Volume Title: Rational Expectations and Economic Policy

Volume Author/Editor: Stanley Fischer, editor

Volume Publisher: University of Chicago Press

Volume ISBN: 0-226-25134-9

Volume URL: http://www.nber.org/books/fisc80-1

Publication Date: 1980

Chapter Title: On Activist Monetary Policy with Rational Expectations

Chapter Author: Stanley Fischer

Chapter URL: http://www.nber.org/chapters/c6265

Chapter pages in book: (p. 211 - 247)

7 On Activist Monetary Policy with Rational Expectations

Stanley Fischer

This paper discusses the potential effectiveness and desirability of activist monetary policy[1] and also rules versus discretion. Recent academic discussions of the role of monetary policy have been heavily influenced by the rational expectations approach to macroeconomics: it has been argued that, from the viewpoint of the behavior of output, any monetary policy rule strictly adhered to is as good as any other (e.g., Sargent and Wallace 1975, Barro 1976). This theoretical viewpoint receives support from empirical work by, among others, Sargent (1976*a*) and Barro (1977*a*, 1978), which appears to show that only unanticipated changes in the money stock affect output.

This paper accepts both rational expectations, as a theory of expectations, and the view that "unanticipated" changes in the money stock have a greater impact on real output than anticipated changes in the money stock. It argues nonetheless that systematic countercyclical monetary policy can affect the behavior of output and that activist monetary policy should be used for that purpose.

The argument starts by asking why economic agents have not made contingent arrangements—for example, wage rates indexed to the money stock or very short contracts—that would insulate them from the effects of unanticipated changes in the money stock. The answer is that such contingent arrangements are costly; the private sector is therefore willing

I am grateful to David Modest for research assistance and to Olivier Blanchard, Rudiger Dornbusch, Jacob Frenkel, Robert Gordon, Robert Hall, Michael Rothschild, Frank Schiff, and members of the M.I.T. Money Workshop for comments. Research support was provided by the National Science Foundation.

1. While I concentrate on the same issue as Franco Modigliani in his 1977 AEA Presidential Address, the approach will be seen to differ from his.

to bear the costs imposed on it by the output deviations resulting from unanticipated money changes.

The potential role for monetary policy is created by those same costs of insulating the private sector from disturbances. The case for active monetary policy is that it is more efficient for the Fed to offset aggregate disturbances than it is for the private sector to do so. The efficient division of labor between the private and public sectors leaves it to macroeconomic management to deal with aggregate disturbances.

The perspective of this paper is one that views the private and public sectors as potentially cooperating in responding to economic disturbances; it contrasts with the view associated with rational expectations theorists that tends to regard monetary policy as working mainly through deception. Once the cooperative view of policy is adopted, the relevant questions about the desirability of activist monetary policy become those familiar from Milton Friedman's (1960) argument for a constant growth rate rule: they concern the possibility that attempts to control the economy could be destabilizing (long and variable lags) and the alleged propensity of the Fed to misbehave.

Although I do not accept the policy perspective of much of the rational expectations literature, this is not an attack on the rational expectations hypothesis. The rational expectations theory of expectations —that individuals form expectations optimally on the basis of the information available to them and the costs of using that information—has become and will remain the leading theory of expectations.[2] But there is nothing inherent in the hypothesis that implies that activist policy is either impossible or undesirable.

Since the paper ranges widely, it is useful to outline the argument. Given recent claims about the ineffectiveness of systematic monetary policy, and the evidence apparently supporting such claims, I have first to establish that there is something to talk about. Sections 1 and 2 therefore lay the groundwork for the claim that, rational expectations-oriented work notwithstanding, systematic monetary policy matters for the be-

2. It is worth distinguishing between the "strong form" of rational expectations, which assumes that individuals' subjective probability distributions are the same as those implied by the models in which they are presumed to be agents, and the "weak form," which is defined in the text. ("Semi-strong" forms of rational expectations may be defined to require that the first n moments of subjective probability distributions coincide with those of the model.) I believe that rational expectations in the weak form, will be the leading theory of expectations in the same sense that utility theory (or its equivalents) is the leading theory of consumer behavior. We frequently use models in which behavioral functions are not explicitly derived from maximization, but are uneasy in doing so, and are reassured if it can be shown that the behavioral functions are consistent with maximization. Similarly, economists will continue to use adaptive and other prespecified models of expectations, but will feel constrained to apologize for, and attempt to justify, doing so.

havior of output. Assuming that claim is established, the issue of whether activist policy *should* be used remains. Section 3 discusses the desirability in principle of activist policy; section 4 discusses activist policy in practice; and, finally, section 5 considers rules versus discretion.

In more detail, it is shown in section 1 that there is a variety of mechanisms through which even fully anticipated monetary policy can affect the behavior of output. These mechanisms, however, are not central to the case for countercyclical monetary policy, which hinges on short-run considerations.

Section 2 therefore reviews some of the evidence that only unanticipated changes in the money stock affect the behavior of output. If it could be established that any systematic monetary policy had no real effect on output, then there would be little to discuss about countercyclical policy except to the extent that price level behavior matters. Recent empirical work by Barro (1978) does indeed appear to establish that only unanticipated money matters for the behavior of output, but in fact Barro's results are quite consistent with the view that systematic monetary policy can be used to affect output: the crucial issue for the potential effectiveness of policy is whether output is affected by expectations that were formed before the monetary authority had to commit itself to a particular level of the money stock. Results presented in the appendix show that if Barro's mechanism of expectations formation is accepted, then the data do not reject the hypothesis that two-year-ahead forecast errors of the money stock affect the behavior of output. Since the Fed can clearly react to events with less than a two-year lag, Barro's results do not force an end to further discussion of countercyclical monetary policy.

Section 2 argues that systematic monetary policy *can* be used to affect the behavior of output. The case in principle for using activist policy is made in section 3, where it is argued that the same factors that make the economy vulnerable to "unanticipated" money suggest that monetary policy should be used to offset aggregate disturbances—if the use of active policy is not itself destabilizing. The discussion in section 4 accordingly centers on older arguments about monetary policy relating to the long and variable lags with which policy works and the lessons of history.

On the issue of rules versus discretion, I conclude with a presumption in favor of a monetary policy that leaves the Fed an important measure of discretion.

1 Equilibrium Considerations: Nonneutralities of Anticipated Money

Since any systematic monetary policy would eventually come to be anticipated, it seems that such a policy can continue to affect output only if anticipated changes in the money stock can affect output;[3] accordingly, the natural place to start in considering the case for activist monetary policy appears to be with the nonneutralities of anticipated money. In this section, I will discuss the nonneutralities of fully anticipated money, by which I mean changes in the money stock that are anticipated at the time decisions relevant to the determination of output are made.

The neutrality of money has always been a central concern of monetary theory, precisely because it has long been obvious that money is not neutral. The implications of this fact for monetary policy depend on the source of the nonneutralities. Traditional discussions of neutrality distinguished between the transitional effects of a once-and-for-all change in the money stock, which were generally thought to affect real variables, and the long-run or equilibrium effects of the change, which analysis suggested were insubstantial.[4] Modern analysis has added two important distinctions to the discussion: (1) that between the neutrality and superneutrality of money, corresponding respectively to the effects of changes in the stock of money and growth rate of money, the latter producing changes in the inflation rate; and (2) that between anticipated and unanticipated changes in the money stock.[5]

Anticipated Inflation

In this section we concentrate on nonneutralities of money that arise from anticipated changes in the money stock and consequent changes in the expected rate of inflation. Informational considerations are deferred to section 2. As long as money pays no interest, changes in the expected rate of inflation change the expected real return from the holding of money, affecting the demand for real balances, and creating the possibility that anticipated changes in the growth of money affect real variables.[6]

3. I shall argue below that this statement is in important respects misleading. A systematic *policy*, i.e., a rule that specifies money supply responses to disturbances, will itself eventually be anticipated, but actual changes in the money stock under such a policy may not have been anticipated as of an earlier date when decisions relevant to the determination of output were made.

4. See, for instance, Irving Fisher 1922.

5. Both distinctions were at least implicit in the older discussions. First, there was typically mention of the elasticity of expectations, suggesting awareness of the importance of changes in the expected rate of inflation. Further, the typical money stock change had people waking in the morning to discover the good news of a doubling of their holdings, reflecting awareness also of the distinction between anticipated and unanticipated events.

6. Two assumptions are maintained until further notice. First, there are no interest payments on money. Second, the government does nothing other than distribute money to the economy through transfer payments, which, however, are not related to individual holdings of money by the transfer recipients. The second as-

Consider first the standard two-period lifetime consumption loans model in its simplest form: there is no production and each individual has an endowment of a nonstorable consumption good in the first period of his life; money is the only vehicle for saving. Changes in the growth rate of money affect the intergenerational allocation of resources in such a model if, say, the lump sum transfers are made to the old. If endowments varied stochastically over time, and there was a somehow agreed-upon social welfare function for weighting generational expected utilities, the government might optimally want to vary the growth rate of money. But since output is exogenously determined, monetary policy obviously does not affect the level of output.

The monetary authority's ability to affect the allocation of resources depends in this case on its ability to affect the real interest rate and thus saving. Higher rates of monetary expansion reduce the real interest rate by raising the expected rate of inflation. If we now allow for the inclusion of an endogenous labor supply (but do not yet add productive capital to the model), it will still be true that the monetary authority affects the real interest rate by varying the growth rate of money. Labor supply, and thus output, will respond to variations in the real rate of interest. A case for activist monetary policy in a context in which there were variations in the productivity of labor could once again be made, given a social welfare function.

