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3 Nonstandard Indicators for Monetary Policy: Can Their Usefulness Be Judged from Forecasting Regressions?

Michael Woodford

In recent years there has been a great deal of interest in proposals to use nonstandard indicator variables as a guide to the conduct of monetary policy. By nonstandard indicators I mean indicators other than the various measures of the money supply that the Federal Reserve System computes, and to which academic economists in the monetarist tradition have long directed their attention, and other than the variables that can be more or less directly controlled by the Fed, such as borrowed and nonborrowed reserves or the federal funds rate, and measures of the Fed's success at achieving its ultimate objectives, such as measures of inflation and economic activity in the recent past.

Some of the new indicators that have been discussed include commodity price indexes, nominal exchange rates, and spreads between the interest yields on longer- and shorter-maturity Treasury securities. Interest in the new indicators seems mainly to have been a response to the perceived instability in the 1980s of the relations between traditional monetary aggregates and nominal aggregate demand. The various new proposals just mentioned all represent variations upon the idea that a desirable monetary policy, that would be able to respond to, and counteract, incipient inflationary pressures before much inflation had developed, could be conducted by monitoring various indicators that are known to be valuable as *forecasts of future inflation,* rather than by giving one's sole attention to the evolution of variables such as the money supply that are thought to be *proximate causes of inflation.*

I do not attempt here to evaluate the likely consequences of any of these specific proposals. Instead, the present note addresses a general issue raised by proposals of this general type, that of how to determine which variables, of all

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the many types of data available to the Fed, are reasonable indicators to use as the basis for a feedback rule for one or another instrument of monetary policy. The discussion of the new indicators has tended to assume that one should simply look for any or all available variables that have proved to be useful in forecasting inflation over some relevant horizon. The advocates of the new variables within the Federal Reserve System have stressed their usefulness as leading indicators of inflation, and much of the academic commentary has addressed itself to formal econometric evaluation of their forecasting ability, typically within a completely atheoretical vector autoregression (VAR) framework.

The question I consider here is whether analysis of this sort is a sufficient ground for choosing variables to be used in making monetary policy. I will argue that such investigations are no substitute for an analysis of the consequences of making policy on the basis of one indicator or another in the context of a specific structural model of the economy, which models both the determination of the indicator in question and the effects of possible monetary policy interventions. Before developing this point further, however, I first review from a purely statistical point of view the literature that has addressed the issue.

3.1 Econometric Evaluations of the Usefulness of Commodity Prices as an Indicator

The type of nonstandard indicator most often discussed has been one index or another of commodity prices. The recent interest in commodities prices began with the revival of interest among "supply-side economists," during the first Reagan administration, in the stabilization of commodities prices (including, perhaps, a return to a gold standard) as a goal of monetary policy, as an alternative to the monetarist program of controlling the growth rate of a monetary aggregate. For example, Reynolds (1982) advocated stabilizing spot commodity prices, especially the price of gold. Miles (1984) discussed a variety of possible types of "price rules," including the stabilization of the spot prices of gold or other commodities, eventually coming down in favor of a rule that would involve stabilization of commodity futures prices, as well as stabilization of long-term bond yields. Others argued for the use of commodity prices as an indicator if not as a target variable; for example, Genetski (1982) argued for making the range of permissible growth rates for the monetary base change automatically in response to movements of a commodity price index. These proposals received some attention among policymakers, especially in 1982, when the United States suffered from a severe recession that was widely attributed to the Fed's attempt to bring money growth under control. At that time, Representatives Jack Kemp and Trent Lott introduced into Congress the Balanced Monetary Policy and Price Stability Act, which would have required the Fed to pursue "price stability" over all other objectives, with price stability to be measured by one of two possible indexes of commodity prices. While the primary goal of such measures was obviously stability of the general price level, some proponents also argued that basing monetary policy on commodities prices would stabilize interest rates and economic activity as well; Genetski (1982) argued that such a rule would have enabled the United States to avoid all major recessions since 1915.

Proposals to actually stabilize commodities prices soon fell out of favor, in the face of skepticism about the feasibility of commodity price stabilization by the Fed (see, e.g., Hafer 1983), and about whether stabilization of commodities prices would in fact imply stability of prices more generally. Because of the high volatility of gold and other commodity prices, relative to the volatility of general price indexes like the consumer price index (CPI), many argued that commodities prices evidently move in response to many factors other than the true current stance of monetary policy—not only factors affecting the supply of and demand for individual commodities, which ought to affect their relative prices, but incorrect perceptions perhaps on the part of market participants about what the current policy stance is—so that the concern with stabilizing these particular prices might actually make monetary policy, and prices generally, more volatile. (See, e.g., Bordo testimony in U.S. House 1987 and DeFina 1988.)

Nonetheless, in the late 1980s commodity prices were again widely discussed as a desirable basis for monetary policy, this time within the Federal Reserve itself, and this time with an emphasis on their usefulness as indicators of inflationary pressures, rather than as a target variable. Federal Reserve governor Wayne D. Angell (1987) proposed that a commodity price guide be used to adjust short-run money-growth target ranges, and Governor H. Robert Heller (1987) endorsed such a rule as a way of bringing about not only domestic price stability, but greater exchange-rate stability as well. Vice Chairman Manuel H. Johnson (1988) suggested that not only commodity prices, but interest-rate spreads and nominal exchange rates, should serve as information variables to evaluate the current stance of monetary policy. The general idea behind these proposals is clearly expressed by Johnson: "Changes in monetary policy should be reflected in these financial auction market prices well before they affect the broader price measures. Thus, there is reason to believe that they may give advance warning of impending change for important concerns such as inflation." A similar idea lies behind the more recent proposal by Robert Hetzel (1990, 1992) that the Fed create a market for a new type of financial instrument, an indexed bond, so that the spread between the yield on this bond and the yield on a nominal bond of similar maturity can be used as an indicator of inflationary expectations, and hence as a guide to monetary policy.[1]

