Appraising Economic Theories

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A Review Article of: *Appraising Economic Theories. Studies in the Methodology of Research Programs.* Neil de Marchi and Mark Blaug, eds. (Aldershot: Edward Elgar, 1991)
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This bulky volume contains the revised proceedings of a conference held in Capri, Italy, from 15-18 October, 1989, funded by the Latsis Foundation. It was, in a sense, a sequel to the Lakatos Conference held in Nafplion, Greece, in September 1974, with lavish support from the same source. From the earlier occasion two volumes appeared: Spiro Latsis ed. *Method and Appraisal in Economics* (C.U.P., 1976) and Colin Howson ed. *Method and Appraisal in the Physical Sciences* (C.U.P., 1976). The first of these has been extensively cited in the economics literature. The latter, curiously enough, does not even get a mention here.

At the earlier conference about half of the fifty or so participants were non-economists--mainly historians and philosophers of science; and as Neil de Marchi recalls in his introduction: "The atmosphere was one of anticipation, and most papers had something of the air of first ventures into a promising but largely unexplored territory" (p. 18). However, this was more true of the economists than the other participants, among them some of Imre Lakatos' colleagues and students who were generally more familiar with and favorably disposed to his ideas than the economists.[1]

In the intervening decade and a half there has been a veritable explosion of new writings on economic methodology and some substantial, at times even startling, new developments. De Marchi and Mark Blaug uncovered seventy or so articles reflecting Lakatos's influence in economics, and the Capri conference was assembled to assess the current state of his contributions. As it transpired, the results were distinctly mixed, as were the editors' reactions. According to de Marchi:

> not a few participants displayed more awareness of the difficulties already encountered in settling the new land, or impatience with the things that had excited the first settlers, than they did a sense of expectancy about possibilities as yet unrealized (p. 18).

However, Blaug, in his "Afterword," was much less sanguine, for he was

> personally taken aback by what can only be described as a generally dismissive, if not hostile, reaction to Lakatos' MSRP [Methodology of Scientific Research Programs]. Of the 37 participants, I estimate that only 12 were willing to give Lakatos a further run for his money and of the 17 papers delivered at the conference not more than five were unambiguously positive about the value of MSRP (p. 500).

Whether this outcome constituted success or failure presumably depended on the organizers' expectations. Blaug, one of the principal initiators of the Nafplion gathering, was obviously disappointed. De Marchi, on the other hand, who has never been as deeply committed to the Lakatosian (or Popperian) approach, was more qualified, yet still sanguine, seeing

> in the Capri papers hopeful signs of Lakatosian and modified Lakatosian work to be pursued in some hitherto neglected directions, even when the authors themselves are more ready to call it a day so far as MSRP is concerned. The reader will have to judge whether my readings are unduly optimistic.[2]

Before responding to this invitation, or describing the contents of the Capri volume, let me first disclose an interest. I was present at and contributed a paper to the Nafplion proceedings, being then an enthusiastic but not uncritical supporter of Lakatos's methodology. Viewed from an history of economics perspective, MSRP seemed much more specific, and to that extent more useful than Kuhn's approach, enormously stimulating though that had been. It incorporated a temporal dimension--since research programmes could be said to emerge, develop, progress or degenerate, or even die off; and this temporal dimension offered suggestive insights and provided ample opportunities for historical investigation and interpretation. Needless to say, back in 1974 MSRP represented promise rather than performance, for there had been few attempts to apply it to economics. Yet even then I was fully aware that my own conference topic, suggested by Lakatos, was far too large, ill-defined, and sprawling to constitute an ideal case study.[3] Now I note with some satisfaction that during the Capri proceedings there were substantial differences of opinion over MSRP strategy--e.g., whether to focus on "broad" (encompassing) or "narrow" (specific) SRP's; whether to study individual SRP's or groups; and how to define their boundaries and isolate their Lakatosian components. These matters will be considered more fully later.

