Explorations in Economic Methodology

From Lakatos to empirical philosophy of science

Roger E. Backhouse

Routledge Frontiers of Political Economy

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Explorations in economic methodology

Economics is an established academic discipline, yet its methods and style of argumentation are far from being well understood. In recent years attempts have been made to understand economics through applying ideas from the philosophy of science (especially from the writings of Popper, Kuhn and Lakatos) and through the study of economists' rhetoric. The result has been intense controversy, with some participants arguing that the study of methodology is a fruitless exercise.

Roger Backhouse has been an active participant in the controversy over economic methodology. This collection of his essays both clarifies and responds to the issues raised by the literature and argues that methodology is an essential activity. The book begins with an application of Lakatos's methodology of scientific research programmes to contemporary macroeconomics and subsequent chapters go on to discuss questions raised by this approach. These argue that although the methodology has severe limitations, it nevertheless provides a useful starting point. After discussing the approaches to methodology of some practising economists, the final chapters consider the perspectives on economics that result from pragmatism and empirical philosophy of science.

Clarifying the issues involved, and outlining a constructive but critical response to the recent literature, this collection will be of interest to students and researchers interested in economic methodology and the philosophy of science.

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Chapter 1

Introduction

1 ECONOMIC METHODOLOGY[1]

Dating the emergence of new disciplines and sub-disciplines is often problematic. In the case of economic methodology, however, it is relatively easy. The volume *Method and Appraisal in Economics,* edited by Spiro Latsis (1976), marked a break with most earlier methodological discussions, and played a role second only to Mark Blaug's *The Methodology of Economics* (1980/92) in establishing economic methodology as an identifiable discipline involving economics, philosophy, and the history and sociology of science.

Method and Appraisal in Economics boasted a distinguished list of contributors, each of whom explored the relevance of Lakatosian ideas to economics. What marked such work off from previous methodological literature was that it focused on the dynamics of the subject – opening up new ways of thinking about how economics had developed over time. In doing this, it opened up the possibility of resolving long-standing puzzles about how the discipline worked. Because it comprised a series of case studies, it was possible for an economist to get interested in the book without any prior concern with abstract methodological ideas. In contrast, the earlier literature on economic methodology, outstanding as some of it was, seemed ahistorical and to be missing something vital. Robbins (1932/35), Hutchison (1938), Friedman (1953) and others seemed, in comparison with the contributions to the Latsis volume, to be offering over-simplified pictures of the subject.

It was, though, Blaug's book that played the major role, defining economic methodology as a sub-discipline of economics. It was, however, more than simply a textbook: it placed philosophy of science up front, and established an agenda and

[1] I tell this story in more detail in Backhouse (1994a).

a challenge. Economists, Blaug argued, generally practised what he termed ‘innocuous falsificationism’, but they should stop doing so. Economics would progress much more quickly if they would take falsificationism seriously. Whenever serious attempts had been made to test economic theories, Blaug contended, the result had been progress. The result was that economic methodology came to be centred on Popperian and Lakatosian ideas. Though Caldwell (1982) argued for a different conclusion (methodological pluralism), he and Blaug were addressing the same issues. The two books reinforced each other in establishing the issues to be addressed.

During the subsequent decade, however, interest in Lakatos waned for a number of reasons. The first was that, as people sought to investigate the Popperian and Lakatosian methodologies and to apply them to economics, they encountered problems. It became clear that they did not provide any magic formula for revealing what was going on in economics. The second reason was the increasing popularity of rhetorical, literary, sociological and other ‘postmodern’ analyses of economics. The key work here was Deirdre (formerly Donald) McCloskey’s ‘The rhetoric of economics’ (1983) and her subsequent book of the same title (McCloskey, 1986), in which she argued that the idea of ‘Methodology’ was misconceived, and premised on an out-moded and philosophically indefensible ‘modernist’ view of the world. Finally, but perhaps more important in the long term, was the increasing awareness amongst specialists on economic methodology of issues in the philosophy of science that extended beyond those raised by Popper, Kuhn and Lakatos. This came about for two reasons. One was that economic methodologists, typically self-taught in philosophy, became aware of wider issues. The other was that a number of philosophers took a serious interest in economics, and showed that much could be learned by approaching the subject from perspectives other than falsificationism.

The result was that, by the end of the decade, Popperian and Lakatosian methodologies had fallen out of fashion. Weintraub abandoned the Lakatosian perspective of *General Equilibrium Analysis: Studies in Appraisal* (1985) in favour of writing ‘thick’ history, inspired by ideas from literary criticism and the sociology of scientific knowledge (Weintraub 1991). Though he continued to see some merit in the Popperian tradition, Hands moved progressively further from falsificationism (see Hands, 1993). De Marchi increasingly played down Popper and Lakatos, in favour of what he termed ‘recovering practice’ (de Marchi, 1992). Rosenberg (1992) and Hausman (1992) produced major studies of economics that took owed nothing to Popper or Lakatos. At the Capri conference on research programmes in economics in 1989 (see de Marchi and Blaug, 1991), supporters of Lakatosian methodology were a beleaguered minority.

2 EXPLORATIONS IN ECONOMIC METHODOLOGY

This is the background against which the essays contained in this volume were written. Like other economists who came to economic methodology at this time, my entry was via Latsis (1976) and Blaug (1980), both, I recall, found accidentally whilst browsing through new acquisitions at a local bookstore soon after their publication. The possibility that it might be worth undertaking more serious study of methodology, however, did not occur to me. Instead, I concentrated on research in macroeconomics and, later, the history of economic thought. Though I could not resist bringing Popper, Kuhn and Lakatos (along with three all-too-brief paragraphs on sociology of science) into *A History of Modern Economic Analysis* (1985), I played this down, the result being a book which, apart from a few sections, rests on no explicit philosophical position. In retrospect, I am surprised at the sceptical attitude towards Kuhn and Lakatos adopted in that chapter.

> One approach to the history of economic analysis would be to appraise economic ideas in terms of a particular methodology, taken from the philosophy of science. Kuhnian paradigms, or Lakatosian research programmes could be identified and the story told in these terms. Such an approach is not without value, but it begs the question of how far the methodology chosen is appropriate for economics. Suppose, for example, that it turned out that little of the history of economic analysis could be fitted into Lakatos's methodology of scientific research programmes. One possible conclusion would be that the history of economics should be judged adversely. Alternatively, it would be possible to draw the conclusion that the methodology was simply inappropriate for economics.
>
> There would be problems, too, should the methodology explain everything. Would some other methodology, such as Kuhn's paradigms, have performed equally well? Were the criteria for testing the applicability of the methodology sufficiently stringent for the results to mean anything? For example, if economics is divided up into chunks, each of which is to be tried out as a Kuhnian paradigm, or as a Lakatosian research programme, there are many ways in which we might divide it up. We might take the whole of economic inquiry since Adam Smith as one unit. We might separate classical economics,

> marginalist economics and Keynesian economics. Dividing still further we might consider episodes such as the post-Marshallian theory of the firm, or neo-classical growth theory. 'Verification' of a methodology ought to be easy, as there are so many possible ways of applying it.
>
> Why not, then, abandon the philosophy of science altogether? Firstly, the philosophy of science does provide useful concepts and ideas, and it suggests questions which are worth asking. Even though we may conclude that, for example, the marginal revolution does was not a scientific revolution in Kuhn's sense, we may learn something in the process of coming to this conclusion. Secondly, though extreme caution must be applied in doing this, the history of economic analysis is useful for evaluating alternative methodologies. If economists have not followed what appear to be sound methodological principles, there may be a good reason why not (there may, of course, be less respectable reasons too).
>
> (Backhouse, 1985, pp. 9–10)

It was in the late 1980s that I turned more seriously to methodology. I started with the attempt to apply Lakatos's methodology of scientific research programmes to the development of macroeconomics since Keynes (reproduced as Chapter 2), finding that it fitted surprisingly well. By this time, however, Lakatos's methodology of scientific research programmes had become unfashionable. At the Capri conference, Mark Blaug and I sometimes seemed the only people still prepared to take the Lakatosian project further. My response to this was to think much more carefully about Lakatos, the result being the other essays contained in Part I.

The perspective from which all the papers in Part I were written is that though there are problems with applying Lakatos's methodology of scientific research programmes to economics, it none the less provides a useful framework for thinking about certain methodological issues. Rejection of Lakatos has often been associated with rejecting some important issues that are addressed in his methodology: empiricism (loosely defined), the importance of the growth of knowledge' and the tension between appraising economic ideas and at the same time learning from what economists actually do. Even if Lakatos's methodology of scientific research programmes has to be left behind, these should not be thrown out with it.

This conviction that the Popperian–Lakatosian methodology was right in certain key respects, combined with considerable scepticism about the details, explains the way I responded to 'anti-positivism' or postmodernism when it entered the debate over economic methodology. The stimulus was when Roy Weintraub came to Bristol to present an early version of ‘Methodology doesn’t matter, but the history of thought might’ (Weintraub, 1989). It immediately struck me that if one started from a Popperian position, his arguments were of no consequence, for Popperian methodology was based on the premiss that nothing was known with certainty. It was in responding to this paper that I started to analyse McCloskey’s critique of methodology, which overlapped to a considerable extent with Weintraub’s. However, whilst I was convinced that McCloskey’s and Weintraub’s anti-methodological positions were misconceived, and even inconsistent, I became convinced that rhetorical and other related perspectives could shed new light on what was going on within economics. The four essays in Part II make the case that hermeneutic, rhetorical and postmodern analysis have important points to make, but that they need to be treated with the same level of criticism as that to which traditional methodological approaches have been subjected. It is possible to learn from, say, rhetorical analysis, without abandoning the view that the methodological basis for economics needs to be analysed and criticized.

Part III contains responses to a variety of ‘economists’ writing on economic methodology. The word ‘economist’ in this context means someone whose main concern remains to contribute to economics, not to reflect on economics. Thus where McCloskey and Weintraub have chosen to specialize in reflecting on economics, Harcourt, Hahn, Krugman, and Kindleberger, though they have engaged in much methodological reflection, have been concerned primarily with *doing* economics. Their writings, however, all raise important methodological issues. Two themes predominate in these chapters: the merits of a pluralist, eclectic approach to economic theorizing, and the conditions most likely to produce progress in economics. These are related, in that the advocates of an eclectic approach argue that it results in insights that would otherwise be lost. Harcourt and Hamouda, in the survey of post-Keynesian economics discussed in Chapter 10, argue for a ‘horses for courses’ approach, whilst Kindleberger, the subject of Chapter 12, claims that economists should use a variety of tools, no single one being suitable for all problems. These chapters point out that, although the existence of a variety of perspectives and approaches to economics can stimulate fruitful enquiries that would otherwise not have taken place, there is another side to the coin: if ideas are to be tested and developed, a framework is needed. Neoclassical economics, though it has great problems, does provide such a framework.

Of the remaining chapters in Part III, Chapter 11 addresses Hahn's claim, which would be echoed by many economists, that methodological discussions should be avoided as they distract attention from more serious work, and can be harmful. The argument used to counter this claim is that economists make methodological choices all the time, many of them remaining implicit. It is important that they are made explicit and subjected to critical analysis. Chapter 14 views the methodology of Schumpeter's *History of Economic Analysis* (1954) in the light of Kuhn's *Structure of Scientific Revolutions* (1962/70). The two have much in common, but differ in key respects. It is argued that a possible explanation of these differences is that Schumpeter reached different conclusions from Kuhn because, as an economist, his methodology was tailored to fit economics as he saw it. It can, to this extent, be seen as an example of empirical philosophy of science.

This leads into Part IV, 'Pragmatism and empirical philosophy of science'. Chapter 15 uses Peirce's terminology to investigate why economists disagree by turning the question round: how do economists manage to resolve disagreements amongst themselves? It focuses on the question of how econometric or other empirical evidence can be used to resolve disputes, and why it is not more effective in doing so. Several reasons specific to economics are suggested. After this, there follow three chapters on philosophical studies in which methodological conclusions are drawn from a close examination of what practising economists actually do – examples of empirical philosophy of science. These are Hausman's *Inexact and Separate Science of Economics* (1992) and Kincaid's *Philosophical Foundations of the Social Sciences* (1996). In these chapters the theme is that, whilst Hausman and Kincaid are right in adopting an empirical approach to methodology, in many of the conclusions they reach about the nature of contemporary economics, and in some of the recommendations they make to improve the discipline, neither of them focuses on quite the right place. Both are too concerned with economic theory, and not enough with empirical economics. Thus though their characterizations of economic theory are revealing, they are less good on empirical economics.

3 CONCLUSIONS

The essays brought together in this volume, the text of which has been left as it was when they were first published,[2] document a journey that starts with an attempt to appraise economics from within the perspective provided by Lakatos's methodology of scientific research programmes and has to date ended up, in *Truth and Progress in Economic Knowledge* (Backhouse, 1997) with something that is perhaps best labelled (if such a label is required) as an amalgam of pragmatism and empirical philosophy of science. In the essays reprinted here, several themes recur. The first is that the distance between Lakatosian methodology and empirical philosophy of science is much less than most contributors to the literature have suggested. The second is the importance of not taking arguments further than is justified. Perhaps in order to make the ideas look more original and more dramatic than they are, rhetoric and postmodern criticisms of economic methodology have been oversold, and as a result false choices have been presented. Here I make this point in the context of methodological discussions. It can, of course, be applied to economic arguments as well, whether theoretical or empirical. The third is the importance of paying attention to empirical economics, both econometrics and the accumulation and processing of economic data, whether statistical, historical or institutional. To focus on economic theory severely distorts both our perspective on economics and any methodological conclusions drawn.[3]

In an essay that sought to counter any misunderstanding of his position, Mark Blaug (1994) described himself as an 'unrepentant Popperian'. Like Blaug, I see one of the tasks of methodology as being to contribute to a debate over what type of economics should be undertaken if the discipline is to progress and yield better solutions to the problems with which it is confronted. Such a goal involves being prepared to engage in prescription as well as description, and it involves looking in detail at what economists are actually doing, establishing what rationale exists for those practices. Like Blaug, I see Popperian methodology (along with Lakatosian methodology) as a more appropriate starting point than either the logical empiricism that dominated methodological discussions in the 1950s or the postmodemism that has become fashionable since the early 1980s. It leads naturally into a focus on the nature of progress in economics (theoretical and empirical) and on the conditions

[2] Apart from the updating of references, the correction of typographical errors, minor stylistic amendments, an (italicized) introduction to each chapter, and very occasional additional material in footnores (marked with square brackets). Any remaining differences are unintentional.
[3] This is also the theme of Backhouse (1994b).

that foster such progress. Thus, in so far as Blaug's Popperianism is a way of articulating a tough-minded empiricism and a willingness to ask critical questions of economics – which explains his enthusiastic endorsement of Mayer's *Truth versus Precision in Economics* (1993) – I agree with him wholeheartedly.

Why, then, do I hesitate to describe myself as an 'unrepentant' Popperian? One reason is that, as Hausman (1992), Hands (1993) and others have pointed out, there are technical problems with Popper's methodology. It can be argued that once it is modified to accommodate, for example, an element of induction, little is left that is uniquely Popperian. Another reason is that there are other starting points from which tough, critical empiricist positions can be reached, one of these being pragmatism. A final reason is that emphasizing the Popperian connection runs the risk of playing down the importance of looking closely at the what economists do, with a view to establishing whether or not there are good reasons for what they do. Contrary to what Blaug suggests, 'recovering practice' does not necessarily amount to abandoning the attempt to criticize contemporary economics. It is perhaps even dangerous if those who wish to take a critical view of economics describe themselves as Popperians, for it makes it much easier for critics to dismiss them as not having faced up to the problems with Popper's methodology. On the other hand (and here I agree wholeheartedly with Blaug), abandoning the labels does not mean that the concerns that motivated economic methodology's involvement in Popperian and Lakatosian methodology in the late 1970s and early 1980s should be abandoned. They remain as important now as they ever were.

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Part I

Rethinking Lakatos

Chapter 2

The neo-Walrasian research programme in macroeconomics*

(*Appraising Economic Theories: Studies in the Methodology of Research Programmes,* edited by Neil de Marchi and Mark Blaug. Cheltenham and Brookfield, VT: Edward Elgar, 1991, pp. 403–26)

The origin of this chapter was an invitation to contribute a paper on the evolution of post-war macroeconomics to a conference in Leuven, in 1988. In an attempt to provide a novel twist to the story, I tried to tell it in Lakatosian terms, applying Roy Weintraub's idea of a neo-Walrasian research programme. On a visit to England, Weintraub pointed out that I had not succeeded, and explained to me how it should be done. I presented the resulting paper at the History of Economic Thought conference in Bristol that year, where Mark Blaug heard it and invited me to present a revised version of it at the conference in Capri in 1989. It remains the only paper in which I have worked entirely within Lakatos's methodology of scientific research programmes. It is because it is the key to explaining the origin of the other chapters in this volume that it is reprinted here, despite having been reprinted in Backhouse (1995).

The starting point of the chapter is that if Weintraub's idea of a neo-Walrasian research programme were to be applied to macroeconomics, it clearly had to be modified. Assumptions like 'agents have full relevant knowledge' and 'economic outcomes are co-ordinated' are clearly untenable when discussing a branch of economics in which uncertainty and co-ordination failures have been central concerns. I saw the resulting modifications to the neo-Walrasian hard core as

* This chapter is a greatly revised version of part of a paper presented to a seminar on 'Post War Economic Thinking and its Relevance for Policy' at the Katholieke Universiteit, Leuven, in May 1988. I am indebted to the participants in this seminar, and to Mark Blaug, Tony Brewer and Neil de Marchi for helpful comments on various drafts of this chapter. Special thanks are due to Roy Weintraub whose extensive criticisms and invaluable advice led me to modify the original draft very substantially. Needless to say, none of them should be held responsible for the use I have made of their ideas.

very marginal, entirely in keeping with the spirit of Weintraub's original programme. This chapter, therefore, could be seen as corroborating Weintraub's interpretation of general equilibrium theory through showing that it could successfully explain developments in a branch of economics that it had not been designed to explain. This is, of course, one version of Lakatos's appraisal criterion – the successful prediction of novel facts – but used as a criterion for judging the methodology.

Though the modifications made to Weintraub's neo-Walrasian research programme were in one sense very minor, they made it far clearer that it was defined solely by its research strategy, not by any hard-core assumptions about the economic world. 'Construct models in which agents have a well-defined set of information about relevant phenomena' is much more clearly a methodological statement than 'Agents have full relevant knowledge', even though in many contexts they amount to the same thing. The same is true of 'Specify model-specific meanings of equilibrium'. It is this shift towards defining the neo-Walrasian research programme purely in terms of a particular methodology that accounts for why it fits so much of the historical record. Even so, it leaves much unexplained.

Though I would now be a little more sanguine about the merits of reading economics through the lens of Lakatos's methodology of scientific research programmes, it remains, I suggest, a valuable exercise. The list of successfully predicted novel facts may not be as long as one would hope, but neither is it an insignificant one.

1 THE PROBLEM

The notion that contemporary macroeconomics can be viewed as part of a 'neo-Walrasian' research programme or something very similar is hardly novel or, perhaps, very controversial. The desire of contemporary macroeconomists to ground their theories in individual optimizing behaviour and to have a coherent microeconomic foundation for what they do, the main characteristic of neo-Walrasian economics, is almost too obvious to require comment. The issue of how far contemporary macroeconomics can legitimately be viewed as part of such a Lakatosian scientific research programme and the process whereby this came about have, however, never been properly investigated. The purpose of this chapter is to make a first attempt at filling this gap.[1]

[1] Most accounts of macroeconomics since Keynes either discuss only a part of the story (e.g. Leijonhufvud, 1976; Weintraub, 1979; Gerrard, 1988) or discuss it in terms of monetarism and Keynesianism (e.g. Blaug, 1980/92, 1985; Backhouse, 1985). As I argue in Backhouse (1995,

The thesis put forward in this chapter is that mainstream macroeconomics since Keynes[2] can be viewed in terms of the extension of the neo-Walrasian research programme to encompass theories which previously lay outside its domain and to explain an increasing range of macroeconomic phenomena.[3] The starting point (in section 2) is the definition of the hard-core and heuristics of the neo-Walrasian research programme, together with an explanation of what it means to talk about a research programme's being extended to encompass theories developed outside the programme.[4] We then go on (in section 3) to provide a rational reconstruction of the history of macroeconomics since Keynes. After considering, in section 4, some of the issues involved, we discuss, in section 5, some of the novel facts predicted by the neo-Walrasian research programme. Section 6 tackles the all-important issue of whether or not the research programme is progressive. Conclusions are drawn in the final section.

2 THE NEO-WALRASIAN RESEARCH PROGRAMME

The best starting point is Weintraub's definition of the neo-Walrasian research programme (Weintraub, 1985, p. 109). He defines six hard-core propositions.

- HC1 There exist economic agents.
- HC2 Agents have preferences over outcomes.
- HC3 Agents independently optimize subject to constraints.
- HC4 Choices are made in interrelated markets.
- HC5 Agents have full relevant knowledge.
- HC6 Observable economic outcomes are co-ordinated, so they must be discussed with reference to equilibrium states.

(Continued from previous page)
chapter 8), such approaches are legitimate but they play down important aspects of the way macroeconomic theory, both Keynesian and monetarist, has evolved.

[2] Both post-Keynesian and 'Austrian' economics, for example, fall outside the scope of this chapter.

[3] In addition to the particular research programmes with which he was concerned, Lakatos recognized that science as a whole could be seen as one huge research programme (1970, p. 47). The neo-Walrasian programme proposed here clearly comes in between these two levels.

[4] The conventional way to view such issues is to see them in terms of the 'victory' of one scientific research programme over another. Though it might be possible to adopt such an approach it is not followed here: the difficult task of defining a 'Marshallian' research programme (or whatever research programme Keynes was working within) would, even if it were successfully pursued, distract us from the main task.

There are also positive and negative heuristics such as the following.

PH1 Go forth and construct theories in which agents optimize.
PH2 Construct theories that make predictions about equilibrium states.
NH1 Do not construct theories in which irrational behaviour plays any role.
NH2 Do not construct theories in which equilibrium has no meaning.
NH3 Do not test hard-core propositions.

This definition of a neo-Walrasian research programme was designed for analysing the evolution of general equilibrium theory, something Weintraub manages to do very successfully. If we are to analyse the progress of macroeconomics since Keynes in terms of the neo-Walrasian research programme, however, we need to define the programme a little more broadly. In particular, propositions HC5 and HC6 need to be changed. In addition, certain other heuristics, not stated by Weintraub, need to be made explicit.

The need to drop the assumption of 'full relevant knowledge' is obvious, for limited knowledge is a key factor in many present-day macroeconomic models.[5] Even models which assume rational expectations are not assuming perfect foresight. We could replace HC5 with a weaker hard-core proposition, but it seems better to replace it with an additional positive heuristic:

PH3 Construct theories in which agents have a well-defined set of information about relevant phenomena.[6]

Proposition HC6 is more difficult, for a number of reasons. In the literature Weintraub was concerned with, equilibrium was a clearly defined concept. In macroeconomics, on the other hand, there are two problems with the concept of equilibrium. One is that the term 'equilibrium' is used in a number of different ways. The other is that much macroeconomics has been concerned with disequilibrium, and with the failure of the market to co-ordinate economic activities. It would, of course, be possible to

[5] It can be retained, but only at the cost of reinterpreting 'relevant' in such a way as to deprive it of any meaning. Such a strategy fails to capture the two crucial characteristics concerning assumptions about knowledge in neo-Walrasian models: that agents must be in a position to be able to act rationally; and that assumptions must be such as to render formal mathematical analysis possible.
[6] It could be argued that HC5 used to belong to the hard core of the neo-Walrasian research programme, but that it was dropped, being replaced with PH3. This change represented a progressive problemshift. It is discussed further below.

define the terms 'equilibrium' and 'coordination' in such a way as to render HC6 a hard-core proposition for mainstream macroeconomics, but it seems preferable to replace it with something less misleading. Finding an acceptable alternative is difficult, for proposition HC6 covers two aspects of neo-Walrasian economics: the notion that markets co-ordinate the actions of different individuals and the idea that we should, in our theories, allow for the effects of market interactions working themselves through. The best solution is simply to drop HC6. The first aspect of HC6 is already covered by HC4. The second aspect can be picked up by modifying PH2 (which has to be modified anyway for the same reasons as we have to drop HC6). A suitable modification is to replace HC6 with the following:

PH2* Specify the model-specific meanings of equilibrium and disequilibrium and analyse the model in terms of these.

One of the characteristics of neo-Walrasian economics, which distinguishes it from much earlier economic writing, is that formal models are used. In his appraisal of general equilibrium theory it was not necessary for Weintraub to make this explicit: the whole literature took this for granted. In macroeconomics, however, this is not the case, many economists having worked with much more rough and ready methods. To define the neo-Walrasian research programme properly, therefore, we need to add to the above list of positive heuristics.

PH4 Construct fully specified, consistent models, simplifying where necessary in order to be able to do this, and draw only those conclusions which can be proved to be implied by the models.

PH5 Specify the rules governing the interaction of agents (in terms of game theory, make the game explicit).

In many cases PH5 implies competition, but it is obviously more general. Some equilibrium element is of course implied by the assumption that agents optimize: in this limited sense, therefore, equilibrium is implied by HC2 and PH1.

If we accept these modifications to Weintraub's definition of the neo-Walrasian research programme we have a programme with the following hard core and positive heuristics (the negative heuristics are exactly the same as for Weintraub). Note that the positive heuristics have been placed in a more sensible order and have been renumbered (subsequent references all refer to this new list).

HC1	There exist economic agents.
HC2	Agents have preferences over outcomes.
HC3	Agents independently optimize subject to constraints.
HC4	Choices are made in interrelated markets.
PH1'	(PH4) Construct fully specified, consistent models, simplifying where necessary in order to be able to do this, and draw only those conclusions which can be proved to be implied by the models.
PH2'	(PH2*) Specify the model-specific meanings of equilibrium and disequilibrium and analyse the model in terms of these.
PH3'	(PH1) Construct theories in which agents optimize subject to constraints.
PH4'	(PH5) Specify the rules governing the interaction of agents (in terms of game theory, make the game explicit).
PH5'	(PH3) Construct theories in which agents have a well-defined set of information about relevant phenomena.

This is slightly, but significantly, broader than the research programme suggested by Weintraub and it can be used to make sense of the main developments in mainstream macroeconomics since Keynes. General equilibrium theory, as analysed by Weintraub, can easily be seen as a part of this research programme. Overall the research programme presented here has a smaller hard core and a longer list of positive heuristics than Weintraub's: in other words, we are placing a greater emphasis on the neo-Walrasian research programme as a research strategy rather than a set of assumptions about the world. It has to be said, however, that the differences are very slight. Whether we use the term 'neo-Walrasian' to denote the broader programme suggested here, or the 'sub-programme' described by Weintraub, is essentially arbitrary and is of no importance.

Finally, before going on to consider the history of macroeconomics we need to consider the notion that a scientific research programme may be extended to encompass *theories* that previously lay outside its domain, for this is not the same as that of a research programme dealing with anomalies. The idea is that, possibly quite apart from any anomalies, theories may have been developed outside the research programme (they may have been developed within another programme) which can be used and developed in accordance with the programme's positive heuristics. If this happens it is quite possible that theories may be taken over before economists have been able to establish whether they are or are not consistent with the programme's hard core.[7] The theories concerned may contain anomalies, the hard core being protected from these in the usual way, or it may be that the question of consistency with the hard core has, temporarily, been left open. The work needed to do this may be postponed for several reasons: (a) a desire to tackle problems one at a time; (b) an absence of suitable techniques for tackling certain problems; (c) a desire to try out theories before being convinced that it is worth finding out whether or not they can be integrated into the research programme.

3 A RATIONAL RECONSTRUCTION OF MACROECONOMICS SINCE KEYNES

Phase I: The establishment of the neo-Walrasian research programme

Keynes's *General Theory* was certainly not part of any neo-Walrasian research programme. In particular his theory was based on certain 'psychological laws' which had no grounding in optimizing behaviour. Assumptions HC2 and HC3 were not a part of Keynesian economics. Similarly Keynes did not follow the heuristics detailed above. On the other hand, although Keynes himself was reluctant to use his theory in this way, it was only a short step from the *General Theory* to constructing a formal model of the economy as a whole, for it contained all the necessary simplifications. In addition he defined a new concept of equilibrium appropriate to his model. This achievement was succinctly summed up by J. M. Clark who wrote, in a letter to Keynes,

> It has seemed to me that what I call the 'income-flow analysis', of which yours is the most noted presentation, has done something which has not been done in comparable degree since Ricardo and Marx: namely, constructed a coherent logical theoretical system or formula having the quality of a mechanism, growing directly out of current conditions and problems which are of paramount importance and furnishing a key to working out definite answers in terms of policy.
>
> (Keynes, 1971–83, XXIII, p. 191)

The first stage in the extension of the neo-Walrasian research programme to cover macroeconomics was to take Keynes's simplifications and, following PH1' and PH2', to construct from them a coherent, formal model of the economy. This was the achievement of Champernowne (1936), Harrod (1937), Meade (1937) and Hicks

[7] The question of whether the same thing happens in other disciplines is left open.

(1937). In the process of doing this they redefined the notion of equilibrium, expressing it in terms of simultaneous equations (absent from the *General Theory*).[8] The remaining positive heuristics were, at this stage, ignored. The main reason for this was that the most important task was to show that, assuming it could eventually be reconciled with the programme's hard core, the new theory could be used to solve interesting problems: that it constituted a progressive problemshift. This is one of the reasons why Hicks's contribution was so important: although all four worked with what was essentially the same set of equations, it was he who showed, in a way that Champernowne, Meade and Harrod did not, how the new mathematical apparatus could be used to solve the riddle of how Keynes's theory related to that of the classics.

The next stage in establishing the neo-Walrasian research programme was to follow up PH3' and to replace Keynes's somewhat arbitrary (by neo-Walrasian standards) behavioural functions with ones securely based on optimizing behaviour. This took place in the 1940s and 1950s. The main work involved the two most novel aspects of the *General Theory* – the consumption function and the demand for money – and includes the well-known contributions of Friedman, Modigliani, Baumol, Tobin *et al.*[9] The third major component of Keynes's theory, his marginal efficiency of capital, also received attention, though this presented less of a problem as it was believed to correspond fairly closely to traditional concepts thought quite consistent with optimizing behaviour.

At the same time as detailed work was being carried out on the component parts of the Keynesian system, work was also being undertaken on the explicit integration of such optimizing models into a formal model of the economy as a whole, the most important work here being Patinkin's *Money, Interest and Prices* (1956).[10] Earlier

[8] This process is discussed in detail in Young (1987).

[9] These are too well-known for exact references to be required.

[10] There is a problem in knowing how to fit *Value and Capital* (1939) into this story. On the one hand Hicks has claimed that his IS–LM paper and *Value and Capital* grew out of the same earlier work (for references see Young, 1987, p. 46). On the other hand, *Value and Capital,* surprisingly, contains no reference to the IS–LM paper. It is worth noting the observation made by Maes (1988) that Hicks, although he had developed a portfolio theory of money in the 1930s, failed to use it as the foundation for his macroeconomic writing on money; instead, like most of his contemporaries, he adopted the cruder characterization of the monetary sector derived from Keynes's *General Theory*. This suggests that when telling the story of macroeconomics in the 1940s it may be right to leave *Value and Capital* out. Patinkin acknowledges the influence of

writers (e.g. Modigliani, 1944) had contributed to this process by putting Keynesian/Hicksian equation systems forward as aggregative versions of Walrasian models, but the link between micro- and macro-systems was far clearer and more explicit in Patinkin's work than in previous contributions. In addition the major technical problems concerning the compatibility of Keynesian theory, the quantity theory and Walrasian excess demand functions had by then been solved (or were thought to have been solved).[11] The theory, popularized by Samuelson (1955) as 'the neoclassical synthesis' and of which *Money, Interest and Prices* is the outstanding example,[12] was consistent with the neo-Walrasian hard core and in addition all the positive heuristics bar one had been followed.[13] By the mid-1950s, therefore, the neo-Walrasian research programme in macroeconomics had been securely established.[14] There was still much work to do, but subsequent work can be seen as developing and extending the programme rather than as establishing it.

