherence to the real bills doctrine, interest-rate pegging after World War II, and the system of lagged reserve requirements.²⁴

Brunner and Meltzer, like Friedman, pay much attention to the Fed's cognitive errors. In 1964 they published a pathbreaking evaluation of the Fed's thinking and procedures (Brunner and Meltzer, 1964a and b). They found utter confusion. The Fed had no clear idea about the process leading from its open market operations to GDP (that is why Brunner and Meltzer developed the targets and indicator approach), suffered from "money market myopia", and often was confused about whether its policy was expansionary or restrictive. For example, it described its policy in 1960 as expansionary, even though it reduced money and bank credit. Brunner and Meltzer built a strong case for believing that the Fed's strategy was so flawed by technical errors, that counter-cyclical policy was at least as likely to destabilize as stabilize GDP. I have the impression that many monetary economists, who previously had assumed that the Fed was professionally competent, were shocked by what Brunner and Meltzer had found. It is quite possible that even the Fed itself was shocked, because shortly thereafter it strengthened its research staff, and its economic analysis became more professional. (Brunner and Meltzer, 1983, p. 60) In addition, more professional economists, some with strong academic reputations, were appointed to the Board of Governors and to Reserve Bank presidencies.

²⁴ Shortly after Friedman wrote this the reserve requirement system was changed.

But according to Brunner (1983) these are largely superficial changes. Although the Fed may use the jargon and techniques of economic analysis it does not take economic analysis seriously. To do so would be risky for it since economic analysis "injects a subtle constraint on the future range of possible actions," and thus poses a danger to the welfare of the Fed's bureaucracy. (Brunner, 1983, p. 104). The Fed therefore prefers an eclectic position that provides it with a greater range for tailoring its explanation to any conclusion it wants to reach. Brunner argues that it is not alone in this, citing the Bank of England, the Bundesbank, the Belgian National Bank and the Banque de France as other examples. Hence, although the Fed's staff uses econometric models, the results shown by these models are frequently disregarded, (Brunner, 1980, 1984, see also Lombra and Moran, 1980). In addition, the Fed ignores criticism of its policy by academic economists. (Brunner, 1983)

Another monetarist, and former president of the St. Louis Federal Reserve Bank, Lawrence Roos, discusses the cognitive-errors problem with the insight of someone who has served on the FOMC.

Never once in my participation in meetings of the Federal Open Market Committee (FOMC) do I recall any discussion of long range goals of economic growth or desired price levels. It was like trying to construct a house without agreeing upon an architectural design. [T]he Federal Reserve is supposed to solve all sorts of problems, including inflation, unemployment, lagging real output growth, high interest rates, balance of payments disequilibrium, volatile exchange rates, depressed stock prices, a sagging housing industry, and the world debt crisis. ... Imposing such a laundry list ... on the Fed reflects a total lack of understanding what the Federal Reserve is able or not to do. This kind of thinking hampers the workings of the FOMC. ... I recall no consensus on long-range goals nor do I recall serious efforts to set policy on any other than the shortest time horizons. ... I have always had the feeling that the discussion was "Where do you think the economy will be a year or two in the future, and how can we best set targets so that we won't have egg on our face if this doesn't come about?" (Roos, 1986, pp. 772-5)

In their study of the FOMC in the early 1970s Raymond Lombra and Michael Moran (1980, p. 43) conclude that: "without the guidance or discipline offered by an analytic model and formal targets for nonfinancial variables, the formulation of monetary policy often

seemed to be a seat-of-the-pants operation." In commenting on this study James Pierce (1980, p. 84), a former senior FOMC economist (and not a monetarist), reports that:

if the formation of monetary policy is to be understood and reformed, much more work needs to be done in developing an understanding of the group dynamics that produce the kind of FOMC performance described by Lombra and Moran. The members of the FOMC are basically intelligent people, and many have training in economics. They all have access to staff and outside advice. Yet when they get together we get policy by "the seat-of-the-pants.

Elsewhere, Lombra (1988) points out the difficulty that the FOMC faces in taking timely action, such as raising interest rates when unemployment is still high. Given the fragility of forecasts, it is understandable that policymakers are afraid to become restrictive at such times. The results of my own studies of the Fed's thinking during the 1973-75 recession and with respect to Regulation Q (reprinted in Mayer, 1990b, Chapters. 6 and 7) support such critical evaluations

Finally, there are the coordination problems created by counter-cyclical policy. Friedman (1960) objects to the Fed frequently unpredictable changes in its policy, presumably because they make it hard for the public to coordinate its actions, such as price setting, with the Fed's policy. Brunner (1983), too, objects to the Fed's exacerbation of uncertainty. Moreover, Cukierman and Meltzer (1985) argue that central banks have an incentive to create uncertainty. The greater the uncertainty, the longer is the effectiveness of some policies, such as raising output by adopting a more expansionary policy. And keeping one's actions shrouded in mystery is also a good defense against criticism. Brunner (1983) also refers in this connection to time-inconsistency.