The table of contents of the Capri volume gives a clear general indication of the subject matter. De Marchi's introduction, "Rethinking Lakatos" precedes two unequal parts: I "The Criterion of Empirical Progress in Economics," and II

"Program Identification and Program Rivalry." Each part has three subsections, as follows:

Ia. "Theoretical Progress and Excess Content"--papers by Jeremy Shearmur and D. Wade Hands; comments by Kurt Klappholz, Bert Hamminga, and Uskali Maki.

Ib. "Testing"--papers by Jinbang Kim (on Job Search Theory) and Christopher Gilbert (on Demand Analysis and Consumption Analysis); comments by de Marchi and Marjorie McElroy.

Ic. "Empirical Hypotheses in Game Theory and Experimental Economics"--papers by Marina Bianchi and Hervé Moulin (on Game Theory), and Vernon Smith, Kevin McCabe, and Stephen Rassenti (on Experimental Economics); comments by H.M. Collins.

IIa. "On Including and Excluding Lines of Enquiry"--papers by Mary Morgan (On Process Analysis in Econometrics), Roy Weintraub (On Stabilizing Dynamics); and Rod Cross (Alternative Accounts of Equilibrium Employment); comments by Roger Backhouse, Blaug, and Jan Kregel.

IIb. "Delineating Programmes"--papers by Rodney Maddock and Kevin Hoover (On Aspects of New Classical Macroeconomics), and a third by Backhouse (On the Neo-Walrasian Programme); comments by Alessandro Vercelli, Nancy Wulwick and Maarten Janssen.

IIc. "Assessing Sraffian and Austrian Economics"--papers by Ian Steedman and Don Lavoie; comments by Stefano Zamagni, Edwin Burmeister and Bruce Caldwell.

Finally, Blaug's "Afterword" is preceded by Collins's short paper on "History and Sociology of Science and History and Methodology of Economics," for which there was no commentator.[4]

To say that the main value of this volume lies in the dozen detailed studies plus comments on particular aspects or branches of recent economics (not always research programs) involves no denigration of the two useful scene-setting papers by Shearmur and Hands. The latter's penetrating dissection of "The Problem of Excess Content: Economics, Novelty, and a Long Popperian Tale" admirably prepares the reader for some of the difficulties encountered later in the book. In recent years, however, economic methodologists have increasingly recognized the need for closer examination of what economists actually do--i.e., how they go about their business--rather than looking to the philosophy of science for guidance; and from this standpoint, the Capri volume represents a substantial advance. There has, indeed, been genuine progress in knowledge and understanding since Nafplion.

Taken as a whole, the Capri essays reveal the difficulties encountered in proceeding from the high plane of Lakatosian philosophy of science to the nitty gritty details of particular research programs, including what de Marchi calls "local strategies" (p. 20). Doubtless some of these difficulties would arise in applying any general philosophy or methodology to the ideas and activities of working scholars (or, as some might prefer, proceeding in the opposite direction from particulars to generals).[5] In the present case, some participants were clearly uncomfortable with any discussion of abstract epistemological issues whatsoever, while others (perhaps most?) were unfamiliar with the full range of Lakatos's writings. This was a matter of no small significance, given his occasional vagueness and inconsistency, and the major shift of his ideas from the earlier *Proofs and Refutations* to his later MSRP. Some authors, e.g., Lavoie and Steedman, either made no serious attempt to apply MSRP or declared that it was unhelpful or irrelevant; and while conventional neoclassical economists may dismiss Austrian and Sraffian economics as fringe or deviant topics, to which MSRP may be inapplicable, the same cannot be said for econometrics (Morgan), game theory (Bianchi and Moulin) or experimental economics (Smith, McCabe and Rassenti), novel though the last of these is. The authors of the essays on these three subjects evidently did not find Lakatos helpful.[6] Hoover, after a scholarly and revealing struggle with MSRP, declared that "Lakatos's own methodology is falsified like all the others"--softening the blow somewhat by citing Lakatos's admission that: "*All* methodologies, *all* rational reconstructions can be historiographically `falsified': science is rational, but its rationality cannot be subsumed under the general laws of any methodology" (p. 385).