This second generation of proposals for the use of nonstandard indicators is

1. Goodfriend (1993), in an account of the actual conduct of Fed policy during the 1980s, argues that the term structure has in fact been an important source of information about inflationary expectations and an important determinant of changes in the stance of monetary policy.

explicitly based upon the statistical claim that these variables serve as leading indicators of inflation; for example, Angell (1987) presented evidence that over a long historical period peaks and troughs in indexes of commodity prices have preceded peaks and troughs in consumer price inflation. As a result, a considerable scholarly literature evaluating these proposals has given particular attention to econometric tests of the extent to which the proposed indicator variables do in fact forecast inflation—generally in a completely atheoretical way.[2]

Several types of econometric analyses of the relation between commodity prices and inflation have been undertaken.[3] One popular test examines the cointegration of commodity price indexes with general price indexes such as the CPI. For example, Garner (1989) tests for cointegration between several commodity price indexes and the CPI, and concludes that none are cointegrated with it. That is, not only is the real price of commodities (the commodity price index deflated by the CPI) a nonstationary variable, but no linear combination of the logarithms of the two price indexes is found to be a stationary variable, so that independent factors evidently determine the long-run behavior of the two series. He argues on this ground that commodity prices are not useful as an intermediate target for monetary policy, as stabilization of commodity prices would not imply stabilization of consumer prices. Sephton (1991) extends the cointegration analysis and confirms Garner's conclusions. Baillie (1989) reports similar results and draws a similar conclusion. Boughton and Branson (1991) test for cointegration between various commodity price indexes and an aggregate of consumer prices in seven industrial countries, and also find no cointegration.

On the other hand, a number of authors have pointed out that the fact that commodity prices and consumer prices do not move together in the long run does not exclude commodity prices from playing a useful role in improving forecasts of consumer price inflation. Garner tests for Granger causality of CPI inflation by several commodity price indexes, and finds that a null hypothesis of no causality can be statistically rejected; that is, that future CPI inflation can be somewhat better forecasted when the commodity price series is included in one's vector autoregression, than when that variable is excluded. He also uses his (nonstructural) VAR model to decompose the variance in CPI inflation into

2. An exception is the study of Cody and Mills (1990), which uses a "structural" vector autoregression approach. I do not discuss this study here, as an evaluation of the particular identifying assumptions proposed by Cody and Mills is beyond the scope of this note. A number of authors have also sought to evaluate the usefulness of nonstandard indicators through policy simulations in the context of structural models, as I advocate here. See, e.g., Fuhrer and Moore (1992), Porter (1990), and Brayton and Tinsley (1991).

3. I emphasize the literature on commodity prices here, because this literature is explicitly motivated by the monetary policy proposals just mentioned. A considerable literature has also examined the relation between the term structure and inflation, for example, but this literature is not primarily motivated by the suggestion that the term structure should be a guide to monetary policy; on the whole, it seeks rather to test particular theories of the term structure that emphasize variations in inflationary expectations as a source of changes in the term structure.

fractions attributable to innovations in each of the several variables included in the regression, and concludes that innovations in a commodity price index "explain about 25% of the prediction error variance for the CPI after 48 months," even when the monetary base is included in the VAR as another of the forecasting variables. As a result, he concludes that "the empirical evidence suggests that an index of commodity prices may be a useful information variable for policymakers," though he argues that it is not a suitable intermediate target because of the absence of cointegration. Sephton confirms the results of Garner with respect to Granger causality as well. Whitt (1988) similarly finds that commodity prices significantly improve forecasts of inflation.

On the other hand, a number of authors have criticized the conclusion from a finding of Granger causality alone that commodity prices are useful indicators for monetary policy. Webb (1988) finds that a commodity price index enters *significantly* in forecasting regressions for inflation, but that the *magnitude* of the improvement in forecastability is very small. He therefore concludes: "That commodity prices added a *small* amount of predictive power suggests that a *small* improvement in anti-inflation policy could be achieved by using them as an indicator variable." Aguais, DeAngelis, and Wyss (1988) find that commodity prices provide little additional information about future inflation, once wages and measures of supply conditions are also included as regressors, and Baillie (1989) and Barsky (1993) report similar results. Boughton and Branson (1991), in their study of the relation between commodity prices and industrial-country inflation, also report a finding of Granger causality. However, they find that while including the commodity price index in their VAR substantially improves the *within-sample* fit of their model of inflation, it results in no improvement in *post-sample* inflation forecasts. They thus conclude that "the quantitative linkages between commodity and consumer prices are significant, but are not stable enough to permit one to draw quantitative inferences about the extent to which consumer prices might respond to a given change in commodity prices."

I do not propose here to settle the issue of the nature of the statistical relationship between commodity prices and inflation, but rather to address an issue of method raised by this literature, and by studies such as that of Barsky (1993) that appraise other proposed indicator variables using similar methods. This is the question of whether a simple measurement of the extent to which an indicator has been associated in the past with subsequent inflation—even supposing that purely statistical issues such as the stability of the relationship over time can be satisfactorily addressed—suffices to determine the appropriate use that should be made of such an indicator in the formulation of monetary policy.