Evaluation

These monetarist arguments obviously lack rigorous development, and some of them seem more like interesting issues for further research than carefully worked out conclusions.¹¹ It may

¹¹ Thus Levin and Meulendyke (1982) show that Friedman's criticism of the Fed's open market churning is flawed.

seem that this is inevitable since neither the theory nor the empirical evidence required for a more convincing treatment is available. But this is not quite correct. Much work has been done outside the monetarist debate, and even outside of economics that monetarists could have cited

There is an extensive literature on the existence of political business cycles. (For surveys see Beck, 1990, 1991; Woolley, 1994.) Reasonable people might take either side on this issue.²⁶ But the possibility that political business cycles exist is surely not something that should be ignored when discussing the limitations that political pressures place on counter-cyclical monetary policy. Yet few monetarists have taken it up,²⁷ such as public-choice economists, such as Thomas Havrilesky (1993) have generated an extensive literature on the Fed acting in its self-interest.²⁸ Monetarists have not made much use of it either, although Friedman (1986) does praise it.

First, as far as I know, there is no difficulty with the political business-cycle hypothesis in that, at least in the U.S. with its four-year election cycle, there are so few observations. Second, it is not clear whether the statement "there are political business cycles" is intended to mean that there *always* occur at all presidential elections, at many of these elections, or that there has been at least one political business cycle in the postwar period. Carter's bid for a second term provides evidence against the first proposition, while Nixon's successful pressure on the Fed in 1972 (see Woolley, 1995) provides evidence against the third proposition. In countries with a parliamentary system there is an additional problem, elections are sometimes endogenous with respect to economic conditions.

For an exception see Melissman (1986). This is not likely to be due to the rational-expectations criticism of conventional political-business-cycle theory since Friedman is hardly a strong supporter of rational expectations. Moreover, the rational-expectations case against political business cycles is weak (see Mayer, 1991). It may seem that the possibility of political business cycles is irrelevant for a discussion of counter-cyclical policy, since in principle, political business cycles can occur even if the central bank does not pursue a counter-cyclical policy, as long as it does change the monetary growth rate from time to time to take account of secular changes in velocity.²⁹ But counter-cyclical policy, by generating much more central bank activity, makes it much easier to hide political business cycles and thus increases their likelihood.

²⁹ Much, but not all of it is reprinted in Toma and Toma (1987).

There is much less literature, other than the writings of monetarists themselves, on cognitive errors in monetary policy making. But there exists a large literature on the role of cognitive errors in other types of economic behavior, and that may perhaps be relevant for monetary policy.²⁹ Finally, monetarists have paid some, but only limited attention to the one coordination problem of monetary policy that has received great attention, time-inconsistency

It is *possible* that many or even most monetarists believe that political business cycles do not occur, are skeptical of much of the public choice literature on monetary policy and of the evidence on cognitive errors, and do not think that time-inconsistency is a serious problem. They may be right. So perhaps they should not be blamed for not making greater use of the literature on these topics. But where does this leave us? Consider, for example, the hypothesis that Fed policy-makers are motivated by self-interest. At times that will induce them to act in ways contrary to the public interest. But—as Albert Hirschman once remarked, policymakers have an incentive to engage in obituary-enhancing behavior. By and large, that means engaging in socially desirable behavior. Unnecessary secrecy may enhance the prestige of Fed governors, but so does the avoidance of recessions and inflation. The reach of the invisible hand is not confined to the private sector.

Moreover, monetarists may well be correct in saying that the Fed is subject to political pressures, attends to its own interests and makes cognitive errors. There is no doubt that such factors do degrade the quality of discretionary monetary policy. But how much? Do they just reduce the feasible degree of stabilization to a small extent, or do they cause discretionary policy to be destabilizing? Monetarists may claim the latter, but their evidence seems just as

Useful sources are Thaler (1991 and 1993). For an interesting application of such analysis to decision-making in another area of public policy, foreign policy, see Vertzberger (1990). For an attempt to apply cognitive dissonance theory to monetary policy see (Mayer 1990a, Chapter 16).

consistent with the former.³⁰ Here, as in the case of lags and forecast errors, they have raised a serious *challenge* to a fundamental assumption of the Keynesian position, but that is all

The Keynesian Response on Central Bank Behavior

The Keynesian response (if that word is appropriate) to this challenge has been either to ignore it, or to dismiss it with disdain.³¹ This is illustrated by a Keynesian argument that was popular prior to the discovery of time-inconsistency. As Stanley Fischer (1990, p. 1157), following Buiter (1981), tells us:

At the formal level, Friedman's analysis suffers from the logical weakness that discretion seems to dominate rules: if a particular rule would stabilize the economy, then discretionary policy-makers could always behave that way -- and retain the flexibility to change the rule as needed.

This argument completely ignores a substantial part of the monetarist case, even though Fischer does add in a footnote: "However, Friedman did confront the issue of why a formal rule might be preferable to a discretionary policy, making an analogy to the Bill of Rights."³² Friedman in a debate with Modigliani stated that his case for a rule is based "at least as much" on political considerations as on lags and forecast errors." (Modigliani and Friedman, 1987, p. 18) and has written vastly more on these political considerations than merely referring to the Bill of Rights. Yet the just cited footnote is the only mention of Friedman's political argument in Fischer's survey of "Rules versus Discretion in Monetary Policy". To be sure, Fischer devotes much space to the time-inconsistency argument for a

I say they "may" rather than they "do" because all an opponent of counter-cyclical policy has to show is that the effect of these problems when combined with the effects of lags and forecast errors make counter-cyclical policy counter-productive.

There may, of course, be some Keynesians who have taken the monetarists' argument seriously; I do not claim to have read the entire literature. But it is so, their position is unusual and out of the Keynesian mainstream.

