

ON THE LAKATOSIAN APPLE OF DISCORD IN THE HISTORY AND METHODOLOGY OF ECONOMICS*

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While economic methodologists seem to be increasingly dissatisfied with Lakatos's criteria of appraisal, many (internalist) historians of economic thought continue to rely on typically Lakatosian categories in order to identify portions of economic analysis whose historical development is to be 'rationally' reconstructed. This historiographic approach, however, prevents economists from realizing that Lakatosian novel facts may be 'new' not only because previously unknown, but even because previously inexistent. To deny this possibility is tantamount to believing in an incredibly strong version of methodological monism according to which social sciences deal with a subject-matter as immutable over time as that of natural science. (JEL B41)

1. Introduction

In the last decade and half (cf. Latsis 1976) the possibility of usefully applying the Lakatosian methodology of scientific research programmes (hereinafter referred to as MSRP) to the more or less recent history of economics has been one of the most widely debated issues among economic methodologists and historians of economic thought. The end result, how-

ever, is that they now seem to look at MSRP from quite different perspectives and recent literature is characterised by sharply contrasting opinions on the adequacy of Lakatos's approach and its applicability to economics.

Indeed, sharp exchanges often take place when historians and methodologists are jointly attending the same meeting or conference. In this respect the conference on »Transitions in Recent Economics: Studies in Alternative Research Programmes», Capri, 15–18 October, 1989 was particularly instructive.¹ Although purely methodological evaluations of MSRP were basically negative in their overall conclusions, detailed reconstructions of specific subfields of economics nevertheless confi-

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¹ The published outcome of that conference is now in de Marchi and Blaug (1991). See also two other papers by the sociologists Karin Knorr Cetina and H. M. Collins, presented in Capri but not included in this collection, recently appeared in »Minisymposium: Reconstructing Economic Knowledge», *History of Political Economy*, 23, 101–155, 1991.

dently relied on typically Lakatosian notions of the research programmes, hard cores, positive and negative heuristics and so on.

The purpose of this paper is to hint at the possible sources of such a disagreement. After a brief summary of the various arguments, it will be argued that the methodologists' critical attitude towards Lakatosian theses partly echoes critiques raised by philosophers of science with almost exclusive reference to other disciplines, and partly reflects the difficulties encountered in their own repeated attempts at identifying *progressive* research programmes in economics. Historians, in contrast, seem to rely on the Lakatosian notion of the research programme mainly in order to identify portions of economic analysis whose historical development is to be »rationally» reconstructed.

At first sight, because of a number of scholars doing both history and methodology, the distinction between »methodologists» and »historians» might appear ill-defined and therefore somewhat unfounded. For my part, I consider as methodologists those who look at significant chapters of the history of economic thought in order to find possible instances (or case studies) for methodological appraisal in economics. Their main concern with Lakatos's MSRP is with its adequacy to account for the progressiveness of »the acknowledgedly best gambits» in the discipline. Historians I see as those who make use of an explicit methodological perspective in order to achieve historical reconstructions. In a sense their main aim is to avoid carrying on history of economics in a methodological vacuum.²

² For an example of discussion in which the distinction I am hinting at may be easily detected, see Blaug (1976a, 1980, 1987 and 1990a) and Hands (1984, 1985 and 1990a). More references will be given through the paper: see, for instance, notes 11, 12, 13, 14 and 16 below. Some comments on previous drafts of this paper, including some observations by one of the referees, prompt me to recognise that the boundary-line between »historians» and »methodologists» is not always easily detectable. However, I think that distinguishing methodological from historical perspectives in terms of »appraisal» versus »reconstruction» may be accepted, at least provisionally, on the grounds that in the former case the primary object of appraisal is the adequacy to economics of the proposed methodology, while in the latter the dominant concern is with the likelihood of the ensuing historical reconstruction. For a more detailed discussion of the different objects of criticism in economic methodology, see Salanti (1989a).

