MACROECONOMICS AFTER A DECADE OF RATIONAL EXPECTATIONS: SOME CRITICAL ISSUES

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Introduction

It has now been just over a decade since the start of the rational expectations revolution in macroeconomics. In saying that, I am accepting the conventional view that the first papers to be widely influential were those published in 1972 by Robert Lucas.[1] As is well known, these were soon followed by landmark pieces by Thomas Sargent (1973) (1976a), Sargent and Neil Wallace (1975), and Robert Barro (1976) (1977a), as well as others by Lucas (1976) (1977).[2] And, as is also well known, the revolution has been highly controversial because of the criticism of prevailing views that was implicit in the above-mentioned papers and explicit in others (e.g., Barro (1979), Lucas and Sargent (1978)).

Today the disputation seems to be less heated than it was a few years ago, with members of the leading schools of thought openly recognizing weaknesses in their own theories and strengths in those of others. Of course, major differences continue to exist, as consideration of recent papers by Taylor (1982), Kydland and Prescott (1982), and Sargent and Wallace (1982) will emphasize. But the terms of disagreement are no longer about the hypothesis of rational expectations—some version of the latter is utilized in almost all current research—but about the nature of the economy within which agents operate and form expectations.

In this regard, the portion of a macroeconomic model that most strongly affects its policy-relevant characteristics is that pertaining to aggregate supply behavior. Accordingly, I will begin this presentation by discussing some competing theories of aggregate supply currently being utilized in rational expectations (RE) models, with emphasis on the distinction between "equilibrium" and "sticky-price" assumptions. This section will also include a brief description of a model that I find attractive and some discussion of the RE version of the natural-rate hypothesis. In the next section I will more briefly mention a few issues involving specification of the aggregate demand portion of macroeconomic models, with attention devoted to the role of the overlapping-generations framework. Finally, I want to consider a recent attempt to denigrate the importance of Lucas's critique (1976) of traditional policy-evaluation techniques, an attempt that makes use of "vector autoregression" models. Throughout I will take it for granted that there is no need to spend time justifying the rational expectations assumption itself.

Flexible and Sticky Price Models

It is of course widely understood that properties of RE models with multiperiod nominal contracts (e.g., Fischer (1977), Taylor (1979a)) are very different from those in which prices adjust fully within each period. Let us begin by considering which type is more useful for analysis of actual present-day economies.

In my opinion there is at least one reason for believing that some type of sticky-price model is needed to provide an empirically satisfactory description of quarter-to-quarter or even year-to-year fluctuations in prices, output, and other macroeconomic variables. In saying that, I have in mind several empirical regularities or "stylized facts" including the following:[3]

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[1] Specifically, Lucas (1972a) (1972b). Of course a few papers had previously been published using rational expectations in macroeconomic settings, but these did not have a great deal of impact.

[2] Important items were also produced by Fischer (1977), Taylor (1979a) (1979b), and others.

[3] Evidence supporting these facts appears in a large number of studies, including Sargent (1976a), Barro (1977a), Mishkin (1982), Sims (1980), Kennan and Geary (1982), and Gordon (1982).

(i) Output and employment magnitudes exhibit significant "persistence," i.e., positive serial correlation.

(ii) Output and employment magnitudes are strongly and positively related to contemporaneous money stock surprises.[4]

(iii) Output and employment magnitudes are not strongly and positively related to contemporaneous price level surprises.

(iv) Real wages do not exhibit countercyclical tendencies; indeed they appear to be mildly procyclical.

Furthermore, I have in mind a fact of a different kind, namely, that information concerning nominal aggregate variables—including money stock measures and various price indices—is available on a relatively prompt basis. The relevant point, then, is that this availability is hard to reconcile with fact (ii) in a flexible-price equilibrium model, for the existence of real effects of monetary shocks depends, in these models, upon agents' ignorance of contemporaneous values of nominal aggregates.[5] It was suggested by Lucas (1977) that this difficulty might be overcome if the "true" relevant monetary aggregate were unobservable and thus measured with error. King (1981) has shown, however, that if observations are available on a "proxy" variable that differs randomly from the true unobservable aggregate, output and employment should be unrelated to the proxy. Thus, according to these models, output and employment should be unrelated to movements in *measured* monetary aggregates, in contrast with fact (ii). King's analysis has been further developed and implemented by Boschen and Grossman (1983).[6]