Expansion of the menu of assets makes it necessary to provide a rationale for portfolio diversification, particularly the holding of money. The simplest rationale lies in the existence of some form of transaction costs in buying and selling assets other than money.[7] Putting money in the utility function will also generate portfolio diversification; this device is best thought of as being justified by the existence of transaction costs that are not explicitly included in the analysis, but rather implicitly treated as foregone utility. A third possible source of diversification is risk aversion, though here it is necessary to ensure that money is not a dominated asset.

Sidrauski (1967) has elucidated the very strict conditions under which the rate of inflation does not affect the level of output in a model with labor and capital as factors of production and money and capital as assets. Money is superneutral if the optimizing units in the economy are

sumption is designed to rule out, for the moment, real effects of anticipated inflation arising from the tax system.

7. At this stage the consumption loans model becomes more difficult to use, since it tends to emphasize the store of value function of money, while the transaction costs arguments rely on the medium of exchange function. See Bryant and Wallace 1978 for the attempted incorporation of money in a consumption loans model with other assets.

infinitely lived, if the quantity of real balances does not affect the economy's production possibilities, if labor is inelastically supplied, and if consumers have a constant discount factor for comparing utilities over time. The steady state capital stock is determined by the modified golden rule condition that the marginal product of capital be equal to the sum of the consumers' rate of time preference and the growth rate of population. Even this set of restrictions does not, strictly speaking, imply superneutrality, since economic agents are not indifferent to the rate of inflation.

Relaxation of the specified conditions will again produce nonneutralities of anticipated money. If labor supply is not exogenously fixed (Brock 1974), or if consumers do not effectively maximize over an infinite horizon (Drazen 1976), or if money enters the production function, money will not be superneutral. Nor does the superneutrality apply to the behavior of the economy before the steady state is reached (Fischer 1979*a*); more rapid rates of money growth tend to produce more rapid rates of accumulation of physical capital in the transition to the steady state.

Once there is a rationale for the holding of money, expansion of the menu of assets, held on grounds of risk aversion, introduces no fundamentally new issues. It is therefore useful to step back to examine the two basic mechanisms at work rather than continue to catalog possible nonneutralities. The first mechanism arises from the possibility that changes in the real return on holding money affect interest rates on other assets, thus portfolio composition, and possibly the rate of saving and labor supply. The second mechanism operates through the effect of an increase in the expected inflation rate on the level of real balances. Lower real balances may imply more transactions and less resources available for production; they may also produce wealth effects that will affect spending on goods and services and labor supply.

The empirical significance of these mechanisms is not known. But there is a priori reason to think the effects will be small. First, they do not all work in the same direction: the accumulation of physical assets induced by anticipated inflation tends to increase output, whereas the diversion of resources from the production of goods to the production of transactions tends to reduce final output. Second, the base on which the real balance effect works is small; the stock of non-interest-bearing money is less than M_1, since some implicit interest is paid on demand deposits.[8] Further, it is likely that explicit interest payments on demand deposits will soon become legal, leaving currency as the only non-interest-bearing nominal asset.

8. Startz (1978) estimates the implicit rate to be half the competitive rate.

Institutional Effects of Anticipated Inflation

Up to this point, we have confined the government to making lump sum transfer payments in determining the growth rate of money. We want now briefly to consider the real effects of anticipated inflation arising from the nature of the tax system and other government regulations.

There is first the inflation tax itself. Changes in the growth rate of money affect the real revenue the government obtains from the creation of high-powered money and make it possible to vary other taxes, given the level of government spending. Changes in the pattern of taxation will have real effects, though little more definite can be said without considering the details of the tax structure.

The primary nonneutralities of the tax system arise, however, from nonindexation of taxes. The major effects will arise from the payment of taxes on nominal, rather than real, interest (combined with differential rates of personal and corporate taxation), and from the use of historical cost as the basis for depreciation.[9] Each of these features of the tax system implies that increases in the anticipated rate of inflation would discourage capital accumulation. Similarly, despite changes in the method of financing housing investment in the last few years, anticipated inflation still has potentially large effects in reducing the volume of housing investment;[10] the effects may be attributed in part to the existence of government-imposed interest ceilings.

It is worth noting that the specified characteristics of the tax system and housing financing are part of the institutional setting of the economy that has not completely adapted to the existence of ongoing inflation. Their existence thus cannot be relied on as a permanent mechanism through which monetary policy will affect the economy. It is significant, however, that the institutional features remain at least partly in place after twelve or more years of continuing inflation. The costs of changing the institutions of an economy that are based on an implicit assumption of the stability of the value of money to those that are based on the recognition of ongoing inflation must be substantial.

The institutional nonneutralities discussed above tend to make increases in the anticipated rate of inflation reduce the rate of investment and subsequent output. The net effect of anticipated changes in money on output in the current and subsequent periods is thus difficult to predict a priori; it will also probably be a delicate matter empirically to isolate the magnitude of the mechanisms discussed in this section. One place to start is by examining the effects of anticipated changes in money on the real

9. These effects have been emphasized by Feldstein and others; see, for instance, Feldstein and Summers 1978.

10. Details are contained in Modigliani and Lessard 1975.

interest rate. In the next section we also discuss reduced form estimates of the effects of anticipated money on output.

But even if reliable estimates turned out to show that the nonneutralities of anticipated money are not trivial, it would still remain to make the theoretical case for the desirability of activist monetary policy. An initial reaction might be that the factors discussed in this section merely suggest that the growth rate of money should be set at that level which would produce the optimal quantity of money[11] and the economy otherwise left free of monetary interference. However, in a context in which there are other distorting taxes, the inflation tax should also in general be used to raise revenue. Nor, even ignoring the inflation tax, is the optimal quantity of money provided by keeping the growth rate of money constant if the marginal product of capital varies over time. The argument for an activist monetary policy would thus be derived from analysis of the optimal inflation tax: as government expenditure varies, and other disturbances impinge on the economy, the optimal use of the inflation tax would also change. The optimal growth rate of money would therefore change as the state of the economy changed.

There are three main conclusions from this section. First, there are sound theoretical reasons for thinking that anticipated money is not necessarily neutral. Second, we do not at present have empirical knowledge of the net direction and magnitude of the mechanisms underlying the neutralities. Third, there is no reason to think that an optimal monetary policy derived in a model in which nonneutralities are present, and in which revenue from the inflation tax accrues to the government, will be a constant growth rate rule. Put differently, considerations of the type discussed in this section do not attach any sanctity to the constant growth rate of money.

A fourth conclusion should also be drawn: While the nonneutralities of this section may eventually be important in designing a framework for monetary and fiscal policy, they are not of central importance to the debate over countercyclical monetary policy. We therefore turn to the nonneutralities of unanticipated money.

2 Nonneutralities of Unanticipated Money

Emphasis by Lucas (1973) and others on the importance of the unanticipated component of the change in the price level has led to empirical work, of which the best known is by Barro (1977*a*, 1978), which ap-

11. Friedman (1969) suggests that the optimum quantity of money obtains when the economy is satiated with real balances; this requires that money pay a real return equal to "the" real interest rate on other assets. The positive real return on money is achieved by producing deflation.

pears to show that only unanticipated changes in the money stock affect real output and that anticipated changes in money have no real effects. A finding that only unanticipated money affects the behavior of output would be significant for the conduct of monetary policy, though not decisive in establishing the desirability of a constant growth rate rule. The case for activist policy would then have to rest on the effects of the policy on the natural level of output and on its implications for price level behavior. The welfare case for a monetary policy that operates by surprise or deception appears to be a difficult one to make, so that the strong Barro position that only unanticipated money works would tend to support rules over discretion.

For the purposes of this paper, I want to show that Barro's results are not inconsistent with the view that systematic monetary policy can affect the behavior of output. I therefore do not have to enter into a detailed argument about the real meaning of Barro's results or even into the question of whether he has successfully measured expectations of the growth rate of money[12] though fundamental criticisms will doubtless center on this latter issue.

The key point in my argument is that anticipations of money growth for periods other than one year ahead (Barro uses annual data) are relevant to the determination of output. I believe that, to a useful first approximation, the long-run Phillips curve is vertical. That means that fully anticipated changes in the money stock would not affect unemployment significantly. But one can hardly imagine a change in the money stock that has always been anticipated: *every* change in the money stock must be unanticipated as of *some* earlier date. If the Fed can respond to disturbances occurring after decisions relevant to the determination of output are made, then it can systematically affect the behavior of output.[13]

The Barro Output Equation

I review Barro's procedure briefly in the text; more detail is provided in the appendix. Unemployment, or the deviation of output from trend, is explained in a regression using annual data with actual and unanticipated changes in the money stock as regressors. A single stable money supply rule was estimated and taken to have been used in forming expectations, based on information available one year ahead, of monetary

12. David Germany (1978) points out that the restrictions Barro needs to identify the coefficients on unanticipated money in his output equation are literally incredible: it is assumed that expectations are known (by the output regression runner) exactly.

13. This point is worked out in Fischer 1977*a*. That article implicitly accepted the view that systematic monetary policy would be used to "deceive" the private sector, rather than the view of the present paper that systematic policy can be used to produce desirable outcomes more cheaply than is possible with a passive policy.

growth over the period.[14] Barro finds that unanticipated increases in the growth rate of money significantly increase the level of output; the hypothesis that anticipated changes in money also affect the behavior of output is not accepted.

A relevant question about Barro's results from the viewpoint of activist policy concerns the time interval over which "unanticipated" is defined. In an earlier paper (1977) I argued that anticipations of the price level more than one period ahead might enter the output equation. Analogously, it is possible that expectations of the money supply formed two periods back, rather than one period back, could enter the output equation.

