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8. The Methodology of Scientific Research Programmes in Economics: Criticisms and Conjectures

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Methodological writings in economics are not all of a piece; they exhibit considerable diversity. There are the pronouncements of practitioners about how economics is done and these often include, explicitly or implicitly, warnings about how not to do it. Not philosophically self-conscious, such accounts typically look to examples of best practice from within the discipline. Two good examples of this type of work are the articles by Milton Freidman (1953) and Melvin Reder (1982).

Methodological argumentation is also used, this time as a weapon, by critics of 'mainstream economic theory'. Sometimes critics provide philosophical arguments in their attacks. For example, Ludwig von Mises (1949) and Martin Hollis and E. J. Nell (1975) both decry the alleged positivist underpinnings of neoclassical economics and propose a rationalist foundation in its place. Other opponents ignore philosophy and focus instead on the realism of assumptions of economic theory, or its limited predictive ability, or its heavy use of mathematical formalism.

A third approach to methodology is much more explicit in the importance it accords to philosophy, and particularly to arguments from the philosophy of science. Practitioners of this approach include both philosophers of science who are interested in economics as an example of a science and economists who look towards the philosophy of science for insights about how science works. Our focus will be on members of the latter group.

Some famous economists have drawn on the philosophy of science to buttress their methodological arguments. Paul Samuelson is one example, though he also looked towards other sciences (particularly physics) for his exemplars of good practice. Fritz Machlup is another, though his borrowings from the philosophy of science were often eclectic.[1] T. W. Hutchison is probably the best example. His famous (1938) book introduced empiricist philosophy of science to English-speaking economists, and later he became a cautious but consistent advocate of Popperian thought. Mark Blaug who, like Hutchison, borrows heavily from Popper, should also be included. Indeed, he is arguably the leading modern representative among economists of the third tradition.

Though it is arbitrary to fix a date to it, I like to think of 1976 as marking the beginning of the modern era in economic methodology. Two books published that year were seminal. *Microeconomic Laws* by philosopher Alexander Rosenberg was the first extended attempt by a philosopher of science to analyse the structure of economic theory. *Method and Appraisal in Economics*, edited by Spiro Latsis, was a collection of papers, written by economists, whose purpose was to examine whether philosopher Imre Lakatos's methodological framework (the 'Methodology of Scientific Research Programmes', or MSRP) could be applied within their discipline (Lakatos 1970).

In the last decade or so, work within the third tradition has grown dramatically. Mark Blaug played an important role in the expansion by providing the first text-length survey of the field, *The Methodology of Economics* (1980a), the first third of which was devoted to the philosophy of science. In 1985 the journal *Economics and Philosophy* began publication under the editorship of a philosopher (Daniel Hausman) and an economist (Michael McPherson). The ranks both of philosophers studying economics and of economists interested in philosophy have increased steadily. A recent bibliography of the field contains over 2 000 entries, and most of these were published within the last fifteen years (Redman 1989).

Though the third tradition includes both philosophers and economists, it is important to emphasize that each group typically has a different research agenda. In particular, economists have usually been most interested in questions of theory appraisal and have looked to the philosophy of science for separate, independent standards against which to compare economic theories. This certainly has been the approach taken by Mark Blaug, who gradually came to view Lakatos's MSRP as providing the requisite standards for economics. Given his prominence in the field, it is worthwhile briefly to reconstruct how Mark Blaug might come to hold this position.

BLAUG AND THE MSRP: A SITUATIONAL ANALYSIS

There are three good reasons why Mark Blaug might turn towards Lakatos. All of them have to do with Karl Popper's philosophy of science.

Mark Blaug has long been an advocate of Popperian thought and has used his falsificationist framework effectively to assess the development of economic doctrines in the various editions of his text, *Economic Theory in Retrospect*. Popper offers a number of prescriptions for proper scientific procedure. The most important of these is that scientists should test theories – the more severely the better – in an attempt to refute them. The best theories are those which have survived frequent and strenuous attempts at falsification. Popper insists further that falsifications be taken seriously. In particular, he warns against 'immunizing stratagems', or changes in a theory which are undertaken solely to keep it from being refuted. He notes that it is always easy to save a theory from a refutation by simply altering it so that the new modified theory accounts for the anomalous outcome. Popper explicitly prohibits such *ad hoc* theory adjustments.

Popper's pronouncements about proper scientific procedure cause a number of problems for Blaug the historian of science and methodologist.

