The Rational Expectations Challenge to Policy Activism

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I. Two Views on Stabilization Policy

Economists hold a wide range of views on how stabilization policy ought to be conducted, but for our purposes it is enough to divide all views into two opposing camps. The first camp, which we label "policy activism," greatly predominates in number of adherents. Policy activists maintain that there exists an exploitable Phillips curve. Their view suggests that policy can deliberately be manipulated to achieve lower unemployment at the cost of higher inflation. The recommended degree of policy activism, that is, the extent to which the policy instrument should be varied in response to new information in order to achieve full employment, differs among members of this camp. It depends on relative costs subjectively attached to unemployment and inflation, on the perceived tradeoff between the two, and on the degree of confidence with which that perception is held. Nevertheless, activists are bound together in believing that the business cycle and consequent swings in unemployment are disequilibria phenomena which result from shocks to aggregate demand in an economy with slowly adjusting prices, and that stabilization policy ought to be directed at offsetting these shocks.

The policy activism camp went virtually unchallenged from the time Keynesian theory was first espoused until the present decade. There were, however, some economists who argued that because of great uncertainty about the effects of monetary policy, simple constant-growth-rate rules for the money stock (nonactivist policies) can be expected to outperform nontrivial feedback rules (activist policies). This argument for nonactivism turns out to be not very persuasive, however. Given a macroeconometric model with an exploitable Phillips curve, optimal policy will be a constant-growth-rate rule only if the estimates of policy multipliers have infinite variance. Activists took great pleasure in showing that the St. Louis model, the econometric embodiment of monetarist

theory, implies that nontrivial feedback rules for the money supply dominate constant-growth-rate rules. In fact, no one was able to come up with a model where that isn't the case — until recently.

Robert E. Lucas, Jr., produced such a model in 1972 [6].† The concepts contained in that paper laid the foundation for the second camp, which we label "rationality." Lucas's arguments were reduced to their basics, and the policy implications of his model were clarified for general audiences by Thomas Sargent and Neil Wallace [12]. Although this second camp has only appeared on the scene in the last few years, it has already mounted a serious, if not fatal, challenge to policy activism. In contrast to policy activists, economists in this second camp believe that the business cycle and swings in unemployment are compatible with equilibria in stochastic general equilibrium models, where the agents in such models exhibit optimizing behavior. They also believe that stabilization policy, which attempts to systematically offset shocks to aggregate demand will on average be correctly anticipated by the public and will thereby prove ineffective.

In this paper we summarize first the views of policy activists and next the arguments of the second camp which were contained in Sargent-Wallace [12]. We conclude by reporting the criticisms which policy activists have leveled against the arguments contained in that paper.

II. Activist Views on Monetary Stabilization Policy

In standard economic models, households are assumed to maximize expected utility and firms are assumed to maximize expected profits, where all objective functions depend on real quantities. Money in these models is a nominal appendage; that is, all production possibilities and goods distributions which are feasible in the monetary economy would also be feasible under an exchange system without money. Keynesian economists have had to answer how jiggles in a nominal appendage can lead to more production over time. They have offered two explanations.

One explanation is that certain price variables are fixed or sticky over periods of time; thus, changes in nominal demand caused by changes in the stock of money lead to adjustments in real output. Among the price variables mentioned in this regard are wages and nominal interest rates. Notice that if these fixed prices are a result of actions of optimizing agents, this explanation does not constitute a theory. A theory would explain why optimizing agents fix prices over time. Moreover, if prices are set based on anticipations of what policy will be over the period, then it is possible that even with sticky prices, monetary policy would not have real systematic effects.

Some prices are fixed by law, however. A primary channel by which monetary policy is thought (by Keynesians) to affect real output is through

† Note that numbers in brackets [ ] correspond to the reference list on page 63.

the housing sector.† Because of the regulatory environment in which financial intermediaries operate, important deposit and mortgage rates are not always free to adjust, and housing credit must at times be rationed. Regulations which inhibit deposit and mortgage rate adjustments to changing market conditions include state usury laws, Regulation Q, and restrictions on types of assets and liabilities which can be held by thrift institutions. In order to argue that monetary policy affects aggregate real output through the housing market, it still must be shown that housing fluctuations caused by policy changes are not fully offset in other sectors of the economy. However, this last step seems like an easy one to take once it is accepted that a carpenter, for example, is more productive working in the construction industry than in other sectors of the economy.