Using Barro's money supply equation, I have constructed two-period ahead forecast errors for the money stock and included them in the output equation. (Details are in the appendix.) As would be expected, the two-period forecast errors are collinear—though not perfectly so—with the separate one-period forecast errors over the same two years. The inclusion of a two-period ahead forecast error in the output equation reduces the standard error in that equation, but not significantly so. Replacing the first one-period ahead forecast errors with a two-period error reduces the standard error of estimate, though not significantly. I conclude that the data cannot tell us whether only one-year ahead or only two-year ahead errors in predicting money, or both, contribute to explaining the behavior of output—though if forced to choose, the data choose the two-period forecast error. My belief is that both types of forecast error are relevant; there is nothing in the Barro data to reject that view.

The reason the inclusion of the two-period ahead error matters is that it is very hard to argue that the Fed cannot use a monetary rule that reacts within a period of two years to new information. If the two-year expectation is somehow locked in (for example, in labor contracts), then the Fed has ample time to act to affect the behavior of output. That does not mean it *should* act, but rather that it can systematically affect output. Moving in the other direction, though, it is also difficult to believe that the Fed cannot within the period of a year systematically react to information that becomes available to it, after the one-year ahead expectations are locked in. That is, the length of the Barro period suggests that the Fed can systematically produce unanticipated money—by acting on information that becomes available within the year.[15]

14. In an earlier version of his 1977*a* paper, Barro showed that his results were not significantly affected if a money supply equation based only on data available up to the time an expectation was formed was used in generating the expected change in money.

15. It is, of course, true that whether or not the Fed can systematically produce unanticipated money depends on private sector contracting arrangements; I return to this point below.

This possibility raises the familiar mutual causation question, as a potential explanation for the apparent strength of the effects of unanticipated money. It is somewhat surprising that Barro finds a stable money supply process over a period during which the Fed moved from a policy of supporting interest rates to one in which it claims to pay attention to monetary targets; it is also surprising that there is no apparent role for interest rates in Barro's equation.[16] His results might reflect the effects on both money and output of movements of other variables that tend to increase output, with the Fed increasing money to smooth interest rates.[17]

The Lucas Supply Function

Given the uncertainties raised in the preceding paragraphs, it would be useful in judging the importance of Barro's results to know what mechanism might have produced them if they were true. The impact of an unanticipated increase in the growth rate of money by one percentage point produces an increase in output of over 1% in the current year, and nearly 1.2% in the following year. The Fed rolls high-powered dice.

There are two competing explanations for results of the type Barro has obtained. The first is the standard rational expectations supply hypothesis, which will be detailed below. The second is a Keynesian story, which attributes Barro's results to the stickiness of wages that are based on expected prices.[18] The first explanation tends to rule out a role for active policy, while the second does not. The Phillips curve is an implication of both stories and cannot be used to distinguish between them.[19]

In this section I discuss the Lucas supply hypothesis to see whether there is independent evidence suggesting that it underlies Barro's reduced form results. The Lucas supply function is:

$$y_t = y_{nt} + b(P_t - {}_{t-1}P_t) + e_t, \tag{1}$$

where y is the level of output, y_n is the natural or full employment level of output, and P is the price level, each in logarithms; e is a disturbance term, and the notation ${}_{t-1}P_t$ denotes the expectation of P_t that is formed

16. See the comments on Barro's paper (chap. 2) in this volume by Robert Weintraub.

17. Preliminary evidence indicates that unanticipated increases in money (as measured by Barro) are positively correlated with unanticipated increases in short-term interest rates (the expected interest rate is calculated from the term structure), providing some support to the notion that increases in the demand for money partly produce unanticipated money.

18. Backward looking "catch-up" elements are also typically found empirically in the Phillips curve; Taylor 1979 has a model with overlapping labor contracts in which workers are concerned with relative wages, which is consistent with estimated Phillips curves.

19. I am grateful to Robert Hall for emphasizing this point.

on the basis of information available at time $(t - 1)$. The Lucas analysis is most accessibly developed in his 1973 article; the rationale for (1) builds on information confusions, which cause individuals to increase their supply of output when nominal prices increase, under the mistaken impression that the relative price of their output has risen.

The key element in the Lucas mechanism is the increase in the supply of output in response to a rise in the perceived relative price, a story that is most naturally told as the model of an individual supplier of labor services, for whom the price of output is the nominal wage. Lucas (1977) notes, however, a very similar mechanism would operate in the case of firms. The strength of the mechanism would be greatest in response to an increase in the perceived real wage that was thought to be temporary, for in that case workers would like to increase the amount they work in the current period (at a high wage) and substitute more leisure next period (when the wage is expected to be lower than its current level). An increase in the real wage that is expected to be permanent might not elicit any increase in output, since labor supply curves may even slope backward.

Doubts can be raised about the supply mechanism (1). First, as David Small (1977) has pointed out, the assumed reaction of workers to an increase in the current price level requires it to signal an increase (or at least not a large decrease) in the real interest rate; in a model in which monetary growth affects the real interest rate, monetary policy can negate the labor supply response to unanticipated inflation.[20] Second, the mechanism provides no real explanation of a relationship between unanticipated inflation and the unemployment rate—it appears that those who choose not to work when the perceived real wage falls would not be unemployed. Perhaps, however, the existence of unemployment insurance makes it profitable to appear to be unemployed even when workers desire to reduce the amount they work; in addition, movements in the participation rate, as in Sargent (1976*a*), might help explain movements in the unemployment rate. Third, if this mechanism were powerful, temporary income tax changes would be potent instruments for affecting the pattern of output over time—and there is little evidence of such potency. Fourth, given the crucial importance of the mechanism, the empirical support for it is small.[21]

Unanticipated Money and Sticky Prices

The evidence supporting the Lucas supply hypothesis is hardly strong enough to justify the view that it is the main mechanism underlying

20. Bulow and Polemarchakis (1978) have studied essentially this mechanism.

21. Lucas refers to his work with Rapping (1969), to work by Ghez and Becker (1975), and some more casual evidence. The Ghez and Becker evidence does not appear to bear strongly on cyclical labor supply substitution.

Barro's empirical results. Indeed, Barro's (1978) price equation reveals some stickiness of the aggregate price level, leading him to remark that the money-to-price link may be too weak to explain the estimated effects of unanticipated money on output.[22]

The stickiness of prices suggests that a Keynesian mechanism, in which changes in money affect aggregate demand, which affects employment, may be at work. The response of some prices, particularly wages, to changes in demand is sluggish relative to the period over which policy is formulated;[23] Sargent (1976*a*) finds that wage rates may be treated as exogenous in a quarterly macro model. The most plausible generalization of the Lucas supply function is probably this: the longer in advance a given type of change in the money supply has been expected, the greater the effect on prices relative to the effect on output, with the effects being proximately attributed to the stickiness of nominal prices fixed over different horizons.[24]

In the short run (maybe several years) in which prices are sticky, monetary policy can affect the behavior of output in the manner suggested by Keynesian disequilibrium analysis, in which quantities are not necessarily determined at the intersections of supply and demand curves. There is no presumption that any intervention can only worsen the situation in such circumstances.[25]

The conclusions from this section are that there is no strong evidence for the view that only unanticipated (with a one-year horizon) changes in the money stock affect output. The data are not strong enough to force acceptance of the view that it is one year ahead rather than longer or shorter forecast errors that are relevant to the behavior of output. Similarly, while there is some evidence supporting the Lucas supply mechanism, there is also evidence for price stickiness.

We are now free to discuss activist policy.

22. Since interest rates are held constant in Barro's price equation, a more complete analysis might reverse, or for that matter, strengthen, this conclusion.

23. Poole (1976) argues that there is some period short enough that the price adjustments assumed in the equilibrium supply framework do not operate.

24. This comment applies to the extent that money is neutral, price stickiness aside. In Fischer 1979*b* I show that when anticipated money affects output, prices may rise less the longer a given change in money has been expected—because the anticipated money then affects output more.

Taylor's 1979 model produces an adjustment pattern like that referred to in the text.

25. It can and has been objected to the view that short-run price stickiness implies that output is not optimally determined and can be predictably affected by monetary policy, that the private sector would not enter into arrangements that would "predictably" imply a deadweight loss (Barro, 1977*b*). By the same token, the private sector would presumably not enter into arrangements that leave it vulnerable to the effects of unanticipated money.

3 The Desirability in Principle of Activist Policy

The classical argument for government control of the money supply is that a fiat money system is unstable, tending to degenerate into a commodity money system. Historically, central banking developed in response to a slightly different instability: that of a financial system in which the quantity of claims on the existing stock of commodity money was larger than that stock. The Bank of England, for instance, was driven against its will to manage the London money markets by financial crises that threatened private sector financial institutions (Bagehot 1906, Sayers 1957). The private sector can manage financial panics,[26] but the nineteenth- and early twentieth-century record indicates that better management should not be difficult—though the Great Depression proves that worse management is also possible.[27]

At a general level, we can agree that if the government is to control the money supply, it should provide a stable monetary background against which the economy can proceed with its real business of producing and consuming goods. If there were no disturbances to money demand, arising from disturbances affecting the level of output or interest rates, or the random term in the demand function, a stable monetary background would be a stable (predictable) money supply. A constant growth rate rule would serve well.

But there are, of course, disturbances to money demand. In the long run these take the form of changes in the assets that constitute money. Historically, the process has been one of a broadening of the class of assets that serve as the medium of exchange. Price level behavior over the long term would become less and less predictable if monetary policy were devoted to control of the supply of an asset that constituted a decreasing proportion of the money supply. We therefore cannot expect that a constant growth rate rule, or for that matter any other rule, would remain inviolate over the long term; occasions would arise when it would be necessary to change the asset whose growth rate was being controlled.[28] Such changes hardly constitute activism, however.

