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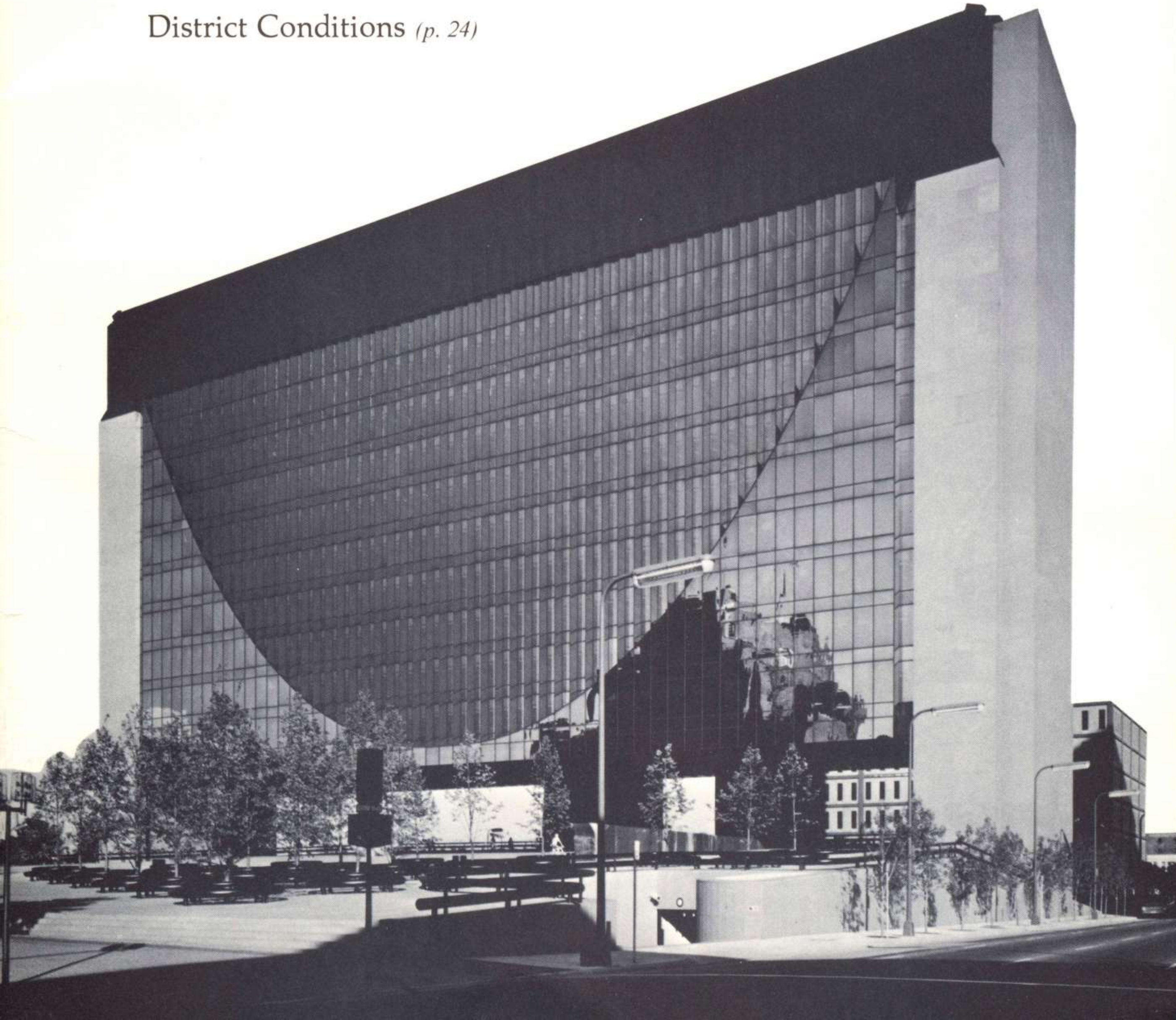

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After Keynesian Macroeconomics*

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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 benefitting 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 as to how this windfall should be distributed, but it seemed a simple lapse of logic to oppose the windfall itself. Understandably and correctly, noneconomists met this promise with skepticism at first; 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 balanced budgets. On the contrary, they were accompanied by massive government 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 now facing contemporary students of the business cycle is to sort through the wreckage, determining which features of that remarkable intellectual event called the 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 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 can be compared to data

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in a systematic way and which 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 is 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 suggests certain principles which a useful theory of business cycles must have. We conclude by reviewing some recent research consistent with these principles.

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' (1936)[1] 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.

