Lakatos Again:

Daniel M. Hausman
University of Wisconsin, Madison

*Appraising Economic Theories* is a collection of excellent papers deriving from a conference held in Capri on October 15-18, 1989. Though the volume may be too expensive to purchase, everyone with a serious interest in economic methodology will want to read the many fine papers it contains. Most of the papers link their discussion of economic methodology closely to specific work in economics, which contributes, I think, to the high quality of the essays. Indeed Neil de Marchi and Mark Blaug are especially to be complimented at having persuaded leading figures in particular subspecialties to try their hand at writing methodology. De Marchi and Blaug also selected a remarkably able group of commentators, whose remarks on the essays are provocative and astute. One further noteworthy and commendable feature of the collection is the comparatively large amount of attention devoted to econometrics both insofar as it is relevant to the methodological assessment of economic theories and insofar as it raises methodological problems of its own.

The areas of economics that are discussed are extremely diverse: Jinbang Kim ties his discussion of the difficulties of testing economic theories to a detailed case study concerning the testing of job search theory. Christopher L. Gilbert contrasts econometric work on theories of consumption, which have, he argues, tested those theories, with econometric demand analysis, which does not test demand theories. He argues that the difference lies in the “location” of the different theories within the overall theoretical enterprise. Marina Bianchi and Hervé Moulin survey problems and results in game theory and argue that much of game theory is best regarded as developing concepts of rationality. Vernon L. Smith, Kevin A. McCabe and Stephen J. Rassenfi make the case that experimental economics makes it possible to have empirically driven progress in economics. Mary Morgan offers a fascinating overview of the history of the controversies between those who favored process analysis and those who favored simultaneous equation methods in econometrics. E. Roy Weintraub links suggestive remarks concerning the character of intellectual history to a discussion of stability theory (and thereby offers a peek at the exciting treatment of stability theory contained in his recent book (1991)). Rod Cross ties his discussion of hysteresis to details concerning alternative theories of unemployment. Both Rodney Maddock and Kevin Hoover focus on new classical macroeconomics in their sharply differing discussions of the relevance of Lakatos’ work to the understanding of theoretical progress and change in economics. Ian Steedman’s discussion of the role of criticism in economics is linked to Sraffian economics. Don Lavoie is concerned with the character and meaning of subjectivism in Austrian economics.

The Capri conference was not just an occasion for a group of scholars interested in economic methodology to come together for a general discussion. To the contrary, de Marchi and Blaug conceived of this conference as a sequel to one held in September, 1974 in Naflplion, which was devoted to exploring applications to economics of Imre Lakatos’ philosophy of science. That conference resulted in an influential volume, *Method and Appraisal in Economics*, edited by Spiros Latsis. Both de Marchi and Blaug participated in the earlier conference and published important essays in Latsis’ volume (Blaug 1976, de Marchi 1976). The point of the Capri conference was to return to the question of the applicability of Lakatos’ work and to find out “whether MSRP, in the hands of methodologists and especially historians of economics, had shown itself to be an appropriate framework for analysing what economics is and is not like” (p.1).

As de Marchi in his “Introduction” and Blaug in his “Afterword” point out, the verdict of the conference appears to have been negative. Few of
the essays found the Lakatos’ views of use. Yet the editors are not willing to accept this verdict. Indeed in his “Afterword” Mark Blaug appears to chastise the contributors for their rejection of Lakatos’ views. Finding that economists often do not comply with Lakatos’ methodology, the authors in the volume are, Blaug believes, too willing to find fault with Lakatos and too reluctant to find fault with economists.

Since there is no way for me to do justice to the many splendid contributions in Appraising Economic Theories, I would like instead to focus on the volume’s underlying concern — that is, on the appraisal of Lakatos’ philosophy of science. Only two essays in the volume attempt any philosophical scrutiny of Lakatos’ views. These are the essays by D. Wade Hands, a philosophically sophisticated economist, and by Jeremy Shearmur, who was formerly Karl Popper’s research assistant, and who is more a philosopher than an economist. Both essays are critical of innovations Lakatos made in Popperian philosophy of science, but since Shearmur and Hands are both Popperians, they are in sympathy with Lakatos’ general project. It is regrettable that those who would have made more fundamental criticisms of Lakatos were not present, because Lakatos’ views are so deeply flawed that a finding that economists conformed to them would be grounds for serious alarm about the discipline.

Lakatos offers philosophical theses about the global theory structure of disciplines and about the appraisal of specific theories. Stripped of some of the snappy terminology, Lakatos’ views on theory structure are simply that within every research enterprise there is (1) a common essence or hard core and (2) suggestions about how to develop particular theories involving the hard core. There are two things wrong with this account. First, “hard” cores are not unchanging, and there need be no interesting single set of things in common among all the theories within a particular research enterprise. Kevin Hoover’s essay, “Scientific Research Program or Tribe? A Joint Appraisal of Lakatos and the New Classical Macroeconomics,” shows with great clarity how many different strands make up new classical macroeconomics. Just as a single rope requires no single thread running its entire length, so a unified research enterprise requires no common feature which is shared by every theory or model in the enterprise. In insisting on identifying a hard core, one winds up with nothing at all or with vapid claims that are too weak to capture what is distinctive about the enterprise. The point is vividly illustrated by the emptiness of the “hard core” of the “neo-Walrasian research program” identified by E. Roy Weintraub (1985, p.109). Yet Weintraub’s hard core is still too strong for Roger Backhouse, who in his sensible and surefooted essay in this volume, “The Neo-Walrasian Research Program in Macroeconomics,” discards two of Weintraub’s six paltry constituents.