Given the range and variety of material in this volume it may be helpful to present some of the principal problems schematically.

1) What is a research program?

A resonably clear answer to this question is essential if MSRP is to serve as a valuable tool of interpretation or appraisal. Difficulties include: the boundary problem--what is inside or outside a given program (pp. 337-8) and what are the relationships between intra-and extra-core variables (p. 445); and how to distinguish between successive variants of a given program and the emergence of a new program (a point admirably revealed in the Maddock and Hoover studies of New Classical Macroeconomics--especially pp. 337-8, 343, 352-3, 368-70, 374-5, with Hoover effectively employing the terms `tribe' and `family' to characterize programs).

Unfortunately Lakatos did not explain how programs emerge, or whether a program can develop in isolation or only in competition with others; and Steedman, discusssing negative contributions, asked: if program (a) is legitimately criticized from the standpoint of program (b) is that a contribution to (a) or (b) or both? (p.443)

As noted earlier, programs can be defined either narrowly or broadly, and each approach has merits and limitations. Janssen, following Zandvoort, distinguished between `guide' and `supply' programs, the former determining the issues to be discussed while the latter provides the language and methods by which they must be tackled. "Roughly speaking, the guide program has a well-defined domain of issues with which it is concerned, but it does not have a method of its own. The reverse holds true for the supply program, which is characterized by a clear-cut method and a rather vague domain of intended applications" (p. 430). Of course, uniformity of interpretation is neither to be expected nor desired. Sometimes, perhaps usually, there is scope for "reasonable interpretation" (p. 132), but none of the Capri authors seemed disposed to supplement Duhem's "reasons of good sense" by incorporating "some hard Popperian standards of appraisal" (as suggested by Lakatos) even "to avoid embarrassment arising from their theories being contradicted by empirical evidence".[7]

2) What is a hard core?

The answer to this question is complicated by the realization that: "The actual hard core of a program does not actually emerge fully armed like Athene from the head of Zeus. It develops slowly, by a long preliminary process of trial and error" (Lakatos, quoted on p. 370). During that process it can undergo articulation, hardening, refurbishment, even crumbling, prompting Hoover to liken it to "Elmer's glue, which is hard enough until it gets wet" (p. 371). Although de Marchi maintains that "putting Lakatos to work seems to *require* clear enunications of hard core and heuristics that can be tested against the historical record, just as Weintraub has taught us to do" (p. 16), he immediately adds the qualifier: "Or does it, in all cases?". Backhouse, who favors a more formal attempt to define hard cores and heuristics (pp. 267), finds it necessary to reconstruct Weintraub's version in examining the Neo-Walrasian Research Program (p. 404 ff). De Marchi warns that if hard core and heuristics "evolve and mutually inform each other," then preoccupation with the former rather than a process of discovery may lead to "misplaced concreteness" (p. 16). This, however, is but one of several pitfalls for interpreters, since Lakatos's admission that "different programs may have a common hard core and be differentiated only by their positive heuristics" (quoted by Hoover, p. 368) provides yet another occasion for disagreement between contentious critics and defenders. More generally, Maddock observes that economic research cannot easily be fitted into the Lakatosian framework because

> one can analyze the same theoretical phenomenon at a variety of different levels and in the context of encompassing research programs. In a large literature this can clearly lead to confusion; the same contributions will be assessed in quite different ways, depending on whether they are defined as being within program A or program B: one person's hard core will be another's protective belt (p. 338).

