

Some Central Concepts in Modern Economics, Historically Contemplated

A.M. Endres

Good histories of aspects of modern economics are difficult to write. This is the case because recent developments in economics have not always proven their durability in the sense that they have had the robustness to survive initial debate over both their logical validity or empirical content. In short, historians of recent economic thought do not often have the luxury of judging whether economic ideas have stood the test of time.

In the work under review, *Expectations, Equilibrium and Dynamics*, the authors - Omar Hamouda and Robin Rowley - attempt to give an account of the history of economic ideas and practices over the last sixty years on some major features of economic theory.¹ Their history covers treatment of expectations, equilibrium and dynamics in macroeconomic theory, microeconomic theory and econometrics. Specialised historians of recent economic thought might be disappointed with the outcome, for the authors provide a *catalogue* of recent developments which is insufficient for their book to count as an integrated history of ideas, commentary and reflections on ideas and practices. To be sure, they unashamedly admit to making "a partisan choice of topics" (p. 6) as well as "omissions and inevitable distortions" (p. 231) in order to write a concise 'history'; however, the grounds upon which such omissions and distortions are made is opaque (e.g., p. 2). May for instance, distortions of the historical record be accepted and justified simply because they were merely a matter of the authors' preferences?

Historians of economic thought will doubtless be impressed by the spirit in which the book is written. The "present state of economics is somewhat disappointing. It is compartmentalized, susceptible to fads, forgetful of its past and, worst of all, often dull" (p. 1). Recent developments in the treatment of expectations in macroeconomics incline the authors to explore

how rationality, equilibrium, expectations and other significant concepts have been refined by economists. Since such concepts are sensitive to the shift from a static to a dynamic perspective, it seems appropriate to place this exploration

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within a wide framework that also stresses both the past attempts by economists to deal with time probability, and their current attempts to appraise the realism or applicability of economic theory (p. 2).

Now this is an ambitious agenda. More than the title suggests we are now going to be offered a history of concepts such as expectations, equilibrium and dynamics, and also a study of rationality in economics; economists' use of probability notions and economists' attempts to appraise the realism of all these concepts. Also promising is the reference made in the "Introduction" to "the giants of economics" who "set the scene" for this book: "Keynes, Knight, Hayek, von Mises, Fisher, Hawtrey, Robertson, Myrdal, Lindahl and Shackle" (p. 2). The promise, at least to the reviewer, is that relatively recent developments on the author's chosen theme - expectations, equilibrium, dynamics and rationality - would be linked closely with the work of these 'giants' - some of whom not only set the scene at the beginning of the period surveyed by the authors; the Austrians, Shackle and Myrdal continued to make important contributions to the discipline post-1950.

Chapter 2 on "Formula and Adjustment" begins the story in the 1920s and 1930s, although there are some brief sketches of earlier developments where the theme dictates. There is extensive discussion of literature on micro dynamics - cobweb models of price adjustment - and macrodynamics, that is, endogenous theories of business cycles and multiplier-accelerator interaction which focussed on quantity adjustment. The authors label the literature part of the "new" economic dynamics". These developments are sweepingly contrasted with the "static approach" of marginalist equilibrium theorising prior to the 1920s (p. 7). Before the 1950s the 'new' dynamics rarely mentioned probability, the subjective notion of expectations, or the probabilistic aspects of expectations (p. 10). One central theme in the 'new' dynamics prior to 1950 is the issue of stability: "namely, the question as to whether market economies, if left to themselves, generate stabilizing forces which prevent them from further collapse" (p. 8). This is later formalised as the problem of "dynamic stability of competitive equilibrium" (p. 31). Originally, Cournot, Walras and Marshall sought conditions for stability in different theoretical contexts; many related issues remained unresolved until Hicks, Samuelson and others were stimulated to search for stability conditions in the 1940s, including those of a "dynamic" nature (pp. 31-37). Later, more comprehensive formulations of stability conditions were provided by Hahn, Negishi, Arrow and others; these are explained after the reader is given a brief, elementary guide to other literature in the field of general economic equilibrium - including *tâtonnement*, the determinateness and existence of equilibrium and convergence.