The second problem is that Lakatos’ account of global theory structure is too thin. There is little to gain by describing theoretical enterprises as possessing hard cores and suggestions for developing specific theories. What is needed is not a better theory of research programs, but a recognition of the limits of such theories. I do not think that one can appreciably improve upon Lakatos’ skeletal account, for the structure of theoretical disciplines depends too much on features of the particular domain of study, the particular theories with which practitioners begin, and sociological factors influencing the investigators. There is no way to write the history of a particular theoretical enterprise by filling out some questionnaire provided by Lakatos or any other philosopher of science. As many essays in this volume show, there’s little practical payoff from asking what propositions all co-workers in some project accept and what suggestions or hints for doing economics they would all support.

Lakatos also offers an absurd account of theory appraisal. It is absurd, because Lakatos rejects the possibility of making a non-comparative appraisals. All one can say is whether one theory is better or worse than another. But both for practical purposes and to know how to test theories and to interpret test results, people need to know how good theories are, not just whether they are better or worse than other theories (whose goodness or badness is itself unknown). Consider analogously a technical report on some new computer that said nothing about how well it performed, but only whether it performed better or worse than other computers, whose performance was also unknown.

Lakatos’ account of theory appraisal is absurd also because Lakatos emphatically rejects all forms of what he calls “justificationism.” Evidence never gives one any good reason to think that a theory is true or reliable or close to the truth. So when a particular amalgam of theories, simplifications, decisions concerning functional forms, data collection methods, tools for data analysis, and so forth leads to a conflict between theory and data, one cannot rely on previous evidence to judge that some parts of this amalgam are fairly secure and worthy of confidence, while other parts are dubious and likely to be mistaken. Instead, one revises any
part of the amalgam (1970, pp.40-41), and if in doing so one loses none of the corroborated content while gaining some new predictions, one makes "theoretical progress." "Empirical progress" results when some of the new predictions are not refuted. No wonder that the case studies don't find economists behaving this way. (And thank goodness that they don't!) Science and practice would grind immediately to a halt if one could not make use of appraisals of how secure various statements are. And such appraisals are impossible, if one cannot take evidence as supporting statements. 2

People continue to discuss Lakatos' (and Popper's) views on theory appraisal only because they don't take them literally. For example, in his afterword Mark Blaug writes.

"...the only way we can be sure that we have achieved objective knowledge of reality is to commit ourselves to the prediction of novel facts. 1 Of course even then we cannot be sure that we have achieved objective knowledge. The case for placing so much emphasis on the importance of making accurate predictions is basically what Hands calls the 'no miracles argument': a theory that successfully predicts out-of-sample data from sample data is likely to have captured some aspect of objective reality because otherwise its record of predictive success is simply miraculous. Theories may also be simple, elegant, general and fruitful but none of these desirable properties in any way guarantees verisimilitude, that is, nearness to truth about an objective world." (p.502)

This seems to say that Lakatos is right that only novel predictions matter because, unlike other desirable features of theories, novel predictions in some way guarantee verisimilitude or enable us to be sure that we have achieved objective knowledge of reality, even though we cannot really be sure that we have. Blaug apparently believes that successful novel prediction makes it likely that the theory has captured some aspect of objective reality. But Lakatos and Popper emphatically deny not only that the success of novel predictions guarantees verisimilitude, but that it constitutes evidence at all! For one cannot give evidence for theories (Popper 1968, p.419; 1972, pp.47f; Lakatos 1970, pp.10-12). Nothing can make it "likely" that a theory has "captured some aspect of objective reality." If supporting evidence were possible, then, to explain how, one would need an inductive logic and a theory of confirmation, both of which Popper and Lakatos totally reject.

In Blaug's view, what is central to falsificationism is the willingness to let disconfirming evidence lead one to modify or reject one's theory (p.510). So those who reject falsificationism appear to Blaug to be espousing the view that disconfirming evidence never counts at all. One can thus readily appreciate why the message of Popper and Lakatos seems to Blaug so vital and undeniable. But there is no controversy among philosophers of science about the significance of disconfirmation, and Popper and Lakatos do not differ in this regard from inductivists, Bayesians, Quineans, and all their kith and kin. Emphatically to reject Popperian and Lakatosian philosophy of science is not necessarily to reject the importance of evidence. It may be instead to insist that one needs to understand better just how evidence can refute (and support) theories.

The contributors to this volume tell persuasive tales of work by well-trained people facing difficult problems in many branches of economics. The essays describe patient, careful, and distinguished mathematical and empirical studies, though they describe blunders, too. Philosophical commitments play some part in the stories, but so do ideology, personal relations, accidents of knowledge and ability, and a thousand contingencies. One may want to understand these histories for many good reasons. One of these good reasons is that one may want to improve the future practice of economics. If this is one's goal, then, I contend, there's little point to studying the history of economics through Lakatosian spectacles. Lakatos' account of theory structure demands that one identify hard cores that simply are not there, and in any event it has too little structure to enable one to say much that is interesting about what unifies individual theories. Lakatos' account of theory appraisal makes nonsense of human practices, and it persists only because people pretend he is saying something else.

For a confirmation of these negative conclusions concerning Lakatos' philosophy of science (and for a great deal of fascinating history and sound methodological comment), read the essays in *Appraising Economic Theories*.

Notes
1. Lakatos' plea for "a whiff of inductivism" amounts only to the view that we can hope that increasing corroborations is a "sign" of increasing verisimilitude (1974, p.158).
2. Furthermore, demanding that theoretical progress occurs only when there is no loss of corroborated content, would make theoretical progress nearly impossible. For further details on these criticisms, see Hausman 1992, ch. 11.
3. Blaug’s footnote number one is concerned with rhetorical views of science and is not germane to the issues discussed.

References