As Bronfenbrenner (1966, p. 174) put it: "The optimal discretionary authority (call him Superman) will always outperform any rule limiting his discretion. ... Making a case against Superman is like making a case against Santa Claus. The first question is, 'Does he exist?' "

monetary rule, but that hardly explains why he ignores Friedman's analysis. This analysis is by the nature of its subject matter not rigorous, but it has much more empirical support than does the time-inconsistency theory of monetary policy. Indeed, why that theory has become so popular, despite its lack of empirical substance (see Mayer, 1993, pp. 87-92) is a topic that deserves a separate paper.³³ The self-interest of economists who need a role for their game-theory tools is a prime suspect.

Perhaps a reason why many economists may brush aside the monetarists' interpretation of Fed behavior is that those of us who are acquainted with Fed policymakers know them to be able and dedicated public servants, not people who would consciously put their own interests above those of the public. I suspect that such personal knowledge can be highly persuasive. But it can be misleading. Friedman concludes his criticism of specific Fed policies as follows:

[L]et me emphasize that I am not saying that people in the [Federal Reserve] system deliberately pursue these measures for these reasons. Not at all. As economists, we treat a business enterprise as if it were solving a large number of complex simultaneous equations. ... We justify that procedure by saying that if we analyze them as if they are rationally and knowingly pursuing the maximization of profit, we'll get a good approximation of their behavior. ... I am trying to analyze the forces at work, not to describe the detailed motivation or personal behavior of the people involved. All of us know that what's good for us is good for the country. (Friedman, 1982, p. 116, emphasis in original.)

One need not view Fed policymakers as incompetent or selfish to allow for the possibility that their policies are far less efficient than our knowledge of economics makes possible. For example, even selfless people often have an exaggerated view of the importance of their organization. Hence, one would expect even the most dedicated Fed officials to

³³ As Fischer (1990, p. 1173n) points out: "Even aside from Switzerland and Germany it is clear from history that inflationary bias is only a sometime thing." Michael Mussa (1994, p. 139n) in discussing the U.S. inflation of the late 1960s and 1970s points out that "there is little factual support" for the hypothesis that the Fed generated this inflation to deceive the public. See also (Mayer, 1993, pp. 63-66.)

overvalue the benefits of the Fed's autonomy, and hence to adopt policies that enhance its political power, and to avoid policies that subject it to politically potent attacks. (Cf. Kane, 1990, Pierce, 1990, Willett and Keen, 1990.) Moreover, central bank officials, like all of us, are tempted to avoid the feeling of regret that comes from realizing that we have made a mistake, and hence may be slow to abandon mistaken policies. They are also likely to be excessively influenced by the views of their colleagues.³⁴ (See Klein, 1994; Mayer, 1990a) Consequently, it is not sufficient to say, as Modigliani (Modigliani and Friedman, 1977, p. 21) does, that: "I have personally no reason to believe that the United States government ... is not able to attract able people who are interested in common welfare and can do a good job."

Ideology and Value Judgments

Keynesians sometimes dismiss the monetarist case for stable monetary growth as politically and ideologically grounded. Thus Tobin (1976, p. 336) contends that:

Distinctively monetarist policy recommendations stem less from theoretical or even empirical findings than from distinctive value judgments. The preferences revealed persistently in those recommendations are for minimizing the public sector and for paying a high cost in unemployment to stabilize prices.

Similarly, Modigliani tells us that the dispute between monetarists and Keynesians on the need for stabilization policy is attributable not only to differences in empirical estimates, but "no less a degree to differences in *social philosophy and attitudes*." (Modigliani, 1986, p. 7, emphasis in original.) Presumably Tobin and Modigliani intend to imply that the monetarists' evidence on stabilization policy should therefore not be taken all that seriously.

Furthermore, policymakers may well have an exaggerated idea of their own abilities, and hence adopt policies that are too risky. They have become policymakers because they have been successful in the past. Part of their success was due to luck, part due to ability. It is only natural for people to underestimate the role of luck and overestimate the role of ability in their success.

Modigliani appears to define "value judgments" very broadly to include what conventionally would be called "prejudice" or "presuppositions". (See Modigliani and Friedman 1977, p. 10)

It is hard to know what to make of this.. Friedman (1960, p. 85) in his discussion of a monetary rule does refer to his general preference for the rule of law over the rule of men, but does so only very briefly. Almost all of Friedman's discussion of the political aspects of a monetary rule deals with positive and not normative considerations.

A cynical interpretation of Tobin's and Modigliani's reference to value-judgments is that this is a rhetorical argument (in the derogatory sense of the term) running something like this: "We Keynesians are concerned about the welfare of ordinary people, and empathize with the poor. So we advocate policies that will reduce unemployment. Those who disagree with us do so because they are less concerned with the misery of the unemployed. We appeal to the reader, who no doubt shares our value judgment, to support our position." But there is no warrant for such a cynical interpretation.

A more plausible reading is that, as discussed below, Modigliani and Tobin simply do not understand how monetarists can disagree with their positive judgments. So, they think that the difference in policy recommendations must be due to a difference in value judgments. And there is a big ideological distance between Friedman, Brunner and Meltzer on the one hand, and Modigliani and Tobin on the other.

OBJECTIONS TO A MONETARY GROWTH-RATE RULE

Instead of trying to meet directly the monetarists' challenge to their assumption about central bank behavior, Keynesians have followed the principle that the best defense is offense, and have devoted considerable effort to showing that a monetary growth-rate rule would not work well.³⁶

³⁶ Goldfeld (1982) provides a useful summary of some of the arguments.

