The Rational Expectations Revolution: An Assessment

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A Revolution?

My task is to write on the "Rational Expectations Revolution." I would not have chosen that title myself. One reason is that I have previously argued partly that the conception of the development of science as periods of "normal" problem solving punctuated by revolutionary upheavals is inappropriate to economics and possibly even to physics (Hoover 1991a). And yet, since Thomas S. Kuhn's *Structure of Scientific Revolutions* (1962), this conception has percolated down from physics through the physical and biological sciences finally to reach economics. I think there is more continuity and mutual comprehension between the putative revolutionaries and the *ancien régime* than Kuhn's account of scientific revolutions supposes. More than that, even though the rational expectations hypothesis is a critical element in its early development, the essence of what I prefer to call the "new classical macroeconomics" is found in the assumption that agents are consistent and successful optimizers, that all markets clear. The rational expectations hypothesis is a subsidiary theme in recent developments in the new classical school. That is not to say that I think the rational expectations hypothesis can be ignored: *liberté, egalité, fraternité* are still emblazoned on French coins even though they are subsidiary to the process and final outcome of the French Revolution.

Reservations to one side, I am not insensitive to the dramatic changes in academic macroeconomics over the past 20 years. Neither of the two schools of thought that dominate macroeconomics would

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be what it now is without the events surrounding the introduction of the rational expectations hypothesis into macroeconomics. (It is interesting to note that for its first decade even the new classicals identified themselves as "rational expectationists."[1] I will, therefore, indulge the revolutionary metaphor, recognizing, however, that the rational expectations hypothesis itself is not what will command our attention; instead we will examine the implications of those developments in macroeconomics for which the rational expectations hypothesis was one of the sparks. In the following impressionistic sketch of the history of those developments, I will pay special attention to the implications for forecasting and policy.

Tinder

The macroeconomics of the 1930s was a rich tapestry. The debates among Keynes, Hicks, Hayek, Hawtrey, Robertson, Robbins, Meade, Myrdal, and others were subtle and full of detail. Careful, thoughtful discussions of economic behavior and the role of expectations were prominent. Most of the debates were carried out in words and not in mathematics. Much of the richness of those discussions was lost in the subsequent developments. Keynesian economics, as it was known after the 1940s, especially in the United States, owes more to Richard Stone and Jan Tinbergen, the fathers of quantitative macroeconomics, than to John Maynard Keynes.[2] Keynes himself was deeply skeptical of Tinbergen's effort to construct a closed, empirical model of the economy (Keynes 1939). And the characteristic prescriptions of Keynesian macroeconomic policy—deficit finance, redistribution, and discretionary monetary policy—find precious little direct support in *The General Theory*.

The early Keynesian macroeconomics was cut from a plain cloth. Simple linear equations, estimated using elementary econometric techniques, connected a few macroeconomic aggregates. Simple forecasts and broad-brush analyses of policy were conducted. But economists were not content with these austere garments and began quickly to embroider them. First, macromodels grew in size and complexity: Where Tinbergen's model of the U.S. economy had 48 equations, the Federal Reserve's MPS Model has 334.[3] Second, economists began to explore the microeconomic behavior that was

[1]The first reference that I know of to a new classical school is in Chapter 16, "Aspects of the New Classical Macroeconomics," of Thomas Sargent's (1979) textbook.

[2]On the general need to distinguish Keynes from the Keynesians, see Leijonhufvud (1968).

[3]See Morgan (1990, p. 115) and Brayton and Mauskopf (1987, p. 93).

presumed to lie behind estimated macroeconomic relations. The work of Milton Friedman and Franco Modigliani on the consumption function, of James Tobin and William Baumol on the demand for money, and of Don Patinkin on general equilibrium foundations for macroeconomics is typical of these developments.[4] Third, economists began to pay more careful attention to the processes through which expectations were formed. A first pass, found for example in Milton Friedman's (1957) theory of the consumption function or Phillip Cagan's (1956) work on money demand in hyperinflations, held that the expected value of a variable was formed on the basis of its history. Extrapolative expectations were empiricially tractable but theoretically unattractive because they were not obviously grounded in sensible economic behavior. By the end of the 1950s, serious research on the formation of expectations was under way. Herbert Simon and John Muth, both at Carnegie-Mellon University, developed divergent approaches.[5] Simon's work led to his theory of bounded rationality, while Muth's ended in the rational expectations hypothesis.

Muth noticed that dynamic economic models use expectations of variables as inputs into decisionmaking processes and, at the same time, generate predictions of the values of those same variables. If the model is correct and if people use the model to make predictions, then consistency should dictate that people should, in fact, expect (and use as an input into their decisionmaking processes) precisely what the model tells them will occur. For Muth, a rational expectation is one that is consistent in this sense. His insight has been refined in a number of ways: Expectations are rational if (a) they are formed as if people knew the true model, and (b) they are correct up to a serially uncorrelated error. That is, prediction errors are unsystematic. To take Muth's own definition, "[T]he subjective probability distribution[s] of outcomes tend to be distributed, for the same information set, about the prediction of the theory (or 'objective' probability distribution of outcomes)" (Muth [1961] 1981, pp. 4–5).