In some respects this divergence of use might be regarded as sufficient to motivate the different views held by the two groups. In this respect, for instance, Wade Hands (1990a, p. 79) has recently commented:

»[T]he history of economic thought has been much better served by Lakatosian philosophy of science than by other inquiring frameworks of recent interest: Kuhn and rhetoric for example [...] Overall I then would still give the MSRP a negative appraisal as an economic methodology but I would support the work of many of the historians of economic thought that have been guided by Lakatosian ideas. Maybe this is as it should be for Lakatos himself [1978, p. 154] stated: 'I hold that philosophy of science is more of a guide to the historian of science than to the scientist. Since I think that philosophies of rationality lag behind scientific rationality even today, I find it difficult fully to share Popper's optimism that a better philosophy of science will be of considerable help to the scientist.'»

The above judgment may well be sound (even if it should be noted that the contrast we are actually interested in is not between the historian and the economist – which would be the proper object of Lakatos's judgment – but between the historian and the methodologist, that is the very distinction Lakatos was trying to assess), but some further reflections on what lies behind the historians' attitude nevertheless seem to be necessary. More specifically, what is worthy of further attention is how historians merge Lakatosian methodology with their specific research tradition, since such a blending involves the very notion of what the history of economics should be, and consequently the long-standing question of what economists should learn from the history of their own discipline.

2. *The bone(s) of contention: preliminary remarks*

Popper, Kuhn and Lakatos are certainly the philosophers of science who have inspired and influenced most research in economic methodology in the last two decades. This is beyond

dispute among both economic methodologists and historians of economic thought, but an increasing number of the former are becoming more and more critical about the past (and even more the prospective) fruitfulness of such a situation.³ In order to better understand their arguments we have to bear in mind what methodological problems MSRP was originally addressed to.

As is commonly acknowledged, Lakatos's MSRP may well be seen as an attempt to apply Popper's falsificationist methodology (originally conceived as a descriptive as well as a prescriptive response to the classical questions of demarcation and appraisal of scientific theories) to the mainly descriptive question of scientific change and its determinants (originally raised, among others, by Kuhn).⁴ The most apparent shift resulting from his attempt regards the definition of the object of appraisal. While in Popper (as in the whole previous tradition of logical empiricism) such an object was simply represented by the single theory, in Lakatos it became the more complex notion of »research programme» composed of: *i*) a »hard core» whose soundness is usually taken for granted and whose function is to ensure the continuity of scientific work within the same programme; *ii*) a sequence of »instructions» about how to do (positive heuristics) or not to do (negative heuristics) further research within it; *iii*) a set of more or less strictly related theories and models which share the same hard core »postulates» and whose additional (and possibly provisional) »assumptions» must in any case be compatible with the other parts of the programme.

Both *ii*) and *iii*) are said to pertain to the »protective belt» of the programme, in the sense that possible anomalies or falsifying instances will prompt scientists to modify parts of it without being continuously forced to question the hard core of the programme. By means of this very ingenious construction

Lakatos⁵ aimed at both preserving Popper's falsificationist »solution» to the problem of induction and at providing a novel interpretation of the dynamic process of scientific progress (able to retain some interesting Kuhnian insights about the dual nature – normal or revolutionary – of scientific research and the paradigmatic framework within which it takes place). As far as the first goal is concerned, MSRP may well explain why single negative results do not usually lead to the rejection of whole theoretical structures (because hard cores are protected by protective belts against such theory killing) and thus at least partly succeeds in providing falsificationism with a way out from the Duhem-Quine critique. Note, by the way, that the Duhem-Quine thesis, according to which it is impossible to test single theoretical hypotheses simply because the actual object of any test is always a more or less wide conjunction of them, is quite relevant in economics (and in all non-experimental sciences, for that matter) as shown, for instance, by Cross (1982). This may explain, among other things, why economic methodologists and historians of economic thought were particularly impressed by Lakatos's message.

His achievement, however, was not without costs. Having focused his account of scientific progress on research programmes instead of single theories, Lakatos could no longer rely on any criterion of scientific rationality implying instant rationality⁶ and he had to look for other definitions of progress suited to the MSRP context. It was in this respect⁷ that he (and those who tried to pursue the same objective after his untimely death) encountered major obstacles and raised worrying (and still unanswered) points of debate. Such difficulties are mainly concerned with two strictly intertwined groups of questions.

⁵ His most important writings on this subject appeared in 1970–1971 and are now collected in Lakatos (1978). For some insights on the place of Lakatos's contribution within the growth of knowledge debate, see also Lakatos and Musgrave (1970).