(a) Falsificationism is a normative doctrine, its goal is to provide criteria for demarcating the scientific from the non-scientific, and for eliminating false theories from science. As such, it cannot be faulted for failing to describe scientific practice, though one could hope that it would be descriptive of certain paradigmatic episodes of 'good science', however they might be determined. The problem for economics is that a review of its history reveals precious little evidence of any falsificationist practice. As Blaug put it in his book on methodology (1980: 128): 'Modern economists frequently preach falsificationism, as we have seen, but they rarely practice it: their working philosophy of science is aptly described as "innocuous falsificationism".'

It turns out that economics was not unique in this respect, nor, for that matter, was falsificationism. By the 1960s there was a growing awareness among philosophers of science that a number of prescriptivist empiricist doctrines (for example, operationalism, logical positivism, logical empiricism) seemed inadequate for describing actual scientific practice, including the so-called paradigmatic episodes. The reaction of some scholars was to reject normative philosophy altogether and to embrace instead the more descriptively adequate models of science being developed within the history and sociology of science. Thomas Kuhn's *The Structure of Scientific Revolutions* (1962) is the most famous example of such an alternative. Though the gains in descriptive adequacy were evident, the problem with such analyses was that they lacked normative punch: they did not provide criteria for judging good science from bad, they were relativistic when it came to matters of appraisal. It should finally be mentioned that, from the present perspective, the tendency towards relativism has if anything gained strength in the intervening years, as deconstructionist and anti-foundationalist elements have worked their way into discourse about the sciences.

As an historian of thought interested in appraising the theories of economists, Mark Blaug faced a dilemma. Popper's normative philosophy of science was too strict, but Kuhn's relativism was too lax. By providing a methodological programme which claimed to be at once prescriptively robust and descriptively adequate, Lakatos's MSRP appeared to save the day.

Even better, Lakatos's promises seemed plausible. The MSRP clearly contained prescriptive content: Lakatos provided explicit criteria for appraising research programmes as either progressive or degenerating. And Lakatos also intended that the MSRP be able to make sense of both the best instances of current practice as well as past episodes of successful scientific achievements. Indeed, he even claimed (Lakatos 1971) that the MSRP could itself be tested against the history of a science by seeing how well it rationalizes the best gambits of scientific progress, as determined by the scientific élite. Given the limitations of Popper and Kuhn, it was natural for Blaug to urge economists to look more closely at the Lakatosian framework. This is just what he did in his contribution to the Latsis volume (Blaug 1976b).

(b) As first noted in Hands (1985a), there exists a tension between Popper's writings on the methodology of the natural sciences and his writings on the methodology of the social sciences. Hands distinguishes between two different Poppers. Popper_n (for natural sciences) is the falsificationist Popper whom we have just met. Popper_s (for social sciences) is an advocate of situational analysis. A social science explanation which utilizes situational analysis proceeds as follows. The explanandum (or event to be explained) is deduced from an explanans, which contains three elements: a description of the situation, an analysis of what action is reasonable in the situation, and a rationality postulate stating that agents will always act reasonably or appropriately. Explaining an agent's action or some other social phenomenon using situational analysis reduces to showing that whatever occurred was reasonable, or understandable, or appropriate, given the agent(s) and the situational constraints.

What happens when the actual outcome does not conform with the prediction of the situational analysis? In such instances, Popper_s advises the social scientist to revise either the description of the situation or the analysis of what a reasonable agent would do. Such revisions occur until one obtains a prediction of what actually took place. Most important,

Popper_s specifically prohibits the social scientist from ever rejecting the rationality postulate.

The source of the tension between Popper_n and Popper_s should now be clear. By manipulating the description of the situation until the 'right' result is obtained, by making sure never to reject the rationality postulate, the social scientist is engaging in an immunizing stratagem of the most egregious kind. Such behaviour flies in the face of Popper_n's prescription against *ad hoc* theory adjustments, but it is precisely the behaviour that Popper_s recommends. How is one to respond to this apparent inconsistency in Popper's thought?

As noted earlier, Mark Blaug likes falsificationism (though he feels it is too little practised by economists). He also (1985b) tends to discount Popper's specific account of situational logic. If I understand his position correctly, Blaug's solution to Popper's apparent inconsistency is to reconstruct situational analysis along Lakatosian lines. Following Latsis (1972), the rationality postulate is viewed as a part of the hard core of neoclassical theory. Its use is justified only to the extent that neoclassical theory continues to progress as a research programme. How progressive has it been? Blaug's assessment of the programme in radical economics sheds some light on the question. Though he remains an 'unconverted neoclassical', he concludes that 'a study of radical economics leaves one ultimately almost as unhappy with orthodox economics as with radical economics' (Blaug 1983: 238). From Blaug's vantage point, the neoclassical research programme needs a lot more work, but at present it still looks like the best game in town.