Muth used the rational expectations hypothesis to solve problems in the dynamics of agricultural markets. It was largely neglected for a decade. The honor of introducing it into macroeconomics belongs to Robert Lucas. Lucas is a scion of the monetarist class, which, at the time he was just entering his intellectual majority, thrived almost exclusively at the University of Chicago.

[4]See Friedman (1957); Modigliani and Brumberg (1954); Tobin (1956, 1958); Baumol (1953); and Patinkin (1965).

[5]See Sheffrin (1983, pp. 1–5) for a short account.

Keynesian economics is founded on the notion that there are often unused resources in the economy. This notion is usually described as disequilibrium in the market for labor: There is involuntary unemployment; the supply of labor is greater than the demand for labor. The Keynesian economics of the 1950s was notably weak in accounting for the dynamics of inflation. Paul Samuelson and Robert Solow (1960) imported the inverse relationship between inflation and unemployment discovered by A. W. Phillips (1958) to fill the gap in the Keynesian model. The Phillips curve appeared to be an empirical fact; its slim theoretical foundations were found in labor market disequilibrium.[6]

Monetarism rejected the central basis of Keynesian economics. The economy does not have unused resources in equilibrium; and, in the long run, the economy returns to equilibrium. Inflation is "always and everywhere a monetary phenomenon." Monetarism is a doctrine steeped in irony. Its theoretical core is the quantity theory of money, which implies that, in the long run, money is neutral: Money is a veil, which masks, but does not alter, real resource allocation in a smoothly functioning economy. Yet, as the name "monetarism" implies, money is important. It is, however, only the pathologies of money that matter. For example, according to Friedman (1968), the observed Phillips curve is a result of workers not at first comprehending that higher money wages as a result of inflation were not also higher real wages; they suffered from money illusion. Once, however, they catch on, once their illusions are dissolved, the monetary pathology is cured and money is once again neutral.

Despite their differences, monetarist and Keynesian economics have an important common element: They are both accounts of the behavior of aggregates. Lucas and Rapping's (1969) article attempts to provide microfoundations for the monetarist labor market. In their article, employment is determined as an equilibrium; the supply of labor equals the demand for labor. The supply of labor, in turn, is derived from individual utility maximization, while the demand for labor is derived from profit maximization by firms. Although I have elsewhere (Hoover 1988, p. 27) referred to Lucas and Rapping's article as "surely the first paper to deserve to be called 'new classical,' " it is in an obvious sense prerevolutionary. Expectations are crucial in the underlying maximization problems; and Lucas and Rapping model them as *adaptive* expectations, not as rational expectations.

[6]On Phillips's own interpretation of the Phillips curve, see Desai (1975) and Wulwick (1987).

Spark

Muth is the Rosseau of the rational expectations revolution; Lucas is its Robespierre.[7] The rational expectations revolution began with a series of papers in which Lucas worked out the implications of Muth's rational expectations hypothesis for the Phillips curve and the analysis of labor markets. Lucas's ([1972a] 1981) article, "Econometric Testing of the Natural Rate Hypothesis," will serve as the paradigm.[8] The article accomplished three things critical to the development of new classical macroeconomics.

First, it undermined the standard interpretation of the empirical evidence about the Phillips curve. A frequent test of the expectations-augmented Phillips curve or the natural-rate hypothesis before Lucas was to check whether the sum of the coefficients on lagged inflation summed to zero in a regression of unemployment (or output) on inflation. The idea was that the economy tended to return to full employment equilibrium (the natural rate of unemployment) if an inflation (money) shock washed out over time. Lucas showed that one should expect such a result only when expectations are rational if monetary policy was purely random. If policy followed any kind of dynamic rule (say, a constant growth rate of money), then people's expectations of the future course of money, based on their internalization of that rule, would imply coefficients that do not, in general, sum to zero even when *ex hypothesi* the natural-rate hypothesis is true. Tests that seem to reject the natural-rate hypothesis might, in fact, be providing evidence for it, so long as expectations are rational.

The second accomplishment of Lucas's article was to shift the analysis of policy away from single policy actions, such as were frequently analyzed in the IS/LM framework, to policy rules. The point is partly ideological, an expression of the Chicago commitment to rules over discretion (or authorities) that goes back to Henry Simons (1936). It is, more importantly, a product of the logic of rational expectations: Any policy, whether intentionally rule based or not, can be divided into a systematic component (upon which rational predictions may be based) and a random component (which is pure noise). The noise, although it can lower the efficiency of the economic system by interfering with relative price signals, cannot be useful for policy, because it is unsystematic. The systematic

[7]I, of course, would not push the analogy too far. In particular, I would not attempt to detail a new classical "terror," although there are perhaps a few macroeconomists who think that it could reasonably be done.

[8]Hoover (1991a) gives methodological reasons why it may not matter very much which of many models or articles one takes to be the paradigm.

component can be integrated into the decisions of firms and individuals. Policymakers cannot ignore that there is an interaction between their policy rules and individual behavior. They must account for such interactions in the design of policies.

The third accomplishment of Lucas's article was to undermine the structural interpretation of estimated econometric models. Keynesians had regarded the Phillips curve as a structural relationship. But Lucas showed that, under the rational expectations hypothesis, the coefficients on lagged inflation depended on the policy rule in place. Thus, when the rule changes, the coefficients themselves must change.