A second reason for doubting the adequacy of flexible-price equilibrium models is provided by econometric studies which suggest that output fluctuations are induced by *anticipated* monetary movements, as well as surprises.[7] These studies have some weaknesses[8] and there is not a strict one-to-one relationship between flexible-price equilibrium models and the absence of real effects from anticipated money movements. The relationship is close enough and the quality of the cited studies high enough, however, that the findings are troublesome for the flexible-price hypothesis.

In this regard I would like to emphasize that acceptance of the idea, that some kind of price-level stickiness is necessary for explaining observed time series data, does not require abandonment of the *equilibrium approach* to macroeconomic analysis. To see this, imagine a model in which nominal multi-period contracts are endogenously explained as the response of rational agents to adjustment, bargaining, or other "transactions" costs.[9] As Lucas (1980, p. 712) has recognized, such a model could be an equilibrium model—one in which all agents optimize relative to correctly-perceived constraints and in which the resulting supplies and demands are equated—though one without perfectly flexible prices. As such, it would incorporate the virtues of equilibrium analysis, including the intellectual discipline that it entails, a specification expressed in terms of policy-invariant relationships, and the possibility of basing policy choices on the utility of individual agents.

Indeed, such a model would seem to be precisely what is needed for the analysis of stabilization policy. As Fischer (1977, p. 204) acknowledged, it is likely that the format and length of nominal contracts agreed to by rational agents would change in response to major shifts in policy. So, even if existing contract models were capable of providing a good explanation of macroeconomic fluctuations within a single policy regime, they would tend to be unreliable if used to predict the comparative effects of alternative regimes.[10]

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[4] Here and below I use the term "surprise" to refer to a one-period expectational error of the form $m_t - E_{t-1}m_t$, in notation discussed below.

[5] This ignorance is required, to be more precise, in the three leading flexible price equilibrium models, namely, those of Lucas (1972a), Lucas (1973), and Barro (1981, pp. 42-50). It is possible that other such models do not have this property.

[6] The relevant point was mentioned by Barro (1981) and was very recently emphasized by Grossman (1982). Grossman recognizes, but does not accept, the possibility that money-output correlations are due to "reverse causation," i.e., monetary responses to output movements generated by shocks to technology or preferences, as suggested by King and Plosser (1982).

[7] See, for example, Gordon (1982) and Mishkin (1982).

[8] Movements in "natural rate" values of output or employment are assumed to be representable by trends, in contrast to the evidence given by Nelson and Plosser (1982). Also, the methods of overcoming the "observational equivalence" difficulty (Sargent, 1976b) are not entirely satisfying.

[9] The difficulty with this exercise comes in understanding why contracts are set in nominal terms without indexation.

[10] The problem is of course compounded in attempts to predict the effects of real-time changes in regimes because expectations are unlikely to adjust immediately to the new policy rule.

From the foregoing perspective, existing nominal contract models are best seen as incomplete models—ones that treat as fixed important parameters that would tend to be constant within regimes but to change across regimes. Even in their present state these models are of interest, however, so I would like to devote a few paragraphs to a comparison and discussion of the two most influential, those of Fischer (1977) and Taylor (1979a) (1980). For simplicity, I shall refer to two-period versions of each.

In both the Fischer and Taylor papers, a rudimentary aggregate demand function—one that makes the quantity demanded a fixed stochastic function of real money balances alone—is utilized, so no difference arises from that component. The wage-price or aggregate supply components are very different, however, despite the common feature of two-period, staggered, nominal wage contracts. Specifically, in each model nominal wages are set at the start of period t to apply to half of the workforce in periods t and t+1, but the values at which these wages are set are chosen according to different principles. In Fischer's model, the wages set for t and t+1 will usually differ from each other and each is chosen, in light of current price-level expectations, so that the real wage is expected to clear the labor market in the relevant period. In Taylor's model, by contrast, the same value is set for periods t and t+1 and is chosen to equal the average of the nominal wage rates expected to prevail for the other half of the workforce in t and t+1, with an adjustment added to take account of (expected) excess demand.