The simple, misleading contrast between the Walrasian and Marshallian adjustment processes based on price and quantity adjustment respectively (p. 39), ignores the important contribution of Davies (1963, p. 539) which found that *both* "Walras and Marshall created price adjuster models. Both also built output adjuster models". Historiographical inaccuracies do not end here. That Walras' "idealization of an exchange economy with multiple markets ignores production [and] the characteristics of intermediaries in actual markets" (p. 38) is correct as far as it goes; but knowledge of the extensive work of Donald Walker on Walras in the last two decades assures us that Walras did not ignore production (see Walker 1987) and that Walras was keenly interested in *real* economic adjustment processes; he wanted to formulate

a realistic theory of time-consuming economic tâtonnement in a freely competitive economy (cf. pp. 38-39; see Walker 1972).

Apart from some glosses on Walras' and Marshall's work, the authors accomplish the task of cataloguing literature on stability analysis from the 1930s to 1960s. More critical appreciation of this literature or some reflections and impressions on its development would have been welcome: critical evaluation is confined to condemning

the seductive attraction of mathematical theories that may stimulate the use of mathematical symbols without prior specification of their meaning or of their identification with actual economic magnitudes (p. 48).

This is definitely not applicable to Walras or Marshall who were concerned with institutional features of real economies, although it may have been applicable to those who worked in the broad field of general equilibrium theory in the period between 1940 and the late 1970s.² Further, it is one thing to be critical of the use of mathematical symbols as ends in themselves and another, more difficult question to appreciate their function both in the development of economic discourse and in the discipline as a whole. The craft of the serious historian of economic thought is usually devoted to the latter task (see Mirowski, 1991). Two additional points stand out in the author's treatment of literature in Chapter 2, one a matter of semantics and another methodological. First, semantical confusions in the literature are irritating to anyone searching for a clear path through some quite sophisticated and often highly mathematical work. There are several different notions of equilibrium and of equilibrium theory alluded to in this book and the term "dynamics" is used too loosely by the authors, (perhaps they are replicating, uncritically, a trend in the literature?). In a book which has made these concepts a central guiding theme, one might have expected a lucid classification to assist the reader. It is significant, I submit, that for the period under review, Machlup was moved to write well-known papers on semantic confusions with the concept of equilibrium (and disequilibrium) (Machlup 1958) and his paper "Statics and Dynamics, Kaleidoscopic Words" (Machlup 1959) deserved mention in the book under review. In reflecting on the literature up to 1959, Machlup's impressions apply equally to my feeling on completing several readings of Chapter 2 in the present book:

The trouble, as I see it, is not that the division of economic analysis into Statics and Dynamics makes *no* sense, but that it makes *too many* senses. Perhaps this is even worse; non-sense can be shrugged off with a laugh, but multiple sense may be a real nuisance (ibid, p. 41, Machlup's emphasis).

Lately, Dixon (1990) has offered a clearer exposition of "equilibrium" and its manifold meanings in economics; he provides a more reliable source for historians of economics. He demonstrates that "there is no general *concept* of equilibrium" (p. 356) in economics; that although there is a loosely defined equilibrium method employed in economics, there are many different types of equilibrium which embody different views of the world and that there are many ways in which the equilibrium method plays a role in the process of explanation in the discipline.

The second point which this reviewer finds unsatisfactory in the development of ideas on stability conditions for equilibrium turns on methodological problems. The methodology of economics was not uppermost in the minds of theorists working

on general equilibrium in the period under review (Samuelson perhaps excepted). Having read Boland (1986) - a text on the methodology of microeconomics and on the recent history of microeconomic methodology - the limitations of *Expectations, Equilibrium and Dynamics* - particularly of Chapter 2 are brought into sharper focus. As Boland (1986, p. xiv) argues:

anyone who has used the idea of equilibrium in other disciplines will be puzzled by the concept of an 'unstable equilibrium' since it is self-contradictory. To avoid the contradictions, we must appreciate that any equilibrium based explanation of the economy must imply a 'stable equilibrium.'