Conflagration

From the initial spark of the rational expectations hypothesis, a great fire raced through macroeconomics, consuming almost nothing completely, but singeing everything. The three great themes of the new classical macroeconomics in the 1970s—policy ineffectiveness, dynamic consistency, and the Lucas critique—were the natural extensions of concerns expressed in Lucas's article.

Policy Ineffectiveness

Only the systematic component of policy could be systematically directed toward the policymakers' ends. Under rational expectations, people could not long mistake the systematic thrust of a policy. Therefore, if, as monetarism maintained, the real economy had a tendency to equilibrium, policy could not drive the economy away from that equilibrium. Monetarism implied that only money illusion gave monetary policy a real effect; the rational expectations hypothesis implied that money illusion was fleeting at best.[9] The famous articles of Sargent and Wallace (1975, 1976) make this obvious point. Most of the attacks on the policy-ineffectiveness proposition centered on the natural-rate hypothesis and not on the rational expectations hypothesis (e.g., Fischer 1977 or Phelps and Taylor 1977).

Dynamic Consistency

The policy-ineffectiveness propositions were concerned with the efforts of policymakers to boost output or to lower unemployment (i.e., with the real economy). They worked because the public integrated the policy rules into their own decisions. The rules themselves

[9]I am ignoring, as does this literature, the beneficial effects identified by Friedman (1969) of a policy of systematic deflation or by Tobin (1965) and Mundell (1971) of a policy of systematic inflation.

were autonomous—independent of the actions or reactions of the public—and were treated as if, once set, they remained in place for all time. The new classicals extended the analysis of rules: First, the policymaker was treated symmetrically with the public—policy rules were analyzed as the outcome of an optimization problem; second, the range of policymakers' concerns was extended beyond the real economy to include an aversion to inflation;[10] and third, rules were analyzed in richer dynamic settings. To take the last point first, Finn Kydland and Edward Prescott (1977) pointed out that, when choosing an optimal rule in a dynamic setting, policymakers may be subject, in addition to feasibility constraints, to a requirement that policy be dynamically consistent. For example, suppose that, to maximize one's child's welfare, a parent proposes the policy that, if the child comes home after midnight, her legs will be broken. If, in fact, the child appears after midnight, such a policy would hardly seem optimal at all, for the child with rational expectations sees no cost in staying out past midnight. The policy is dynamically inconsistent. Robert Barro and David Gordon (1983a, 1983b) *inter alia* analyze monetary policy in an environment in which the policymakers lose utility from both higher unemployment and higher inflation. Permanently positive inflation may result in such a model because, even though inflation cannot permanently decrease unemployment, it is not dynamically consistent for the policymakers to eschew the temptation to attempt to fool the public for a gain of temporarily lower unemployment.[11]

The Lucas Critique

Lucas (1976) generalized the message of his "Econometric Testing of the Natural Rate Hypothesis": Aggregate, econometrically estimated relationships are not, in the presence of rational expectations, invariant to changes in policy regime. The point, however, goes beyond the rational expectations hypothesis. In general, if people are optimizers, the higher-order implications of their optimization problems are not likely to be invariant to changes in the environment. Invariance can be secured only if one gets down to the level of the underlying optimization problem, taking only tastes and technology as given.[12]

[10]Exactly why inflation should be seen as costly in typical new classical models was unclear since money was essentially neutral.

[11]See Rogoff (1989) for a review of the literature.

[12]It is difficult to know where one can stop, since both tastes and technology have been analyzed as objects of choice rather than as primitives.

The upshot of these three themes for forecasting is devasting: Using large-scale macroeconomic models to make conditional forecasts makes no sense. First, these models are, at best, incorrectly specified, because they do not consistently integrate rational expectations, even at the aggregate level.[13] Second, policy experiments are incorrectly conceptualized in them—issues such as dynamic consistency were not faced. Finally, they simply are not accurate representations of economic structures that would remain invariant to changes in the policy regime; therefore, they could not be used to forecast changes that are conditional on a new regime.

From Republican Virtue to Faction

The new classical macroeconomics self-consciously embraces a foundational myth, laying claim to the legacy of pre-Keynesian neoclassical economists (see, e.g., Lucas [1977] 1981, pp. 215, 217; [1980] 1981; and 1987, p. 47). It seeks to erect a republic of virtue on the principles of an earlier age—a macroeconomics that is strictly based in the microeconomics of resource allocation. The implications for forecasting are grave. Consider two types of forecasts. People adjust their behavior to weather forecasts (they take umbrellas to work and they cancel picnics), but their behavior does not change the weather. On the other hand, when water-use forecasts suggest that demand may outstrip supply, people often adopt conservation measures that will ensure that actual outcomes diverge from the initial forecast—indeed that is the point of the forecast. Keynesian macromodels implicitly assumed that their forecasts were like weather forecasts: All agents (except, notably, the government) could treat the forecast as independent of their own behavior. The new classical economists explicitly assume that all forecasts are like water-use forecasts. Recognizing this distinction suggests severe difficulties in applying standard econometric techniques to macroeconomic models. The new classical macroeconomics has fractured into factions defined by their methodological response to these problems.