Phase II: The development of the neo-Walrasian research programme

The first major challenge to the neoclassical synthesis, as it is reasonable to call the macroeconomic theory that had emerged by the end of the 1950s, was posed by Clower (1965).[15] The neoclassical synthesis explained unemployment in terms of wage rigidity: the labour market was not in equilibrium. Clower's argument was that if the labour market were not in equilibrium the conventional model of consumer decisions would be mis-specified. It was necessary to re-write the consumer's maximization problem such that demands for goods depended not only on endowments and prices but also on realized sales of labour (or any other good the consumer is trying to sell). More generally, if an agent faces a constraint in one market his or her demands or supplies in all other markets will typically be affected. Clower presented these arguments as a challenge to the prevailing theory, but it is

(Continued from previous page)
Value and Capital only on the microeconomic part of *Money, Interest and Prices*: the macroeconomic section refers to IS–LM but not *Value and Capital*.

[11] I have in mind the literature associated with the real balance effect and the determinacy of the price level, and the question of whether wage rigidity was necessary or sufficient for an unemployment equilibrium to emerge.

[12] The third edition of his textbook, published in 1955, was the first to use this term.

[13] HC5' will be considered later.

[14] Samuelson (1955) claimed that the neoclassical synthesis had the support of 90% of American economists.

[15] There were also important elements of disequilibrium in Patinkin (1956/65) but these were not presented as undermining the neoclassical synthesis. Clower's (and later Leijonhufvud's) rhetoric was very important.

important to note that, though Clower saw them as presenting a fundamental challenge to orthodox ideas, they did not constitute any challenge to the neo-Walrasian research programme *as it is defined* here: indeed, his work could be construed as following its heuristics very closely. He observed an inconsistency in the existing theories and modified the theory in such a way as to protect the hard core.

Clower's ideas were soon taken up by several economists (e.g. Solow and Stiglitz, 1968; Barro and Grossman, 1971; Malinvaud, 1977)[16] who used them as the basis for new models of macroeconomic equilibrium. The assumption of wage inflexibility made in the neoclassical synthesis models was generalized to the assumption that all prices took time to move,[17] the new 'rationing models' describing the equilibria that would result as long as prices were out of equilibrium. These models were formal optimizing models, specifying a new set of rules governing the interaction between agents. They defined a new equilibrium concept (fixed-price equilibrium) in terms of which the economy was analysed. This was all in accordance with the positive heuristics of the neo-Walrasian research programme.

For a few years this extension to the neo-Walrasian research programme appeared both theoretically and empirically progressive. Focusing on the failure of prices to adjust instantaneously rather than on wage inflexibility seemed to remove an anomaly in explanations of unemployment. In addition, the use of rationing models led to further successful predictions, some of which are discussed in section 5 below. These new insights, however, proved limited. More important than this, by the mid-1970s the assumption of price stickiness came to be seen as a major anomaly, and attempts were made to solve it through what turned out to be a progressive extension of the research programme.

The problem with existing theories was that variations in the level of output and employment could be explained only by assuming that prices were inflexible, an assumption which seemed inconsistent with the hardcore postulate of optimizing

[16] This list includes only authors of macroeconomic theories. Microeconomic general equilibrium theory was also affected, economists such as Arrow, Hahn, Benassy, Dreze and many others working on such models. For a survey see Drazen (1980). I have not included Leijonhufvud, whose name is frequently bracketed with Clower's in this context, for though his book (1968) did much to stimulate interest in this type of theory, he did not construct formal models in the way required by the neo-Walrasian research programme. Whilst his 1968 book was definitely Walrasian in its approach, his later work took him outside this research programme.

[17] This was explained by Clower as being the consequence of removing the artificial device of the Walrasian auctioneer.

behaviour: if a market is out of equilibrium it is in the interests of both buyers and sellers to change the price. This hard-core assumption had to be protected by assuming that it was costly for firms to change prices, that changing prices took time and other assumptions which were *ad hoc* in the sense that they had nothing to do with either the hard core or the positive heuristics of the research programme. The response to this came in papers by Phelps (1967) and Friedman (1968) who showed how limited information could be used to explain why changes in the level of demand frequently affected output and employment. Phelps explained this using his famous 'island' parable (later taken up by Lucas). The economy consists of a number of islands and whilst workers are fully informed about prices and wages on their own island, information flows between islands are costly. If wages on one island fall, workers will not know whether this reflects an economy-wide fall in wages or whether it is specific to their own island. Some workers will thus choose to become unemployed in order to search for better wages elsewhere. A fall in wages will, therefore, even if prices fall in the same proportion, lead to a fall in employment.

It is with the work of Phelps and Friedman that PH5' becomes important to the neo-Walrasian research programme: prior to this economists had not given much attention to the information available to agents. The assumption that workers have incomplete information made it possible to reconcile unemployment with the hard-core postulate of maximizing behaviour (Phelps described himself as sticking 'doggedly' to this assumption) and turned the 'anomaly' of the Phillips curve into corroborating evidence for the neo-Walrasian programme. In addition, when economists began to examine the implications of imperfect and asymmetric information and to allow for different attitudes towards risk they started to explain previously unexplained phenomena: for example, the existence of long-term labour contracts (e.g. Baily, 1974; Azariadis, 1975; Calvo and Phelps, 1977; Hall, 1980) and why employers may pay more than the market-clearing wage rate (Weiss, 1980). Anomalies have been turned into corroborating evidence for the programme.

The most widely known extension to the neo-Walrasian programme has been its extension to the formation of expectations: the theory of rational expectations. As with price stickiness, the assumption that expectations adjusted slowly in response to changes was *ad hoc*: it was part of the protective belt, required to protect the hard core. The theory of rational expectations, developed by Muth (1961) and applied to macroeconomics by Lucas (1972) showed how this anomaly could be eliminated. More important than this, however, the assumption of rational expectations opened up a new set of macroeconomic models capable of generating new predictions. Such models included not only the 'new classical macroeconomics', associated with Lucas, Sargent, Wallace, Barro and their followers, but also work such as that of Dornbusch (1976). The introduction of rational expectations was undoubtedly *theoretically* progressive. Whether or not it was *empirically* progressive is considered below.

4 EVALUATING THE NEO-WALRASIAN RESEARCH PROGRAMME

Novel facts[18]

There seems little doubt that the neo-Walrasian programme in macroeconomics has been *theoretically* progressive. It has managed to explain an increasing range of economic phenomena without resorting to an increasing number of *ad hoc* auxiliary assumptions. The critical question, however, is whether or not it has also been *empirically* progressive. This is important for two reasons. The obvious one is that we are interested not only in explanation but also in appraisal: we want to know whether the subject is developing in a satisfactory manner. The other reason is related to this. If Lakatos's methodology of scientific research programmes were an appropriate model of how scientists (including economists) behaved, we would expect economists to support progressive research programmes. Therefore, if we could show that the neo-Walrasian research programme were empirically progressive, we would have shown those economists working within the programme (the vast majority of post-war macroeconomists, if the rational reconstruction given here is appropriate) to have been behaving in a rational manner. We would thus have corroborating evidence for the adequacy of the rational reconstruction contained in section 3. As Lakatos put it, 'In the light of better rational reconstructions of science one can always reconstruct more of actual great science as rational' (Lakatos, 1971, p. 132),

The fundamental notion here is that a theoretical change is progressive if it is content-increasing: if theories explain not only the facts that they were designed to explain, but also some novel facts. It is the prediction of such novel facts that distinguishes progressive research programmes from series of *ad hoc* rationalizations.

[18] The ideas in this section are all taken from Lakatos. It is included because when I presented early drafts of this chapter I was criticized *both* for paying too much attention to predictions *and* for not paying enough attention to them. I concluded that it was necessary to explain in more detail what I was trying to do.

The key issue here is what constitutes a 'novel fact'. The most clear-cut examples of novel facts are, of course, facts not known when the theory is proposed: genuine *pre*dictions which turn out to be successful. Such predictions clearly provide corroborating evidence for a research programme. It is, however, necessary to broaden the definition of novel facts to include new interpretations of already-known facts and facts which were not previously explained within the research programme (Lakatos, 1970, pp. 32, 70–1). If a new theory explains the phenomena it was designed to explain and in addition explains what had previously been an anomaly, an unexplained fact or fact explicable only by some *ad hoc* auxiliary hypothesis, this can be interpreted as corroboration of the programme. When it comes to comparing two research programmes, a programme can also be corroborated if it explains facts which played no essential role in the competing programme (they may have been explained only with the aid of *ad hoc* hypotheses, or they may have been ignored as being unimportant).[19]

To show that the neo-Walrasian research programme in macroeconomics has been progressive we must, therefore, find examples of where it has successfully predicted novel facts. To do this we must do three things. (1) We must find examples of novel facts, which can be (a) predictions before the event, (b) facts that were not previously explained, (c) new interpretations of old facts or (d) facts which played no role in competing research programmes. These facts must all be facts that the theory was not specifically designed to explain. (2) We must show that these novel facts follow from the hard core and heuristics of the neo-Walrasian programme. (3) We must show that these were corroborated. Note that it is not necessary that all, or even most of, the predictions made within the neo-Walrasian programme be confirmed: rather, to quote Lakatos, 'what matters is a few dramatic signs of empirical progress' (Lakatos and Zahar, 1976, p. 179).

Alternative programmes

In appraising the neo-Walrasian research programme we must not forget the existence of alternative research programmes. In particular, it is important that any 'novel facts' we use as evidence for the programme's success do not follow equally well from the heuristics of some competing programme. A programme which is especially important here is what can best be called the 'Chicago' programme. This is important

[19] Lakatos and Zahar (1976, p. 185); cf. Zahar (1973). They argue that it was only this category of novel facts that, for many years, provided any corroborating evidence in favour of Copernicus's research programme *vis à vis* Ptolomy's.

because there is such a large overlap between the two programmes. Though they overlap, however, they are not the same.[20]

The main distinguishing feature of Chicago economics is what Reder has termed 'Tight Prior Equilibrium' theory, based on the hypothesis of Pareto-optimality; namely that

> decision makers so allocate the resources under their control that there is no alternative allocation such that any one decision maker could have his expected utility increased without a reduction occurring in the expected utility of at least one other decision maker.
>
> (Reder, 1982, p. 11)

Imperfect competition, market failure and government intervention are taken to be sufficiently infrequent and have a sufficiently limited impact that the hypothesis of perfect competition provides a good approximation to the way in which markets work (ibid., p. 15).

A further difference between Chicago and neo-Walrasian economics concerns the role of formal model-building. Although many Chicago economists do construct formal mathematical models, and although much Chicago economics could be seen as also lying within neo-Walrasian economics, the construction and use of formal models does not receive the same emphasis. There is a strong tradition in Chicago economics, of which Friedman is undoubtedly the main representative, which favours simple models. Models which would be quite respectable Chicago economics (such as are found in most of Friedman's work) would not be seen as 'fully specified' in the sense of heuristic PH1'.[21]

[20] It would clearly be desirable to discuss the hard core and heuristics of competing research programmes in the same detail as we have discussed those of the neo-Walrasian programme. However, because this is too large a task to perform here Reder's characterization of Chicago economics is taken as defining the programme adequately for the purposes of this chapter.

[21] In section 5 below we consider the consumption function and the expectations-augmented Phillips curve. In both cases Friedman's contribution exemplifies the Chicago, non-neo-Walrasian approach, with the work of Ando and Modigliani (on consumption) and Phelps (the Phillips curve) representing the neo-Walrasian approach.

5 SOME NOVEL FACTS GENERATED BY THE NEO-WALRASIAN RESEARCH PROGRAMME

The consumption function

One of the earliest examples of successful prediction in post-war macroeconomics is the life-cycle/permanent income theory of consumption, of which Friedman's *The Theory of the Consumption Function* (1957) is the outstanding example. Note that although there are substantial differences between the approaches of Chicago economists (Friedman) and non-Chicago ones (Ando and Modigliani), with the latter providing a much more formal treatment of the maximizing hypothesis, we are not attempting here to distinguish between them, accepting the generally-held view that the two theories yield substantially the same empirical predictions.

The basic facts the life-cycle theory was designed to explain were:

> (a) the rough constancy of the average propensity to consume in the United States over the past half-century, as measured by time series data, despite a substantial rise in real income; (b) the rough similarity of the average propensity to consume in budget studies for widely separated dates, despite substantial differences in average real income; (c) the sharply lower savings ratio in the United States in the period after World War II than would have been consistent with the relation between income and savings computed from data for the interwar period.
>
> (ibid, p. 38)

Friedman managed to explain not only these broad generalizations, but his theory also predicted several additional 'novel facts' concerning things such as shifts in the consumption function over time, the differences between farm and non-farm savings behaviour, inter-country differences and so on. He confronted these predictions with detailed evidence, in all cases coming to the conclusion that the data either supported his theory or were not inconsistent with it (the data were often inconclusive). Friedman claimed that

> Perhaps the two most striking pieces of evidence for the hypothesis are, first, its success in predicting in quantitative detail the effect of classifying consumer units by the change in their measured income from one year to another; and, second, its consistency with a body of data that *have not heretofore been used in analyzing consumption behavior* or, indeed, *even regarded as relevant*

> *to consumption behavior*, namely, data on the measured income of individual consumer units in successive years.
>
> (ibid., p. 225)

The theory was developed to explain one set of data and was then used to make predictions concerning other things. Friedman and many other economists considered that these predictions were mostly confirmed. Though there are now signs of a possible movement away from the permanent income–life-cycle theory, and it is conceivable that it might at some stage be abandoned (see Gilbert, 1991), the theory was thought for many years to have been important in predicting a broad range of facts about the behaviour of consumption and income.

Inflation and unemployment

The expectations-augmented Phillips curve as developed by Phelps and Friedman led to a number of predictions concerning the relationship between the level of aggregate demand, inflation and unemployment. The main ones were that: (a) the level of unemployment consistent with a constant inflation rate is independent of the inflation rate; (b) persistent low unemployment will lead to accelerating inflation; (c) increasing the growth rate of demand produces a short-term rise in unemployment, but in the long run produces only a rise in inflation.

The first of these predictions is difficult, if not impossible, to test conclusively because, as Friedman admitted, the 'natural' rate of unemployment is not constant. There have been numerous attempts to estimate the natural rate, but the problems are sufficiently great that it is hard to regard such work as being anything like a satisfactory test of the theory. The best we can say is that the hypothesis has not been refuted by empirical evidence. The other two, related, predictions, on the other hand, could be regarded as having been corroborated by the experience of the 1970s.[22] The predictions were first made in 1967, when inflation had started to rise, but had not yet reached levels which suggested anything substantially different from earlier experience: there was little evidence that inflation was accelerating or that the 'conventional' Phillips curve had broken down (seeFigure 2.1).

[22] This is when the results were first published, Friedman's 'The role of monetary policy' (Friedman, 1968) having been delivered as his Presidential Address to the AEA the previous year.

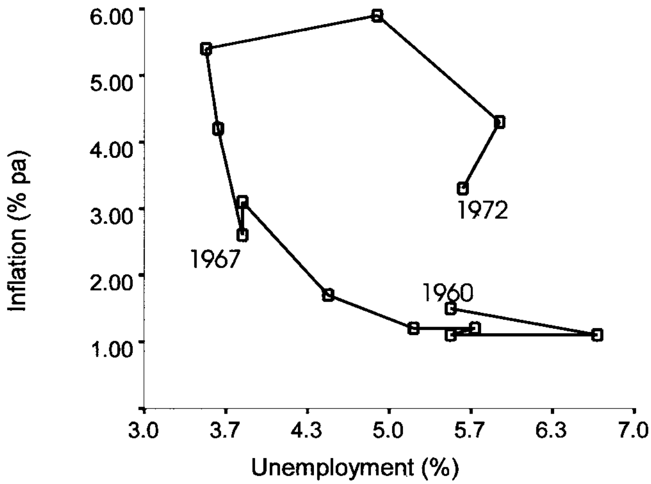

Figure 2.1 **Inflation and unemployment in the USA, 1960–72**

During the late 1960s there was a large expansion of aggregate demand, due first to Kennedy's application of Keynesian policies and later to the cost of the Vietnam war. The sharp rise in inflation, without much change in unemployment, in 1968–9 could be explained in terms of a non-linear Phillips curve, but once this high inflation rate began to feed into expectations, the economy moved off the 1960s Phillips curve. Rising unemployment was needed in 1970–1 simply to stop inflation rising still further: even with unemployment rates comparable with those of the early 1960s, inflation remained over 3%. Friedman and Phelps could well argue that these events vindicated their predictions, and that their predictions were made well in advance of events.[23]

Models of price stickiness

One of the problems with the consumption function and the expectations-augmented Phillips curve as examples of the neo-Walrasian research programme generating novel facts is that they do not discriminate between the neo-Walrasian and Chicago research programmes. In order to do this, therefore, we turn now to examples of predictions made by models which lie squarely within the neo-Walrasian research programme but which are clearly outside the Chicago programme. Start with rationing

[23] We have provided data only for the years up to 1972 in order to leave aside the complications caused by the oil-price rise of 1973–4 and the productivity slowdown, confining our attention to the period when the main cause of inflation was high aggregate demand.

models. These were designed to explain the problem of Keynesian unemployment, but the logic of the simple two-market models initially discussed led naturally to a threefold classification of Keynesian unemployment, classical unemployment and repressed inflation (a logically possible fourth category was dismissed as economically uninteresting). Though these models are notoriously difficult to estimate, let alone to test, disequilibrium phenomena causing severe econometric problems, it could be argued that they were corroborated by the response of different economies to OPEC I. Labour market institutions were very different in Europe and the USA, with real wage rigidity in Europe and nominal wage rigidity in the USA. This should, if rationing models are correct, result in classical unemployment in the USA and Keynesian unemployment in Europe. Econometric evidence on real wage gaps is too tenuous to provide any firm evidence that the theory was confirmed, but it is reinforced by evidence that European governments responded as though the problem was one of classical unemployment (they tried to reduce real wages) whilst the USA tried to deflate demand. If we assume that governments were acting rationally, this suggests that these economies were behaving in conformity with rationing theory. What makes this significant is that this was happening in circumstances that had not previously been observed, the oil shock with the simultaneous fall in productivity and deflation of aggregate demand being unlike anything that had happened before.

A more dramatic and much more conclusive example is the ‘Dornbusch model’ (Dornbusch, 1976). This combined the assumption of perfect foresight in financial markets (equivalent to rational expectations in the deterministic model Dornbusch was using) with sluggish price-adjustment in goods markets. His prediction was that a monetary expansion (contraction) would produce a depreciation (appreciation) of the exchange rate, which might overshoot its long-run equilibrium. The characteristic feature of Dornbusch’s model, distinguishing it from previous exchange-rate models, was that flexibility and rational, forward-looking behaviour in financial markets *contributed to* instability, rather than reducing it.

As dramatic a test of this theory as one might reasonably expect to see came in the UK in 1979–80 when the newly-elected Conservative government introduced an exceptionally tight monetary policy at the same time as financial markets were becoming much more flexible. From the third quarter of 1979 (the election was in October 1979) the growth rate of both broad and narrow monetary aggregates fell sharply: after being positive in the first half of 1979 growth rates of the real money supply fell to *minus* 10 per cent per annum in the first half of 1980.[24] Real interest rates rose very sharply. The result, as the Dornbusch model predicts, was a massive appreciation of sterling. Relative unit labour costs, perhaps the best measure of the real exchange rate, rose by an unprecedented 54 per cent in two years.

There are, as always, other factors to take into account. North Sea oil was coming on stream and this would account for part of the appreciation, especially after OPEC II. However, most calculations of the 'oil premium' suggest that it was substantially below the appreciation that was observed. In addition, oil exploration had been under way since the mid-1970s and the results, though obviously subject to uncertainty, must to a substantial extent have been anticipated. Furthermore, the severity of the recession, due to a massive de-stocking as firms failed to export goods, suggests that the exchange rate had risen well above the level compatible with international competitiveness. It could be argued, therefore, that the 'Thatcher experiment' corroborated the predictions of the Dornbusch model.

The new classical macroeconomics

The new classical macroeconomics produces such strong predictions, with such dramatic implications for government policy and presenting suitable econometric challenges that there have been numerous attempts to test them. The predictions resulting from the new classical macroeconomics include: (a) anticipated changes in aggregate demand will have no effect on real output (structural neutrality), with the corollary that only unpredictable movements in aggregate demand will affect real variables; (b) the more unpredictable is aggregate demand, the smaller is the effect on real output of any given unpredictable movement in aggregate demand. Lucas (1973) produced empirical evidence in support of the latter whilst Barro (1977, 1978; Barro and Rush, 1980) produced evidence for the former.

These predictions and the empirical evidence which seemed to corroborate them have stimulated much econometric work, with many economists coming to the conclusion that the new classical theories were not consistent with the data. For example, Gordon (1982) found that anticipated changes in aggregate demand *did*

[24] The one exception to this pattern was sterling M3, the aggregate to which the government was paying most attention. Monetary base, M1 and the aggregates broader than sterling M3 all behaved as described here. The errant behaviour of sterling M3 may explain why the government effected such a severe recession despite policy statements which appeared to imply a 'gradualist' outlook. The severity of the monetary policy, and the mechanism whereby it affected the economy, are clearly documented in Buiter and Miller (1981).

have a significant effect on real output. He reconciled this with Barro's evidence, according to which anticipated changes in the money supply had no such effects, by suggesting that the link between monetary growth and the growth rate of demand was weak. The reason for Barro's results was the traditional Keynesian notion that money had only a limited effect on aggregate demand. Other economists criticized Barro and Lucas on more technical grounds (lag lengths, the type of statistical tests they performed and so on).

The new classical assumptions have also been applied to generate other predictions. One of the most dramatic predictions concerned the consumption function, where Hall (1978) claimed that 'no variable apart from current consumption should be of any value in predicting future consumption' (ibid., p. 971). As with Lucas's and Barro's results, subsequent empirical evidence has suggested that this is not so: that other variables do affect consumption.

Finally, it is worth pointing out that the logic of the new classical theory is that there should be no business cycle. According to the new classical theory it is only unanticipated, and hence unpredictable changes in aggregate demand that cause departures from full employment. Fluctuations in real output about full-employment output should, therefore, be uncorrelated with any information available to decisionmakers, a prediction which is inconsistent with the existence of a business cycle (i.e. with deviations from full-employment output exhibiting positive serial correlation). Had there been no business cycle, we can be sure that this would have been cited as corroborating evidence for the new classical programme (a part of the Chicago programme) so it seems reasonable to cite the existence of a business cycle as evidence against it.[25] Lucas and others have, of course, attempted to explain why there is a business cycle, but the assumptions they introduce to do this are essentially *ad hoc.*

6 IS THE NEO-WALRASIAN PROGRAMME IN MACROECONOMICS PROGRESSIVE?

The first thing to look for is predictions before the event, for, although it is not necessary that predictions be of this type, there can be no doubt that these constitute corroborating evidence. Due to the nature of economics such predictions are inevitably hard to obtain – until a new circumstance has arisen economists

[25] See Okum (1980) for a discussion of new classical theories of the cycle and the evidence against them.

often have no incentive to construct a theory capable of generating predictions. Furthermore, once a prediction has been made, behaviour may change in response to this prediction so as to ensure that it is never tested.

Two predictions before the event have been cited above: the Friedman– Phelps prediction of the consequences of sustained high aggregate demand in the late 1960s, and Dornbusch's prediction of the consequences of a severe monetary shock in a world of floating exchange rates, mobile capital and rational expectations. Doubts have been expressed about both, but it can none the less be argued that these have been dramatically corroborated.

Broadening our use of the term 'prediction' to include predictions of anything that the theories concerned were not designed to predict, we have the example of Friedman's theory of the consumption function, designed to explain a few basic facts, but which explained a large number of related facts. To this could be added numerous instances of neo-Walrasian theories explaining other phenomena they were not designed to explain. Indeed, it could be argued that the main feature of neo-Walrasian economics, too obvious to need documenting, is its ability to generate and explain apparently disparate phenomena in terms of maximizing behaviour.

There is, however, a serious problem here, connected with the question of what constitutes the relevant theory. Take an example. Dixit (1978) uses a rationing model, of the type used by Barro, Grossman, Malinvaud *et al.* to analyse the problem of the balance of trade. He managed to explain in terms of this theory 'facts' about the relationship between the government deficit and the balance of trade which were essentially *ad hoc* assumptions (believed to be empirically justified) in another research programme (the so-called 'New Cambridge' theory).[26] The issue here is whether this constitutes an example of disequilibrium macroeconomics, designed to explain the problem of Keynesian unemployment in a closed economy, successfully generating predictions in an area that it was not intended to cover; or whether we have to regard Dixit's model as a new model designed to explain the balance of trade. At another level, could the whole of neo-Walrasian *macro*economics be regarded as generating predictions from a theory aimed at explaining something very different (whatever neo-Walrasian *micro*economics is about)? We might wish to examine assumptions used in different neo-Walrasian models in detail to determine

[26] The thesis was that a change in the government deficit would, in the short run, cause the balance of trade to change by an equal and opposite amount. The New Cambridge explanation used the accounting identity that the sum of sectoral deficits must be zero together with the assumption that the private sector deficit was approximately constant. The latter assumption was believed to have empirical support, but there was no theoretical rationale for it.

when we have a 'new' theory and when we have simply an application of an old theory, but we are in the end faced with a subjective decision about what constitutes a new model.[27] We thus have to make a subjective decision as to whether a prediction has been generated from a model designed to explain something else, or whether it has been generated by a purpose-built one. To criticize neo-Walrasian economics we focus on theoretical novelties; to defend it we focus on the fact that many theories are 'essentially' the same.

There is also the issue of how neo-Walrasian economics compares with other competing, and possibly overlapping, programmes – in particular, of what can be claimed *vis à vis* the Chicago programme. The overlap between these two programmes is so large that to a certain extent they stand or fall together. The examples cited in section 5, however, suggest that there is a strong case for claiming that neo-Walrasian macroeconomics remains progressive in a way that Chicago *macro*economics (we are making no comment on Chicago *micro*economics) does not. There are two sides to this. On the one hand, important predictions from the non-Chicago part of neo-Walrasian economics have been successful. On the other hand, the dramatic predictions made within the new classical macroeconomics have not been confirmed.

7 CONCLUDING REMARKS

In this chapter I have viewed contemporary macroeconomics from a Lakatosian perspective for two reasons. The first is to provide another case study which can be used in evaluating the relevance of Lakatosian ideas for economics. This is one reason why Lakatosian ideas have been applied relatively rigidly: we need to see how far we can push them before they cease to work. Here, the main conclusion to emerge from the chapter is that the rational reconstruction of macroeconomics since Keynes, provided in this chapter, fits the facts very well. There are the problems of pre-test bias and of having fewer observations than we would like. In addition, we have not explored the alternatives to the neo-Walrasian programme adequately.

[27] A related point is made, in a very different context, in Backhouse (1995, chapter 2).

On the other hand, the fact that we can reconstruct the period's 'great (macroeconomic) science' as rational can be taken as corroborating the Lakatosian perspective.[28] Furthermore, the key feature of the neo-Walrasian programme as described here is its stress on rational behaviour. This is something that most of the theorists concerned would immediately acknowledge as a key assumption underlying their work and underlying post-war economic theory. In addition, possibly the most common defence of neo-Walrasian economics (from practitioners, not methodologists) is that 'it works'. By this economists usually have in mind something akin to the Lakatosian criterion for a progressive research programme: that their theories provide a source of new predictions, a significant number of which turn out to be correct. The Lakatosian perspective can thus be defended on the grounds that it is in accordance with the way economists see their work.[29]

The second reason for adopting a Lakatosian perspective is to appraise contemporary economics. I am assuming that the superstructure of economic theory needs an empirical justification, and that Lakatos's methodology provides a possible way of doing this. Here, the conclusion to emerge from the chapter is that the neo-Walrasian research programme appears to be both theoretically and empirically progressive. By way of a postscript it is worth noting a corollary to the argument advanced here. This concerns 'monetarism' and 'Keynesianism'. These are often presented as two rival research programmes. According to our interpretation of post-war macroeconomics, however, much of the debate between monetarism and Keynesianism emerges as a debate within the neo-Walrasian programme.[30] This would explain why Keynesian and monetarist theories have developed in such similar ways during the post-war period.[31] The very great changes which have taken place in macroeconomics since 1970 should, according to this interpretation, be seen not in terms of the replacement of one research programme with another but in terms of a shift of attention within a larger research programme.

[28] I have let the conclusions stand as they were when the chapter was first published in 1991. Now I would be a little more guarded in my support for a Lakatosian methodology, though still arguing that a Lakatosian perspective can be revealing. See Chapter 5.

[29] I am grateful to Harry Collins for drawing my attention to this issue.

[30] It is not, of course, entirely within neo-Walrasian economics. We might wish, for example, to think in terms of a 'National Bureau' research programme, dominated by Wesley Clair Mitchell, having heuristics that are very different from those of the neo-Walrasian programme. Friedman's work would fit into such a programme much better than into the neo-Walrasian programme.

[31] This is argued in more detail in Backhouse (1995, chapter 8).

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Chapter 3

Lakatos and Economics*

(*Perspectives on the History of Economic Thought*, volume VIII, edited by Todd Lowry. Cheltenham and Brookfield, VT: Edward Elgar, 1992, pp. 19–34.)

This chapter represents my first attempt to take stock after the criticism of Lakatos's methodology of scientific research programmes to which I had been exposed at the Capri conference. It reaches two conclusions. The first is that, though its epistemological foundations may be problematic, there remain strong reasons for continuing to attach importance to the Lakatosian appraisal criterion – that research programmes successfully predict of novel facts. The second is that Lakatos's concept of a hard core is inadequate to capture the varied nature of the relationships that exist between different economic theories, and that alternatives should be explored. There is no reason why research programmes cannot be defined in different ways, and still be judged according to whether they exhibit empirical progress.

1 INTRODUCTION

Aims

Since the mid-1970s there has been much debate over the relevance of Lakatos's methodology of scientific research programmes (MSRP) to economics. However, although economists have continued to find the concept of a scientific research programme valuable in interpreting the history of economic thought, Lakatos's methodology has recently been subjected to a number of very severe criticisms.

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The aim of this chapter is to survey these and to make some tentative suggestions as to what we should, and should not, retain from Lakatos's methodology.

In this chapter our prime concern is with general issues, concerning the relevance of Lakatosian ideas to economics as a whole. Many of the examples, however, will be taken from contemporary macroeconomics. There are two reasons for this. The obvious one is my own interests (e.g. Chapter 2). The second is that some of the most interesting papers, to which I wish to respond (notably Hands, 1985, 1990; Maddock, 1991; Hoover, 1991), deal with macroeconomics.

Before venturing into a discussion of Lakatos's methodology it is important to note that there are two reasons why we may be interested in methodology. We may be searching for a methodology which does no more than describe what economists actually do. Alternatively, we may be concerned with appraisal: with comparing what economists actually do with what they ought to do. This chapter is based on the assumption that we should be concerned with appraisal. It is important to make this clear, because it limits the range of conclusions we can accept. In particular, it means we cannot simply say that Lakatos's MSRP applies to some areas but not to others. If we were concerned merely with description there would be nothing inherently wrong with such a conclusion (though we might wish to ask 'Why?'), but if we are concerned with appraisal we cannot stop there. It may be that the MSRP does not fit one area of economics because, for some reason, it is an inappropriate methodology to adopt in that area; or it may be that economists working in that area are guilty of bad practice – that they should be following the methodology but, for some reason, they are not doing so. The difference between these two cases is of vital importance.

