After Keynesian Macroeconomics

Robert E. Lucas and Thomas J. Sargent

1. Introduction

For the applied economist, the confident and apparently successful application of Keynesian principles to economic policy which occurred in the United States in the 1960s was an event of incomparable significance and satisfaction. These principles led to a set of simple, quantitative relationships between fiscal policy and economic activity generally, the basic logic of which could be (and was) explained to the general public, and which could be applied to yield improvements in economic performance benefiting *everyone.* It seemed an economics as free of ideological difficulties as, say, applied chemistry or physics, promising a straightforward expansion in economic possibilities. One might argue about how this windfall should be distributed, but it seemed a simple lapse of logic to oppose the windfall itself. Understandably and correctly, this promise was met at first with skepticism by noneconomists; the smoothly growing prosperity of the Kennedy-Johnson years did much to diminish these doubts.

We dwell on these halcyon days of Keynesian economics because, without conscious effort, they are difficult to recall today. In the present decade, the U.S. economy has undergone its first major depression since the 1930s, to the accompaniment of inflation rates in excess of 10 percent per annum. These events have been transmitted (by consent of the governments involved) to other advanced countries and in many cases have been amplified. These events did not arise from a reactionary reversion to outmoded, "classical" principles of tight money and balance budgets. On the contrary, they were accompanied by massive governmental budget deficits and high rates of monetary expansion: policies which, although bearing an admitted risk of inflation, promised according to modern Keynesian doctrine rapid real growth and low rates of unemployment.

That these predictions were wildly incorrect, and that the doctrine on which they were based is fundamentally flawed, are now simple matters of fact, involving no novelties in economic theory. The task which faces contemporary students of the business cycle is that of sorting through the wreckage, determining which features of that remarkable intellectual event called the

Robert E. Lucas is Professor of Economics at the University of Chicago and Thomas J. Sargent is Professor of Economics at the University of Minnesota. The authors wish to acknowledge the benefit of criticism of an earlier draft by William Poole and Benjamin Friedman.

Keynesian Revolution can be salvaged and put to good use, and which others must be discarded. Though it is far from clear what the outcome of this process will be, it is already evident that it will necessarily involve the reopening of basic issues in monetary economics which have been viewed since the thirties as "closed," and the reevaluation of every aspect of the institutional framework within which monetary and fiscal policy is formulated in the advanced countries.

This paper is in the nature of an early progress report on this process of reevaluation and reconstruction. We begin by reviewing the econometric framework by means of which Keynesian theory evolved from disconnected, qualitative "talk" about economic activity into a system of equations which could be compared to data in a systematic way, and provide an operational guide in the necessarily quantitative task of formulating monetary and fiscal policy. Next, we identify those aspects of this framework which were central to its failure in the seventies. In so doing, our intent will be to establish that the difficulties are *fatal:* that modern macroeconomic models are of *no* value in guiding policy, and that this condition will not be remedied by modifications along any line which is currently being pursued.

This diagnosis, if successful, will suggest certain principles which a useful theory of business cycles must possess. In the latter part of this paper we shall review some recent research which is consistent with these principles.

2. Macroeconometric Models

The Keynesian Revolution was, in the form in which it succeeded in the United States, a revolution in *method.* This was not Keynes's [13] intent, nor is it the view of all of his most eminent followers. Yet if one does not view the revolution in this way, it is impossible to account for some of its most important features: the evolution of macroeconomics into a quantitative, *scientific* discipline, the development of explicit statistical descriptions of economic behavior, the increasing reliance of government officials on technical economic expertise, and the introduction of the use of mathematical control theory to manage an economy. It is the fact that Keynesian theory lent itself so readily to the formulation of explicit econometric models which accounts for the dominant scientific position it attained by the 1960s.

As a consequence of this, there is no hope of understanding either the success of the Keynesian Revolution or its eventual failure at the purely verbal level at which Keynes himself wrote. It will be necessary to know something of the way macroeconometric models are constructed and the features they must have in order to "work" as aids in forecasting and policy evaluation. To discuss these issues, we introduce some notation.

An econometric model is a system of equations involving a number of endogenous variables (variables that are determined by the model), exogenous variables (variables which affect the system but are not affected by it), and stochastic or random shocks. The idea is to use historical data to estimate the model, and then to utilize the estimated version to obtain estimates of the consequences of alternative policies. For practical reasons, it is usual to use a standard linear model, taking the structural form[1]

[1] Linearity is a matter of convenience, not of principle. See Section 6.3, below.

(1) $A_0 y_t + A_1 y_{t-1} + \ldots + A_m y_{t-m} = B_0 x_t + B_1 x_{t-1} + \ldots + B_n x_{t-n} + \epsilon_t$

(2) $$R_0 \epsilon_t + R_1 \epsilon_{t-1} + \ldots + R_r \epsilon_{t-r} = u_t,\ R_0 \equiv I.$$

Here y_t is an (Lx1) vector of endogenous variables, x_t is a (Kx1) vector of exogenous variables, and ϵ_t and u_t are each (Lx1) vectors of random disturbances. The matrices A_j are each (LxL); the B_j's are (LxK), and the R_j's are each (LxL). The (Lx1) disturbance process u_t is assumed to be a serially uncorrelated process with $Eu_t = 0$ and with contemporaneous covariance matrix $Eu_t u_t' = \Sigma$ and $Eu_t u_s' = 0$ for all $t \neq s$. The defining characteristic of the exogenous variables x_t is that they are uncorrelated with the ϵ's at all lags so that $Eu_t x_s'$ is an (LxK) matrix of zeroes for all t and s.

Equations (1) are L equations in the L current values y_t of the endogenous variables. Each of these structural equations is a behavioral relationship, identity, or market clearing condition, and each in principle can involve a number of endogenous variables. The structural equations are usually not "regression equations"[2] because the ϵ_t's are in general, by the logic of the model, supposed to be correlated with more than one component of the vector y_t and very possibly one or more components of the vectors $y_{t-1}, \ldots y_{t-m}$.

The structural model (1) and (2) can be solved for y_t in terms of past y's and x's and past shocks. This "reduced form" system is

(3) $$y_t = -P_1 y_{t-1} - \ldots - P_{r+m} y_{t-r-m} + Q_0 x_t + \ldots + Q_{r+n} x_{t-n-r} + A_0^{-1} u_t$$

where[3]

$$P_s = A_0^{-1} \sum_{j=-\infty}^{\infty} R_j A_{s-j}$$

$$Q_s = A_0^{-1} \sum_{j=-\infty}^{\infty} R_j B_{s-j}.$$

The reduced form equations are "regression equations," that is, the disturbance vector $A_0^{-1} u_t$ is orthogonal to $y_{t-1}, \ldots, y_{t-r-m}, x_t, \ldots, x_{t-n-r}$. This follows from the assumptions that the x's are exogenous and that the u's are serially uncorrelated. Therefore, under general conditions the reduced form can be estimated consistently by the method of least squares. The population parameters of the reduced form (3) together with the parameters of a vector autoregression for x_t,

(4) $$x_t = C_1 x_{t-1} + \ldots + C_p x_{t-p} + a_t$$

[2] A "regression equation" is an equation to which the application of ordinary least squares will yield consistent estimates.

[3] In these expressions for P_s and Q_s, take matrices not previously defined (for example, any with negative subscripts) to be zero.

where $Ea_t = 0$ and $Ea_t . x_{t-j} = 0$ for $j \geqslant 1$ completely describe all of the first and second moments of the (y_t, x_t) process. Given long enough time series, good estimates of the reduced form parameters – the P_j's and Q_j's – can be obtained by the method of least squares. Reliable estimates of those parameters is all that examination of the data by themselves can deliver.

It is not in general possible to work backwards from estimates of the P's and Q's alone to derive unique estimates of the structural parameters, the A_j's, B_j's, and R_j's. In general, infinite numbers of A, B, and R's are compatible with a single set of P's and Q's. This is the "identification problem" of econometrics. In order to derive a set of estimated structural parameters, it is necessary to know a great deal about them in advance. If enough prior information is imposed, it is possible to extract estimates of the (A_j, B_j, R_j)'s implied by the data in combination with the prior information.

For purposes of *ex ante* forecasting, or the unconditional prediction of the vector $y_{t+1}, y_{t+2}, \ldots$ given observation of y_s and x_s, $s \leqslant t$, the estimated reduced form (3), together with (4), is sufficient. This is simply an exercise in a sophisticated kind of extrapolation, requiring no understanding of the structural parameters or, that is to say, of the *economics* of the model.

For purposes of *conditional* forecasting, or the prediction of the future behavior of some components of y_t and x_t *conditional* on particular values of other components, selected by policy, one needs to know the structural parameters. This is so because a change in policy *necessarily* alters some of the structural parameters (for example, those describing the past behavior of the policy variables themselves) and therefore affects the reduced form parameters in highly complex fashion (see the equations defining P_s and Q_s, below (3)). Without knowledge as to which structural parameters remain invariant as policy changes, and which change (and how), an econometric model is of *no* value in assessing alternative policies. It should be clear that this is true *regardless* of how well (3) and (4) fit historical data, or how well they perform in unconditional forecasting.

