correct, enough are in error, poorly presented or inadequately explained to force the reader new to the area to be skeptical generally. Thus, for example, per capita peasant income in 1979 was at least one-third higher than the 83.4 yuan Tung cites (p. 71); her figure is for income from collective labor only and excludes private plot income. The 218-fold increase in steel output cited (p. 55) between 1949 and 1979 omits mention of the depressed level of 1949 output due to wartime disruption; since 1949–52 was a recovery period, most informed accounts take 1952 as a base year. On page 75, the production team is cited as comprising 12–20 households (actual average: about 30 households); on page 141, confusion, it would appear, between average peasant per capita income and average income per member of the agricultural labor force leads to the totally incorrect conclusion that rural incomes doubled between 1979 and 1980; the discussion of changes in the accumulation (investment) rate neglects to explain the differences in national income accounting between China and Western countries (pp. 65–66), and so forth. *Chinese Industrial Society after Mao* simply assembles a great deal of material and discusses it superficially, without critical insight and with frequent error.

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130 ECONOMIC FLUCTUATIONS; FORECASTING; STABILIZATION; AND INFLATION

Studies in business-cycle theory. By ROBERT E. LUCAS, JR. Cambridge, MA and London: MIT Press, 1981. Pp. x, 300. ISBN 0-262-12089-5. *JEL 81-0992*

This volume reprints the famous, oft-cited essays that gave Professor Lucas the leadership in defining the issues and reforming the methods of contemporary macroeconomic research. Chapters 2–8 contain the scrupulously crafted models that are a Lucas trademark. Seven additional, more discursive essays give us Lucas' views on a broader range of substantive and methodological problems in macroeconomics.

It is useful to get both sets of papers between two covers. Lucas defines progress in economics (rather sternly) as the provision of better and better model analogues to real economies. He is himself a master at building models that will give sharp answers to particular questions. He is also careful in delimiting the claims made for them. Even so, the precise significance of analogues is always a tricky business and much of the profession has viewed Lucas' path-breaking models apprehensively as the luminous tips of icebergs bearing down upon us in the dark. To be constantly branded a menace to intellectual navigation (and often for no good reason) is not an enviable lot, but perhaps it is one that any successor to Friedman and Keynes as a shaker and mover of macroeconomics must expect. This volume gives one a better idea of the subsurface ramifications of Lucas' contributions, although probably only his students or close collaborators will be confident that they can chart them in detail. This reviewer still has problems with Lucas' brand of monetarism and with his equilibrium method.

I.

The early papers in the collection are all focused on wage-employment behavior. The central issue is exploitable tradeoff theory vs. natural rate theory. In reconciling the latter with the short-run Philips curve, Lucas assumes throughout that aggregative disturbances are purely nominal so that the key to employment movements is to be sought in the failure of nominal wages to move proportionally. Ad hoc arguments for wage stickiness are spurned. Thus, the points of departure are those of Milton Friedman's 1968 presidential address. The extent to which Lucas has been able to push beyond Friedman's informally argued position to arrive, in particular, at the famous critique of "Econometric Policy Evaluation" demonstrates forcefully the merits of his methodological precepts.

Lucas departs from Friedman's monetarism in judging "the volatility of business investment over the cycle . . . at least as severe a paradox as the cyclical behavior of employment" (p. 15). But he still postulates monetary shocks as triggering the accelerator and remains in this sense monetarist. The regularities in the covariation among aggregative time-series documented by Wesley Mitchell, Lucas argues, compel one to adopt a "single-shock" theory of business cycles; this shock could only be monetary (pp. 16, 217–78).

I do not find this monocausal monetarism persuasive. For the last 15 years or so, the recognition and anticipation of exogenous nominal shocks must admittedly have been our representative transactor's most frequently occurring aggregative problem. But for most of the cycles surveyed by Mitchell, the monetary regime was not one of unanchored fiat money manipulated at capricious will by the authorities. Instead, most have occurred within one or another monetary regime designed (imperfectly) to insure against purely nominal shocks while allowing some endogenous "elasticity" of the currency. Real shocks propagated within such a setting generate a cycle-hypothesis that might well deserve exploration along Lucasian rational expectations equilibrium lines.

How would such an alternative hypothesis go? Start with a rise in the future real income perceived to be derived from present factor employment in some sizeable sector of the economy. Assume (as does Lucas) a significant supply response to the future real return to present labor. This will allow a temporary equilibrium employment expansion in one sector without equal contraction elsewhere; thus the natural rate of unemployment is not a constant but depends on the marginal efficiency of capital. Suppose further that the additional saving matching the increased investment is partly intermediated by the banks. Investment, real interest rates, and employment all rise and the expansion of the banking system (and of nonbank trade credit) allows this to happen without downward pressure on prices. (Note that a Friedman rule imposed on a broad monetary aggregate, which Lucas favors, might obstruct intermediation and thus the equilibrium system-response in this case).