3) The criterion (criteria?) of progress

For historically-minded methodologists, one of the initial attractions of Lakatos's approach was his distinction between progressive and degenerating research programs; but, unfortunately, this too constituted a difficulty for the Capriciosi. As Hands notes, theoretical progress occurs with the prediction of some novel, hitherto unexpected fact, whereas empirical progress requires that this excess empirical content be also corroborated (p. 64). De Marchi stresses Lakatos's emphasis on the primacy of theory--"the relative autonomy of theoretical science"; yet he also notes Lakatos's claim that: "All propositions are theoretical, even those we call observational or 'factual'" (p. 134). Lakatos recognized progress in heuristic power, and in technical refinements, provided the resulting succession of new models involves an increase of content at each step, though apparently this may not be essential (cf pp. 133; and 135-6, 11-12, 16). However, Backhouse, commenting on Morgan, asserts that: "Novel facts and empirical testing can be brought in only when we consider economic ideas as well as econometric techniques" (p. 269). He doubts that research programs in such techniques constitute research programs in economics, in the Lakatosian sense. (pp. 266-7)

Novel facts, the key element in excess content (which is essential to the Lakatosian idea of research progress), cause further trouble. Lakatos offers three definitions of novel facts (p. 133) whereas Hands, who has undoubtedly "spent a great amount of time with the Lakatosian literature" confesses that he has "no idea what Lakatos 'really meant' by novel facts." His pursuit of this quarry in his "novel fact hunt" yields at least five different definitions, consequently one sympathizes with his distaste for "novel fact fetishism."[8]

4) Theories, empirical models, and the role of testing.

The discussion of these matters, which are central to present-day technical economics, constitutes one of the most important elements in the Capri volume. But the outcome is by no means clear.

A reiterated theme is the "continuing interplay between empirical testing, theoretical modification, further testing, and so on" (Smith *et. al.*, pp. 218-9), while Hoover remarks that: "There is a division of labour in scientific research between theory and empirics with influence running in both directions. And, as any economist would expect, there are gains from trade" (p. 383). Gilbert, who, describes "testing theories as a major professional obligation," emphasizes that "economic theory seldom relates directly to data. Instead economists are required to intermediate theory and data with empirical models but we cannot directly test theories. Whether or not we may interpret these tests of models as indirectly testing theory is a more complicated question" (p. 139; cf. pp. 139-40). Whereas Morgan contends that economists test empirical models, not theories, Gilbert qualifies this assertion, adding that to concede that a theory can be tested only through an empirical model is not to argue that theories can never be tested. His paper aims to demonstrate that "in demand analysis we are only testing the appropriateness of particular empirical models, while in consumption analysis tests of empirical models are genuine tests of the underlying theory".[9] Hence, "whether or not a theory can be tested depends at least in part on the character of the theory" (p. 162), an observation he makes no effort to specify more fully.

Kim flatly contradicts the conventional interpretation of Lakatos by arguing that "few economists regard it as necessary for new empirical models to contain excess empirical content in order for them to replace earlier models. . . . It is therefore a category mistake always to seek for content-increasing changes in a series of empirical models" (p. 128). Moreover, two economists will seldom end up with the same empirical model even though they started with the same theory. (p. 115)

No careful reader of the Capri volume will conclude that the process of developing and testing empirical models is easy. These models may be more or less simplified than theoretical models; and: "The appropriate empirical model may have to combine implications from different theories; to acknowledge the possibility that implications of the theory under investigation may be blurred by failure of the theoretical *ceteribus paribus* conditions; and to admit nuisance parameters to account for theoretically extraneous effects" (Morgan, p. 141; cf. Kim, p. 105).

Clearly there is no consensus on the relationships between theories, empirical models, and testing; and de Marchi may be somewhat overoptimistic in concluding that despite all the conditions and qualifications the essays in Part I are still valuable by "forcing us into a subtler, richer understanding of the role of theory in economics and especially of the interactions between theoretical and empirical models, and between models, measurement and testing in the ongoing process of theory development" (p. 21). Whether this enhanced understanding could have been obtained without Lakatos's assistance, is quite another matter.

