

School of Economics who for a time reinvigorated a moribund Popperian tradition. His best book was his dissertation, *Proofs and Refutations* (written in 1961, when he was 39 years old, and published in 1976 two years after his premature death), a brilliant inquiry into the rhetoric of mathematical proof. But he is chiefly known to methodologists for his extension of Karl Popper's insight that empirical science holds debates, an extension systematized in Lakatos' notion of the "Methodology of Scientific Research Programs" ("MSRP" throughout, and similarly NWRP, PKRP, PH3', RE-PIH by the bushel; can we have a moratorium on such acronyms?) In a nutshell the Methodology asserts that scientific theories are to be judged by their success in predicting astounding and novel facts, such as that light bends around the sun or that trade will equalize factor prices.

The excellent papers and comments collected here "reflect on the question whether the [Methodology of Scientific Research Programs] has proved useful" in thinking about economics (p. 1). If the Methodology, which claims to be a science of science, is applied to itself, the answer must be that it has not. To summarize the rich argument of the book in a sentence: after twenty years of highly intelligent attempts to apply it to economics the Methodology has been shown to be a degenerate research program.

Neil de Marchi's Introduction, a characteristically lucid performance, makes a brave case that economics fits the Lakatosian description and, further, that the way economists talk about their work fits it, too. (He also discusses the contrary view that economics does *not* fit the description, a balance one is thankful to have.) De Marchi quotes a letter from Vernon Smith (who also contributed to the volume): "on almost every page of Lakatos I find articulate expressions of what I have been doing as an experimentalist for years. I never got this much out of my much earlier reading of Kuhn and Popper" (p. 21). That puts Smith and perhaps his coauthors (Kevin McCabe and Stephen Rasenti) in the minority, although in their paper they admit to being amateurs in philosophy and are diffident in its defense. Aside from the two editors, Roger Backhouse is the one contributor to emerge as a whole-hearted enthusiast for Lakatos. Mark Blaug's editorial Afterword, la-

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Imre Lakatos was a Hungarian-born philosopher of science at Cambridge and the London

menting the "jaundiced reaction to Lakatos of many of the participants" (p. 510) rounds out a rear guard of three and perhaps six. But from the other 26 participants the news is bad.

The opening paper by Jeremy Shearmur for example is a polite demolition of a Lakatosian history of economics that ignores the other sorts of history of economics: it is "a strange kind of history," he notes, that supposes about scientists that "what they were really doing was to be explained in terms of Lakatos's ideas about methodology" (p. 45). The demolition proceeds in an elegant paper by D. Wade Hands, the culmination of a research program which ought to be more widely known, concluding that the Popperian-Lakatosian light is incoherent and that other lights should shine. The foundation of Lakatos's proposal is that a theory in science should be justified by its production of Truth, capital-T, the Truth of God rather than of mere mortals. Hands remarks dryly, "However recurrent such pleas have been in the history of philosophy it is doubtful whether such a justification will soon be upon us" (p. 67). Warren Samuels, in an important book in another tradition (*Economics as Discourse* 1990, p. 3), has noted that big-T Truth has the $x:X$ Problem. "Inasmuch as our only knowledge of X is x , how do we know that x is representative of X ?" How indeed.

Christopher L. Gilbert's essay answers the question, "Do Economists Test Theories?" with a qualified "No," thus again falsifying Lakatosianism as a description of the field. He thoroughly surveys the econometrics of demand and of permanent income. Perhaps he should have pointed out that Edward Leamer anticipated his results (in *Specification Searches* 1978, and later writings). Jinbang Kim, like most of the authors in the book, does more of the empirical work necessary to find out if the Lakatosian model works; he examines the literature of job searching. He shows in detail the reach of the so-called Duhem-Quine thesis, namely, that any experiment has side conditions, and therefore the "test of the hypothesis" is also a joint test of the side conditions. Econometricians of course know this well. Kim argues persuasively that the specification searches that economists engage in are not always content-reducing. On the contrary, as Leamer again has argued, we dance with the data in a way captured poorly

by Lakatos' Methodology of Scientific Research Programs.

So it goes, in richer detail than can be conveyed here. The Methodology has failed to capture the insights from game theory (Marina Bianchi and Harvé Moulin), to account for the suppression of process analysis in econometrics (Mary Morgan), to apply to stabilizing dynamics (E. Roy Weintraub, an especially fine essay), to explain the anxiety of econometricians to nest their hypotheses about equilibrium unemployment (Rod Cross), to predict the sociology of rational expectations (Rodney Maddock), to grasp the anthropology of new classical macroeconomics (Kevin D. Hoover), to allow for critical work such as Sraffa's (Ian Steedman), to encourage a practical subjectivism that reflects our human viewpoint (Don Lavoie, another winner among many), and to fit the history, sociology, and rhetoric of science (Harry M. Collins).

And so Lakatosianism has failed as a research program. That way of putting it, however, shows that its failure does not mean the book of Lakatos is to be closed forever. One can use his helpful rhetoric of "research programs," "novel facts," "degenerate programs," "justification," "falsification" (a Popperian survival), "content-reducing," and the rest without elevating it to a 3" x 5" index card Guide to the Essence of Science.

De Marchi and Blaug treat Lakatos' work as the Five Books of Moses, whose "hints" (pp. 9, 12, 19) are to be elaborated by the faithful. Nonbelievers nonetheless can still learn from the Bible. The core dogma that research programs are the right unit of analysis has in practice the ethical problem, as J. A. Kregel notes (p. 327), of masking an unwillingness to communicate across research programs: you have your research program, I have mine. But it has also the related problem that it would consign the genius of Lakatos to the scrap heap merely because the system in which it was expressed is bankrupt. This is bad economics, a sort of scientific block booking.

In a comment on one of the papers Philip Mirowski concludes that

Weintraub's linguistic turn . . . [is] the breaking of a deadlock in the Lakatosian tradition, the transcendence of the incoherent notions of rationality by means of research into the way lin-

guistic and mathematical practice structures our history. (p. 292)

That observation seems correct, and could lead to research into how Lakatos' passionate and intelligent rhetoric, however misleading as theology, has structured the recent history of economics and its philosophy.

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