The empirical versimilitude of this cyclical expansion story is obviously not a settled matter. But it is of theoretical interest as a counterexample to the notion that a "failure" of wages and prices to adjust as they should must be invoked to explain why real and nominal income move together. It is one of the ironies of recent debates that the stickiness of money wages has become a sine qua non of employment theory particularly to those who would dispute that all aggregative shocks are of a purely nominal nature, while the case of market-clearing employment fluctuations in response to (misperceived) changes in the real rate of return has been worked out by Lucas, who accepts the nominal shock hypothesis. His treatment of the labor market will actually fit more easily into an equilibrium *real* business cycle model—the incompetence of the auctioneer in embedding expected inflation into nominal interest rates is then not a problem (cf., pp. 205–06). Such a model would be of interest also as a bridge to the Wicksell-Keynes class of theories which looms so large in the business cycle literature (Leijonhufvud, 1981). In these theories, the real rate of interest fails to find its "natural" level so that household saving and business investment are not efficiently coordinated; the result is fluctuations in income and employment of larger amplitude than in the equilibrium benchmark case. Parts of such a Wicksell-Keynes story could be told in language that Lucas also uses: the "islands" parable (Phelps, 1970), to my mind, carries more conviction applied to the investment-expectations of individual entrepreneurs than in its original context. But it is not at all clear that these hypotheses can be modeled in strict adherence to Lucas' equilibrium method.

II.

Lucas concedes that much progress on short-term forecasting was made with pre-rational expectations macromodels. The important lesson that he and Sargent have taught us most effectively is that success with such short-term extrapolation cannot give warrant for policy-conditional predictions. His own interest seems to be entirely in conditional forecasting models. These models, he argues, will have to be equilibrium models and the appropriate definition of equilibrium must incorporate rational expectations.

The analysis of the effects of a policy should be based on the comparison of well-defined equilibria, not of "unintelligible disequilibria" (p. 225). The boundary between the two is marked, very simply, by the postulate that people will not let perceived gains from trade go unexploited. This, by itself, would seem to leave various gradual adaptations and equilibrating interactions interpretable as learning processes in some borderline class of doubtful intelligibility. But, insofar as the business cycle is a repetitive phenomenon, Lucas argues, the maximizing postulate should imply that agents have learned what there is to learn about it (e.g., p. 244). Although the door to incorporation of Bayesian learning was left ajar by Lucas and Sargent (1978), Lucas seems to have decided on further reflection that it had better be slammed shut. Gradual adjustment processes add a profusion of "free parameters" resulting in econometric models with far too easy a fit (pp. 278–79, 287–91).

Lucas is not concerned to deny that disequilibria occur or that learning behavior may be significant. Rather, they are unsystematic components of cycles. To the extent that the cycle is not a repetitive phenomenon, but poses novel problems for agents to cope with, it will be econometrically unpredictable. This applies as well to discretionary policy-actions obeying no well-understood rule—they cannot be objects of *"scientific* quantitative policy evaluations" (p. 125). (Lucas objects to such policies *not* because they would be "ineffective" but because their effects are incalculable.) Since such policy evaluation is his main objective, the equilibrium approach is the only appropriate one. Disequilibria, if any, will have their empirical reflection only in the error terms of this approach.

This is a strong argument. I will not quarrel with it here. It leaves, however, the question of whether there are other legitimate "scientific" objectives to be pursued in macroeconomics besides the two types of quantitative predictions that Lucas discusses. It seems to me entirely obvious that there are. Attempts at systematic explanation can be useful even when they do not lead to quantitative predictions—or, indeed, to predictions at all. (The theory of evolution is the standard example.) If the lesson is that we have no reliable way of using past data to predict quantitatively how the economy will react to unprecedented change, we would still like to understand qualitatively how the system reacts to unforeseen and ill-understood developments, such as the Great Depression. Such explanation will have to make use of the concepts of excess demand and supply that have no place in Lucas' approach. One hesitates to suggest it but the despised notion of economic policy as an "art" (rather than a quantitative science) might even have a place in such circumstances.