A second explanation Keynesians give for the potency of monetary policy is that agents in the private sector make systematic prediction errors. Policy works according to this explanation by continually frustrating agents' expectations. If labor units bargain for a nominal wage including an inflation premium based on adaptive price expectations, then an expansionary monetary policy can lower the real wage and increase employment and production by pushing the aggregate price level higher than expected. This second explanation is once again not a theory. It does not give any rationale for why agents do not predict optimally based on the information they have.

Even if we accept these two explanations of why monetary policy can be expected to have systematic real effects, it does not necessarily follow that countercyclical policy is optimal in a broad sense. It might follow, instead, that the regulatory environment should be changed and that the monetary policy rule coupled with unconditional, unbiased forecasts should be announced. Nevertheless, if we take the regulatory environment and the secrecy of policy as "givens" of the problem, then Keynesian economists would argue for using control theory applied to the "best" model from the current genre of macroeconometric models in order to make monetary policy.

III. The Sargent-Wallace Arguments and Subsequent Activist Criticisms

We now turn to a brief and nontechnical summary of ideas from the recent literature that claim to, or may be interpreted as claiming to, challenge activist monetary policy. We will attempt to represent these notions by at least heuristic reference to a specific simple macro model with built-in rational expectations used illustratively in some of the Sargent-Wallace papers.

†See deLeeuw-Gramlich [2] and Pierce-Graves [10].

The "structure" of the model is

$$(1)\ y_t = a_0 + a_1 y_{t-1} + a_2(m_t - m_t^*) + u_t$$

$$(2)\ m_t = g_0 + g_1 y_{t-1} + e_t$$

$$(3)\ m_t^* = E(m_t | I_{t-1}).$$

In (1) y_t is some real variable of policy concern (say, unemployment rate though it could be deviation of actual output from trend) and m_t is a policy instrument (say, money supply though it could be inflation rate). The a_i's are fixed parameters, and u_t is a random variable. The equation represents an economy whose real sector is driven by three active factors:

- Its own momentum (y_{t-1}).
- The achieved value of the policy instrument (m_t) *but only* to the extent that the achieved value differs from what was expected by the agents for that period (m_t^*) viewed from the immediately prior period.
- Nonsystematic elements reflected through u_t.

Equation (2) represents the policy-setting process involving a feedback rule with two parameters, g_0 and g_1, an observation on last period's unemployment rate, and a random "miss," e_t. Where the policy objective is to minimize the variance of y, for example, the settings of g_0 and g_1 that will guarantee this achievement can be determined from knowledge of the parameters of (1). That follows, of course, from solving a straightforward minimization problem within this model.

Equation (3) represents agents' expectations about the policy variable, m_t. Rational expectations are here portrayed as the mathematical expectation of m_t given I_{t-1}: knowledge of (2) and information about the true value of y_{t-1}. Since the policy maker is assumed here to have settled on values for g_0 and g_1, agents know everything in the policy maker's response function (2), except the random element, e_t. Since e_t is known to be representable as randomly drawn from a distribution with mean zero and fixed variance, the forecast m_t^* under rational expectations is by (3)

$$m_t^* = g_0 + g_1 y_{t-1}.$$

That, of course, means that in (1), which tells us how the real economy is driven, the magnitude of the "surprise" element reflecting policy, $a_2(m_t - m_t^*)$, is equal to $a_2 e_t$, an unsystematic and unpredictable part of the policy maker's decision process.

This simple representation of a rational expectations economy can be used to illustrate the nature of various types of criticism we can draw from the literature. The model could be elaborated and made empirically richer without altering the key lessons. We've sorted criticisms into three categories to be discussed in subsequent sections under separate author-

ship. In the first category, which will be discussed in Section IV, are questions essentially about the validity of using (3) — mathematical expectations — to represent agents' forecasts of the policy variable. A contrasting point of view is that people aren't as clever or knowledgeable or well informed as such a representation requires. If they aren't, then the impotency of policy as modeled may incorrectly reflect characteristics of the real-world economy.