Boland then gives us a *tour de force* of such issues as the foundations of comparative statics; the state of equilibrium as an optimum; optimisation versus equilibrium and the foundations and limitations of equilibrium methodology (pp. 15-127), all of which have substantial historical components.

Compared with Chapter 2, chapter 3 of this book makes a useful contribution in explaining both stochastic models and how expectations came to play a major role in mainstream economics from the 1940s to the 1960s. The emergence of expected utility as an organising principle is documented as is the treatment of optimality in the presence of uncertainty. The latter it turns out, is treated exclusively on the level of probabilistic risk in mainstream work; the authors follow this stream of thought although they are aware that probabilistic risk and uncertainty were not synonymous for all the "giants" who "set the scene" for their book. Keynes (of the *QJE* 1937), Shackle and the post-1950 contributions of leading Austrians (e.g. Lachmann) who drew inspiration from Hayek and von Mises, are mostly ignored. Shackle's framework is described as "awkward" and is left thereafter to languish on the periphery throughout this book (thus paralleling treatment of his work by the mainstream), (pp. 52, 61-62, 123). All this is unfortunate since the decision to omit substantial discussion of challenges to probabilistic schemes makes it harder to see any intellectual linkages in the immediate post-1950 decades between Keynes' and Hayek's treatment of expectations and Lachmann's and Shackle's work in the 1950s. Furthermore, the linkages between this early work and modern Post-Keynesian (Davidson 1991) and Austro-American treatments (O'Driscoll and Rizzo 1985) are greatly obscured. Post-Keynesian and Austro-American perspectives are mostly ignored in this book. Nevertheless, due weight is given to von Neumann's and Morgenstern's work which made great strides in formalising decisions in a game theoretic framework and which showed how subjective expected utility theory and subjective probability theory could be developed out of this framework. Due obeisance is also paid to A.G. Hart who attempted to analyse the economic significance of anticipations data and who clarified the adjustments of individual's anticipations through time.

The work of Mills and Muth broke new ground in the 1950s and early 1960s by introducing the concept of rational expectations and by combining it with optimality in a stochastic framework involving random shocks, equilibrium and optimising. By way of contrast, Chapter 3 also offers some helpful comparisons with other approaches which stressed procedural rationality rather than substantive rationality as distinguished by Herbert Simon. Simon made some significant contributions to the literature in the 1950s and 1960s; he considered the effectiveness of procedural decisions at the micro-level given limits on agents' computational capacities. Atten-

tion in this tradition was focussed on decisions at the level of individual firms, including inventory adjustment problems and employment scheduling. Operations research, dynamic programming and linear decision rules using mathematical formulations were also pursued with vigour in this tradition. Optimising without limits in computational capacity - what Simon called substantive rationality - was favoured by Mills and Muth:

The most significant feature of the rational expectations approach to substantive rationality stems from the particular way in which Mills and Muth chose to introduce a linear stochastic framework, their avoidance of important differences in probabilistic notions, and their reliance on a simple all-encompassing hypothesis of unbiased predictions conditional on the features of economists' theoretical models (p. 86).

Muth's work, in particular, extended the idea of optimality of expectations formed on the basis of univariate models to expectations formed on the basis of structural models. Moreover, agents are conceived as forming their expectations on the basis of the 'true' model of the economy. Muth's work did not have an immediate impact on economic thought or practice; it was long neglected. This book does not give the impression that the rational expectations hypothesis (REH) was integrated into the work of economists, especially into macroeconomics in the early 1960s (p. 117). However, the retarded acceptance of Muth's idea that agents act in a substantively rational manner when forming their expectations, including its consequences for, and assimilation into macroeconomic theory, is an important historiographical question which requires resolution.