To be sure, careful econometric analysis of the extent to which a given indicator does in fact forecast inflation provides a better basis for policy discussions than mere impressions gleaned from the financial press. Some of the early enthusiasts of a monetary policy rule based on commodity prices seemed to think that the mere fact that commodity prices are observed to be *volatile* suggests their importance as a source of information,[4] and certainly regression analysis quickly exposes the fallacy in such reasoning. Still, the econometric literature often seems to regard as obvious the fact that policy rules can be directly formulated on the basis of the kinds of nonstructural regression relationships that are estimated. Even when the authors are critical of the proposed policy rules, they write as if it *would* make sense to stabilize commodity prices if they *were* found to be cointegrated with the CPI, or to adjust the stance of monetary policy in response to a commodity price index if its marginal forecasting power were found to be larger than it happens to be in the data examined. This is what I wish to challenge.

It might seem that shifting the terms of the discussion of desirable indicators for monetary policy, from consideration of what causes inflation to a simple consideration of how to forecast it, would have great advantages. One might argue that the monetarist case for tracking the money supply was based upon an assertion that money growth forecasts future inflation; however appealing the theoretical case for such a link, one can test econometrically whether other variables might not allow inflation to be forecasted even more accurately, and if so, it might seem that one should base monetary policy upon those other variables instead. Furthermore, it is a more straightforward matter to settle which variables have had what degree of forecasting power than to show conclusively that certain causal mechanisms have been operative.

I wish to argue that these advantages are more illusory than real. Despite the greater difficulty of deciding what a correctly specified structural model for the analysis of the effects of monetary policy should look like, we have no alternative but to undertake such an inquiry. In particular, supposing that one finds that a particular indicator *has* been reliably associated with subsequent inflation, I wish to argue that it matters a great deal whether the association exists because (*a*) the indicator is itself a measure of, or is directly influenced by, the underlying *causes* of inflation, and will indicate those inflationary pressures regardless of whether market participants understand them and regardless of whether the inflation is in fact allowed to develop; or (*b*) the indicator is influenced by market participants' *expectations* of inflation, and so responds to the underlying causes of inflation, but only insofar as market participants are aware of them and actually expect inflation to result. The basic idea behind the interest in nonstandard indicators has been a suggestion that indicators that work for the second reason are actually the *better* guides for monetary policy; but I wish to argue exactly the opposite.

There are several reasons why a conclusion about the right type of feedback rule for monetary policy on the basis of atheoretical forecasting regressions alone can be highly misleading. I take them up in sequence.

4. For example, Reynolds (1982) stresses the importance of controlling a "sensitive measure of price," and writes: "Since broader price indexes are too insensitive, what about narrowing the list to only one commodity—namely, gold—that is notoriously sensitive to every whiff of inflation or deflation?"

3.2 Low Forecasting Power May Not Justify Ignoring an Indicator

First, the mere fact that an indicator is found not to enter significantly in an inflation forecasting regression does not necessarily mean that the Fed should be advised not to pay any attention to that variable. For the absence of forecasting power might simply mean that the variable is already being used by the Fed in making policy, and in approximately the right way, from the point of view of minimizing the variability of inflation.

The point can be illustrated by the following extremely stylized model. Suppose that inflation over the relevant horizon is determined by a relation of the form

$$\pi_{t+1} = s_t + u_t + \varepsilon_{t+1}, \tag{1}$$

where π_t is inflation between dates t and $t + 1$, s_t is an indicator observed at date t, u_t is a control variable of the monetary authority chosen at date t, and ε_{t+1} is a mean-zero random variable not forecastable at date t. Equation (1) is intended to represent a causal effect of u_t on the probability distribution of possible values for π_{t+1} which is understood by the monetary authority; for simplicity, this effect is assumed to be a simple shift in the conditional mean, and the size of the effect is assumed to be independent of the value of s_t that is observed. The appearance of s_t on the right-hand side of (1) need not have a causal interpretation; s_t may simply be correlated with factors that influence inflation independently of the control variable. The realizations of s_t and ε_{t+1} are assumed to be independent of the choice of u_t, and the realization of ε_{t+1} likewise independent of s_t.

It follows that the variance of π_{t+1} is minimized by a policy feedback rule of the form $u_t = -s_t$. If such a rule is followed, $\pi_{t+1} = \varepsilon_{t+1}$, and a regression of π_{t+1} on s_t will yield (asymptotically) a zero coefficient. But it would be incorrect in such a case to tell the monetary authority to stop monitoring the value of s_t before choosing the value of u_t. Since many of the indicators the recent literature is concerned with have been argued to be useful by officers of the Fed, one can hardly be certain that the Fed's policy actions during the period from which one's data are drawn did not respond to these variables; and in fact Goodfriend (1993) argues that Fed policy for at least the last decade has been guided to a great extent by the movements of an interest-rate spread. Hence the finding of low incremental forecasting power for some of these variables might be due to the way in which they are already being used. Without an attempt to understand the data in terms of a structural model (which would include a model of the Fed's policy rule), it is hard to reach a conclusion about this.

One might argue that in any event a finding of insignificant forecasting power for a given indicator allows one to make the recommendation that policy should respond to that variable to exactly the extent that it already does—neither more nor less. But this is not a particularly useful sort of recommendation. In the absence of a determination of what the Fed's current policy rule is (or has been in the period under study), it would not be clear what sort of response had been shown to be desirable. (The Fed itself might not know what it would mean to "maintain the status quo"; would a new Board member be required to adopt the opinions of whatever member he or she had just replaced in order to maintain some balance of forces?) Also, if one were recommending that *some* indicators could be used to reduce inflation variability relative to past policies, it would not be clear that the optimal response to the other variable (that is believed to have been used optimally in the past) would be unchanged if the use of the former variables were changed.