Our discussion to this point has been at a high level of generality, and the formal considerations we have reviewed are not in any way specific to *Keynesian* models. The problem of identifying a structural model from a collection of economic time series is one that must be solved by anyone who claims the ability to give quantitative economic advice. The simplest Keynesian models are attempted solutions to this problem, as are the large-scale versions currently in use. So, too, are the monetarist models which imply the desirability of fixed monetary growth rules. So, for that matter, is the armchair advice given by economists who claim to be outside the econometric tradition, though in this case the implicit, underlying structure is not exposed to professional criticism. *Any* procedure which leads from the study of observed economic behavior to the quantitative assessment of alternative economic policies involves the steps, executed poorly or well, explicitly or implicitly, which we have outlined above.

3. Keynesian Macroeconometrics

In Keynesian macroeconometric models structural parameters are identified by the imposition of several types of *a priori* restrictions on the A_j's, B_j's, and

R_j's. These restrictions usually fall into one of the following categories:[4]

(a) *A priori* setting of many of the elements of the A_j's and B_j's to zero.
(b) Restrictions on the orders of serial correlation and the extent of the cross serial correlation of the disturbance vector ϵ_t, restrictions which amount to *a priori* setting many elements of the R_j's to zero.
(c) *A priori* categorization of variables into "exogenous" and "endogenous." A relative abundance of exogenous variables aids identification.

Existing large Keynesian macroeconometric models are open to serious challenge for the way they have introduced each category of restriction.

Keynes's *General Theory* was rich in suggestions for restrictions of type (a). It proposed a theory of national income determination built up from several simple relationships, each involving a few variables only. One of these, for example, was the "fundamental law" relating consumption expenditures to income. This suggested one "row" in equations (1) involving current consumption, current income, and *no other* variables, thereby imposing many zero-restrictions on the A_i and B_j. Similarly, the liquidity preference relation expressed the demand for money as a function of income and an interest rate *only*. By translating the building blocks of the Keynesian theoretical system into explicit equations, models of the form (1) and (2) were constructed with many theoretical restrictions of type (a).

Restrictions on the coefficients R_i governing the behavior of the "error terms" in (1) are harder to motivate theoretically, the "errors" being by definition movements in the variables which the *economic* theory cannot account for. The early econometricians took "standard" assumptions from statistical textbooks, restrictions which had proved useful in the agricultural experimenting which provided the main impetus to the development of modern statistics. Again, these restrictions, well-motivated or not, involve setting many elements in the R_i's equal to zero, aiding identification of the model's structure.

The classification of variables into "exogenous" and "endogenous" was also done on the basis of prior considerations. In general, variables were classed as "endogenous" which were, as a matter of institutional fact, determined largely by the actions of private agents (like consumption or private investment expenditures). Exogenous variables were those under governmental control (like tax rates, or the supply of money). This division was intended to reflect the ordinary meaning of the word "endogenous" to mean "determined by the [economic] system" and "exogenous" to mean "affecting the [economic] system but not affected by it."

By the mid-1950s, econometric models had been constructed which fit time series data well, in the sense that their reduced forms (3) tracked past data closely and proved useful in short-term forecasting. Moreover, by means of

[4] These three categories certainly do not exhaust the set of possible identifying restrictions, but in Keynesian macroeconometric models most identifying restrictions fall into one of these three categories. Other possible sorts of identifying restrictions include, for example, *a priori* knowledge about components of Σ, and cross-equation restrictions across elements of the A_j, B_j, and C_j's. Neither of these latter kinds of restrictions is extensively used in Keynesian macroeconometrics.

restrictions of the three types reviewed above, it was possible to identify their structural parameters A_i, B_j, R_k. Using this estimated structure, it was possible to simulate the models to obtain estimates of the consequences of different government economic policies, such as tax rates, expenditures or monetary policy.

This Keynesian solution to the problem of identifying a structural model has become increasingly suspect as a result of developments of both a theoretical and statistical nature. Many of these developments are due to efforts to researchers sympathetic to the Keynesian tradition, and many were well-advanced well before the spectacular failure of the Keynesian models in the 1970s.[5]

Since its inception, macroeconomics has been criticized for its lack of "foundations in microeconomic and general equilibrium theory." As astute commentators like Leontief [14] (disapprovingly) and Tobin [37] (approvingly) recognized early on, the creation of a distinct branch of theory with its own distinct postulates was Keynes's conscious aim. Yet a main theme of theoretical work since the *General Theory* has been the attempt to use microeconomic theory based on the classical postulate that agents act in their own interests to suggest a list of variables that belong on the right side of a given behavioral schedule, say, a demand schedule for a factor of production or a consumption schedule.[6] But from the point of view of identification of a given structural equation by means of restrictions of type (a), one needs reliable prior information that certain variables should be *excluded* from the right-hand side. Modern probabilistic microeconomic theory almost never implies either the exclusion restrictions that were suggested by Keynes or those that are imposed by macroeconometric models.

[5] Criticisms of the Keynesian solutions of the identification problem along much the following lines have been made in Lucas [17], Sims [33], and Sargent and Sims [31].

[6] [This note was added in revision, in part in response to Benjamin Friedman's comments.] Much of this work was done by economists operating well within the Keynesian tradition, often within the context of some Keynesian macroeconometric model. Sometimes a theory with optimizing agents was resorted to in order to resolve empirical paradoxes by finding variables that had been omitted from some of the earlier Keynesian econometric formulations. The works of Modigliani and Friedman on consumption are good examples of this line of work, a line whose econometric implications have been extended in important work by Robert Merton. The works of Tobin and Baumol on portfolio balance and of Jorgenson on investment are also in the tradition of applying optimizing microeconomic theories for generating macroeconomic behavior relations. In the last thirty years, Keynesian econometric models have to a large extent developed along the line of trying to model agents' behavior as stemming from more and more sophisticated optimum problems. Our point here is certainly *not* to assert that Keynesian economists have completely foregone any use of optimizing microeconomic theory as a guide. Rather, it is that, especially when explicitly stochastic and dynamic problems have been studied, it has become increasingly apparent that microeconomic theory has very damaging implications for the restrictions conventionally used to identify Keynesian macroeconometric models. Furthermore, as Tobin [37] emphasized long ago, there is a point beyond which Keynesian models must suspend the hypothesis either of cleared markets or of optimizing agents if they are to possess the operating characteristics and policy implications that are the hallmarks of Keynesian economics.

To take one example that has extremely dire implications for the identification of existing macro models, expectations about the future prices, tax rates, and income levels play a critical role in many demand and supply schedules in those models. For example, in the best models, investment demand typically is supposed to respond to businessmen's expectations of future tax credits, tax rates, and factor costs. The supply of labor typically is supposed to depend on the rate of inflation that workers expect in the future. Such structural equations are usually identified by the assumption that, for example, the expectation about the factor price or rate of inflation attributed to agents is a function *only* of a few lagged values of the variable itself which the agent is supposed to be forecasting. However, the macro models themselves contain complicated dynamic interactions among endogenous variables, including factor prices and the rate of inflation, and generally imply that a wise agent would use current and many lagged values of many and usually most endogenous and exogenous variables in the model in order to form expectations about any one variable. Thus, virtually any version of the hypothesis that agents behave in their own interests will contradict the identification restrictions imposed on expectations formation. Further, the restrictions on expectations that have been used to achieve identification are entirely arbitrary and have not been derived from any deeper assumption reflecting first principles about economic behavior. No general first principle has ever been set down which would imply that, say, the expected rate of inflation should be modeled as a linear function of lagged rates of inflation alone with weights that add up to unity, yet this hypothesis is used as an identifying restriction in almost all existing models. The casual treatment of expectations is not a peripheral problem in these models, for the role of expectations is pervasive in the models and exerts a massive influence on their dynamic properties (a point Keynes himself insisted on). The failure of existing models to derive restrictions on expectations from any first principles grounded in economic theory is a symptom of a somewhat deeper and more general failure to derive behavioral relationships from any consistently posed dynamic optimization problems.

As for the second category, restrictions of type (b), existing Keynesian macro models make severe *a priori* restrictions on the R_j's. Typically, the R_j's are supposed to be diagonal so that cross equation lagged serial correlation is ignored and also the order of the ϵ_t process is assumed to be short so that only low-order serial correlation is allowed. There are at present no theoretical grounds for introducing these restrictions, and for good reasons there is little prospect that economic theory will soon provide any such grounds. In principle, identification can be achieved without imposing any such restrictions. Foregoing the use of category (b) restrictions would increase the category (a) and (c) restrictions needed. In any event, existing macro models do heavily restrict the R's.

Turning to the third category, all existing large models adopt an *a priori* classification of variables into the categories of strictly endogenous variables, the y_t's, and strictly exogenous variables, the x_t's. Increasingly, it is being recognized that the classification of a variable as "exogenous" on the basis of the observation that it *could* be set without reference to the current and past values of other variables has nothing to do with the econometrically relevant question of how this variable has *in fact* been related to others over a given historical period. Moreover, in light of recent developments in time series econometrics, we know that this arbitrary classification procedure is not necessary. Christopher Sims [34] has shown that in a time series context the hypothesis of econometric exogeneity can be tested. That is, Sims showed that the hypothesis that x_t is strictly econometrically exogenous in (1) necessarily implies certain restrictions that can be tested given time series on the y's and x's. Tests along the lines of Sims's ought to be used as a matter of course in checking out categorizations into exogenous and endogenous sets of variables. To date they have not been. Prominent builders of large econometric models have even denied the usefulness of such tests.[7]

4. Failure of Keynesian Macroeconometrics

Our discussion in the preceding section raised a number of theoretical reasons for believing that the parameters identified as structural by the methods which are in current use in macroeconomics are not structural in fact. That is, there is no reason, in our opinion, to believe that these models have isolated structures which will remain invariant across the class of interventions that figure in contemporary discussions of economic policy. Yet the question of whether a particular model is structural is an empirical, not a theoretical, one. If the macroeconometric models had compiled a record of parameter stability, particularly in the face of breaks in the stochastic behavior of the exogenous variables and disturbances, one would be skeptical as to the importance of prior theoretical objections of the sort we have raised.