(c) So far I have argued that it was natural for a Popperian like Mark Blaug to be enthusiastic about Lakatos, because Lakatos improves on Popper in just those areas where an historian of thought and a methodologist might judge Popper to be weakest. None of this would matter, however, if it turned out that the MSRP did not apply to economics.

But happily, it seemed that the MSRP fits economics remarkably well. The Lakatosian categories of hard cores, protective belts and positive and negative heuristics made sense to economists, especially neoclassical economists, when they thought about their discipline. Though economists might differ as to its exact content, the neoclassical programme appears to have certain hard-core elements, irrefutable parts which are specifically insulated from criticism. Though these hard-core elements are protected, there is also obviously a tremendous amount of empirical work within economics, and it is natural to characterize such work as lying in a protective belt around the hard-core assumptions. Economists would also find Lakatos's portrayal of how testing takes place in the protective belt to be an accurate description of their practice. Lakatos claimed that scientists often seek confirmations rather than falsifications, that crucial tests are rare and instant rationality unavailable, and that appraisal of a programme's progressivity is only possible over long periods of time. All of these notions resonate with economists, for they seem to characterize accurately the way empirical work is actually done, the frustrations associated with it, and the importance of being cautious about drawing conclusions from data whose interpretation is often ambiguous. Neoclassicals also work with a set of positive and negative heuristics which guide their research, suggesting which lines of questioning might hold the most promise. To be sure, the heuristics vary across the discipline, depending upon such things as whether one is principally a theorist or an applied economist, or whether one was trained at MIT or Minnesota or Chicago. But their presence is evident.

Thus the Lakatosian framework not only solved certain philosophical problems associated with Popperian thought, it apparently provided a vehicle for analysing the science of economics. This completes the situational analysis of Mark Blaug's movement away from Popper and towards Lakatos, and rational indeed his exit seems to be.

The purpose of this paper is to suggest that another alternative might be preferable. As such, it may be helpful to point out first those areas where I am in agreement with Mark Blaug (or better, with my reconstruction of Blaug's views). I agree with Blaug that it is difficult to apply falsificationism in economics, and further that the tension between Popper_n and Popper_s is a real one. As far as I know, I also agree with most of Blaug's substantive judgements about which research programmes within economics are doing the best work and which are least impressive. What separates us is how best to characterize the process by which such judgements might be reached.

Rather than accepting the Lakatosian framework, I am an advocate of critical pluralism, which is a modified version of Popper's critical rationalism. Like Blaug's solution, my alternative remains squarely within the Popperian tradition. It may also be the case that critical rationalism provides a solution to the dilemma of the two Poppers (Caldwell 1991). I shall argue presently that it is more accurate to view critical rationalism as occupying a middle ground between Lakatos and his relativist opponents than it is to see Lakatos as the golden mean between Kuhn and Popper. And most important, I do not think that the Lakatosian framework fits economics very well at all, at least along the dimensions that really matter. Let us turn to this key question first.

THE MSRP AND ECONOMICS

One should be suspicious when a framework which was developed with the natural sciences in mind appears to rationalize the practice of economists so comfortably. Why does the MSRP work so well in economics?

At least one reason is that many of the Lakatosian categories, when interpreted broadly enough, would fit *any* discipline. For example, the hard core of a research programme is simply the starting point of an analysis. Such starting points are generally taken as given by those working within the programme, which is another way of saying that they are taken to be irrefutable. (Contingent starting points have a way of not remaining starting points for very long.) And because every analysis must start *somewhere*, every programme ends up having a hard core. Positive and negative heuristics are simply the instructions one follows to do the analysis: some approaches are favoured and others are ruled out. All reasoning is pursued in this way. Finally, the protective belt is where some connection with the world is made. And Lakatos is right: often when one holds a theory, one simply looks to the world to confirm it. His observation that sometimes people hold on to theories even in the face of disconfirming evidence is also clearly true. Neither behaviour is particularly exemplary, but both are commonly encountered.

The point is simple: the Lakatosian categories which seem to make so much sense of economics would also make sense of virtually any form of human inquiry. They make sense within the sciences, but they also make sense of astrology, of textual criticism and of the kind of casual empiricism which characterizes everyday reasoning. What form of human reasoning lacks a starting point, or a strategy for identifying which questions to ask, or some contact (usually with the intent of rationalizing one's prior views) with the world?