One problem with a growth-rate rule is the difficulty of finding a measurable counterpart to the theoretical term "money". Is it M-1, M-2, M-3 or what? As Tobin (1965) has remarked Friedman admits that he does not know what money is, but he wants whatever it is to grow at a fixed rate. Monetarists respond by saying that it does not matter all that much whether one picks M-1, M-2 or M-3, since their growth rates are highly correlated. But, even in the period 1959-1982 the correlation is far from close. Subsequently, the correlation between the growth rates of M-1 and M-2, as well as the correlation of the growth rates of M-1 with the growth rate of M-3 disappears. Only the growth rates of M-2 and M-3 are highly correlated.³⁷ Moreover, the current measures of M-1, M-2 and M-3 are not the only possible measures of money, and some other measure may be the appropriate one.

Keynesians therefore have here a good argument against a stable monetary growth-rate rule. But it is not a valid argument for counter-cyclical policy. The central bank could acknowledge that it does not have an operational definition of money, and hence shift occasionally (but not frequently) in an ad hoc fashion between various measures of money. Or, though this would not be acceptable to monetarists, it could use an interest rate or the growth rate of credit as its target variable, without attempting counter-cyclical policy.

The other problem with a monetary growth-rate rule is the familiar one that whichever measure of money is chosen, its velocity is likely to be unstable, if not now, then sooner or later. When in the early 1980s the velocity of M-1 became erratic monetarists pointed to the

³⁷ For the period 1959-60 to 1981-82 the adjusted R^2 's between the annual growth rates of M-1 and M-2, M-1 and M-3 and M-2 and M-3 are 0.30, 0.46 and 0.61 respectively. For the 1982-83 to 1993-94 period they are 0.02, 0.00 and 0.85. These R^2 come from regressions unadjusted for serial correlation, despite some of the Durbin-Watson statistics being very low, because they are not intended to search for causality, or even for a good forecast, but merely to show how different a policy of stable M-1 growth would be from a policy of, say stable M-2 growth.

stable velocity of M-2. But in the early 1990s that velocity, too, became unstable. Adding shares in mutual bond funds to M-2 helped for a time -- but not for long.

But here, too, one needs to distinguish between a strict monetary growth-rate rule, and a moderate rule of abstaining from counter-cyclical policy, while adjusting the monetary growth rate for longer-run changes in velocity. The two key questions are again whether the central bank can predict cyclical changes in velocity and the effect of its policy actions with sufficient accuracy, and whether it can be trusted with the power to adjust the monetary growth rate counter-cyclically. These questions are not answered by pointing to substantial fluctuations in velocity. Secular changes in velocity and in the appropriate measure of money are only weak arguments for counter-cyclical policy.³²

EMPIRICAL TESTS

Since the historical record does not provide the opportunity for a good direct test of a monetary growth-rate rule, relatively few empirical tests have been undertaken. Bronfenbrenner (1961a 1961b) calculated the growth rate of money that is optimal for price stability on the assumption that velocity is unaffected by the policy followed.³³ He then compared this optimal growth rate of money to (a) the actual growth rate that occurred under Fed discretion, (b) two versions of a stable monetary growth-rate rule, and (c) an adaptive rule that made the monetary growth rate a function of the prior year's or quarter's increase in the labor force, labor productivity and change in velocity. Bronfenbrenner's results with quarterly data (though not those with yearly data) favored the adaptive rule.

Modigliani (1964) criticized Bronfenbrenner's assumptions that velocity is unaffected by the growth rate of money, and that money affects income without a significant lag. However,

³² Weak, but not entirely groundless. Since the central bank cannot tell right away whether a change in velocity is cyclical or secular, if it refrains from responding to what it thinks are only cyclical changes it will tend to be late in responding to secular changes.

As Argy (1971) points out, this is a strong assumption.

Modigliani assumed that the effect of money on income occurred within half a year, an assumption that is both critical to his results and open to question. (See Attiyeh, 1966; Mayer, 1967). Moreover, Attiyeh (1966) pointed out that Modigliani ignored the lagged effects of monetary policy, as well as Okun's law. On the other hand, in a subsequent paper designed to deal with these problems, McPheters and Redman (1975) found that discretionary policy was on the whole superior to a fixed monetary growth-rate rule, or to a rule that put limits on the monetary growth rate.

It may therefore seem that Modigliani's test, as further developed by McPheters and Redman, provides the rationale that Keynesians relied on in rejecting the monetarist challenge. But that would be pseudo-history. Although Modigliani's paper has received a respectable number of citations, these citations are not ones that justify stabilization policy by referring to this paper, but deal with other issues.