5) Economic Methodology

Several of the conference participants explicitly dissociated themselves from any kind of methodological monism, even of the Lakatosian species. Steedman, for instance, cites Zamagni's "warnings that each science may need its proper (different) methodology, that methodology follows science rather than leads it, and that it is indeed undesirable that methodologists be permitted to prescribe what is good procedure in science" (p. 447). Likewise, Gilbert mentions the "danger that in discussing methodology one starts with a view of the way scientific activity is or should be carried out and then looks for examples which illustrate this view" (p. 137). He considers that "methodologies are to a large extent implicitly defined by the questions being asked" (p. 159). Hoover, who cites Wittgenstein and Lakatos in support, maintains that "one must discover the general principles in the particular cases. . . . Scientific practice, therefore, consists of groups of related and partly overlapping models and practices. . . . No model is primary," although: "When many variants on a model exist, any one may, for many purposes at least, serve as an exemplar" (pp. 375, 373). As Wulwick acknowledges, although Lakatos tried to demarcate science from non-science, he recognized that scientific practice was in fact often unscientific (p. 395) and this should be borne in mind by those seeking to employ Lakatos's method of rational reconstruction.

6) Lakatos, history and the sociology of science

The Lakatosian approach to science is essentially historical (p. 25), and Lavoie suggests that the impetus Lakatos gave to intellectual history may indeed be his most valuable contribution (p. 473). Yet once again we encounter serious philosophical methodological and interpretative problems that limit Lakatos's value to historians of economics. De Marchi believes that despite all the difficulties, MSRP still "offers hints that help to make sense of what economists do and have done in the past." He strongly endorses the multiple interpretative possibilities inherent in Weintraub's constructivist approach (p. 27; cf p. 273 ff) although Weintraub himself, like Shearmur, is very critical of Lakatos's monistic view of the history of science as a story to be told exclusively in Lakatosian methodological and epistemological terms (p. 44). It ends up

> recommending as *the* historiography of science, the writing of the kind of historical account that in his view is needed for evaluative purposes. . . . But what is written for the purposes of theory appraisal is a strange kind of history; and it seems to me unlikely that we will gain much by treating the activities of historical agents whose concerns and criteria are different from ours as if what they were really doing was to be explained in terms of Lakatos's ideas about methodology (pp. 44-5).

Shearmur concedes that there is

> a sense in which a personal familiarity with certain kinds of problem solving under theoretical constraints can be of the greatest interest in understanding the ideas of historical figures. . . [Yet] some very bad history has been written by people who assume that what historical figures were *really* concerned with was the discovery of ideas with which we are familiar, or even that ideas familiar to us are to be found implicitly lurking behind their texts (p. 45)

In a hard-hitting paper, Weintraub argues that Lakatos's rational reconstruction commits the storyteller to create a rational program which becomes the main actor in the story; so that the history of science becomes an exemplar of rationality itself. This is especially clear in the case of economics, in which

> producers and consumers of the history of economics have accustomed themeselves to read the past from the present's perspective, . . . [leading to] a concentration on issues of precursors, harbingers, logical slips, factual errors, misuse of evidence, and all manner of past writing which illuminates the present state of economics or helps us to understand what we currently believe. (p. 279)

Of course there is nothing wrong in principle in pointing out precursors, harbingers, logical slips, factual errors, misuse of evidence etc. But Weintraub is entirely justified in pointing to Donald Walker's Presidential Address as a series of simple instructions how to do history of economics 'proper' (p. 280). Whether a constructivist approach means any one interpretation is as 'good' as any other is, however, quite a different matter, which opens a can of worms best left closed for the time being.[10]