In fact, however, the track record of the major econometric models is, on any dimension other than very short-term unconditional forecasting, very poor. Formal statistical tests for parameter instability, conducted by subdividing past series into periods and checking for parameter stability across time, invariably reveal major shifts (for one example, see [23]). Moreover, this difficulty is implicitly acknowledged by model-builders themselves, who routinely employ an elaborate system of add-factors in forecasting, in an attempt to offset the continuing "drift" of the model away from the actual series.

Though not, of course, designed as such by anyone, macroeconometric models were subjected in the 1970s to a decisive test. A key element in all Keynesian models is a "tradeoff" between inflation and real output: the higher is the inflation rate, the higher is output (or equivalently, the lower is the rate of unemployment). For example, the models of the late 1960s predicted a sustained unemployment rate in the United States of 4 percent as consistent with a 4 percent annual rate of inflation. Many economists at that time urged a deliberate policy of inflation on the basis of this prediction. Certainly the erratic "fits and starts" character of actual U.S. policy in the 1970s cannot be

[7] For example, see the comment by Albert Ando [35, especially pp. 209-210], and the remarks of L. R. Klein [24].

attributed to recommendations based on Keynesian models, but the inflationary bias *on average* of monetary and fiscal policy in this period should, according to all of these models, have produced the lowest average unemployment rates for any decade since the 1940s. In fact, as we know, they produced the highest unemployment since the 1930s. This was econometric failure on a grand scale.

This failure has not led to widespread conversions of Keynesian economists to other faiths, nor should it have been expected to. In economics, as in other sciences, a theoretical framework is always broader and more flexible than any particular set of equations, and there is always the hope that, if a particular specific model fails, one can find a more successful one based on "roughly" the same ideas. It has, however, already had some important consequences, with serious implications both for economic policy-making and for the practice of economic science.

For policy, the central fact is that Keynesian policy recommendations have no sounder basis, in a scientific sense, than recommendations of non-Keynesian economists or, for that matter, noneconomists. To note one consequence of the wide recognition of this, the current wave of protectionist sentiment directed at "saving jobs" would have been answered, ten years ago, with the Keynesian counter-argument that fiscal policy can achieve the same end, but more efficiently. Today, of course, no one would take this response seriously, so it is not offered. Indeed, economists who ten years ago championed Keynesian fiscal policy as an *alternative* to inefficient direct controls increasingly favor the latter as "supplements" to Keynesian policy. The idea seems to be that if people refuse to obey the equations we have fit to their past behavior, we can pass laws to *make* them do so.

Scientifically, the Keynesian failure of the 1970s has resulted in a new openness. Fewer and fewer economists are involved in monitoring and refining the major econometric models; more and more are developing alternative theories of the business cycle, based on different theoretical principles. In addition, increased attention and respect are accorded to the theoretical casualties of the Keynesian Revolution, to the ideas of Keynes's contemporaries and of earlier economists whose thinking has been regarded for years as outmoded.

At the present time, it is impossible to foresee where these developments will lead. Some, of course, continue to believe that the problems of existing Keynesian models can be resolved within the existing framework, that these models can be adequately refined by changing a few structural equations, by adding or subtracting a few variables here and there, or perhaps by disaggregating various blocks of equations. We have couched our preceding criticisms in such general terms precisely to emphasize their generic character and hence the futility of pursuing minor variations within this general framework.

A second response to the failure of Keynesian analytical methods is to renounce analytical methods entirely, returning to "judgmental" methods. The first of these responses identifies the quantitative, scientific goals of the Keynesian Revolution with the details of the particular models so far developed. The second renounces both these models and the objectives they were designed to attain. There is, we believe, an intermediate course, to which we now turn.

5. Equilibrium Business Cycle Theory

Economists prior to the 1930s did not recognize a need for a special branch of economics, with its own special postulates, designed to explain the business cycle. Keynes founded that subdiscipline, called *macroeconomics,* because he thought that it was impossible to explain the characteristics of business cycles within the discipline imposed by classical economic theory, a discipline imposed by its insistence on adherence to the two postulates (a) that markets be assumed to clear, and (b) that agents be assumed to act in their own self-interest. The outstanding fact that seemed impossible to reconcile with these two postulates was the length and severity of business depressions and the large scale unemployment which they entailed. A related observation is that measures of aggregate demand and prices are positively correlated with measures of real output and employment, in apparent contradiction to the classical result that changes in a purely nominal magnitude like the general price level were pure "unit changes" which should not alter real behavior. After freeing himself of the straight-jacket (or discipline) imposed by the classical postulates, Keynes described a model in which rules of thumb, such as the consumption function and liquidity preference schedule, took the place of decision functions that a classical economist would insist be derived from the theory of choice. And rather than require that wages and prices be determined by the postulate that markets clear—which for the labor market seemed patently contradicted by the severity of business depressions—Keynes took as an unexamined postulate that money wages are "sticky," meaning that they are set at a level or by a process that could be taken as uninfluenced by the macroeconomic forces he proposed to analyze.

When Keynes wrote, the terms "equilibrium" and "classical" carried certain positive and normative connotations which seemed to rule out either modifier being applied to business cycle theory. The term "equilibrium" was thought to refer to a system "at rest," and both "equilibrium" and "classical" were used interchangeably, by some, with "ideal." Thus an economy in classical equilibrium would be both unchanging and unimprovable by policy interventions. Using terms in this way, it is no wonder that few economists regarded equilibrium theory as a promising starting point for the understanding of business cycles, and for the design of policies to mitigate or eliminate them.

In recent years, the meaning of the term "equilibrium" has undergone such dramatic development that a theorist of the 1930s would not recognize it. It is now routine to describe an economy following a multivariate stochastic process as being "in equilibrium," by which is meant nothing more than that at each point in time, postulates (a) and (b) above are satisfied. This development, which stemmed mainly from work by K. J. Arrow [2] and G. Debreu [6], implies that simply to look at any economic time series and conclude that it is a "disequilibrium phenomenon" is a meaningless observation. Indeed, a more likely conjecture, on the basis of recent work by Hugo Sonnenschein [36], is that the general hypothesis that a collection of time series describes an economy in competitive equilibrium is *without content.*[8]

The research line being pursued by a number of us involves the attempt to discover a particular, econometrically testable equilibrium theory of the business cycle, one that can serve as the foundation for quantitative analysis of macroeconomic policy. There is no denying that this approach is "counter-revolutionary," for it presupposes that Keynes and his followers were wrong to give up on the possibility that an equilibrium theory could account for the business cycle. As of now, no successful equilibrium macroeconometric model at the level of detail of, say, the FMP model, has been constructed. But small theoretical equilibrium models have been constructed that show potential for explaining some key features of the business cycle long thought to be inexplicable within the confines of classical postulates. The equilibrium models also provide reasons for understanding why estimated Keynesian models fail to hold up outside of the sample over which they have been estimated. We now turn to describing some of the key facts about business cycles and the way the new classical models confront them.

For a long time most of the economics profession has, with some reason, followed Keynes in rejecting classical macroeconomic models because they seemed incapable of explaining some important characteristics of time series measuring important economic aggregates. Perhaps the most important failure of the classical model seemed to be its inability to explain the positive correlation in the time series between prices and/or wages, on the one hand, and measures of aggregate output or employment, on the other hand. A second and related failure was its inability to explain the positive correlations between measures of aggregate demand, like the money stock, and aggregate output or employment. Static analysis of classical macroeconomic models typically implied that the levels of output and employment were determined independently of both the absolute level of prices and of aggregate demand. The pervasive presence of the above mentioned positive correlations in the time series seems consistent with causal connections flowing from aggregate demand and inflation to output and employment, contrary to the classical "neutrality" propositions. Keynesian macroeconometric models do imply such causal connections.

[8] For an example that illustrates the emptiness at a general level of the statement that "employers are always operating along dynamic stochastic demands for factors," see the remarks on econometric identification in Sargent [29]. In applied problems that involve modeling agents' optimum decision rules, one is impressed at how generalizing the specification of agents' objective functions in plausible ways quickly leads to econometric under-identification. A somewhat different class of examples is seen in the difficulties in using time series observations to refute the view that "agents only respond to unexpected changes in the money supply." A distinguishing feature of the equilibrium macroeconometric models described below is that predictable changes in the money supply do not affect real GNP or total employment. In Keynesian models, predictable changes in the money supply do cause real GNP and employment to move. At a general level, it is impossible to discriminate between these two views by observing time series drawn from an economy described by a stationary vector random process (Sargent [28]).