The General Rationale for Countercyclical Monetary Policy

The important issues arise in the short run. Short-run disturbances to money demand arise both from goods market disturbances that affect the level of income and the interest rate and from random shifts in

26. Friedman and Schwartz (1963) suggest that the private sector would have handled what became the Great Depression better than the Fed had the latter not existed.

27. I assume that enough has been learned (and that institutions have changed) so that the Fed would not again act as it did in the early 1930s.

28. The 100% money plan would have difficulty in controlling the development of money substitutes.

money demand; the money demand function does not fit perfectly even for the sample period 1955–73. The evidence reviewed in section 2 suggests that by reacting to these disturbances, the Fed can affect the subsequent behavior of output, interest rates, and prices, even if the policy actions constitute a regular pattern of behavior and are in that sense anticipated.

I shall also argue that it is at least potentially desirable that the Fed seek to offset disturbances. The argument most usefully starts from the recognition that there would be no reason for disequilibria to emerge as a result of monetary disturbances in the absence of transactions and information costs. In the absence of such costs, the private sector would closely monitor the aggregate price level and aggregate money stock and make contracts contingent on them. Unanticipated money—or any other disturbance—would create disequilibrium, or an unsatisfactory state of affairs, for only as long as the arbitrarily short period over which prices and wages were fixed. There is, of course, noise in both price and money data, but some information is better than none.

It might be suggested that the private sector does not enter into complicated arrangements contingent on aggregate variables because aggregate fluctuations account for only a small part of the risk facing individual economic units. Such an argument is both correct and incomplete; it has to be combined with the obvious assumption that there are costs of acquiring and processing information, of writing detailed contingent contracts, and of reducing the length of contract periods, if it is to account for the nonexistence of the contracts that would render the private sector immune to aggregate disturbances.

The costs that prevent the private sector from insulating itself against aggregate disturbances lead also to temporarily sticky prices that produce the presumption that private sector output is not continuously optimal. Those costs are the underlying reason there is a potential role for activist monetary policy in attempting to offset aggregate disturbances.

If one takes the view that monetary management has the task of offsetting aggregate disturbances that the private sector has not made arrangements to deal with, the goals of policy are the standard ones of full employment (minimizing the deviations of the unemployment rate from the natural rate) and price stability.[29] Price stability is desirable in part for the reasons emphasized in the Lucas supply mechanism: it enables the price system to operate more efficiently.[30] But this cannot be

29. This sentence glides over some difficult issues, particularly in relation to price stability versus price predictability.

30. It has, of course, been recognized that a desire for price level stability would support an activist monetary policy even if anticipated money did not affect output (Sargent and Wallace 1975). But it is important to realize that price level *predictability*, as well as stability, can in principle be increased by the use of active feed-

the full explanation for the weight that inflation aversion has in public opinion polls.[31]

To say that monetary policy should have worthwhile goals is hardly a policy prescription. Detailed prescription cannot be expected from a paper that does not present an empirical model as a basis for prescription, though I do in the next two sections discuss general characteristics of desirable monetary policy. In principle, the optimal monetary policy to be used for stabilization can be studied using an appropriately specified macroeconometric model, which pays due attention to the effects of changes in policy regime on the structure of the model.[32] Such models are not inherently impossible to build.

4 Activist Policy in Practice

There is no inconsistency in accepting the general argument of section 3 for activist policy and in urging the immediate acceptance of a constant growth rate rule (CGRR). After all, we do not know the optimal activist policy. In this section I concentrate on a comparison among a number of monetary policies, leaving the rules versus discretion issue to section 5.

The first policy is the most difficult to describe: it is the current system, in which the Fed makes monetary policy as best it can, with input from business, academic, and other sources of pressure, and in ways that change over time. The second is the constant growth rate policy (CGRP) or a passive policy. Most studies of alternative monetary policies have compared these two, with history serving as the representation of Fed policy. Third I will consider a policy that is intermediate between the

back rules. The predictability at issue is that of prices in the more distant future. In a number of models, the one-period ahead variance of the price level is the same whatever the monetary rule that is being followed. But the uncertainty today about the level of prices in the distant future in general is greater if monetary policy does not respond to current disturbances than if it does attempt to stabilize prices. To the extent that price level predictability more than one period ahead is relevant to the allocation of resources, activist monetary policy might be desirable on those grounds alone.

31. Fischer and Modigliani (1979) list many of the real effects of inflation on the economy; these may in part account for popular attitudes to inflation, which are frequently ascribed to irrationality.

32. The warning in Lucas 1976 that the structure of econometric models will not remain invariant to policy changes applies also to the structure of contracts. The monetary policy of the last three decades has, by some accounts, been largely in error but the private sector has allowed itself to be left in the position where, by some estimates, a 1% unanticipated change in the money supply affects output by 1% within a year, and more the next year. If monetary policy were to improve, the private sector would make itself *more* vulnerable to the effects of unanticipated money, by adopting longer-term contracts and paying less attention to monetary variables.

first two—one in which policy is basically passive except in the face of major actual or anticipated disturbances.

The major arguments for CGRP as compared with actual policy are familiar from earlier discussions: they are that ignorance of the structure of the economy makes policy intervention destabilizing ("long and variable lags"); that most serious disturbances have been caused by inept policies; and that political pressures lead to monetary mismanagement. Underlying these arguments is an interpretation of the historical record that claims the Great Depression would have been more moderate had the Fed followed a CGRP (Friedman and Schwartz, 1963) and that macroeconomic behavior in a number of subsequent episodes would likewise have been better had the Fed been following such a policy (Friedman, 1960).[33]

At the theoretical level it is correct that increased uncertainty about the structure of the economy supports the use of more passive policies. Similarly, it is entirely possible for naive policies to be destabilizing. Whether ignorance and naiveté have in practice caused policy to be destabilizing and will do so in the future are difficult questions to answer. The historical record, to which we turn shortly, casts some light on these questions.

Before we examine the record, though, we have to ask whether the entire post-1913 history of the Fed, including the Great Depression, should be thrown into the scales, or whether it is reasonable to assume the Fed has learned something. As previously noted, I will proceed on the assumptions that the Fed can and has learned from history and that deposit insurance, memory, and the persuasive evidence of Friedman and Schwartz, will prevent a repetition of the behavior of the monetary authority during the early 1930s. Similarly, I believe that the Fed is now more aware of the potentially destabilizing influence of stabilizing nominal interest rates than it was in the sixties and that it pays more attention to the behavior of the monetary aggregates than it did.[34]

The Historical Record

The record of monetary policy up to 1960 was studied by Friedman (1960), who emphasized the debacle of the Great Depression and regarded post–World War II monetary policy as less obviously defective (p. 94).

The evaluation of monetary policy in the post–World War II period (or in any other period) presents substantial difficulties. The natural

33. Poole's contribution to this volume makes that claim for the 1971–75 period.

34. The need for this paragraph may not be obvious to all readers. However, some comments on the first draft of this paper persuaded me that the question of whether the monetary authority has learned anything is central to disagreements about CGRP.

way to proceed appears to be to use an econometric model to compare the historical performance of the economy with that which would have occurred under CGRP. Such experiments typically show actual monetary policy outperforming, or not being markedly worse than, a passive policy (for example, Modigliani 1977, Eckstein 1978[35]). Unfortunately these experiments are subject to the reservations emphasized by Lucas (1976) in his discussion of econometric policy evaluation.

The other method of evaluating policy is less formal. It is to select particular episodes for discussion, criticism, and comparison with the results of a passive policy. For instance, it is reasonably clear that the growth rate of money was too high in 1968 and early 1969 and that a policy that maintained the growth rate of money at, say, the average rate of the sixties would have been better.

Similarly, Poole provides an interesting evaluation of the 1971–75 period (see chap. 9). Poole argues convincingly that monetary policy was too expansionary in 1971–72, especially given the existence of wage and price controls. He also suggests that more expansionary monetary policy in the first half of 1974—as urged at the time by, for instance, Modigliani (1974)—would have produced substantially more inflation but little more output than actually occurred. He argues, interestingly, that the Fed could not really have followed a more expansionary policy in the first half of 1974 because such a policy would not have looked right at a time of high inflation and relatively low unemployment. He absolves the fall in monetary growth in the second half of 1974 from most of the blame for the recession. And he argues for a constant growth rate rule.

Although exercises of this type are subject to both the Lucas critique and selection bias, the argument is sufficiently interesting to be worth pursuing. The initial appearance is that Poole's analysis does not support the case for CGRP. The implication of Poole's argument is that monetary growth should have been reduced below the trend rate in 1971–72 to accompany wage and price controls, and it should have been increased above its trend level in the second half of 1974. (Poole seems to be agnostic about the first half of 1974.) If political forces indeed restrained monetary growth in the first half of 1974, then one of the major arguments for rules—that they remove the Fed from unfortunate political pressures—appears redundant.

However, there is more to be said in defense of CGRP. In the first place, although optimal policy in 1971–72 would not have been CGRP, the latter would have been better than actual policy. And second, it is

35. Eckstein's passive policy controls the growth rate of unborrowed reserves rather than M1. The growth rate of money under such a policy is not much more stable than the historical path.

open to proponents of CGRP to argue that there would have been no need for wage-price controls in 1971 if the rule had been in effect in the sixties.

Although Lucas's critique of econometric policy evaluation makes any statements about the historical record difficult to support strongly at this stage, the following remarks are in order. First, monetary policy in the post–World War II period has not on average been markedly worse than a constant growth rate rule, and has probably been somewhat better. Second, it is easy to find particular episodes for which one can confidently assert that actual policy was worse than a constant growth rate policy. Third, we can on general grounds be sure that a 4% growth rule would have produced a lower inflation rate between 1960 and the present than actually occurred. But without an econometric model, we do not know whether overall economic performance—including the behavior of the unemployment rate—would have been better under such a policy.