Lars Hansen and Thomas Sargent ([1980] 1981) and others treat the problem as a failure of econometricians consistently to enforce the implications of rational expectations in their estimation of large macromodels. Thus, they attempt to answer the Lucas critique by deriving econometric specifications from underlying optimization problems, taking only tastes and technology as given. They then

[13]It should be recalled that Sargent and Wallace's policy-ineffectiveness results were derived in an IS/LM model with rational expectations, and not in a model with consistent microfoundations.

estimate these equations using extensions of standard econometric techniques that account for the nonlinearities introduced by rational expectations (the cross-equation restrictions). They appear to be happy if their models are not, in Sargent's memorable phrase, "obscenely at variance with the data" (Sargent [1976] 1981, p. 547).

Obscenely or not, models of the Sargent and Hansen type are almost always at variance with the data. Kydland and Prescott (1990) and Lucas (1987) regard this as the inevitable result of the failure of the microeconomic theory to map properly onto the macroeconomic data. The world is too complex for a model to possess both sound theoretical foundations and sufficient detail to describe data accurately. Where Sargent and Hansen attempt to save the project of macroeconometric modeling through improved technique, Lucas and Prescott wish to adapt the viewpoint of the engineer to save policy analysis.[14] They see models as schematic representations that, while not realistic, are nonetheless similar enough in key dimensions and, above all, are tractable so that the implications of a policy can be worked out in all relevant detail. Lucas and Prescott see Sargent and Hansen's approach as impossible to implement; Sargent sees Lucas and Prescott's approach as empirically unsupported.[15] Where Sargent and Hansen attempt to perfect the forecasting enterprise, Lucas and Prescott give it up altogether.

A third faction saves the traditional macroeconometric models at the expense of their structural interpretation. Christopher Sims (1980, 1982, and 1986) argues that if policymakers are part of the economic process, so that policy is endogenous, then in some sense there are no changes of regime. Rather, what appear to be regime changes are just different realizations of a stable supergame.[16] Thus, while the Lucas critique is correct in principle, the coefficients of macromodels rarely change, and the models may be used for forecasting. The only problem is that the reason one wants to have forecasts in the first place is to guide changes of behavior. Here forecasts are valid, but only because they do not induce changes in behavior. Macroeconomic forecasting models are saved; policy is emasculated.

[14]One should not be misled by the fact that the business-cycle models of Kydland and Prescott do not model policy at all. These are prototypes of models that are meant to be used to address policy issues—this much is evident from Prescott's methodological musings. Lucas's (1987) calibration exercise, assessing the costs of the business cycle, is clearly policy evaluation.

[15]A fuller discussion of this debate is found in Hoover (1991b).

[16]This overstates Sims's position. He actually says that regime changes are rare, not unknown. Cooley, LeRoy, and Raymon (1984a, 1984b) take a starker view. See Hoover (1988, chap. 8, sec. 8.4) for an extensive account of the issues.

As well as factions based on methodological difference, the new classical macroeconomics is also fractured over issues of substance. A principal goal of new classical research since the mid-1970s has been to provide a satisfactory account of business cycles. The early business-cycle models of Lucas ([1972b] 1981, 1975) and Barro (1976) were monetary models: Unanticipated changes in the money stock initiated the cycle, while the dynamics of capital-stock adjustment propagated it over time. In the late 1970s, empirical investigation of unanticipated money as the driving variable in business cycles held the center stage in macroeconomic research.[17] Although there has been no absolutely decisive result, the weight of evidence is now against unanticipated money as the source of cyclical fluctuations. Real-business-cycle models (models in which the source of cyclical fluctuations is shocks to technology) now predominate. Latent monetarism, however, keeps some new classicals searching for alternative ways to model the influence of money.[18]

Legacy

The whole of academic macroeconomics is touched by the rational expectations hypothesis. Although it has been criticized on conceptual grounds and as empirically inadequate, the rational expectations hypothesis sets a noncontroversial standard for modeling expectations in macroeconomics.[19] Alternative approaches are either out of the academic mainstream (e.g., approaches based on Herbert Simon's notion of bounded rationality) or are positively shunned (all adaptive and extrapolative approaches). The rational expectations hypothesis holds the field by default. Beyond this, the rational expectations revolution was accompanied by, and to some extent caused, an increase in the technical sophistication of academic macroeconomics. Whether or not one thinks this to be a good thing, a comparison of randomly chosen articles in macroeconomics from journals circa 1965 and 1990 will confirm a revolution in formalization.

Still, for all of the upheaval in academic macroeconomics, practical forecasting and policy analysis have been largely untouched. When Congress, the Federal Reserve, the Treasury, corporations, or newspapers want a forecast, they turn to macroeconomic models (larded, of course, with a dose of professional judgment) that are only marginally

[17]See, for example, Barro ([1977] 1981, [1978] 1981); Mishkin (1983). Hoover (1988, chap. 8, sec. 8.2) summarizes the evidence.

[18]See, for example, Lucas (1987, secs. 6 and 7).

[19]There are, of course, some critics, for example, Lovell (1986).

different from those of the late 1960s. Part of the problem is tractability. It is simply very difficult to impose the full implications of the rational expectations hypothesis on models like the Fed/MPS model or the DRI model.