⁶ For an extensive discussion of the change of perspective about scientific rationality required in a growth of knowledge approach to the philosophy of science, see Musgrave (1974).

⁷ The relevant literature is too extensive to be quoted in detail. The interested reader may see, for example, Cohen et al. (1975), Radnitzky and Anderson (1978), Pitt (1985), and Gavroglu et al. (1989).

³ See, for example, Mäki (1990) and its bibliographical references. Other recent discussions of the state of economic methodology are in Caldwell (1989), Hausman (1989) and Salanti (1989b).

⁴ To be sure, every notion of progress involves some normative judgment. I refer to it in the text as a mainly descriptive question because Lakatos, as Blaug (1990a, p. 103) rightly points out, »goes beyond Popper in arguing that [MSRP] may also be defensible on positive grounds in terms of the history of science».

The first set of questions derive directly from Popper's unsatisfactory treatment of the notions of corroboration and verisimilitude and affect Lakatos's criteria of progressiveness (which, however, have other problems of their own). Within MSRP a research programme is said to be *theoretically* progressive if it provides excess empirical (that is, falsifiable) content, and *empirically* progressive if, in addition, such content is corroborated (otherwise a research programme is theoretically or empirically degenerating). As is well-known, Popper's attempts to provide sound foundations for his notion of corroboration were never really convincing because he never succeeded in explaining why a theory which has repeatedly survived falsificationist tests ought to be *rationally* regarded with increasing confidence and eventually preferred to others. Indeed it is very difficult, not to say impossible, to provide an answer to such a question without resorting to some form of inductivism.⁸

Anyway, what »to be corroborated» actually means is possibly even more obscure in Lakatos than in Popper, because MSRP requires a notion of corroboration able to justify increasing confidence not simply – as within Popperian falsificationism – in a single theory, but in a whole research programme and its (unfalsifiable) hard core. Indeed, trying to specify the notion of excess empirical content, Lakatos placed much emphasis on novel (*i.e.* hitherto unexpected) facts, which a theoretically progressive research programme should discover. However, such a criterion turned out to be much too restrictive and was seen as very seldom satisfied in the history of (natural) science(s). For these reasons Worrall (1978), generalising some hints already contained in Zahar (1973), provided a less stringent definition of the novel fact, which simply requires that it must not be employed in the construction of the theory. Worrall (1978, p. 48) summarises his neoLakatosian interpretation as follows: »This methodology embodies the simple rule that one cannot use the same fact twice; once in the construction of a theory and

then again in its support». It can be easily seen, however, that in economics the same »fact» (think, for example, of Keynesian unemployment) is actually used for three purposes: the construction of the theory, its support, and the criticism of rival explanations of the same phenomenon. Even such a definition, therefore, appears to be too demanding for most research programmes in economics.⁹

The second source of troubles is represented by the metacriterion of appraisal proposed by Lakatos as appropriate to MSRP. A consequence of the Lakatosian definition(s) of scientific progress is that we ought to expect scientists to withdraw from degenerating programmes and to further pursue progressive ones (in a sense this may be interpreted as the criterion of scientific rationality proper to MSRP). But a doubt arises whenever we observe a degenerating programme that is nonetheless still followed by a scientific community. In such cases it is not clear at all what must be inferred. One could alternatively conclude that: *i*) a degenerating programme is put aside only when a more promising one is available (a reasonable conclusion in a Kuhnian perspective); *ii*) scientific rationality is nothing but an illusion so that every attempt to capture it by means of unrealistically demanding definitions is doomed to failure (a not surprising conclusion in the realm of the rhetorical approach); *iii*) the Lakatosian characterisation of a degenerating paradigm is not suited to its intended purpose and needs substantial modifications (a conclusion which sends us back to the problem of how scientific »progress» is actually achieved).

Finally, note that if the problem of the progressiveness of a scientific enterprise is a serious one within a Popperian perspective, it becomes really crucial for MSRP. While for Popper, after all, the *potential* falsifiability of a theory is enough to guarantee its scientific status, MSRP can ascertain the scientific »rationality» behind one's commitment with a certain research programme *only* through its *actual* compliance with the above mentioned criteria of theoretical and empirical progress. This is so because, if loosely interpreted (cf. Caldwell, 1991a), most Lakatosian categories turn out to fit almost any discipline, whether »truly scientific» or not.