Prices, moreover, are assumed to move in unison with the average wage in Taylor's model, so that there is no systematic (or unsystematic) cyclical variation in the real wage. Fischer, on the other hand, assumes that firms select employment (hence, output) magnitudes in each period so as to equate the marginal product of labor to the observed real wage. Consequently, there is a tendency for the real wage to be high when employment is low.

Of these two models, Taylor's has attracted more attention and has been the more influential. One reason, undoubtedly, is that Taylor himself has produced a number of technically sophisticated and economically interesting applications involving actual data and policy issues of current concern. I suspect that there is an additional reason, however, which is the existence of a widespread belief that Taylor's model is substantially more consistent with crucial facts. In particular, it is believed that Taylor's model is more plausible than Fischer's because it generates more persistence (for a given contract length) and does not yield the counterfactual implication that real wages move countercyclically. Consequently, I think that it is important to understand that neither of these observations is entirely compelling and that Taylor's model has some implications of its own that are theoretically unattractive.

With respect to the persistence issue, it should be kept in mind that there are several plausible ways of rationalizing persistence in any RE model. Among these are the existence of employment adjustment costs, the presence of finished-goods inventories, and the inability of agents to distinguish between permanent and transitory shocks.[11] Any of these features could be included in a variant of Fischer's model without altering the properties that his paper focussed upon. Furthermore, the relevant theoretical concepts involve output or employment measured *relative* to capacity (natural rate) values. But of course we do not possess direct observations on these relative magnitudes; the stylized fact (i) refers to raw measures of output and employment or to measures adjusted by the removal of a deterministic trend. And recent work by Nelson and Plosser (1982), which relies upon stochastic trend removal, suggests that there is much less persistence in the relevant adjusted series than the raw or deterministically-detrended measures have indicated.

Next, the countercyclical real wage in Fischer's model does not come from its wage-setting specification, but from an independent assumption regarding employment determination—i.e., that firms equate the marginal product of labor to the real wage. Now the counterpart of that relation in Taylor's model is the condition that the (detrended) real wage is constant. But that condition implies that product prices behave in the same way as average nominal wages, which also seems counterfactual.[12]

These arguments suggest that the above-mentioned reasons for preferring Taylor's model to Fischer's are not compelling. A point of equal or greater importance is that Taylor's model possesses a questionable feature, namely, a presumption that labor supply-demand behavior is fundamentally concerned with relative, rather than own, wages. As a result of this feature, together with contract staggering, the

[11] The last two features have been analyzed by Blinder and Fischer (1981) and Brunner, Cukierman, and Meltzer (1980), respectively, while the first has been emphasized most notably by Sargent.

[12] My argument is not that real wage movements induce business cycles, but that some systematic movements in real wages are observed.

model does not possess the *natural-rate* property as defined by Lucas (1972b).[13] That is, the model is one in which a suitably-designed monetary policy is capable of yielding a *permanent* increase in output relative to its natural-rate value: monetary policy can keep unemployment "low" forever.[14]

Having mentioned various shortcomings of the Fischer and Taylor models, let me now discuss an alternative that I find attractive, one which conforms to the natural rate hypothesis and also to all of the stylized facts mentioned above.[15] For the sake of simplicity and ease of comparison, the discussion will presume a rudimentary aggregate demand schedule. This can be expressed formally as

$$(1) \quad y_t = b_0 + b_1(m_t - p_t) + v_t \qquad b_1 > 0$$

where y_t, m_t, and p_t are logs of output, the money stock, and the price level while v_t is a white-noise disturbance. Also for simplicity, the log of the "natural rate" level of output, $\bar{y}_t$, is assumed to deviate from its previous value only by virtue of a white-noise disturbance, u_t:

$$(2) \quad \bar{y}_t = \bar{y}_{t-1} + u_t.$$

In addition—and again only for the sake of simplicity—I assume that output is perishable, so that no inventories are held.