The main issues

Lakatos's MSRP comprises three main elements.

1. *The research programme*, made up of a hard core and sets of positive and negative heuristics, as the object of appraisal.
2. *An appraisal criterion*, of corroborated excess empirical content.
3. *A criterion by which the MSRP can be appraised* (which has been termed 'the methodology of *historical* research programmes', or MHRP) based on the thesis that if the MSRP is appropriate, economists will abandon degenerating research programmes in favour of progressive ones.

Though they are sometimes grouped together, as though forming part of an indivisible package, these three elements must be considered separately, for different issues arise in respect of each of them.

2 THE CONCEPT OF A SCIENTIFIC RESEARCH PROGRAMME

Problems which arise in identifying SRPs

Many economists have tried to identify Lakatosian SRPs in economics, but though there have been successes, the verdict has in many cases been that Lakatos's categories are at the same time too rigid and too imprecise.

The first problem to arise is that of whether SRPs are to be defined on a large or a small scale. At one extreme we can view neoclassical economics as one SRP, ranged against various heterodox programmes (institutionalism, post-Keynesianism and so on). At the other extreme we can define research programmes at a micro-level, each encompassing a small, well-defined section of the literature. For example, microeconomics can be analysed in terms of a neo-Walrasian SRP (Weintraub, 1985) or in terms of smaller programmes: human capital theory (Blaug, 1980, chapter 13); the economics of the family (ibid., chapter 14). In macroeconomics we might argue in terms of a neo-Walrasian SRP (Chapter 2; Weintraub, 1979) or we might distinguish monetarist and Keynesian SRPs (Cross, 1982). Within 'monetarism' we might distinguish between the new classical SRP (Maddock, 1984, 1991) and the earlier SRP associated with Milton Friedman. Within Keynesian economics there is a strong case for distinguishing the literature on rationing models as a distinct research programme, set against, for example, the later literature which rejects the fix-price assumption in favour of imperfect competition (such as Hart, 1982; Blanchard and Kiyotaki, 1987; Marris, 1991). Alternatively, we might distinguish even more micro-level SRPs (such as the literature on the implications of rational expectations for the theory of the consumption function, or attempts to test the efficiency of foreign- exchange markets). Lakatos's MSRP is sufficiently elastic that none of these different interpretations can, *a priori*, be ruled out. Each has some merit.

Once we have decided how we are going to apply the MSRP, a further set of problems arises.

1. Programmes may overlap, with some theories apparently fitting into two different programmes.
2. Different programmes may be related to each other.
3. It is sometimes difficult to identify a hard core that is unchanged over the life of the research programme, and which is common to all the theories that are considered to form part of the programme.

To illustrate these problems we shall consider two examples: the use of a neo-Walrasian SRP to interpret macroeconomics since Keynes; and the characterization of the new classical macroeconomics as a SRP. These two cases are chosen because they seem relatively strong examples where Lakatos's methodology appears to work rather well. In both cases, however, all three problems arise.

Example I: Neo-Walrasian macroeconomics

Weintraub (1985) has defined the hard core and heuristics of a neo-Walrasian programme which can be used to interpret the literature on the existence of general competitive equilibrium. Only minor modifications are required for it to be possible to use this SRP to make sense of the history of macroeconomics since Keynes (this is discussed in detail in Chapter 2, on which this and the following three paragraphs are based). Two features are central to this programme: the assumption of individual optimizing behaviour and the construction of fully-specified, consistent models. This means that the SRP is defined in terms of a modelling technique, rather than a set of beliefs about the economy, a point to which we shall return.

Given the undoubted importance of the assumption of rational behaviour in post-war economics it is, at first sight, hard to see how such an interpretation of Lakatos's MSRP could fail to fit. It would seem simply to demarcate neo-Walrasian economics from various heterodox approaches (institutionalism, post-Keynesianism and so on). Even this application of Lakatos, however, raises some serious problems. Consider the example of Milton Friedman. His aversion to complex, formal modelling, together with his approach to empirical work, especially on money, place him outside the neo-Walrasian SRP as we have defined it here. To accommodate Friedman we might wish to define a 'Chicago' research programme (say as defined by Reder, 1982). This, however, raises the question of overlap between the neo- Walrasian and Chicago SRPs, for some work (notably Friedman's AEA presidential address) is important to the evolution of both programmes.

There are also problems with the early phase of Keynesian economics, for Keynes's *General Theory* and much of the early work interpreting it fell outside the neo-Walrasian SRP (Keynes, for example, reasoned in terms of 'propensities' rather than optimizing behaviour, in a manner more reminiscent of Adam Smith or Alfred Marshall than of neo-Walrasian economics). We thus have to tell the story in terms of neo-Walrasian economics' encompassing theories which previously lay outside its domain. In so far as this just represents the victory of one SRP over another, this presents no difficulties. What we have, however, is not so much a competition between programmes, as the taking over by one programme of theories developed within another programme. In other words, we have to explore the nature of the relationship between competing SRPs in a way that goes beyond simple competition.

Finally, there is the problem of defining the hard core. The main reason why we do not have more problems is that the hard core has been defined so as to have very little definite economic content beyond the assumption of individual optimizing agents operating in interlinked markets. This means that the positive heuristics, requiring the construction of fully-specified, consistent models, are important in defining the character of the programme. They do more than simply protect the hard core, for they are important in defining the programme's modelling strategy. Even with such a restricted hard core, however, there are problems. Those associated with the work of Keynes and that of some of his early followers have already been discussed. Another problem concerns information. Prior to the 1960s little attention was paid to the information available to agents, but since then it has acquired great significance. In contemporary neo-Walrasian economics it is important that individuals are assumed to have a well-defined set of information, whereas in the 1950s this was not the case.

Example II: New classical macroeconomics

The same problems arise when we try to distinguish SRPs on a smaller scale and with a more definite economic content. As our example, consider the case of the new classical macroeconomics. It is possible to characterize the new classical macroeconomics as a Lakatosian SRP, the hard core of which comprises: the 'Lucas' aggregate supply function; the natural rate hypothesis; and rational expectations. Maddock has persuasively argued that such a programme was well established by around 1972, and that such a programme 'provides an adequate framework with which to explain the sequence of literature associated with the new classical tradition' (Maddock, 1991). Even so, all three of the problems listed above arise.

Consider first the general equilibrium component of the new classical macroeconomics. This could be viewed as a component of different SRPs, in each of which it plays a different role. Within the new classical SRP it forms part of the negative heuristic: it allows research to proceed without being distracted by a series of microeconomics-based critiques. Within the neo-Walrasian programme, however, it forms part of the positive heuristic.

Problems involving the relationship between different programmes are emphasized by the fact that important parts of the new classical literature are directed at competing research programmes, rather than contributing towards the development of the new classical programme itself. Lucas (1972a), for example, was concerned with the weaknesses of standard (non-new classical) testing procedures. From the viewpoint of Lakatos's MSRP, such arguments were superfluous. An even stronger example is Lucas's critique of econometric policy evaluation, which sought to point out a major internal inconsistency within an alternative research programme (Lucas, 1976). This paper is arguably the most influential paper Lucas has ever written, and yet, it is hard to see a role for it if we adopt a strict Lakatosian approach.

The difficulties involved in defining a clear hard core, even for such a seemingly well-defined programme as the new classical macroeconomics, can be illustrated by citing examples of papers that violate parts of what would normally be thought of as the new classical hard core. Lucas and Rapping (1969), considered by one commentator to be the first paper of the new classical macroeconomics (Hoover, 1988, p. 27), uses adaptive, not rational expectations. Sargent and Wallace (1975) used a modified IS–LM model, not a model based on maximizing behaviour. Sargent and Wallace (1982) present a model incorporating the real bills doctrine, according to which changes in nominal money balances affect real consumption decisions, violating the 'hard-core' assumption that real magnitudes are determined independently of nominal magnitudes. There are, of course, explanations for all these 'aberrations'. Lucas and Rapping wrote before the SRP had been fully articulated. Sargent and Wallace used the IS–LM model in order to attack their opponents on their home ground. When they adopted the real bills doctrine they reinterpreted the notion of 'neutrality of money'. These explanations do not, however, get rid of the problem, for if the concept of the hard core is to have its full meaning, work within the programme should not conflict with it.

3 THE APPRAISAL CRITERION

'Rhetorical' perspectives

Some of the most thoroughgoing critiques of Lakatos's appraisal criterion come from those economists who advocate replacing economic methodology, at least as it is traditionally understood, with an analysis of economists' rhetoric. McCloskey has argued that *any* prescriptive methodology (of which Lakatos's is one) is presumptuous and laughable: how can an outsider (the methodologist, or philosopher of science) tell a practitioner (the scientist) the best way to conduct his or her research? According to McCloskey, 'Einstein remarked that "whoever undertakes to set himself up as a judge in the field of Truth and Knowledge is shipwrecked by the laughter of the gods". ... Any methodology that is lawmaking and limiting will have this risible effect' (McCloskey, 1986, p. 104). If it were the case that methodological prescriptions were made without any reference to economists' practice, McCloskey's criticism would have some force. An important aspect of Lakatos's MSRP, however, is that there is an interaction between prescriptive methodology and scientific practice, which comes about through the methodology of *historical* research programmes. If the MSRP succeeds in making explicit the methodology underlying the best practice of economists (which is what the MHRP is about) McCloskey's criticism is avoided.

A more direct and very different attack on Lakatos is provided by Colander. Colander claims that,

> Most mainstream economists who are satisfied with the state of economics [how many of these are there?] follow (probably implicitly, because few study methodology) some brand of Popperian or Lakatosian methodology of science, both of which are refinements of logical positivism. [!] A principle of these scientific methodologies is that economics (or any other science) is advanced by the empirical testing of well-specified propositions.
>
> (Colander, 1990, p. 189)

He equates 'well-specified' with formal mathematics, and empirical testing with formal econometrics, with the result that he has little problem in showing that there is more to economic analysis than this. Though it *may* be true that many economists (and in particular many graduate students – who are the main focus of Klamer and Colander's book) do interpret theory formulation and empirical testing in this restrictive way, there is no need to do so: precision does not necessarily imply mathematics (though for some problems it may do) and empirical testing does not necessarily involve quantitative data. Good historical research (and I have in mind 'traditional' history, not simply cliometrics) can legitimately be seen as involving the formulation of clear hypotheses which are tested against empirical data (c.f. Blaug, 1980, p. 127). Provided that we avoid falling into the traps indicated by Colander, therefore, we need not abandon a methodology based on empirical testing. (I leave on one side the question of whether or not Colander is justified in describing Popper and Lakatos as 'positivist'.)

Eclecticism, relativism and critical pluralism

A similar, though not identical, position towards Lakatos is adopted by economists who explicitly endorse an eclectic approach towards methodology. A clear example here is Dow's endorsement of what she terms 'Babylonian' methodology (Dow, 1985). Dow's position is that economists should be using a variety of approaches, not a single, internally consistent one. This may indeed be a fair account of the situation in which we find ourselves, in that it may in practice be impossible to find a single, adequate methodology. To say this, however, is not the same as showing that we should not *seek* a consistent methodology.

A similar refusal to choose between methodologies emerges from parts of the literature on the sociology of knowledge. Knowledge is seen as the product of a particular community, without meaning outside that community. From such a perspective the search for a single methodology is misguided. The response to this is twofold. First, we do not have to be positivists to accept that there are empirical data which constrain knowledge, and which cause different communities to have certain things in common. Secondly, in the literature on economic methodology we are concerned with a particular, albeit somewhat extended, scientific community. It is thus reasonable to assume that shared knowledge and beliefs are sufficiently great to make communication within this community possible.

Though he has been accused of this, Caldwell's 'critical pluralism' is not the same as eclecticism, but if it is (wrongly) presented as an *alternative* to Lakatos's MSRP, its implications sound very similar. Loosely, Caldwell's position (see Caldwell, 1982, 1988, 1990) is that we should not confine our attention to any one single methodology (whether Lakatos's or any other), for there are, at least at present, no grounds for claiming that any one methodology is superior to all others. This position is differentiated from eclecticism by the contention that all methodologies should be critically appraised. Critical pluralism is, therefore, not an alternative to Lakatosian methodology, but a meta-methodology according to which alternative methodologies are to be appraised. Thus the arguments presented here, which are aimed at evaluating one particular methodology, are consistent with a critical pluralist position.

Specific criticisms of excess content

Lakatos's appraisal criterion of corroborated excess content, or 'novel facts', has also been criticized by Hands, an economist who is sympathetic towards many other aspects of Lakatos's methodology (see below). He has two distinct lines of criticism to offer. The first is that good economic theories involve much more than simply the successful prediction of novel facts:

> Why would we want to accept the position that *the sole necessary condition for scientific progress is predicting novel facts* not used in the construction of the theory? Surely humankind's greatest scientific accomplishments have amounted to more than this. 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.
>
> (Hands, 1990, p. 78)

He goes on to cite Smith's invisible hand, Walras's notion of multimarket interdependence, Marshall's welfare economics and Keynes's notion that aggregate demand determines the level of output and employment as constituting great economics. These, he argues, have given insight and progress, not just 'an occasional novel fact' (ibid.).

The response to this is that the prediction of novel facts is important because it enables us to be sure, in a way that the other criteria cited by Hands do not, that we are making progress in understanding the real world. As Blaug has put it,

> 'scientific progress' is progress in achieving 'objective knowledge' and the only way we can be sure that we have achieved objective knowledge is to commit ourselves to the prediction of novel facts.
>
> (Blaug, 1990, p. 102)

It is here that the second strand of Hands's criticism is relevant, for such a statement has to be based on some epistemology. Hands argues that Lakatos's appraisal criterion is based on the Popperian notion of empirical content (Hands, 1985, pp. 5-8). Furthermore, the emphasis on novel facts rather than falsification (characteristic not only of Lakatos, but also of certain of Popper's writings) was an attempt to move away from Popper's original emphasis on falsificationism to a position more consistent with Popper's view of verisimilitude as the aim of science. Popper's theory of verisimilitude, however, has been subjected to extensive criticism. Hands thus argues that because Lakatos completely abandoned falsificationism in favour of the prediction of novel facts, he was left with an appraisal criterion that lacked any firm epistemological basis (see Hands, 1991). Hands thus concludes that we should abandon the prediction of novel facts as our appraisal criterion, even if we retain other elements in the MSRP.

4 THE 'METHODOLOGY OF HISTORICAL RESEARCH PROGRAMMES'

Appraising the best gambits

Lakatos's contention is that

> a rationality theory – or demarcation criterion – is to be rejected if it is inconsistent with an accepted 'basic value judgement' of the scientific élite.
>
> (Lakatos, 1971/78, p. 124)

He compares this criterion with Popper's methodology, arguing that the latter depends on the existence of '(relatively) singular statements on whose truth-value scientists can reach unanimous agreement' (ibid). He then defends his meta-methodology against the charge that differences of opinion are too great to permit its use:

> While there has been little agreement concerning a *universal* criterion of the scientific character of theories, there has been considerable agreement over the last two centuries concerning *single* achievements. While there has been no *general* agreement concerning a theory of scientific rationality, there has been considerable agreement concerning whether a particular step in the game was scientific or crankish, or whether a particular gambit was played correctly or not.
>
> (ibid.; emphasis in original)

He then draws the conclusion that 'an acceptable definition of science (methodology) 'must reconstruct the acknowledgedly best gambits as "scientific": if it fails to do so it must be rejected' (ibid).

This meta-criterion for appraising methodologies is crucial because it provides the necessary link between methodology and the judgements of working scientists. Lakatos's exposition of it has been quoted at length in order to make the point that applying it to economics is less straightforward than it might appear. A necessary condition for it to work is that we can define both a 'scientific élite' and a set of scientific gambits in such a manner that there is *unanimity* amongst this élite over both whether these gambits were 'scientific' and whether they were 'applied correctly'. In the sciences with which Lakatos was concerned, this condition is easily satisfied, but in economics the case is far less clear-cut.

To see this, consider Hands's attempt to apply this meta-criterion (Hands, 1985, 1990). To avoid unnecessary controversy he considers only two major achievements: Keynes's *General Theory* and general equilibrium theory. But do even these satisfy

Lakatos's condition? Surely a major issue underlying, implicitly if not explicitly, recent methodological debates has been the question of whether the general-equilibrium paradigm has led economists astray. Whilst Hands may be right in working on the assumption that the answer to this is 'No', it is important to be clear about what is being assumed.

Sociological factors

Implicit in Lakatos's MHRP are three assumptions: that 'truer' theories will be more successful than 'less true' theories in predicting novel facts; that scientists believe (know) this; and that scientists are motivated to search for the truth. The first of these is discussed elsewhere in this chapter. If we assume that it is correct, then it is not difficult to argue for the second assumption. Problems arise, so it has been argued, with the third assumption. Colander (1990), for example, argues the following:

> Unlike the Popperian and Lakatosian methodologies, a sociological approach does not assume that scientists are searching for the truth. Truth is one of their goals, but only one; professional advancement, recognition, and wealth are others of perhaps equal or more importance. *Good science is made possible by institutional conventions that make it in scientists' interests to follow reasonable conventions that are most likely to limit subjectivity and bias.*
>
> (ibid., p. 191; emphasis in original)

This leaves open the possibility, to which attention was drawn long ago by Leontief (1971), that the structure of incentives in the profession may be such that the economics as a whole (i.e. not just the work of a few 'perverse' individuals) may move in the wrong direction. If this is the case, then we have to be careful before applying Lakatos's MHRP. For example, a Lakatosian interpretation of the new classical macroeconomics requires that, at least in the early stages when it was attracting adherents from Keynesianism, it appeared progressive. If this were not the case, this would, according to Lakatos's meta-criterion, undermine Lakatos's MSRP. If, on the other hand, we can establish that the structure of incentives in the profession during the 1970s was such that searching for truth was pushed aside in favour of lines of enquiry, we could not draw any useful conclusions concerning the validity of progressivity as an appraisal criterion.

5 WHERE DO WE GO FROM HERE?

The argument so far

In reviewing the problems various economists have found with Lakatos's methodology a number of criticisms have already been answered. There remain, however, some serious problems which need to be dealt with.

1. The concept of a scientific research programme, as defined by Lakatos, captures important aspects of economic inquiry, but it appears to be inadequate in that it fails to account for the diversity of interrelationships between theories that we observe.
2. We lack a firm epistemological foundation for the use of novel facts as an appraisal criterion.
3. The conditions required for rational reconstructions to provide unambiguous appraisals of Lakatos's MSRP are so restrictive that they are unlikely to be satisfied in economics. Rational reconstructions of the history of economic thought, however successful, are thus unlikely to provide a clear-cut answer as to the relevance of the MSRP.

Alternative responses

How should we respond to these criticisms of Lakatosian methodology? The easiest would be to abandon Lakatos's MSRP. There are, however, two major problems with this. The first is that if we want to understand economics, we need to find an alternative methodology, and it is not clear that anything better is available. If we abandon methodology altogether, we have to give up the important task of appraisal. As an example, consider Hoover's (1991) response to the difficulties of applying Lakatos's MSRP to the new classical macroeconomics. He proposes that we should view economic theories much as an anthropologist might view a society, basing his analysis on a 'kinship table', showing the relationships between members of the society: there are several families, with relationships between individuals also extending across family boundaries. The problem is whether such an approach, however well it may describe the multifarious links between various parts of the literature (and Hoover's table is excellent in this respect), can ever go beyond description. Any appraisal of theories must be on an *ad hoc* basis.

An alternative is to modify Lakatos's methodology. Remenyi (1979) has proposed modifying Lakatos's concept of the SRP by allowing for what he terms 'demi-cores': sets of assumptions which constitute a hard core for parts of the research programme. We thus have a structure of programmes and sub-programmes. Though this may work in some contexts, however, it is not clear that such an approach is an adequate response to the problems discussed above. It does not get us very far, for example, in understanding the complex set of relationships within the new classical macroeconomics that we find in Hoover's kinship table. By weakening the metaphor of the hard core, Remenyi's scheme loses much that is attractive in Lakatos's MSRP, without bringing us much by way of compensation.

Not surprisingly, in view of the nature of his criticisms, Hands chooses to modify Lakatos's MSRP by abandoning novel facts as the appraisal criterion, in favour of a broader range of criteria (such as those cited above). As regards the concept of a research programme, he is open-minded about whether a 'substantially modified version' of the MSRP might be successful, citing Remenyi's and Weintraub's work as examples. He is definite, however, that to be successful such a modified version 'must be written with the actual history of economic thought (at least the best gambits) squarely in sight' (Hands, 1985, p. 13). Putting aside the difference over the appraisal criterion, this conclusion is compatible with the following suggestion as to where we might go from here.

Given the arguments against completely abandoning Lakatos's MSRP, and given the problems involved with applying it unchanged, we are faced with the problem of how to modify it. Remenyi's strategy of complicating it seems unlikely to work, for it fails to allow for the diversity of structures we find within economics. This suggests that we should perhaps be moving in the opposite direction: that of radically simplifying Lakatos's MSRP. The suggestion offered here, therefore, is the following: (1) that we should retain Lakatos's appraisal criterion; but (2) that we should replace his concept of a scientific research programme with something much broader.

The appraisal criterion

What justification is there for retaining Lakatos's appraisal criterion? The main one is that it seems to be the criterion which economists use when asked to justify the theories they use. When neoclassical economists claim that their theories 'work', what they mean is something very close to Lakatos's novel facts. It is thus an appraisal criterion based on the judgements of practising economists.

There is also a strong 'common sense' argument in favour of novel facts as an appraisal criterion. Such a criterion is widely used in econometrics where, despite a large battery of statistical tests, it is considered necessary to do more than test a model against the data set on which it was estimated. It is thus common practice for econometricians to reserve data in order to conduct out-of-sample tests, and to test models on datasets other than those on which they were originally estimated. The defence of such procedures is that if an estimated model is a true representation of the model generating the data, its predictions will be correct (subject, of course, to random elements). If the estimated model differs from the true model we would expect there to be some set of data that it will fail to predict correctly. Out-of-sample predictive success, a criterion similar to 'novel facts', is thus a necessary condition for a model to be correct. Do the arguments that have been used against 'novel facts' as an appraisal criterion also invalidate such out-of-sample tests in econometrics? It could be argued that when we compare two models, neither of which is the true model (presumably this is almost universally the case in economics), we come up against 'second-best' problems: a 'less correct' model may perhaps predict better than a more correct one, but this raises the problem of how we measure the correctness of a model.

These arguments are not intended to deny the desirability of a more secure epistemological foundation for any criteria – that is not in dispute. The point is rather that the failure of philosophers to provide an acceptable epistemological foundation does not *necessarily* mean that it is wrong to use Lakatos's appraisal criterion.

The unit of appraisal

If we are to retain Lakatos's appraisal criterion without accepting his definition of a research programme, what should be appraised? The simplest answer would be to abandon the concept of a research programme in its entirety. Such a response would take us back to Popper who presented an appraisal criterion very close to Lakatos's (Popper, 1972, chapter 10, especially pp. 240-8). A better solution is to retain Lakatos's insight that it is not individual theories, but *sequences* of theories that should be appraised. The theories in such a sequence must, however, be related to each other in some way if the object of appraisal is not to be completely *ad hoc*. The relationship between theories that Lakatos specified was that of sharing a common set of hard-core assumptions (the heuristics of a research programme being concerned simply with protecting this hard core), but there is no reason why some other relationship should not be specified. For example, the interpretation of the 'neo-Walrasian research programme' offered in Chapter 2 is further from Lakatos's methodology than it might appear to be. The reason is that the programme's positive and negative heuristics (as defined there) do more than protect the hard core: the neo-Walrasian research programme is thus an example of a research programme defined by its heuristics as much as by its hard core.

The suggestion, therefore, is that we should apply Lakatos's appraisal criterion, and use it to appraise research programmes, where research programmes are *not necessarily* defined in terms of their hard cores. The function of the hard core in Lakatos's MSRP was to eliminate *ad hoc* sequences, but there is no reason why *ad hoc* sequences cannot be eliminated by other means. This is not to say that Lakatos's criterion of a common hard core will never be appropriate: sometimes it will be. The point is that it is not a *necessary* condition for a coherent research programme. In other words, Lakatos has provided us with just one example of what a research programme might look like.

It is natural to argue that Lakatos's methodology of historical research programmes should be used to appraise this revised version of the MSRP. The problem here is that our revised MSRP cannot possibly perform worse than Lakatos's, the reason being that any history that is compatible with Lakatos's MSRP is *a fortiori* compatible with ours. Using Popperian language, the revision we have introduced is content-decreasing, in that the set of possible histories that would lead us to reject the MSRP has been reduced. The most effective way to defend our modification, therefore, is to argue that defining research programmes in terms of sets of hard-core assumptions was *ad hoc* and that all we have done is to remove this *ad hoc* element.

6 CONCLUDING REMARKS

Lakatos's MSRP has been subjected to many criticisms, but these are not fatal. The most telling criticism would seem to be the failure of the history of economic thought to fall into the simple categories implied by Lakatos's framework. Rather than taking this as a reason for abandoning Lakatos altogether, it seems preferable to modify his MSRP to allow for a greater variety of types of research programme, retaining his appraisal criterion intact. If this suggestion were accepted, it would be natural to regard one of the major tasks facing historians of economic thought as being to discover the defining characteristics of research programmes in economics. These programmes could then be appraised according to their success in predicting novel facts. This suggestion, therefore, strengthens the connection between methodology and the history of economic thought that was already a feature of Lakatosian methodology.

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Chapter 4

Lakatosian perspectives on general equilibrium analysis*

(*Economics and Philosophy* 9(2), 1993, pp. 271–82.)

Lakatos's approach to the appraisal of scientific theories is based on the assumption that, though it may be impossible to reach a consensus on what constitutes 'good science' in general, it may be relatively easy to reach agreement on specific examples of 'good science'. There is little doubt, for example, that Newtonian mechanics or the discovery of DNA are examples of successful science. In economics, however, things are more difficult, for we should not assume that the most prestigious types of economics are necessarily successful. The chapter questions whether general equilibrium analysis should be regarded as lying in the hard core of the neo-Walrasian programme, and emphasizes the fact that theories can be appraised according to different criteria: it is quite possible that, when appraised as mathematics, general equilibrium theory appears progressive, but when evaluated as an empirical science, it does not. Theoretical progress and empirical progress are very different things.

Though the focus of this chapter is on Lakatosian methodology and the way it has been applied to a branch of economics, it also raises questions about the methodological status of general equilibrium theory, suggesting that one way to appraise it is as mathematics. There is, in principle, nothing wrong with such an approach, but it has strong implications for our view of the subject. This is an issue that is taken up again in Chapters 16 and 17, and is explored much further in Truth and Progress in Economic Knowledge *(1997).*

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1 INTRODUCTION

General equilibrium theory, as has been widely recognized, is methodologically very puzzling. It has a high status within the economics profession, and yet its empirical emptiness is recognized even by many of its supporters. Roy Weintraub's *General Equilibrium Analysis: Studies in Appraisal* (1985), which sought to provide a Lakatosian defence of general equilibrium analysis,[1] offered a solution to this paradox. His interpretation of general equilibrium analysis, however, has been challenged on several points, notably by Rosenberg (1986) and Salanti (1991). The main conclusion reached in these papers is that Lakatos's methodology of scientific research programmes, and in particular his criterion that research programmes generate novel facts, are inappropriate for appraising general equilibrium analysis.

The aim of this chapter is to provide a different perspective on this issue, less critical of the Lakatos – Popper appraisal criterion, and more critical of general equilibrium theory. In arguing the case for this perspective, more attention will be paid to Salanti than to Rosenberg. There are two reasons for this. The first is a concern to focus on issues directly related to economics and the history of economic thought, many of which are raised by Salanti's paper. In contrast, Rosenberg's critique of Weintraub, though it is clearly informed by an understanding of the economic issues, is primarily philosophical and raises issues wider than those that will be discussed here. To provide a defence of Lakatos's appraisal criterion is beyond the scope of a short paper.[2] The second reason is that in attempting to criticize Weintraub's position, Salanti paper raises a number of issues that merit further discussion.

The chapter starts by examining the ways in which Weintraub and Salanti seek to defend general equilibrium theory. It then focuses on the role of general equilibrium analysis within the neo-Walrasian research programme and suggesting an alternative to Weintraub's view.[3] The argument then turns to the question, crucial to a Lakatosian

[1] Weintraub's terminology here, using the term 'general equilibrium analysis' to refer to the literature concerned with establishing existence, uniqueness and stability of general competitive equilibrium.

[2] As Mark Blaug put the matter in an exchange with Wade Hands, 'We disagree over two things ... . We may be able to resolve the first of these disagreements in the pages of this journal but I doubt whether we will resolve the second [whether predictability ought to be the ultimate test of scientific research programmes, including economic research programmes] in this year or the next' (Blaug, 1990, p. 102).

[3] Heijdra and Lowenberg (1988) also distinguish between general equilibrium analysis and a wider research programme, choosing to refer to general equilibrium theory as neo-Walrasian, and calling the wider programme 'neoclassical'. The position adopted here is that it is useful to refer

appraisal, of whether an appropriate methodology must deliver a favourable verdict on general equilibrium analysis. Conclusions are then drawn.

2 LAKATOSIAN DEFENCES OF GENERAL EQUILIBRIUM ANALYSIS

Weintraub (1985)[4]

Weintraub claimed that

> the sequence beginning with the Schlesinger paper and continuing through those of Wald, von Neumann, Koopmans, Arrow, Debreu, and McKenzie should be recognized as a hardening of the hard core of the neo-Walrasian research program. This hypothesis makes sense of the historical record in a way no other explanation offered so far does.
>
> (Weintraub, 1985, p. 112)

He deduces from this that

> The hard core as presented can be said to have existed only as early as the early 1950s. The recognition that Arrow, Debreu, and McKenzie had accomplished a major feat was precisely the recognition that the hard core of the neo-Walrasian program was, by their work, no longer problematic.
>
> (ibid., p. 113)

This work established that propositions about maximizing agents were consistent with propositions about equilibrium. A consistency check requires the creation of a model in which a competitive equilibrium exists. The literature on the existence of a competitive equilibrium is seen as involving 'a sequence of interpretations of the terms of the hard core such that (1) each successive interpretation is manifest in a consistent model, (2) each successive interpretation contains the interpretation of its predecessor, and (3) each allows a concept uninterpreted by its predecessor to be interpreted' (ibid., p. 117). Such a sequence of models exhibits theoretical progress, to be evaluated as one would evaluate progress in mathematics (ibid., p. 117).

(Continued from previous page)
to a neo-Walrasian programme, defined as much by its modelling strategy (by its heuristics) as by its hard core assumptions, which is different from other 'neoclassical' research programmes, such as the Marshallian. See Backhouse (1991) for a discussion of this programme which makes it clear that it need not be equated with general equilibrium analysis.

[4] When citing Weintraub's views on appraising economic theories the dates are important, his attitude having changed very substantially between 1985/1988 and 1991.

Seeming nonchalance about empirical content is instead a sensible division of labour between hard core and protective belt. One uses criteria appropriate to mathematics, such as those developed in Lakatos's *Proofs and Refutations* (1976), to measure 'the growth of knowledge associated with the hardening of the hard core of the neo-Walrasian program', and one uses falsificationist techniques to evaluate work in the belts of the program.

> To ask about the falsifiability of the Arrow–Debreu–McKenzie model is not to be hard-headed, positivistic or rigorous. It is to be confused.
>
> (ibid., p. 119)

Hard core propositions may be fixed, but their interpretation (e.g. what is an 'agent'?) is not. Theoretical work reinterprets the hard core propositions. There is no fixed referent for the term 'general equilibrium theory' (ibid., p. 122).