⁸ This is a point which economists often tend to overlook. As I have tried to argue elsewhere (Salanti, 1987, pp. 383–384), a glaring example of how easy it is to confuse »falsificationism» with »induction by elimination» is provided by Friedman's shallow allusions to falsificationism in his famous 1953 methodological essay.

⁹ For bibliographical references on this point, see note 14 below.

3. Different perspectives of methodologists and historians¹⁰

What I have tried to summarise in the previous section may be considered common knowledge among economic methodologists (see, for a similar judgment, Hands, 1990b): no wonder, therefore, if in recent years they have been showing their dissatisfaction with the state of the debate.¹¹ At the same time, however, the feeling among historians of economic thought seems to be very different. Blaug (1980) and Hutchison (1978 and 1981) continue to uphold the necessity of falsificationism in economics (despite admitting that it is almost never practised), and the most recent literature in the field frequently mentions the utility of Lakatosian insights in reconstructing the most important chapters of the history of economic thought and modern economics.¹²

So far, in my opinion, the existence of the above different perspectives has not received sufficient attention it should have. The reasons for this are twofold: first, some commentators dismiss the divergence by observing that the two contrasting points of view are due quite naturally to the different aims pursued by methodologists and historians in their re-

search. The same people may make little of the methodological incoherence of some historians by claiming that good work in the field of the history of economic thought can be done even in presence of questionable methodological assumptions.¹³

Second, to judge from the few direct exchanges between the two camps, the main bone of contention would seem to be merely the (in)adequacy of the Lakatosian characterisation of scientific progress with reference to the »best gambits» (namely general equilibrium analysis and Keynesian macroeconomics) in twentieth-century economics.¹⁴

I do not wish to deny here either that methodologists and historians have different goals, or that the possibility of detecting novel facts in the Lakatosian sense is one of the issues at stake. My contention is simply that there are some deeper reasons for the sympathetic attitude of historians towards Lakatosian methodology, connected with their commitment to an absolutist or internal (as opposed to a relativist or external) approach to the history of thought. As is well known, indeed, *an internal history of economic thought can be justified if a positive answer can be given to the question »Has economic theory progressed over its history?»*¹⁵, and *MSRP provides historians with just such a criterion of progressiveness.*

¹⁰ About the distinction between methodologists and historians the reader may refer to what has been previously said in the introductory section and in note 2 above.

¹¹ Critical assessments of falsificationism in economics can be found, for example, in Boland (1982), Caldwell (1982, 1984 and 1991b), Hands (1992), Hausman (1985 and 1988), Morgan (1988 and 1990) and Salanti (1987). As far as *MSRP* is specifically concerned, see Hands (1985 and 1990a).

¹² Latsis (1972) was probably the first published attempt to apply Lakatosian methodology within economics with reference to the neoclassical theory of the firm. The task of applying Lakatos's *MSRP* to the major theoretical changes in the history of economics was undertaken by Blaug (1976a) with reference to the Keynesian revolution. For other attempts in the same perspective, see Blaug (1976b and 1981), de Marchi and Blaug (1991), Latsis (1976), Fisher (1986), Weintraub (1985a). As far as textbooks of history of economic thought are concerned see, for example, Backhouse (1985, pp. 2–8 and 408–414), Negishi (1985, pp. 4–6 and 1989, pp. 1–5). Even those (like, for instance, Ekelund and Hébert, 1983 and Landreth and Colander, 1989) who are unwilling to commit themselves to a specific methodological approach devote some attention to economic methodology and acknowledge the usefulness of some Lakatosian insights. I found only two exceptions, both devoted to classical economics: Hollander (1987) and O'Brien (1975).

¹³ For Hands's (1990a) conclusions in this respect, see the passage quoted on p. 31. In the same vein, reviewing Weintraub (1985a), Boland (1987, pp. 662–663, n. 1) writes: »I do not wish to suggest here that Weintraub's book is beyond criticism. While I think Weintraub's study represents good history of thought, I think it falls short in its discussion of methodology. [...] Fortunately, his narrow view of methodology does not seriously detract from the major contribution he makes to the study of the history of the modern mathematical economics.»