The crucial aspect of the model is the way in which prices are determined. It is assumed that p_t is set, at the end of period t—1, at a level that is expected to make the quantity demanded in t equal to a weighted average of y_{t-1} and $\bar{y}_t$. Two basic ideas are involved in this assumption. The first is that firms find it optimal to meet all demands at the quoted price.[16] Second, firms experience adjustment costs whenever y_t differs from y_{t-1} but also suffer opportunity costs whenever there is any discrepancy between y_t and $\bar{y}_t$. Then if both of these cost functions are quadratic, producers will aim at some value between y_{t-1} and $\bar{y}_t$ which we denote as $\lambda y_{t-1} + (1-\lambda)\,\bar{y}_t$, with the parameter λ $(0 \leq \lambda < 1)$ reflecting the *relative* costliness of output changes. Consequently, the price level is set at a value that satisfies (1) expectationally, with $\lambda y_{t-1} + (1-\lambda)\, E_{t-1}\,\bar{y}_t$ inserted in place of y_t:

$$(3) \quad \lambda y_{t-1} + (1-\lambda)\, E_{t-1}\,\bar{y}_t = b_0 + b_1(E_{t-1}\,m_t - p_t).$$

Here, of course, $E_{t-1}(.)$ denotes the mathematical expectation of the indicated variable, conditional upon realizations of all variables in period t—1 and earlier. The price-setting relation (3) can be expressed in various ways. One version that I have emphasized elsewhere takes the form of a modified expectational Phillips-Curve relationship, namely

$$(3') \quad p_t - p_{t-1} = \gamma(y_{t-1} - \bar{y}_{t-1}) + E_{t-1}(\bar{p}_t - \bar{p}_{t-1}), \qquad \gamma = (1-\lambda)/b_1 > 0,$$

in which the relevant expected inflation rate is that pertaining to $\bar{p}_t$, the value of p_t that equates y_t to $\bar{y}_t$ in (1).

The other main component of the model incorporates Fischer's scheme of nominal wage determination. Let w_t be the log of the average nominal wage in period t and let z_t denote the log of the real wage, $z_t = w_t - p_t$. Also let $\bar{z}_t$ be the natural-rate value of z_t, which evolves over time as a random walk related to that generating $\bar{y}_t$:

$$(4) \quad \bar{z}_t = \bar{z}_{t-1} + \zeta_t, \qquad E(u_t\zeta_t) > 0.$$

Then with half of the wage contracts prevailing in t having been set at the end of t—1, and the other half at the end of t—2, we have

$$(5) \quad w_t = (1/2)\, E_{t-1}(\bar{z}_t + p_t) + (1/2)\, E_{t-2}(\bar{z}_t + p_t).$$

Finally, to complete the system we suppose that the monetary authority sets m_t according to some policy feedback rule, utilizing data from periods t—1 and before. Without specifying the form of the systematic component, we can write

$$(6) \quad m_t = E_{t-1}\,m_t + e_t,$$

thereby defining e_t as the (white noise) random component of policy behavior. In principle, equa-

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[13] With staggering, relative wages pertain to values set in different periods. If the relationship between such values depends upon output (relative to capacity), as Taylor's model assumes, then the latter variable will be affected by the trajectory of nominal wage settlements. I am indebted to Taylor for explaining to me that it is not an assumed concern for relative nominal wages, as opposed to relative real wages, that is responsible for this feature.

[14] Fischer's model, by contrast, does possess the natural-rate policy.

[15] This specification is mentioned, but not investigated, in McCallum (1980, p. 735).

[16] The analogous requirement would not seem extreme or unusual in a version of the model in which inventories are held.

tions (1)-(6) govern the evolution of the six variables y_t, $\bar{y}_t$, p_t, m_t, $\bar{z}_t$, and w_t (with z_t given definitionally as $w_t - p_t$).

It is easy to see from equations (1), (2), (3), and (6) that, in this model, output conforms to the process

$$(7) \quad y_t - \bar{y}_t = \lambda(y_{t-1} - \bar{y}_{t-1}) + b_1 e_t + v_t - u_t.$$

Thus we can verify by inspection that stylized facts (i), (ii), and (iii) are mimicked by our model: output is positively related to monetary surprises but not to one-period price level surprises (as $p_t = E_{t-1}p_t$), and both y_t and $y_t - \bar{y}_t$ are positively autocorrelated. Furthermore, it can be shown that, for a wide class of specifications for the systematic component of monetary policy, z_t and y_t are positively correlated. Thus the model also conforms to the stylized fact (iv). And from (7) it is obvious that the natural-rate property obtains.