This would not constitute a criticism of Lakatos had he set for himself the task of providing a general description of how human reasoning works. But he claimed to do much more, to have developed a programme which would allow one to distinguish scientific from other forms of human reasoning. As such, in assessing the MSRP, we must ignore the characteristics which describe human reasoning in general, and focus on those elements which are unique to scientific reasoning. Given this goal, I submit that the part of the MSRP upon which we should focus all our attention is Lakatos's criterion for appraising scientific progress. A research programme is judged progressive if it is capable through time of generating novel facts, some of which are ultimately corroborated. If we wish to see how well the MSRP works in economics, we must do so not by looking for hard cores and protective belts, or for positive and negative heuristics, but for instances of progressive problem-shifts. What does the evidence suggest?

Mark Blaug and Neil de Marchi co-directed a conference in Capri in 1989, and one of the goals of the conference was to answer just this question. I participated in the conference as a discussant, and my impression at the time was that too few authors had really tried to answer it. A conference volume is forthcoming (Blaug and de Marchi 1991), however, and perhaps if some of the papers are revised, more attention will be paid to whether and when economists have, through the process of theory revision, come across novel facts, some of which were confirmed.

Of course, the Capri conference was not the first time the question had been asked. In his introduction to the volume, de Marchi (1991) summarizes some of the findings contained in the already large Lakatosian literature in economics. One of his conclusions is that 'methodological analysis . . . has given us a better appreciation of the difficulties in applying the criterion of empirical progress in economics' (p. 10). In his assessment of the literature, Wade Hands (1985b) reaches much the same conclusion. At least so far there is reason to doubt that there exist many examples of progressive research programmes in economics.[2]

BLAUG'S CASE STUDY: THE KEYNESIAN REVOLUTION

I think that Mark Blaug would accept the argument that the best test of the MSRP is to see whether there are instances of progressive problem-shifts in economics. This would at least explain why he seems unconvinced by Roy Weintraub's (1985) ingenious reconstruction of the development of existence and stability proofs within general equilibrium theory as constituting a Lakatosian 'hardening of the hard core' (Blaug 1990a: note 2). But Blaug answers the charge that there are no instances of progressive problem-shifts in economics by providing an example of one in his detailed study, 'Second Thoughts on the Keynesian Revolution' (1992). An assessment of Blaug's paper is thus in order.

Blaug chronicles the meteoric rise of Keynesian ideas in the 1930s and 1940s, as well as the equally rapid collapse of that research programme in the 1970s. He does a masterful job of documenting the multitude of factors responsible for the ascendancy of the Keynesian monolith (1992). Blaug notes that Keynesian theory appealed to those who had been radicalized by the Great Depression. Its comparative static presentation conveyed both a sense of rigorous simplicity and of determinateness. The theory fits nicely with categories already being developed by national-income statisticians. When compared with its rivals, it gave clear and directly applicable policy advice. The Keynesian message was delivered at 'the optimum level of difficulty'. And its fecundity and appeal to puzzle-solvers guaranteed that followers would have much work to do. Blaug corrects the still-popular myth that Keynes stood alone in calling for public works to counteract the Depression. About the only point that he neglects to mention was Keynes's flair for self-promotion.

In addition to all this, Blaug mentions that Keynes predicted a novel fact, indeed, a number of them (some of which later turned out not to be true). The chief novel fact was that the multiplier exceeds unity; this implies the prediction that government can spend us out of a recession, which the Second World War soon corroborated. Having cited all the other causes, Blaug settles on this as the decisive one: it was responsible for the quick successes of the Keynesian Revolution. And because it was *an empirical fact* that was corroborated the Keynesian Revolution represents a Lakatosian problem-shift. I wish to raise three criticisms of this account.

(a) It should first be noted that Mark Blaug is too good an intellectual historian to be anything more than a lousy Lakatosian. A good Lakatosian would have emphasized the novel fact in the text and relegated the rest of Blaug's fascinating discussion to the footnotes. This is not a mere quibble. Were historians to take Lakatos's advice seriously, the resultant history would look bizarre indeed: pages of footnotes would have to be appended to every paragraph of text. Alternatively, if the footnotes were left off, Lakatosian history would devolve into just another variant of monocausal explanation. Is the history of science written from the point of view of the discovery of novel facts any more substantial than the history of mankind written from the point of view of the class struggle? Is the causal nexus ever so neat and simple?