The historical record since World War II does not tell the unambiguous story that proponents of CGRP find in it, even though there are episodes in which CGRP would have been better than actual policy.

Modified Activist Policy

The arguments against activist policy outlined in this section, and the evolution of actual policy, point in the same direction—toward a policy that responds very little or not at all to minor actual and prospective disturbances, but with proportionately more vigor to actual and potential major disturbances. For want of a better term, I shall refer to this policy as modified activist policy, or MAP.

The arguments made by Friedman against activist policy are telling against fine tuning: given uncertainty about the structure of the economy, policy has to be cautious in reacting to information contained in minor disturbances, in part because data revisions are often large. However, there is no reason why policy should not react to major disturbances, actual or prospective, when it is clear that either expansionary or contractionary policy is required.[36] In saying this, I assume that major disturbances could occur even in the absence of government policy: the nineteenth- and early twentieth-century record suggests that possibility. If it should be the case that large disturbances have been the fault of the Fed, the absence or mildness of fine tuning would soon establish itself as a major success—unless political pressures make it impossible to run a cautious policy.

The discussion of the three policies of this section can conveniently be continued in the next section, under the heading of rules versus discre-

36. The monetary policy required in the case of a demand disturbance is usually clear, but the response to supply disturbances presents greater difficulties.

tion. In practice, a monetary rule would almost certainly be written as a constant growth rate rule, and discretion would mean continuance of the present evolving system of monetary control. In operation, a monetary rule would be much like MAP, for the rule would likely be adapted or changed in response to an anticipated or actual crisis.

5 Rules versus Discretion

The general issue of rules versus discretion in monetary policy amounts to the question of whether the Fed should be given a narrowly defined task by legislation specifying the behavior of variables fairly directly under its control (rules), or alternatively, should be left to decide the appropriate means of achieving ultimate targets of monetary policy (price stability, full employment, etc.) specified by legislation (discretion). As with most convenient distinctions, there is no hard and fast line: a rule that would leave the Fed with a minimum of discretion would prescribe the behavior of its own portfolio; the current situation in which various ultimate targets are mentioned in legislation, but the appropriate weights and the means of reaching those goals are not, gives the Fed a much larger measure of discretion. For convenience, we can draw the line between legislation that controls the behavior of a monetary aggregate (or several aggregates) as being a rule and legislation that prescribes the goals of stabilization policy without specifying the behavior of monetary aggregates as providing discretion.[37]

Any monetary rule would have to be amended as the financial system evolved, as we have already noted. Changes in the rule might also have to be made in the short run, if it proved defective in operation. Indeed, the proposal for a monetary rule is equivalent to the suggestion that monetary policy be subject to the same legislative process as tax changes unless it is seriously suggested that the rule be embodied in a constitutional amendment. The latter suggestion reflects excessive confidence (or hubris) in conclusions reached on the issues discussed in section 4.

Two complementary methods for changing the monetary rule suggest themselves. First, there could be hearings on the performance of the rule at fixed intervals: the Fed might be required to report regularly on the workings of monetary policy and make recommendations for changes. Second, changes could be proposed as the Congress or the Fed or any other agency saw the need.

37. On this definition, Henry Simons (1952) argued for discretion in the 1930s; his proposed monetary rule was that the Fed aim to achieve price stability. At the time he was concerned about the instability of the demand for money. He argued that an optimal system would have 100% money and a fixed amount of it, and he believed that such a system could eventually be set up.

The Case for Discretion

The benefit of discretion, or leaving monetary policy in the hands of the Fed, is flexibility. There are two aspects of flexibility. The first relates to the classic lender of last resort function of the central bank, in which flexibility enables the central bank to intervene in potential financial crises. Such intervention was useful in the Penn Central and Franklin National cases, even if the methods of intervention in the latter case were not optimal. In neither of these cases, though, did it seem that there was any threat of a run on high-powered money, and it may be that the advent of the FDIC has indeed removed the need for a lender of last resort. Further, a rule that fixes the growth rate of M1 would provide an element of built-in stabilization since increases in the demand for currency at the expense of demand deposits would be accommodated automatically. But the basic source of the instability that underlies a panic—the multiple expansion of credit—would not be removed by CGRR.

There is thus no certainty that panics would be avoided under CGRR and accordingly it is important that there be some agency in a position to deal with potential panics in the financial markets. The most natural agency for this purpose would be the Fed, which should have left open to it the possibility of discounting freely and/or conducting large-scale open market operations.

The second type of flexibility is that which permits the Fed to react to business cycle developments. The argument here would be that there might be business cycle developments to which the Fed should react and that the details are too subtle to spell out in legislation. If a rule were in operation, the Fed could ask the Congress for authorization to engage in extraordinary measures if the need were foreseen, but delays in the legislative process and uncertainty about its outcome might well exacerbate any underlying disturbance.

The loss of flexibility that a constant growth rate rule would imply for the Fed in dealing with run-of-the-mill small disturbances would probably not be any great loss; it would essentially be the end of fine tuning. But economic instability might be seriously worsened if the legislative process made it impossible for the Fed to react to a financial panic or to react in a situation, such as a deep recession, when action was clearly called for.

The Case for a Rule

The advantages of a rule are in large part the disadvantages of discretion. The alleged tendency of the Fed to undertake action that is too much and too late would be reduced by the introduction of CGRR, or any other rule, for the decision lag of discretionary policy would be avoided. Policies that reduce the money stock at a time when it should be increased—as during the Great Depression—would be avoided. The accountability of the Fed for its actions would be enhanced, since its task would be well defined. The record shows that CGRR would not have been much worse than actual monetary policy during the post–World War II period.

Another argument against discretion has recently been advanced by Kydland and Prescott (1977).[38] The Kydland and Prescott argument is essentially that the Fed always or usually has an incentive to change monetary policy (the argument is a general one that applies to any policy) once the private sector has committed itself to a set of plans based on given expectations of policy. For instance, to take a not irrelevant example, if the public has adjusted to a relatively low rate of inflation, it might be in the Fed's interest to accelerate the inflation rate, apparently improving the short-run situation.

If the Fed has discretionary power, it might sometimes face the incentive to exploit the short-run Phillips trade-off. By a similar token, it rarely seems a good time to reduce the inflation rate. But why should the Fed want to act in a way that invalidates the private sector's expectations. The typical argument is that the Fed reads the election returns and that it, discretely to be sure, does the bidding of the president. This argument implies the view, no longer novel, that political success can be bought by policy which is not in the public's real interest. (It also implies that the Fed can systematically affect output.) Although democracy is frequently invoked in the argument for rules, it is not clear what democracy requires in this case.

I believe there is in fact a conflict between the short- and long-run interests of the public in the political business cycle and that some weight should on that account be given to rules. But I would feel much easier about this argument for rules if I did not have the suspicion that it is a rationalization of the typical economist's belief (shared by the public) that inflation is a more serious problem than the revealed preference of the political process, or any serious economic analysis, suggests, and that inflation control has therefore to be imposed, if necessary by rule.

A Modified Constant Growth Rate Rule or MAP

Friedman (1960) made only modest claims for CGRR, namely, that it would prevent the Fed from making major mistakes. The serious draw-

38. A similar problem is examined in Calvo 1978. The remarkable feature of the Kydland-Prescott result is that it can apparently occur even if the policy authority is maximizing the expected utility of the representative individual, and if individual tastes are consistent through time.

back of a strict form of CGRR is the possibility that monetary policy will be immobilized precisely at a time when it is obviously useful.

The question that then arises is whether CGRR would not in practice be the best of all worlds, given the right of the Fed to ask for changes in the rule. There would then be CGRR in the ordinary course of events, and active monetary policy when circumstances warranted—which is precisely the modified activist policy described in section 4. However, given the delays of the legislative process, CGRR in practice could well be destabilizing,[39] particularly in the case of a financial panic.

A similar solution, which I favor, would leave the initiative for taking action with the Fed, but would maintain the presumption that in the ordinary course of events, monetary policy would be passive. Under such a solution, the Fed would be expected to maintain a constant growth rate rule and would be required to explain ex post (within some specified period) all deviations from the constant growth rate path to a congressional review panel.

This latter solution is very close to the current situation. It is beyond the scope of this paper, and my ability, to specify the legislative formula that would be required to make the Fed follow its targets more closely than it has since 1975. More Congressional supervision and more public explanation from the Fed of what it is doing are both to be welcomed in any event.

It is not clear to me whether the proposed policy is a rule or discretion. It is a rule in that it prescribes expected conduct for the monetary authority, but it leaves the Fed with sufficient discretion to take quick action if that is necessary.

6 Concluding Remarks

I will not repeat the summary of this paper, which is contained in the introduction. I want to make three final points. First, the purpose of the paper was to discuss the possibility of countercyclical, activist monetary policy in the light of developments in macroeconomics associated with rational expectations. Much of the paper was therefore devoted to the question of whether systematic monetary policy can have *any* real effects on output. Given the need to concentrate on that question, and the absence from the paper of a well-specified macro model, only the most general of policy prescriptions could be made.

Second, the reader will have been struck by the number of places in the paper at which it is asserted that there is no very strong evidence favoring one position over another. The only strong statement the evi-

39. Tax rates are not typically changed rapidly.

dence on adoption of a constant growth rate policy supports is that we do not know how such a policy would work. The conservative course is not to immobilize monetary policy when it might be useful in a recession or panic.

Third, the terms in which the argument is couched may seem unusual. But the general argument that is made for activist policy is not new. In Keynesian terms, the issue that is being discussed is whether "we should, in effect, have monetary management by the Trade Unions, aimed at full employment, instead of by the banking system" (Keynes 1936, p. 267). The answer given in this paper is that the central banking system rather than the private sector should provide monetary management.