To this reviewer, it is of particular interest that the sociology of economics (or knowledge, or science) figures much more prominently in the Capri than in the earlier Nafplion volume. In recent years we have learned that science viewed from close range, as for example, in studies of 'laboratory life', is very different from that seen through the philosopher's inverted telescope. The familiar distinction between the 'internalist' and 'externalist' approaches to the history and philosophy of science may be crude; nevertheless, several of the Capriciosi are critical of Lakatos's hostility to what he once termed "externalist rubbish". Shearmur, for instance, argues that "methodological appraisal and the study of the sociology and the rhetoric of science are not necessarily enemies, but, rather, complement each other," for there is no incompatibility between Popper's and Lakatos's concerns and an interest in "the sociology of institutions or power relationships, or perhaps even to the wider issues of political economy" (pp. 47, 45). Collins, the one *bona fide* sociologist of science featured in this volume, complains that historians of economics fail to utilize their specialist insights into the discipline as practitioners, "preferring to do the sort of history which does not require any special *understanding* of the subject".[11] Fortunately there are clear signs that this kind of misallocation of intellectual resources is nowadays gradually being rectified.

In this volume Hoover's study of the New Classical Macroeconomics and the neoclassical "family" or "tribe" is the most explicitly critical attack on Lakatos's neglect of the social aspects of research. In the case of rational expectations, Hoover argues, the program's ability

> to capture the interest of the generation of economists being trained in the 1970's was crucial to its great popularity in that decade and perhaps to the rapid exhaustion of its potential. The ability to convert was partly explained by the advertising undertaken on its behalf. There is a clear feedback from advertisement to recruitment, and from recruitment to the pace of exploitation of the positive heuristic of any program. It is an element of the sociology of knowledge rather than its logic, but the two are related in a way that lies outside Lakatos (p. 354; cf p. 347 ff).

Shearmur, more generally, emphasizes the inter-subjective element in testing (pp. 48-9; cf p. 382). The pressure for consensus is sociological, and

> if one sees the `empirical basis' of our knowledge as the product of inter-subjective agreement--and thus of argument and negotiation between different parties: as a social construction--then clearly one should be interested in empirical and theoretical investigations of how all of this currently takes place. . . a *critical* sociology of knowledge. . . will lead us to suggest improvements in our institutions from the perspective of our pursuit of scientific knowledge (p. 50).

According to Andrea Salanti, historians of economics have accepted MSRP because of the dominant influence of `internal' history of the kind Lakatos favored (cited on p. 28). However, that attachment may have been permanently undermined by the present volume. Despite de Marchi's editorial efforts to present the Lakatosian approach in a favourable light, he concedes that the work of Weintraub, Maddock, Klamer, Hoover and others raises pertinent questions about the

> social conditions of the production of `knowledge' . . . which, when based on modern pragmatic philosophy's challenge to rationality as a privileged standpoint, threaten to undermine the comfortable link in MSRP between knowledge and rationality (p. 14).

Conclusion

The Capri volume is a striking testimony to Lakatos's influence on the history and methodology of economics--an influence that now clearly seems to be waning rapidly. In part this is but one manifestation of a more general movement away from the earlier fashion of applying comprehensive philosophical and methodological frameworks to economics, or indeed to other scientific disciplines.

The meticulously detailed studies of specific research programs in this volume, written by acknowledged experts and commented on by their informed critics, do not merely display Lakatos's limitations--though that may be the overwhelming impression most readers take from this collection. They also demonstrate, more constructively, how much can be learned when practicing economists take the trouble to examine critically what they and their professional colleagues have been up to, and where it may lead. This is undoubtedly a contribution of lasting value, for which the editors are to be congratulated.

Unfortunately the same cannot be said for the proofreading. In a volume of this size the dozen or so typographical errors are not excessive. But the concluding general bibliography is not complete, as readers are entitled to suppose, for a number of items cited in the text or in the bibliographies appended to most of the articles and comments are missing, while others are incorrect. One page reference (on p. 446) is meaningless--presumably it refers to the author's manuscript; the discussant mentioned in a footnote on p. 58 does not appear in the list of participants; while in commenting on Cross, Blaug cites two passages and one author that do not appear in Cross's essay. Presumably it was revised without Blaug's knowledge.