3.3 Pitfalls in Basing Policy upon Indicators That Have Forecasted Inflation in the Past

On the other hand, the mere fact that an indicator is found to be useful in forecasting inflation does not tell much about the desirability of a policy that involves feedback from that indicator. There are several reasons why such an inference requires caution. One is that the ability of the indicator to signal the underlying sources of inflationary pressures that one wants to respond to may be impaired *by the very fact that the monetary authority responds to it.*

This point is, of course, simply a variant of the by now familiar Lucas (1976) critique of econometric policy evaluation. The Lucas critique is perhaps sometimes invoked in too sweeping a way, to discredit all attempts at econometric policy evaluation as such (an issue to which I return below). However, the point seems particularly likely to be an important one in the present context. This is because the nontraditional indicators with which the recent literature is concerned are clearly *not* believed to be useful for forecasting inflation because of being causal determinants of inflation. In fact, they are not even believed to be proxies for such causal determinants (because, say, of a direct causal effect of the underlying sources of inflationary pressure on the indicators); instead, they are believed to be of interest because of being strongly affected by (and hence signaling) the state of inflationary expectations. But if the connection between the underlying sources of inflationary pressure (that one wishes to respond to) and the indicator is mediated primarily by expectations, one is in the situation where it is most plausible that the relation should radically change in the case of a policy intervention that modifies the relation between the underlying states and the inflation that eventually occurs.

The point may be illustrated by a simple example. (This admittedly has not been proposed by any of the advocates of nonstandard indicators, but is in the spirit of some of those proposals.) If the best indicator for monetary policy is simply the variable that best forecasts future inflation, why not simply use published inflation forecasts as the basis for policy? I expect that if one were to include among the regressors some kind of consensus forecast of inflation in forecasting regressions like those in Barsky (1993), one would find not only that it had marginal significance, but that there was little marginal significance for the other variables once the forecast was included in the regression.

But should the Fed then be advised to simply respond to the current consensus forecast of inflation, to the exclusion of all other available information? Such a proposal could have paradoxical effects. The use of the forecasts in setting policy would change the relation between the forecasters' information variables and inflation, and so would lead them to change the way they form their forecasts. But once they did, the relation between their forecasts and the underlying sources of inflationary pressure would change, so that the Fed's optimal response to the forecast would change. And if the Fed changes its response, as it ought to, this again changes the way in which forecasters ought to form their forecasts. This process of adjustment between the two sides need not admit any equilibrium.

A simple example shows why. Let (1) again describe the determination of inflation, but now suppose that the variable s_t is not observed by the monetary authority. Instead, suppose that s_t is observed by a private forecaster, and that the authority observes the forecast f_t. Finally, suppose that the forecaster seeks to minimize the expected squared forecast error $(\pi_{t+1} - f_t)^2$. It follows that f_t will equal the conditional expectation of π_{t+1}, given the forecaster's observation of s_t. If the monetary authority uses a feedback rule of the form

$$u_t = -\lambda f_t, \tag{2}$$

then optimal use of the forecaster's information requires that the forecast be

$$f_t = \alpha s_t, \tag{3}$$

where $\alpha = \alpha(\lambda) \equiv (1 + \lambda)^{-1}$. On the other hand, if the forecaster uses any rule of the form (3), the monetary authority's optimal feedback rule (in order to minimize the variance of π_{t+1}, given its observation of f_t but not of s_t) is of the form (2), where $\lambda = \lambda(\alpha) \equiv \alpha^{-1}$. However, the curves $\alpha = \alpha(\lambda)$ and $\lambda = \lambda(\alpha)$ never intersect, for any values of α and λ.[5]

This result is reminiscent, of course, of the celebrated Grossman-Stiglitz paradox, according to which traders in financial markets cannot use private information about the future value of assets being traded to earn higher returns, because their trade on the basis of the information should cause the market price to reflect the information, so that they should have no informational advantage over other traders. This result is often viewed as posing an analytical challenge for theorists (to explain why the result is not true despite its appeal-

5. Bob Hall and Greg Mankiw (chap. 2 in this volume) suggest, in the context of a different assumed objective of monetary policy, that the minimization of the variance of a variable should be achieved by eliminating variation in the consensus forecast of that variable. Furthermore, they argue that it should be possible for the Fed to eliminate variation in the consensus forecast by an appropriate choice of policy, given timely information about the forecast. But in the present model, this is not possible. "Pegging the forecast" would mean choosing a policy rule that induces forecasters to choose $\alpha = 0$, but there is no choice of λ that achieves this. Thus the possibility of such an objective for the Fed depends upon details of the economy's information structure.

ing logic) rather than a problem for traders who expend effort in learning about companies whose shares are publicly traded; for it seems obvious that traders with superior information can make money from it (some who trade on the basis of "inside information" are sent to jail for it), whether our models can explain it or not. I do not think the result just presented is of the same kind. In the case of the Grossman-Stiglitz analysis, it is a rather subtle consequence of a specific model of market equilibrium that the market price should fully reveal the private information of the informed traders; and one can avoid this conclusion in various ways, for example, by assuming the presence of "liquidity traders." One may well suppose that in reality there is not full revelation, and this resolves the paradoxical features of their analysis. In the present case, there is no similar reason to doubt the assumption that the informed agents' forecasts are fully revealed to the monetary authority. First of all, the forecasters can make use of their superior information only insofar as they make their forecasts public—they have no motive to allow only a "noisy" report of their forecasts to become known. But more important, the policy proposal above is premised upon the authority's ability to observe the private forecasts; there is plainly no possibility of following such a rule if the private forecasters seek to and are able to keep their forecasts confidential.