Appendix: The Barro Output Equation

A typical Barro output equation, estimated from data in Barro (1978), over the sample period 1948–76 is:[40]

$$
\begin{aligned}
(1) \qquad \log y_t = {} & \underset{(0.016)}{5.98} + \underset{(0.23)}{1.03}\, DMR_t + \underset{(0.23)}{1.18}\, DMR_{t-1} \\
& + \underset{(0.24)}{0.49}\, DMR_{t-2} + \underset{(0.25)}{0.20}\, DMR_{t-3} \\
& + \underset{(0.11)}{0.55}\, MIL_t + \underset{(.0004)}{.035}\, t
\end{aligned}
$$

$$SER = 0.0168,\ SSR = .00622,\ DW = 1.81$$

In this equation, y is the level of real GNP, DMR is the unanticipated component of the growth in the money stock, MIL is a measure of the proportion of the prime age male labor force that has been drafted,[41] and t is time. If one adds the current and three lagged values of the actual growth rate of money to the regression (this is equivalent to including the anticipated component of the growth rate of money), the sum of squared residuals falls to .005872. An F-test indicates that the hypothesis that the anticipated component of money contributes to the explanation of the behavior of output, given the inclusion of the variables in (1), is not accepted.

Barro also estimates an equation in which the actual rather than unanticipated growth rates of money serve as regressors, and fails to accept the hypothesis that the coefficients on the anticipated and unanticipated growth rates are the same, for his sample period. I find that I do accept that hypothesis for the 1948–76 period, but the power of the test is very

40. This sample period was chosen because I later introduce a variable that was conveniently available only over these years.

41. Barro expresses some dissatisfaction over the inclusion of the MIL variable in the output equation.

weak. Further, there is really no good reason to have a null hypothesis that the coefficients on anticipated and unanticipated money are the same, since verticality of the long-run Phillips curve is inconsistent with that view.

As noted in the text, a more relevant question about Barro's results from the viewpoint of activist policy concerns the time interval over which "unanticipated" is defined. I have constructed a variable *2DMT* that is the anticipation, based on information available at the end of period $(t-2)$, of the growth rate of money in period t. The construction is straightforward insofar as the money rule depends on lagged growth rates of money. It also depends on the unemployment rate, for which I formed expectations using Barro's unemployment equation (1977*a*). Finally, the exogenous variables *FEDV*, *MIL*, and *MINW*[42] were assumed known with perfect foresight. As might be expected, the constructed variable is collinear with *DMR* (correlation coefficient of 0.65) and *DMR* lagged once (correlation coefficient of 0.82).[43] As might also be expected, the data are not able to tell us whether the two-period ahead unanticipated growth rate of money has significant independent effects on output. Adding the variable $(DM_t - 2DMT)$ to the Barro equation (1) reduces the sum of squared residuals from .00622 to .00547. If the current value of the *DMR* variable is then deleted from the regression, the sum of squared residuals rises only slightly, to .00553. Neither variable has a significant coefficient when both are included in the equation. We conclude that the data cannot tell us whether only one-year ahead or only two-year ahead errors in predicting money or both contribute to explaining the behavior of output.[44]

Comment Robert E. Hall

It is noteworthy that Lucas and Fischer have no disagreement about the rationality of expectations, in spite of their very different views about the conduct of macroeconomic policy. The largest point of disagreement is the fixity of prices in the short run. Fischer believes prices to be sufficiently rigid over a span of, say, two years to provide a fulcrum for monetary policy to move real output. Lucas is skeptical both on the existence of such a fulcrum in all but the shortest runs and on the wisdom of encour-

42. For definitions see Barro 1977*a*, 1978.

43. The sample period 1948–76 was used because (DM-2DMT) was available only over that period.

44. *F* tests are inconclusive: given the inclusion of the two-year forecast error, the hypothesis that the DMR variable is irrelevant to the explanation of output is accepted, and vice versa.

aging the monetary authorities to make use of it. Neither is dogmatic on the point. Fischer recognizes the weakness of the evidence supporting the hypothesis that active monetary policy can smooth real output in a desirable way, while Lucas concedes the possibility of effective policies of this kind as a matter of theory.

Many speakers at this conference have emphasized the inadequacy of current knowledge on the key question of price rigidity. Though Lucas was a great pioneer in trying to make sense of the hypothesis within standard economic theory, he does not try to develop his views any further here. Fischer points out that many relevant transactions take place under contracts. It is costly to make these contracts contingent on aggregate economic variables apart from consumer prices. But this line of argument seems to start from a presumption that the natural noncontingent contract predetermines prices (or wages) and lets the buyer determine quantity later in response to further information. If such contracts are common, aggregate supply will be highly price-elastic, and a relatively Keynesian set of conclusions and prescriptions will follow. There is no good reason for this type of contract to be the starting point, though. As far as I can see, most contracts for goods have the simple form of predetermining both quantity and price. Nobody argues that this kind of contract yields Keynesian conclusions, and it certainly involves no expensive contingencies. In the labor market, contracts do seem to permit quantity variations during the contract, but they do not predetermine the wage. Rather, compensation varies with employment along a schedule established in the contract. At the theoretical level, this problem is studied in an important paper by Calvo and Phelps (1977) and in a subsequent paper of mine with David Lilien (1979). My own empirical work (1974) has shown the importance of variations in wage rates during the term of a union contract. Obviously, much more work needs to be done on this important question.

Fischer indicates the importance of the issue of the way that monetary policy affects real output with a lag. If the lag of two years arises because prices are sticky over a two-year period, then the scope for useful monetary policy is enlarged. If it arises because a monetary surprise brings about a predictable shift in the economy's equilibrium level of output, a very different and probably less activist prescription for monetary policy emerges. Without further assumptions, that data cannot distinguish these two hypotheses (Sargent 1976*b*). In the empirical work reported here, Fischer's implicit identifying assumptions relate to the irrelevance of fiscal and other variables in predicting fluctuations in real output. As he discovers, even these assumptions are inadequate to make the distinction. The problem can be put in the following ways: The two-year-ahead forecast error for money incorporates new information available this year and new information available next year. Essentially (but not exactly) the same information goes into the current one-year-ahead forecast error and into next year's one-year-ahead error. The result is severe multicollinearity among these three variables and the inability to distinguish the two hypotheses.

Fischer presents an admirably cautious discussion of the aggregate supply function that underlies Barro's evidence of monetary nonneutrality. As he points out, there are two leading explanations, one of Lucas's and a Keynesian alternative. I share some of Fischer's misgivings about the relevance of Lucas's model for, say, the American economy. I would add that Lucas's critical assumptions of the unavailability of information about the aggregate economy seem particularly inappropriate. But my own work (1979) has suggested that the necessary amount of intertemporal substitutability of labor supply may actually be present. I reach a mixed, but generally negative, verdict about the application of Lucas's model to the behavior of the U.S. economy, just as Fischer does. But Fischer does not apply the same level of criticism to the Keynesian hypothesis of wage-price stickiness. There is a slight suggestion in the paper that the Keynesian hypothesis must be right to the extent that Lucas is wrong. To my mind, we lack so far any presentation of Keynesian ideas on the same level of rigor and clarity as Lucas's work. The model implicit in most Keynesian work says that prices and wages are sticky *and* that labor demand, not labor supply, determines employment (this is certainly true for the basic IS-LM model). In the Keynesian story, the labor market operates off the labor supply function, for reasons which so far have not been successfully explained. It is not enough just to invoke the practical reality that wages and prices are sticky. We need to explain why demand wins and supply loses in the contest to determine employment in the face of stickiness. Obviously I agree with Fischer's basic theme that we are far from understanding the sources of monetary nonneutrality.

On the policy issues discussed by Lucas and Fischer, my own views are not especially strong and I do not have too much to say. I have learned that policymakers do not listen to unsolicited advice from economists. Economists are invited to advise in two very different circumstances. The Federal Reserve and the White House ask for recommendations about what to do in the next few months. Here the economist who replies that the wrong approach to policy formulation is being taken and that a simple fixed policy rule should be instituted in its place is never taken seriously and is never asked back. At those meetings, it seems to me, the best we can do is suggest that negative policy surprises not occur in recessions nor positive surprises in booms. I might even go a little further and recommend a positive surprise in a recession.

The second opportunity is congressional consideration of changes in the rule of policymaking. Congress has come close to imposing a fixed monetary growth rule in the past and it might again. Then I confess some ambivalence about the desirability of a fixed growth rule for monetary policy. On the one hand, I find Fischer's description of good discretionary policy very attractive. No rule can remotely approach the flexibility of an intelligent, well-trained, and well-intentioned human being. If I thought Stan Fischer were going to make monetary policy unilaterally, I would happily endorse his approach to the conduct of policy. On the other hand, it seems clear to me that we would have been much better off under a fixed growth rule than under the kind of discretionary policy we have had under the Federal Reserve system. By and large, money growth seems to have accelerated in booms and slowed in recessions, though the facts apparently admit several interpretations. Fischer's discussion seems excessively optimistic about our potential for reversing the dismal record of past discretionary policy.

Comment Mark H. Willes

I suppose I have been asked to make some comments at this conference because, as a policymaker, I am a consumer of macroeconomic analysis. I cannot presume to discuss things at a sophisticated technical level. As a consumer, I feel the conference—specifically the two papers by Lucas and Fischer that I was asked to discuss—has provided several important contributions to the policy debate.

First, both of these papers highlight what seems to be a growing awareness that policymakers should not ask for policy solutions, because at the moment economists are not capable of providing them. It is true that Fischer says, and I think most of us would agree, that in principle there should be models available that can supply the policies we want. As he puts it, "In principle, the optimal monetary policy to be used for stabilization can be studied by using an appropriately specified macroeconometric model, which pays due attention to the effects of changes in policy regime on the structure of the model. Such models are not inherently impossible to build."