A second category of criticism, discussed in Section V, has to do with an assumption of price and wage flexibility implicit in the model. For the moment, think of m_t as a price inflation variable and y_t as the (real) supply decision. The model assumes that decisions of agents as per (3) utilize all of the information available to the policy maker through time $t-1$. However, if agents are constrained — whether because of contractual obligations or for other reasons — from immediately making the price or output setting that some of the I_{t-1} would otherwise dictate, then the policy innovation $a_2(m_t - m_t^*)$ in (1) need not wash out and some role would appear for predictable policy effects on real variables. This is not to say that agents are incapable of making unbiased forecasts of m_t on the basis of I_{t-1}. The point is that it may be an irrelevant exercise, since they entered into contracts on the basis of forecasts made conditionally on I_{t-2}.

A third category of criticism seeks to raise fundamental questions about the validity of models in which only the "surprise" part of monetary variable settings, $(m_t - m_t^*)$, can have an effect on real variables. Money serves no exchange role in the rational expectations model. Would the level of money — in addition to the surprise part — have an effect on real variables in a model where money is useful as a medium of exchange?

IV. Expectation Formation

This section looks at some ideas from the literature that relate to the question. What if expectations are not formed in the neatly mathematical way specified in rational expectations models?

One competing possibility is that there are information differences among agents or between agents and the policy authority. We should take note there are two more or less distinct aspects to the human decision-making process. One is cognitive knowledge, and the other is information. Note, too, that knowledge or information as possessed may be partially or totally in error. Existence of differences in knowledge among participants in an economy could have quite different implications from existence of differences in information. Obviously, costs of acquiring knowledge and the amount of time involved can be very different from that associated with acquiring information.

Sargent [11] treats information differences, discussing what he terms "partly rational" expectations, in an article demonstrating the usual rational expectations results. The usual model, as presented, supposes

that rational agents' expectations of m_t^*, say, are formed by preparing linear regressions on all past "conditioning variables"— call this the full information set I_{t-1}, for which we write $m_t^* = E(m_t | I_{t-1})$ as in (3). He then considers a more limited information set, say, I'_{t-1}. If agents form expectations using $E(m_t | I'_{t-1})$, then in general, the "surprise," $m_t - m_t^*$, will contain some systematic elements that the monetary authority knowing I_{t-1} can use in a limited way. The kind of play given to the monetary authority, Sargent emphasizes, is not such as to make it possible for the policy maker to achieve a chosen target for the real variable with any regularity, but is (on average) only the power to determine a greater or lesser variance for the real variables of the model over some run of time.

But there are other ways to conceive and model "partly rational" behavior. To say that all agents perform least squares regressions using complete or large information sets (or act equivalently) calls for a form of sophistication not thought to be representative of the real world by many economists.

James Tobin [14], commenting on Lucas's paper [7] at the Board's 1970 Price Determination Conference, exemplifies these views:

> Lucas's paper provides a rigorous defense of the natural rate hypothesis, and the study's rigor and sophistication have the virtue of making clear exactly what the hypothesis requires. The structure of the economy, including the rules guiding fiscal and monetary policy, must be stable and must be understood by all participants. The participants not only must receive the correct information about the structure but also must use all of the data correctly in estimating prices and in making quantitative decisions. These participants must be better econometricians than any of us at the Conference. If they are, they will always be — except for the unavoidable mistakes due to purely random elements in the time sequence of aggregate money demand — at their utility — and profit-maximizing real positions. These positions are invariant to any systematic changes in the sequence of aggregate money demand, either in the level of such demand or in any of its time derivatives.
>
> Once again, a pragmatist might conclude that he agrees with the natural rate hypothesis in principle but also believes that, in as long a run as can be of concern to policy-makers in an uncertain and changing world, a trade-off does exist for policy-makers as well as for statisticians.