Salanti

Salanti's main criticism of Weintraub's defence of general equilibrium analysis is that it rests on a confusion concerning the nature of the hard core of the neo-Walrasian programme.[5] The literature on the existence of competitive equilibrium with which Weintraub is concerned is based on the hard core, but it is particular models that are analysed, and these models include additional assumptions. Salanti chooses to characterize these as 'heuristic' assumptions: assumptions which are adopted as one stage in a method of successive approximations.[6] His criticism is that 'it remains somewhat of a mystery how theories in the protective belt ... could be "derived" from a theoretical construction entirely based on "axioms" that, at most, may be regarded as "heuristic" assumptions' (Salanti, 1991, p. 228). The problem, as Salanti sees it, is that necessary domain assumptions (which specify the set of events the theory is intended to explain) are made only in the theories in the protective belt, which means that their adequacy has to be checked in each single case.

Whilst Salanti, correctly, wishes to draw a sharper distinction between the neo-Walrasian hard core and the models that are the subject of general equilibrium analysis, he remains close to Weintraub in regarding them as playing a 'fundamental' role in the neo-Walrasian programme, using the term 'fundamental' in the sense

[5] He argues, following Rosenberg and many others, that in using Lakatos's MSRP to appraise theories in the protective belt, Weintraub fails to realise 'the importance of all the well-known difficulties concerning the testability of economic theories' (Salanti, 1991, p. 225).

[6] The terminology used here is taken from Musgrave (1981).

defined by Green (1981). We need to examine this claim in more detail. Green starts off by defining 'fundamental' theory to mean no more than 'pure' theory (ibid., p. 5), but later on defines a 'fundamental economic theory' as 'a body of propositions which describe, in general terms, the relation between institutional structure and individual behaviour', giving as paradigmatic examples game theory and general equilibrium theory (ibid., p. 7). He goes on to suggest ways in which such theories are useful, such as suggesting restrictions on parameter values, reconciling apparently divergent theories and providing informal criteria of coherence. However, despite the use of the word 'fundamental', neither Green nor Salanti shows that general equilibrium analysis is fundamental in the sense that it is necessary for theories in the protective belt. Given this, it could be argued that the term 'fundamental' is inappropriate: 'pure' or 'abstract' would be less misleading.[7]

3 THE PLACE OF GENERAL EQUILIBRIUM ANALYSIS IN CONTEMPORARY ECONOMICS

Weintraub (1985, chapter 2) argues that general equilibrium analysis is fundamental to the hard core in the sense that it proves that the concept of general equilibrium, which is the basis for much work in the protective belt, is coherent. Such a defence has some force, but is subject to an important qualification. Much work in the protective belt of the neo-Walrasian programme goes beyond the models used in general equilibrium analysis, in that it uses models which are not special cases of these models – for many such models, the results of general equilibrium analysis are irrelevant.[8]

As Weintraub has pointed out, to obtain the models that form the subject of general equilibrium analysis it is necessary to supplement the neo-Walrasian hard core with further assumptions: in *The Theory of Value,* for example, Debreu assumes, amongst other things: (1) a complete set of futures markets; (2) full information; (3) the absence of money. These assumptions could be regarded as a set of domain

[7] It is worth noting that, though Green cites general equilibrium and game theory as paradigmatic examples, many of his examples of 'useful' 'fundamental' theory have nothing to do with general equilibrium analysis. The work of Cournot on oligopoly and Spence on signalling falls well outside the domain of the Arrow-Debreu-McKenzie model.

[8] Any application of general equilibrium theory must also confront the problem of stability. As Hahn has put it, 'it was clear from the beginning that we have only half a theory anyway since there was (and is) no rigorous account, derived from first principles, of how the Arrow–Debreu equilibrium comes to be established' (Hahn, 1984, pp. 308–9).

and negligibility assumptions.[9] They specify, for example, that economies with incomplete futures markets, incomplete information or money, fall outside the domain of the theory. It is true that they define a domain which does not include the real world, but that is precisely the critics' point: they are still domain and negligibility assumptions, comparable to those made in 'applied' theory. Debreu's theory, therefore, should be placed alongside other theories in the protective belt.

This argument can be illustrated using a diagramatic treatment. Weintraub's position is summarized by Figure 4.1 (taken from Weintraub, 1985, p. 134). His placing of general equilibrium *within* the hard core reinforces Salanti's criticism that he does not adequately distinguish the two. The arguments of the previous paragraph suggest that a more appropriate picture is Figure 4.2, which places general equilibrium analysis within the protective belt.[10] In Figure 4.2, general equilibrium analysis has no privileged status.[11]

[9] Referring to general equilibrium theory as well as to other theories, Debreu has pointed out that 'When it acquires an axiomatic form, its explicit assumptions delimit its domain of applicability' (1991, p. 3).

[10] Weintraub places 'monetarism' within the neo-Walrasian programme. Given the importance to monetarism of Milton Friedman, whose economics is not Walrasian, this seems hard to defend. We have thus replaced monetarism with the new classical macroeconomics. The field of finance has been added, as an example of a field which could be seen as empirically progressive, and which has very significant overlaps with general equilibrium analysis. The choice of examples, however, is not important to the present argument.

[11] It is, of course possible that general equilibrium analysis overlaps one or more other elements in the protective belt. Insofar as it does this, it might be possible to argue that it could be regarded as 'fundamental' to those branches of the subject.

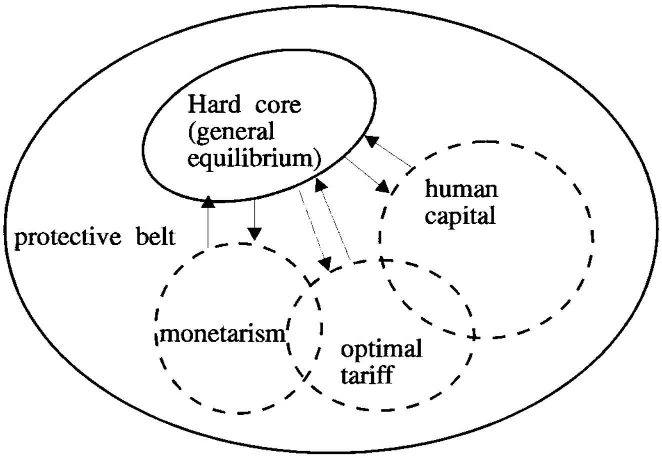

Figure 4.1: **Weintraub's view of the neo-Walrasian programme**

The view of the neo-Walrasian programme offered in Figure 4.2 has the merit of making clear that the models analysed in the literature on the existence of general competitive equilibrium are models alongside other neo-Walrasian models. Some of these will be 'general' equilibrium models, others only 'partial' equilibrium models. It also provides a useful framework for thinking about the relationship between general equilibrium analysis and the neo-Walrasian programme. Firstly, it makes clear that, if we assume that it is only other parts of the protective belt that generate novel facts, the relevance of general equilibrium analysis depends on the importance of the overlaps with other parts of the protective belt. In contrast, Weintraub's placing general equilibrium analysis within the hard core implies overlaps with *all* fields of the neo-Walrasian programme. Secondly, it provides a way of thinking about Weintraub's defence of general equilibrium analysis in relation to some of the criticisms that have been levelled at it.

The crucial issue is the strength of the links represented by the arrows. Weintraub was arguing that, of the arrows leading *to* the hard core, it was that from general equilibrium analysis that was, and should be, crucial. If this were not the case, the story of the neo-Walrasian programme would be seriously incomplete without consideration of fields other than general equilibrium analysis.[12] Salanti (1991, pp.

[12] This is, however, not the only way the story could be told. Backhouse (1991) interprets the hard core in such a way as to imply feedback from macroeconomic concerns to the hard core, implying that to tell the story of neo-Walrasian economics more fully it is necessary to consider other fields.

28–30) cites evidence that the links from other fields to the hard core are weak or non-existent. In contrast to Weintraub, many critics of general equilibrium analysis argue that the links from other fields to the hard core should be stronger.

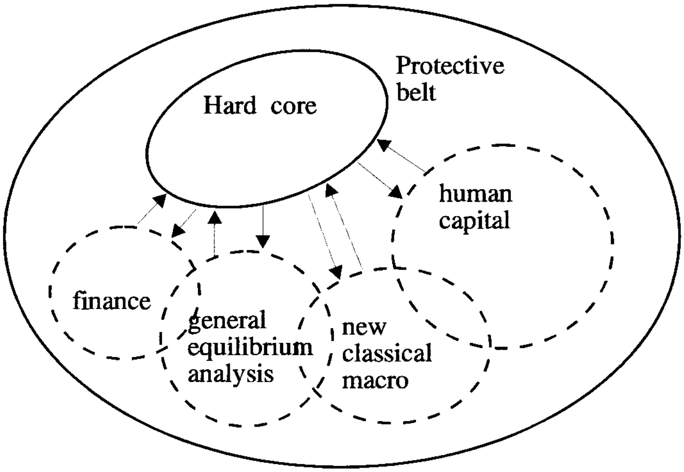

***Figure 4.2*: An alternative view of the neo-Walrasian programme**

4 SHOULD WE *ASSUME* THAT GENERAL EQUILIBRIUM ANALYSIS IS GOOD ECONOMICS?

Salanti's conclusions are strongly influenced by his apparent assumption that general equilibrium analysis is good economics:[13] such an assumption provides the only justification for his claim that because of 'the impossibility of appraising them by means of any version of falsificationism', 'the methodologist must perforce seek some specific methodological accommodation for this part of economic theory' (Salanti, 1991, p. 221). Without the assumption that general equilibrium analysis is good economics such a statement is indefensible. Such an assumption also explains his comment that 'applications' such as neoclassical growth theory are 'neither the most suited for appraisal along Lakatosian lines nor, even more importantly, the most likely to emerge from such a scrutiny as empirically progressive' (ibid., 1991, p. 231). It also explains his conclusion that 'Each of these different ways of practicing

[13] In this chapter 'good' is defined simply in terms of satisfying whatever appraisal criterion we are employing.

economics [from pure theory to applied work] is likely to require a methodological assessment of its own' (ibid., 1991, p. 232).

If Salanti were making this assumption simply as an initial working hypothesis, in order to give general equilibrium analysis a fair hearing, that would be fine. However, there is no indication that it is an assumption that he regards as in any way open to question: it would appear to be part of the hard core of his methodological research programme.

In taking such an attitude, Salanti would appear to be following Lakatos's methodology of *historical* research programmes, rejecting Lakatos's methodology of scientific research programmes on precisely the grounds on which Lakatos argued it should be appraised. Salanti would seem to be following Lakatos's advice that,

> a rationality theory — or demarcation criterion — is to be rejected if it is inconsistent with an accepted 'basic value judgement' of the scientific élite.
> (Lakatos, 1971, p. 124)

Lakatos, however, is very specific about what he means by this statement. He argues that, in the subjects with which he is concerned, there is 'considerable agreement' about whether particular scientific achievements were played correctly or not. If we are to apply his methodology of historical research programmes, therefore, we must be able to define (a) the scientific elite and (b) a set of scientific gambits, in such a manner that there is general agreement, amongst this elite, that these gambits were scientific and applied correctly. It is, certainly in the 1990s, sheer wishful thinking to suggest that general equilibrium analysis falls into this category of acknowledgedly best gambits. The number of eminent critics is too large. To give one example, Frank Hahn, surely one of the relevant élite, has argued that none of the crucial questions facing the discipline can be answered by 'the old procedures' of axioms and theorems (Hahn, 1991). Rather than make a sweeping judgement concerning whether or not an appropriate methodology should reconstruct general equilibrium analysis favourably, a more careful historical account is needed.

5 THE HISTORICAL DEVELOPMENT OF NEO-WALRASIAN PROGRAMME

Weintraub is entirely correct to emphasize the need to appraise general equilibrium theory in relation to the historical development of the neo-Walrasian programme. However, whilst he provides a close analysis of the 'hardening' process, he does not provide a close historical analysis of either how the neo-Walrasian programme developed as a whole, or of how the place of general equilibrium analysis within that programme changed.[14] Weintraub (1988) provides one example to demonstrate that the programme is empirically progressive, and whilst such an example is important, it is no substitute for a historical analysis covering the entire relevant period.

Providing a comprehensive account of the evolution of the neo-Walrasian programme would be an enormous task, so all that can be done here is to sketch, as a plausible conjecture, what such an account might show. The 1950s were a period of great optimism, as regards both neo-Walrasian economics, with the programme being extended to, amongst other areas, macroeconomics. General equilibrium analysis was, in this period, believed to be laying the foundations for microeconomics in just the way that Weintraub and Salanti assume. The problems of existence and stability had not been solved, but they would soon be solved. Around 1960, however, things began to change. It was made clear that there were no general stability results to be obtained, and that major extensions to the domain of the Arrow–Debreu–McKenzie existence proofs were not going to be found. In addition, the growth of game theory, whilst it could be used to gain new insights into the nature of competitive equilibrium, provided an alternative conceptual basis for economics, whilst remaining within the neo-Walrasian programme.

More worrying for general equilibrium analysis, the progressive elements of the neo-Walrasian programme were increasingly the parts that lay outside the Arrow–Debreu–McKenzie framework. Economists became less concerned to derive formal results, and more willing to work with specific models in order to tell stories that could not be handled within a formal, general equilibrium setting (dealing with problems of incomplete information, externalities, moral hazard and so on). Thus we might tell a story in which neo-Walrasian economics was progressive throughout the period from the 1950s to the present, but in which the role of general equilibrium analysis changed from being 'fundamental' to a large part of the subject, to one where it was 'fundamental' to less and less important parts. In Figure 4.2, we might

[14] In so far as he was concerned with the literature which culminated in Debreu (1959), this may have been justifiable, but when we appraise general equilibrium theory today, we are concerned with its development over a longer period. It should be mentioned that Weintraub (1991) does provide a broader perspective on the historical development of general equilibrium analysis, but this does not extend to its relation to applied fields.

say that the important parts of the programme (the parts that were producing corroborated novel facts) increasingly lay outside general equilibrium analysis.

This account is very general, and probably vulnerable at many points. It is offered simply to make the point that such an account, which is not implausible, casts a different light on the subject. If such an account has any substance, it is necessary to tell the wider story to understand the evolution of the hard core, and hence to appraise the role of general equilibrium analysis within the programme.

6 CONCLUSIONS

There are several points on which Weintraub and Salanti agree, and which should be reiterated.

1. Good mathematics is not a sufficient condition for a body of analysis to be good economics.
2. The precise nature of the linkages between the hard core and the protective belt is very important.
3. The relationship between hard core and protective belt may simply be heuristic.

On other points, however, we wish to part company with them:

1. It is wrong to assume that general equilibrium analysis is good economics — the purpose of our methodological inquiries is to ask whether or not it is.
2. General equilibrium analysis is much less fundamental to the neo-Walrasian research programme than either Weintraub or Salanti suggests. Whereas Weintraub places it in the hard core, and Salanti suggests seeing it as a demi-core in the sense of Remenyi (1979), we see no reason not to place it in the protective belt, with all that this implies for the way it is appraised.

If we part company with Weintraub and Salanti on these points, the way is open for a joint appraisal of general equilibrium analysis and the neo-Walrasian research programme which falls in between the appraisals offered by Weintraub and Salanti. Following Weintraub (1985), we can argue that the neo-Walrasian research programme should be appraised according to its ability to predict novel facts and the extent to which these are corroborated.[15] On the other hand, we should not

[15] The concept of a 'novel fact' has been defined in a variety of ways and could be regarded as problematic. For the present argument, however, the precise definition of the term does not matter.

regard general equilibrium analysis as occupying a uniquely privileged position within the programme.[16]

If we accept this position, we can appraise general equilibrium analysis in two ways. We can look at specific theories, appraising them as economics, according to their success in predicting novel facts. Though it may have appeared progressive in the 1950s, it no longer looks that way now. Alternatively, we can argue that general equilibrium analysis simply provides tools, concepts and theorems that are used elsewhere in the programme, and that, like chaos theory or catastrophe theory, it should be judged as mathematics. There is nothing inconsistent in saying that when judged as mathematics, whether according to the criteria of Lakatos's *Proofs and Refutations* or any other, it exhibits progress, but when judged as economics it does not.

Such a conclusion, claiming that the neo-Walrasian programme has been progressive, but that general equilibrium analysis has become a peripheral part of that programme, makes sense of much historical evidence. It is consistent with the conclusion reached by Ingrao and Israel (1990) that general equilibrium theory has failed according to its own criteria: that the general results concerning existence, uniqueness and stability which were the programme's goals, have been shown to be unattainable; a conclusion reinforced by Hahn's conclusion, cited above.[17] A division of labour whereby developments in the hard core proceed without reference to the protective belt, would appear to be undesirable. As Weintraub has emphasized, the models that are the subject of general equilibrium analysis are instantiations of the hard core. But then so too are the models used in 'applied' fields. One might argue that the evolution of the hard core should be governed primarily by progressive areas of the programme.[18]

What are the implications of this perspective for Lakatos's methodology of scientific research programmes? If we accept, with Rosenberg and Salanti, that

[16] In placing general equilibrium analysis in the protective belt, and pointing out overlaps with other branches of the program I am suggesting that the structure of research programmes in economics may be more complicated than Lakatos suggests (cf. Chapter 3 above; Hausman, 1992, chapter 6). However, I would argue that, though we should be open to other ways of characterizing research programmes, the Lakatosian scheme captures enough of what is going on to be useful.

[17] For a discussion of Ingrao and Israel (1990) in the context of Debreu (1991) and Hahn (1991), see Backhouse (1994).

[18] Though he is not arguing from a Lakatosian perspective, this conclusion is consistent with McCloskey's criticism of economics for having adopted 'the intellectual values of the Math Department – not the values of the Departments of Physics or Electrical Engineering' (1991, p. 8).

general equilibrium analysis should be appraised as mathematics, are we conceding the case for methodological pluralism? The answer is that we are not. There is nothing inconsistent in saying that when appraised as mathematics, general equilibrium theory exhibits progress, but appraised as economics, on the assumption that economics is concerned with the real world, it does not exhibit progress. We should not follow Debreu (1991) in abandoning the notion that economics is an empirical science, for the appraisal of which Lakatosian, or other empiricist, appraisal criteria are appropriate.

Rather than argue that relaxing our appraisal criterion, and abandoning the notion that good economics has empirical content, we should instead recognize that general equilibrium analysis, along with much other mathematical economics, is mathematics and should be regarded as such: it should not be confused with economics 'proper'. This is not to belittle it. Mathematics is important, not only for providing methods of argumentation, theorems and so on, but also for its heuristic power. We should, however, not conclude that general equilibrium analysis has a role any more privileged than, for example, time-series econometrics, a branch of mathematics which has had an important impact on our conception of certain economic phenomena.

Lest this seem a dangerously unorthodox and destructive attitude, it is worth noting that it is exactly the attitude of one of the greatest economists of the past century, Alfred Marshall, who wrote:

> That part of economic doctrine, which alone can claim universality, has no dogmas. It is not a body of concrete truth, but an engine for the discovery of concrete truth, similar to, say, the theory of mechanics. ... But I conceive no more calamitous notion than that abstract, or general, or 'theoretical' economics [is] economics 'proper'.
>
> (Marshall, 1925, pp. 159, 437)

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Chapter 5

The Lakatosian legacy in economic methodology*

(*New Directions in Economic Methodology,* edited by Roger E. Backhouse. London: Routledge, 1994, pp. 173–91.)

This chapter represents a second attempt to respond to critics of Lakatosian methodology. Three themes are developed much further than in Chapter 2. The first is the appeal of Lakatos to economists committed to economics as an empirical science. Milton Friedman and David Hendry disagree fundamentally over how empirical research should be conducted, but both accept that the prediction of novel facts is the criterion by which theories should be judged. Hendry goes even further in arguing that Lakatosian methodology provides the basis for his notion of a progressive research strategy. They emphatically do not endorse Lakatosian ideas because they offer a soft option. The second theme is the role of the economic methodologist. It is argued that seeking to understand contemporary economics ('recovering practice' to use Neil De Marchi's expression) has to be combined with appraising what economists are currently doing. Methodologists do not have magic formulae with which they can say which lines of enquiry will succeed and which will not, but neither should they abandon appraisal. Though others (e.g. Hausman and Kincaid – see Chapters 16–18 in this volume) reach the same position via other routes, I argue that it follows naturally from a Lakatosian starting point.

Lakatos is currently unfashionable amongst economic methodologists. The reasons for this are understandable. Many of the ideas that make up his methodology of scientific research programmes can be found elsewhere in the philosophy of science literature. The prediction of novel facts as an appraisal

* I am indebted to Tony Brewer, Wade Hands and Dan Hausman for reading a draft of this chapter and providing useful comments. They bear no responsibility for any remaining inadequacies.

criterion, for example, has a history going back at least to Herschel's work in the 1830s. In addition, Lakatos remains too close to Popperian falsificationism with its excessively radical rejection of the possibility of inductive knowledge. What this chapter, like the preceding ones, suggests, however, is that, for all the problems with Lakatosian methodology, it can provide a useful starting point in thinking about economics: he is right in his empiricism, in maintaining the tension between description and appraisal, and in emphasizing the importance of the growth of knowledge.

1 INTRODUCTION

Lakatos's methodology of scientific research programmes (MSRP) played a crucial role in the upsurge of interest in economic methodology in the 1980s (see Backhouse, 1994). Though there had been earlier explorations of the relevance of Kuhnian and Lakatosian ideas to economics, the key work was arguably the volume edited by Latsis, *Method and Appraisal in Economics* (1976). In this volume a number of distinguished economists grappled with the issues of how economic knowledge grew and how it might be appraised in a way that made earlier debates on methodology seem almost arid in comparison. For about a decade after this book, Lakatosian methodology, augmented by the Popperian falsificationism advocated by Blaug, formed the major focus of debates in economic methodology. From around the middle of the 1980s, however, interest in and support for Lakatosian ideas waned to such an extent that, referring to a conference organised in 1989 to reassess Lakatosian methodology, one of the organizers expressed the view that,

> I was personally taken aback by what can only be described as a generally dismissive, if not hostile, reaction to Lakatos's MSRP. Of the 37 participants, I estimate that only 12 were prepared to give Lakatos a further run for his money and of the 17 papers ... only five were unambiguously positive about the value of MSRP.
>
> (De Marchi and Blaug, 1991, p. 500)

The judgement of many leading writers on economic methodology is that Lakatosian methodology has little to offer (see, for example, Hausman, 1992; Rosenberg, 1992; Hands, 1993a; and several chapters in Backhouse, 1994). At the same time, however, many economists, as will be explained below, still find Lakatosian ideas helpful.

The aim of this chapter is to reflect on the Lakatosian legacy in economic methodology, asking how far Lakatos's MSRP can be defended, focusing on a number of issues: how far have criticisms undermined Lakatosian methodology? What is its attraction for economists? Where should economic methodology be going, and how does this relate to the Lakatosian perspective? In answering these questions particular attention will be paid to the role of the metahodologist.[1]

2 LAKATOSIAN METHODOLOGY

The methodology of scientific research programmes

Lakatos's MSRP (Lakatos, 1970)[2] involves appraising scientific research programmes in terms of their ability to successfully predict novel facts.[3] A *scientific research programme* is defined by sets of rules, or heuristics, governing research within the programme. These fall into two categories. *Negative heuristics* direct researchers not to question the *hard core* of the programme – the set of assumptions regarded as irrefutable by anyone working within the programme. Thus if 'Agents optimize subject to constraints' is a hard-core assumption, the corresponding negative heuristic would be 'Do not construct theories in which irrational behaviour plays a significant role'. *Positive heuristics,* on the other hand, provide contain rules by which research is to be conducted. These rules lay out the strategy by which anomalies are to be dealt with, and how the research programme is to be developed. They are concerned with the programme's *protective belt:* the assumptions and procedures which need to be made to apply the hard-core assumptions to specific problems, but which can be modified without calling the programme into question. Examples of positive heuristics might be 'Explain Pareto-inefficient allocations of resources by finding missing markets', or 'Start by assuming identical agents and full information, dropping these assumptions later on'.

Research programmes are, however, not static. New facts are discovered, new problems emerge, and as a result modifications have to be made to the protective belt. Lakatos, therefore, argues that research programmes should be appraised according to the way they evolve over time. If the modifications made to a programme

[1] For other attempts along similar lines, see de Marchi's 'Introduction: rethinking Lakatos' and Blaug's 'Afterword' in de Marchi and Blaug (1991). Some of the issues are explored in Chapter 3 in this volume, but that chapter did not go far enough.

[2] Though his work on mathematics (Lakatos, 1967) was arguably more original, it had very little impact on economists and will not be considered here.

[3] Lakatos also provides a meta-methodology for appraising methodologies, his methodology of *historical* research programmes. This, however, is best considered in the next section.

do no more than explain away new evidence, he terms the programme *degenerating*. If, on the other hand, modifications not only explain anomalies but also lead to the prediction of new facts – facts the modifications were not designed to explain – Lakatos calls the programme progressive. It is *theoretically progressive* if new facts are predicted. It is *empirically progressive* if these new facts are corroborated.

Finally, research programmes do not exist in isolation. There will typically exist rival research programmes. Appraisal, therefore, involves choosing between competing research programmes. Lakatos's claim is that scientists should abandon degenerating research programmes in favour of progressive ones. One of the problems with this criterion, however, is that research programmes may go through progressive and degenerating phases. One might, for example, argue that Keynesian economics was progressive in the 1940s, the novel facts it predicted including the multiplier and the consequences of fiscal policy changes, but degenerated in the 1960s, the modifications being introduced to explain inflation not leading to the prediction of new, unexpected facts. It is even possible that programmes may degenerate for a while, but later become progressive. Rational scientists need to be forward-looking, and the fact that a programme is less progressive than a rival does not mean that it will continue to be so in future. It may be rational, therefore, to allow fledgling research programmes time to develop.

When taken in isolation, few components of Lakatos's MSRP were original with Lakatos. A scientific research programme is very similar to what Kuhn termed a 'disciplinary matrix', the collection of assumptions and procedures that define a period of normal science.[4] Lakatos has narrowed the definition slightly, and has suggested that science will be characterized by competing programmes where Kuhn argued that a single disciplinary matrix would typically be dominant at any time, but beyond that there is little difference. As for the appraisal criterion, that a programme successfully predict novel facts, this has a long history going back at least to Whewell and Herschel in the mid-nineteenth century. The Lakatosian account of how one programme supersedes another places greater emphasis on rationality than does Kuhn's account of paradigm-shifts, and the irrational, 'Gestalt-shift' aspect of the process is completely absent. In both the Kuhnian and Lakatosian frameworks, however, the main force for change is the need to modify theoretical frameworks to take account of anomalies and deal with new problems.

[4] This is one of the senses in which Kuhn uses the term 'paradigm'.

The main source of Lakatos's ideas, however, is Popper. Though, with important exceptions discussed in Backhouse (1994), Lakatos became influential in economic methodology before Popper, Lakatos presented his methodology of scientific research programmes as a natural development of Popper's falsificationism. Popper, according to Lakatos's interpretation, started out a naive falsificationist, stressing the asymmetry between confirmation and refutation of a theory (one observation is sufficient to refute a theory whereas no finite number of observations can confirm it with complete certainty). In response to the problems inherent in naive falsificationism, however, Popper moved on to a position Lakatos characterized as sophisticated methodological falsificationism. Greater stress was laid on predicting novel facts, and less on falsifiability. Popper even wrote about metaphysical research programmes (1983, pp. 189–93).[5]

When compared with certain of Popper's writings, Lakatos seems hardly to go beyond Popper (see for example Popper, 1972, pp. 240–8). The MSRP appears to represent a minor variation on what Lakatos termed Popper's sophisticated methodological falsificationism, distinguished from the latter as much by Lakatos's new terminology as by its content. Lakatos, however, altered the emphasis is some key respects. Notably, his concept of a research programme involves placing certain assumptions (the hard core) beyond criticism. Though Popper saw the heuristic power of metaphysical hypotheses, this is very un-Popperian.[6] Furthermore, though he still thinks of empirical content in a Popperian way, as the set of potential falsifiers, he places greater emphasis on corroboration than on falsification. Progressive research programmes are ones whose predictions are corroborated.

Why have economic methodologists turned against the MSRP?

Lakatos's MSRP has been criticized at a number of levels. First there are criticisms of the concept of a research programme, perhaps the most distinctively Lakatosian aspect of his methodology. Second there are objections to the Popperian epistemology out of which Lakatos's methodology arose, and of which it forms a part. Finally, there is the argument that it is pointless trying to provide *any* general philosophical analysis of how scientific knowledge evolves. These will be considered in turn.

(1) Perhaps the most direct criticism of the methodology of scientific research programmes is the argument that Lakatos's definition of a research programme in

[5] Though published in 1983, this was written in the 1950s.
[6] Cf. Boland (1994).

terms of an invariant hard core is too narrow: that research programmes in economics need to be characterized in more complex ways so as to allow for change over time.[7] A good example of such criticism is provided in Hoover (1991) who argues that the new classical economics (which most economists would think of as a coherent, well-defined research programme) cannot be characterized in terms of an invariant set of hard-core assumptions. One of the most persuasive attempts to define a research programme in Lakatos's sense is the neo-Walrasian programme outlined by Weintraub (1985). This programme, however, is defined by assumptions and heuristics that are primarily methodological (commitment to rational behaviour and the use of optimizing models) with the result that its hard core has little economic content. It is a programme held together by modelling strategy as much as anything else. As a result the neo-Walrasian research programme thus has a character very different from those postulated for physics (such as Newtonian mechanics) where the hard core typically includes some substantive hypotheses (such as Newton's laws of motion).

There are additional, though related, problems concerning the overlaps between programmes. In economics there is, for example, a strong case for speaking of a neo-Walrasian research programme, dominated by a commitment to mathematically rigorous, formal analysis of the consequences of individual optimizing behaviour. Much post-war macroeconomics clearly forms part of such a programme: macroeconomists have sought to provide rigorous micro-foundations for macroeconomic theories.[8] Milton Friedman's work, first on the consumption function and later on the expectations-augmented Phillips curve, forms an indispensable part of the history of such a programme. Equally, it can be argued that there is a Chicago programme, defined along the lines outlined by Reder (1982) in terms of commitment to the assumption that the world is approximately Pareto-efficient, and that Friedman is a key figure in this programme. However, it cannot be argued that Chicago economics forms a sub-programme within neo-Walrasian economics: the commitment to formal modelling and mathematical rigour is missing. Friedman's 'Marshallian' methodology, stressing the importance of empirical evidence and simple models, is clearly not Walrasian. There is thus a strong case for speaking in terms of two overlapping programmes, with work conducted within one programme providing a crucial input into another programme. Research programmes are thus

[7] These arguments are also discussed in Chapter 3 in this volume.
[8] This is argued in detail in Chapter 2 in this volume.

not self-contained enterprises. Whilst it would be possible to argue that at least one research programme is inappropriately defined, it is perhaps more persuasive to argue that this example shows the limitations of the concept of a research programme as defined by Lakatos.

This conclusion that interdependence between research programmes is an essential feature of any analysis of economics is reinforced by the argument that criticism of rival programmes is frequently important in a programme's development. Hoover (1991) has cited the importance of the Lucas critique for the new classical macroeconomics. Steedman (1991) has emphasized the importance for Sraffian economics of its critique of neoclassical theory. The conclusion both Hoover and Steedman reach is that Lakatos's methodology of scientific research programmes is unhelpful in trying to understand the relationships between different economic theories.