(b) Blaug demonstrates convincingly that Keynes produced a corroborated novel fact, that a progressive problem-shift occurred within the Keynesian research programme. But the rest of his excellent account raises doubts as to whether the Keynesian programme as a whole was progressive. In the last part of his article Blaug (1992) documents how the programme generated not one but a number of novel facts. Unfortunately, most of the rest of these facts turned out not to be true. Is a programme to be considered progressive if one out of a number of its novel predictions turns out right while the rest turn out wrong? This is important because one suspects that in most empirical sciences one is bound to stumble across an occasional corroborated novel fact. Isolated progressive problem-shifts will be found. Much rarer in economics are sustained periods of progress in which a series of corroborated novel facts are unearthed within a single research programme.[3]

Blaug seems to anticipate this kind of objection when he points out (1992) that many of Keynes's predictions, though incorrect, were none the less fruitful: they inspired much theoretical and empirical work, both in the immediate aftermath of the revolution and in more recent years. Most economists would consider this later work to be evidence of the richness of the Keynesian programme. It is less clear how it would fit into a straight Lakatosian account, however, except to provide more material for the footnotes.

(c) Blaug asserts somewhat provocatively that the novel fact predicted by Keynesian theory and corroborated by the war experience *was the most important factor* in persuading the profession of the truth of Keynesian ideas. His own rich, detailed account of the revolution provides the ammunition if one chose to pose the counter-argument: other factors were also present that might account for Keynes's quick success. But by emphasizing the significance accorded by economists to the discovery

> of novel facts, Blaug gets at a very important point. Unexpected empirical discoveries sometimes have a huge impact on the profession.[4] Where Blaug goes wrong is to attribute the size of the impact to the discovery of a novel fact.

Economists are sometimes swayed by the discovery of unexpected facts, but not in the way Lakatos imagines. The sorts of 'facts' that produce reactions among economists are broad historical confirmations or disconfirmations of a particular dominant world-view. The Great Depression (which challenged the efficiency-of-markets view) and the stagflation of the 1970s (which challenged the efficacy-of-demand-management view) are two excellent examples. Future historians will probably add the widely heralded 'collapse' of communist regimes in Eastern Europe to our list of paradigm-shattering episodes.

Such events give rise to large and sometimes extreme reactions among the various sciences which study social phenomena. These reactions are not well-described by models which emphasize the rationality of scientists responding to novel empirical evidence. Indeed, the ascendancy of the Keynesian programme and its desertion in the face of the stagflation of the 1970s illustrates convincingly just how coarse the evidence can be and still be capable of swaying virtually an entire profession. As Blaug demonstrates, a good Lakatosian would have to judge the Keynesian episode as a theoretically and empirically progressive problem-shift. It would count as *scientific progress*. But the actual history suggests that economists (and other social scientists) over-react to the sorts of disruptions which occasionally transform the social world. Bandwagons are jumped upon. A methodology which ends up characterizing such responses as scientifically progressive is fundamentally flawed.

On a more positive note, I think that progress *does* occur within economics. Progress takes many different forms, not all of them having to do with the empirical. But I would also agree with Mark Blaug's fundamental intuition that the most important sorts of progress within economics must be linked to the empirical. What the precise links are constitutes the sixty-four dollar question.

Blaug himself points out (1992) that the most common form of economic research involves trying to explain a known set of facts better. This usually occurs when a model is modified so that previously anomalous facts can be incorporated into it. If one employs the Popperian situational analysis framework, it is the description of the agent and of his situation which typically get modified. This is theoretical work, but it is guided by empirical anomalies. Judging by the reward structure of the profession, this sort of work is highly regarded by most economists.

I end this section with a conjecture. It seems to me that the ability to explain an ever-broader number of facts (by developing ever-more-general theories) typically also involves a diminution of one's ability to predict specific outcomes. In a way, and contrary to many expressly empirical methodological doctrines, progress in economics is often associated with the use of models of heightened explanatory power but of decreased predictive adequacy. As a result, it may be that the increases in knowledge that economists gain involve in a fundamental way a better understanding of the limitations of economics as a predictive science. Some may think it strange to characterize a better understanding of a discipline's limitations as a form of progress. But it is not an unprecedented view: Hayek certainly understood it very well. In any case, it is a view which cannot be easily discussed within the Lakatosian framework, nor within any framework in which progress is equated with increased predictive ability.