Suppose only some agents in an economy are representable as rational in the foregoing sophisticated sense. Others are more limited, myopic, or even stupid. Expectations for the economy as a whole, then, are in some sense "mixed" rational and nonrational. Following the formulation in Robert Gordon [3], we may depict aggregate expectations formation relevant to (3) as a simple weighted-average of naive, adaptive expectations (for the forecast value, use last period's value), and rational expectations $m_t^* = \lambda m_{t-1} + (1-\lambda)E(m_t | I_{t-1})$.

The result of such a formulation is to more or less restore to monetary policy its short-run potency, depending on how close λ is to 1 or to 0. Rational expectationists may argue that decision makers who use $m_t^* = m_{t-1}$ are not making optimal forecasts and will, if in business, lose out to those who forecast using $m_t^* = E(m_t | I_{t-1})$. That may certainly indicate a mode through which actions among some subsets of economic players will tend to move λ in the direction of zero. But that provides no basis for judging that λ is now (or indeed will be evermore) *at* zero. Empirically, by appeal to casual observation it is clear that some major sectors of decision makers (consumers) face no terminal consequences to persistent use of poor or wrong forecasts. Again, it seems clear that some, if not many, businesses survive over considerable periods guided by seat-of-the-pants judgments in which endemic systematic biases are neither ruled out by formal logic nor prohibited by sudden-death extinction. In fact, over some periods of time biased or suboptimal forecasts could outperform unbiased, optimal forecasts merely as a result of chance. It seems clear that a plausible Darwinian model of the business sector could be constructed that would generate systematic bias in the aggregate even though any firm would eventually be eliminated if its decision-process results (however arrived at) deviated greatly from those of optimal forecasting firms.

Richard Cyert and Morris DeGroot [1] discuss a model in which learning takes place through feedback of information from the market. If firms do not know the form of the model of the process that determines price and in fact use a 'wrong' model (an "inconsistent" model), then learning from market information can lead to an equilibrium, though the process may converge slowly or even not at all. "Consistent" models, as Cyert and DeGroot term them, are those for which parameters may be unknown but the correct form is known. With Bayesian learning, consistent models lead progressively toward a rational expectations world. While that type of stylized learning experience can be fairly readily formalized, the step that carries firms from wrong models to the correct model is less tractable. The authors say only: "If firms have models that diverge drastically from reality, it seems reasonable to assume that management would recognize this condition and change the model. The firms would continue searching for a model that produced predictions that coincide more closely with actual observations."†

Other authors have looked at possibilities for systematic, real effects of monetary policy during a learning period in which the public adjusts its prior beliefs (assumed perhaps to be suddenly wrong, possibly because some structural change has occurred). John Taylor [13] talks of transitional expectations in such a setting. As is by now obvious, if the agents start with a false model yielding biased predictions and only gradually adjust this through learning as new data flow in, then the Phillips curve

†See Cyert-DeGroot [1].

only gradually becomes unexploitable during the transition and monetary policy can systematically influence real variables during transition.

A premise underlying all of these learning-transition papers is that the natural resting point toward which the adaptive expectations process will move (regardless of whether the rate of transition is fast or slow) is rational expectations. That contention, we should note, is an empirical matter, and it is not at all obvious that such a proposition is true or even approximately true. If it were, Taylor among others points out, the policy-during-transition issue is more or less hollow. If the agents' forecasts are consistently biased merely because they do not have some of the information (or knowledge of new structure, say) that the policy authority possesses, then the policy authority has two policy options: (a) keep the information secret and work hard to exploit the gap or (b) make all of the information public and relax. Whether the latter approach is in some higher sense effective, Taylor points out, depends on the cost of distributing and using information. The latter notion serves to remind us that even in a micro-optimizing, rational expectations model, costs in acquisition and use of information (and knowledge) may drive a wedge between m_t^* and full information $E(m_t | I_{t-1})$.

But even if there is no wedge between m_t^* and full information $E(m_t | I_{t-1})$, the simple form of the rationality model discussed here does permit policy to affect real variables when the natural rate hypothesis does not hold exactly. That is, even when expectations are formed according to (3), policy could systematically affect y if (1) were of the form

$$(1') \qquad y_t = a_0 + a_1 y_{t-1} + a_2(m_t - a m_t^*) + u_t$$

where $a \neq 1$.