In good Chicagoan methodological spirit, Muth was interested in the predictive power of the REH and not the realism of his premises. Methodologists have demonstrated that there was not widespread acceptance in practice of Friedman's methodology of positive economics in the late 1950s and early 1960s. Hamouda and Rowley do not consider the methodological foundations of Muth's work. There is an isolated discussion of the importance of developments in philosophy of science for economics in Chapter 4 (pp. 103, 137-43); it is not connected at all with literature of expectations. And as Pesaran (1987, p. 22) maintained in his more comprehensive historical study on the REH:

the application of Friedman's methodology to models of expectation formation involves additional difficulties that chiefly emanate from the nature of expectations themselves. This is because expectations are either unobservable or can only be observed with errors, thus making direct tests of the predictive performance of models of expectation formation either impossible or at best subject to a high degree of uncertainty.

Here perhaps is one reason why Muth's work did not take a firm hold in the work of macroeconomists in the 1960s. Hamouda and Rowley refer obliquely to testing problems with the REH (p. 103); they imply that probabilistic notions of 'tests' became more popular in economics during the 1960s as exemplified by Bayesian approaches to decisionmaking by agents and in the modelling of risk, search and stochastic utility. They also document problems with the Cowles Commission orthodoxy in econometrics which turned away from 'testing' toward estimation. Morgan (1990) has now given us a more systematic evaluation of the history of

econometrics in the 1950s and 1960s which is consistent with the conclusions of the work under review.

The authors refer to a "loss of optimism and the emergence of discontent" in mainstream economics in "the 1960s" (p. 101). Literature on large scale econometric models, growth theory and multiplier analysis is surveyed to illustrate the evolution of economics in the 1960s. That the 1960s was a period of "drift" is not very well-established. Lately Mankiw (1990, p. 1647) maintained that the "consensus in macroeconomics.... prevailed until the early 1970s" and that in "the late 1960s ... the consensus view was still in its heyday." Now the precise dating of any "radical reawakening" (p. 161) in economic theory, or of any fundamental "discontinuity" or "severe dislocation" (p. 121) in economic thought in the 1960s and 1970s is of first importance to historians of economic thought. This book will not satisfy historians of thought that such a dislocation occurred in the 1960s or "by the end of the 1960s" (p. 161). In following the stream of ideas in economic theory and the evolution of econometric practice in the 1950s and 1960s the reader will receive the undeniable impression that these ideas and practices 'floated in the minds' (to paraphrase Schumpeter) of the practitioners as if they were removed from contemporary circumstances, events and policies. There are fleeting comments on the failure of economists to address pressing practical problems faced by economic policymakers (pp. 135, 185-6) but they are insufficient at least for those who incline to the view that developments in economic thought are ultimately conditioned by contemporary policy problems, by the broad sweep of economic history and political events. For instance, there is a valuable discussion of mostly European contributions to disequilibrium macroeconomics (Malinvar, Dreze, Benassy) in chapter 5; these are contrasted with the work of modern proponents of REH (Lucas, Sargent, *et al*) and equilibrium business cycle theory. Would that the authors had seriously considered or at least speculated on whether aspects of European economic history and prevailing European institutional conditions from the late 1960s through to the mid-1980s contributed to the development of disequilibrium theory in that part of the world. At a very high level of generality I found allusions to other reasons for holding a theoretical stance. For instance, the "Keynesian revolution, monetarist revolution and rational expectations revolution all involved ideological overtones, which prevented clear evaluation of their scientific features... Perhaps this indicated the basic irrationality of economics" (p. 140). Of course this presumes that the ideological content can be completely sundered from 'scientific' elements; it is a presumption which accords with the presuppositions of most of the mainstream literature surveyed in this book.