¹⁴ As far as the »neo-Walrasian research program» is concerned, Weintraub's (1985a, 1985b and 1987) attempt to show its progressiveness according to a Lakatosian perspective has remained remarkably isolated. Apart from explicit criticism (see Rosenberg 1986, Boland 1987 and Salanti 1991), there seem to be a wide consensus on the fact that general equilibrium theory does not fit Lakatosian criteria of progressiveness (for an open acknowledgement, see Blaug 1990a; cp. also Backhouse 1993 and Salanti 1993). The possibility of accounting for the novelty embedded in Keynes's *General Theory* along Lakatosian lines is much more controversial: see the discussions between Blaug (1976a, 1987 and 1990a), Hands (1985 and 1990a) and Ahonen (1989, and 1990).

¹⁵ See, for instance, the introductory chapter in Blaug (1985), surely the most prominent and influential »inter-

To be sure, this does not imply that to do internal history necessarily ends by producing Whig history. One might well appraise the evolution of the inner logic of a piece of economic theory and then conclude, for example, that it represents a degenerating programme in the Lakatosian sense. However, such an outcome must surely constitute an exception, rather than the rule: otherwise the history of thought would become a rather strange activity perversely inclined to intellectual hypochondria.

Given the almost unanimous preference in recent decades for internal histories of economics, the historians' relish for Lakatosian methodology is quite understandable. It is not by chance that to Hands's (1990a, p. 79) apparently uncontroversial observation that

»We in economics and those in every other branch of science choose theories because they are deeper, simpler, more general, more operational, explain known facts better, are more corroborated, are more consistent with what we consider to be deeper theories: and for many other reasons. Even if we can find a few novel facts here and there in the history of economics, and even if those novel facts seem to provide an occasional 'clincher', the history of great economics is so much more than a list of these novel facts.»

Blaug (1990a, p. 103) bluntly replies:

»If Hands wants to say that the distinction between internal and external history of science is silly, that there is no point in isolating those occasional 'clinchers' in the history of any science, and that successful scientific theories succeed for a multitude of reasons which are all equally true – in short, that all historical explanations are arbitrary and that explanations of intellectual history are the most arbitrary of all – why not say so, instead of hanging it all on a confusing diatribe against Lakatos?».

As the reader can easily see, Blaug's main worry (and that of other historians, for that

matter)¹⁶ is precisely about the possible consequences of a withdrawal from Lakatos as far as the justification of an internal approach to the history of economics is concerned. Such a strict association, however, appears to be neither really necessary, nor very helpful in order to promote more interest in the field among economists. To understand why, it suffices to recall that the »internalist« tradition in the history of economic thought goes back (at least) to the still outstanding work of Schumpeter (1954), i.e. well before Lakatosian methodology came on stage. In contrast, the question (paraphrasing Winch, 1962) »What price the internal history of economic thought?« is much more complex and deserves more detailed discussion (the object of the following section).

As far as Blaug's methodological position is concerned, there is a further point which seems to be even more open to question. This concerns his (1980) justification of Popperian falsificationism, based on the observation that falsificationism is a *normative* proposal which, as such, cannot be faulted for failing to *describe* scientific practice. No wonder, therefore, if economists preach it but nevertheless rarely practice it. So far so good. But then, how is it possible – as Blaug claims when he reconstructs pieces of the history of economics along Lakatosian lines (see note 14 above for references) – to have *progress* in economics? If only for sake of consistency Blaug ought to admit that falsificationism is simply useless and/or unnecessary as a simple *prescription* for doing good science, if »good science« is to be taken as »working out *progressive* research programmes».¹⁷ Otherwise

¹⁶ In addition to the references already provided in note 12 above, a recent paper by Backhouse (1992) is worthy of mention in this respect. The author critically examines Hands's position and takes just the opposite route: he suggests the retention of Lakatos's appraisal criterion (based on novel facts) and its application to »sequences of [related] theories«, irrespective of the conformability of such sequences to the notion of research programme.