A rough metaphor may help. Consider three kinds of research that might be conducted on a different type of self-regulating system, the human body. (1) One set of questions concerns the effects of various dietary or medicinal regimes on altogether healthy people. (2) Another set concerns the behavior of bodies with perfectly efficient immune systems as they are infected from time to time with "recognized" bacteria. (3) The third category deals with problems of reduced immunological capacity or infections either of a "massive" nature or by novel strains of bacteria. The rational expectations group is sometimes caricatured as dealing only with questions of the first type (and finding, mostly, that perfect health is hard to improve upon). It would be more accurate to say of Lucas that he deals with the business cycle as posing questions of the second type, regards so doing as the only "scientific" game in town, and sees the pursuit of the third as productive of nothing but quackery in medical practice.

The boundaries between these categories depend on one's concept of "equilibrium." If the concept used is the simplest one of "perfect health," (2) and (3) merge because all infections end up in the unpredictable "disequilibrium" category. If, on the other hand, one succeeds in extending the equilibrium concept to cover the dynamics of efficient recovery from infections, the line between equilibrium and disequilibrium descriptions will fall between (2) and (3) instead. (By so doing, one does not deny the unfortunate fact that people get sick.) One can hope, moreover, by further useful generalizations of the equilibrium concept, to shrink the residual disequilibrium category further.

How much of the business cycle process will the equilibrium approach capture? (The more, the better for us, obviously.) The stochastic equilibrium concept developed by Lucas and Prescott (following Muth's lead) will encompass a much broader range of phenomena than older concepts. The papers in this volume, however, pass awfully quickly from the postulate of maximizing behavior, via a quick mention of competition as the regulating principle of interaction (pp. 289–91), to the assertion of price-taking Walrasian equilibria. Uniqueness, stability and the potentiality of generalizing to behavior other than pure price-taking are taken on faith. Obviously, one should not ask that Lucas & Co. postpone the econometric application of their cross-equation restrictions until such time as the general equilibrium theorists give them the "All clear!" But how far the approach will carry us is hard to conjecture.

In the latest of the papers included, Lucas links his equilibrium approach to the contingency market general equilibrium model of Arrow and Debreu. Thus the Lucas economy is to be viewed "as if" it were a stochastic "clockwork" system where all allocation decisions were made and reconciled at the beginning of time. Agents have some trouble with "nature" but none with each other. Contingent on the state of nature, activities are always perfectly coordinated. It is not clear to me how the "islands" parable or other versions of the Lucas theme (where the cycle arises from agents being "misled" by price changes) can be retold within the Arrow-Debreu complete markets framework. What does seem clear is that the pre-reconciliation of plans in the complete market model will rule out of consideration the intertemporal coordination failures central to the Wicksell-Keynes class of cycle theories. This must not be done on methodological grounds.

One should grant Lucas his claim to having broadened the empirical applicability of the equilibrium approach. His critical reflections not only on older macroeconometrics but on recent fix-price models, implicit contracts models and (I'm afraid) verbal non-models are often bulls-eyes. He may be right that we have no operational way to distinguish voluntary from involuntary unemployment or to measure excess demands and supplies. But whether we have a good analytical and empirical handle on it or not, the coordination problem is a real one. I find nothing in this book to persuade one that it can be put aside in trying to explain the business cycle.

III.

The state of business cycle research gives Lucas "a sense of having severely limited theoretical options" (p. 17), a sense which he does not perceive to be widely shared. And, indeed, it strikes one at first as paradoxical. What might be true is limited only by what is taken as known to be true. Lucas, surely, would be the first to insist that we do not have much in the way of such firmly established knowledge. He does, of course, treat the monetarist nominal shock hypothesis as firmly established and that does limit his options. But the constraints Lucas has in mind are not, I think, of this substantial sort. They stem rather from his objective and his methodology. He aims for quantitative policy-conditional predictions and sees the rational expectations equilibrium method as the only possible firm basis for such predictions. Progress in this direction beyond the point reached will not be cheaply bought. Part of the excitement one gets from studying Lucas' work is simply that of watching someone perform technically difficult feats supremely well. (An insidious attraction for the best young talent, one wonders?)

This book will repay the reader in three different currencies. First, one can learn some macroeconomics from it. Second, one is challenged to learn a lot about how to do macroeconomics. Third, one may read it for what it shows of the intellectual qualities that (sometimes) will enable an individual to exert sustained and significant influence on an entire field. The deepest enjoyment of the book is found at this level: Without question, Robert Lucas is a superb economist.

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210 ECONOMETRIC, STATISTICAL, AND MATHEMATICAL METHODS AND MODELS

Structural analysis of discrete data with econometric applications. Edited by CHARLES F. MANSKI AND DANIEL MCFADDEN. Cam-