The rational expectations "reawakening" figures prominently in the "radical" changes which took place in economic theory discussed in the final chapter. In this context, the work of Lucas as part of the new classical approach is given considerable attention. However, the historical antecedents of Lucas' definition of equilibrium which "was quite distinct from any notion of a system being at rest, and it need not mean either competitive price-taking or social optimality" (p. 184) remain opaque. Is this equilibrium a characteristic *classical* notion? If so what is its pedigree and status vis-à-vis neo-Ricardian versions of classicism, including the neo-Ricardian treatment of expectations? In a recent book on a similar subject to the one under review, Torr (1988, p. 130), in a very different analysis, concludes rather provoca-

tively:

Modern classical economists, whether avowing allegiance to Lucas or Sraffa, are confronted with the problem of indeterminacy raised by Keynes. Modern Ricardians avoid the problem of indeterminacy introduced by expectations by avoiding expectations. Modern classicists employing rational³ expectations avoid such indeterminacy by making expectations determinate.

Lucas was firmly committed to reconciling aggregate dynamics with the optimising behaviour of individual agents. Whether this amounted to the same thing as an adequate "explication of underlying microeconomic behaviour" (p. 140) as implied in this book is questionable if one is an adherent of an institutional and conventional view of expectations formation as advanced by Keynes (1936, chapter 12) or as maintained in the work of Herbert Simon. The significance of recent Austro-American views on this subject are also missed; as well, Machlup's article on the "Rationality or 'Rational Expectations' " (1983) which, among many other contributions in the early 1980s, emphasised the heroic information assumptions of the REH, is not mentioned. To amplify an earlier observation, references to Post-Keynesian critiques of the REH developed in the 1970s and early 1980s cannot be found in this book (see e.g. Lawson, 1981). Granted, Shacklean Kaleidics is mentioned cryptically and *en passant* (p. 227). Behavioural approaches are well-sketched near the end of the book (pp. 219-28) but what place these perspectives have in challenging the oft-repeated "radical reawakening" is not made clear. They are all described as approaches to equilibrium, expectations and dynamics "remote from the rational expectations of Muth and Lucas and ... most optimization models" (p. 220), although the assessment of these alternatives is inconclusive. The exposition amounts mostly to an annotated bibliography of key figures and contributions in various fields.

The book's conclusion is disappointing. The "last word" as it is termed (pp. 230-31) makes up about 300 words but does not function as an assessment or an evaluation of the evolution of ideas and practices. Readers are required to judge for themselves whether "progress" was made and whether economists managed to "accumulate relevant knowledge" on equilibrium, expectations and dynamics from the 1930s to the 1980s. As I have indicated frequently during this review, this book is best read as a preliminary to, if not in conjunction with, more specialised historical studies of the development of economic ideas. In particular, histories of recent economic thought and practice in econometrics, rational expectations, new classical macroeconomics and methodology should be consulted because they are likely to be more systematic.⁴

* Department of Economics, University of Auckland, Auckland-1, New Zealand

Notes

1. O. Hamouda and R. Rowley, *Expectations, Equilibrium and Dynamics: A History of Recent Ideas and Practices*, Hemel Hempstead, Harvester Wheatsheaf, 1988. ISBN 0-7450-0685-X, pp. xi+268, Aus \$24.95
2. The reader will find Weintraub (1985), not mentioned in this book, a more reliable source on this subject.
3. Torr quotes Keynes on indeterminacy: "... you ought not to feel inhibited by a difficulty in making the solution precise. It may be that a part of the error in the *classical* analysis is due to that attempt. As soon as one is dealing with the influence of expectations and of transitory experience, one is, in the nature of things, outside the realm of the formally exact" (p. 130, emphasis in original).
4. Other than specialised histories already cited in this review, see also Hargreaves Heap (1989) on the development of rationality concepts in twentieth century economics and Hoover (1988) on new classical macroeconomics.

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