Indeed, it is clear from (7) that the famous *policy-ineffectiveness* proposition obtains in the model at hand. But while that result is useful as a counter-example to some mistaken notions about necessary conditions for validity of the ineffectiveness proposition, I do not think that very much should be made of it. The reason is that the result is not highly robust: while it holds if the aggregate demand specification (1) is changed to

$$(1') \quad y_t = \beta_0 + \beta_1(m_t - p_t) + \beta_2 E_{t-1}(p_{t+1} - p_t) + v_t,$$

it does not hold if instead we have

$$(1'') \quad y_t = \beta_0 + \beta_1(m_t - p_t) + \beta_2 E_t(p_{t+1} - p_t) + v_t.$$

Nor, more importantly, does it hold if the information set used in computing the expectation of p_{t+1} includes the current interest rate, as well as past values of all variables. This last specification would seem to be empirically relevant, given the existence of daily reports on interest rates in nation-wide markets.

But while I do not want to argue for the general validity of the ineffectiveness proposition, even as a matter of theory, I do want to mention parenthetically that many of the alleged theoretical demonstrations of its invalidity rely on a misinterpretation. The point is that the proposition asserts that the systematic components of monetary and fiscal policies have no influence on the evolution of output or employment *relative to* their natural rate (capacity, full-information) values—not to the raw values themselves. The proposition is designed to pertain to issues about countercyclical *stabilization* policy, which has always been conceived of as a device for keeping output and employment close to their natural-rate values, not for altering the paths of the latter variables. A more extended discussion of this issue, including some examples of published misinterpretations, is presented in McCallum (1980, pp. 726-729).

The model outlined above can be extended in many ways—by including fiscal variables and/or inventory holdings, by positing more realistic processes for $\bar{y}_t$ and $\bar{z}_t$, etc.—without altering its main properties. Thus it provides, in my opinion, an attractive and useful framework for thinking about macroeconomic fluctuations and stabilization policy. It has some weaknesses, however, that should be acknowledged. First, the implicit assumption that price changes are prohibitively costly within each period, but costless between periods, is extreme and difficult to justify except by definition of the "period." And with that justification there is no guarantee that the periods so defined will correspond to the quarter-year periods in which most actual data is reported. Also, the *length* of a theoretical period could be affected by extreme conditions, such as those experienced during hyperinflations. Consequently, the period definition may not be fully policy-invariant.

Perhaps the most basic weakness of the model is the absence of any compelling explanation for the absence of indexing.[17] Why is it, in other words, that posted prices do not come with a proviso that automatically adjusts them in response to monetary surprises? The usual answer is that such arrangements are costly, but the validity of that answer is by no means self-apparent. The difficulty is, however, one that is not specific to this model. It merely reflects economists' incomplete understanding of why contracts are often made in nominal terms. More generally, the above-mentioned flaws are a reflection of the fact that this model is incomplete, in the sense described above. An equilibrium rationalization of its price-setting arrangements has not been developed.

To conclude my discussion of issues involving aggregate supply, I would like to return to the subject of the natural rate hypothesis (NRH) and comment upon its present status. In particular, I want to emphasize that a number of influential researchers

[17] This issue was introduced by Barro (1977b).

in the Keynesian tradition[18] have in recent years expressed agreement with the NRH, yet have continued to conduct analysis in models that do not possess the NRH property.[19] A prominent example of a specification of this type is provided by models that incorporate the concept of a "nonaccelerating-inflation rate of unemployment" (NAIRU). Clearly, if there exists a stable negative relationship between unemployment and the acceleration magnitude (i.e., change in the inflation rate), then the unemployment rate can be permanently lowered by permanently accepting a higher rate of change of inflation—in contradiction to the NRH. Another example is provided by models that include demand and supply functions expressed in real terms together with a partial adjustment relation for a nominal price variable and the assumption that the transaction quantity is the smaller of supply and demand (or that demand is determining).[20] In such a formulation, there is an implied permanent tradeoff between the rate of change of the price variable and real excess demand.