A number of theoretical conditions seem to argue that (1′) is a better description of output response to policy than (1). Agents who are risk averse or who have loss functions which are asymmetric with respect to the policy outcome could generate an output response like (1′). So could agents who act as if they are playing a game against the policy authorities and follow, say, a minimax strategy. In these cases the argument that the government needs to "fool" agents in an economy in order to make policy have real effects does not carry a pejorative coloring when agents are maximizing utility. They may be looking at more than the first moment of the distribution of outcomes.

But there are two important reasons for *not* pushing this criticism of the rationality model too far. First of all, the model discussed here focuses only on the first moment of policy actions on agents' decisions (although the variance of policy does affect the variance of output). A more general version of the rationality model requires that agents' subjective probability distributions of the policy process be equal to the actual

distribution generating the policy outcomes. Thus, all moments of the relevant distributions enter the agents' decision process which generates y so that, when viewed from a system perspective, the more general models support the case for (1) rather (1′).

Second and perhaps more importantly, in none of these models is it clear that a policy which can systematically affect real variables (fool agents) is capable of producing an increase in welfare relative to a policy which never systematically fools anyone. That is, optimal policy is one which minimizes uncertainty for all agents.

V. Price Flexibility and Persistence

In this section we will focus on the basic model described earlier as

$$(1) \qquad y_t = a_0 + a_1 y_{t-1} + a_2(m_t - m_t^*) + u_t$$

$$(2) \qquad m_t = g_0 + g_1 y_{t-1} + e_t$$

$$(3) \qquad m_t^* = E(m_t | I_{t-1})$$

where we now take y to be the unemployment rate and m to be the inflation rate.

One of the fundamental premises underlying the theory of rational expectations is that prices and nominal wages are "flexible" in the current period. Flexibility, as modeled in (3), means that agents act on the basis of expectations about the current period values of these variables, and the expectations are determined endogenously rather than being predetermined by actions or events of earlier periods. The accordance of this premise with reality has been questioned by a number of persons.

Edmund Phelps [9] argues that many current period prices are set well in advance of the current period.† For example, goods marketed in period t will be priced according to advertisements, catalogs, etc., that were printed in period $t-1$. And the firm had to issue printing orders on the basis of information known at the end of period $t-2$. Phelps also points out that this lag in price setting is of no particular importance to the rational expectations theory as long as the policy authorities are making their decisions on the basis of the same information as the firms. However, it seems reasonable to suppose that policy decisions are conditional on information available through period $t-1$. This difference in information, Phelps argues, will generally produce an optimal feedback-type policy rule.

Arthur Okun [8] also argues against price flexibility as modeled by (3)

†It might also be noted that Phelps [9] appears hesitant to accept the natural rate hypothesis, since he prefaces his remarks with the statement, "If the level of the expected inflation rate really made no difference for any variables, like the desire to work or save, many discussions of monetary policy-choice would never have taken place."

by asserting that search costs and tradition are very important elements of economic life. In a world of "customer" (as opposed to auction) markets, it is quantity which adjusts to demand shifts rather than price. And in a world of "career labor markets," wage policies are largely determined by tradition rather than current conditions.

Phelps [9] also points out that one premise of the theory of rationality is that the dispersion of the conditionally predicted inflation rate does not affect the short-term equilibrium values of real variables. This point is related to (3). Phelps argues that higher-order moments may indeed matter, especially in a multi-period setting. For example, a fixed rate money supply rule may cause a greater variance in the inflation rate in the current period than some feedback rule, but the variance 10 years out may be smaller. Thus, to the extent that variance affects agents' decisions, the particular policy rule may affect today's decisions.

Robert Hall [5] argues that the issue of wage flexibility is of critical importance in understanding the behavior of unemployment. He focuses on wages in the "nonentrepreneurial" sector (government, nonprofit institutions, and regulated industries) and claims that the sluggishness of wages in this sector causes the distribution of wages to widen when demand falls. Employing a search model of unemployment, Hall argues that when wage differentials widen during a contraction, the extra unemployment associated with the contraction is the result of optimal supply behavior on the part of the unemployed as they join queues for good jobs in the rigid-wage sector rather than accept lower paying work in the competitive sector. Hall is led to this search explanation of unemployment because other theories appear unable to adequately explain the persistence of unemployment. Once the unemployment rate moves away from its equilibrium value, it tends to stay away (on the same side) for a number of years.