Because of this, neither the success of the Keynesian Revolution nor its eventual failure can be understood at the purely verbal level at which Keynes himself wrote. It is 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 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[2]

$$A_0y_t + A_1y_{t-1} + \ldots + A_my_{t-m} = B_0x_t + B_1x_{t-1} + \ldots + B_nx_{t-n} + \varepsilon_t \qquad (1)$$

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

Here y_t is an $(L\times1)$ vector of endogenous variables, x_t is a $(K\times1)$ vector of exogenous variables, and ε_t and u_t are each $(L\times1)$ vectors of random disturbances. The matrices A_j are each $(L\times L)$; the B_j's are $(L\times K)$, and the R_j's are each $(L\times L)$. The $(L\times L)$ disturbance process u_t is assumed to be a serially uncorrelated process with $Eu_t=0$ and with contemporaneous covariance matrix $Eu_tu_t' = \Sigma$ and $Eu_tu_s' = 0$ for all $t\neq s$. The defining characteristics of the exogenous variables x_t is that they are uncorrelated with the ε's at all lags so that Eu_tx_s' is an $(L\times K)$ 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[3] 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

$$y_t = -P_1y_{t-1} - \ldots - P_{r+m}y_{t-r-m} + Q_0x_t + \ldots + Q_{r+n}x_{t-n-r} + A_0^{-1}u_t \qquad (3)$$

where[4]

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[1]Author names and years refer to the works listed at the end of this paper.

[2]Linearity is a matter of convenience, not principle. See *Linearity* section below.

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

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

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

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

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

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

where $Ea_t = 0$ and $Ea_t \cdot x_{t-j} = 0$ for $j \geq 1$ 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. All that examination of the data by themselves can deliver is reliable estimates of those parameters.

It is not generally possible to work backward 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's, B's, 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's, B_j's, 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, $s \leq t$, 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, that is, 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 a highly complex way (see the equations defining P_s and Q_s above). Unless one knows 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 highly general, 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.

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 three categories:[5]

(a) A priori setting of many of the elements of the

[5]These three categories certainly do not exhaust the set of possible identifying restrictions, but they're the ones most identifying restrictions in Keynesian macroeconometric models fall into. 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's, B_j's, and C_j's, neither of which is extensively used in Keynesian macroeconometrics.

A_j's and B_j's to zero.

(b) Restrictions on the orders of serial correlation and the extent of cross-serial correlation of the disturbance vector ε_t, restrictions which amount to a priori setting of many elements of the R_j's to zero.

(c) A priori classifying of variables as 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 type of restriction.

Keynes' *General Theory* was rich in suggestions for restrictions of type (a). In it he 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's and B_j's. Similarly, the liquidity preference relation expressed the demand for money as a function of only income and an interest rate. 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 because the errors are 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 proven 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, thus 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 meanings of the words *endogenous*—"determined by the [economic] system"—and *exogenous*—"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 restrictions of the three types reviewed above, their structural parameters A_i, B_j, R_k could be identified. Using this estimated structure, the models could be simulated 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 both theoretical and statistical developments. Many of these developments are due to efforts of researchers sympathetic to the Keynesian tradition, and many were advanced well before the spectacular failure of the Keynesian models in the 1970s.[6]

Since its inception, macroeconomics has been criticized for its lack of foundations in microeconomic and general equilibrium theory. As was recognized early on by astute commentators like Leontief (1965, disapprovingly) and Tobin (1965, approvingly), the creation of a distinct branch of theory with its own distinct postulates was Keynes' 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.[7] But

[6]Criticisms of the Keynesian solutions of the identification problem along much the following lines have been made in Lucas 1976, Sims forthcoming, and Sargent and Sims 1977.

[7]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 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; its econometric implications

Continued on next page

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 suggested by Keynes or those imposed by macroeconometric models.

Let us consider one example with 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 the best models, for example, investment demand typically is supposed to respond to businesses' expectations of future tax credits, tax rates, and factor costs, and 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 the expectation about, say, factor prices or the rate of inflation attribute to agents is a function only of a few lagged values of the variable 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 they 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 act 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 them 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 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_j's.

Turning to the third category, all existing large models adopt an a priori classification of variables as either strictly endogenous variables, the y_t's, or 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 (1972) has shown that in a time series context the hypothesis of econometric

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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 30 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 emphasized long ago by Tobin (1965), 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.

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' ought to be used routinely to check classifications 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. (See, for example, Ando 1977, pp. 209-10, and L. R. Klein in Okun and Perry 1973, p. 644.)

Failure of Keynesian Macroeconometrics

There are, therefore, a number of theoretical reasons for believing that the parameters identified as structural by current macroeconomic methods are not in fact structural. That is, we see no reason 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 Muench et. al. 1974.) 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 to a decisive test in the 1970s. A key element in all Keynesian models is a trade-off 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 U.S. unemployment rate of 4 percent as consistent with a 4 percent annual rate of inflation. Based on this prediction, many economists at that time urged a deliberate policy of inflation. Certainly the erratic "fits and starts" character of actual U.S. policy in the 1970s cannot be 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 rates 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 model based on roughly the same ideas. The failure has, however, already had some important consequences, with serious implications for both economic policymaking and 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 counterargument 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 such controls 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, more attention and respect is accorded to the theoretical casualties of the Keynesian Revolution, to the ideas of Keynes' contemporaries and of earlier economists whose thinking has been regarded for years as outmoded.