Proponents of such specifications would no doubt admit that their implications regarding unemployment magnitudes under conditions of sustained accelerating inflation are implausible, but would presumably contend that the models are not intended to be applicable to extreme policies of that type. For predicting the consequences of less extreme policies, they would claim, the models are appropriate. It is not clear, however, that such a claim is justifiable. What is needed for the model's predictions to be plausible is that the policy followed be essentially the *same* as that of the sample period used in estimating the relationship. But to agree to that limitation is to admit that the model cannot be used for most interesting questions. In terms of Tobin's (1980, pp. 66-68) exercise, for example, I would say that a gradual but reliable and sustained decrease in the rate of growth of nominal GNP—or the money stock or any other nominal aggregate—is very *unlike* the policies of the past two decades. Thus the simulation predictions are not persuasive.

More generally, I would argue that the nonconformity of any model to the NRH property provides *prima facie* evidence of some implied form of irrationality and an associated vulnerability of the model to the famous Lucas (1976) "critique." In other words, nonconformity of any model to the NRH indicates that it will be systematically unreliable in predicting the consequences of alternative policy choices.[21] Other points concerning the Lucas critique will be discussed in the sections that follow.

Aggregate Demand

To this point we have been concerned with issues involving aggregate supply behavior. Let us then more briefly consider some developments having to do with aggregate demand.[22]

As our previous discussion hinted, Lucas, Sargent, and other leaders in the RE area have advocated the use of aggregative general equilibrium models for macroeconomic policy analysis. The object of this strategy is to avoid the weaknesses of traditional macroeconomic models, weaknesses that were emphasized in Lucas's critique (1976). The hope is that it may be possible to develop models that are genuinely structural—i.e., policy invariant—by working "at the level of objective functions, constraint sets, and market-clearing conditions" (Sargent, 1982, p. 383). Since this equilibrium *approach* does not limit the user to flexible price models, it is almost impossible not to sympathize with it, at least at the level of principle. Adherence to the approach is not a guarantee of success, however: if a model is based on a poorly-specified objective function it will be a poor model, explicit maximization analysis notwithstanding.

Since this last qualification is obvious to the point of triviality, an example of how the approach can go astray may be of some interest. The example that I have in mind involves the application of a class of overlapping-generations (OG) models to problems in monetary economics. The class of OG models in question is that in which, although there is an inherently useless entity called "fiat money," the specification excludes any cash-in-advance or money-in-the-utility-function feature that would represent a transactions-facilitating property for that entity. Accordingly, the entity does not serve, in these models,

[18] Including Tobin (1980), Modigliani (1977), and Gordon (1982).

[19] See Tobin (1980, pp. 66-68), Modigliani and Papademos (1975), and Gordon and King (1982).

[20] This sort of formulation mars, for example, an interesting and otherwise attractive study by Smyth (1982).

[21] This is, I would suggest, the true message of Lucas (1972b) and one of the most basic messages of the RE revolution.

[22] Of course the distinction is not a clean one in equilibrium models, since agents in such models make factor supply and commodity demand choices simultaneously and in response to the same wealth and price variables. What is here meant by an "aggregate demand" topic is one that focuses attention on saving and/or asset-demand relationships.

as a medium of exchange; its only function is as a store of value.[23] Consequently, several striking and unusual conclusions are obtained when the entity is interpreted as money. For example, if the government causes the stock of money to grow at a rate even slightly in excess of the rate of output growth, the price level will be infinite (i.e., money will be valueless). Second, equilibria in which the price level is finite will be Pareto optimal if and only if the growth rate of the money stock is nonpositive. Third, "open-market" increases in the stock of money have no effect on the price level. I have argued at length, however, that these unusual conclusions obtain because of the model's neglect of the medium-of-exchange role (McCallum, 1983). If the model is modified so as to reflect this role for the entity called money, its unusual conclusions vanish. Consequently, the unmodified class of OG models evidently provides a misleading vehicle for the analysis of economies in which there is a medium of exchange.