In particular, Hall argues that the rational expectations theory is unable to explain the persistence of unemployment. If in the above model we let $a_1=0$, then (1) can be viewed as a Phillips curve which may be written as

$$y_t = a_0 + a_2(m_t - m_t^*) + u_t$$

where a_0 is the mean of the unemployment rate. Let $v_t = m_t - m_t^*$ be the innovation in prices. Then the unemployment rate path may be described by

$$y_t = a_0 + a_2 v_t + u_t.$$

Written in this form, it is clear that the rational expectations model can easily "explain" the unemployment rate at any point in time via innovations or random shifts in the Phillips curve. However, there is nothing in

the model to explain why y should remain on the same side of y^* for extended periods of time.

One possible explanation is that u is serially correlated. But this is not a satisfactory explanation because u is a variable which represents that which is not explained by the theory and relabeling u as persistence still leaves persistence unexplained. Similarly, if $a_1 \neq 0$, then the model will produce persistence, but there is no reason to expect this in the rational expectations framework. Thus, persistence of unemployment is another one of the stylized facts that is still without explanation.

VI. Money as a Medium of Exchange

The final criticism we raise applies to all macroeconomic models and is really a sad commentary on the state-of-the-art. The criticism is that we do not have a theory of money as a medium of exchange. There does not exist an acceptable model of an exchange economy in which the use of money allows production possibilities or goods distributions that are not also feasible under a nonmonetary, classical auctioneer system where all transactions occur at a single point in time. Hence, the good called money is not essential in describing the equilibria of these modeled exchange economies.†

Before examining what is required to construct an acceptable theory of monetary exchange, let us look at what the lack of such a theory implies about the Sargent-Wallace model. The aggregate relationships they posit can be derived from Lucas's "Expectations and the Neutrality of Money" model [6]. Lucas's model consists of n goods and m agents who maximize objective functions dependent only on real quantities of the n goods. Each agent lives for two periods, working in the first period and retiring in the second. Thus, in each period there exists two generations of agents, workers and retirees. The n goods are not storable, however, so by introducing money as the $(n+1)$th good and making it storable, it is given a useful role. Money is useful because it allows some intertemporal, intergenerational exchanges which would not be feasible if there were no storable good. Yet, money is not useful as a medium of exchange because exchange is not assumed to use up resources. It seems clear in Lucas's model that in each period the usefulness of money as a store of value is maximized by making its exchange rates for goods in the future period as predictable as possible. This is accomplished by making the rule governing the quantity of money deterministic and known by the economic agents. Seen in this light it is not surprising that Lucas and Sargent-Wallace find that one deterministic rule for the money supply is as good as any other. But would this implication follow from a model of a monetary exchange economy?

†For a fuller discussion of what it means for money to be "essential," see Hahn [4].

Economists a long time ago recognized that the use of money facilitates trade. At least three advantages are claimed for a monetary economy over a nonmonetary system of exchange:

- It allows a reduction in resources committed to the activity of exchange.
- It eliminates the double coincidence of wants (that is, an individual who wants to trade bread for shoes does not have to find someone willing to trade shoes for bread).
- It allows specialization of labor in production.

The problem economists face is in trying to formalize these intuitive notions. A model of the economy which attempts to formalize these intuitive notions should include costs to transacting, uncertainty about future prices, economies of scale to labor specialization, and absence of some markets. The nonconvexities (increasing returns to scale, etc.) inherent in such an economy are in stark contrast to the nice properties exhibited by standard competitive models without money. The feature that some markets are absent implies that a complete theory of money should include a theory on the existence of markets. In other words the existence of markets should be endogenous in a model with money.

Since we are so far away from having a theory of money, does it make sense in macroeconomics to talk about the optimal quantity of money or about the optimal monetary policy rule? If the answer is "yes," then we should at least make an attempt to gauge the impact of different monetary policies on the usefulness of money as a medium of exchange.

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