Beyond that, however, the very nature of new classical models reinforces the divergence between practical and academic macroeconomics. Those in government and business usually want to know what next year's GNP or interest rates will be or, at best, what the path will be over some short horizon. To the new classicals such questions are wrongly posed, and their models are not adapted to answering them. Business cycle models are constructed around neoclassical growth models. Typically, it is easier to get answers about "steady states" than short-run paths from such models. Trend rates of growth, or for that matter the levels of real variables, are often modeled as beyond the influence of policy. If the models have money, money is neutral. Most importantly, where decisionmakers generally consider policy actions, the new classical macroeconomics insists on evaluating policy rules. Real-business-cycle models are rarely estimated. Instead, they are simulated with repeated realizations of their random shocks. The output of such simulations is never a forecast of a dated value of a variable. Rather, it is a set of variances and covariances between variables that reflect the operating characteristics of a policy rule. Such models answer a question that decisionmakers rarely ask.

The new classicals have, of course, not had the field entirely to themselves. There is a Keynesian counterrevolution. The new Keynesians revel in the increased technical level of the past 20 years. The rational expectations hypothesis is not a point of controversy. And although they too are no longer involved with large macroeconometric forecasting models, they do take seriously the message suggested by those models: Markets do not clear, sometimes for relatively long periods, and the performance of the economy is often suboptimal. This message is the basis for an academic attack on the new classical macroeconomics. The attack is conducted largely in the style of microfoundations of macroeconomics (i.e., for market failures, it seeks explanations that are consistent with individual optimization). The attack has four main threads.

First, prices or wages are sticky so that increases in aggregate demand are reflected partly in output and employment and are not absorbed entirely in price increases. Justifications of sticky prices or wages include the existence of wage contracts, quality of labor that depends on the wage paid, and costly price adjustment.[20] Second,

[20]See, for example, Phelps and Taylor (1977); Fischer (1977); Akerlof and Yellen (1985,

imperfect competition may produce suboptimal levels of output and employment.[21] Third, economies of millions of people may have difficulties coordinating economic activity in the manner of the Walrasian general equilibrium models preferred by new classicals. Coordination problems may lead to multiple and self-sustaining, but suboptimal, equilibria.[22] And fourth, the monetary system, as opposed to narrow definitions of money, may not be neutral. The problems of financing real activities on credit markets subject to their own upheavals may contribute to the suboptimal use of resources.[23]

The fourth point carries a final irony: For years monetarists insisted against some Keynesians that money matters; now the Keynesians are the ones to insist against many new classicals (whose roots were in monetarism) that money matters.[24]

Republicans and Monarchists

My broad-brush history of the new classical macroeconomics has not been a particularly hostile one. Yet, I think it is easy to see any number of conceptual, methodological, and ideological grounds on which one might oppose the new classical macroeconomics. My suspicion is that there are simpler grounds for opposition; namely, many of the key propositions of the new classical economics are simply not true: Expectations are not, in fact, rational; markets are not perfectly competitive and do not clear; both the policymaker and the public play a pretty unsophisticated game; money is not neutral; and technology shocks do not drive the business cycle. But the truth is rarely simple. So I expect that just as France on the 200th anniversary of its revolution remained divided between supporters and opponents of the revolution, so macroeconomics will long debate the implications of the rational expectations hypothesis and the modes of analysis that followed in its wake.

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1986); Mankiw (1985).

[21]See, for example, Blanchard and Kiyotaki (1987).

[22]See, for example, Cooper and John (1988).

[23]See, for example, Greenwald and Stiglitz (1988); Bernanke (1983).

[24]See, for example, Romer and Romer (1989).

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Rational Expectations and the New Classical Macroeconomics

David I. Meiselman

A Unified Theory

There has been a major change in the content, emphasis, and style at the frontiers of macroeconomics and monetary theory. The result is a sharp paradigm shift in the larger body of economic theory that has largely destroyed the old academic distinctions between macroeconomics and microeconomics. Now, for better or worse, we essentially have one economic theory again.

I emphasize the word "theory" because there is not yet much evidence that the new body of analysis has had much effect, apart from its impact on academic research and teaching. It has essentially left untouched the substantive macroeconomic and monetary questions raised on Wall Street; or on public policymaking inside the Washington Beltway or elsewhere; or on the production, consumption, and authority of economic forecasting exercises. By contrast, the rational expectations revolution and its offspring, the new classical macroeconomics, have had a profound effect on academic teaching, research, and journal publications. One need go no further than skimming the titles of articles in the leading academic economics journals for convincing evidence of the major change.

Demise of the Keynesian System

The rational expectations revolution has cleaned up much of the shambles left by the earlier, now defunct, Keynesian revolution. In retrospect, it should now be clear that much of the Keynesian revolution was so fundamentally and fatally flawed that Keynes

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himself would surely have repudiated it.[1] These exercises and efforts to explain short-period economic disturbances and business fluctuations mocked the very foundations of economics by ignoring its central analytical core, which is derived from utility and profit maximization, market pricing, and market-clearing processes. Those processes include the discounting of actual or anticipated future events such as prices, incomes, interest rates, and public policies. Thus, expectations of future events are central and necessary to the discounting process.