We now have rigorous theoretical models which illustrate how these correlations can emerge while retaining the classical postulates that markets clear and agents optimize.[9] The key step in obtaining such models has been to relax the ancillary postulate used in much classical economic analysis that agents have perfect information. The new classical models continue to assume that markets always clear and that agents optimize. The postulate that agents optimize means that their supply and demand decisions must be functions of real variables, including perceived relative prices. Each agent is assumed to have limited information and to receive information about some prices more often than other prices. On the basis of their limited information—the lists that they have of current and past absolute prices of various goods—agents are assumed to make the best possible estimate of all of the *relative* prices that influence their supply and demand decisions. Because they do not have all of the information that would enable them to compute perfectly the relative prices they care about, agents make errors in estimating the pertinent relative prices, errors that are unavoidable given their limited information. In particular, under certain conditions, agents will tend temporarily to mistake a general increase in all absolute prices as an increase in the *relative* price of the good that they are selling, leading them to increase their supply of that good over what they had previously planned. Since everyone is, on average, making the same mistake, aggregate output will rise above what it would have been. This increase of output will rise above what it would have been. This increase of output above what it would have been will occur whenever this period's average economy-wide price level is above what agents had expected this period's average economy-wide price level to be on the basis of previous information. Symmetrically, average output will be decreased whenever the aggregate price turns out to be lower than agents had expected. The hypothesis of "rational expectations" is being imposed here because agents are supposed to make the best possible use of the limited information they have and are assumed to know the pertinent objective probability distributions. This hypothesis is imposed by way of adhering to the tenets of equilibrium theory.

In the preceding theory, disturbances to aggregate demand lead to a positive correlation between unexpected changes in the aggregate price level and revisions in aggregate output from its previously planned level. Further, it is an easy step to show that the theory implies correlations between revisions to aggregate output and unexpected changes in any variables that help determine aggregate demand. In most macroeconomic models, the money supply is one determinant of aggregate demand. The preceding theory easily can account for positive correlations between revisions to aggregate output and unexpected increases in the money supply.

While such a theory predicts positive correlations between the inflation rate or money supply, on the one hand, and the level of output on the other, it also asserts that those correlations do not depict "tradeoffs" that can be exploited by a policy authority. That is, the theory predicts that there is no way that the monetary authority can follow a systematic activist policy and achieve a rate of

[9] See Edmund S. Phelps *et al.* [25] and Lucas [15], [16].

output that is on average higher over the business cycle than what would occur if it simply adopted a no-feedback, X-percent rule of the kind Friedman [8] and Simons [32] recommended. For the theory predicts that aggregate output is a function of current and past unexpected changes in the money supply. Output will be high only when the money supply is and has been higher than it had been expected to be, i.e., higher than average. There is simply no way that on average over the whole business cycle the money supply can be higher than average. Thus, while the preceding theory is capable of explaining some of the correlations long thought to invalidate classical macroeconomic theory, the theory is classical both in its adherence to the classical theoretical postulates and in the "nonactivist" flavor of its implications for monetary policy.

Small-scale econometric models in the sense of Section 2 of this paper have been constructed which capture some of the main features of the equilibrium models described above.[10] In particular, these models incorporate the hypothesis that expectations are rational, or that all available information is utilized by agents. To a degree, these models achieve econometric identification by invoking restrictions in each of the three categories (a), (b), and (c). However, a distinguishing feature of these "classical" models is that they also heavily rely on an important fourth category of identifying restrictions. This category (d) consists of a set of restrictions that are derived from probabilistic economic theory, but play no role in the Keynesian framework. These restrictions in general do not take the form of zero restrictions of the type (a). Instead, the restrictions from theory typically take the form of *cross-equation* restrictions among the A_j, B_j, C_j parameters. The source of these restrictions is the implication from economic theory that current decisions depend on agents' forecasts of future variables, combined with the implication that these forecasts are formed optimally, given the behavior of past variables. These restrictions do not have as simple a mathematical expression as simply setting a number of parameters equal to zero, but their economic motivation is easy to understand. Ways of utilizing these restrictions in econometric estimation and testing are being rapidly developed.

Another key characteristic of recent work on equilibrium macroeconometric models is that the reliance on entirely *a priori* categorizations (c) of variables as strictly exogenous and endogenous has been markedly reduced, although not entirely eliminated. This development stems jointly from the fact

[10] For example, Sargent [27]. Dissatisfaction with the Keynesian methods of achieving identification has also led to other lines of macroeconometric work. One line is the "index models" described by Sargent and Sims [31] and Geweke [10]. These models amount to a statistically precise way of implementing Wesley Mitchell's notion that there is a small number of common influences that explain the covariation of a large number of economic aggregates over the business cycle. This "low dimensionality" hypothesis is a potential device for restricting the number of parameters to be estimated in vector time series models. This line of work is *not* entirely a-theoretical (but see the comments of Ando and Klein in Sims [35]), though it is distinctly unKeynesian. As it happens, certain equilibrium models of the business cycle do seem to lead to low dimensional index models with an interesting pattern of variables' loadings on indexes. In general, modern Keynesian models do not so easily assume a low-index form. See the discussion in Sargent and Sims [31].

that the models assign important roles to agents' optimal forecasts of future variables, and from Christopher Sims's demonstration that there is a close connection between the concept of strict econometric exogeneity and the forms of the optimal predictors for a vector of time series. Building a model with rational expectations necessarily forces one to consider which set of other variables helps forecast a given variable, say income or the inflation rate. If variable y helps predict variable x, then Sims's theorems imply that x cannot be regarded as exogenous with respect to y. The result of this connection between predictability and exogeneity has been that in equilibrium macroeconometric models the distinction between endogenous and exogenous variables has not been drawn on an entirely *a priori* basis. Furthermore, special cases of the theoretical models, which often involve side restrictions on the R_j's not themselves drawn from economic theory, have strong *testable* predictions as to exogeneity relations among variables.

A key characteristic of equilibrium macroeconometric models is that as a result of the restrictions across the A_j, B_j, and C_j's, the models predict that in general the parameters in *many* of the equations will change if there is a policy intervention that takes the form of a change in one equation that describes how some policy variable is being set. Since they ignore these cross-equation restrictions, Keynesian models in general assume that all other equations remain unchanged when an equation describing a policy variable is changed. Our view is that this is one important reason that Keynesian models have broken down when there have occurred important changes in the equations governing policy variables or exogenous variables. Our hope is that the methods we have described will give us the capability to predict the consequences for all of the equations of changes in the rules governing policy variables. Having that capability is necessary before we can claim to have a scientific basis for making quantitative statements about macroeconomic policy.

At the present time, these new theoretical and econometric developments have not been fully integrated, although it is clear they are very close, both conceptually and operationally. Our preference would be to regard the best currently existing equilibrium models as prototypes of better, future models which will, we hope, prove of practical use in the formulation of policy. But we should not understate the econometric success already attained by equilibrium models. Early versions of these models have been estimated and subjected to some stringent econometric tests by McCallum [20], Barro [3], [4], and Sargent [27], with the result that they do seem capable of explaining some broad features of the business cycle. New and more sophisticated models involving more complicated cross-equation restrictions are in the works (Sargent [29]). Work to date has already shown that equilibrium models are capable of attaining within-sample fits about as good as those obtained by Keynesian models, thereby making concrete the point that the good fits of the Keynesian models provide no good reason for trusting policy recommendations derived from them.

6. Criticism of Equilibrium Theory

The central idea of the equilibrium explanations of business cycles as sketched above is that economic fluctuations arise as agents react to *unanticipated* changes in variables which impinge on their decisions. It is clear that *any* explanation of this general type must carry with it severe limitations on the ability of governmental policy to *offset* these initiating changes. First, governments must somehow have the ability to foresee shocks which are invisible to private agents but at the same time lack the ability to reveal this advance information (hence defusing the shocks). Though it is not difficult to write down theoretical models in which these two conditions are assumed to hold, it is difficult to imagine actual situations in which such models would apply. Second, the governmental countercyclical policy must *itself* be unanticipatable by private agents (certainly a frequently realized condition historically) while at the same time be systematically related to the state of the economy. Effectiveness then rests on the inability of private agents to recognize systematic patterns in monetary and fiscal policy.

To a large extent, criticism of equilibrium models is simply a reaction to these implications for policy. So wide is (or was) the consensus that *the* task of macroeconomics is the discovery of the particular monetary and fiscal policies which can eliminate fluctuations by reacting to private sector instability that the assertion that this task either should not, or cannot be performed is regarded as frivolous independently of whatever reasoning and evidence may support it. Certainly one must have some sympathy with this reaction: an unfounded faith in the curability of a particular ill has served often enough as a stimulus to the finding of genuine cures. Yet to confuse a possibly functional *faith* in the existence of efficacious, re-active monetary and fiscal policies with scientific evidence that such policies are known is clearly dangerous, and to use such faith as a criterion for judging the extent to which particular theories "fit the facts" is worse still.

There are, of course, legitimate issues involving the ability of equilibrium theories to fit the facts of the business cycle. Indeed, this is the reason for our insistence on the preliminary and tentative character of the particular models we now have. Yet these tentative models share certain features which can be regarded as *essential,* so it is not unreasonable to speculate as to the likelihood that *any* model of this type can be successful, or to ask: what will equilibrium business cycle theorists have in ten years if we get lucky?