THE QUEST FOR UNIVERSAL CRITERIA OF APPRAISAL

Blaug's paper on the Keynesian revolution was a response to an earlier paper by Wade Hands. Hands has been a frequent critic of attempts to apply the MSRP in economics, and his exchanges with Blaug have been illuminating (Hands 1985b, 1990, 1991 a and b; Blaug 1976b, 1991, 1990a). But on at least one issue Hands (1985b: 2) agrees with Blaug, as may be seen from his assertion 'that economists should start not with Lakatos' methodology itself – i.e., not with his result – but with his questions, with the problem which originally motivated his methodology'. Hands suggests that though Lakatos's answers are less than satisfactory, the questions he pursued (namely, a search for specific criteria for appraising the progress of science) are the right ones. Both Blaug and Hands agree that coming up with a set of criteria for demarcating science from non-science and for appraising scientific theories as better or worse is a proper and important goal of methodology. They disagree only on how well Lakatos's MSRP fits the bill.

Why is the search for a specific set of criteria so important? There are some very good reasons. On the positive side, such a set of criteria, if ever attained, would make the job of methodologists much easier. And the goal seems a reasonable one. After all, most scientists believe that they know what science is, even if they are not able to come up with a list of necessary and sufficient conditions for identifying it. Astrology seems categorically different from astronomy, and creationist science seems categorically different from evolutionary theory. A universal set of criteria would not only allow one to distinguish pseudo-science from real science, it might even allow one to determine ahead of time which research programmes were most likely to lead to positive results and which were not.

Another good reason for coming up with such standards is to provide a buttress against the forces of relativism. This is a pressing problem in the current environment. As is well known, despite the efforts of many great minds the search for universal criteria of appraisal within the philosophy of science has so far yielded no success stories. This has led some to conclude that the quest is chimerical. It was mentioned earlier that the relativism in Thomas Kuhn's model of science was unacceptable to Mark Blaug. In the intervening years, other models for analysing science have been developed which make Kuhn's analysis seem absolutist in comparison. Some have responded to the failures of prescriptivist programmes by renouncing all prescription. Others have taken the failure to discover universal criteria of appraisal to imply that all criteria are equally valid, and the inability to measure progress to imply that sciences do not progress. Some have presumed that the absence of a demarcation criterion implies that science is no different from any other form of human activity. Others have concluded that the failure to come up with an unproblematical criterion of truth implies that truth is a meaningless concept. Most generally, these critics have taken the failures of prescriptivist philosophy of science to achieve its goals to imply that the philosophy of science should be replaced altogether by some other, non-epistemically based mode of inquiry. The doctrines of anti-foundationalism in philosophy and of deconstructionism in literary criticism have begun to make inroads into economics. Some economists have urged that we turn to fields like the sociology of science or rhetoric the better to understand theory choice, and often their arguments begin from the assumption that epistemology is a failed academic enterprise. The quest for standards embraced by methodologists like Mark Blaug and Wade Hands is meant as an antidote to these recent trends.[5]

CRITICAL PLURALISM AS THE MIDDLE GROUND

But it seems to me that there is another alternative, one which I view as a middle ground between relativism and the quest for universal criteria. The position that gradually emerged in a series of works (for example, Caldwell 1982, 1986, 1988, 1989) is one that I labelled critical pluralism. It is not a full-fledged epistemological position, but neither is it so vague that it cannot be articulated. The major tenets include:

1. A disavowal of quests for a single demarcation criterion or for universally applicable criteria of theory appraisal.
2. An emphasis on criticism: the role of the methodologist is to assess the strengths and weaknesses of various research programmes.
3. An objectivity constraint: in reconstructing a programme and its methodological content, one should try to give it its strongest possible portrayal.
4. An insistence on viewing all criticism as problem-dependent. The content of the criticism will depend on the sorts of problems the programme seeks to answer. A programme could be found to be adequate for the solution of certain problems and inadequate for the solution of others.
5. Finally, the critical pluralist values novelty. Though criticism is a key, new programmes should be encouraged to flourish and permitted a grace period in which they are not severely criticized. One a programme is established, though, the critical process begins.

I view critical pluralism as providing a rough set of guidelines for economic methodologists to follow. The sort of writers whose works I think are broadly supportive include a number of Popperians: Popper himself in his critical rationalist writings (this is why whenever I criticized Popper I always talked about the 'falsificationist Popper'); Kurt Klappholz and J. A. Agassi (1959) (reading this article first set me on the path towards critical pluralism); Larry Boland (1982); and Bill Bartley (1984). I would also include, somewhat paradoxically, the early Lakatos, the author of *Proofs and Refutations* (1976). Finally, certain people whose work lies outside of the Popperian tradition would also be included, especially Larry Laudan (1977), whose detailed discussion of science as a problem-solving enterprise is wholly compatible with critical pluralism.[6] That such different writers can provide inspiration indicate just how eclectic critical pluralism is.