In his intellectual history of the demise of the Keynesian system, Kevin Hoover (1992) emphasizes the empirical weakness of the Keynesian theory and Keynesian models in periods of inflation such as the 1970s, as well as the presumed inflation-unemployment tradeoff of the Phillips curve, which was an adjunct of the standard Keynesian model. But Hoover overlooks the fact that Keynesian theory never really had any predictive content in earlier periods. Almost 30 years ago, well before the inflation of the 1970s and 1980s, Milton Friedman and I (1963) used U.S. data going back to 1897 to demonstrate that the standard Keynesian model was essentially empty. Similar studies were replicated for many other countries and periods and had essentially the same results as our original research. It was not the stagflation of the 1970s that destroyed the Keynesian model and exposed its shortcomings; the model had no substantive or predictive content either before the 1970s or since.

A New Macroeconomics

The present state of macroeconomics and monetary economics leaves many people in a puzzled and dissatisfied state. The rational expectations critique and the new classical macroeconomics were major factors in destroying the old Keynesian macro theories that relied on haphazard, non-optimizing rules of behavior and on presumed and untested empirical rules of thumb. Instead, economists in the rational expectations and the new classical macroeconomics traditions have tried to base a new macroeconomics on standard elements of microeconomics. Those economists have used both

[1]Some years ago in a paper I wrote with Paul Craig Roberts (1979) evaluating the Congressional Budget Office's Keynesian-based forecasting model, I showed that the standard Keynesian model featured "demand without supply, inflation without money, interest rates without capital, outputs without inputs, employment without wage rates or a labor market, and investment without saving or any change in the capital stock." In that model, expectations were static, consumption depended only on current disposable income, and fiscal policy had no supply-side effects.

rigorous price theory and rigorous statistical and econometric tools in evaluating evidence.

Many results of the rational expectations critique have been devastating to formerly received doctrine. Yet, these analyses have little to offer public officials or private sector businesspeople and investors who continue to ask the same serious questions about making and using short-run economic forecasts and about the short-run effects of various public policies, including so-called stabilization policies. Also, the new models generally assume a closed rather than an open economy. They neglect influences to and from abroad—a serious omission, particularly in a world increasingly open and integrated, especially in financial markets.

Impact on the Quantity Theory of Money

By contrast, I do not believe that the rational expectations revolution has had the same dire impact on the quantity theory of money, the old major alternative to the Keynesian model. First, monetarism never attempted to spell out a complete or general equilibrium model for the economy as a whole. Instead, it focused on the role and impact of monetary change and on the determination of the value of money—no small or unimportant chore. The quantity theory distinguished between the effect of changes in the stock of money on real variables in the short run and its neutrality in the long run. The long-run neutrality of money stemmed from the inability of nominal money to systematically affect real variables—apart from its pathology that imposed various deadweight welfare and real income losses. In the long run, with a lag, the stock of money generally affected only the price level. Also, the lags in the effect of monetary change were understood to be both long and variable.

To be sure, some monetarists, both at the St. Louis Fed and elsewhere, responded in part to the demand for forecasts and desired also to present forecasting alternatives to widely cited Keynesian-based forecasting models. Thus they created and generally believed their short-run forecasting models, which, for a time, successfully competed with and even outperformed the Keynesian-based models. Moreover, these monetarist-inspired exercises typically found that monetary change was destabilizing in the short run and neutral in the long run.

Monetarists always distinguished real values from nominal ones. Real variables and relative prices, including fiscal policy tax and government expenditure variables, determined resource allocation and economic growth. The best possible monetary policy kept money

from causing inflation or deflation and from adding to short-run instability. Even the very best monetary policy could not alleviate or cure problems of poverty, of slow or no economic growth, of retarded technological change, or whatever. Indeed, use of monetary policy to affect real variables, relative prices, and interest rates could not systematically alter those real variables; it could only add monetary disturbances, which often had the perverse effect of making worse the supposed problem that more (or less) money was intended to alleviate. For example, as we all know now from much painful experience, trying to lower interest rates by rapid money growth not only failed to achieve lower interest rates, but ended up causing still higher rates.

The quantity theory was never intended to be a general equilibrium model. It focused on only a few important phenomena such as the long-run price level and short-run economic instability. The full range, precise content, and dynamics of monetary change were generally not fully articulated, explored, or tested. The quantity theory separated real effects from nominal ones, except that short-run monetary disturbances were empirically associated with real short-period business cycle disturbances. In the long run, nominal money affects only the price level. Real variables, including real interest rates, depend solely on real, not monetary, variables. In other words, money is neutral in the long run.

The quantity theory is in the Marshallian partial equilibrium tradition, not the Walrasian general equilibrium tradition. It proceeds on the basis that we do not need to know much about everything in order to predict and understand much more about a few important variables such as the price level.

There is merit in efficient use of appropriate abstractions in building and using theory (and elsewhere). It is costly to increase the number of variables and the range of phenomena to be considered, and the added costs are not necessarily balanced by the added benefits of increased precision or by the broader range of implications.