Four general reasons for pessimism which have been prominently advanced are (a) the fact that equilibrium models postulate cleared markets, (b) the assertion that these models cannot account for "persistence" (serial correlation) of cyclical movements, (c) the fact that econometrically implemented models are linear (in logarithms), and (d) the fact that learning behavior has not been incorporated. We discuss each in turn in distinct subsections.

6.1 Cleared Markets

One essential feature of equilibrium models is that all markets clear, or that all observed prices and quantities be explicable as outcomes of decisions taken by individual firms and households. In practice, this has meant a conventional, competitive supply-equals-demand assumption, though other kinds of equilibrium can easily be imagined (if not so easily analyzed). If, therefore, one takes as a basic "fact" that labor markets do not clear one arrives immediately at a contradiction between theory and fact. The facts we actually have, however, are simply the available time series on employment and wage rates, plus the responses to our unemployment surveys. Cleared markets is simply a principle, not verifiable by direct observation, which may or may not be useful in constructing successful hypotheses about the behavior of these series. Alternative principles, such as the postulate of the existence of a third-party auctioneer inducing wage "rigidity" and noncleared markets, are similarly "unrealistic," in the not especially important sense of not offering a good description of observed labor market institutions.

A refinement of the unexplained postulate of an uncleared labor market has been suggested by the indisputable fact that there exist long-term labor contracts with horizons of two or three years. Yet the length *per se* over which contracts run does not bear on the issue, for we know from Arrow and Debreu that if infinitely long-term contracts are determined so that prices and wages are contingent on the same information that is available under the assumption of period-by-period market clearing, then precisely the same price-quantity process will result with the long-term contract as would occur under period-by-period market clearing. Thus equilibrium theorizing provides a way, probably the only way we have, to construct a *model* of a long-term contract. The fact that long-term contracts exist, then, has *no* implications about the applicability of equilibrium theorizing. Rather, the real issue here is whether actual contracts can be adequately accounted for within an equilibrium model, that is, a model in which agents are proceeding in their own best interests. Stanley Fischer [7], Edmund Phelps and John Taylor [26], and Robert Hall [12] have shown that some of the "nonactivist" conclusions of the equilibrium models are modified if one substitutes for period-by-period market clearing the imposition of long-term contracts drawn contingent on restricted information sets that are exogenously imposed and that are assumed to be independent of monetary and fiscal regimes. Economic theory leads us to predict that costs of collecting and processing information will make it optimal for contracts to be made contingent on a small subset of the information that could possibly be collected at any date. But theory also suggests that the particular set of information upon which contracts will be made contingent is not immutable but depends on the structure of costs and benefits to collecting various kinds of information. This structure of costs and benefits will change with every change in the exogenous stochastic processes facing agents. This theoretical presumption is supported by an examination of the way labor contracts differ across high-inflation and low-inflation countries and the way they have evolved in the United States over the last 25 years.

So the issue here is really the same fundamental one involved in the dispute between Keynes and the classical economists: Is it adequate to regard certain superficial characteristics of existing wage contracts as given when analyzing the consequences of alternative monetary and fiscal regimes? Classical economic theory denies that those characteristics can be taken as given. To understand the implications of long-term contracts for monetary policy, one needs a model of the way those contracts are likely to respond to alternative monetary policy regimes. An extension of existing equilibrium models in this direction might well lead to interesting variations, but it seems to us unlikely that major modifications of the implications of these models for monetary and fiscal policy will follow from this.

6.2 *Persistence*

A second line of criticism stems from the correct observation that if agents' expectations are rational and if their information sets include lagged values of the variable being forecast, then agents' forecast errors must be a serially uncorrelated random process. That is, on average there must be no detectable relationships between this period's forecast error and any previous period's forecast error. This feature has led several critics to conclude that equilibrium models are incapable of accounting for more than an insignificant part of the highly serially correlated movements we observe in real output, employment, unemployment and other series. Tobin has put the argument succinctly in [38]:

> One currently popular explanation of variations in employment is temporary confusion of relative and absolute prices. Employers and workers are fooled into too many jobs by unexpected inflation, but only until they learn it affects other prices, not just the prices of what they sell. The reverse happens temporarily when inflation falls short of expectation. This model can scarcely explain more than transient disequilibrium in labor markets.
>
> So how can the faithful explain the slow cycles of unemployment we actually observe? Only by arguing that the natural rate itself fluctuates, that variations in unemployment rates are substantially changes in voluntary, frictional, or structural unemployment rather than in involuntary joblessness due to generally deficient demand.

The critics typically conclude that the theory only attributes a very minor role to aggregate demand fluctuations and necessarily depends on disturbances to aggregate supply to account for most of the fluctuations in real output over the business cycle. As Modigliani [21] characterized the implications of the theory: "In other words, what happened to the United States in the 1930s was a severe attack of contagious laziness."

This criticism is fallacious because it fails to distinguish properly between "sources of impulses" and "propagation mechanisms," a distinction stressed by Ragnar Frisch in a classic 1933 paper [9] that provided many of the technical foundations for Keynesian macroeconometric models. Even though the new classical theory implies that the forecast errors which are the aggregate demand "impulses" are serially uncorrelated, it is certainly logically possible that "propagation mechanisms" are at work that convert these impulses into serially correlated movements in real variables like output and employment. Indeed, two concrete propagation mechanisms have already been shown in detailed theoretical work to be capable of performing precisely that function. One mechanism stems from the presence of costs to firms of adjusting their stocks of capital and labor rapidly. The presence of these costs is known to make it optimal for firms to spread out over time their response to the relative price signals that they receive. In the present context, such a mechanism causes a firm to convert the serially uncorrelated forecast errors in predicting relative prices into serially correlated movements in factor demands and in output.

A second propagation mechanism is already present in the most classical of economic growth models. It is known that households' optimal accumulation plans for claims on physical capital and other assets will convert serially uncorrelated impulses into serially correlated demands for the accumulation of real assets. This happens because agents typically will want to divide any unexpected changes in the prices or income facing agents. This dependence assets. Thus, the demand for assets next period depends on initial stocks and on unexpected changes in the prices or income-facing agents. This dependence makes serially uncorrelated surprises lead to serially correlated movements in demands for physical assets. Lucas [16] showed how this propagation mechanism readily accepts errors in forecasting aggregate demand as an "impulse" source.

A third likely propagation mechanism is identified by recent work in search theory.[11] Search theory provides an explanation for why workers who for some reason find themselves without jobs will find it rational not necessarily to take the first job offer that comes along but instead to remain unemployed for some period until a better offer materializes. Similarly, the theory provides reasons that a firm may find it optimal to wait until a more suitable job applicant appears so that vacancies will persist for some time. Unlike the first two propagation mechanisms mentioned, consistent theoretical models that permit that mechanism to accept errors in forecasting aggregate demand as an impulse have not yet been worked out for mainly technical reasons, but it seems likely that this mechanism will eventually play an important role in a successful model of the time series behavior of the unemployment rate.

In models where agents have imperfect information, either of the first two and most probably the third mechanism is capable of making serially correlated movements in real variables stem from the introduction of a serially uncorrelated sequence of forecasting errors. Thus, theoretical and econometric models have been constructed in which in principle the serially uncorrelated process of forecasting errors is capable of accounting for any proportion between zero and one of the steady-state variance of real output or employment. The argument

[11] For example [19], [22] and [18].

that such models must necessarily attribute most of the variance in real output and employment to variations in aggregate supply is simply wrong logically.

6.3 *Linearity*

Most of the econometric work implementing equilibrium models has involved fitting statistical models that are linear in the variables (but often highly nonlinear in the parameters). This feature is subject to criticism on the basis of the indisputable principle that there generally exist nonlinear models that provide better approximations than linear models. More specifically, models that are linear in the variables provide no method of detecting and analyzing systematic effects of higher than first-order moments of the shocks and the exogenous variables on the first moments of the endogenous variables. Such systematic effects are generally present where the endogenous variables are set by risk-averse agents.

There is no *theoretical* reason that most applied work has used linear models, only compelling technical reasons given today's computer technology. The predominant technical requirement of econometric work which imposes rational expectations is the ability to write down analytical expressions giving agents' decision rules as functions of the parameters of their objective functions and as functions of the parameters governing the exogenous random processes that they face. Dynamic stochastic maximum problems with quadratic objectives, which give rise to linear decision rules, *do* meet this essential requirement, which is their virtue. Only a few other functional forms for agents' objective functions in dynamic stochastic optimum problems have this same necessary analytical tractability. Computer technology in the foreseeable future seems to require working with such a class of functions, and the class of linear decision rules has just seemed most convenient for most purposes. No issue of *principle* is involved in selecting one out of the very restricted class of functions available to us. *Theoretically,* we know how to calculate via expensive recursive methods the nonlinear decision rules that would stem from a very wide class of objective functions; no new econometric principles would be involved in estimating their parameters, only a much higher computer bill. Further, as Frisch and Slutsky emphasized, linear stochastic difference equations seem a very flexible device for studying business cycles. It is an open question whether for explaining the central features of the business cycle there will be a big reward to fitting nonlinear models.