But his paper does not contain such a model, nor am I aware of any generally accepted model of that kind. In fact, the discussion of this conference points out to me, at least, the significant difficulties of both theory and estimation, which are yet to be overcome before such a policy model is in fact available. In the meantime, it seems to me that policymakers should not demand so much of policy advisors, and in return, policy advisors should not offer so much specific advice to policymakers. In this respect I find it encouraging that both Lucas and Fischer are properly humble and cautious in their policy prescriptions; Fischer, who comes out in favor of some "activist" policy, goes on to state that "fine tuning has to be cautious" and "fairly passive." Clearly, those are rather muted calls for action compared with some I have heard.

A second significant contribution to the policy debate, related to the first, that I see emerging from these two papers is a growing consensus that policymakers ought to be thinking not in terms of putting out fires but in terms of developing an acceptable and stable *process* or *rule* for setting monetary policy instruments.

Macroeconomists now seem to agree about what it would mean to have a *quantitative* solution to the problem of "making policy optimally," at least within the confines of a given institutional structure. First, it would be necessary to have in hand an econometric model (with actual numbers estimated for the parameters) that accurately describes how people would behave over an interesting range of alternative situations. Second, after the policymaker reveals his preference for alternative possible aggregate economic outcomes, determining policy becomes the (undoubtedly difficult) technical matter of deriving the "optimal control law" for the policy instruments that the authority controls. The control theory expert is clear in what he means by a "policy": it is a feedback rule or, in effect, an entire probability distribution for the government policy instrument, contingent on information that the authority will have in hand at the time that it must act. For a collection of mathematical equations to qualify as a model in the sense used above, it is necessary for it to describe peoples' economic behavior over the range of possible policies that the policymakers and their control theory experts want to consider.

Many of us in the late 1960s gave the impression that we possessed collections of equations (or soon would possess them) that qualified as models in this sense. Finding good methods for calculating the optimal feedback rules for those systems became an important topic.

As Fischer implies, however, it understates matters to say that the optimism of the late 1960s about the early successful completion of this research program has evaporated in recent years. Two related factors caused this. First, the best big models failed to predict important aspects of the 1970s, including unemployment-inflation interactions. Second, partly in response to the first event, the existing econometric systems could not be taken seriously as models of behavior that could be expected to hold up under a variety of hypothetical monetary and fiscal policies (feedback rules), as Lucas argued on theoretical grounds. As I understand it, Lucas's point was that for macroeconometric work it will just not do to formulate theories and econometric equations at a level that corresponds only to demand curves or supply curves. Formulating things at this level is too *shallow*, in the sense that economic theory predicts that peoples' dynamic demand curves and supply curves ought to change with a change in the nature of the environments that they face. But the changes in monetary and fiscal policy feedback rules necessarily occasion such changes in the environments that economic decision makers cope with. Therefore, their demand or supply curves, that is, their rules for setting decisions as functions of the things that they see, will change with a change in the policy rule.

One important negative implication of this argument of Lucas's is that big econometric systems in the style of the late 1960s cannot be regarded as *models* that will remain invariant under policy changes. This is because they consist only of collections of estimated demand curves and supply curves and nothing deeper. It follows that the systems of equations comprising most current econometric models are not suitable objects to hand over to a control theory expert for calculating the optimal rule. I find this argument of Lucas's compelling in its logic, even if it is disconcerting in implying that we are now much further from being able to promise a quantitative prescription for optimal monetary policy than we seemed to be ten or fifteen years ago.

An equally important positive element of Lucas's argument is his pointing the way to how macroeconometric work can be done in a manner designed to isolate those aspects of economic behavior that *will* remain invariant across different choices of policy rule. Put differently, the strategy must be to estimate objects that will enable us to predict how economic actors will change their dynamic demand curves and supply curves in response to changes in the random environments that they face. Ideally, this strategy involves rolling back what is estimated from the stage of demand and supply curves, to the stage of the parameters of the preference functions, production functions, and random elements that agents face. Then when agents are assumed to face new and different environments, predictions can be made about how their demand and supply curves will change. All of this is much easier said than done. Work along these lines is in its infancy and involves a number of difficult econometric and theoretical problems. Serious work along this line is being done at the Federal Reserve Bank of Minneapolis and elsewhere. But it is my understanding that we are a very long way from having a quantitative, empirically verified econometric model of the economy that meets the standards that have been delineated. Nevertheless, I have been encouraged by what I sense is a rather widely held view, at least by participants in this conference, of the need to build models in a different way from in the past and with emphasis on policy *rules*, rather than on ad hoc policy advice to meet short-run economic problems.

One final point flows from what I have said so far. Fischer seems to suggest that devotion to rational expectations implies that its adherents forswear the use of countercyclical stabilization policy. Coming from one of the hotbeds of rational expectations, may I say that such a conclusion is not required in principle. For example, as I understand it, Sargent and Wallace (1975) did not argue that *no* economic models could be imagined in which effective systematic countercyclical policy was feasible; rather, I understand the point of that paper to be that within the context of the simple model that they studied, it mattered a great deal for the choice of the policy rule whether expectations are assumed to be rational instead of being fixed in the face of alternative choices of rules. That main implication of their results would also characterize models modified to incorporate the various nonneutralities catalogued by Fischer. The potential existence of such nonneutralities has at least arguable implications for the present policy choice if one simultaneously admits that the currently available macroeconometric models cannot be used to analyze their quantitative importance. Even if one subscribes to some or all of the nonneutralities listed by Fischer, his declaration that "The rational expectations theory . . . has become and will remain the leading theory of expectations" in effect concedes that we are presently without a model for analyzing their quantitative dimensions and policy implications. Consequently, until such models are available, it would seem that the burden of proof might well rest on those who advocate activist policy intervention, rather than on those of us who argue for a rather steady policy course.

Comment Peter Howitt

Although the conference revealed many important points of disagreement among the participants, I believe there are some important points of potential agreement that were not brought out in the discussion. The purpose of this note is to highlight some of these points.

The first point is that whatever course of action is pursued by the monetary authorities, it should be announced as clearly as possible and as soon as it is conceived. On the one hand, this point should certainly be acceptable to those who argue that announced monetary changes can have no real effects. According to this view the sooner and the more clearly are any given monetary changes announced the less potential harm they do. On the other hand the point should also be acceptable to those who believe that even announced monetary changes can have real effects. According to the most extreme version of this view, monetary actions will have the same real effects whether an-

EDITOR'S NOTE: This comment was submitted after the conference closed; it is included here because its contents are most closely related to the discussion of the Lucas and Fischer papers.

nounced or not, so that the policy of announcing any monetary changes, while it won't do any good, at least won't do harm either. The less extreme version, according to which expectations are formed rationally but money prices are constrained in the short run by the existence of long-term contracts, implies that this policy of announcing should improve economic welfare, because contracts signed after an announcement but before the corresponding policy change would otherwise have been inferred by agents can incorporate more accurate information as a result of the announcement, whereas other contracts will be unaffected.

Acceptance of this point of agreement implies acceptance of an even more important one—namely, that in response to any clearly recognized deflationary shock in aggregate demand the money supply should be increased above what it would otherwise have been. This may appear to be a contentious point that could be accepted only by an activist who denied the hypotheses of rational expectations and/or the Lucas aggregate supply function. But even to an advocate of these hypotheses the point is at worst innocuous. For according to his view, as long as the monetary change is announced, it can have no real effects; it does no good but it does no harm either. Indeed, if some weight is given to price stability as a goal, then even this extreme view implies that the policy, as long as it does not *over*react, will be positively beneficial.

This last point is even consistent with believing in rules rather than discretion, as long as the rule allows for feedback loops. It says that the rule ought to adapt to clearly identifiable deflationary disturbances.

There may be some doubt raised about whether or not such disturbances exist. I believe that this existence question is answered affirmatively by the single example of the events of late 1929 and early 1930. No one doubts that aggregate demand declined in this period in a way that could have been recognized in time to prevent the monetary collapse following October 1930, and not even Friedman and Schwartz argue that the decline was entirely attributable to monetary policy itself.

Some doubt may also be raised about how much of a reaction there should be. This depends upon one's estimate of the relevant parameters and the size of the shock. But the point is that in the face of such a deflationary shock it is hard to imagine how anyone's guess about the best monetary reaction could be other than positive. In any such situation there should be some positive reaction in the money supply that all among a finite number of economists agree is better than nothing.

Advocates of the rational expectations equilibrium approach may not appreciate being placed upon this common ground of agreement. But it seems to be implied by the logic of their argument. In my view this reflects a previously unseen aspect of the Sargent-Wallace proposition. The argument that systematic monetary policy is useless for affecting output also implies that it can do no harm to output. Thus while it warns of the potential limitations of activism, at the same time it strengthens the case for activism by showing how to avoid the potential dangers of which the rational expectations argument has warned, namely, by making sure that all monetary changes are announced.

General Discussion

In response to Hall's comment that in most labor contracts firms had to choose labor input at a wage that varies with the input level, Fischer said that the crucial issue was whether the overtime wage was based only on the amount of work, as he believed, or else changed depending also on the macroeconomic disturbance affecting the economy.

Lucas commented that there was confusion over what is required to make a case for activism. Such a case requires an argument to the effect that enough is known of the workings of the actual economy to permit successful activist policy, as opposed to purely hypothetical examples of economies in which activist policies might be successful.

Alan Blinder claimed that policy had not always been bad. Taxes were cut in 1964–65 and raised in 1968, as they should have been, though perhaps the tax increase had been delayed too long. Barro's reaction function, in which the Fed increased the growth rate of money when unemployment rose, showed that the Fed followed the tenets of good monetary policy as outlined in Robert Hall's remarks. Finally he noted that real output has been much more stable since World War II than it was before, and this was supposedly the period in which policymakers had followed Keynesian policies.