Victor Argy (1979) in a somewhat similar study of seven countries took residuals from a stable growth trend of money and, using an econometric estimate of the lag, calculated their impact on industrial production. On the whole, his results showed that departures of money growth from its trend destabilized industrial production. In another test for eight countries he looked at the effect of money growth on velocity, and again found discretionary policy to be destabilizing. However, both tests require the strong assumption that money is exogenous, as well as the acceptance of Argy's estimate of the lag. Argy attributed the poor performance of discretionary policy to political pressures on the central banks, and to their focus on goals other than stabilization.⁴⁰

Another test is to see whether GDP grew at a stabler rate in those periods in which the monetary growth rate was fairly stable. Modigliani (1986) found that in two postwar periods

⁴⁰ In an earlier study using seventeen countries Argy (1971) had obtained inconclusive results, but as Argy points out, his procedure biases the results against the monetary growth-rate rule.

when the U.S. money supply was growing at a relatively stable rate, GDP was highly unstable, though in a third period of relatively stable monetary growth, GDP was also growing at a stable rate. From this Modigliani (1986, p. 37) concluded that stable money growth is neither a necessary nor a sufficient condition for economic stability. There are two problems with this conclusion. First, there is the question whether one should look at periods when the growth rate of money was stable, or as Friedman (Friedman and Modigliani, 1977) maintains, at periods when the growth rate of money was changing at a stable rate." Second, given the Fed's tendency to accommodate changes in the demand for money one would expect money growth to be more erratic at times when GDP growth is more erratic.

In a somewhat similar test Starleaf and Floyd (1972) compared the relative stability of monetary growth rates and GDP in thirteen countries. They showed that the countries with a stabler money growth rate also had stabler GDP growth. However, it is not clear whether unstable money growth caused unstable GDP growth, or converse.

Using an (apparently independently developed) framework similar to Friedman's (1953) Robert Fix and Charles Silvesind (1978) looked at the correlation between changes in velocity and changes in the monetary growth rate in a sample of countries. They compared the actual changes in money growth to the changes that would have been appropriate, given the correlation between changes in velocity and money. They found substantial variation among countries; in particular the U.S. experience was not representative. Their results generally support discretionary policy. However, they had to make the strong assumption that the observed changes in velocity are independent of changes in the monetary growth rate.

Friedman's argument is that recessions occur when money is growing at a slower rate than the public expects, and that the public forms its expectations regressively. Tobin (1983, p. 508) also argues that from 1950 until 1973, a "remarkably stable" period the Fed was "vague, responsive and active." But it was so also in the subsequent less successful period.

Another test of counter-cyclical monetary and fiscal policy jointly is to compare the stability of the U.S. economy in the postwar period, when stabilization policy was used, with its stability before 1929. (See, for instance Modigliani, 1977, and Heller in Friedman and Heller, 1969.) Such a test is not persuasive because the postwar economy differs from the pre-1929 economy also in many other ways. Thus, Keynesians should expect greater stability because of the greater relative size of the governmental sector and the prevalence of automatic stabilizers, while monetarists should expect greater stability because of the avoidance of the massive bank failures that used to accompany many major recessions.⁴²

All in all, these empirical tests do not provide monetarists with the evidence they need to make strong claims, nor do they provide Keynesians with a justification for rejecting the monetarist position.

RECENT DEVELOPMENTS

In recent years the debate has been swept up in the controversy about new classical theory. New classical economists have reached the same policy conclusion as hard-core monetarists, but have done so for a very different reason, the rapid and efficient response of the private sector to changes in aggregate demand, and not the slow and inefficient response of the public sector. It has also been substantially changed by two other developments. One is the importance that many economists now attribute to the danger of time-inconsistency. The other is the erratic behavior of both velocity and the demand for money. These two developments have led to a focus on a compromise position; the use of feedback rules. Feedback rules represent a compromise accepted by some monetarists, such as Meltzer (1987), but not by others, such as Friedman. Such rules represent an obvious adaptation of monetarism to the brute fact that velocity is no longer stable. It is compatible with their distrust of central banks.

⁴² In addition, as the debate originated by Christina Romer (1986) has shown, the difference in stability is not as great as was believed in the 1970s.

and with their belief that we lack the knowledge required for effective counter-cyclical policy. At the same time, feedback rules also have some appeal for others because by constraining central banks they avoid the time-inconsistency problem.

That does not mean, of course, that the debate is over. Presumably many, perhaps most Keynesians prefer an entirely discretionary monetary policy. Thus two leading Keynesians, Benjamin Friedman (1988) and Modigliani (1988) have questioned the case for feedback rules. All the same, there now exists a coherent compromise position.

WHAT WENT WRONG?

The three failures that occurred in the debate can now be summarized. First, a false dichotomy was posed. Instead of asking the broad question whether a fixed growth-rate rule is preferable to all types of discretionary monetary policy, two distinct questions should have been posed, one about the validity of the hard monetarist position, and the second about the validity of the moderate position.

The second failure was that many monetarists overstated their case, in part, by treating evidence for their moderate position as though it were evidence for their hard position. They also treated a possibility argument -- or perhaps one should say a plausibility argument -- about lags and forecast errors as though it were much more conclusive than it actually is. Similarly, they did not develop their political argument against discretionary monetary policy sufficiently. They did present a serious challenge to the naive view of the central-bank as a good and wise deus ex machina, but then jumped to the conclusion that it behaves more like either a dunce or Satan. Their evidence that various factors inhibit rational and socially efficient central bank behavior fails to show that these factors prevent discretionary monetary policy from doing good on average. And their historical evidence of central bank failings, if

interpreted as more than a suggestive argument, is open to the objection that learning takes place.

The third failing of the debate is that the Keynesians did not treat sufficiently seriously the evidence presented by their opponents. They ignored Friedman's fundamental contribution of treating the stabilization problem as a problem in minimizing the sum of two variances. They also brushed aside the monetarists' skepticism about the motivation of central banks, despite the evidence that the monetarists and public-choice economists had provided for it. To be sure, by writing as though a fixed monetary growth-rate rule were the only alternative to a counter-cyclical policy, monetarists made it tempting for Keynesians to criticize such a rule instead of addressing the serious questions about the status of their basic assumptions. But Keynesians should have resisted the temptation.