Though I view critical pluralism as a middle ground, there is no time to offer an extended demonstration of how it is an improvement over both extremes. The focus of this paper has been on those who endorse a specific normative role for methodology. Continuing that theme, I shall offer two arguments, one practical and the other epistemological, for why accepting critical pluralism makes more sense than trying to resuscitate Lakatos (Blaug's route) or holding out the hope that some day a set of criteria of appraisal or of demarcation may be discovered (Hands's route).

TWO ARGUMENTS AGAINST THE QUEST FOR UNIVERSAL CRITERIA

(a) To make the practical argument, let us begin with an analogy. You are a professor and a student who is flunking your course comes into your office for a conference. What course of action do you recommend to the student? Drop the course and focus on doing better in other classes? Get a tutor? Improve attendance? Read more of the assigned materials? Improve note-taking? Join a study group? Another question is, what sort of attitude do you display towards the student? Concern? Sympathy? Encouragement? Or do you play 'tough cop', pointing out the student's reprehensible attendance record, poor attitude, laziness or absence of good study skills? Or are you coolly objective, stating simply but precisely what the student's situation is and laying out the options that are available? These are tough questions. And there is a further, distinctly methodological, question in all this, namely: how does one go about answering these questions?

Too often, of course, we as professors answer them according to our own moods, either our general dispositions or our moods as they happen to be on the day of the conference. For example, those whose time is short (or who do not want to get involved with students generally) remain coolly objective. Those who are having bad days (or who believe that most students are lazy) are indifferent or recriminating. And those who suddenly decide to make a difference in a student's life (or who are possessed with perpetual missionary zeal) are encouraging. If there is no feedback from students, such professors might continue their behaviour for years. It takes a severe anomaly (for example, a student suicide or a classroom revolt) to get them to question their procedures.

This sort of professor is analogous to economists who are not interested in methodology and who unselfconsciously practice the methods they learned in graduate school. They assume that those methods are the correct and scientific ones, and that identifying science is easy. They only turn to methodology when something happens to call all such beliefs into question: a major policy failure, for example, or the desertion of the paradigm by a number of prominent advocates.

Another way to answer these questions is to try to come up with some sort of optimal response, a set of instructions and a way of dealing with students which are universally applicable. The response might be based on case studies of past student successes and failures, or empirical data collected by departments of education on the prevalence of various student 'types' in the population, or on psychological studies of motivation, or (most likely) on some intuitive notion of what works. But the goal is to come up with an optimal response.

This, I suggest, is the analogue of the type of programme which Hands and Blaug are pursuing. They seek a universal set of criteria for demarcating and appraising scientific theories. This is not to suggest that they are inflexible about how such standards are to be applied. They may even be prepared to grant that the standards can change through time; for example, that the observation of practice can lead to 'corrections' of a particular set. But the *goal* is to find specific and universally applicable standards.

A third way to answer the question is to find out more about the student's problem situation, to begin asking the student questions, and to focus intently on what the student has to say. The goal of this process is to find the *source* (or sources) of the student's problem. Once the sources are identified, the number of appropriate responses are narrowed down considerably. Crucially, it will become apparent through this process that *some responses are better than others*. But as to which *particular* responses are best, that decision cannot be made until one specifies the student's problem: our responses are *problem-dependent*. Note finally that students with different problems may require responses which are *diametrically opposed*: for example, one encourages students with self-image problems and one confronts those with bad study habits. There is no room for such contradictory advice within a universalist framework.

This is the way of the critical pluralist. The critical pluralist is sensitive to the diversity of science, just as a good professor is sensitive to the diversity of students. The goals of the pluralist (make theories better, or get students to perform better) are the same as those of his universalist colleagues. But his problem-dependent methodological solution is very different. It is not a methodology of 'anything goes': standards are explicitly used. It is just that the pluralist refuses to announce the standards prior to the determination of the problem. Nor does the pluralist see much sense in trying to specify in advance the hundreds of types of problems that science might try to solve (or hundreds of types of students who might walk through his door). Case studies can be instructive and illuminating, but a methodology consisting of case law would be as dull as it was static.