Of course it is still possible, under the circumstances described above, for the monetary authority to reduce the variability of inflation to a great degree by choosing a very large value for λ. But this is hardly an attractive method. (It gives the forecaster extreme power over the economy, on the understanding that it will not be rational for him to use it.) If the monetary authority could observe s_t as well as f_t, it is plainly more desirable for the authority to choose a rule involving feedback from s_t (ideally, $u_t = -s_t$) than any rule of the form (2). Yet this fact would not be revealed by a simple consideration of the relative forecasting power of the variables s_t and f_t for inflation; for the two variables would be perfectly correlated (as long as forecasters use a rule of the form [3]), and neither would enter a forecasting regression with a more significant coefficient than the other.

A second pitfall in basing policy upon an indicator variable simply because it is found to enter significantly in forecasting regressions is that monetary policy interventions may affect the determination of the indicator, in such a way as to create a feedback loop from policy to the indicator back to policy that results in policy instability (and hence inflation instability as well). An absurd policy proposal serves to illustrate this. One variable that has been shown to forecast inflation is the return on Treasury bills (Fama 1975). A control variable that the Fed frequently uses to respond to changes in the perceived threat of inflation is the Federal funds rate. Would it then be a reasonable policy proposal to direct the Fed to raise the funds rate whenever higher than average T-bill yields are observed? Of course no one would propose this. But this is because of additional structural information that we have about the determination of these variables; specifically, we know that when the Fed raises the funds rate, T-bill yields rise as a result. It is because of this that we would expect the proposed rule to create instability. This illustrates the general point that an evaluation of policy rules must be based upon a structural model, both of the determination of the indicator variables and of the effects of monetary policy.

This example, while drawn as a caricature to make the point simply, is not without relevance to recent proposals for the use of nonstandard indicators. One proposal is to raise short-term interest rates whenever a long-term bond rate rises.[6] Now the connection between the funds rate and long-term rates is not as transparent as in the case of the previous absurd proposal. But still, according to the expectations theory of the term structure, the expectation that short rates will remain high makes long-term rates high. This raises the possibility that a disturbance to the long-term bond rate could trigger a policy shift whose effects on long-term rates would justify continuation of the policy, keeping long-term rates high, and so on.[7]

3.4 Feedback from Inflation Indicators Can Create Instability due to Self-Fulfilling Expectations

A further pitfall is the possibility of creating, through the monetary policy rule, a feedback loop that allows arbitrary changes in expectations to become self-fulfilling. This possibility again is one that can be evaluated only in the context of a structural model. It seems, however, particularly likely to be a problem in the case of rules that create feedback to policy from indicators that are themselves very sensitive to expectations. Thus an approach to the selection of policy rules that simply emphasizes the use of indicators with good forecasting power may direct attention to precisely the kind of policy rules that are most likely to create feedback loops that allow expectations to become self-fulfilling.

The following provides a simple example of a policy rule that might appear desirable, based upon the correlations that would be observed prior to the introduction of feedback from the indicator, but that would make possible "sunspot equilibria" (in some of which the variability of inflation would be higher than in the equilibrium that exists in the absence of feedback). The analysis is based upon an IS-LM model with rational expectations. Let output and nominal interest rates be determined by log-linear equations of the familiar sort:

$$m_t - p_t = y_t - \gamma i_t + \varepsilon_{mt} \tag{4}$$

$$y_t = -\delta[i_t - (E_t p_{t+1} - p_t)]. \tag{5}$$

Here m_t is the log of the money supply, p_t the log of the price level, y_t the log of output, and i_t the nominal interest rate; I assume that $\gamma > 0$ and that $0 < \delta$

6. This is the policy that Goodfriend (1993) describes the Fed as having actually followed.

7. This is, I believe, essentially the reason for the negative conclusion of Fuhrer and Moore (1992) regarding the consequences of feedback rules of this type.

< 1.[8] To eliminate constants from the equations, units are chosen so that in the steady state equilibrium with a constant money supply and no disturbance, $y = 0$, $i = 0$, and $m = p$. (Thus y_t should be interpreted as the percentage deviation of output from its "natural" level.) The price level p_t is assumed to be chosen at date $t - 1$, in such a way that[9]

$$E_{t-1}(y_t) = 0. \tag{6}$$

The disturbance term in (4) represents stochastic variation in money demand. I assume that ε_{mt} follows a random walk (independent of monetary policy), whose increments $\Delta\varepsilon_{mt}$ are bounded mean-zero random variables, and that it is not observed by the monetary authority. It is this source of inflationary pressure for which the authority would like to find an indicator. I consider this particular type of disturbance because the recent discussion of nonstandard indicators seems to have been largely a response to the perceived instability of the demand for familiar monetary aggregates during the 1980s.

Consider first a policy regime in which the money supply is constant. (We may without loss of generality suppose that $m_t = 0$ for all t.) In this case, there is a unique rational expectations equilibrium (REE) in which the variables $z_t = (y_t, i_t, m_t - p_t, \Delta p_t)$ are forever bounded.[10] In this equilibrium,

$$p_t = -\varepsilon_{mt-1}$$

$$y_t = -\frac{(1 + \gamma)\delta}{\delta + \gamma} \Delta\varepsilon_{mt}$$

$$i_t = \frac{(1 - \delta)}{\delta + \gamma} \Delta\varepsilon_{mt}.$$

8. The assumption that $\delta < 1$ in this model is needed in order for an unexpected permanent increase in the money supply to result in a temporary decrease in the nominal interest rate.