(2) Lakatos's methodology of scientific research programmes is also vulnerable to criticisms of the Popperian epistemology on which it is based. Two such criticisms will be considered here. The first is the argument that Popper's rejection of induction as a principle underlying knowledge is taken too far. His argument was that, however many observations we have, we can *never* be sure that the next observation is not going to disprove a theory. For Popper it followed that *all* knowledge is provisional and uncertain, and that we can never have evidence in favour of a theory. It can be argued (e.g. Hausman, 1992) that we *do* know some things, and that it is wrong to reject induction completely.

Hands (1991a) has taken this argument further by arguing that there is a close link between prediction of novel facts as an appraisal criterion and Popper's attempts to solve the problem of induction. Popper and Lakatos are both scientific realists, wanting to show that scientific theories could, if developed in accordance with their methodologies, become closer to the truth. Popper noted that rejecting theories which fail severe tests reduces the falsity content of science, whilst the requirement that theories have interesting, testable consequences should increase its truth content. The prediction of novel facts is thus linked to the aim of increasing the 'verisimilitude' or truthlikeness of scientific theories. Popper's formal theory of verisimilitude has, however, serious flaws. Without it, Hands has argued, there is no reason to attach special significance to the prediction of novel facts – Lakatos's appraisal criterion is left hanging in the air.

The second main criticism of the Popperian framework is that it is too narrow. Mäki (1990) has criticized what he calls the 'Popperian dominance', by which he means not that economic methodologists advocate Popperian ideas, but that methodological discussion has been dominated by a narrow range of issues:

> epistemological questions related to rational theory choice or rational theory development, formulated in the dynamic but anti-inductivist and asocial framework of Karl Popper or Imre Lakatos, have dominated the field.
>
> (ibid., p. 79)

Similarly, de Marchi (1992, p. 3) blames the narrow range of questions with which economic methodology has been concerned is the result of the acceptance of Popper's notion that science results from following certain rules. For Popper it is adherence to certain rules that guarantees that false claims will be exposed.

Of the many issues Mäki and de Marchi see as having been neglected, one is the possible significance of the context of discovery. Fundamental to Popper's work is the idea that there is a clear distinction between the contexts of discovery and justification, and that the former forms no part of philosophy. The basis for this is the argument that the way in which an idea is discovered is irrelevant to its truth: it is relevant to psychology but not to the logical analysis of theories, discovery involving an irrational element, creative intuition, which is not susceptible to logical analysis (Popper, 1934/59, chapter 1). The sociology of science is thus neglected as being concerned simply with this irrational side of science. This view is too narrow, for three reasons. The first is that the study of algorithms and methods for generating theories is just as much a philosophical question as the study of how theories can be justified. The second is that ruling out these lines of enquiry means that methodologists are handicapped in their attempts to understand what economics is like. The third and perhaps most important reason is, it has been argued (de Marchi, 1991, 1992), that the context in which ideas are discovered can be relevant to their appraisal. For example, it is impossible to evaluate the results of econometric work without knowing the beliefs and convictions of the economists undertaking the work (de Marchi, 1992, pp. 6–7).

(3) Finally there is the argument that it is pointless to look for general philosophical principles that can be used to appraise scientific theories. The main work on which such arguments are based is Rorty's *Philosophy and the Mirror of Nature* (1980). In this book Rorty sought to undermine the notion that philosophy was a discipline which stood above other disciplines and which could pass judgement on them. His argument that we should cease to think of '"knowledge" as something about which there ought to be a "theory" and which has "foundations"' (ibid., p. 7). This view,

he argued, rested on the notion of the mind as a mirror containing representations, some accurate, some inaccurate, of reality. The notion that philosophy could provide criteria for appraising the accuracy of such representations rested on the (indefensible) assumption that philosophers had privileged access to the truth. Instead, he contended, we should think of knowledge as pertaining to a conversation – as a matter of social practice – as socially constructed.

For Rorty, such arguments undermined the project of philosophy. Others have used similar arguments to undermine what they see as the privileged status of other disciplines. It has been argued that it does not make sense to look for an account of interpretation in general, for that would imply that the literary theorist had privileged access to the meaning of texts. Meanings are socially constructed – the property of interpretive communities. Science, too, has been criticized in this way. The sociology of scientific knowledge literature approaches scientific knowledge as produced by scientists' social practices (see Hands, 1994). The question of whether scientific theories are true (in the sense of corresponding to reality) is not asked, for, in the absence of any privileged access to knowledge, it is not seen as a question to which a meaningful answer can be provided.

'Constructivist' or 'postmodernist' arguments of this nature have been used to undermine the project of methodology, of which Lakatosian methodology is a part.[9] McCloskey (1986) and Weintraub (1989) have argued explicitly that methodology (in the sense of normative methodology) is a pointless exercise. The conclusion has been drawn that in writing the history of economic thought, we should not ask about whether or not there has been progress, but that we should provide accounts of the social processes underlying the construction of economic knowledge (for example, Weintraub, 1991). The tension between positive and normative methodology that we find in Lakatos has been resolved by completely abandoning any normative aims.

Why does Lakatos's MSRP appeal to economists?

Given that Lakatos's methodology of scientific research programmes has been criticized so strongly, why have economists found it so attractive? One suggestion has been provided by Hands (1993b). His claim is that 'The places where Lakatos differs from Popper are exactly the places where Lakatos is likely to win the favour of economists since these happen to be areas where there is substantial tension between falsificationism and the actual practice of economics' (ibid., p. 68). These

[9] Hands (1993a, chapter 11) provides a survey of such positions which draws finer distinctions between different views than is possible here.

include: the existence of unfalsifiable, metaphysical hard cores; the preference for corroboration rather than falsification; and the importance attached to theoretical progress. Where Lakatos is closest to Popper, on the other hand, economists are most likely to part company with him. In other words, Hands is suggesting that Lakatosian ideas are likely to appeal to economists because they are 'softer' than Popperian falsificationism, and because they can be used to defend economists' existing practices.

In a similar vein, de Marchi (1991, pp. 2–6) suggests that Lakatos's MSRP captures a number of features that are also attractive to mainstream economists: that economics is rational, rationality being defined in terms of progress, and that it is research programmes, not individual theories, that should be appraised. Citing Latsis and Rosenberg, de Marchi argues,

> Lakatos holds certain attractions for economists precisely because he offers a less bizarre-sounding replacement for Friedman's unrealism-of-assumptions methodology to justify their convictions in the face of falsifying evidence.
>
> (De Marchi and Blaug, 1991, p. 6)

For de Marchi, as for Hands, economists find Lakatosian methodology attractive because it provides a way of defending what they do. This arises from the nature of economic theorizing as it exists in the mainstream of economics today. Economic theory is dominated by the attempt to explain a variety of economic phenomena on the basis of a very limited range of behavioural assumptions. Explaining a phenomenon involves demonstrating how it follows from the assumption of rational behaviour, any other assumption being viewed as *ad hoc,* for agents ought to behave rationally. Furthermore, given that assumptions can rarely be tested directly (experimental work being both problematic and in its infancy, and econometric testing frequently being inconclusive) the only option open to economists wishing to test theories is to derive further predictions which can be compared with other evidence. Thus when economists defend theories on the grounds that they 'work', what they usually have in mind is the prediction of novel facts in the sense of facts which were not used in the construction of the theory.[10]

This conjecture suggests that support for Lakatosian ideas should be strongest amongst theorists (for whom notions of theoretical progress and a metaphysical hard core should be attractive) and amongst those applied economists who do not wish to be critical of existing theory. This is not, however, what we find. Perhaps the

[10] This is, of course, a weak sense of the term 'novelty'. See Hands (1991b) and Backhouse (1997).

clearest example of support for Lakatos is provided by Hendry, who refers to the 'distinguished contributions' of Popper and Lakatos having 'revolutionised our understanding of "science"' (Hendry, 1993, p. 12). He supports his methodology of encompassing by arguing that it corresponds to a 'progressive research strategy', where this is understood in a Lakatosian sense (ibid., p. 440). Most economists, however, are less explicit, preferring to claim that their theories 'work'. This phrase, however, should be in a 'Lakatosian' way: that theories account for out-of-sample data, that they explain things their rivals cannot explain, and that they demonstrate connections between phenomena that had previously been thought unconnected.[11]

Another clear statement of the importance of predicting novel facts is provided by Friedman and Schwartz:

> A persuasive test of their results must be based on data not used in the derivation of their equations. That might mean using their equations to predict some kind of phenomena for other countries, or for a future or earlier period for the United Kingdom, or deriving testable implications for other variables. ... Similarly, that is the *only* kind of evidence that we would regard as persuasive with respect to the validity of our own results.
>
> (Friedman and Schwartz, 1991, p. 47; emphasis added)

This position is very close to that advocated by Friedman in his influential essay on methodology (1953).

These are only two examples,[12] but they suggest some alternatives to the conjectures made by Hands. The first is that Lakatosian ideas are expressed by economists deeply committed to economics being an empirical science, driven by data. Prediction of novel facts is used, especially by Hendry, not because it is a soft option, but because it is both feasible (in a way that naive falsificationism is not) and demanding. The second is that prediction of novel facts has a history in economics that goes back well before Lakatos. It is plausible to conjecture that economists found Lakatos attractive because the appraisal criterion he used was already, perhaps for very good reasons, well-established.

[11] Lakatosian is placed in quotation marks because his appraisal criterion is not original with him.
[12] De Marchi (1991) and Hands (1993b) are notable for the paucity of examples provided.

3 THE ROLE OF THE ECONOMIC METHODOLOGIST

Before Kuhn

Prior to Kuhn's *Structure of Scientific Revolutions* (1962/70) many of the most important writers on economic methodology saw themselves as applying standards and criteria that went beyond economics. Hutchison (1938), following the perspective provided by logical positivism, analysed the propositions of economic theory with a view to establishing their logical status, distinguishing in particular between propositions which are conceivably falsifiable and those which are not. The way in which the *ceteris paribus* condition was used in much economic theory, he claimed, rendered much pure theory unfalsifiable. He also provided a critique, extremely prescient from a present-day perspective, of the assumption of rationality. On the basis of such arguments he drew the conclusion that there were severe limitations on what pure theory could achieve. Economists, he argued, needed to go out and look at how the world worked, not simply theorize about it.

Hutchison's argument that the propositions of economic theory should be refutable was challenged vigorously by Machlup (1955). By the 1950s logical positivism had largely been displaced by logical empiricism, which emphasized the testability of a theory taken as a whole, not of its individual components. Propositions which, taken on their own, were untestable, might none the less form part of a theoretical system which produced testable propositions. If the theoretical system were successfully tested, the propositions embodied in it could be seen as being indirectly tested. The key propositions of economics, such as utility and profit maximization, which Hutchison criticized as being untestable, were, Machlup claimed, indirectly testable.

Though they differed sharply in their attitudes towards economic theory, Hutchison and Machlup shared important common attitudes. Methodology involved logical analysis of economic propositions, establishing whether or not they had empirical content. Though they made the assumption that methodological criteria taken from science could be applied to social science and to economics. For both Hutchison and Machlup the role of the economic methodologist was to apply logical analysis, using insights obtained from contemporary philosophy, to the propositions of economic theory. Though Hutchison drew critical conclusions where Machlup defended economic orthodoxy, both saw themselves as offering arguments that economists ought to find compelling.

Such an attitude also underlay Friedman's 'Methodology of positive economics' (1953) and the debates that arose out of it, and the interest in Popperian methodology which arose at the London School of Economics around 1960. Friedman started from a premise about science in general: 'The ultimate goal of a positive science is the development of a "theory" or "hypothesis" that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed' (ibid., p. 7). From this starting point he proceeded to argue that the only relevant test of the validity of a theory was comparison of its predictions with experience, with the associated claims about the realism of assumptions. Friedman, as Hirsch and de Marchi (1990) have convincingly argued, was arguing as a practising economist, seeking to offer advice that would raise the quality of economic theories. His argument was, therefore, rooted in economics rather than in philosophy. He nonetheless drew freely on natural science examples to make what he saw as points about the nature of science in general.

In debating Friedman's essay, Nagel (1963) and Samuelson (1963) adopted the same point of view. Nagel, a leading philosopher of science, clearly drew on contemporary philosophy in providing a logical dissection of Friedman's claims concerning theories and assumptions. Samuelson uses set theory in an attempt to demonstrate the fallaciousness of what he termed the 'F-twist' – Friedman's claim that the realism of a theory's assumptions is irrelevant to its worth. Economics, for Samuelson, was science, with methodological analysis involving the use of formal logic to evaluate claims about scientific propositions and the way they should be tested.

An emphasis on prediction as the criterion by which to evaluate economic theories was also characteristic of the group of economists centred on Lipsey and Archibald working at the LSE around 1960.[13] Their aims were the quantification and the testing of economic theory. As with the economists just mentioned, they started with a view of the nature of science, which was assumed to apply with minimal modifications (greater reliance on the law of large numbers) to economics (see Lipsey, 1963, chapter 1).

After Kuhn

This view of the philosophy of science was challenged by Kuhn. His perspective was sociological, informed by the history of science. His account of periods of normal science separated by scientific revolutions was derived from an analysis of history. Influenced by Kuhn and, shortly afterwards, Lakatos, economic methodology came to be linked much more closely to the history of economic thought, with numerous attempts being made to establish whether the latter could

[13] This paragraph draws heavily on de Marchi (1988).

be explained in terms of Kuhn's paradigms, Lakatos's research programmes, or some other pattern. Though much of this work was seeking to ascertain whether patterns believed to characterize natural science were also to be found in economics, it involved a new role for the economic methodologist. Though philosophy of science still provided economic methodologists with ideas on the nature of science, these ideas were now regarded as hypotheses to be tested rather than as statements about the nature of science that economists should not be question. Economic methodology now involved looking at economics in order to understand it.

This change towards the methodologist being seen as understanding economists' practices rather than seeking to criticize them is even more marked in the constructivist literature. McCloskey claims no more than that the study of rhetoric will make economists' conversations more civilized – if they understand what they are doing, disagreements will be less ill-tempered. The implied perspective for the methodologist (if we can so call the student of economic rhetoric) is that of a therapist, the role Rorty sees for the philosopher.

Lakatos and the role of the economic methodologist

Kuhn, however, was not altogether clear on the relationship between description and appraisal: was he describing the way science was, or was he arguing that this was how it should be? Lakatos, in contrast, provided a clear explanation of how he saw the relationship between the history and philosophy of science: his methodology of *historical* research programmes (MHRP). This defined a new role for the philosopher or methodologist.

Lakatos's MHRP involves the following four stages: (1) Obtain agreement on a list of successful scientific achievements. (2) Provide a history of these scientific achievements as though they had developed in accordance with the methodology one is trying to appraise – what Lakatos calls a 'rational reconstruction' of the history. (3) Compare this rational reconstruction with the actual history. (4) If the two histories are very different, conclude that the methodology is inappropriate: that it is incompatible with the decisions made by practising scientists. This is based on the assumption that 'an acceptable definition of science (methodology) must reconstruct the acknowledgedly best gambits as "scientific"' (Lakatos, 1971, p. 124).

Lakatos's methodology of historical research programmes is thoroughly Popperian in inspiration. First, appraisals are ultimately based on agreement or convention – in this case agreement over a list of successful scientific achievements. There are no indubitable foundations on which we can build. Second, it is based on a process of conjecture and refutation. Philosophy provides the conjecture (a methodology) which is then evaluated through referring to history.

Certain aspects of Lakatos's MHRP are open to criticism. It does not provide, any more than does Popperian falsificationism, a clear-cut formula by which to judge methodologies. The significance of differences between rational reconstructions and history is necessarily a matter of interpretation, as is the direction in which it is necessary to modify methodologies that do not fit. More seriously, it can be argued that the premises on which it rests are not satisfied in economics: it is difficult to agree on a list of undisputed scientific achievements, it has been argued that the structure of the economics profession distorts the incentives facing economists, so that it cannot be taken for granted that what is commonly regarded as best practice is directed towards discovering the truth. Against this Lakatos's MHRP implies a role for the philosopher that is at least potentially different from the pre-Kuhnian one. Scientists' practices play a vital role in appraising methodology, which means that the philosopher is required to examine what scientists actually do. This, however, is achieved without abandoning appraisal.

4 TOWARDS A POST-POPPERIAN ECONOMIC METHODOLOGY

Constructivism

One reason for the decline in interest in falsificationist methodology, whether Popperian or Lakatosian, has been the set of arguments associated with constructivism or postmodernism, and in particular Rorty's critique of epistemology. These perspectives have undoubtedly taught us much about the creation of knowledge, and about new questions which arise when we view knowledge as socially constructed. However, like Blaug (1994) I am not a constructivist. There are three main reasons. (1) Constructivism is but one perspective amongst many. Arguments about the constructedness of economic knowledge must be applied to constructivism itself—the problem of reflexivity (discussed in Hands, 1994). Why should one adopt this perspective rather than any other? The answer must depend on the standards by which methodological ideas are to be evaluated, and hence by the questions to which we are seeking answers (cf. Hutchison, 1994). (2) Constructivism is a very conservative doctrine, which in economics is extremely dangerous. It seems hardly a coincidence that the first thoroughly constructivist account of a branch of the history of economic thought (Weintraub, 1991) deals with general equilibrium theory, not an applied field such as labour economics or macro. General equilibrium theory is vulnerable to falsificationist criticism (of almost any variety) and constructivism provides an ideal way to defend it (see Backhouse, 1992b). (3) The most powerful strand in constructivism is the sociology of scientific knowledge. This is empirically based. Detailed observation of the way scientists work is used as the basis for a thorough-going rejection of any guidance from philosophy. Methodologically it is an inductive approach, subject to all the problems associated with induction. If instead methodology is seen as a process of conjecture and refutation, on the other hand, there is no reason to reject guidance provided by philosophy – any philosophical ideas that are taken up will be tested against evidence from economics.

Taken together, these reasons provide a strong case for rejecting the postmodernist hostility to philosophical arguments. Furthermore, for all its defects, one of the major strengths of the Popperian perspective is that it is not vulnerable to Rorty's critique (Backhouse, 1992a, [1997]; Hands 1993a, chapter 11). However, though the Popperian perspective provides a viable starting point, it is necessary to go beyond Popper. I wish to suggest that Lakatos still provides valuable pointers as to how this might be done.

Positive and normative methodology

The move towards 'recovering practice' has been important in so far as it has forced methodologists to think seriously about what distinguishes economics from other disciplines, what the key features of economics are, and whether there is a coherent rationale for what economists currently do. But whilst there may be good reasons for current practice, this cannot be taken for granted, for one function of methodology is to ask critical questions about current practice. There is thus a tension which needs to be maintained between positive and normative methodology – between seeking to understand what economists do and seeking to evaluate it. This tension was largely absent from pre-Kuhnian economic methodology, and is again absent from the recent literature which abandons normative methodology. It is quite consistent to accept that there may be problems with the methodology of historical research programmes whilst at the same time holding that Lakatos maintains this tension between positive and normative methodology.

Research programmes

Even though they are not necessarily thinking in Lakatosian terms, economists find the notion of evaluating research programmes, or sequences of theories, attractive. This is, arguably, something that should be taken seriously. What, however, is to be gained from analysing research programmes using the Lakatosian devices of heuristics, hard core and protective belt? Two answers suggest themselves. (1) Whether or not Lakatos was necessary for this, the effect of Lakatosian methodology has been to direct economists towards detailed studies of episodes in the history of economic thought, and away from making broad, under-researched, generalizations about research programmes in economics. (2) Lakatos's concepts have provided a set of questions that can form a useful starting point in analysing historical episodes.

The main conclusion to be drawn from the criticisms, discussed above, of Lakatos's concept of a research programme is that characterizing them in terms of an invariant hard core is too narrow. A broader concept of research programme, allowing for a greater variety of interaction between programmes, and for hard cores which change over time, would appear to be required. Here, it is useful to remember that, in setting out his MSRP, Lakatos attached prime importance not to the hard core, but to methodological rules. He introduced research programmes in the following way:

> I have discussed the problem of objective appraisal of scientific growth in terms of progressive and degenerating problemshifts in series of scientific theories. The most important such series in the growth of science are characterized by a certain *continuity* which connects their members. This continuity evolves from a genuine research programme adumbrated at the start. The programme consists of methodological rules: some tell us what paths of research to avoid (*negative heuristic*), and others what paths to pursue (*positive heuristic*).
>
> (Lakatos, 1970, p. 132; emphasis in original)

Though Lakatos went on to relate these heuristics to the concepts of hard core and protective belt, there is no need to do this. To illustrate this, Hausman's *The Inexact and Separate Science of Economics* (1992) could be seen as articulating a heuristic, or set of rules, underlying contemporary mainstream microeconomics. Though one might argue that his rejection of Lakatos's appraisal criterion had deprived the exercise of its bite, this could be seen as, in a sense, an exercise in the spirit of Lakatos. It may be true, as Hausman claims (see Hausman, 1994), that Lakatos provides little guidance as to the nature of the heuristics characterizing economics, but why should we expect him to provide this?

Predicting novel facts

Perhaps the most important aspect of the Lakatosian legacy, however, is his emphasis on predicting novel facts as an appraisal criterion. There are several reasons for suggesting this.

(1) It fits very closely with the way economists think of what they are doing. To understand economics, therefore, we need to understand why this is so. One explanation runs in terms of the structure of neoclassical theorizing: in the absence of hard empirical criteria, consistency with rational behaviour is used to decide what is and is not *ad hoc.* Another explanation focuses on econometrics, pointing to the relation between predicting novel facts and tests using out-of-sample data, encompassing and so on.

(2) Prediction is an appraisal criterion that will not go away. Not only do some philosophers still attach great importance to it (for example Rosenberg 1992 and 1994) but so do policy makers, who wish to know the consequences that will follow from the various actions they might take. In so far as the main aim of economics is the provision of guidance to policy makers, prediction must be an important goal. Economics should accordingly be appraised, at least in part, according to its ability to predict. A merit of work in the Lakatosian tradition is that some of it distinguishes between different types of prediction. These distinctions arise through asking what is meant by 'novelty'. Novel facts might mean, for example, facts of which no one was aware when the predictions were made; facts unknown to the person making the prediction; facts not used in making the prediction; or one of a number of other things.[14] It can be argued that each of these types of prediction has a different significance (see Backhouse, 1997, pp. 114–15).

(3) Even though it may be impossible to defend prediction of novel facts as an appraisal criterion using Popper's theory of verisimilitude, this does not mean that it cannot be defended in other ways. It can be argued, for example, that predicting novel facts, in the sense of facts that a theory was not designed to predict, is especially important in a discipline where controlled experiment is not possible. Controlled experiments enable scientists to isolate phenomena. When this is not possible theories have to be tested by applying them to new situations – by predicting facts which are novel in various senses of the term.[15]

5 CONCLUSIONS

The Lakatosian legacy in economic methodology is substantial, in two senses. Historically, Lakatos's MSRP played a major role in stimulating interest in economic methodology and in bringing about a change in perspective. It may be that such a shift, towards analysing heuristics and thinking about methodology in the context

[14] For a list of five definitions of novel facts, see Hands (1991b, pp. 96–9).
[15] An attempt is made to develop this argument more fully in Backhouse (1997).

of the history of economic thought, would have happened anyway, but as it happened it was Lakatos who caught the imagination of so many economists. More important, Lakatos provides a number of pointers as to lines along which economic methodology might develop.

Lakatos's MSRP has, I suggest, more to offer than many critics admit. To say this is not to argue that Lakatos's MSRP provides a simple formula by which means of which economics can be analysed and appraised. The criticisms outlined earlier in this chapter make it clear that it does not. Neither does it imply that the only important questions are those that can be analysed within the Lakatosian framework. Mäki (Mäki, 1994) is surely right to argue that methodologists need to address a wider range of issues than those with which they were, by and large, been concerned for most of the 1980s. Knowledge is a multi-faceted, complex phenomenon that can be approached in many ways. There remains, however, an important range of questions concerning the nature and growth of economic knowledge, for which the concepts developed and put forward by Lakatos provide a valuable starting point.

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Part II

Rhetoric and postmodernism in economics

Chapter 6

The hermeneutic challenge to economics

This chapter, written in 1991 but not previously published, grew out of a review of Economics and Hermeneutics, *edited by Don Lavoie. From the criticisms made in that review it focuses on what the advocates of a hermeneutic approach need to do if they are not to be ignored by economists: to abandon the rhetoric of anti-positivism; to get down to the details of economists' practices, recognizing the variety that exists within the discipline; to pay attention to the aims of economic enquiry; and to provide well-worked-out examples of what the hermeneutic approach can achieve. In the absence of such changes, the hermeneutic challenge will remain at a programmatic level, and be dismissed alongside the criticisms offered by other heterodox approaches. Though these complaints were made in 1991, they remain valid in the late 1990s.*

1 INTRODUCTION

Economists, it has been claimed, should turn their attention to hermeneutics. Of the reasons given for this, the obvious one is that it would improve communication amongst economists if they realized that the meaning of a text was not unique and objectively given, but was the result of a process of interpretation, and if they understood better the nature of such interpretive processes. Thus McCloskey (1986) has drawn economists' attention to Rorty (1979) and Booth (1974); and Gerrard (1991) has turned to Ricoeur (1976, 1981). There is, however, a much more important reason for turning to hermeneutics. It has been argued that, though written text is the paradigmatic case, all human knowledge is the result of a process of interpretation. For economists, as for any social scientists, this is important because it should, so it is claimed, affect not just the presentation but also the *content* of our theories: if economic agents are engaged in a process of interpreting the world around them, hermeneutics should, through helping us understand this interpretive process, lead to new perspectives on the way the economy works. In so far as this claim can be substantiated, it is a powerful and fundamental challenge to conventional ways of constructing economic theories.

This more ambitious view of the role for hermeneutics is taken up in many of the contributions to *Economics and Hermeneutics,* edited by Don Lavoie. The purpose of this note is to explore some of the questions raised by this book, and to make a number of suggestions concerning the way the hermeneutic challenge to mainstream economics needs to be posed if it is to provoke an informed debate over the direction in which economics should develop. This note is not intended as a balanced review of the book, but is intended merely to initiate a debate over what seem to be some central questions that are not being addressed.[1] The questions are formulated under three main headings: the anti-'positivist' crusade; the aims of economics; and the positive contributions of hermeneutics.

2 THE ANTI-'POSITIVIST' CRUSADE

Discussion of hermeneutics and economics has inevitably been in the context of the debate over 'rhetoric' and economics initiated by McCloskey's widely discussed article in the *Journal of Economic Literature* (1983). This article and McCloskey's subsequent book raised many important questions, but it could be argued that they presented a false choice between methodology and rhetoric, and that the 'positivist' or 'modernist' position which McCloskey attacked represented an oversimplified straw-person (see, for example, Chapter 7 below; Caldwell and Coats, 1984).[2] In so far as McCloskey's purpose was to disturb economists' complacency, and to shock them into paying attention, such a strategy was in a sense justified. McCloskey, in this sense, should be seen as the writer of a manifesto.

McCloskey's strategy of attacking a monolithic 'positivist' or 'modernist' position was also (to a certain extent) defensible for a more subtle reason. One of his main tasks was to show the difference between economists' explicit rhetoric (what they said about their methodology) and their implicit rhetoric (the methodology implicit in what they actually did), and in the way they presented their *economic* arguments. Having seen the discrepancy, and having realized that there were good reasons

[1] See Backhouse (1991) for a review of the book.
[2] The terms 'positivist' and 'modernist' are placed in quotation marks, for it is not wished to tackle the question of whether or not the terms have become so abused as to be unusable. For the present argument the precise definition of these terms is not critical.

why they did things the way they did, economists would, McCloskey hoped, drop the misleading ('positivist') explicit rhetoric. He was emphatically *not* trying to change the way economists carried on their economic enquiries. Given that many of the explicit methodological statements to which he was taking exception, and which he wished to eradicate, were (so he claimed) 'positivist' it was natural for him to choose this target.

For Lavoie and most of his co-authors, on the other hand, the situation is very different, in two respects. The first is that we are now [1991] nearly a decade into the debate opened up by McCloskey's article, and the time for manifestos has passed. What is needed now, it can be argued, is careful, detailed work to establish just how far arguments about rhetoric can be taken. Without this, the debate will get nowhere. The second and more important reason is that Lavoie and his colleagues are, unlike McCloskey, concerned to alter the way economists think about the economy, not simply the way they think about what they are doing. This means that they must take issue not with economists' explicit methodology, but with the methodology that is implicit in what they actually do. This methodology, if we accept McCloskey's arguments on this point, is *not* 'positivist' or 'modernist'.[3] This means that it is necessary for the advocates of a hermeneutic, or interpretive, economics to address very carefully the question of what economists actually do. This is a complex task, involving the nature of the assumptions made in economic theorizing; the relationship between partial and general equilibrium models; the interpretation placed on *ad hoc* assumptions; the way data are used; the relationship between theory and econometrics; and so on.

When such issues are considered in detail, the resulting picture is one of great variety. As an example, consider the literature concerned with the existence, uniqueness and stability of general equilibrium. It has been argued that *even within this very restricted field,* within which one might, *a priori,* expect to find great homogeneity, there are fundamental differences in approach between, for example, von Neumann and Debreu on the one hand, and Arrow and Hahn on the other (Ingrao and Israel, 1990; cf. Backhouse, 1994). When we range more widely over the

[3] Though we may not be ready to accept his conclusions (some, though not all, of the reasons are discussed below), this is something which Mirowski (1987) has at least tried to do in stressing the Cartesian nature of mainstream economic thought. Though at first glance this might be thought yet another synonym (or euphemism?) for 'positivist' (and there are certainly similarities) it could be argued that his position is differentiated from McCloskey's in that he is concerned with what economists actually do, not just with their rhetoric.

field of mainstream economics we will find even greater differences. For example, it is far from clear how much there is in common, methodologically, between the axiomatic approach of Debreu, the statistically based approach of Hendry, or the more mixed approach of economists such as Solow or Tobin. Given that there is this variety, economists who wish to argue the merits of a hermeneutic perspective need to make it clear just what their target is, and to argue case by case.

It is thus arguable that the advocates of a hermeneutic approach to economics should, if they really wish to change what economists actually do, abandon the anti-'positivist' bandwagon, and should conduct the debate at what could be a much more productive level.[4]

3 THE AIMS OF ECONOMICS

When applied to economic agents, hermeneutics argues for the construction of economic theories that take account of people having to interpret the world. Many of the contributors to *Economics and Hermeneutics* are thus critical of mechanistic models of *homo economicus,* and of models which are based on such a view of economic agents. In one sense this is unexceptionable. People are both more complex and less calculating than the neoclassical view of them suggests. However, before we jump to the conclusion that economic theories which view economic agents as interpreting the world around them are superior to neoclassical ones, we need to consider very carefully what are the aims of our theorizing.

If we accept the perspectives of hermeneutics, interpretations are not unique, dictated by brute facts that admit of only a single interpretation. Thus the hermeneutic perspective of economic agents interpreting their environment must not be taken as an established 'fact' which any theory must take into account. It is merely one perspective amongst many, of which *homo economicus* is another. To decide between alternative perspectives on economic agents, we need to think about the aims of our theorizing, and to ask which perspective is best fitted to achieving those aims. Traditionally (see Hutchison, 1992) the aim of economic enquiry has been the provision of guidance for policy-makers. If we accept this aim, it is arguable that the ability to predict *does* matter: for example, someone who knows that abolishing tax relief on loans for house purchase will, *ceteris paribus,* lower the price of housing does know something important about the economy. In other words, the ability to

[4] Some of the contributors to *Economics and Hermeneutics* do attempt to get down to discussing individual theories in detail, but anti-'positivism' is prominent in many of them.

predict may be a significant form of understanding. If the advocates of an interpretive economics are to persuade mainstream economics to abandon the attempt to predict, they have to show that the understanding provided by such an economics is superior to that provided by other approaches.[5]

This argument is open to a number of objections. It might be argued that the predictions emanating from neoclassical economics are worthless or misleading, through the latter's focus on static resource-allocation problems, to the neglect of dynamic issues (an 'Austrian' view). This may or may not be true, and it may or may not be the case that hermeneutics can provide us with insights that enable us to develop the Austrian view, but neither of these can be settled with what Weintraub (1989) has termed 'Methodological' pronouncements, short-circuiting the economic issues. Another objection arises from presuming that predictions must be quantitative and precise. Nothing in the above argument requires this. Smith and the classical economists, for example, made predictions with the aim of aiding policy-makers, but they were not quantitative. Finally, it might be argued that focusing on policy-relevance involves a blinkered view of economics, comparable to exalting the merits of engineering over pure scientific research. Saying that the provision of advice to policy-makers is the aim of the subject, however, is not the same as saying that all research should be directed at producing predictions or policy implications. Far from it: the provision of good advice may, in the long run, require an immense amount of 'basic' research.