6.4 *Stationary Models and the Neglect of Learning*

Benjamin Friedman and others have criticized rational expectations models apparently on the grounds that much theoretical and almost all empirical work has assumed that agents have been operating for a long time in a stochastically stationary environment. As a consequence, typically agents are assumed to have discovered the probability laws of the variables that they want to forecast. As Modigliani made the argument in [21]:

> At the logical level, Benjamin Friedman has called attention to the omission from [equilibrium macroeconomic models] of an explicit learning mechanism, and has suggested that, as a result, it can only be interpreted as a description not of short-run but of long-run equilibrium in which no agent would wish to recontract. But then the implications of [equilibrium macroeconomic models] are clearly far from startling, and their policy relevance is almost nil (p. 6)

But it has been only a matter of analytical convenience and not of necessity that equilibrium models have used the assumption of stochastically stationary "shocks" and the assumption that agents have already learned the probability distributions that they face. Both of these assumptions can be abandoned, albeit at a cost in terms of the simplicity of the model.[12] In fact, within the framework of quadratic objective functions, in which the "separation principle" applies, one can apply the "Kalman filtering formula" to derive optimum linear decision with time dependent coefficients. In this framework, the "Kalman filter" permits a neat application of Bayesian learning to updating optimal forecasting rules from period to period as new information becomes available. The Kalman filter also permits the derivation of optimum decision rules for an interesting class of nonstationary exogenous processes assumed to face agents. Equilibrium theorizing in this context thus readily leads to a *model* of how process nonstationarity and Bayesian learning applied by agents to the exogenous variables leads to time-dependent coefficients in agents' decision rules.

While models incorporating Bayesian learning and stochastic nonstationarity are both technically feasible and consistent with the equilibrium modeling strategy, almost no successful applied work along these lines has come to light. One reason is probably that nonstationary time series models are cumbersome and come in so many varieties. Another is that the hypothesis of Bayesian learning is vacuous until one either arbitrarily imputes a prior distribution to agents or develops a method of estimating parameters of the prior from time series data. Determining a prior distribution from the data would involve estimating a number of initial conditions and would proliferate nuisance parameters in a very unpleasant way. It is an empirical matter whether these techniques will pay off in terms of explaining macroeconomic time series; it is not a matter distinguishing equilibrium from Keynesian macroeconometric models. In fact, no existing Keynesian macroeconometric model incorporates either an economic model of learning or an economic model in any way restricting the pattern of coefficient nonstationarities across equations.

The macroeconometric models criticized by Friedman and Modigliani, which assume agents have "caught on" to the stationary random processes they face, give rise to systems of linear stochastic difference equations of the form (1), (2), and (4). As has been known for a long time, such stochastic difference equations generate series that "look like" economic time series. Further, if viewed as *structural* (i.e., invariant with respect to policy

[12] For example, see Crawford [5] and Grossman [11].

interventions) the models have some of the implications for countercyclical policy that we have described above. Whether or not these policy implications are correct depends on whether or not the models are structural and not at all on whether the models can successfully be caricatured by terms such as "long run" or "short run."

It is worth reemphasizing that we do not wish our responses to these criticisms to be mistaken for a claim that existing equilibrium models can satisfactorily account for all the main features of the observed business cycle. Rather, we have argued that no sound reasons have yet been advanced which even suggest that these models are, as a class, *incapable* of providing a satisfactory business cycle theory.

7. Summary and Conclusions

Let us attempt to set out in compact form the main arguments advanced in this paper. We will then comment briefly on the main implications of these arguments for the way we can usefully think about economic policy.

First, and most important, existing Keynesian macroeconometric models are incapable of providing reliable guidance in formulating monetary, fiscal and other types of policy. This conclusion is based in part on the spectacular recent failures of these models, and in part on their lack of a sound theoretical or econometric basis. Second, on the latter ground, there is no hope that minor or even major modification of these models will lead to significant improvement in their reliability.

Third, *equilibrium* models can be formulated which are free of these difficulties and which offer a different set of principles which can be used to identify structural econometric models. The key elements of these models are that agents are *rational,* reacting to policy changes in a manner which is in their best interests privately, and that the impulses which trigger business fluctuations are mainly unanticipated shocks.

Fourth, equilibrium models already developed account for the main qualitative features of the business cycle. These models are being subjected to continued criticism, especially by those engaged in developing them, but arguments to the effect that equilibrium theories are, in principle, incapable of accounting for a substantial part of observed fluctuations appear due mainly to simple misunderstandings.

The policy implications of equilibrium theories are sometimes caricatured, by friendly as well as unfriendly commentators, as the assertion that "economic policy does not matter" or "has no effect."[13] This implication would certainly startle neoclassical economists who have successfully applied equilibrium theory

[13] A main source of this belief is probably Sargent and Wallace [30], in which it was shown that in the context of a fairly standard macroeconomic model, but with agents' expectations assumed rational, the choice of a reactive monetary rule is of *no* consequence for the behavior of real variables. The point of this example was to show that within *precisely* that model used to rationalize reactive monetary policies, such policies could be shown to be of no value. It hardly follows that *all* policy is ineffective in *all* contexts.

to the study of innumerable problems involving important effects of fiscal policies on resource allocation and income distribution. Our intent is not to reject these accomplishments, but rather to try to *imitate* them, or to extend the equilibrium methods which have been applied to many economic problems to cover a phenomenon which has so far resisted their application: the business cycle.

Should this intellectual arbitrage prove successful, it will suggest important changes in the way we think about policy. Most fundamentally, it directs attention to the necessity of thinking of policy as the choice of stable "rules of the game," well understood by economic agents. Only in such a setting will economic theory help us to predict the actions agents will choose to take. Second, this approach suggests that policies which affect behavior mainly because their consequences cannot be correctly diagnosed, such as monetary instability and deficit financing, have the capacity only to disrupt. The deliberate provision of misinformation cannot be used in a systematic way to improve the economic environment.

The *objectives* of equilibrium business cycle theory are taken, without modification, from the goal which motivated the construction of the Keynesian macroeconometric models: to provide a scientifically based means of assessing, quantitatively, the likely effects of alternative economic policies. Without the econometric successes achieved by the Keynesian models, this goal would be simply inconceivable. Unless the now evident limits of these models are also frankly acknowledged, and radically different new directions taken, the real accomplishments of the Keynesian Revolution will be lost as surely as those we now know to be illusory.

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Discussion

Benjamin M. Friedman

Professors Lucas and Sargent have done an admirable job of providing a paper that stimulates our thinking along several different lines, all central to the inflation-and-unemployment theme of this conference. Consequently there is much to which I could respond in my assigned role as discussant. For example, I could easily spend my allotted time applauding their path-breaking work on expectations and their progress to date in integrating this work into modern macroeconomics. Or I could concentrate entirely on the relationship of their work to that of the other economists whom their paper so harshly criticizes. Or I could focus on their interpretation of historical facts, or on their exegesis – both stated and implied – of the literature of macroeconomics. Their paper is indeed thought-provoking in a variety of directions. Given the limited available time, I will reluctantly leave their fine accomplishments on the expectations front to speak for themselves and will instead focus my discussion on what I interpret to be the principal message of their paper.

Professors Lucas and Sargent argue vigorously that a methodological divide separates their work from the existing corpus of modern macroeconomics. Specifically, they state that "the Keynesian Revolution was . . . a revolution in method . . ." and that " . . . if one does not view the revolution in this way, it is impossible to account for some of its most important features." They further state that equilibrium business cycle theory, for which their paper so eloquently argues, is essentially characterized by the adoption of a different methodological approach to macroeconomic research. According to Professors Lucas and Sargent, the central distinction between Keynesian macroeconomics on the one hand, and the work which they and their associates and followers pursue on the other, lies in the rejection by the one and the acceptance by the other of the "classical" postulates of market clearing and especially of optimizing behavior on the part of economic agents including businessmen, consumers, and so on. According to their description, the methodological essence – and therefore the fundamental feature – of the Keynesian revolution was the abandonment of the attempt to derive behavioral models from the assumption that people act as well as they can in their own self-interest, and in its place the systematic resort to " . . . a model in which rules of thumb . . . took the place of decision functions that a classical economist would insist be derived from the theory of choice." As examples of such ad hoc, arbitrary rules of thumb standing at the core of Keynesian macroeconomics, they cite the familiar consumption, investment and

Benjamin M. Friedman is Associate Professor of Economics at Harvard University.

money demand functions – and, of course, Keynes's own assumption of a money wage rate determined outside the model. By contrast, the feature of the new equilibrium business cycle research that Professors Lucas and Sargent emphasize is that it eschews such resort to nonoptimizing behavior in favor of derived behavioral propositions, parameter restrictions only to the extent that the time series data validate them, and cross-equation restrictions derived especially from processes of dynamic optimization.

Let me say straight out that I cannot recognize this methodological distinction drawn by Professors Lucas and Sargent, except perhaps as some gross caricature out of date by more than a generation. Hence I believe that the central message of their paper does not stand up to careful appraisal. To explain why will require some brief comments on both the tradition which they derogate and that which they advance. I find that somewhat unfortunate, because I have no taste for shouldering responsibility for any broad-based defense of what could be regarded as "status quo" economics. (I am here reminded of Secretary of State Acheson's remarks in the matter of Mr. Edmund Clubb.) Instead, my purpose is merely to discuss critically the principal point argued by Professors Lucas and Sargent in their paper, the sharp methodological distinction posited between Keynesian macroeconomics and equilibrium business cycle theory.