¹⁷ The reader should note that I am not maintaining that *progressiveness* can never be attained through falsifications. After all, the »growth of knowledge« story may well be told according to a strictly Popperian perspective (see Popper 1963, ch. 10). What I want to say is instead that it would be very surprising to identify episodes of truly Lakatosian progress involving scientists who simply disregard falsificationist precepts.

nal» history of economics of the last two decades. In the bibliographical notes appended to this chapter (as well as in Blaug 1990b) the reader may find a number of useful references on the variety of approaches in the field.

one would not understand how it is possible to do good science without taking falsificationism seriously. How may it be possible to be »critical of what economists actually do as distinct from what they say to do» (Blaug 1984, p. 33) and at the same time to maintain that (important pieces of) what economists actually do can be shown to be »progressive» according to Lakatosian criteria?

Alternatively, Blaug could maintain his advocacy of falsificationism, but then he ought to show that »Lakatosian progressive» chapters of the history of economics (e.g. the Keynesian revolution) were marked by an effective practice of falsificationism: a claim that would be quite difficult to prove.

4. *History of economics and modern economic theory*

As Mark Blaug has rightly observed:

»[T]here is room for a variety of approaches to the history of economics and it is not at all obvious [...] that narrow absolutism has done more harm to the subject than shallow relativism.» (Blaug 1985, p. 9; cp. also 1990b, p. 28).

The problem, however, is that both approaches (particularly in their extreme forms) have had harmful effects on discussions (these too by no means new)¹⁸ about the role of the history of economic thought in the understanding of modern economic theory. Such an outcome is inevitable for the »relativist» approach, external histories often referring to things which have nothing to do with the theoretical progressiveness of the discipline and therefore may be easily dismissed as uninteresting and/or irrelevant by the representative economist. However, the matter is more intricate as far as the »absolutist» approach is concerned.

The paradox of the concept of the history of economics as »economic theory in retrospect» is that, although it convincingly emphasises the need to know modern theory in order to

understand that of the past¹⁹, it hardly provides a credible justification for the need to study history in order to understand modern theory. To be sure, the more we focus on the inner intertwinement of progressive theoretical »trajectories», the more difficult it is to provide a satisfactory answer to the question »Why then should we go back to old authors and rehearse outmoded views?». Attempts to »rationally» reconstruct these trajectories according to MSRP make things possibly even worse. In such cases, indeed, attention ends up focusing in an exclusive way on a single (progressive) research programme so that the scope of studies in the history of thought becomes narrower and narrower.

Even a cursory reading of the introductory chapters to the best known textbooks of history of economic analysis is quite instructive in this respect. They usually try to convince the reader with some not very original reiterations of one or two of the well-known threefold motivation envisaged by Schumpeter (1954, pp. 4–6), that is: *i*) a pedagogical advantage, because »the state of any science at any given time implies its past history and cannot be satisfactorily conveyed without making this implicit history explicit»; *ii*) the possibility of attaining new ideas, since »our minds are apt to derive new inspiration from the history of science», and *iii*) gaining »insights into the ways of the human mind». Blaug (1985) represents a pleasant exception. He is too expert a writer to fall into the trap of boring his readers from the outset with useless repetitions, and solves the problem by quoting T.S. Eliot's elegant aphorism (»Someone said: 'The dead writers are more remote from us because we know so much more than they did.' Precisely, and they are that which we know») as both initial epigraph and last sentence of the book. This is undoubtedly a brilliant rhetorical device, but, quite obviously, it is not of more use than the twice-told tales of less brilliant colleagues.

Indeed, a contemporaneous commitment to both an internalist approach to the history of economics and a strictly Lakatosian MSRP

¹⁸ See, for instance, the contributions by Gordon, Gramp, Fetter and the following discussion in the session on »The contribution of the history of economic thought to the understanding of economic theory, economic history, and the history of economic policy» at the 1964 AEA annual meeting.

¹⁹ I am unwilling to enter here into a discussion on whether »economic theory in retrospect» represents »doxography» or, more simply, the method of rational reconstructions. This because the distinction between such characterisations »is at best a subtle one» (Blaug, 1990b, p. 28) and, moreover, it would not make any difference to the present discussion.