Thus the advocates of an interpretive economics need to show either that their approach will provide advice for policy-makers that is superior to that provided by mainstream economics, or they have to argue that the aim of the subject is something else. There is nothing wrong with the latter course, but the proposed aims of the subject should be explicit and open for debate.[6]

[5] It can, of course, be argued that much mainstream economics has lost sight of this goal (see Hutchison, 1992).

[6] This challenge to be explicit concerning the aims which are being pursued, applies of course not only to hermeneutic economics but also to areas of neoclassical economics, a point Hutchison (1992) makes very forcefully.

4 POSITIVE ACHIEVEMENTS

If the arguments of the preceding two sections are accepted, it is necessary for the advocates of hermeneutics economics to show what can actually be done using a hermeneutic approach. It is inadequate to outline a general programme, or to use hermeneutics to provide a critique of mainstream theory. If the aim of providing policyadvice is accepted, for example, it is necessary to provide examples which show how the prescriptions emanating from neoclassical economics can be bettered. Some contributions to *Economics and Hermeneutics* come close to doing this, but none goes far enough. For example, Tyler Cowen argues the case for a non-Paretian welfare economics, and provides some interesting arguments. He fails, however, to show how his non-Paretian approach would provide better advice concerning, for example, whether or not to build a second bridge across the River Severn, or where to locate a fourth London airport. As another example, take the paper by Ralph A. Rector, which criticizes rational expectations for not being rational. The concept of rational expectations was formulated to solve a specific problem – the problem of how to model the formation of expectations in models where the outcome depended critically on such expectations. To be forceful the criticism needs either to provide an alternative expectations formation mechanism or, more radically, to show how the problem of, say, inflation and unemployment can be analysed without using models which require us to postulate that agents hold such expectations. If, on the other hand, the role of hermeneutics is not to provide an alternative to mainstream economics, but is, for example, to provide a means of evaluating parts of mainstream economics compared with others, we need to have this made clear. In either case, clarity and precision are required.

Until examples are provided of what can be done using the techniques provided by hermeneutics, or of what the application of hermeneutics to economics can achieve, mainstream economists will, with some justification, remain sceptical.[7]

5 CONCLUSIONS

The remarks in this note are critical. The reason for this is not a desire to belittle the importance of hermeneutics for economics. To the contrary, I regard the perspective

[7] Gerrard (1991) does try to use hermeneutics to solve a particular problem: that of why economists have disagreed for so long about the interpretation of Keynes's *General Theory.* I am not convinced, however, that his example gets us very far. It is not an *economic* problem, but a problem in the interpretation of economics.

provided by hermeneutics as an interesting new development in economics, but one the potential of which has yet to be determined. Rather, is written from the belief that if hermeneutics is to be taken seriously, and if the possibilities it provides are to be explored properly, an approach needs to be adopted which appears to be different from that being taken by many of its proponents. Otherwise, hermeneutics is in danger of being dismissed out of hand by mainstream economists as simply no more than part of the latest anti-rationalist trend, which fails to appreciate what economics is about. Economic theory as it now exists has great limitations, and it may be the case that the perspective provided by hermeneutics will help overcome some of these. It would be a pity if an opportunity were lost simply because hermeneutic ideas were put forward in such a way that they were never explored properly.

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Chapter 7

Rhetoric and methodology*

(*Perspectives in the History of Economic Thought,* volume 9, edited by R. F. Hébert. Aldershot: Edward Elgar, 1993, pp. 3–17.)

The case made in this chapter is a simple one: that methodology and rhetoric are complements, not substitutes. It explains how I can at the same time be critical of the anti-positivism of McCloskey, Weintraub and others, and yet engage in rhetorical and linguistic analysis myself. There is no conflict between the two, for the study of rhetoric is a valuable and informative exercise that has nothing to do with anti-positivism. Indeed, it can be argued that rhetoric and methodology are inseparable in that an understanding of economists' rhetoric (how they seek to persuade) is an important part of understanding the principles of reasoning underlying their work.

This chapter, some of the arguments in which are developed at much greater length in Truth and Progress in Economic Knowledge *(1997), is above all an appeal for clarity.*

* My presentation on this topic at the 1991 meeting of the History of Economics Society (HES) focused on the arguments concerning the case against methodology, most of which are examined in much more detail in Backhouse (1992). By referring, where necessary, to this article, it has been possible to keep the case against methodology sufficiently short in this chapter so as to accommodate more detailed discussion of the positive contribution of rhetorical analysis, and hence to say something much more substantial about the relative merits of methodology and rhetoric. This chapter thus fulfils the promises made in the original synopsis (which remains unchanged) much better than did the paper actually presented. In the process of sorting our these ideas I have benefited enormously from written comments made by Mark Blaug, Vivienne Brown, Sheila Dow, Daniel Hammond, Willie Henderson, Donald McCloskey, Uskali Mäki, and Judith Mehta; and from numerous spoken comments and words of encouragement from participants in the HES meeting. I am also indebted to Willie Henderson for numerous discussions on rhetoric which have been important in helping me clarify my ideas. Needless to say, the usual caveat applies, absolving them from all responsibility for the way I have responded to their ideas.

1 THE ISSUES

In 1983 Donald McCloskey challenged the economics profession to abandon positivist methodology and instead to embrace rhetorical analysis. His argument was that methodology[1] did not describe what went on in economic science; that progress would stop if its prescriptions were followed; and that to give methodological advice was presumptuous and laughable. According to McCloskey (1983, p. 482) if economists were to abandon methodology in favour of analysing the rhetoric of economics, their conversations would be improved and 'the real arguments would then be joined'.

The notion that we should pay more attention to the way economists actually persuade one another, though not original with McCloskey, has opened up a vast area for research. Not surprisingly, many new ideas have been introduced into the discussion, not least because it was possible to draw on ideas from other disciplines, in particular from literary criticism. McCloskey made use of the arguments of Wayne Booth (1974); Weintraub (1989, 1991) has drawn on the work of Stanley Fish (1980); Gerrard (1991) has relied on Ricoeur. Amariglio (1988, 1990) and Rossetti (1990) have brought such names as Althusser, Foucault, Gadamer and Lyotard to our attention. The terms 'modernism' and 'postmodernism', long familiar in other social sciences and in literature, have been brought into discussions of economics (e.g. Amariglio, 1990; Klamer, 1987a, 1987b; Dow, 1992).

It is a sign of the richness of the area opened up, and a tribute to McCloskey's perception in opening it up, that there has been such a ferment of new ideas. The decade since 1983 has been a very exciting one for those working in this area. The cost of this, however, has (at least in this writer's view) been that the process of taking stock of new developments has failed to keep up with the generation of new ideas. Issues have been confused, and there have been too few attempts to isolate the really important contributions from lesser ones. Furthermore, because ideas are being brought in from philosophy, literature and other disciplines, extensive stock-taking is necessary if economists not specializing in it are to be able properly to appreciate what is being done. The aim of this chapter is to contribute to this process by examining the claim that rhetoric should displace methodology as the subject studied by those seeking to understand the nature of economics. After disentangling the many arguments that have been put forward, I argue that far from

[1] McCloskey differentiates between 'methodology' and 'Methodology'. In view of the confusions caused by this separation it will not be used here.

being alternatives, rhetoric and methodology are complementary. Both should be pursued, with each informing the other.[2]

2 THE CASE FOR RHETORIC

Explicit and implicit rhetoric

McCloskey audaciously challenged economists to face up to the inconsistency between their methodological pronouncements and their actual practice:

> Economists do not follow the laws of enquiry their methodologies lay down. A good thing too. If they did they would stand silent on human capital, the law of demand, random walks down Wall Street, the elasticity of demand for gasoline, and most other matters about which they commonly speak. ... Economists in fact argue on wider grounds, and should. Their genuine, workaday rhetoric, the way they argue inside their heads or their seminar rooms, diverges from the official rhetoric.
>
> (McCloskey, 1983, p. 482)

McCloskey (ibid., p. 484) argues that the 'official' rhetoric of economics, more appropriately (and less emotively) called its *explicit* rhetoric,[3] is positivist or modernist: 'an amalgam of logical positivism, behaviorism, operationalism, and the hypothetico-deductive model of science'. Citing Booth (1974), he argues that it is based on the notion that 'we know only what we cannot doubt, and cannot know what we can merely assent to' (McCloskey, 1986, p. 5). According to this methodology, scientific reasoning involves observable, objective, quantitative data produced by reproducible experiments. Other forms of argument are dismissed.

Contrasted with this is the methodology implicit in the arguments economists actually use. Here we find something very different, for economists seek to persuade each other using arguments which have no place in their explicit methodology.[4] To

[2] Some of the issues discussed here are touched on by Caldwell and Coats (1984) – see also McCloskey (1984). My conclusion is reinforced by Mäki's (1993) argument that justification and truth are different concepts, which can be used alongside each other.

[3] Although McCloskey proposes the terms 'implicit' and 'explicit' as alternatives to 'official' and 'unofficial', he seems to prefer the latter terminology.

understand the principles which underlie economic enquiry as it is actually undertaken, therefore, we must look not at the explicit methodological pronouncements of economists, but at how they actually seek to persuade each other; we must change our perspective and view economists as persuaders. In other words, to understand the nature of economics we must focus on rhetoric, not methodology.

Economists as persuaders

In *The Rhetoric of Economics* (1986), McCloskey set out to show that economists use rhetoric, and to study the nature of that rhetoric.

> The question is whether the scholar ... speaks rhetorically. Does he try to persuade? It would seem so. ... It seems on the face of it a reasonable hypothesis that economists are like other people in being talkers, who desire listeners when they go to the library or the laboratory as much as when they go to the office or the polls. The purpose here is to see if this is true, and to see if it is useful: to study the rhetoric of economic scholarship.
>
> (ibid., p. xviii)

McCloskey does not use the term 'rhetoric' in its pejorative sense (as in 'mere rhetoric') but rather deals with all means people use to persuade each other. McCloskey (1983, pp. 482–3; 1986, pp. xvii–xviii) follows Booth (1974) closely in this respect. In view of its centrality, it is worth quoting several of the ways in which he defines rhetoric. Alternatively, rhetoric is:

1. 'the art of probing what men believe they ought to believe, rather than proving what is true according to abstract methods' (Booth, 1974, p. xiii; quoted by McCloskey, 1983, p. 482, and 1986, p. 29);
2. 'the art of discovering good reasons, finding what really warrants assent, because any reasonable person ought to be persuaded' (Booth, 1974, p. xiv; quoted by McCloskey, 1983, p. 482, and 1986, p. 29);
3. 'careful weighing of more-or-less good reasons to arrive at more-or-less probable or plausible conclusions – none too secure but better than would be arrived at by chance or unthinking impulse' (Booth, 1974, p. 59; quoted by McCloskey, 1983, pp. 482-3, and 1986, p. 29);

[4] McCloskey takes this position because he claims that the explicit methodology is impossible to follow, but to discuss this problem here would take us into issues that are best discussed in the next section.

4. the ‘art of discovering warrantable beliefs and improving those beliefs in shared discourse’ (Booth, 1974, p. xiii; quoted by McCloskey, 1983, p. 483, and 1986, p. 29), its purpose being not ‘to talk someone else into a preconceived view; rather it must be to engage in mutual inquiry’ (Booth, 1974, p. 59; quoted by McCloskey, 1986, p. 29);
5. ‘the paying attention to one’s audience’ (McCloskey, 1986, p. xvii);
6. ‘the proportioning of means to desires in speech’ (McCloskey, 1986, p. xviii);
7. ‘an economics of language, the study of how scarce means are allocated to the insatiable desires of people to be heard.’ (McCloskey, 1986).

By these definitions, it is clear that economists do use rhetoric, indeed, how could they avoid it? It is obvious also that economists use figures of speech and literary devices; as McCloskey puts it, the economist is ‘self-evidently a linguistic actor’ (ibid., p. 57). Once the question has been posed, for example, it is obvious that economics is metaphorical, and that many of the metaphors used by economists are not simply ornamental.

A more interesting question concerns how economists seek to persuade: what is the nature of the literary and other devices they use, and the ways they are used? It is because this is an important question that economists need to take note of literary criticism.

> The service that literature can do for economics is to offer literary criticism as a model for self-understanding. ... Chiefly it is concerned with making readers see how poets and novelists accomplish their results. An economic criticism of the sort exercised below is not a way of passing judgement on economics. It is a way of showing how it accomplishes its results.
>
> (ibid., p. xix)

McCloskey’s brilliant analysis of texts by Samuelson, Becker, Solow, Muth and Fogel provides a taste of the insights that can be achieved using such methods.

Rhetoric and good conversation

If we adopt a rhetorical perspective, McCloskey argues, we start to think of economic discourse in terms of a conversation and this leads us to use a broader range of criteria with which to judge what constitutes good economics. According to McCloskey (ibid., p. 27), ‘what distinguishes good from bad in learned discourse, then, is not the adoption of a particular methodology, but the earnest and intelligent attempt to contribute to a conversation’.

Whether or not a conversation is going well is not something that can be captured with simple methodological rules, but is something that one can recognize 'with ease' in one's own field. Furthermore, overlaps between fields are sufficient for one to be almost as sure about neighbouring fields.

McCloskey then argues that there is a market mechanism which maintains standards:

> examining the overlap is what editors, referees, and members of research panels do. The overlaps of the overlaps, as Polanyi once observed, keep us all honest if some try to be. Q.E.D.: the overlapping conversations provide the standards. It is a market argument. There is no need for philosophical lawmaking or methodological regulation to keep the economy of the intellect running just fine.
>
> (ibid., p 28)

The question of possible market failure is not addressed. We turn to it below.

Why should such arguments matter to economists? McCloskey argues that an awareness of the rhetorical nature of economic discourse would improve conversation amongst economists, because it would encourage them to drop the facade of positivist methodology and make it easier for them to have an open discussion of what they are actually doing: 'If it understood its own way of conversing – its rhetoric – maybe some of its neurotic behaviour would stop, such as compulsive handwashing in statistical procedures' (ibid., p. xix).

In his interpretation of Keynes's *The General Theory,* Bill Gerrard (1991) has adopted an approach that is very much in the spirit McCloskey advocates. Drawing on hermeneutics, and particularly the work of Ricoeur, he points out that texts do not have single correct interpretations. The *General Theory* is, he argues, rich in interpretive content, the sign of a great work. Thus economists should not be concerned that different people interpret Keynes in different ways. Gerrard is trying to improve the conversation amongst interpreters of Keynes by using ideas from literary criticism to disabuse them of the false notion that texts have but a single correct interpretation.

A second reason why the nature of economics discourse should be of concern to economists concerns education. During their education as economists, students learn more than simply a set of facts and techniques; they learn how to use a new language. Analysing this language can, it has been argued (Henderson and Dudley-Evans, 1990; Klamer, 1990), suggest new ways of teaching the subject, thus improving the conversation between teachers and their students.

3 THE CASE AGAINST METHODOLOGY

Knowledge as a social construct

In the preceding section we confined our attention to the arguments in favour of rhetoric. The proponents of such views, however, have frequently linked their arguments for rhetoric with arguments against methodology. The most powerful of these attacks on methodology (which, at least in part, underlies other arguments discussed below) is the argument that knowledge is socially constructed: that it is impossible to find a secure foundation for knowledge that is independent of the standards and values of the community that holds the knowledge. This argument provides a further connection with the study of literature, for not only is it found in philosophy, where it has been used to argue against the whole project of epistemology, but it has also been used to attack the idea that there can be a general theory of how literary criticism should be undertaken.

In philosophy the most prominent recent exponent of this viewpoint is Richard Rorty (1980), who has argued that it is meaningless to talk of knowledge as involving the accurate representation of nature. Such a view of knowledge would, he argues, be possible only if we knew what nature was 'really' like, but we could know this only if we had some privileged access to knowledge. This is clearly not possible, which means that we must replace the conception of knowledge as accurate representation with a view of knowledge as 'a matter of conversation and social practice' (ibid., p. 171). It follows that philosophers should be concerned not with finding objective truth, but with keeping the conversation going. The role of the philosopher is thus that of a therapist, not a judge.

Stanley Fish (1980) provides a very clear statement of a constructivist position in the field of literary criticism. The context here is debates over whether it is the text or the reader that is the source of meaning in literary texts. Fish's answer is that it is neither, meanings being the products of 'interpretive communities' which share certain strategies for interpreting texts. There are such things as facts concerning texts, but these facts are the result of interpretation. Meaning, for Fish, is thus neither subjective (totally dependent on the individual reader) nor objective (originating in the text), but public and conventional, being dependent on the set of beliefs shared by the relevant community.

The most rigorous critique of economic methodology based on these ideas is that of Weintraub (1989). He characterizes methodology as, 'a special project in economics: the attempt to govern appraisal of particular economic theories by [appealing to] an account of theorizing in general' (ibid., p. 264). Methodology cannot succeed, he argues, because it is impossible.

Any account of theorizing in general would have to be based on a position outside economics; outside the discourse community within which economic knowledge is created. Such a position is unobtainable: 'there *is* no position totally apart from the doing of economics which can inform the consideration of the doing of economics' (ibid., p. 272). In other words, foundationalism is impossible because there are no foundations available which are not themselves the product of interpretation.

However, where Rorty and McCloskey see rhetoric as therapeutic, Weintraub follows Fish in arguing that anti-methodology has no consequences. Since methodology can have no consequences, neither can its negation.[5]

Positivism, modernism and postmodernism

According to McCloskey the explicit rhetoric of economics is positivism. As he defines it, the main characteristic of positivism appears to be the demand for complete objectivity, which leads to an emphasis on prediction, observability and quantification, evidence that does not meet these criteria being dismissed. Though, like Weintraub, he has logical positivism as one of his targets,[6] his attack makes most sense if seen not as an attack on work by specialists in methodology, but as an attack on methodological 'asides' by practising economists, few of whom have made any serious study of methodology – what McCloskey has termed a '3 × 5 card' view of science.

This definition of positivism is very close to his definition of modernism which, following Booth, he sees as centred on the search for certainty.

> Modernism gleams diamond-hard from many facets, and the word can be fully defined only in use. But in a preliminary way it can be said to be, as the literary critic Wayne Booth has put it, the notion that we know only what we cannot doubt and cannot know what we can merely assent to. ... Philosophically speaking, modernism is the program of Descartes, regnant in philosophy since the seventeenth century, to build knowledge on a foundation of radical doubt. (McCloskey, 1986, p. 5).

[5] Methodological and anti-methodological statements can of course have consequences in the same sense that any idea, however mistaken, can cause people to behave differently. His point is that they have no logical consequences.

[6] Weintraub provides a one-paragraph summary of the history of science which, interestingly, ends with logical positivism (1989, p. 265).

This definition certainly goes part of the way towards defining modernism but, due to certain important characteristics of contemporary economics, other definitions, at first sight very similar, lead to significantly different conclusions.

An alternative definition (cf. Klamer, 1987a, 1987b; Dow, 1992) incorporates the following elements.

1. A break with the past involving: (a) the search for universal theories; (b) a commitment to the idea of progress.
2. Formalism, with a preference for axiomatic, reductionist, dualist reasoning and the use of mathematics.
3. Compartmentalization (e.g. between positive and normative, and between disciplines) and a turning inwards involving, amongst other things, the use of jargon and self-referential discourse.

This alternative definition has the advantage that it can be related more clearly to developments in art, including music and architecture. More important, for our purpose, it captures the stress on formal, axiomatic methods which is arguably the most significant feature of economics in the post-war period. Though the difference is not quite so clear-cut as this, because the two definitions of modernism overlap significantly, one might argue that much of the explicit rhetoric in post-war economics has been modernist according to McCloskey's definition, whilst the implicit rhetoric has been modernist according to the Klamer/Dow definition. This is significant because of the importance of the distinction between the explicit and the implicit rhetoric of economics.[7]

Non-constructivist criticisms of methodology

Whilst the most powerful argument against methodology is perhaps the constructivist one, a number of other criticisms of methodology have also been advanced. These include the following (McCloskey, 1986, pp. 13, 15, 51):

1. Falsificationism is not cogent – controlled experiments are not possible, so crucial experiments are not possible;

[7] Paul Wendt (1990, p. 49) offers yet another definition, which has the great attraction of unifying what might be thought rather disparate elements. Wendt sees modernism as centred on the metaphor of the machine. On the basis of this he argues that the three essential characteristics of modernism are: foundationalism (analysis of an object into its components); objectivism (seeing the object of study as separate from the observer); and control (as a machine is controlled). Universalism and rationality, though sometimes proposed as features of modernism, are, in Wendt's view, inessential.

2. Cartesian doubt is inefficient – it is not necessary to check everything;
3. Important work violates the conventions of modernism;
4. People might have found something interesting to say about 'truth', but they have not;
5. The rhetoric of science has produced many studies of actual episodes in science, whereas methodology has produced very few;
6. Prediction is impossible in economics – otherwise economists would be rich;
7. Anyone who discovered a formula for scientific success would become 'a scientific millionaire' which implies such formulae have not been found (McCloskey, 1990).

These are important points, but quite separate from the constructivist critique outlined above. The impossibility of prediction in economics is a statement about the world, either a generalization from experience or a statement of belief. The paucity of studies of science inspired by methodology is an appraisal of a body of literature. Indeed, it is not clear that all these criticisms are consistent with the contructivist critique: in particular, the constructivist argument seems inconsistent with the claim that people might have found something interesting to say about truth.

4 THE CASE FOR METHODOLOGY

The possibility of a non-foundationalist methodology

The fundamental case against methodology is that methodology is impossible, due to the conventional nature of knowledge and the consequent impossibility of saying anything about knowledge in general. From this it follows that the only sensible alternative is to analyse the way economists actually persuade each other. For this reason, rhetoric is seen as anti-methodology, or as an alternative to 'science', where 'science' is understood in a narrow, positivist sense.

If methodology were, as its critics have argued, an impossible project, it would be hard to avoid this conclusion. The problem with this line of argument, however, is that it ignores the possibility of a non-foundationalist, non-positivist methodology. To give one example, Popperian falsificationism is non-foundationalist: for Popper there is no such thing as certain knowledge, all knowledge being potentially liable to be falsified. Furthermore, he makes it very clear that the empirical basis of science is conventional. In *The Logic of Scientific Discovery,* arguably his major work, he devoted a chapter to 'the empirical basis' of science, concluding that,

> The empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being.
>
> (Popper, 1934/59, p. 111)

Given that so much economic methodology is, directly or indirectly, Popperian, the anti-foundationalist case against methodology needs, at the very least, further justification.[8]

Turning to the non-constructivist arguments, we find that though cogent against much naive '3 × 5 card' methodology, they are not valid against more sophisticated methodologies, such as have dominated methodological discussions since at least the mid-1970s. To see this, consider the first three of the criticisms of methodology listed above in relation to Imre Lakatos, whose work had an enormous influence on writings on economic methodology during the late 1970s and 1980s.[9] The impossibility of crucial experiments and the impossibility of naive falsificationism were critical reasons why Lakatos postulated the appraisal criterion of corroborated excess content, a concept which itself is similar to Popperian sophisticated falsificationism. The fact that doubting everything is inefficient has much in common with the reasons for holdhag to a hard core of provisionally accepted assumptions, and for the principle of tenacity according to which new programmes and theories are not abandoned the first time they are apparently falsified. If economists were following Lakatosian methodology, we would expect them to violate the tenets of modernism. Furthermore, Lakatos's methodology of historical research programmes is an approach the whole point of which is to address discrepancies between methodology and actual histories of science.

The claim that philosophers (or methodologists) might have found something interesting to say about truth (how economic knowledge grows) is a coherent, though debatable, position. It is, however, not clear that it fits well alongside the constructivist critique discussed above. According to that critique, there are good reasons why interesting things to say about truth are simply not there to be found.

[8] Further ways of defending methodology are discussed in Backhouse (1992).
[9] It is not intended to imply that Lakatosian methodology constitutes the only, or even the best, response to these criticisms; it is given simply as an example.

The remaining two criticisms are based on some questionable assumptions. The claim that prediction is impossible in economics is a generalization that overlooks the differences between many types of situations and types of prediction. In addition, it presumes that all predictions provide an opportunity for profit, an assumption for which there is no justification. An ability to predict stock market prices correctly would give an opportunity for profit, but what about an ability to predict that, because the government has raised taxes, consumer spending is going to fall? The claim also neglects the role of competition: if forecasting is a competitive activity, with free entry, why should super-normal profits be available?

Finally, the claim that methodology cannot be valid because otherwise methodologists would be scientific millionaires mistakes the role of methodology. It may be quite possible to make generalizations about what constitutes good scientific practice without being able to say what the next development in science will be.

The limitations of rhetoric

Suppose we were to accept the arguments just put forward concerning the possibility of doing methodology. This still leaves open the desirability of doing so. Why should we bother with methodology? The answer is that we need to ask critical questions about the way in which economic enquiry is being undertaken – in other words, we need to evaluate and appraise the way the discipline is developing. Economic methodology, understood as dealing with the principles of reasoning underlying the subject, is concerned with such evaluation in a way that rhetoric is not.

This process of appraising the process of economic enquiry is something that McCloskey does not consider necessary. He uses a Chicago-style argument about ‘the economy of the intellect running just fine’ even without any ‘methodological regulation’. To appreciate this viewpoint, it is important to note his remark, *‘Immo, civis Chicagonus sum, subspecies TP* (cf. Reder, 1982)’ (McCloskey, 1986, p. 9, n. 2). Reder’s definition of a ‘tight prior equilibrium’ theory, with which McCloskey here identifies himself, involves a commitment to the notion that the world is Pareto-efficient (or at least approximately so), a view to which many (most?) economists would not subscribe. It is hardly surprising, therefore, that he sees no need to appraise economic theorizing, and hence no need for methodology.

If we abandon this Chicago position, we find at least prima facie evidence that ‘the economy of the intellect’ is not running smoothly, many economists believing that the structure of incentives in the profession works in a seriously imperfect manner (cf. Colander, 1991). If we start investigating such problems, we start to investigate the principles underlying economic reasoning; we start doing methodology. It may be that we will never succeed, but to abandon methodology is to abandon the attempt to ask awkward questions concerning the status quo.

In view of the charge that methodology is attempting to impose criteria taken from outside economics, three points need to be made. The first is that the use of outside criteria could be viewed as implicit in the argument that it is the overlaps between conversations that keep the conversations healthy. Thus even if we were to accept the conversational perspective, there would be a role for outside criteria: disciplinary boundaries are to a certain extent arbitrary, so we might argue that overlaps between conversations in economics, philosophy, literature, linguistics, physics and other subjects are necessary. An instance of such overlap is the use of philosophical ideas by economic methodologists.

The second point is that outside criteria are relevant if economics is to be concerned with guiding policy. It may be that the conversation between economists is considered healthy by those participating in it, but outsiders, including those who pay economists' salaries, are entitled to ask what the subject's objectives are, and about whether these objectives are being achieved. If, for example, significant areas of economic theory have become nothing more than a mathematical game, this fact ought to be recognized, and its implications faced, even if the participants in the conversation believe it to be healthy.[10]

[10] Hutchison (1992) criticizes the way economics has developed since the 1950s on the grounds that the criterion of policy-relevance has, in significant areas of the subject, been abandoned. He blames this change of purpose for what he sees as methodologically unsound developments in economics since then. His criticism of the economists he terms 'the new conversationalists' centres on their failure to recognize that the methods a subject needs to follow are related to its aims.

Links between rhetoric and methodology

The anti-methodological positions of McCloskey and Weintraub rest on the ability to draw clear distinctions between rhetoric, methodology and the standards of the economics community. These distinctions are far from clear-cut because the critical traditions of the economics community, which are intimately connected with economists' rhetoric, are the result of a long historical process. This historical process has involved methodological arguments. It may be fashionable to argue that economists should turn their backs on philosophers such as Popper or Lakatos, but what about Jevons or John Stuart Mill, both as well-recognized for their work as philosophers of science as for their work on economics? 'Methodological' arguments are part of the heritage of the economics community. Persuasiveness and methodological ideas are inextricably linked, with the result that we cannot simply eliminate methodology.

McCloskey has been critical of economists' 'official rhetoric' and has used market arguments to question the need for methodology. Yet market arguments could equally well be used to argue against his dismissal of 'modernist' methodology. The conventions underlying academic writing, including writing in economics, presumably did not develop by chance. Writers will have written with a view to persuading some audience, and conventions (such as those implicit in the 'scientific paper') will have grown up as the largely unintended consequences of such attempts to persuade (cf. Backhouse, Dudley-Evans and Henderson, 1993). Why should such arguments not apply to methodology?

As I have pointed out elsewhere (Backhouse, 1995), Milton Friedman provides a good example of the dangers which arise from trying to separate methodology. It is easy to see Friedman's methodology as an example of modernism. If one does this, it is easy to see an inconsistency between his rhetoric and his methodology: he does not falsify hypotheses, but amasses empirical evidence for his theories; he does care about assumptions; he attaches an 'un-modernist' weight to history; and so on. Alternatively, one can start from his work and use this to make sense of his methodology – by refusing to accept a dichotomy between his explicit methodology and his implicit rhetoric. This approach, as demonstrated by Hirsch and De Marchi (1990), leads to a different view of Friedman's methodology. It is then important to ask whether or not this methodology has merits *vis-à-vis* more conventional methodologies.

These three arguments suggest that even if it were desirable to wean economists away from their affection for methodology, such a course could never be comprehensive. Some implicit methodological judgements (influenced by, amongst others, philosophers of earlier generations) would remain. Furthermore, if this campaign is directed against 'modernist' methodology, there is the danger that such a crusade may lead to the abandoning of practices and methodologies that are defensible on pragmatic, or even postmodernist terms. Far from improving the rhetoric of economics, an anti-modernist crusade might weaken it.

5 CONCLUSIONS

An appeal for clarity

Discussion of rhetoric and methodology has been bedevilled by confusion. When considering methodology, a number of important distinctions must be made:

- Prescriptive versus non-prescriptive methodology;
- Foundationalist versus non-foundationalist methodology;
- Implicit versus explicit methodology;
- Methodological writing by 'methodologists' versus that undertaken by economists in the course of their writing on economics;[11]
- Contemporary versus past methodological writing.

These distinctions are all different from each other, yet they have been conflated in the literature on rhetoric and economics, and the result has been confusion.[12]

Rhetoric and methodology

The rhetoric of economics *is* important. Certainly, methodological debate involves taking seriously the constructivist critique, as well as the reasons why economists argue as they do. The critics of methodology are right to say that we cannot turn to philosophers of science for readymade methodologies.

To analyse the rhetoric of economics, however, we must go beyond the over-simplified antithesis of rhetoric versus methodology. We will not get very far analysing rhetoric if we confine our attention to generalizations about persuasion, and to analogies between economics and literature. Analysing the rhetoric involves

[11] This distinction is not hard-and-fast, but it is workable. The crucial issue is specialization.
[12] One particular irritant has been the habit of using Methodology (with a capital M) to refer to something different from methodology (with a small m). The former is foundationalist, prescriptive and reprehensible; the latter is open-minded and defensible. The fact that Methodology is merely a subset (arguably a small one) of methodology, as the term is commonly used, easily gets forgotten, by the reader if not the author.

exploring the language of economics, relating economists' language to the purposes they are trying to achieve.[13] If we consider economic ideas in relation to what economists are seeking to achieve, then we cannot avoid facing up to the practice of methodological enquiry. Methodological enquiry, drawing upon but not dominated by the philosophy of science, is not only possible: it is vital for the health of the discipline.