No one can 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 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 developed so far. 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.

Equilibrium Business Cycle Theory

Before the 1930s, economists 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 explaining the characteristics of business cycles was impossible within the discipline imposed by classical economic theory, a discipline imposed by its insistence on adherence to the two postulates (a) that markets clear and (b) that agents act in their own self-interest. The outstanding facts that seemed impossible to reconcile with these two postulates were the length and severity of business depressions and the large-scale unemployment they entailed. A related observation was that measures of aggregate demand and prices were 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 straightjacket (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 some used both *equilibrium* and *classical* interchangeably with *ideal*. Thus an economy in classical equilibrium would be both unchanging and unimprovable by policy interventions. With terms used in this way, it is no wonder that few economists regarded equilibrium theory as a promising starting point to understand business cycles and design policies to mitigate or eliminate them.

In recent years, the meaning of the term *equilibrium* has changed so dramatically that a theorist of the 1930s would not recognize it. An economy following a multivariate stochastic process is now routinely described 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 (1964) and G. Debreu (1959), 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 (1973), 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 some of us

[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 identi-
Continued on next page

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 counterrevolutionary, 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 Federal Reserve-MIT-Penn 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 inexplicable within the confines of classical postulates. The equilibrium models also provide reasons for understanding why estimated Keynesian models fail to hold up outside 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 was its apparent 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. 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. But the pervasive presence of 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.

We now have rigorous theoretical models which illustrate how these correlations can emerge while retaining the classical postulates that markets clear and agents optimize (Phelps 1970 and Lucas 1972, 1975). 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 still assume that markets clear and that agents optimize; agents make their supply and demand decisions based on real variables, including perceived relative prices. However, 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 necessary 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 tend temporarily to mistake a general increase in all absolute prices as an increase in the relative price of the good they are selling, leading them to increase their supply of that good over what they had previously planned. Since on average everyone is making the same mistake, aggregate output rises above what it would have been. This increase of output above what it would have been occurs whenever this period's average economy-wide price level is above what agents had expected it to be on the basis of previous information. Symmetrically, aggregate output decreases whenever the aggregate price turns out to be lower than agents had expected. The hypothesis of *rational expectations* is being imposed here: agents are assumed to make the best possible use of the limited information they have and to know the pertinent objective probability distri-

fication in Sargent 1978. 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 underidentification.

A somewhat different class of examples comes from the difficulties in using time series observations to refute the view that agents only respond to unexpected changes in the money supply. In the equilibrium macroeconometric models we will describe, predictable changes in the money supply do not affect real GNP or total employment. In Keynesian models, they do. 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 1976b).

butions. This hypothesis is imposed by way of adhering to the tenets of equilibrium theory.

In the new classical 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 easy to show that the theory implies correlations between revisions in 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 new theory can easily 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 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 (1948) and Simons (1936) 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, that is, 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 theory can explain some of the correlations long thought to invalidate classical macroeconomic theory, it 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 standard sense have been constructed which capture some of the main features of the new classical theory. (See, for example, Sargent 1976a.)[9] In particular, these models incorporate the hypothesis that expectations are rational or that agents use all available information. To some 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 rely heavily 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 they 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. The 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 rapidly being 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 that the models assign important roles to agents' optimal forecasts of future variables and from Christopher Sims' (1972) 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, the Sims' theorems imply that x cannot be regarded as exogenous with respect to y.

[9]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 (1977) and Geweke (1977). These models amount to a statistically precise way of implementing Wesley Mitchell's notion that a small number of common influences 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 atheoretical (but see the comments of Ando and Klein in Sims 1977), though it is distinctly un-Keynesian. 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 1977.

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's, B_j's, 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. We think this is one important reason Keynesian models have broken down when the equations governing policy variables or exogenous variables have changed significantly. We hope that the new 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.

So far, these new theoretical and econometric developments have not been fully integrated, although clearly they are very close, both conceptually and operationally. We consider 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 (1976), Barro (1977, forthcoming), and Sargent (1976a), with the result that they do seem able to explain some broad features of the business cycle. New and more sophisticated models involving more complicated cross-equation restrictions are in the works (Sargent 1978). Work to date has already shown that equilibrium models can attain 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.