(and/or MHRP, for that matter)²⁰ leaves open only two possibilities of justifying the role of the history of analysis in understanding (modern) economic theory. For those who are more inclined to stress the variety of rival paradigms within economics, the task of historians is merely to remind economists of the »polyparadigmatic character of contemporary economics« (Coats, 1987, p.12), presumably in the conviction that acquaintance with the history of the discipline can form an appreciation of how »an idea that is outmoded relative to one of the currently dominating paradigms may be useful for the development of another, possibly new, paradigm« (Negishi, 1985, p. 5), or, in more strictly Lakatosian terms, that

»With a new protective belt theory, even a currently hibernating research programme can make a triumphal return. The outmoded ideas of such a research programme are, however, not well reflected in the currently dominating economic theories, so that one cannot understand the possibility of the return of the former ideas simply by studying the latter theories only. This is why we should study the history of economics.« (Negishi, 1989, p. 5).

To those who instead pay more attention to the way in which the »protective belt« of a single (usually the mainstream) research programme evolves over time through »conjectures and refutations«, history has the function of pointing out that even within the same research programme the pattern of the historical development of economic theory is not a steady growth toward the »truth«, and that in any time the currently dominating theory is not necessarily superior to past theories in every respect.²¹

In my opinion both these attempts²² to show

the utility of the history of economic thought are somewhat ineffective. To the former it may be objected that greater interest in the history of the subject does not necessarily lead to the acknowledgement of the plurality of paradigms (or research programmes), for it might be argued that what is needed is merely to pay more attention to a wider spectrum of *contemporary* literature. Indeed, if we take for granted that within every single research programme there has always been some progress (and Lakatosians cannot avoid such a view, not only to avoid explaining what others might see as an excessive number of degenerating programmes, but also simply in order to retain their confidence in scientific rationality), we are led to be more tolerant and possibly pluralist as far as methodology is concerned, but not – *per se* – more interested in the history of economics.²³

Analogously, the second justification for the relevance of the history of economics can be countered by saying that what is really necessary for the intended purposes is simply to read the relevant literature of the last fifty (instead of, say, five) years and(/or?) good surveys of such a literature. But for this, however, there is no need for the historian, since every scholar, I presume, is able to unravel the literature in his own specific field of research by himself, irrespective of the date of publication.

economics has actually developed. Note, however, that Lakatosians like Backhouse (1992) or Blaug (1991) seem more attached to the Lakatosian criterion of progress than to any other feature of MSRP.

²³ As I noted elsewhere (Salanti, 1986), Pasinetti's (1986, pp. 429–430) concluding statements on this matter are very illuminating and worthy of being fully quoted here:

»Given the crude simplifications that economists are compelled to make, some considerable dose of humility about the merits and applicability of one's theories, and at the same time the avoidance of quite unwarranted attitudes of intolerance against other theorists' theories, would seem a code of behaviour much more appropriate to the state and nature of knowledge in economics.

We might well need some new rules of the theoretical game, precisely in order to enhance the development of economic analysis. Meanwhile, the coexistence of competing paradigms, even for long periods of time, far from being regarded as a sign of anomaly, might well come to be recognised as the normal way of proceeding in the development of economic theory.»

²⁰ For an account of the distinction between MSRP, the methodology of scientific research programmes, and MHRP, the methodology of historical research programmes, see Blaug (1990a).

²¹ As far as I can see, the most coherent presentation of this view (even if not explicitly formulated in the Lakatosian terminology) is to be found in Cesarano (1983). A quite similar conclusion, however, may also be reached from a »rhetoric« standpoint (cf. Weintraub, 1989).

²² Note that I am referring to the only two possibilities within a strictly Lakatosian approach to economic methodology. Admittedly, as pointed out by one referee, if we are prepared to explore different criteria of progressiveness we may well increase our knowledge about how

As Cesarano (1983, p. 63) himself rightly observes:

»At present, review articles are the closest equivalent to traditional research by historians of economics. However such works are based on a more limited analytical perspective and are usually confined to the very recent literature.»

Obviously review articles are more limited in scope and content than scholarly research in the history of economic thought. But they constitute almost all that is really needed if the problem is presented (*ibidem*, pp. 67–68) as follows:

»The essential issue is not to establish whether the course science takes towards a 'true' theory is that of an upward trend or a stationary path, but how the path behaves relative to the trend. Indeed, even in the presence of convergence, the path of scientific development [...] may be characterised by fluctuations along the positively inclined trend so that, for some periods, the state of advancement of the discipline may be a lower level than the achievements of the past. [...] A sufficient condition for the usefulness of the history of a subject is the failure to comply with the requirement of a non-decreasing monotonic development path.»