Which of the two shall we address first? I prefer to begin with a quotation which may be familiar to some people here:

> The economic theory which underlies the construction of our model is classical in its methodology. We view the economic system as composed of two groups. One group consists of households and the other of business firms. It is assumed that the individuals in each group follow specific types of behavior patterns . . . For example, we assume that entrepreneurs behave so as to maximize profits, subject to the constraint that they operate according to the technological possibilities expressed by their production functions . . . we should not be misled by those economists who insist that entrepreneurs do not know the meaning of partial derivatives and hence do not behave so as to maximize profits or psychic income of some type . . . We assume further that households behave so as to maximize their satisfactions or utilities, subject to budgetary constraints; and in this way we obtain the equations of consumer demand.

No doubt, one supposes after reading Professors Lucas and Sargent, these must be the words of either a pre-Keynesian classical theorist or a modern proponent of equilibrium business cycle theory. Correct? No. The publication date was in fact 1950. In that case, no doubt the author must have been an anti-Keynesian dissident whom the mainstream of the Keynesian macroeconometric literature either rejected or simply passed by without notice. Correct? No, again. The author was in fact Lawrence Klein, and the source was his *Economic Fluctuations in the United States* – the single book that, more than any other, set the path for a generation of quantitative research on Keynesian macroeconometrics.

Since the identification of the quotation's source has now revealed what its substance did not – that is, that I have begun my discussion with Keynesian economics rather than equilibrium business cycle theory – let us next examine somewhat closer the ad hoc "rules of thumb" which Professors Lucas and Sargent cite among the basic building blocks of the modern Keynesian macroeconometric model. Does the large literature of the life-cycle model of consumer behavior, in which the crux of the decision is resource allocation over a lifetime, jibe with the description of the consumption function as an arbitrary rule of thumb not derived "... from any consistently posed dynamic optimization problems"? Does the proliferating literature of investment behavior call to mind something that "... took the place of decision functions that a classical economist would insist be derived from the theory of choice"? And what about portfolio behavior in general and the demand for money in particular – in fact perhaps the most obvious place to note the application of explicitly derived optimizing behavior including the use of cross-equation restrictions? Finally, as for Keynes's own use of the exogenous money wage assumption, I will not go into the many attempts (mostly unsuccessful) to explain wage-setting behavior either analytically or econometrically. Professors Lucas and Sargent have cogently argued that exogeneity is a statistical property subject to rigorous testing along the lines set out by C.W.J. Granger and Christopher Sims, and they advocate such tests as an essential first step in empirical model construction. It is therefore interesting to note in this context that the battery of Granger-Sims tests presented with Professor Sargent's well-known "Classical Macroeconometric Model for the United States" by and large suggested that the money wage rate was indeed exogenous with respect to the variables in the model (which, incidentally, the money stock was not) while itself having a causal influence on the unemployment rate and the interest rate.

I could proceed in this vein for some time, enumerating examples of the use, by economists *within* the existing macroeconomic tradition, of behavioral relationships explicitly grounded in optimizing behavior. I will not do so for two reasons. First, with limited time available it will be more interesting to focus directly on equilibrium business cycle research. And, second, I have already stated my unwillingness to assume the role of all-purpose defender of any status quo body of economics as it currently exists. Then, too, Ray Fair (from whose recent book I could have chosen a quotation just as apt as the one from Klein a quarter-century ago) will presumably provide examples of explicit optimizing behavior in his own work when he presents his paper tomorrow. I can summarize my discussion so far simply by saying that one-half of the methodological contrast asserted by Professors Lucas and Sargent – in particular, the absence of optimizing behavior in Keynesian macroeconomics – does not withstand close inspection. Equilibrium business cycle theory has no monopoly on optimizing behavior.

What about the other half of this supposed contrast in basic method? Is it true that equilibrium business cycle theory eschews arbitrary restrictions? Here, to argue that it does not, I will cite only two examples, one theoretical and one empirical. But I think these two examples go quite to the heart of the matter.

My theoretical example is the derivation of the aggregate supply function, originally posited by Professor Lucas, that provides the key to a form of the "natural rate" hypothesis consistent with a negative short-run correlation between unemployment and inflation. Professors Lucas and Sargent concisely summarize the argument in their paper:

> On the basis of their limited information – the lists that they have of current and past absolute prices of various goods – agents are assumed to make the best possible estimate of all of the *relative* prices that influence their supply and demand decisions. Because they do not have all of the information that would enable them to compute perfectly the relative prices they care about, agents make errors in estimating the pertinent relative prices . . . In particular, under certain conditions, agents will tend temporarily to mistake a general increase in all absolute prices as an increase in the relative price of the good that they are selling, leading them to increase their supply of that good over what they had previously planned . . . This increase of output above what it would have been will occur whenever this period's average economy-wide price level is above what agents had expected this period's average economy-wide price level to be on the basis of previous information. Symmetrically, output will be decreased whenever the aggregate price turns out to be lower than agents had expected.

The story sounds plausible enough. If a cobbler sees shoe prices rising and does not yet realize that leather prices (and all others) are rising in step, he will mistakenly perceive a relative price shift giving an advantage to producing more shoes. As a good optimizer he will accordingly increase production because of an imperfectly perceived rise in all prices.

But what if, instead, the cobbler first learns that the price of leather is rising and does not yet realize that the market will bear a higher price for his shoes? In this case he will mistakenly perceive a relative price shift giving a *dis*advantage to producing shoes. As a good optimizer he will now *de*crease production because of an imperfectly perceived rise in all prices.

The point of this illustration is that the crucial aggregate supply function on which equilibrium business cycle theory relies is valid if, and only if, agents learn the prices of goods they are *selling* before learning the prices of goods they are *buying.* If instead a producer typically learns the price he has to pay for his *inputs* before learning the price at which he can market his *output,* this aggregate supply function implies results exactly opposite to those which it is assumed to produce in equilibrium business cycle theory as described by Professors Lucas and Sargent.

I do not have evidence adequate to decide, for an economy with complicated market arrangements like those in the United States, what is on average the correct chronological order of price learning. The input-then-output ordering however, seems to me at least as plausible as the output-then-input ordering that Professors Lucas and Sargent require. In the absence of an outright assumption grounded only on the premise that it must be thus in order to fit the data – an assumption that would, if made by someone else, probably be called an ad hoc arbitrary restriction – how do they know that the output-then-input ordering is the right description of the imperfect information flow in the modern economy?

My second example is empirical. Professors Lucas and Sargent caution against any tendency to "... understate the degree of econometric success already attained ..." by equilibrium business cycle models, stating that "these models have been subjected to testing under standards more stringent than customarily applied to macroeconometric models ..." Of the three studies which they then cite (one is Professor Sargent's model, to which I have already referred), one is Robert Barro's well-known demonstration that unemployment in the United States is correlated only with the *un*anticipated component of money growth and not with the anticipated component. Since the Federal Reserve publishes no series entitled "unanticipated money growth," one naturally asks how this test proceeds. Before answering this question, however, it is instructive to recall some remarks of Professors Lucas and Sargent about Keynesian models:

> Such structural equations are usually identified by the assumption that, for example, the expectation about the factor prices or rate of inflation attributed to agents is a function *only* of a few lagged values of the variable itself which the agent is assumed to be forecasting ... the restrictions on expectations that have been used to achieve identification are entirely arbitrary and have not been derived from any deeper assumption reflecting first principles about economic behavior. No general first principle has ever been set down which would imply that, say, the expected rate of inflation should be modeled as a linear function of lagged rates of inflation alone with weights that add up to unity ...

How, then, did this test, supposedly under more stringent than customary standards, proceed? In fact, the "anticipated money growth" series was simply a two-period lag on past money growth, plus an allowance for Federal expenditures and the unemployment rate. Not surprisingly, this rather crude "anticipated money growth" series accounted for only a part of the variance of actual money growth during the sample period, leaving much of the actual variance – as well as the covariance with unemployment – for the residually determined "unanticipated money growth" series.

Did this procedure – that would, if used by someone else, probably be called an ad hoc arbitrary restriction – make a difference for the outcome of the test? Yes, it did. David Small has shown that allowing agents' anticipations of money growth to rely on a less restrictive view of how Federal expenditures influence money growth, especially during wars, produces an "anticipated money growth" series that accounts for much more of the variance of actual money growth – and with it the covariance with unemployment.

As promised, I will now stop this line of argument after but those two important examples. I can summarize this part of my discussion by saying that the second half of the methodological contrast asserted by Professors Lucas and Sargent – in particular, the lack of arbitrary restrictions in equilibrium business cycle models – does not stand up either. Keynesian macroeconomics has no monopoly on ad hoc restrictions.