Criticism of Equilibrium Theory

The central idea of the equilibrium explanations of business cycles sketched above is that economic fluctuations arise as agents react to unanticipated changes in variables which impinge on their decisions. Clearly, any explanation of this general type must imply severe limitations on the ability of government policy to offset these initiating changes. First, governments must somehow be able to foresee shocks invisible to private agents but at the same time be unable to reveal this advance information (hence, defusing the shocks). Though it is not hard to design 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 unforeseeable 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, regardless 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, reactive 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 questions about how well equilibrium theories can 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 equilibrium business cycle theorists will have in ten years if we get lucky.

Four general reasons for pessimism have been prominently advanced:

(a) Equilibrium models unrealistically postulate cleared markets.
(b) These models cannot account for "persistence" (serial correlation) of cyclical movements.
(c) Econometrically implemented models are linear (in logarithms).
(d) Learning behavior has not been incorporated in these models.

Cleared Markets

One essential feature of equilibrium models is that all markets clear, or that all observed prices and quantities are viewed 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 equilibria 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 uncleared 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 long-term labor contracts with horizons of two or three years exist. 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 (1977), Edmund Phelps and John Taylor (1977), and Robert Hall (1978) 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 the 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 of 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 U.S. 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: Should we regard certain superficial characteristics of existing wage contracts as given when analyzing the consequences of alternative monetary and fiscal regimes? Classical economic theory says no. To understand the implications of long-term contracts for monetary policy, we need 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.

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 a period's forecast error and any previous period's. This feature has led several critics to conclude that equilibrium models cannot account for more than an insignificant part of the highly serially correlated movements we observe in real output, employment, unemployment, and other series. Tobin (1977, p. 461) has put the argument succinctly:

> 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. "In other words," as Modigliani (1977) has said, "what happened to the United States in the 1930's 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 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, detailed theoretical work has already shown that two concrete propagation mechanisms do precisely that.

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 they receive. That is, 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 output.

A second propagation mechanism is already present in the most classical of economic growth models. Households' optimal accumulation plans for claims on physical capital and other assets convert serially uncorrelated impulses into serially correlated demands for the accumulation of real assets. This happens because agents typically want to divide any unexpected changes in income partly between consuming and accumulating 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 (1975) showed how this propagation mechanism readily accepts errors in forecasting aggregate demand as an impulse source.

A third likely propagation mechanism has been identified by recent work in search theory. (See, for example, McCall 1965, Mortensen 1970, and Lucas and Prescott 1974.) Search theory tries to explain why workers who for some reason are without jobs find it rational not necessarily to take the first job offer that comes along but instead to remain unemployed for awhile until a better offer materializes. Similarly, the theory explains why a firm may find it optimal to wait until a more suitable job applicant appears so that vacancies persist for some time. Mainly for technical reasons, consistent theoretical models that permit this propagation mechanism to accept errors in forecasting aggregate demand as an impulse have not yet been worked out, but the mechanism seems likely eventually to 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 mechanisms and probably the third can make 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 can account for any proportion between zero and one of the steady-state variance of real output or employment. The argument that such models must necessarily attribute most of the variance in real output and employment to variations in aggregate supply is simply wrong logically.

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 way to detect and analyze systematic effects of higher than first-order moments of the shocks and the exogenous variables on the first-order moments of the endogenous variables. Such systematic effects are generally present where the endogenous variables are set by risk-averse agents.

There are no *theoretical* reasons 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 they face. Dynamic stochastic maximum problems with quadratic objectives, which produce linear decision rules, do meet this essential requirement—that 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 semed most convenient for most purposes. No issue of principle is involved in selecting one out of the very restricted class of functions available. Theoretically, we know how to calculate, with 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 are 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.

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. Therefore, agents are typically assumed to have discovered the probability laws of the variables they want to forecast. Modigliani (1977, p. 6) put the argument this way:

> At the logical level, Benjamin Friedman has called attention to the omission from [equilibrium macroeconomic models] of an explicit learning model, 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.

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 they face. Both of these assumptions can be abandoned, albeit at a cost in terms of the simplicity of the model. (For example, see Crawford 1971 and Grossman 1975.) 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 rules 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, we know of almost no successful applied work along these lines. One probable reason for this is 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 initial conditions and would proliferate nuisance parameters in a very unpleasant way. Whether these techniques will pay off in terms of explaining macroeconomic time series is an empirical matter: 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 (that is, invariant with respect to policy 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 simply 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.

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.

Our first and most important point is that existing Keynesian macroeconometric models cannot provide reliable guidance in the formulation of monetary, fiscal, or 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 to identify structural econometric models. The key elements of these models are that agents are rational, reacting to policy changes in a way 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 unable to account 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."[10] This implication would certainly startle neoclassical economists who have successfully applied equilibrium theory 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 will focus attention on the need to think 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 predict the actions agents will choose to take. This approach will also suggest 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. However, 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.

[10] A main source of this belief is probably Sargent and Wallace 1975, which showed 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.

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