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[13] This takes us into, amongst other areas, applied linguistics. See Backhouse, Dudley-Evans and Henderson (1993).

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Chapter 8

A decade of rhetoric*

(*Journal of Economic Methodology,* 2(2), 1995, pp. 293–311.)

The major conclusion of this review of McCloskey's Knowledge and Persuasion in Economics *(1994) is that, for all the promise of the programme he initiated a decade earlier, there has been little progress. The reason for this lack of progress is, I contend, McCloskey's anti-methodology position. Arguing against the possibility of doing methodology is an unproductive activity, not simply because the argument is wrong in the sense that it is taken much too far to be defensible, but because it distracts attention from much more important and interesting issues.*

One issue that is pushed aside by McCloskey's anti-positivism is that although positivist attitudes can be found, there is much more to the contemporary economics than this. As Chapters 16 to 18 make clear, understanding modern economics is far from being a simple task, for the discipline contains many methods and approaches, not all consistent with each other. Attempts to test economic theories by confronting quantitative predictions with empirical evidence coexist alongside a priori *methods in which basic theoretical premises are not considered open to serious doubt. To categorize them all as 'positivist' is a claim that could not be substantiated without much more careful analysis.*

Another important issue swept aside by McCloskey is that much more careful attention needs to be paid to why *economists adopt the rhetorical practices they do. It is all very well to denigrate the 'scientistic' style of the 'scientific paper', but such practices presumably developed for some reason. It may be true that the main reason is that economists are trying to appropriate to themselves the prestige of the natural sciences by imitating their writing style, but there are possibly more fundamental reasons. Given that applied linguists and discourse analysts have suggested good reasons for some aspects of scientists' writing styles (for example, the habit of using a standard format, with 'Introduction', 'Theory' and*

* A review of McCloskey (1994). Unattributed page references within this chapter are to this book.

'Conclusions', whether this is chosen voluntarily by the author or imposed by an editor), these need to be considered very carefully before dismissing them simply as 'scientism'.

There is also a danger in McCloskey's approach. 'The rhetoric of economics' has undoubtedly made many economists much more aware that they use a variety of rhetorical strategies, and that science is a more complex activity than they had realised. On the other hand, in so far as it serves to persuade economists that they can safely ignore economic methodology, in the sense of systematic enquiry into the principles of reasoning underlying economics, it is dangerous. There may be no grand scheme, such as Popperian falsificationism or Lakatos's methodology of scientific research programmes, that fits all areas of economics, but that is a far cry from claiming that methodology is a pointless exercise, or that methodologists cannot legitimately raise critical questions concerning the nature of economic knowledge and whether or not economists are behaving in a way that is appropriate to the ends they are hoping to achieve.

1 THE SETTING

It is now over ten years since McCloskey published his pathbreaking work on the rhetoric of economics (1983, 1986).[1] Though not the first to argue that we should pay attention to the way economists actually persuade each other, McCloskey was, more than anyone else, responsible for the literature that has since mushroomed on this subject. Though many others have joined in this new conversation, McCloskey has remained a leading figure, actively involved in debate and in seeking to open up new avenues of enquiry. His writing has always been controversial, for different readers have responded to it in very different ways. His own summary is that 'philosophers outside of economics', 'economists without analytic-philosophical leanings' and 'many humanists, journalists and social scientists' have understood his point quickly and accepted it, as have 'most of the methodologists of economics' (pp. 181–2). In contrast, other members of this last group have sought to read his work closely, finding many of his arguments confusing, imprecise and wrong. The present book provides an ideal opportunity to take stock, for it contains both a restatement of McCloskey's case for rhetoric and his replies to his numerous critics.

Before proceeding further, the reader should be warned that the author of this review is (like Blaug) someone who has, McCloskey claims, 'not thought much

[1] Published a year earlier in the US. The US and UK editions are identical.

about the claims of *Sprachethik* – tolerance, for example' (p. 100); who (like Coats, Caldwell, De Marchi, Hausman and McPherson) sometimes indulges in 'a McCarthyism of the center' (p. 280); who (like Blaug and Coats) evinces 'a lack of curiosity about the revolution in science studies (p. 265); who has noted 'grimly' the explosion of interest in the rhetoric of economics (p. 181); and who has missed the point of *The Rhetoric of Economics* in *'exactly the same way'* as Blaug, Caldwell, De Marchi, Coats, Roth, Hoover, Bellofiore and Hausman (p. 307; emphasis in original).

2 RHETORIC AND PERSUASION IN ECONOMICS

The book is divided into six parts – Exordium, Narration, Division, Proof, Refutation, and Peroration – definitions of these terms being provided to assist readers who are not trained in the classical rhetoric from which they are taken.

Exordium The beginning of the story, McCloskey confesses, was in 1964 when a young graduate student became an ardent convert to 'the new religion' of positivism. It provided a why to be scientific, but without the need to know very much about what one was studying. It was, a picture he uses repeatedly, 'a 3? × 5? -card philosophy of science'. In 1968, however, he began teaching at Chicago, and discovered that other social scientists, though not positivists, 'were not misled dolts'. He soon learned, as he put it, to kick the dead horse.

Narration McCloskey sees 'a conversation about the conversation [economics]' as having begun in the late 1980s. He suggests this might be interpreted either as an extension of an old, methodological, conversation started by J. S. Mill, or as arising from a battle between schools within economics. His preferred explanation, however, is that it arose from economics having, at long last, joined in the wider human conversation ('outsiders are surprised at how far economics since the 1940s has wandered away from the human conversation', p. 28). He then moves on to outline 'The rhetoric of this economics'.

Division[2] McCloskey makes four points in this section. (1) Economists, he claims,

> are neurotic about 'science'. They think that knowing, really knowing, means following something called 'scientific Method'. They think that if you don't know it that way then you don't know much.
>
> (p. 55)

[2] The setting out of points of agreement and points that are contested.

This 'magical' sense of the word 'science', he argues, is an idiosyncratic English usage of the term that should be abandoned. Science should be understood, as in the rest of the world, simply as 'systematic inquiry'. (p. 56). (2) Economics, he argues, can be, and should be, applied to itself. His main argument is his well-known argument concerning the impossibility of profitable prediction. (3) Philosophy of science (particularly Popperian or Lakatosian) leads to ways of reading economics that are too 'thin' to work.[3] (4) Rationality involves not 'Method' but moral virtues. In meaning it is close to terms such as 'sane' or 'reasonable'.[4]

Proof Here McCloskey documents the rise of a 'scientistic' style in journal articles in economics. This covers the use of language, the types of argument employed by economists, the implied author and the structure of economic articles. This raises many interesting issues, as does the chapter on 'analytical' Marxism, but the key chapters in this section are those on formalism, the movement that has transformed economic theory since either the 1930s or the 1940s.[5] Economists, McCloskey argues, have fallen in love with proofs. They have adopted the values, not of the physics department, but of the mathematics department. Because it is such an important argument, it is considered in more detail below.

Refutation This section, the longest in the book, comprises eleven chapters in which McCloskey answers his critics, most of them economic methodologists. Because of its importance, it is considered in detail below.

Peroration Having established that the metaphor of a conversation is persuasive, McCloskey goes on to argue that it applies not simply to economics, but to the economy itself. But his main point is that an awareness of rhetoric will force economists to do economics differently. Clearly style would be improved ('An audience of better readers of economics would demand that the writers be better' p. 386) but substance would change too, for style and substance are inseparable. Rhetorical awareness would force economists to adopt pragmatic, rhetorical standards, abandoning unpersuasive, positivist ones. Thus although rhetoric 'is consistent with any number of beliefs about the economy, between which one can

[3] Interestingly, he finds Lakatos's early work on the philosophy of mathematics 'thicker' than his later, more widely known, work.

[4] McCloskey points out that Blaug's rhetoric is 'that of the moralist, not the describer or rational reconstructer' (p. 94). This reading of Blaug is quite correct, as Blaug has himself made clear (Blaug, 1994).

[5] In dating this he cites Ward (1972) and Mirowski (1991), but overlooks Ingrao and Israel (1990).

toggle' (p. 395), it will, McCloskey predicts, result in substantial changes in economic theory.[6] Reverting to the theme of his opening chapters, McCloskey concludes,

> Rhetorically self-conscious argument, when all is said, is something like growing up. Perhaps the time has come, after a useful childhood spent in positivism, for economics to grow up too.
>
> (p. 396)

3 WHAT IS NEW?

Compared even with *The Rhetoric of Economics,* the book draws on a vast range of sources in philosophy, literature, rhetoric and many other fields. There is thus much that is new in the details even though the basic argument (that rhetoric matters and positivism/Methodology does not) is exactly the same. Amongst the changes we find, for example, (1) an emphasis on positivism as something that was once useful,[7] but which has outlived its usefulness; (2) the claim that rhetoric 'encompasses' the logic of enquiry (p. 36); and the abandoning of the argument that prediction is impossible in economics in favour of the weaker claim that *profitable* prediction is not possible. There are, however, three big issues to focus on. The first is that McCloskey provides a critique of economics that goes beyond anything found in his early work. The second is McCloskey's debates with economic methodologists concerning the thesis he put forward in 1983/85. The third is his new, very precise, answer to the question of why the rhetoric matters. All three merit discussion.

4 MCCLOSKEY ON ECONOMICS

Central to McCloskey's critique of economics is his argument that economists are, unlike physicists, obsessed with proving theorems. Economics has become, using the metaphor taken from his 1991 paper, 'a search through the hyperspace of assumptions'.[8] This is a conclusion that many economists have reached (for example Fisher, 1989). McCloskey's critique of this method of enquiry rests on what he calls the A-prime, C-prime theorem:

[6] These are discussed in detail below.

[7] It served as an argument against the a priorism of the 1930s (p. 5), and it gave economists the strength to carry on (p. 23).

[8] Though it does not weaken the argument at all (it may even strengthen it), it might be argued that a better metaphor would be searching through a hyper*plane* of assumptions, in that there are certain assumptions (notably rational behaviour) to which economists are generally committed. Assumptions cannot be changed in all dimensions.

Metatheorem on Hyperspaces of Assumptions

> For each and every set of assumptions A implying a conclusion C and for each alternative conclusion C' arbitrarily far from C (for example, disjoint with C) there exists an alternative set of assumptions A' arbitrarily close to the original assumption A, such that A' implies C'.
>
> (p. 138)

No formal proof is offered[9] – as an empirical scientist, he can leave that to the mathematicians – but recent work on game theory, suggesting that equilibrium outcomes are highly sensitive to detailed assumptions and choice of solution concept, supports it. If true, it implies that not much is achieved simply by being able to show that there is some set of assumptions, A, from which one's desired conclusions, C, can rigorously be deduced. It is necessary to know whether the assumptions are true to some acceptable degree of approximation. Economic rhetoric must become quantitative.[10] McCloskey argues that economists are simply fishing for theorems, discovery of new theorems being an end in itself, not part of any wider rhetorical plan. Scientists, he contends, think differently. Economics has become permeated with the values of the mathematics department. The scientific values of the physics or the engineering department are foreign to economics.

At the same time, however, McCloskey sees economics as having been extremely successful.

> Economics in its modern and mathematical form has grown into a brilliantly successful science. Unquestionably it has. Its arguments are for the most part true, and even when they are false they are interesting. Its facts are true and rich and astounding. People who disbelieve this ... have not read enough economics.
>
> (p. xi)

Juxtaposing these two theses raises many questions.

1. How can a discipline dominated by such an inappropriate set of values have been so successful?
2. If economics has been 'brilliantly successful', on what grounds can McCloskey claim that its values are inappropriate?

[9] McCloskey cites a game Richard Feynman used to play with mathematicians as empirical support for this theorem.
[10] And yet he is scathing about the 'positivist' claim that things must be quantifiable.

3. On what basis does McCloskey judge economics to have been successful?
4. In what sense is he using the terms 'true', 'false' and 'interesting'?

One answer that would make sense would involve a consensus theory of truth: truth about the economy is what the community of economists believes. But if McCloskey has a consensus theory of truth, it is hard to see the grounds on which he argues that an entire community has gone astray.[11] In order to make these judgements, McCloskey appears to have standards, going beyond those of *Spachethik,* but it is not clear what they are.

5 MCCLOSKEY ON ECONOMIC METHODOLOGY

McCloskey opens his 'refutation' of the arguments of the Methodologists with an attack on the idea of epistemology, the philosophical 'big brother' of Methodology, originally written in response to the reflections of Hausman and McPherson (1988) on the problem of standards in an inter-disciplinary journal. This exchange of views brings out McCloskey's attitude very clearly. He criticizes Hausman and McPherson for believing that 'Methodology' and 'Epistemology' are desirable. Their argument is that the exercise of informed judgement must rest on explicit or implicit epistemological principles (ibid., p. 6) from which it is a short step to the conclusion that such principles should be made explicit and analysed. This is methodology or epistemology. Why does McCloskey object to this conclusion?

1. He argues, citing Friedman (p. 183), that methodology bears no relationship to actual scientific work, whereas literary criticism, rhetoric and the like have illuminated 'every text of our civilization' (p. 183). He also claims that the fact that when we use mathematics or metaphors we are talking is interesting.
2. Methodological standards 'are in practice used as conversation-truncating sneers' (p. 186). In contrast, 'effective persuasion is what makes for free communities' (p. 188).
3. Literary criticism does not debar one from also assessing the merits of a poem or other text. It is thus not correct to argue that literary/rhetorical analysis undermines standards.
4. Philosophers have failed to find anything general to say about 'Truth' and 'Knowledge'. They should face up to this failure.

[11] Mäki (1995) argues that McCloskey moves between several, unreconciled, theories of truth.

These points, however, completely fail to answer the points that his critics are making. Maybe rhetorical analysis is interesting (I agree strongly[12]), but this is not to say that is a substitute for philosophical analysis, for the two types of analysis address different questions. Similarly we may agree that economists often use methodological arguments to end conversations, but this is an argument *in favour* of methodological analysis, not against it. Effective persuasion presumes standards, and why should these standards not be analysed? McCloskey's argument that, because the project of epistemology has failed to date, it should be abandoned, would fit well with, for example, Lakatos's injunction to abandon degenerating research programmes, but on what basis does McCloskey make such a pronouncement? Either his case rests on some implicit philosophy of knowledge (in which case there would appear to be an inconsistency at the heart of his argument) or he has no basis for this opposition to methodology.

Having disposed of the argument that there is a role for methodology/ epistemology in analysing standards, McCloskey attacks a particular type of argument that has been levelled against him: what he terms the *'tu quoque'* argument. This is the argument that someone is relying on premises that they explicitly reject. Thus relativism (the argument that there are no absolute standards) is self-contradictory, for it involves putting forward an absolute standard (that there are no such standards). Just as easy, he argues, is the rhetorician's *tu quoque:* 'a philosopher is committed to rhetorical thinking at the very moment of arguing against rhetoric' (p. 200). 'The game of three line *tu quoque,* popular though it is among philosophers, is,' McCloskey argues, 'a trifle silly' (p. 201). But what is wrong with arguments that are three lines long, and why is it silly to point out inconsistencies in peoples' arguments? What matters is the substantive arguments that McCloskey by-passes by his 'Methodological' argument that 'three line *tu quoques'* are, in principle, silly. But this argument that McCloskey is using a 'Methodological' argument to argue against 'Methodology' is itself a *tu quoque.*

McCloskey goes on to criticize 'Armchair philosophers of economics' (notably Hausman and Rosenberg), 'The Popperians' (notably Peter Munz) who attempt to do philosophy of science without epistemology, what he terms Rosenberg's 'Reactionary modernism', and a range of other critics of his work (notably Mäki, Blaug, Coats, McPherson, Hoppe (an 'Austrian'), Heilbroner, Rossetti and Mirowski). He also includes a dialogue with a friendly critic, Klamer.

[12] Backhouse (1993), Henderson, Dudley-Evans and Backhouse (1993).

6 ECONOMICS IS RHETORICAL: SO WHAT?

Few, if any, of McCloskey's critics have denied that economists use rhetoric, or that the rhetoric of economics is an interesting subject for study. But many critics considered that, despite claiming that it would have a big effect, he had failed to show just *how* an understanding of rhetoric would, in practice, affect economics. McCloskey's most substantial explicit argument was that understanding rhetoric would improve economists' ability to communicate with each other. Beyond this there was the implicit argument that, once certain rhetorical devices had been exposed, they would lose their force. Samuelson's parading the authority of mathematics, and Muth's 'scientism' are perhaps rhetorical devices that derive their power from the reader's not seeing through them. In the present book, however, he provides some concrete examples.

(1) Pressure from readers would force economists to write well:

> Attractive prospects open: of economic writing without table of contents paragraphs ('The organization of this paper is as follows') or without pointless acronyms ('The coefficient on DMWITSCI is significant at the .05 level and the coefficient on FAKESCHL at the .01 level').
>
> (p. 386)

What is missing, however, is a discussion of why economists might choose to write like this. Many linguists would hesitate before being so dismissive about aspects of style. The increased use of a separate 'Introduction', for example, can be related to changes in the way scientists *read* journals, and the need to catch a reader's attention. Similarly, in econometric studies, it is frequently very efficient to use variable names such as those McCloskey cites – the reader knows immediately what the text refers to. It may be true that many economists are unable to write well, but there is no reason to believe that improving economists' writing skills would seriously improve the discipline.

(2) The economy depends on the faculty of speech, therefore 'the economy will *require* verbal interpretation' (p. 377; emphasis added). Markets 'will *need* to be read in terms of human intentions and beliefs' (ibid., emphasis added). He thus dismisses Rosenberg's closely argued case to the contrary,[13] without even discussing it.

(3) Economists would have to abandon discussion of the macroeconomics of closed economies, and they would not confuse openness with size (as do Kindleberger, Tobin, Lucas and Friedman). Macroeconomics should be rewritten.

[13] Rosenberg (1992).

> Throw away all the previous work. Because we are not paying attention to our rhetorical standards, we economists blew it. Amazingly, entirely. Modern macroeconomics is erroneous. (Don't get mad: think about it.) The theorizing is misinformed and therefore irrelevant to an economy in a world. The empiricism is wrong.
>
> (p. 389)

As a European, who teaches macroeconomics from a textbook that puts the real exchange rate centre-stage, and which brings the current account surplus even into discussions of consumption and saving behaviour, I fully support McCloskey's criticism of closed-economy macroeconomics. But surely he has short-circuited the argument? The issue is whether closed-economy models are good enough approximations for the purposes in hand, and in deciding this, size relative to the world economy may be important. These arguments have not been addressed.

(4) In the debate over the relevance of perfect competition, 'We should come to agree on some particular, human, rhetorical standard by which the quarrel can yield progress' (p. 391). Good prediction might provide such a standard, but the decision about how to interpret this is a rhetorical one, dependent on our purposes. But what reason is there to assume we would agree any more easily than at present?

(5) McCloskey implies that a rhetorically aware economics be more cumulative than economics is at the moment.

> Economics since the war has been mostly noncumulative. What do we know about international trade that we did not know in 1965? Oh, yeah? What large issue in economics since 1940 has been settled by an econometric finding. I said 'large'. Why has economic history, where arguments are open and broad-based, mainly because its practitioners are forced to speak to both economists and historians, made cumulative progress since 1960, and labor economics, similarly catholic in its arguments, since 1970? What argument about the economic world has general equilibrium theory advanced since 1950? I said, 'about the economic world'.
>
> (p. 393)

Economics would become more like economic history or evolutionary biology.

These examples reveal very clearly what it is that McCloskey hopes the conversation about the conversation will accomplish. Economics has, he now claims, taken a number of wrong turns, which he attributes to the damaging effect of positivism on economists' standards. Once freed from positivism, economists will start to adopt more pragmatic standards, becoming open to a wider variety of forms of argument and evidence. Though he does not cite Peirce, preferring Dewey and Rorty, his faith in the ability of pragmatic decision-making to result in better economics resembles Peirce's faith in the method of science to lead scientists towards the truth.

The main weakness in the argument, it seems to me, is its assumption that economics is the way it is because of 'positivist' attitudes. Much of the subject's explicit rhetoric is 'positivist' or 'scientistic', and this rhetoric no doubt influences many graduate students. Many of these, however, as Klamer and Colander (1990) have shown, remain profoundly sceptical about this rhetoric. More fundamentally, positivism is a profoundly *empirical* philosophy of science – indeed, one of the problems with logical positivism was that the notion of empirical observation on which it rested was too weak to bear the immense burden placed upon it. In contrast, much modern economics is, as McCloskey has eloquently argued, is very *un*empirical. My conjecture is that to understand modern economics and the intellectual values that underlie it, we will need to delve into the history and the sociology of the profession. McCloskey's belief that there is a single cause, namely positivism, is unpersuasive.

7 MCCLOSKEY'S RHETORIC

McCloskey accuses his economic-methodologist critics of having failed to understand his message, which is that he, along with people like Arjo Klamer and Roy Weintraub, is 'advocating the study of how economists actually persuade each other and the world' (p. xv). But he has persistently either misunderstood or misrepresented his critics. As far as I know, *none* of McCloskey's critics has denied that it can be interesting, and even valuable, to examine how economists actually persuade each other. Hausman (1992) has argued the case for empirical philosophy of science; de Marchi (1992) has advocated 'recovering practice'; I (chapter 7 [1993]) have argued that rhetoric and methodology are complementary, and that both should be pursued. What McCloskey's critics object to is not this general thesis about the value of studying rhetoric, but specific arguments he uses, and his tirades against methodology. That is the reason why his critics have fastened on the first three chapters of *The Rhetoric of Economics.*[14]

McCloskey preaches *Sprachethik,* or conversational ethics, arguing that it is such values are fundamental to science.

> Don't lie; pay attention; don't sneer; cooperate; don't shout; let other people talk; be open-minded; explain yourself when asked; don't resort to violence or conspiracy in aid of your ideas. These are the rules adopted by the act of joining a good conversation.
>
> (p. 99)

He alleges that 'Methodologists' care nothing for such values.

> In the writing of the Methodologists, any old violation of the *Sprachethik* is permitted. Anything goes.
>
> (p. 186)

But how does he square *Sprachethik* with attributing to people views they have never held,[15] and generally seeking to ridicule critics? My experience is that his critics have tried seriously to understand his arguments, and they generally do follow the rules of *Sprachethik,* arguably more closely than McCloskey does himself.

Where McCloskey's criticisms about the use of 3? × 5? card methodological slogans to truncate discussion do have some force is when applied to *economists.* Sometimes he makes it clear that this is his target: 'in a Chicago seminar you can shut someone up by sneering use of a Methodological rule about the unrealism of assumptions' (p. 186).

> Like nations and religions, the schools in economics and in philosophy maintain their solidarity and their definitions of barbarians by means of Methodological talk. Such-and-such is 'serious scientific work', namely, the way we Hellenes talk; the rest is barbaric, bar-bar-bar.
>
> (ibid.)

[14] For example, referring to the later chapters of this book, I have written 'He [McCloskey] produced a series of brilliant case studies from which he drew the conclusion that what persuaded economists was not empirical testing or successful prediction, but things that no explicit methodology took into account: mathematical virtuosity, arguments by analogy, symmetry and so on' (Backhouse, 1994, p. 10).

[15] I have never thought or written that 'The rhetoricians are attempting to ... Debunk our accepted knowledge of science or mathematics ... Expose science as a matter of force and fraud ...' or any of the other statements listed in his 'Barnes table' (p. 286). I conjecture that the same goes for Blaug, McPherson, Hausman and Rappoport too.

Such rules prevent Austrians working with continuous production functions, or 'fresh-water rational expectors' using macro arguments without microfoundations (ibid.). Even here, however, the question arises as to the grounds on which we argue against such narrowness, for in arguing that a broader, more tolerant approach is desirable, McCloskey is making a methodological judgement.

Confusion arises in that McCloskey includes in the category of 'modernist Methodologists', not simply economists who use methodological arguments in this way, but those who analyse such arguments. Many of these methodologists share McCloskey's belief that the range of arguments employed by economists has become too narrow. They have turned to methodology because they wish, like McCloskey, to understand the nature of economic enquiry.

8 CONCLUSIONS

Newcomers to the subject will, especially if they share McCloskey's view of economics, find *Knowledge and Persuasion in Economics* full of provocative, stimulating ideas, drawing on a vast range of literature. Those who are looking for progress beyond the arguments found in *The Rhetoric of Economics* are, however, likely to be disappointed. The challenging part of that book was its case studies, but these remained tantalizingly brief forays into uncharted territory. What was required was (in this reviewer's opinion) further case studies, detailed textual analysis, and above all the development of techniques that would enable the argument to be taken a step further. McCloskey has drawn on literary criticism, and he cites work in applied linguistics and the sociology of scientific knowledge, but the results are disappointing. Instead of new case studies applying and developing the insights that can be obtained from rhetorical analysis, we find a series of attempts to put down his critics, often without taking their arguments seriously – a book that goes against the *Sprachethik* that he espouses.

Time and time again, McCloskey makes statements that presume methodological standards that go beyond *Sprachethik,* yet he persistently argues that Methodology, which is nothing other than the analysis of such standards, is not only a waste of time but also positively harmful. Why? Perhaps the answer is to be found in McCloskey's youthful attachment to the militant positivism so eloquently described in the opening chapters of the book. It has often been argued that the most passionate opponents of any religion or political dogma are those who were once converted, but who have since lost their faith.

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Chapter 9

Should economists embrace postmodernism?*

(*Keynes, Knowledge and Uncertainty,* edited by Sheila Dow and John Hillard. Cheltenham and Brookfield, VT: Edward Elgar, 1995, pp. 357–66.)

This chapter addresses the issue of postmodernism through considering two texts written from an explicitly postmodern perspective. Amariglio and Ruccio argue that modernism sought to 'tame' uncertainty, and that a truly postmodern economics would face up to the existence of radical uncertainty. Keynes, they contend, had moments when he glimpsed at such a vision. Klamer's method is to contrast the 'modernist' rhetoric of Samuelson with the non-modernist writing style of Keynes. The claim made in this chapter is that, whilst there is some basis for such arguments, they are taken much too far. Amariglio and Ruccio, in their rejection of prediction as a modernist concern, fail to address the questions of what it is that economists are, and should be, trying to achieve. These aims might easily be such that 'modernist' methods are appropriate. This is not to say that they are – merely that the question of aims needs to be addressed if their critique is to be effective. Klamer is undoubtedly correct when he points to a difference between the literary styles of Keynes and Samuelson, and when he argues that this has much to do with Samuelson's vision of economics as a science. However, it is also possible to view Keynes's General Theory *in a way that emphasizes the closeness of Samuelson to Keynes. This makes the point that there are many dimensions to rhetoric.*

This chapter reinforces the argument made in Chapters 7 and 8 that to use 'modernism' as a category with which to criticize contemporary economics is to evade many important issues. It is, therefore, sceptical about postmodernism not because rhetoric or discourse analysis cannot be used to reveal important things about the way economists argue – they can – but because they way the concept is often used is to create an oversimplified, and therefore misleading, picture of what is going on in economics.

* I wish to thank Sheila Dow and Brian Loasby for helpful criticisms of an earlier draft.

1 INTRODUCTION

Modernism and postmodernism are clearly such important phenomena in twentieth-century culture that it is difficult to place economics in the context of wider intellectual movements without taking them very seriously. Klamer (1995) and Amariglio and Ruccio (1995), however, claim more than this – that viewing economics from a postmodern perspective explains why certain rhetorics, or ways of arguing, are privileged over others and makes sense of uncertainty, especially in relation to contemporary economics, thereby pointing to an alternative conception of economic theory. In choosing Keynes, Samuelson and Harrod as the examples around which to develop their more general theses, Amariglio, Ruccio and Klamer also provide new interpretations of the part of the history of macroeconomics. Though he does not use the term 'postmodernism', these arguments relate to some of the issues Loasby (1995) raises concerning what are acceptable explanations in economics. The purpose of this note is to respond to these various claims.

2 MODERNISM AND POSTMODERNISM

Amariglio and Ruccio pick out three main characteristics of modernism:

1. The possibility of certain knowledge;
2. The role of reason in establishing universal meanings;
3. That 'Man' is the proper origin and object of knowledge (Amariglio and Ruccio, 1995, pp. 334–5).

Postmodernism, in contrast, emphasizes incommensurability and plurality of knowledges. Amariglio and Ruccio find 'postmodern moments' in anything that transcends the boundaries of modernism.

> And it is these elements of 'undecidability' and 'indeterminacy' that threaten to overrun the boundaries of modern economics and that, *therefore,* represent the postmodern moments of uncertainty.
>
> (ibid., p. 335; emphasis added)

There are clear problems with such a dualism.[1] The quest for certain knowledge predates modernism (Descartes), whilst many of the economists whom one might wish to classify as modernist defend their theories in Popperian terms. Popper,

[1] The word 'therefore' in the quotation can be read as implying that anything that is not modernist is postmodernist – that there is no third category.

though clearly not postmodernist, emphasized the provisional nature of all knowledge, and the absence of any completely secure epistemological foundations. It is, therefore, not clear that such a (purely epistemological) definition of modernism is satisfactory.

In contrast, Klamer's list of eight characteristics of modernism (Klamer, 1995, pp. 319–20) has a different emphasis: modernism finds universal meanings in invariant structures that underlie appearances; it favours formal, abstract reasoning; it is ahistorical and self-referential. But above all, the machine is the dominant root metaphor. Klamer's list is, as he freely admits, imprecise. If one is considering art, architecture or literature, this may not matter: modernism is an identifiable movement, seen as such by its adherents, which the list is trying to characterize. If one is considering economics, however, where it is necessary to define the boundaries of modernism at the same time as characterizing it, it is, I would suggest, more important to have a precise definition. This point will, I hope, be illustrated in what follows.

3 UNCERTAINTY AND COHERENCE

For Amariglio and Ruccio, undecidability and indeterminacy are crucial characteristics of postmodernism. Modernism sought to 'tame', 'conquer' or 'domesticate' uncertainty by reducing it to randomness, risk and probability. Such reduction is important in making it possible to maintain the modernist commitment to viewing economic agents as rational decision makers (as economists understand this term). It is this commitment that leads to 'true or radical' uncertainty having implications that many economists would consider nihilistic – true uncertainty would make it necessary to abandon or radically alter the notion of rationality that underlies most present-day economics.

Loasby is equally critical of the notion of rationality, but unlike Amariglio and Ruccio, who seem ambiguous as to whether the consequences of accepting true uncertainty are nihilistic, he is emphatic that 'giving up the economist's peculiar concept of rational choice ... does not mean giving up the idea of taking decisions for good reasons' (Loasby, 1995, p. 6). He sees no problem with indeterminacy, arguing that single-exit, or deterministic, models are inappropriate whether at the level of economic theory (there may be more than one reasonable choice for the economic agent to make) or methodology (there may be more than one reasonable economic theory).

Though some of his arguments might be seen as postmodern, however, he does not use the term. Instead he chooses to approach knowledge as having a psychological base. Acceptable explanations are ones with which people are comfortable – which maintain ‘the tranquillity of the imagination’ (ibid., p. 12). He develops this by arguing that tranquillity requires the discovery, or creation, of connecting principles. Such connecting principles do not need to be as general as universal maximizing behaviour, or as rigorous as the modern theory of competitive equilibrium, but may be much more limited in scope. He argues that Austrian and managerial theories, and above all Marshall’s economics, are capable of providing acceptable connecting principles, even though they have never been expressed rigorously, and even though they are not all mutually consistent. This search for ‘connecting principles’ has something in common with the search for fundamental underlying principles that Klamer associates with modernism.