(b) The analogy presented above was meant to suggest not only how difficult it is, as a practical matter, to come up with some universal set of specific criteria for demarcating and appraising theories, but also that such a quest may be inappropriate, given the diversity of science. But let us assume that the search culminates in the discovery of a set of criteria which are widely supported by the scientific and methodological communities. Even then, a question remains. This epistemological question is the one that is dear to anti-foundationalist philosophers as well as to non-justificationists like Popper and Bartley: How does one justify, or defend, or ground these criteria? Do we look at the history of science? If so, history as written from which interpretative framework? Do we consult the scientific élite? If so, which élite? Do we consult our pre-analytic intuitions? If so, whose intuitions? More generally, how, without recourse to what John Losee (1987: chs 5,6) dubs 'a principle worthy of inviolable status', can such a set of criteria be defended? How, without knowledge of what knowledge in the future looks like, are we to come up with criteria to distinguish what should count as knowledge in the future?

Critical pluralism takes these questions seriously, and perhaps that is a mistake. But given that the forces of relativism have grown rather strong of late it is important to formulate a response to them. One of the virtues of Popper's critical rationalism is that it is an attempt to meet the anti-foundationalist, the non-justificationist, on his own ground, and to show him that there still is a place for critical discourse. Critical pluralism follows Popper on this question.

It may be that Popper is unsuccessful in this venture, or that his thought leads to a dead end, or that, if successful, his success is bought at too high a price. It seems to me that his programme, though radical, is at least consistent, and it has the further virtue of focusing our attention on the questions the relativists pose. And it seems to me that if one insists, as I think Blaug and Hands do, that we must seek out a universal set of criteria, then one is at least obligated to address this conundrum explicitly and to try to offer a solution to it.

And perhaps such a solution will some day be forthcoming. I am prepared to be open and to pursue critical pluralism until that day. And if they agree that no such candidate is currently on the scene, I invite Professors Blaug and Hands to join me in the meantime in the middle ground provided by critical pluralism. It is not so uncongenial a place as they might imagine.

NOTES

1. Samuelson's borrowings from philosophy have been criticized by Caldwell (1982), and his use of analogies from physics has been attacked by Mirowski (1989). Ironically, though the value of his explicitly methodological writings have been called into question, Samuelson's *practice* of economics has obviously

had a profound effect on the actual methods followed by economists. Machlup's methodological articles are collected in Machlup (1978).

2. Of course, one might conclude from this that economics is not a science. This option is not open to a Lakatosian economist, however, who is forced to judge the MSRP as deficient if it cannot rationalize the history of his discipline.

   As an aside, if we use another demarcation criterion (namely, the presence of causal arguments) to determine whether a discipline is a science, the literature on folk psychology raises some serious doubts concerning the scientific status of many of the social sciences (Rosenberg 1988). I think that much might be gained if the term 'social science' was changed to 'social studies'. By denying such studies scientific status, the hubris of many practitioners would be reduced. For their part, philosophers and methodologists would be forced to address the new and more difficult question of what is worthwhile in social studies rather than to continue mouthing the (rather easily demonstrable) proposition that the social sciences are in various ways inadequate. Perhaps best of all, since acronyms would be unaffected, such a move could be accomplished economically.
3. A framework provided by Larry Laudan (1977) may be a better vehicle for discussing the Keynesian Revolution. Laudan's crucial move is to uncouple the notions of progress and truth, which has important implications for the assessment of the Keynesian programme. For example, at the time of its development the Keynesian Revolution was viewed as progressive by scientists. And as Blaug points out, even in retrospect the programme was progressive in terms of its fecundity. Laudan's model would allow one to judge the Keynesian programme as progressive even though it was later recognized that certain of the claims of the programme were not true.
4. Obviously, sometimes empirical anomalies are simply ignored. The ubiquitous presence of preference reversals has had little impact on textbook presentations of choice theory, for example.
5. In the opening chapters of his book on rhetoric, Donald McCloskey (1985) grounds his approach, at least in part, on the anti-foundationalist philosophy of Richard Rorty. This is why many methodologists, most of whom one assumes would otherwise be sympathetic to McCloskey's general message, reacted critically to the rhetoric programme. It is ironic that McCloskey (by grounding his work on the writings of anti-foundationalists) provoked the ire of those who insist on foundations and who therefore oppose the philosophy of anti-foundationalism!
6. One reason Laudan is able to discuss problem-solving in so detailed a way is because of his willingness to disengage the notion of progress from truth. Popper and Popperians are not prepared to do this. As a result, although they emphasize that science solves problems and that progress occurs when problems are solved, Popperians seldom extend the analysis of problem-solving much further. If problems are solved only by the discovery of the truth, there is not much more to say.