William Poole argued that there is an overwhelming case that a stable money growth policy would have been superior to the policies we have had. The one exception he saw to the optimality of stable money growth was that the central bank should intervene in liquidity and financial crises such as the Penn Central and Franklin National episodes. The overall goal of the central bank should be to avoid doing damage.

Herschel Grossman commented first on Fischer's paper. He presented two reasons why unanticipated money might affect output: (1) utilizing information is costly, in which case there is room for systematic monetary policy to have an effect, and (2) current information is noisy, in which case it does not follow that systematic monetary policy can have real effects.

Turning to Lucas's paper, Grossman remarked that while Lucas felt that we did not at present know enough to develop rules better than the Friedman rule, he himself doubted we would ever know enough to do so.

He was risk averse and was satisfied that the Friedman rule would at least prevent catastrophe. He did wonder whether there would be a transitional problem in moving to a constant growth rate policy.

Robert Gordon asked what basis Lucas had for the recommendation of balancing the budget on average, in contrast to Martin Bailey's conclusion that the optimal budget surplus could be positive or negative. He also commented that it would be useful to look at other countries, such as Germany, in studying labor contracts.

Robert Solow addressed himself to the political theory of fixed rules: Congress cannot legislate a permanent money rule because the next Congress may amend or repeal the legislation. It was only people present in the room who thought the money supply was (in a manner of speaking) the most important thing in the world and that it should be determined by fixed rules. If constitutional amendment were suggested to implement a money rule, we would be creating the precedent for constitutional rules for foreign policy, taxes, tariffs, and other matters that are at present adequately handled by legislation.

Solow added that modesty and caution in the making of policy did not necessarily imply sweeping changes such as a constant growth rate rule for money.

Karl Brunner said that the type of statement made by Solow should not be made without reading Buchanan and Tullock on the behavior of politicians and bureaucrats. He argued that it was perfectly legitimate to ask what the role of the government should be and what governmental actions should be constrained by constitutional rules.

Frank Morris said that the disarray of policymakers at present mirrored the disarray of economists; policy had currently to operate in a theory vacuum. He agreed with Hall that the rate of growth of money should be increased in recessions and decreased in booms. He thought that monetary policy rules make considerable sense in a stable environment, but not during an unstable period.

Phillip Cagan remarked that as a practical matter there was little difference between a nonactivist and mildly discretionary policy. To achieve a constant growth policy from where we were at present, he thought the best way was to slowly decelerate. Although this would result in a period of slack, when it came to be realized that policy had definitely changed, expectations would adjust and the economy would move more readily toward full employment with reduced inflation. He was not sure, however, that the initial period of slack would be politically acceptable.

Jerry Green discussed the nonexistence of full contingent contracts. He argued that if people are rational enough to form rational expectations, they should be able to write contingent contracts against major disturbances, but he doubted whether the welfare gains from these contracts could be sufficient to provide the incentive to write them.

References

Bagehot, W. 1906. *Lombard Street.* London: Kegan Paul.

Barro, R. J. 1976. "Rational Expectations and the Role of Monetary Policy." *Journal of Monetary Economics* 2:1–32.

———. 1977*a*. "Unanticipated Money Growth and Unemployment in the United States." *American Economic Review* 67:101–15.

———. 1977*b*. "Long Term Contracting, Sticky Prices, and Monetary Policy." *Journal of Monetary Economics* 3:305–16.

———. 1978. "Unanticipated Money, Output, and the Price Level in the United States." *Journal of Political Economy* 86:549–80.

Brock, W. A. 1974. "Money and Growth: The Case of Long-Run Perfect Foresight." *International Economic Review* 15:750–77.

Bryant, J., and Wallace, N. 1978. "Open Market Operations in a Model of Regulated, Insured Intermediaries." Research Department Staff Report no. 34. Minneapolis: Federal Reserve Bank of Minneapolis.

Bulow, J., and Polemarchakis, H. 1978. "Retroactive Money." Unpublished.

Calvo, G. 1978. "On the Time Consistency of Optimal Policy in a Monetary Economy." *Econometrica* 46:1411–28.

Calvo, G., and Phelps, E. S. 1977. "Appendix: Employment-Contingent Wage Contracts." In *Stabilization of the Domestic and International Economy*, edited by K. Brunner and A. H. Meltzer, pp. 160–68. Carnegie-Rochester Conference Series on Public Policy, vol. 5. New York: North-Holland.

Drazen, A. 1976. "Essays on the Theory of Inflation." Ph.D. diss., MIT.

Eckstein, O. 1978. *The Great Recession.* Amsterdam: North-Holland.

Feldstein, M., and Summers, L. 1978. "Inflation, Tax Rules, and the Long-Term Interest Rate." *Brookings Papers on Economic Activity* 1:61–99.

Fischer, S. 1977. "Long-Term Contracts, Rational Expectations, and the Optimal Money Supply Rule." *Journal of Political Economy* 85:191–205.

———. 1979*a*. "Capital Accumulation on the Transition Path in a Monetary Optimizing Model." *Econometrica*, forthcoming.

———. 1979*b*. "Anticipations and the Non-Neutrality of Money." *Journal of Political Economy* 87:225–52.

Fischer, S., and Modigliani, F. 1979. "Towards an Understanding of the Real Effects and Costs of Inflation." *Weltwirtschaftliches Archiv* 114:810–32.

Fisher, I. 1922. *The Purchasing Power of Money.* New York: Macmillan.

Friedman, M. 1960. *A Program for Monetary Stability.* New York: Fordham University Press.

———. 1969. "The Optimum Quantity of Money." In *The Optimum Quantity of Money*. Chicago: Aldine.

Friedman, M., and Schwartz, A. J. 1963. *A Monetary History of the United States, 1867–1960*. Princeton, N.J.: Princeton University Press.

Germany, J. D. 1978. "Can the Effects of Unanticipated Policy Be Estimated?" Unpublished.

Ghez, G., and Becker, G. 1975. *The Allocation of Time and Goods over the Life Cycle*. New York: Columbia University Press.

Hall, R. E. 1974. "The Process of Inflation in the Labor Market." *Brookings Papers on Economic Activity* 2:343–93.

———. 1979. "Labor Supply and Aggregate Fluctuations." Carnegie-Rochester Conference.

Hall, R. E., and Lilien, D. 1979. "Efficient Wage Bargains under Uncertain Supply and Demand." *American Economic Review*, forthcoming.

Keynes, J. M. 1936. *The General Theory of Employment, Interest and Money*. London: Macmillan.

Kydland, F. E., and Prescott, E. C. 1977. "Rules rather than Discretion: The Inconsistency of Optimal Plans." *Journal of Political Economy* 85:473–91.

Lucas, R. E., Jr. 1973. "Some International Evidence on Output-Inflation Tradeoffs." *American Economic Review* 63:326–34.

———. 1976. "Econometric Policy Evaluation: A Critique." In *The Phillips Curve and Labor Markets*, edited by K. Brunner and A. H. Meltzer, pp. 19–46. Carnegie-Rochester Conference Series no. 1. New York: North-Holland.

———. 1977. "Understanding Business Cycles. In *Stabilization of the Domestic and International Economy,* edited by K. Brunner and A. H. Meltzer, pp. 7–29. Carnegie-Rochester Conference Series on Public Policy, vol. 5. New York: North-Holland.

Lucas, R. E., Jr., and Rapping, L. A. 1969. "Real Wages, Employment and the Price Level." *Journal of Political Economy* 77:721–54.

Modigliani, F. 1974. "A Critique of Past and Prospective Monetary Policies." *American Economic Review* 64:544–57.

———. 1977. "The Monetarist Controversy, or Should We Forsake Stabilization Policies?" *American Economic Review* 67:1–19.

Modigliani, F., and Lessard, D., eds. 1975. *New Mortgage Designs for Stable Housing in an Inflationary Environment*. Boston: Federal Reserve Bank of Boston.

Phelps, E. S. 1970. "Money Wage Dynamics and Labor Market Equilibrium." In *Microeconomic Foundations of Employment and Inflation Theory*, edited by E. S. Phelps. New York: W. W. Norton.

———. 1973. "Inflation in the Theory of Public Finance." *Swedish Journal of Economics* 75:67–82.

Poole, W. 1976. "Rational Expectations in the Macro Model." *Brookings Papers on Economic Activity* 2:463–514.

Sargent, T. J. 1976*a*. "A Classical Macroeconomic Model for the United States." *Journal of Political Economy* 84:207–37.

———. 1976*b*. "The Observational Equivalence of Natural and Unnatural Rate Theories of Macroeconomics." *Journal of Political Economy* 8:631–40.

Sargent, T. J., and Wallace, N. 1975. " 'Rational' Expectations, the Optimal Monetary Instrument, and the Optimal Money Supply Rule." *Journal of Political Economy* 83:241–54.

Sayers, R. S. 1957. *Central Banking after Bagehot*. London: Oxford University Press.

Sidrauski, M. 1967. "Rational Choice and Patterns of Growth in a Monetary Economy." *American Economic Review Proceedings* 57:534–44.

Simons, H. C. 1952. "Rules versus Authorities in Monetary Policy." In *Readings in Monetary Theory*. London: George Allen & Unwin.

Small, D. N. 1978. "The Endogeneity of Rational Expectations and Labor Supply." Unpublished.

Startz, R. 1978. "Interest Bearing Demand Deposits." Ph.D. diss., MIT.

Taylor, J. B. 1979. "Aggregate Dynamics and Staggered Contracts." *Journal of Political Economy*, forthcoming.