I believe it was Lord Russell who said that a half-truth is a good enough stick to beat a dogma with. If so, the over-statements of the monetarists perhaps deserve some understanding. But they surely did not contribute to the advance of knowledge. All in all, both sides claim to possess knowledge they do not have.

RELATION TO PHILOSOPHY OF SCIENCE

In evaluating this debate I have not made use of various philosophies of science because they deal with more sophisticated issues than the ones that have hobbled this debate. But it is useful to see if the way it has proceeded has any implications for the relation of philosophy of science to economics.

My evaluation supports falsificationism, albeit in a weak and indirect way.. A major criticism of falsificationism is that it is prescriptive and not a good description of what scientists actually do.. But that is a problem only when it is applied to sciences that are successful enough for the philosopher-critic to have relatively little to contribute. The debate about the

monetary growth-rate rule has not been so successful. Regardless of the epistemological status of falsificationism, a whiff of falsificationism would have improved this debate. While neither of the rival hypotheses could have been compellingly falsified, more empirical testing would have been useful. Moreover, a Popperian mind-set, particularly if it were what Boland (1994) calls the Socratic version of Popperianism, would have made economists on both sides more aware of the precariousness of their positions. In particular, it might have induced Keynesians to ask whether their assumptions about the central banks' information set and motives are valid. It might also have induced monetarists to realize that much of their case consists of possibility arguments. To be sure, such a development would not have required falsificationism, but emphasis on falsificationism would have made it more likely. In academia it often takes a steam-shovel to do what could, in principle, be done just as well with a spade.

A Kuhnian interpretation of the debate would have little to contribute. To be sure, on the issue of central bank behavior there seems to be something that resembles a Kuhnian incomprehension of the other sides' position, but the resemblance is far from close. Both sides agree on what the question is, and would look at essentially the same evidence if it were available. The difference is that monetarists judge central bank misbehavior to be a major problem, while Keynesians judge it to be a minor problem, and both sides are puzzled by the other side's judgment. On the issue of lags and forecast errors there is no sign of paradigmatic differences. Suppose for example, that a new econometric model would predict GDP extremely accurately. Then, despite the previously-discussed difference between predicting GDP and predicting the effects of policy actions, it is likely that many monetarists would concede that the Fed now has the skills needed for effective counter-cyclical policy. Conversely, if forecasts were to deteriorate substantially in the future, at least some Keynesians would shift in a monetarist direction, as they would if the Fed were to blunder

again as it did in the 1930s. Both sides look at the same evidence, albeit through different glasses.

Lakatosian methodology fits at most only slightly better. If one formulates the debate in Lakatosian terms one would say that for Keynesians the hard-core propositions are that central bankers are capable and dedicated to their stabilization task, and that they can forecast with sufficient accuracy.⁴³ Disputes about such issues as the length of the lag, and the reason the Fed targets money market conditions, or its ability to control money growth are disputes in the protective belt. But such a Lakatosian formulation is not insightful. The words are right, but the music is out of tune. The debate about protective-belt propositions seems stunted, or else related to many other issues than a stable monetary growth-rate rule. Supposedly hard-core propositions are often attacked or defended directly, so that the distinction between the hard core and the protective belt is not salient. The problem is that if the Lakatosian concept of research programs fits anything, it is more likely to be the entire monetarist and Keynesian programs, or better still, neoclassical economics as a whole, rather than just positions on a stable monetary growth-rate rule.

Superficially the debate seems to fit the Hegelian schema, with the Keynesian thesis and the monetarist antithesis giving rise to the synthesis of feedback-rules. But it is far from obvious that feedback rules are widely enough accepted, and that they will survive the ongoing dispute long enough to be considered a synthesis. Moreover, the driving force behind them was not so much an intellectual development as an outside factor, the instability of velocity in recent years.

It may seem strange to call a hypothesis about the behavior of central bankers a hard-core proposition. But the importance of Keynes' own faith in the goodwill and ability of an intellectual governing class formed an important part of his political views. (See Buchanan and Wagner, 19-17, pp. 78-81.) In the U.S. some eminent Keynesians seem to take a somewhat similar position.

Quite apart from the philosophies of science just discussed, as a description of prevailing practice, this debate unfortunately provides some support for a moderate constructionist view, closer to what Mäki (1992) calls the ethnographic version than to the Edinburgh version. That adherents to the dominant paradigm ignored the questions which monetarists were raising shows how in this case knowledge was socially constructed, in the sense that it was considered acceptable to ignore relevant information. It is likely that the disposition to ignore the monetarist challenge had something to do with the tendency of many major U.S. economics departments to employ no, or almost no Chicago graduates, who tend to congregate in a minority of departments. The monetarist challenge is therefore something that one might -- or might not -- read about, but does not have to face on a day-to-day basis. The way academic networks are constructed may well affect the way economics is constructed.