Indeed, in his book, *The New Classical Macroeconomics*, Hoover (1988) has an interesting analysis of important differences between the Marshallian partial equilibrium approaches of Milton Friedman and most other monetarists and the general equilibrium Walrasian approaches of many rational expectations and new classical macroeconomic theorists. The practical difficulty, or impossibility, in testing anything other than relatively simple models with relatively few variables argues for the Marshallian approach in empirical matters. Constructing theories that have readily testable content is a desirable goal. However, it would seem that the increasing complexity,

formalization, and mathematization of macroeconomics and monetary theory in recent years have often made models less (rather than more) amenable to empirical testing and verification.

Although the rational expectations critique has been highly useful in questioning older, flawed theories and in raising valid questions about the many and varied tinkerings with monetary and fiscal policies, it is not clear that the rational expectations hypothesis and new classical macroeconomics have contributed a usable alternative to the quantity theory, particularly in a world with national domestic economies open to international trade and international financial transactions. To be sure, emphasizing the role of expectations, the consistency of expectations and underlying theory, the dependence of expectations on the policy regime, and the simple fact that people learn from experience have all been important, even if not always quite as novel or as innovative as some would have us believe.

The Problem of Testing the New Models

Despite the important analytical role given to expectations, we have made little progress in developing fruitful, testable, or tested models of expectations formation. It is not enough to assert that expectations are or ought to be consistent or generally unbiased over the long pull. Such an assertion is a far reach from a body of validated knowledge of how, in fact, expectations are formed and how economic agents and markets act on the basis of these expectations. This knowledge is generally necessary to independently test theories of how economic agents act on the basis of expectations, or how people respond to policy and other disturbances that alter expectations and behavior on the basis of those expectations. Validated knowledge of expectations formation is also required to test central elements of the signal extraction problem and process that has such a prominent role in much of the rational expectations literature. Moreover, trying to bypass expectations by assuming perfect foresight in a world otherwise characterized by uncertainty may yield interesting analytical results but is typically empty of substantive ones.

In my judgment, these approaches are likely to be sterile. Indeed, they are sterile without better—much better—tested theories of expectations formation. There is surely more to analysis that either critiquing the sins and errors of other theories or working out the formal, theoretical properties of increasingly complex systems.

Incorporating Public Choice Theory

Optimization behavior by private economic agents on the basis of utility or profit maximization—the essence of economics—is one

thing. But what of governments and policymakers? What are they trying to maximize or optimize? It is surely both naive and wrong to believe that (a) governments and bureaucrats are idealists or saints devoted to optimizing a time-consistent social welfare function that maximizes economic growth, minimizes inflation, and the like; or that (b) governments and bureaucrats have the information and knowledge to do so. Such beliefs ignore both the multiple, conflicting, and often inconsistent pressures on governments and the multiple, conflicting, and inconsistent policies that governments typically pursue. These policies reflect, in part, the range of diverse pressures on governments and the interests of their diverse clients, as well as the separate self-interests of governments and bureaus, per se, and of their personnel. This non-idealistic view of governments—that governments and bureaus have their own self-interests—is part of the essence of the whole field of public choice, which largely seems to have been neglected in the public policy modeling in the rational expectations tradition. This oversight is, indeed, a serious defect. If we must use the best analytical, mathematical, and statistical tools to analyze private choice and market decisionmaking, should we not use the best tools for analyzing nonmarket decisionmaking by governments, bureaus, politicians, and legislatures in the formation of public policies?

Much of the policy ineffectiveness and dynamic inconsistency discussion in Hoover's paper never treats governments and bureaus as having any self-interest goals for themselves, their clients, their politicians, or their personnel. This serious flaw is not only in the Hoover paper but is in the entire body of analysis that the paper surveys.

Several years ago, I pointed out that most of the policy ineffectiveness analysis of the rational expectations approach proceeded on the basis of essentially a one-person game, where the central bank, attempting to achieve stabilization goals, adopted repetitive but unannounced strategies (Meiselman 1986). Like the proverbial sampling from a fixed urn, through time the public would learn of such unchanging strategies. Then, by anticipating central bank actions, à la rational expectations, the public's behavior would tend to undermine or eliminate policy surprises and, thereby, the real impacts of such strategies.

The true contents of the policy urn were assumed to remain the same. Thus, so-called stabilization actions came to be essentially known ahead of time and discounted. The result was that actual actions of stabilization agencies neither imposed surprises nor provided new information. In the long run, these policies were rendered

ineffective as were the agencies operating such policies—hardly comforting words for the Federal Reserve whose chairman is now widely regarded as the second most powerful man in Washington.

To protect and enhance such power, as well as the role of the institution and its personnel, the Fed and other discretionary authorities cannot and will not commit institutional suicide by adopting a fixed strategy, including any stabilization or monetary policy rules. Indeed, the Fed will not adopt such rules and will fight, as it has fought, any attempt to have rules or fixed strategies imposed on it that would make monetary policy predictable. Instead, the Fed's institutional incentives are to adopt a strategy that permits it to continue to impose surprises on the public, and thereby to retain its power and to have real effects in the short run on income, employment, interest rates, and so forth. One further important by-product of this strategy is the impairment of accountability.