Finally, what can we say about equilibrium business cycle theory on its own merits, apart from the question of a methodological divide or lack thereof between it and Keynesian macroeconomics? I have argued before that such theories have an essentially long-run character – that is, that they use a form of what I call "asymptotic reasoning" to deal with questions that many people pose, and some economists attempt to answer, within a shorter time frame. In response to a question about whether or not to implement a particular monetary policy to combat today's problems, for example, the familiar refrain notes that if we always alter money growth in response to economic conditions, optimizing agents will discover that fact and act accordingly. Indeed, Professors Lucas and Sargent explicitly state in their paper that equilibrium business cycle theory "... directs attention to the necessity of thinking of policy as the choice of stable 'rules of the game,' well understood by economic agents." I think that that is my point too. *Over the long run,* there is no coherent way of describing a policy that consists of a set of unrelated single actions. But in many circumstances people do want to be able to discuss whether, for example, a $20 billion tax cut in 1978 is helpful or harmful – not whether it would be wise or foolish to enact a rule calling for a tax cut of similar proportion at the corresponding point of all future business expansions. Already in 1978 businessmen, workers, and consumers (economic agents, if we must call them that) are forming expectations and taking actions accordingly. To argue that repeated tax cuts would over time come to alter their expectations is to apply asymptotic reasoning to a different kind of problem. (It is true, of course, that one should always keep in mind the future consequences of his current actions; but the points at issue here are, I believe, more fundamental than the mere assertion that the political process applies too high an interest rate in discounting the future.)

In the work to which Professors Lucas and Sargent refer in their paper, I argued on the basis of information requirements that the conclusions about the impotence of monetary policy, from what they now call equilibrium business cycle theory, were really *long-run* conclusions and hence not very surprising, since most economists accept them and most macroeconometric models embody them as descriptions of long-run equilibrium. After reading their new paper, I see yet further reasons why one should regard these models as having a fundamentally long-run orientation. The primary example from the paper is the question of institutional wage- and price-setting arrangements. When they first evolved, these models simply assumed the existence of flexible wages and prices. The next cut added some realism by noting the undeniable existence of long-term wage contracts. More recently researchers in this vein have acknowledged widespread "stickiness," both explicit in formal contracts and also implicit in less formal understandings, of wages as well as prices. In their paper, however, Professors Lucas and Sargent reply by noting that even these institutional arrangements have to be determined somehow and that they should be considered not exogenous but endogenous to the model. While I sympathize entirely with this approach, I again ask what is the time frame of a model that fully endogenizes the determination of such institutional arrangements. To cite only one example from an area familiar to most of the nonacademic participants at this conference, well-developed financial markets are often noted as a field of business in which innovation is, by comparison with the rest of the economy, relatively inexpensive and therefore rapid. Nevertheless, despite more than a decade of rapid and variable price inflation in the United States, our financial markets have yet to produce an instrument with which the investor willing to pay for it can buy protection of his purchasing power.

I especially applaud, although with some feeling of irony, the explicit recognition by Professors Lucas and Sargent of the role of the constraints subject to which equilibrium business cycle theory assumes that people optimize. I note some irony here because, heretofore, those of us who have emphasized the implications of transactions costs and have constructed arguments crucially depending on slow adjustments have often met with the automatic (though unwarranted) criticism of denying optimizing behavior. The presumption, of course, was that behavioral relations more explicitly derived from simpler models were necessarily better than behavioral relations less explicitly derived from more complicated models; and realistic models of dynamic adjustment in the presence of transactions costs can be very complicated indeed. Perhaps, now that Professors Lucas and Sargent have turned to costs of adjustment as the route to explaining the "persistence" of unemployment using equilibrium business cycle theory, there may be opportunities for more constructive interchange here.

As equilibrium business cycle theory comes to rely more heavily on such adjustment costs, however, I hope that it will be possible for it to assume a testable – that is, a potentially falsifiable – form, rather than degenerate into a mere semantic distinction. In practice, it is often extremely difficult to distinguish a theory which asserts that markets always clear but that adjustment costs temporarily (and how long is that?) make people's demands and supplies different from what they will be later on, from an alternative theory which asserts that because of adjustment costs markets temporarily do not clear. In my own work on price and yield determination in financial asset markets, for example, I have always used the former verbiage, and I think that that is what Professors Lucas and Sargent have in mind too; many other people, however, choose to interpret this work as equivalent to positing nonclearing markets. No one knows, of course, whether this new emphasis on adjustment costs will produce better business cycle models, or whether the best route lies instead in some other approach, but I for one can certainly wish them all good luck in the effort.

In conclusion, therefore, I think that there is much to applaud in the work that Professors Lucas and Sargent are doing, and that it is not so far removed from what others of us do as they suggest. Indeed, if their paper had simply said that the inadequate treatment of expectations constitutes a major weakness in modern macroeconomics, and that they had already made significant progress on this point and were continuing to pursue it, my own discussion would have been altogether different from what I have said. In fact, however, the main argument of their paper is that their work marks a fundamental methodological departure from the corpus of Keynesian macroeconomics, and here I have been forced to disagree sharply. Equilibrium business cycle theory has no monopoly on optimizing behavior, and Keynesian macroeconomics has none on ad hoc arbitrary restrictions.

The same problem arises in interpreting the recent empirical evidence. A reader of Professors Lucas and Sargent who had not independently been exposed to the data would probably be surprised to learn that in the United States, which has pursued one kind of macroeconomic policy, the unemployment rate has fallen from 9 percent at the recession's trough three years ago to 6 percent today (which many economists argue is almost full employment), while throughout Europe, where fiscal policies especially have been starkly different, unemployment has not fallen at all. (Furthermore, such a reader would probably be surprised, too, to learn that in the United States the primary macroeconomic problem is now accelerating inflation, while in Europe inflation rates have decelerated markedly and continue to do so.) I will not pursue these casual observations, especially since Stephen McNees' paper has already presented the relevant evidence in substantial detail. Whether that evidence strikes our hypothetical reader as showing that, in the words of Professors Lucas and Sargent, macroeconometric models' predictions have been "wildly incorrect," and whether he would recognize in it "the spectacular failure of the Keynesian models in the 1970s" and the associated "econometric failure on a grand scale," I leave to others to decide. Nevertheless, here as well as with respect to the premises on method that comprise the central focus of their paper, a lower rhetorical profile would better advance the cause of scientific interchange.

Response to Friedman†

Robert E. Lucas and Thomas J. Sargent

Our understanding of the purpose of the Conference was to discuss certain outstanding issues in macroeconomics in the hope of increasing general understanding of the potential role of economic theory in improving public policy. Since both of us are on record as rather severe critics of Keynesian macroeconometric models, we assumed that we were included in the program to express this dissenting view as forcefully and as accurately as possible. This we attempted to do, using both plain English and the technical language of econometrics and economic theory as best we could.

Benjamin Friedman's comments provide clear testimony to the complete failure of our efforts to engage in substantive discussion of the reliability of current macroeconomic models. Most of his comments are devoted to a defense of the proposition that: "Equilibrium business cycle theory has no monopoly on optimizing behavior, and Keynesian macroeconomics has none on ad hoc arbitrary restrictions." Friedman makes no effort to explain either how this proposition is related to anything in our paper (it is not) or what possible bearing it might have on the questions of economic policy which we thought were under discussion.

Professor Friedman also expressed skepticism on some details of our recent research, as well as on some valuable related work by Robert Barro. Though we do not agree with all these comments, they are, in tone and in substance, no more critical of that research than we have been ourselves, both elsewhere and in our paper. For example, we view the technical considerations raised in Sargent (28) as providing more compelling reasons for exercising caution in interpreting Sargent's and Barro's empirical results than do Friedman's remarks. Further, the reader can judge whether or not Friedman has strengthened the extensive caveats made in Sargent (27).[1] Although we feel Friedman's detailed substantive comments are all answerable, we will not respond to them further here.

In his concluding paragraph, Friedman objects to our "rhetorical profile," an objection which several others also expressed at the Conference. To illustrate his point, he cites our reference to "wildly incorrect" predictions of Keynesian

[1] It should be pointed out that the econometric work in Sargent (27), Sargent and Sims (31), and Sims (33) does not reveal that the "money wage rate was indeed exogenous with respect to the variables in the model." Reference numbers refer to those in the Lucas-Sargent paper.

†This reply was written after the conclusion of the Conference and is not intended as a transcript or summary of any remarks made there.

macroeconometric models, to "the spectacular failure of the Keynesian models in the 1970s," or their "econometric failure on a grand scale." These phrases were intended to refer to a specific and well-documented historical event. In 1970, the leading econometric models predicted that an inflation of 4 percent on a sustained basis would be associated with unemployment rates less than 4 percent. This prediction was not one which was teased from the models by unsympathetic critics; on the contrary, it was placed by the authors of these models and by many other economists at the center of a policy recommendation to the effect that such an expansionary policy be deliberately pursued. We recognize that comparison between the experience of the 1970s and the tradeoffs for this period which were forecast at the beginning of the decade may induce some discomfort, but if one is to discuss this well-documented discrepancy, what language is appropriate? Should these forecasts be termed *"accurate,"* or "an econometric *success?"* Or shall these questions be left, as Friedman suggests, "to others to decide"?

The "rhetorical profile" adopted in our paper was not chosen independently of the arguments developed using more precise and technical language in the text, and more fully developed by each of us in earlier writings. It was, on the contrary, an attempt to summarize the main implications of this work in as clear and graphic a way as we could find. If this research is flawed in some essential way, it is difficult to see how softening our rhetoric will help matters. If the implications we have drawn are close to the mark, how can "the cause of scientific interchange" be best served by summarizing them in a way which averages what we believe to be true with what others find pleasant or familiar?