Note, moreover, that the above »sufficient condition» virtually holds for all disciplines (including mathematics, if Lakatos, 1976, is to be credited); nevertheless in many of them the general esteem for their own history is even lower than in economics.

5. Concluding remarks

Internal approaches to the history of economics have in common two main weaknesses. First, their justification of the discipline refers exclusively to modern economic analysis²⁴, thus obliging historians to attempt (usually unsuccessfully) to persuade their fellow economists of the importance of their subject.

Second, the (often hidden) methodological assumption that »before a comprehensive history of modern economics can be written, a

holistic, general theory of disciplinary evolution is needed» (Remeny, 1979, pp. 30–31) is at odds with all the recent attempts to approach economic methodology in a pluralist perspective²⁵, and brings about contrasting judgments on methodological matters.

Admittedly, as is often the case when such matters are concerned, criticism is easier than the provision of a sound alternative, and this is even more true for the issue discussed here. Indeed, the traditional alternative, well summarised by Cesarano as follows

»[...] while economists have sought to find a role for the history of the discipline, most scholars have advanced arguments for the enlargements of the sphere of the subject, as opposed to arguments that ascribe a new importance to traditional forms of history of economic *analysis*.» (Cesarano 1983, p. 65),

offers a number of heterogeneous proposals²⁶ none of which, however, has gained a general consensus.

I do not wish either to revive here the old quarrel between »internalists» and »externalists» in the history of economic thought, or to evoke the ghosts of the *Methodenstreit* of a century ago. Apart from space constraints, I do not think that an unreserved endorsement of all externalist claims would contribute very much to improve the current state of affairs. There is one point, however, to which we should pay more attention.

As Goodwin (1980) aptly pointed out in a too quickly forgotten presidential address to the History of Economics Society (and Barber, 1990, duly repeated on the same occasion ten years later), major changes in the history of economics have been prompted by events outside the discipline, and progressiveness (*pace* Lakatos) has been achieved through attempts to deal with such events. That the subject matter of economic inquiry changes over time is an obvious fact of (historical) experience²⁷,

²⁵ For further comments and detailed references, see Salanti (1989b).

²⁶ Cf., for instance, Winch (1962), Gordon (1965), Grampp (1965), Goodwin (1972), Samuels (1974), Coats (1969 and 1978).

²⁷ Perhaps it is a bit more obvious in Italy (and in continental Europe) than in Anglo-American(ized) countries. Cf., for example, Screpanti and Zamagni (1992) and Sylos Labini (1990).

²⁴ Cf., for instance, Stigler (1969), Corry (1975), Remeny (1979), Cesarano (1983), Negishi (1985, ch. 1), and Coats (1987).

and that the utility of any scientific theory ultimately consists in its capacity to explain (and possibly predict) facts ought to be equally plain. To acknowledge these two things, far from leading us into some form of »shallow relativism«, could represent a first step towards a more insightful history of economics. How can we imagine a history of economics completely divorced from economic history (and from other kinds of history, for that matter) when even the desirability of the presently complete separation between modern economics and economic history is by no means obvious (see, for instance, Parker, 1986)?

After all, it is not at all plain that Lakatosian novel facts are not new simply because previously unknown, rather than because previously inexistent. To deny this possibility is tantamount to believing in an incredibly strong version of methodological monism according to which social sciences deal with a subject-matter as immutable over time as that of natural science.

Arguments for more historically conscious theorising in economics are well-known. Instead of repeating them for the hundredth time, I prefer to conclude drawing the attention of the reader on the necessity of further reflections on the reasons why a fact so obvious as the historically contingent character of the subject-matter of our discipline commonly fails to be fully appreciated even by those (the historians of economic thought) who should be perfectly aware of it.²⁸

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²⁸ Stimulating observations in this regard may be found in Mini (1974), a somewhat unduly neglected book (probably because published before the nowadays renewed interest in economic methodology).

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