The changing and changeable Fed policies and operations keep those policies and operations essentially hidden from the public. The result is that Fed policies and operations are essentially unforecastable and undiscountable. Expectations of Fed policy may be rational in the sense that they are not systematically biased, but such expectations are typically so far off the mark that large numbers of people typically, consistently, and inevitably make large forecasting errors. Even the best and the brightest, including Nobel Prize winners who devote much of their professional lives to studying the Fed, have poor records forecasting Fed actions. In fact, if Nobel Prize winners, or others, become able to forecast Fed policies, the Fed can easily change its policies to keep itself unforecastable and still capable of imposing surprises. Thus the Fed remains both powerful and effective in imposing surprises.

Analogous to the Heisenberg principle in physics, when the public observes the Fed, that observation causes a change in the Fed's behavior. Similarly, when the public uses those observations to anticipate the Fed's actions, that changes the public's behavior, too. The result is unstable, dynamically inconsistent expectations of each other held by both the Fed and the public. Policies and actions chosen today as optimal, given today's expectations about tomorrow, may no longer by optimal when tomorrow comes because policies, actions, and anticipations can and frequently do change in unpredictable, essentially unforecastable ways.

One result is essentially "bilateral dynamic inconsistency," which is a process of sequentially shifting anticipations, behavior, and uncertainties of *both* the public and the Federal Reserve stemming from discretionary and unconstrained policy. Another result is that

otherwise stable empirical relations and dependable lags in the effect of monetary change are likely to be disrupted and altered. Under discretionary monetary policy, because the underlying structure keeps changing and in ways that essentially cannot be specified, monitored, or measured, empirical links become less dependable and observed lags tend to become more variable. It is as if white noise or a random component were added to each variable. All become more difficult or impossible to isolate and to quantify. Underlying systematic relationships become blurred or buried.

The irony is that the Fed cannot hope to be credible if it fails to make itself effective in achieving stabilization goals, and the Fed cannot be consistently effective in achieving stabilization goals if it makes itself unpredictable. The Fed cannot avoid the genuine dilemma.

Discretionary policy essentially generates a two-person rather than a one-person game. Because the Fed cannot know what expectations the public holds or how the public's expectations will respond to the Fed's actions, the Fed's policies, too, are unavoidably subject to error in achieving Fed goals. Because the public cannot know what the Fed will be doing, the public, too, has more uncertainty and makes more errors in evaluating present and future markets and in making appropriate choices and decisions. Discretionary monetary policy cannot be rendered wholly ineffective by the public, because the public cannot determine with confidence what current policies are or what future policies will be. Monetary policy continues to generate uncertainty and dead-weight losses that have real short-run and long-run effects. Fine-tuning contains the seeds of its own self-destruction. Only in the world of a hypothetical, once-for-all change in money or of similar experiments would such policies have no permanent or no long-run impact on real variables, uncertainty, and economic welfare. In this sense, discretionary monetary policy is inconsistent with the long-run neutrality of money.

Policy Consensus

Most of us—old monetarists, middle-aged rational expectations theorists, and many new classical macroeconomists—come out with essentially the same policy conclusions. Our surprising and welcome consensus includes the following:

- Rules rather than authorities.
- Slow, steady, predictable growth of money.

- Other policies consistent with a monetary rule, such as nonintervention in markets for foreign exchange, securities, or interest rates.
- Stable, predictable tax and expenditure programs.
- A goal of zero inflation, or at least an avoidance of the deliberate use of inflation to solve or cover up other problems such as unemployment, a weak housing market, and the like. In other words, we would shun the Phillips curve snare or the delusion that monetary change has dependable and controllable real effects.

All of this sounds like much of the original Reagan program. The Reagan administration never really pursued those policies, but for a brief period many Reagan officials discussed them.

A Learning Process

The rational expectations hypothesis and new classical macroeconomics have been faulted for lack of answers or involvement regarding some traditional questions of macroeconomics dealing with the analysis and forecasts of short-period business conditions and short-period stabilization policies. With some justification, rational expectations people respond by saying that (a) the old macro models may have tried to do so but could not forecast, either, and that (b) discretionary, short-period monetary and fiscal policies also did not work as planned. Instead, discretionary, short-run policies often caused long-run problems of inflation, bloated government, and resource misallocation. So, whatever the change of emphasis, by trying to frame the questions properly and to answer them on the basis of rigorous microeconomics, there is the potential for finally arriving at answers to fundamental questions regarding economic fluctuations, business cycles, and appropriate public policy.

If we examine some of this criticism, we see that it is as if researchers in cell biology and genetics, after having made significant progress in understanding cell and tissue physiology, were faulted because they had not yet found definitive answers to the causes of cancer or had not yet developed a cancer cure. But surely the preliminary and incomplete state of their knowledge is no justification either for stopping the fundamental research; for continually resorting to witchcraft, magic, and incantation; or for prescribing medications that poison rather than cure. In similar manner, the not wholly satisfactory state of the new understanding of macro phenomena does not mean we should discontinue our search for knowledge.

Perhaps some day we will know more of both pathologies and their cures.

Conclusion

My comments are not meant to be critical of Hoover's scholarly paper, which is an excellent summary and critique of major and fundamental changes in macroeconomics and monetary theory in the past 20 years. His paper and, even more, his book, *The New Classical Macroeconomics,* are excellent guides to the literature and to the issues. Both works are well written, finely nuanced, and remarkably fair in an area of economics usually dominated by shrill acrimony.

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