9. In the specification of aggregate supply, I follow McCallum (1989).

10. One might wonder why I consider only bounded solutions when I wish to suggest that the existence of multiple equilibria indicates a real source of economic instability. I regard equations (4) and (5) as log-linear approximations to some exact structural relations, whose approximation is accurate only as long as the variables z_t remain sufficiently close to their steady-state values. Hence explosive solutions to (4) and (5) need not even approximate solutions to the exact structural relations. Furthermore, there may be additional requirements for equilibrium besides the relations that (4) and (5) approximate that are satisfied by all solutions in which the values of the variables z_t do not leave a certain region, but need not be satisfied more generally. For example, when the liquidity-preference relation (approximated by [4]) is derived from a Sidrauski-Brock model of money demand, there is also a transversality condition that must be satisfied in equilibrium. Discussion of whether additional solutions have any economic meaning in the present case would thus require that more explicit foundations for equations (4)–(6) be provided. Nonetheless, consideration of the nonexplosive solutions alone should suffice to show that alternative policy rules can have very different consequences for the determinacy of equilibrium.

For examples of analyses of how the uniqueness of rational expectations equilibrium depends upon the monetary policy rule, in the context of explicit general equilibrium models and with consideration of the complete set of equilibria, see Smith (1994) and Woodford (1994).

Note that an innovation $\Delta\varepsilon_{mt} < 0$ results in a temporary increase in output (one period only), a temporary decline in the nominal interest rate (also one period only), and a permanent increase in the price level (after a one-period lag). Thus the shocks to money demand, not accommodated by any change in money supply, produce fluctuations in both economic activity and in the price level. Hence a policy of targeting the money supply might be judged undesirable.

Now suppose that the monetary authority, observing the fluctuations that occur under the constant money supply policy, seeks an indicator of the shocks that can be used to reduce the volatility of both output and inflation. The price level itself is not the best indicator, for it responds to the shock with only a one-period lag. (Of course the monetary authority does not know this unless it knows the structural model set out above; but it can observe that using other variables price-level variations can be forecasted earlier than they can be using only the price level itself.) If output is not observed immediately (and this is probably realistic) it also may not be the best indicator. The nominal interest rate reflects the shock immediately, and this variable is easy to monitor in practice, so that it would seem to be the most promising indicator.

This might suggest a policy of expanding the money supply whenever the nominal interest rate rises above its steady-state level, to offset the increase in money demand that one infers to have occurred. A well-known problem with this proposal, of course, is that nominal interest rates can also be high because of expected inflation; so there is the danger of expectations of inflation being self-fulfilling because they raise nominal interest rates and so bring about the money growth that in turn brings about the inflation. This is the reason for an interest instead in interest-rate spreads of various sorts. Let there be observed as well a two-period nominal interest rate, that I denote i_{2t}, and let it be related to the single-period rate in the way indicated by the expectations theory of the term structure, that is,

$$i_{2t} = \frac{1}{2}i_t + \frac{1}{2}E_t i_{t+1}.$$

If we define the spread as $s_t = i_t - i_{2t}$, we obtain

$$s_t = -\frac{1}{2}E_t(\Delta i_{t+1}) \tag{7}$$

to complete our model. Note that in the equilibrium for the case of a constant money supply,

$$s_t = \frac{(1-\delta)}{2(\delta+\gamma)}\Delta\varepsilon_{mt},$$

so that the spread is also an indicator of the current money demand shock, and like the single-period interest rate it should be observed by the monetary authority almost immediately.

This may suggest the desirability of a feedback rule for monetary policy of the form

(8) $$\Delta m_t = \lambda s_t,$$

for some $\lambda > 0$, so that the money supply is permanently increased whenever the term structure spread indicates that an increase in money demand has occurred. Note that according to our model, if a permanent increase in the money supply of the right size can be arranged each time such a shock occurs, there will never be any variations in either output or in the price level. Furthermore, the monetary authority might be led to consider such a response, even in the absence of knowledge of the structural model, based upon a comparison of the observed consequences of a temporary increase in the spread (in the absence of any change in the money supply) with the consequences of an increase in the money supply (assuming that independent stochastic variations in the money supply have also been observed).

In fact, *one* nonexplosive REE that always exists in the case of a policy of the form (8) is an equilibrium similar to the one described above, but one in which the amplitude of the fluctuations in prices and in output is reduced. In this equilibrium,

$$p_t = -\alpha\varepsilon_{mt-1}$$

$$m_t = (1 - \alpha)\varepsilon_{mt}$$

$$y_t = -\frac{(1 + \gamma)\delta}{\delta + \gamma}\alpha\Delta\varepsilon_{mt}$$

$$i_t = \frac{(1 - \delta)}{\delta + \gamma}\alpha\Delta\varepsilon_{mt}$$

$$s_t = \frac{(1 - \delta)}{2(\delta + \gamma)}\alpha\Delta\varepsilon_{mt},$$

where now

$$\alpha = \frac{2(\delta + \gamma)}{2(\delta + \gamma) + \lambda(1 - \delta)}.$$

Note that this implies that $0 < \alpha < 1$ for any $\lambda > 0$. Hence the fluctuations in both output and prices are reduced in amplitude. But one should also note that for *no* value of λ are they completely eliminated. This is another example of the problem discussed earlier, in which the use of the indicator to stabilize reduces its information content. (Note that the response of s_t to $\Delta\varepsilon_{mt}$ is proportional to α as well.)