It remains to be explained what this OG example has to do with the equilibrium approach. To understand the connection let us recall that an essential aspect of the approach is the development of policy-invariant relations. Now in dynamic settings, as Sargent (1982) has stressed, standard asset demand functions may not be policy-invariant; one must look "beyond decision rules to the objective functions that agents are maximizing and the constraints that they are facing" (p. 383). But the influence on agents' constraints of the store-of-value function of money is clear and simple to express analytically, while the influence of the medium-of-exchange function is just the opposite. Indeed, it is extremely difficult to devise a general equilibrium setting in which the medium-of-exchange role is both rigorously and convincingly depicted. The traditional method has of course been to include real money balances as an argument of agents' utility functions, but that is an unsatisfying practice which clearly must be proxying for something more fundamental. Together these considerations encourage analysts to shun the traditional approach and adopt ones that focus attention on money as a store of value. And because they are well-suited in important ways for the analysis of store-of-value issues, OG models provide an attractive vehicle. Thus it is not very surprising that an OG model without medium-of-exchange features would be adopted by researchers striving to overcome the Lucas critique. But that attempt will nevertheless be unsuccessful if the model is used for certain monetary issues, for neglect of the medium-of-exchange function constitutes a potentially serious specification error. The Lucas critique itself amounts to a reminder (of an especially important type) that specification errors will keep a model from being policy invariant.

Turning to a substantive matter, it is interesting to note that an OG model of the type discussed above has recently been used by Sargent and Wallace (1982) in an attempted rehabilitation of the infamous "real bills" doctrine. Since one of Henry Thornton's important contributions to monetary economics was his criticism of that doctrine, a few brief remarks should be in order. In their recent paper, Sargent and Wallace argue that (among other things) the price level is determinate under a real-bills policy regime that pegs the interest rate at zero, a finding that contrasts sharply with the price-level indeterminacy result of their famous (1975) paper. Examination of the recent argument indicates, however, that determinacy is not actually established. What the paper shows is that each agent faces the same real budget constraint under the real-bills regime as under a "laissez-faire" regime in which the stock of fiat money is held fixed. But this implies only that the real aspects of the model's equilibria are the same under the two regimes; nothing is implied about nominal magnitudes. Furthermore, the interest rate in the Sargent-Wallace (1982) model does not, because of this model's neglect of the medium-of-exchange role of money, correspond to interest rates in actual economies. Thus pegging its real value at zero does not require a negative real return on money (i.e., positive inflation) as is the case in settings in which nonmonetary assets command higher rates of return than money because of the latter's transaction-facilitating properties. Consequently, the recent Sargent-Wallace paper does not provide a convincing reason for believing Thornton's analysis to be incorrect.

The VAR Challenge to the Lucas Critique

The final topic to be discussed also concerns the Lucas critique. Previously I have claimed that its basic message—i.e., that traditional econometric models are poorly designed for policy evaluations because their basic relationships are unlikely to be policy invariant—has been very widely accepted, even

[23] Notable items in the literature in question are Bryant and Wallace (1979), Sargent and Wallace (1982), and Wallace (1980).

by economists who dispute other notions associated with the RE revolution (McCallum 1979, 1980). That situation still prevails, I believe, but within the past few months a notable challenge has arisen. More specifically, a number of prominent economists, who are certainly well aware of the critique, have authored papers in which so-called vector autoregression (VAR) models are used for policy analysis.[24] These VAR models are, as is well-known, constructed in a manner that involves no attempt to represent structural relationships; they consist of a set of reduced-form equations in which lagged values of the system's variables are used to explain current values, with all variables treated as endogenous. Consequently, VAR systems would seem to be even more vulnerable to the critique than the traditional econometric models that Lucas considered. One is naturally led, then, to ask: what is the justification given by those who have used VARs for policy analysis? In fact most users have provided no justification themselves, but have referred to a recent paper by Christopher Sims, the originator of VAR techniques. Let us then consider the argument put forth in that paper (Sims, 1982).

One important theme of Sims's discussion is that equilibrium-approach econometric techniques (exemplified by Hansen and Sargent (1980)) are unlikely to lead to accurate predictions of the effects of real-time *changes* in policy rules, as opposed to cross-regime steady-state comparisons. As it happens, that suggestion seems to me to be correct. But it also seems rather beside the point, since Lucas, Sargent, and other equilibrium-approach leaders have not claimed to be able to use their models in that way. Instead, they have expressed the aim of being able to make valid comparisons of the properties of stochastic steady states generated by alternative maintained policy regimes.