To be sure, one might respond that it is not knowledge *per se*, but only the "knowledge" of a particular group that is here socially constructed; that when one group of scientists ignores important evidence, another group will bring this evidence into the open. This did happen here. But was it inevitable? Suppose that Friedman had become, say a statistician instead of an economist, that Karl Brunner had not received the fellowship that allowed him to come to the Cowles Commission at Chicago, and that Allan Meltzer would therefore have studied with someone else. Would there have been a thriving monetarist school to challenge the implicit Keynesian assumptions? One can respond that the points made by these monetarists were out there waiting to be discovered, if not by them, then by others. (Cf. Maki. 1992.) Self-interest impels economists to search for undervalued arguments. But if they had been discovered and advocated by economists less gifted than the three just mentioned, and in particular if these others had lacked the persuasive skills of Friedman, would many people now

know about it? I believe that eventually the monetarist challenge would have to come into its own, but eventually is a long time.

WHY DID THIS DEBATE GO SO BADLY?

One can only speculate about the reasons why the debate was not more productive. The first failure, asking too broad a question, is probably related to the vehemence of the general monetarist debate. Mussa (1994, pp. 138-39) refers to it as "fundamentally a religious controversy, intrinsically related to the age-old dispute over free will versus predestination." Pierce (1995, pp. 33 and 30) also calls the overall monetarist-Keynesian debate "largely a religious debate," and refers to "a singular unwillingness for the proponents on either side to listen to the propositions of the other."

In turn, the vehemence of the debate may be due to some extent to ideological factors that is to the Chicago school sharply disagreeing with other economists on so many issues, and that Friedman being so strongly identified with pronounced free market view;. Is this combination of monetarism and a strong belief in free markets an accident of history as Tobin (1981) suggests? Surely, one can believe that the lag of monetary policy is long and variable, and that our ability to forecast and to predict the effect of monetary policy is limited, without opposing the welfare state. One can also accept the monetarist argument about political pressures on central banks and about their inefficiencies and self-interested behavior, and yet subscribe to a "progressive" political philosophy. That is true in principle. But those who consider the government benevolent and efficient enough to remedy various market failures are likely to believe that it can also remedy that great market failure, unemployment. Similarly, those who question the ability and benevolence of the government in general, are not likely to see the central bank as able and benevolent. Much of the dogmatism shown in this debate

may therefore be ascribed to its political nature, thus illustrating the difficulty of doing "scientific" work on a politically charged topic.

Apart from the vehemence of the debate, professional rivalries may also have made the leaders on both sides disinclined to look for a compromise that would have separated the hard monetarist position from the moderate one. Neither side had all that much of an incentive to look for such a compromise. Keynesians, being in the majority, had little incentive since if the debate focused on the moderate monetarist position, there was at least some danger that they would lose their majority status. A fixed monetary growth rate, come what may, is a much more inviting target to attack than is a policy of eschewing counter-cyclical policy. And until velocity became highly unstable in the early 1980s, monetarists may have lacked the incentive to strengthen their case against counter-cyclical policy at the expense of weakening their case against changing the monetary growth rate in response to secular changes in velocity. They may have been afraid that if the central bank is allowed any discretion at all it will surreptitiously follow counter-cyclical policies.

The second lapse from good practice, the monetarists' overstatement of their case, is not all that unusual a lapse, and not only in economics. In his *Treatise on Probability* Keynes (1921, p. 427) tells us: "In writing a book of this kind the author must, if he is to put his point of view clearly, pretend sometimes to a little more conviction than he feels. He must give his own argument a chance, so to speak, not be too ready to depress its vitality with a wet cloud of doubt." It should not be difficult to find examples of overstatements even in the most respectable sciences. All the same, the fewer the better, and monetarists certainly deserve to be criticized for their overstatements.

The third lapse, the Keynesians' dogmatic disregard of the monetarists' evidence, is due, in part, to the politicization of the debate. It is also due, in part, to the monetarists

challenging, not some technical issue in Keynesian theory, but something in which Keynesians take great pride, the ability to reduce unemployment and thus prevent much misery. At the risk of engaging in psycho-babble, it seems that it may also be due to a fundamental metatphysical belief shared by many Keynesians: that with sufficient goodwill and intelligence it *must* be possible to reduce human suffering.⁴⁴

Another possible reason is that, as already discussed, many Keynesians may not have taken the monetarists' suspicions of the Fed seriously. What we may have here is an inability or reluctance to understand the opponent's paradigm. The public-choice view of government, which monetarists generally adhere to, and the public-interest view, which many Keynesians adhere to, are different "visions". As David Colander (1994a and b) has stressed, economists are reluctant to confront differences in vision.

Colander (1994a and b) also discusses the useful distinction between the science of economics and the art of economics, the latter adding to economic hypotheses certain hypotheses from other fields, such as politics or public administration, that are needed to make policy recommendations. Economists feel uneasy about discussing the art of economics because they cannot do so with the degree of rigor to which they pretend. So they concentrate their discussion on the "science" part of the argument. But this is usually of little general interest, and most economists do want to deal with matters of widespread interest. To avoid this dilemma they act as though the strength of an argument is as strong as its strongest link, and hence as though only the science of economics matters. (See Mayer, 1993.) With respect to the "art" part of the argument, they behave as there were a minimum

⁴⁴ (See Mayer, 1994) The flavor of such an attitude is conveyed by the following statement of Modigliani "Nonmonetarists accept wha

standard of rigor, and that on any issue that cannot be treated with that degree of rigor, all is lost, and one person's opinions are as good as another's. Hence, they are tempted to disregard the art statements of those they disagree with as though they were mere asides. But that means sweeping problematic assumptions under the rug, and pretending to more knowledge than one possesses.