The fluctuations can nonetheless be very greatly reduced in amplitude (in this particular equilibrium) by choosing λ to be sufficiently large. But this creates another problem. As long as $\lambda < 2(1 + \gamma)$, one can show that the above solution is the unique REE in which the variables z_t are forever bounded. But if the monetary authority chooses a value $\lambda > 2(1 + \gamma)$, there exists a very large multiplicity of nonexplosive REE. These include both "sunspot" equilibria, in which prices and output fluctuate in response to random events that do not affect any of the equilibrium conditions (4)–(7), and equilibria in which there is no effect of such events, but the response to money demand shocks is different from that described above. The fluctuations in output and inflation associated with these other equilibria can be arbitrarily large (whether they involve "sunspot" effects or not). Hence the choice of a large value of λ need not succeed in stabilizing either output or prices.[11]

I will not describe in detail the additional equilibria, but I wish to at least indicate why the multiplicity arises only in the case of large values of λ. Taking the expectations of (4) and (5) conditional upon information at date $t - 1$, and using (6) and the fact that ε_{mt} is a random walk, yields

$$E_{t-1}(m_t - p_t) = -\gamma E_{t-1} i_t + \varepsilon_{mt} \tag{9}$$

$$E_{t-1} i_t = E_{t-1}(p_{t+1} - p_t). \tag{10}$$

If we consider the equation corresponding to (4) at date $t + 1$, similarly take its expectation condition upon date $t - 1$ information, and subtract this from (9), we obtain

$$E_{t-1}(\Delta m_{t+1} - \Delta p_{t+1} + \gamma \Delta i_{t+1}) = 0.$$

Using (10), this becomes

$$E_{t-1}(\Delta m_{t+1} - i_t + \gamma \Delta i_{t+1}) = 0.$$

Then substituting (8) and using (7), we obtain

$$E_{t-1}(A(L) i_{t+2}) = 0, \tag{11}$$

where

$$A(L) = \lambda - (\lambda + 2\gamma)L + 2(1 + \gamma)L^2.$$

The uniqueness of bounded equilibrium then depends upon the roots of $A(L)$, for reasons of the sort developed in Blanchard and Kahn (1980). If $\lambda < 2(1 + \gamma)$, $A(L)$ has both roots inside the unit circle, and so (11) is satisfied only if $E_{t-1} i_t = 0$. In this case, it follows from (10) that $E_{t-1}\, \Delta p_{t+1} = 0$. Furthermore, (7) then implies that $E_{t-1} s_t = 0$, and (8) that $E_{t-1}\, \Delta m_t = 0$. From these results one then easily shows that the equilibrium described above is the only one. On the other hand, if $\lambda > 2(1 + \gamma)$, $A(L)$ has both roots outside the unit circle, and

11. Because the equilibrium conditions (4)–(7) are linear, a linear combination of any two solutions (with weights summing to one, but not necessarily both positive) is also a solution. So once we know that there exist at least two distinct solutions, we know that there exist solutions in which the amplitude of fluctuations is arbitrarily large.

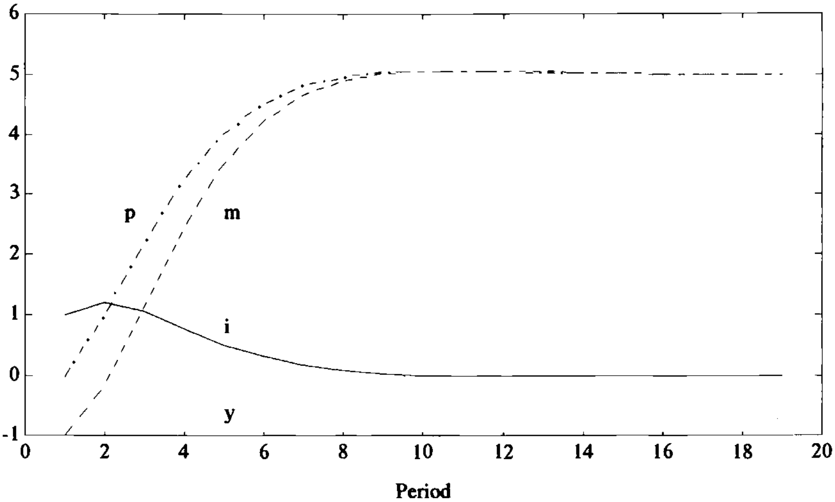

Fig. 3.1 An example of self-fulfilling expectations of inflation

there are many bounded solutions to (11). To these there then correspond a multiplicity of equilibria for the model.

Figure 3.1 gives an example of how expectations of inflation can be self-fulfilling when λ is sufficiently large. The plots represent a possible perfect foresight equilibrium in the case of no shocks to money demand, and given a predetermined initial price level $p_1 = 0$, for parameter values $\gamma = 1$, $\delta = 0.5$, $\lambda = 10$. In the absence of money demand shocks, one possible equilibrium is for m_t, p_t, y_t, and i_t to equal zero forever; this is the equilibrium described previously. In the equilibrium shown in the figure, instead, the price level steadily rises after date 1, asymptotically approaching a higher level. This expected inflation causes nominal interest rates to rise above their steady-state level at date 2 and thereafter, falling back to the steady-state level as inflation returns to zero asymptotically. (Note that [10] implies that there cannot be any deviation of ex ante real rates from the steady-state level that is anticipated a period or more in advance.) During these periods, there is an inverted yield curve (as interest rates are expected to decline), and this positive spread triggers money growth, bringing about the inflation as expected. The fact that this is expected to occur beginning at date 2, in turn, makes the two-period interest rate already high at date 1. This results in a negative spread at date 1, leading to contraction of the money supply for one period only, as a result of which single-period interest rates are also above their steady-state level at date 1. Expectations may switch from the zero-inflation path to this one at date 1 (after p_1 has already been fixed) as a result of an arbitrary random event; and a stationary "sunspot equilibrium" may be constructed as a stochastic process in which events of this kind occur each period, in each case triggering a dynamic response of the kind shown in the figure.[12]

It is also worthwhile to consider briefly the policy rules that become possible if aggregate output y_t is also observed at date t. Note that, under the constant money supply policy, y_t and s_t would be observed to always move together, so that the two variables would have *identical* power in inflation forecasting regressions. Thus it might seem that a policy of the form

$$\Delta m_t = -\lambda y_t \tag{12}$$

for some $\lambda > 0$ should have no advantages over one of the form (8). And in fact, a policy of the form (12) can support the equilibrium previously described for policy (8), although α is now given by

$$\alpha = \frac{\delta + \gamma}{\delta + \gamma + \delta(1 + \gamma)\lambda}.$$

But with the policy rule (12), this is the *unique* nonexplosive REE, no matter how large λ is. Hence it is possible (by choosing $\lambda > 0$ large) to reduce the fluctuations in output and inflation to any desired degree, without creating the possibility of self-fulfilling expectations.