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Notes

1 While the Capri participants included a "sprinkling" of philosophers and sociologists of science (Cf. Blaug's Afterword), there was an overwhelming preponderance of economists. I see no evidence of the physicists who, Blaug says, were also present. Unless otherwise indicated, all subsequent page references are to this volume.

2 p. 19. De Marchi's introduction gives an excellent overview of the subject matter of the conference, together with thoughtful comments on past uses (and misuses) of Lakatos' work in the economics literature and indications of opportunities missed. While agreeing with many points in his account, I nevertheless feel that his conclusions are more favorable than an examination of the volume suggests.

3 See my "Economics and Psychology: the death and resurrection of a research programme," in Spiro Latsis, *op.cit.* pp. 43-64; and my earlier, even more exploratory effort, "Situational determinism in economics: The implications of Latsis' argument for the historian of economics," *The British Journal of the Philosophy of Science* Vol. 25 (1974) pp. 285-8. To judge by de Marchi's Appendix, pp. 29-30, the first of these efforts was not "creative," although from my standpoint it represented an attempt to find some merit in the Lakatosian model, without being "mechanical".

4 For the provenance of Collins's paper, see p. 22.

5 For differing views on this point see, for example, pp. 18, 27, 137, 336, 364-5.

6 However, interestingly enough, in a post-conference letter to Blaug, Vernon Smith wrote: "On almost every page of Lakatos I find articulate expressions of what I have been doing as an experimentalist for years. I never got that much out of my much earlier reading of Kuhn and Popper . . ." (quoted by de Marchi, p. 21). Possibly others of the Capricies may have had similar afterthoughts.

7 (pp. 319-20). On pp. 24-5 de Marchi argues that "it is not the singularity of designation that makes `research program' the appropriate unit of appraisal, but the notion of dynamically linked sequences of theories, which cohere as to research strategy (and perhaps techniques) and which share some underlying metaphysical presuppositions." It

seems doubtful that many of the Capri authors would accept this rather loose interpretation.

8 (pp. 70, 94ff). As de Marchi observes, "the narrowness of the excess content (novel fact) criterion for judging progress not only causes MSRP to exclude many of the acknowledgeably best gambits of economics, but means that most of economics, indeed of all science, is irrational" (p. 26, n. 6 para 6).

9 P. 142. It should be noted that the commentator, McElroy, firmly disagrees with Gilbert's interpretation. (Cf. pp. 169, 174).

10 The following remarks are both wise and constructive: "It is no news to the historian to be told that it is impossible to construct a convention-free rational history; nor, of course, to hear that all histories are constructs, and the rational is but one of many possible histories; or to read that rational history fails to portray scientific practice `truly'. One writes methodologically self-conscious and, ideally, multiple histories for any episode, in the same spirit as a working scientist tries to be aware of the procedures he or she selects, and of the alternative procedures that might have been chosen or are in fact being used by others. This is not an instrumental defense for ignoring philosophical objections, but a pragmatic argument for proceeding anyway, dealing with the objections on the basis of the best solutions currently available." (De Marchi, p. 27)

11 (p. 496). As Steedman observes, "appraisal needs to be based on the *fullest* knowledge and understanding of a body of work" which only the `insider' possesses; but does it necessarily follow that an insider is likely to be lenient in his or her appraisal? Admittedly, "someone who suspects, on whatever grounds, that a body of work is not that valuable, may not be motivated to acquire the requisite knowledge and understanding for adequate appraisal. . . . The `insider' might be tempted to abuse Lakatos's work to glorify his own scientific allegiance, the `critic' to abuse it to cover his own illiteracy" (pp. 442-3). In other words, as historians well know, there are no impartial, objective criteria of appraisal.