Another theme of Sims's paper is that genuine policy-rule or regime changes are extremely rare in actuality. Most policy *actions* involve instead the resetting of policy instruments in response to recent developments in the economy, a type of activity that Sims calls "normal policymaking." Again I would agree with the observation—but point out that it is in no way inconsistent with the Lucas critique.

In addition, however, Sims claims that VAR methods can be useful in the context of normal policymaking. Since this claim appears to be inconsistent with the message of the critique, let us briefly examine the argument. Under a given policy regime, a policymaker's objectives are by definition unchanging through time. So if the structure of the economy were known and also unchanging, policy feedback rules would be unchanging and there would be no purpose for policy exercises using any kind of model. But of course the true structure of any actual economy is imperfectly known and probably changing, so there could often be some potential gain from re-estimation of models used to design policy. And with objectives constant, autoregressive representations of expectational variables may be changing only slowly and gently, so VAR models may not go badly astray in the way described by Lucas. Thus there could be some benefits from period-by-period re-estimation of VAR systems and their utilization in the selection of current instrument settings.

In this case, the argument seems plausible but not extremely consequential. What it suggests is that VARs can be helpful to policymakers, but only if the latter continue to behave in approximately the same way as in the past. There is no claim that VARs could be useful in evaluating the effects of substantially different sustained policies. Furthermore, the argument provides no compelling reason for believing that VAR methods would be superior, even in the context of normal policymaking, to Hansen-Sargent techniques.

Now let me turn to my outright disagreements with Sims's paper, of which there are two. The first involves an application of VAR methods in the context of an analysis of announced policy plans of the Reagan administration. I think it is fair to say that these plans, as announced, represent a substantial break with past policies. How, then, does Sims justify use of the VAR models? Apparently, his presumption is that the public does not believe that a genuine regime change will actually take place: "Precisely because those vying for control of policy will propose to make permanent changes in the rule much more often than they will succeed in doing so, the public is likely to discount their rhetoric and react to the actual course they set for policy as if it were a disturbance to the existing probabilistic structure" (1982, p. 139). Given this assumption that the public disbelieves in a regime change, there are two possibilities: either the public is correct in its disbelief or it is incorrect. But note that if Sims is assuming the former—that the "proposed paths of policy variables are . . . not attainable"—then he is evaluating the effects of a hypothetical change in

[24] Examples are provided by Friedman (1982), Gordon and King (1982), and Litterman (1982). Friedman does not carry out policy **simulations** but his "two-target" proposal for monetary policy is based in part on an assumption that VAR relationships are policy invariant.

policy under the assumption that there is no change in policy. This, clearly, involves a logical contradiction that negates any conclusion. The other possibility is that the public is incorrect in believing that there is no change in regime. In this case there is no logical contradiction, but the analysis presumes systematically incorrect expectations. To the extent that the public (correctly) believes in the policy change, Sims's predictions will be incorrect. And Sims shows no inclination to assume systematically incorrect expectations as a general matter. Thus his arguments concerning the Reagan plans are unsatisfactory.[25]

My other objection is that the general tone of Sims's discussion seems likely to encourage economists to conceive of policy in terms of isolated actions rather than sustained rules. Such encouragement is, of course, in direct opposition to the advice of Lucas, Sargent, and other RE advocates. Lucas (1976) (1978) has argued eloquently that economists should focus their attention on sustained rules, in part because understanding their effects is the most that there is any chance of doing well. This position seems to me correct. The profession hardly knows enough about *deterministic* steady states to evaluate their relative merits—consider the difficulties in conceptualizing the costs of anticipated inflation—much less, those of stochastic steady states or alternative sequences of arbitrary policy actions. Furthermore, actual policymakers are strongly inclined to focus attention on today's situation, to the neglect of both future and past. To me it seems undesirable for the economics profession to encourage them in this inclination, as it did during the period of time between the Keynesian and rational expectations revolutions.

[25] This is not, of course, an endorsement of these plans.

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