The advantage of real activity y_t as an indicator is that it indicates the occurrence of a money demand shock for a different reason than do financial variables such as s_t. When output deviates from its "natural" level, this indicates that current aggregate demand has deviated from the level it was expected to have when prices were set. The change in the indicator thus represents a relatively direct effect of the shock itself, rather than an indication of the existence of expectations of future inflation. Indeed, an "output gap" can be taken to directly indicate a condition that *will cause* inflation in the future if a change in demand conditions does not occur in the meantime, insofar as it indicates an existing discrepancy between current prices and/or wages and those that the contracting parties would have wished to choose ex post. Because the indicator is not valuable solely due to its sensitivity to expectations, policy can be based upon it without creating the sort of feedback loop that allows expectations to be self-fulfilling. (For the same reason, policy can be based upon it without the use of the variable as a basis for policy rendering it useless as an indicator of inflationary pressures.) But this sort of difference between the two possible indicators cannot be revealed by a consideration of inflation forecasting regressions alone.

12. In a sunspot equilibrium made up of repeated fluctuations of this particular kind, there are no deviations of output from its "natural" level. But this is only because of the particular example of self-fulfilling expectations that I have chosen to exhibit. There also exist sunspot equilibria in which arbitrary changes in expectations cause fluctuations in output, and these can be of arbitrarily large magnitude.

3.5 Conclusions

All of these considerations point in the same direction: one cannot conclude too much about the desirability of alternative indicators without analyzing the effects of proposed operating procedures for monetary policy in the context of a structural model. I have not attempted here to provide an evaluation of specific proposed rules for monetary policy, or even of the usefulness of specific proposed indicators. This would require an analysis of the effects of candidate policies in the context of a structural model, which I have not undertaken in anything more than an illustrative way.

I have suggested, however, that there are general grounds for skepticism about a general approach to monetary policy that would advocate responding to variables because they have served in the past to forecast inflation, without regard to whether they forecast inflation because they represent (or are directly affected by) *proximate causes* of inflation, or because they indicate the *inflationary expectations* of other economic agents. The analysis above suggests important advantages to finding indicators of the causes of inflation rather than of inflationary expectations. What these variables would be depends upon one's model of the inflationary process. Various measures of the money supply can certainly be argued for on these grounds, but one cannot say on the basis of economic theory that these are the only such indicators or even the best ones. The evidence for recent instability of money demand certainly suggests the desirability of finding other indicators as well. In the simple model analyzed in section 3.3, the "output gap" can be a more useful indicator of inflationary pressure than the money supply. Whether it is in practice depends upon the details of an empirically adequate structural model of the sources of aggregate fluctuations and of the nature of aggregate supply and price-setting.

At this point it might be asked what sort of "structural model" I have in mind. Presumably one of the main reasons that policy evaluation on the basis of dynamic simulations of complete macroeconomic models is not practiced as much now as twenty years ago is that nowadays there is little agreement about the type of model that should be used for such a purpose. Fischer (1991) attributes this paralysis to the Lucas (1976) critique of econometric policy evaluation.

But the evaluation of policy is still one of our most important tasks. This volume shows that interest in that task is far from extinct, even among academic economists. The impression I get from some of the papers in this volume, however, is that it is hoped that the evaluation of policy in the context of econometric models that do not even pretend to be structural can somehow sidestep the difficult business of justifying a structural model. This is an odd response to Lucas's article. His point, after all, was that when a model is used to predict the consequences of adopting a new policy rule, it is important that one believe the model's equations to be structural (in the sense of continuing to hold if a policy change of the kind being considered were made). It is hard to see why the potential pitfalls in reasoning that he warns of should be avoided by resorting to econometric specifications that make even less claim to representing structural relations than did the consumption function or the Phillips curve that Lucas discusses. Perhaps the implicit argument is that, as virtue has been shown to be impossible, vice no longer requires an apology.

I do not see any reason to accept such a nihilistic view. Of course, certainty is impossible as to the true structural relations, and the claims we can make about the consequences of adopting one policy or another must be qualified on this account. But this is no reason not to attempt an analysis. Criticisms of analyses as being based upon relations that may not truly be structural need not be answered unless the critic proposes an alternative specification that is argued to be more accurate and the critic furthermore can show that under the alternative specification the answer reached would be quite different. Lucas provides examples of criticisms of that sort, but these are not criticisms that lead to nihilism; one has a specific alternative to consider, and the justification for the change and its quantitative significance can be discussed in the light of available evidence. Agreement may be difficult to reach (for the facts seldom speak unambiguously), but productive discussion is possible once there are clearly posed alternative models to confront.

There is also some wisdom in the method followed by Bennett McCallum (1990), in which the effects of a proposed rule are simulated under several different specifications of the basic structural relations. The point of this, I think, is not that one can often hope that different specifications will make no important difference for the effects of a proposed policy change. But it is surely important to know *which* differences in specification make a crucial difference for the predicted outcome of a particular policy experiment, so that one can then search for evidence that bears upon the adequacy of one or another model in this particular respect.

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