That may also explain why neither side made much, if any, use of the political-science literature dealing with the Fed and with bureaucratic behavior in general. This sort of parochialism is common in economics. (See Hausman 1992.) An alternative explanation is that this literature does not provide enough strongly confirmed propositions. But if political-science propositions are a necessary part of the argument, then that is less of a justification for ignoring what is available in the political-science literature than for expressing one's conclusions with humility.

The profession's great emphasis on the application of sophisticated techniques probably also played a part. Central bank behavior is an issue that is best studied by looking at specific events using tools more akin to those of the historian than those of the mathematician. Few economists are willing to do that type of work.

Still another reason why Keynesians did not attend adequately to the monetarist challenge may be the way monetarists presented their argument. Had they presented it explicitly as a challenge to Keynesians to bring into the open and to justify their implicit assumptions about the central banks' ability to predict and its motivation, the debate might have been more productive. Instead, by putting their argument in terms of the desirability of a monetary rule, they presented Keynesians with a much easier target than having to discuss their own assumptions. It is much easier to criticize Friedman's rule (see Lerner, 1962; Goldfeld, 1982; Tobin, 1983) than directly to justify the Keynesian assumptions.

Finally, both monetarists and Keynesians claimed more knowledge than they possessed because! they were addressing not only a professional audience, but also policymakers and the politically aware public. Friedman's main statement of his position (Friedman, 1960) originated in a series of lectures he gave to a general audience, while Keynesians advocated their position in many popular magazines and in congressional testimony. Such audiences are used to overstatements and indeed would not be moved by arguments that start with: "we don't *really* know, but it seems likely to me that... ." Policy debate; foster overstatements; economists who say "on the one hand. ... but on the other hand" are not popular¹⁵.

IN CONCLUSION

All in all, economists *cannot* feel proud of the debate about the monetary growth-rate rule. But before placing an order for sackcloth and ashes one should consider three things. First it is precisely this type of topic that is likely to generate an unsatisfactory debate, in part because of its high art component, and in part because it engages political sympathies so strongly. The difficulty of bringing empirical evidence to bear must also have contributed to the low quality of the debate. Other aspects of the overall monetarist debate, such as the debate about the behavior of velocity, or about the St. Louis model, are much closer to meeting the standards of good conversation. It might be useful to see to what extent other debates that engage political attitudes, such as the debate between the two Cambridge's, and the debate about the appropriate level of unemployment benefits, fail to measure up to reasonable standards.

¹⁵ There is the story of a businessman who told a friend that he plans to hire a one-armed economist because he is tired of hearing his current economist say "on the one hand ..., but on the other." His friend said, "yes, but before you hire such an economist ask him how he came to lose his other arm."

Second, as Feyerabend has taught us, even in the physical sciences debates do not always conform to idealized notions of "science".¹⁰ It would be interesting to see if in the natural sciences those debates that challenge basic presuppositions are conducted all that much better than the debate reviewed here. A study of controversies in fields like medicine might also throw some light on whether it is common for debates that question the ability to do good tend to be particularly vehement.

Third, despite all the roadblocks the discussion has advanced. The avoidance of counter-cyclical policy is no longer identified with a fixed monetary growth-rates rule. Moreover, I suspect, though I cannot document it, that more economists now pay **serious** attention to the **monetarists'** concerns about **lags** and forecast errors and **to the** possibility of socially perverse behavior of central banks than was true in 1986, the end of the period I am discussing. (See Blinder, 1987.) At the same time, it is likely that many monetarists have abandoned a fixed growth-rate rule, and like Meltzer now support feedback rules. Admittedly, the hard fact of velocity no longer being stable may have been more influential in this than the sweet voice of reason.

¹⁰ Thus Feyerabend (1995, p. 64) reports: "An iron curtain formed by a firm belief in the excellence of Maxwell's equations, and so on and so forth, protected physics from Ehrenhaft -- an iron curtain of precisely the same kind as had protected Galileo's opponents."

Appendix

Table 1 shows the effect of changes in the stock of unborrowed reserves or money on nominal income as estimated in 1975 by eight econometric models. The substantial differences shown by these models also appear in a comparison of models for a later period than the one discussed here. (See Adams and Klein 1990)

Table 1
Dynamic Multipliers: Nominal GNP/Unborred Reserves or Money Stock

Quarter of change	BEA	DRI-74	FRB St. Louis	MPS	Wharton Mark 111*	H-C Annual	Wharton Annual	Liu-H. Month
1	0.0	0.3	1.2	0.4	1.3	N.A.	N.A.	0.0
4	0.2	4.3	5.3	3.6	4.1	1.5	1.8	
8	0.5	3.8	5.1	11.4	6.8	1.3	7.6	
12	0.8	7.9	5.1	17.4	9.2	0.9	16.0	14.0
16	0.9	4.9	5.1	24.7	8.0	0.1	25.8	N.A.

Model Codes:

REA..... U.S. Department of Commerce, Bureau of Economic Analysis
 DRI..... Data Resources Inc.
 FRB St. Louis. Federal Reserve Bank of St. Louis
 Wharton..... Wharton Econometrics
 H-C..... Hickam-Coen model
 Liu-Hwa..... Lie-Hwa monthly model

* Standard Anticipations Model

Note: The policies simulated with the various models are not exactly the same, but are close.

Source: Fromm and Klein (1976, p. 25)

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