One of the ironies of this linking of postmodernism and uncertainty concerns Terence Hutchison’s *The Significance and Basic Postulates of Economic Theory* (1938). This book is accepted by many as marking the introduction of logical positivism (surely a modernist movement) into economics. Yet the message of the book was that the assumptions of perfect knowledge and rationality might have to be abandoned.

4 ‘MODERNIST’ RHETORIC

Klamer uses an analysis of Samuelson’s article on multiplier-accelerator interaction to show that it, unlike Keynes’s *General Theory,* ‘embodies the introduction of modernistic elements into economics’ (Klamer, 1995, p. 318). In one sense this is all obvious and well known. Samuelson was inspired by physics and, as McCloskey (1986) has convincingly shown, used many rhetorical devices in his attempt to persuade economists to see mathematics as central to their discipline. In contrast Keynes, though trained in mathematics, was a pupil of Alfred Marshall, with a long career behind him as a journalist and political campaigner. If modernism is to do with attitudes towards mathematics, then clearly Klamer’s case goes by default. I would suggest, however, that this is not the only way in which the rhetoric of Samuelson’s ‘Interactions’ article (1938) can be read. Compare its opening paragraph not with Keynes’s ‘Notes on the trade cycle’, a chapter close to the end of the book, but with the opening paragraph of the *General Theory* (1936). Consider Samuelson’s opening paragraph, sentence by sentence.

> [1] Few economists would deny that the ‘multiplier’ analysis of the effects of governmental expenditure has thrown some light upon this important problem.
> (Samuelson, 1938, p. 75)

This technique of 'suggesting agreement where there is little' is also Keynes's strategy. He writes of

> the *classical* theory of the subject, upon which I was brought up and which dominates the economic thought, both practical and theoretical, of the governing and academic classes of this generation, as it has for a hundred years.
>
> (Keynes, 1936, p. 3)

A footnote to the word 'classical' refers the reader not only to well-known sources, but also to the work of Pigou. Four pages later he refers to Pigou's *Theory of Unemployment* (1933) as 'the only detailed account of the classical theory of employment which exists' (Keynes, 1936, p. 7). Throughout the rest of the book, Pigou is selected as his target. Thus Keynes has managed to present a recent, mathematical exposition of the subject, that was understood by few economists (Keynes's colleagues had great difficulty in understanding Pigou's book) as representative of a well-understood orthodoxy.

As for Samuelson's addressing only economists ('Few economists would deny'), though Keynes hoped to reach a wider audience, his main target was economists: 'This book is chiefly addressed to my fellow economists. ... its main purpose is to deal with difficult questions of theory' (ibid., p. xxi)

> [2] Nevertheless there would seem to be some ground for the fear that this extremely simplified mechanism is in danger of hardening into a dogma.
>
> (Samuelson, 1938, p. 75)

Talk of dogma is strongly reminiscent of Keynes, with his talk of economists being 'wedded to' the classical theory and of the difficulty of escaping from the old ideas which 'ramify ... into every corner of our minds' (Keynes, 1936, pp. xxi and xxiii).

> [3] It is highly desirable, therefore, that model sequences, which operate under more general assumptions, be investigated, possibly including the conventional analysis as a special case.
>
> (Samuelson, 1938, p. 75)

Though the language of models and assumptions is different, this is precisely Keynes's strategy in the *General Theory* – arguing that his theory was more general than the classical, and that the assumptions which defined the classical special case 'happened not to be those of the economic society in which we actually live' (Keynes, 1936, p. 1).

To appreciate the similarity of Samuelson's rhetoric to that of Keynes, contrast it with the introduction to what is arguably one of the seminal papers in modern general equilibrium theory, von Neumann's 'A model of general economic equilibrium' (1938).

> The subject of this paper is the solution of a typical economic equation system. The system has the following properties: (1) Goods are produced not only from 'natural factors of production,' but in the first place from each other. ... (2) There may be more technically possible processes of production than goods ... In order to be able to discuss (1), (2) quite freely we shall idealize other elements of the situation ... Most of these idealizations are irrelevant, but this question will not be discussed here.
>
> (ibid., p. 296)

The contrast between this and the opening paragraph of Samuelson (1938) could hardly be greater.[2] It is worth noting that much of the most prestigious work in post-war economic theory (e.g. Debreu, 1959) is much closer in style to von Neumann than to Samuelson.

The purpose of these remarks is, I repeat, not to deny that there are important differences between Keynes and Samuelson. It could hardly be otherwise. It is to point out that the picture is rather more complicated than Klamer suggests. Samuelson sees the role of mathematics very differently from Keynes, and as a result presents his arguments in a different style, using different rhetorical devices. Modern economists have followed Samuelson, not Keynes. There is, however, as the comparison with von Neumann suggests, more to their rhetoric than that. Many of the features of Samuelson's rhetoric to which Klamer has drawn our attention follow from his addressing, like Keynes, a different audience from the mathematical audience being addressed by von Neumann, where conventions and, more important, the purposes of economic writing were different.[3, 4]

[2] In other respects there are of course similarities between von Neumann's style of argument and Samuelson's. Some of the features of Samuelson's *Foundations* to which McCloskey (1986) has drawn attention are also features of von Neumann's work.

[3] By way of a corollary it is worth noting that the above argument illustrates the point that rhetorical analysis and postmodernism are by no means coterminous.

[4] It is perhaps appropriate to compare Samuelson with Robert Fogel (see McCloskey, 1986) as seeking to create a new audience of economists who accepted mathematical modes of argument. Von Neumann, like Albert Fishlow, was not.

5 THE PURPOSES OF ECONOMIC ENQUIRY

Amariglio and Ruccio, and Klamer, claim that economists are resistant to the idea that economics should be seen as part of a broader culture:

> many economists remain averse to thinking in terms of the 'embeddedness' of economic theory in a larger cultural setting.
>
> (Amariglio and Ruccio, 1995, pp. 337–8)

> Mapping this intricate and variegated mosaic that economics presents onto this modernist frame is a daunting task. Resistance in the discipline is strong. ... Parallels between the scientific practices of economics and artistic ventures are not supposed to exist.
>
> (Klamer, 1995, p. 322)

But is this significant, even if it is true? Could it not be that economists simply see the issue as irrelevant to their purposes?

To see the significance of this, consider the question of prediction. Though not mentioned explicitly by Amariglio and Ruccio or Klamer, this is commonly seen as a modernist preoccupation. Yet the need for prediction, broadly interpreted, is a consequence of economics being a policy science. The desire for predictions, can plausibly be argued to follow from the increased role of the state in economic life and the growth and development of business organization, both of which have resulted in an increased demand for predictions. *In part,* many of the developments in economics that are commonly branded as modernist follow from economists' attempts to fulfil this demand. Uncertainty has to be 'tamed', so economists have believed, in order to be able to provide useful predictions when it is present. Formal analysis is required in order to know what a theory does predict. Econometrics provides a means for making predictions quantitative rather than simply qualitative. It may, of course, be that economists have been wrong to believe that theirs was the best route to the provision of useful policy advice. But to make such a case it is insufficient merely to dismiss the desire for prediction as modernist. It is necessary to find alternative ways of dealing with decision-making under uncertainty and to show policy-makers, whether government or business, that these are appropriate – persuading them, for example, that rather than asking for deterministic or even probabilistic predictions it is enough to think in terms of alternative scenarios (Loasby, 1992). Nihilism is simply not an available option – decisions have to be made.

Failure to pay sufficient attention to the purposes of economic writing also raises questions concerning Klamer's analysis of Samuelson's article. As applied linguists have shown, many aspects of the way in which journal articles (and other types of writing) are written can be related to reading practices and the way academic communities are organized. As an example, consider Klamer's comments on having a 'Conclusion' to an article.

> Modern articles call for closure. That is the function of the conclusion. The implied reader is made to feel that there was a reason for going through the trouble of reading the preceding text.
>
> (Klamer, 1995, p. 331)

This may be intended as a light-hearted way of bringing his own article to a close, but several comments are worth making about it. Though he uses the term 'Modern' articles it is natural to infer that he means 'modernist'. Having a conclusion is thus part of the formal, closed scientific style associated with modernism. But why should readers not assume articles are written with a purpose, and expect authors to make clear what that purpose is? If any writing with a purpose is modernist, then it is hardly surprising if economists are sceptical about postmodernism. There is, however, another point which raises issues concerning Klamer's way of approaching textual analysis (see Backhouse, Dudley-Evans and Henderson, 1993 for further discussion of this point in relation to McCloskey's work). Klamer analyses Samuelson's article without reference to the genre of which it is an example. Applied linguists argue that, like any genre, academic journal articles can best be understood in relation to the purposes of their authors, the reading practices and the nature of the community within which they are read. Klamer notes that disciplines become inward-looking, but goes no further than this. As an example, of the difference reading practices make, consider the practice, dubbed 'Modern' by Klamer, of having a 'Conclusion'. It has been argued that the increased volume of academic writing means that journals are now read much more quickly, with the result that scientists no longer have time to do more than skim through most articles. If an article is to be read, therefore, it must attract attention quickly in a way that was not necessary a century ago. What Klamer sees as a 'Modern' practice may thus reflect changes in the structure of the profession rather than simply 'modernism'.

In contrast, Loasby is alert to the aims of economic enquiry.

> Now one cannot get very far in studying knowledge without considering the means whereby knowledge may be obtained, but the means should be studied in relation to the ends which they serve.
>
> (Loasby, 1995, p. 7)

This may explain why, despite his acceptance of many 'Keynesian' arguments about uncertainty, he does not follow the route of talking in terms of postmodernism. He is emphatic that though such decisions cannot satisfactorily be explained in neoclassical terms, firms normally have good reasons for their decisions and that, provided that account is taken of circumstances and the way in which decisions are made, these decisions can be understood.

6 MODERNISM, POSTMODERNISM AND THE HISTORY OF ECONOMIC THOUGHT

Amariglio and Ruccio provide a fascinating account of how Keynes glimpsed at the possibility of 'radical' uncertainty, moving between such moments and theorizing which attempted to provide a more formal analysis of economic activity. But is it helpful to analyse this in terms of modernism and postmodernism? The example of Terence Hutchison shows that one can retain 'modernist' ideals (notably prediction) and at the same time accept that there may be limits to prediction in economics, and even to rationality itself. It may be that Keynes should be located on the boundary between modernism and postmodernism, but to establish this we should, I suggest, explore the relationship between his economics and his involvement in the philosophical and artistic movements of his times.

This link with culture is much more explicit in Klamer's, much more persuasive, definition of modernism. Modernism, for Klamer, is a cultural movement which 'influenced' Samuelson. I have little doubt that he is, up to a point, right. Yet, I am not convinced that his account provides the best history that can be told. One may be influenced by something of which one is unaware, but in the absence of direct links it is natural to ask how the influence came about. More important, explanations in terms of modernism and postmodernism need to be weighed up against alternative explanations. Economists' preference for formal, axiomatic reasoning based on the assumption of rationality may be a modernist characteristic, but there are other stories that can be told. The mathematization of economic theory has roots which antedate modernism.[5] It is arguable that once economists had accepted the logic of marginalism, notably in its Jevonian and Walrasian variants, they were well on the road to the mathematization of the subject. The formalization of economics can be linked to an increased focus on resource allocation problems, to developments in

[5] Ingrao and Israel (1990) trace these roots back to the eighteenth century.

the structure of the economy, to the professionalization of the subject, and to other developments that are not dependent on modernism. Once can accept that economists sought to follow the example of physics (Mirowski, 1989) or mathematics (Ingrao and Israel, 1990; McCloskey, 1991) without accepting that they necessarily embraced the tenets of modernism.

Where Klamer claims that modernism influenced Samuelson, Amariglio and Ruccio make the seemingly weaker claim that modernism 'enabled' developments in economics. I interpret this as meaning that without the intellectual values supplied by modernism, economics could never have developed in the way that it did – that modernism was a necessary condition for the emergence of contemporary economics. This is very plausible, but the meaning of such a statement depends critically on how modernism is defined. If modernism is defined in terms of a preference for formal, axiomatic reasoning (one component of Klamer's definition) it is tautological. If modernism involves the believing that certain knowledge is attainable (part of Amariglio and Ruccio's definition) it is false: Popperian or even constructivist epistemology will do instead. If modernism is defined with reference to culture, its relation to the evolution of economics is something that needs to be established.

7 CONCLUSIONS

The notion of postmodernism, with its stress on plurality of meanings and discourses, can serve to widen our horizons – to make us aware that there are other ways to do economics. In particular, it is arguable that modernist ideas made it easier for economists to embrace a set of intellectual values in which logical rigour ranked higher than empirical relevance, and in which the desire for closed models and internal coherence led to the exclusion of all aspects of behaviour other than one concept of rationality.[6] To this extent, therefore, postmodernism is valuable.

For most economists, however, such general methodological arguments will be unconvincing, not because of any commitment to modernism, but because they cannot perceive alternative ways of thinking about the economic problems they are trying to solve. It is always difficult to discover new concepts that can provide alternatives to old ways of thinking. It is equally important, however, to show how these new ways of thinking can answer the problems to which economists have to

[6] I owe this point to Brian Loasby. It has much in common with Hausman's (1991) thesis that economists' attitudes towards psychologists' theories of behaviour is explicable in terms of the goal of making economics a 'separate' science.

provide answers.[7] This is well illustrated by Loasby's work. It goes beyond most critiques of economics that are centred on postmodernism in that it couples 'internal' criticism with suggestions that go beyond Keynes's as to how economists might construct a new theory of how people behave in the presence of uncertainty. There are many mainstream economists, I suspect, who will find his ideas interesting, and who would in principle be prepared to work with them, but who find them no more than suggestive. They will not be convinced that, however suggestive his ideas, it is possible to create a new economics that is capable of answering the questions to which answers are required.

Klamer, Amariglio and Ruccio all endorse the indeterminacy and pluralism that is associated with postmodernism. For critics of mainstream economics, I suggest, this is a dangerous strategy. The argument that standards are relative to specific discourse communities, and that work should be appraised only from within the relevant community, constitutes the ideal defence of contemporary economic theory.[8] Even arguments about indeterminacy cause few problems for a subject that increasingly accepts game theory instead of general equilibrium theory as its unifying paradigm.

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[7] This may, of course, be done in two ways: by showing that new ways of thinking answer old questions, or by changing economists' views on the questions to which answers must be found.

[8] Backhouse (1992).

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Part III

Economists on methodology

Chapter 10

The value of Post Keynesian economics

A neoclassical response to Harcourt and Hamouda

(*Bulletin of Economic Research* 40(1), 1988, pp. 35–41.)

The origins of this chapter lie in my being asked to act as one of the referees on Harcourt and Hamouda's survey article on Post Keynesian economics. I offered such comments as I hoped would be helpful to the authors, and told the editor that whilst I found the paper a valuable statement of various Post Keynesian positions, there were several points where I was in fundamental disagreement. His response was that I would be welcome to write up these views and submit them to the journal. This chapter is the result. What it shares with several of the chapters in Part II, is an aversion to programmatic statements that are not followed up by more detailed work. Though there is interesting Post Keynesian work, there also appears to be much rehearsing of theories that have been around for many years. The contrast with mainstream economics, where the subject has been transformed almost beyond recognition since the early 1970s, is striking. This chapter is, therefore, written from the perspective of an economist who finds much that is attractive in Post Keynesian economics, but who is sceptical about how much progress is being made, and who feels that many Post Keynesians are failing to face up to the variety that is found within contemporary mainstream economics. The positive things I find in Post Keynesian economics are, therefore, rather different from what Harcourt and Hamouda find. I see it not as an alternative to mainstream economics, preferring to focus on specific examples of theorizing rather than broad categories like these, but as a valuable source of critical questions concerning particular areas of economics.

Looking back on this paper after ten years, I still agree with the comparison of neoclassical and Post Keynesian economics. I would, however, be more tentative in arguing that neoclassical economics has been successful. That it is theoretically progressive seems beyond doubt. What is more problematic is the extent to which it has been empirically *progressive. A case can be made (see Chapter 2 above) but much more work is required, for the evidence is not at all clear-cut.*

1 INTRODUCTION

Harcourt and Hamouda (1988) have performed a very valuable service in providing a clear overview of Post Keynesian Economics (henceforth PKE). Although they have not provided any simple definition of PKE they have made it much easier for outsiders to identify its main themes. Their survey raises a number of questions for neoclassical economists, in particular how to respond to Post Keynesian criticisms of their methodology, and how to assess the positive contributions of PKE to specific areas of economics. This note is simply an attempt to respond to these questions, looking first at Post Keynesian criticisms of neoclassical economics, and then at PKE itself: it is not a comment on the work surveyed by Harcourt and Hamouda.

2 THE POST KEYNESIAN METHODOLOGICAL CHALLENGE TO NEOCLASSICAL ECONOMICS

The main methodological critique of neoclassical economics offered by Harcourt and Hamouda is their claim that we need to adopt a ‘horses for courses’ approach. There are two points to make in response to this. The first is that, to a certain extent, contemporary neoclassical economists do adopt such an approach. In the 1950s and 1960s the emphasis in much neoclassical theory was on looking for more and more general results. This is, however, something that has since changed, for in the past decade neoclassical economists have been much more content to work with simpler models in order to get insights into specific problems. Examples include work on signalling (Spence, 1973), credit rationing (Stiglitz and Weiss, 1981), and insurance (Rothschild and Stiglitz, 1976): in all of these papers, the economists concerned work with specific simplifying assumptions, ruling out some problems so as to be able to analyse others. In part this change has arisen because work undertaken in the 1950s and 1960s made it clear that there were few very general results to be obtained. In part it has arisen because of increased Interest in problems involving uncertainty, imperfect competition, and limited information. When dealing with such problems, very general models are unmanageable.

The more important point to make, however, is that there is a strong case for *not* adopting a completely eclectic approach. The main characteristic of neoclassical economics is that problems are reduced to constrained optimization problems, models are specified very precisely so as to be amenable to mathematical treatment, and individuals’ preferences are taken as given (for a more thorough and more precise appraisal see Weintraub, 1985). It can be argued that, far from being an impediment, it is the adoption of such an approach which enables neoclassical economists to discover otherwise hidden structures underlying the problems with which they are dealing. As for the value of these revealed structures, it must be assessed in terms of whether or not they lead to successful predictions. This is the Lakatosian criterion for evaluating a research programme and its associated hard core. In other words, neoclassical economists may adopt the methods they do, not because they reject the 'horses for courses' approach, but because their methods appear to work (see Cover, 1987). Finally, it is puzzling that Post Keynesian tolerance towards different approaches does not extend to neoclassical economics: are there no problems for which neoclassical methods are appropriate? At times some Post Keynesians seem to have much more rigid presuppositions than do many neoclassical economists (see Backhouse, 1986).

Another appealing Post Keynesian demand is for models set in 'historical' time. There are several points that need to be made in response to this, the first of which is that many neoclassical models are 'historical' models according to Joan Robinson's definition of the term: they specify technology, behaviour and arbitrary initial conditions. Equilibria are benchmarks, and the economy may or may not approach equilibrium. It may be the case, for example, that neoclassical models do not allow adequately for the non-homogeneity of the capital stock, but this is a criticism of specific assumptions made, not of the underlying methodology.

This willingness of neoclassical economics to consider dynamic 'historical' models goes back a long way. Walras, for example, made it very clear that the prices determined by his system of simultaneous equations was of little use if he could not show that the prices it generated were the same as those reached by the market. His 'tatonnement', for all its inadequacies, was an attempt at providing a 'historical' model.

In contrast, Joan Robinson, despite her appeals for the construction of 'historical' models, preferred, much of the time, to analyse only models of golden age growth (her work does also contain less formal 'historical' models, such as in her writing on the inflation barrier). These models of golden age growth are clearly not 'historical' models, for the starting point cannot be arbitrarily chosen: if an economy is in equilibrium, she claimed, it must always have been in equilibrium.

In addition to neoclassical models of 'historical' time there are, of course, also important areas of neoclassical theory which contain merely 'logical', not 'historical' time, the outstanding example being the theory of intertemporal general equilibrium.

The defence of such theories has to be, as their proponents have consistently pointed out, that they provide benchmarks, and that they provide a means of clarifying conceptual issues that cannot be handled in more realistic models. Referring to the model of intertemporal general equilibrium Bliss writes,

> Of course, that model does not serve to represent reality and that is not its purpose. Where the simple model of an intertemporal economy with all the forward markets functioning can prove useful is as a point of departure, as a guide to which concepts are central and fundamental and which peripheral.
>
> (Bliss, 1975, p. 301)

Another reason why neoclassical economists sometimes use 'ahistorical' models is that they attach great importance to saying no more than can properly be deduced, and as a result they are sometimes forced to neglect problems of time and uncertainty in order to keep their models manageable. Referring to the way in which many neoclassical economists argue, Hahn has pointed out that

> this mode of theorising is closed and self-contained. Deductions are demonstrated from assumptions and what cannot be captured by the formal apparatus is not discussed. ... In fact the best of the technical economists display an engaging modesty in not attempting to say more than can be properly deduced.
>
> (Hahn, 1984, p. 961)

Such an approach may have costs in that many difficult problems (often to do with time and uncertainty) are not addressed, but it has the corresponding advantage of making it very clear just what can and what cannot be said.

There is a further methodological criticism of neoclassical economics implicit in the version of the history of economic thought outlined by Harcourt and Hamouda. According to this, Marshall 'emasculated' the theory of value and distribution through explaining long-period natural prices in terms of supply and demand, rather than in terms of 'dominant and persistent forces', such as the ability of the system to reproduce itself, and the ability of the economy to produce a surplus over and above the necessities of production (pp. 4–5). His system was then completed with Say's law (to remove the need for a theory of the level of aggregate output) and the quantity theory of money (which determined the overall price level). Keynesian, and Post Keynesian, economics is then portrayed as involving a reaction against this misleading conception of the economy.

Say's law and the quantity theory of money were, however, as much classical as neoclassical doctrines. Keynes was, as he stated explicitly, liberating himself from

Ricardo as much as from Marshall: in the *General Theory* it is Ricardo's influence, not Marshall's, of which he is most critical. If we look for direct links between Ricardo and Keynes, these are found not in the sphere of value theory, but in the quantity theory of money, of which Ricardo was so strong an exponent. Harcourt and Hamouda portray Keynes as, inadvertently, having, in his *Treatise on Money,* provided an alternative to the quantity theory. Keynes himself, however, saw his 'fundamental equations' simply as one version of the quantity theory (cf. 1971, I, p. 125). Like Wicksell (who saw the quantity theory as the only scientifically respectable theory of the value of money, but used income–flow analysis to explain how price levels changed), Keynes saw his analysis of income flows as explaining what happened out of equilibrium. Keynes accepted that the 'unique relationship' between the quantity of money and various price levels postulated by what he described as the 'old fashioned quantity equations' held 'in equilibrium' (ibid., p. 132). Even the *General Theory* contained (in chapter 21) what Keynes described as 'a generalised quantity theory of money' (1973, p. 285).

In contrast to this connection with Ricardo via the quantity theory, Keynes's views on value remained thoroughly Marshallian, being based on supply and demand. His 'fundamental equations', based on the distinction between normal earnings and windfall profits, two Marshallian concepts, lead easily into Kaldorian distribution theory. Their connection with neo-Ricardian views of pricing is much more tenuous.

The fundamental objection to the argument about Marshall emasculating the classical theory of value is, however, that prices may be indices of scarcity, but scarcity reflects the underlying conditions of production. Post Keynesians have not shown how these two views are incompatible. The logical equivalence of Sraffa prices with the prices produced by a suitably specified general equilibrium system (see Hahn, 1982) would seem to be evidence against this.

At the risk of digressing slightly, it is worth noting that this interpretation of the history of economic thought rather misleadingly starts with the English classical economists. This starting point makes it much easier to portray neoclassical economics as a diversion, with PKE involving a return to the older tradition. If a longer perspective is adopted, encompassing the extensive eighteenth-century work on value, it is English classical political economy, and above all its Ricardian strand, that appears as the detour.

3 THE VALUE OF POST KEYNESIAN ECONOMICS

Solow's assessment of PKE is probably representative of the views held by many 'orthodox' economists:

> I don't see an intellectual connection between a Hyman Minsky ... and someone like Alfred Eichner ... except that they are all against the same thing, namely the mainstream, whatever that is. ... It [PKE] seems to be mostly a community which knows what it is against, but doesn't offer anything very systematic that could be described as a positive theory.
>
> (Solow, quoted in Klamer, 1984, pp. 137–8).

Harcourt and Hamouda come very close to conceding that Solow's appraisal is justified, but rather than seeing such diversity as a problem, they see it as a virtue. They argue that there are 'coherent frameworks and approaches' within PKE, but they regard the task of looking for a coherent whole amongst all these different strands as a misplaced exercise (p. 34). To do so would be simply to replace one 'box of tricks' (that of neoclassical economics) with another.

This thesis, that we should not be looking for a single, logically consistent theory, is one that has been argued even more forcefully by Dow (1985). Dow has produced many reasons why complete logical consistency is, in practice, never possible, but what neither she nor any other Post Keynesian has demonstrated is that it should be abandoned as an ideal. This is crucial when we evaluate PKE, for most of the methodological frameworks in terms of which we might evaluate PKE, assume that logical consistency is desirable. Suppose, for example, we were to evaluate PKE as a Lakatosian research programme. We might well conclude that it was not progressive in the way that neoclassical economics is (see e.g. Weintraub, 1982, pp. 302–3; Backhouse, 1985, pp. 410–12). Because of the methodology underlying PKE, theoretical innovations within PKE frequently appear relatively 'ad hoc', rendering it much more liable to classification as 'degenerating' in Lakatos's sense. Given what Dow, Harcourt and Hamouda see as the Post Keynesian methodological position, however, progressivity as defined by Lakatos is simply not an appropriate appraisal criterion.

It is tempting to infer from this methodological position that PKE and neoclassical economics should be evaluated as two, largely incommensurable, Kuhnian paradigms (cf. Dow, 1981). The implication of these Post Keynesian methodological views, however, would rather seem to be that economics ought to remain in a 'pre-paradigm' stage, with no generally accepted theoretical framework. Whilst this may be something we have to live with, it is far from clear that such a state of affairs is desirable.

The obvious way to view PKE is as a critical movement, acting as a 'conscience' of neoclassical economics. Post Keynesian economists have persistently asked awkward questions, sometimes effectively, sometimes less effectively. The theory of capital is the classic example, other examples including the implications of 'real' uncertainty (as emphasized by Shackle) and the lack of generality implicit in any theory which takes consumers' preferences as endogenous (see, e.g. Bharadwaj, 1978, pp. 60–2). The significant aspect of the capital theory controversies, for the present argument, is that it concerned issues which *could* be discussed within the framework of neoclassical economics. It is thus tempting to argue, paraphrasing Blaug's (1983) appraisal of Radical Economics, that the most fruitful approach is to tackle problems raised by PKE using the methods of neoclassical economics.

The most effective aspect, therefore, of the Post Keynesian challenge to neoclassical economics is neither the challenge to neoclassical methodology, nor the claim to be presenting an alternative framework for economic analysis, but the questions raised about specific areas of economics. It is thus quite reasonable for neoclassical economists to conclude from Harcourt and Hamouda's survey that whilst there may be problems with neoclassical economics, and whilst Post Keynesians may have many interesting and important things to say, they have not yet managed either to provide a suitable alternative to the neoclassical research programme, or to show that the methodology underlying neoclassical economics (say as represented by Blaug, 1980) is misconceived.

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Chapter 11

Should we ignore methodology?

(*Royal Economic Society Newsletter* 78, July 1992, pp. 4–5.)

When this short note appeared in the Royal Economic Society Newsletter, *the reactions I received made me wonder (probably incorrectly!) whether it was the most widely read and best-received piece I had written. This is the reason why it is included, even though its main argument, that economists have had a profound effect on the way the subject has developed, has been discussed at much greater length in* Truth and Progress in Economic Knowledge *(1997).*

In the April 1992 issue of the *RES Newsletter,* on the occasion of his retirement from Cambridge, Frank Hahn offered some 'Reflections' which took the form of advice to the young. As advice to young economists seeking to publish in prestigious journals and to further their careers, it has much to recommend it. From a broader perspective, however, Hahn's advice contains some remarks that are potentially dangerous and which should be challenged, namely his warnings to 'avoid discussion of "mathematics in economics" like the plague', and to 'give no thought at all to "methodology"'.

Hahn is entirely correct to argue that there are many problems in economic theory for which mathematics is essential, and that to argue for its abandonment is futile. However, this does not mean that there are no interesting or important issues to be discussed. Even if we accept that mathematics is here to stay, it is still essential to debate issues such as the use of abstraction and formalism, and the importance attached to rigorous proof. *Ceteris paribus,* greater rigour is clearly preferable to less. The problem, however, is that the *ceteris paribus* condition is usually violated.[1] In order to provide rigorous proofs it is frequently necessary to abstract from issues that may be important, with the result that the course of research becomes dictated by requirements imposed by the mathematics, not by the importance of the problems involved. As McCloskey has argued, many economists have, despite their professed regard for the natural sciences, imbibed the values and criteria of mathematics, not physics.[2] It is important to emphasize that in arguing this, I am not arguing that formalism is bad, or that mathematical rigour is undesirable – simply that there are potentially costs as well as benefits, and that these should be openly discussed, not swept under the carpet.

The most worrying, because potentially most dangerous, of Hahn's claims is the claim that economists should 'give no thought at all to "methodology"', such discussions being 'similar to discussions of the use of mathematics, only worse'. His view appears not to be that methodology should not be studied at all, but that it should not be studied by *economists*. He gives three reasons for this: economists are at best amateurs and cannot do the subject justice;[3] methodological discussions make little difference; and when they do the results are 'by no means unambiguously good'. Of these, the first is clearly a red herring. If economists need to discuss methodological issues, but have insufficient training in philosophy, they should learn the philosophy, just as they should learn mathematics when they find their mathematical training inadequate. As for the third, can it be said of any area in economic theory that the results have been unambiguously good? Surely all areas of enquiry are open to misuse.

This leaves the charge that methodological discussions make little difference. The best way to criticize this is to consider some examples. (1) Hahn claims that 'Game theoretic approaches have turned old Industrial Organisation writings into stone age theory'. This can be contrasted with Franklin Fisher's much more sceptical assessment of these developments, an assessment which results from asking whether game theory has led to any testable hypotheses that were not known to the architects of what Hahn calls 'stone age theory'.[4] My point here is not to argue that Fisher is right and Hahn wrong. It is simply to point out that it does make a difference whether we accept Hahn's perspective or Fisher's, and that these different

[1] As recognised by Debreu (1991, p. 5).
[2] McCloskey (1991).
[3] Cf. Hahn (1984, p. 135).
[4] Fisher (1989, 1991).

perspectives can be discussed properly only by addressing methodological issues. (2) Hahn argues that it was 'obvious from the start that the New Classical Macroeconomics was doomed'. Such a judgement must rest on some methodological presuppositions. Had such presuppositions been made explicit and debated openly, the new classical macroeconomics might perhaps have had less impact. (3) Although Hahn defends strongly the use of mathematics, his attitude towards axiomatic theory is very different from that of Gerard Debreu.[5] It is of great consequence whether economists follow the advice of Hahn or Debreu, and it is impossible to decide between them without making, either implicitly or explicitly, methodological judgements.

The most powerful answer to Hahn, however, is to point out that even economists who, like him, deprecate methodology, are continually making methodological statements. Methodology is unavoidable, and should be made explicit so that it can become the subject of rational discussion. Indeed, if Hahn intends his advice to apply to all economists (and though it is offered to the young there is no indication that it should not apply equally to the less-young) he goes against his own advice, for he makes several statements which can only be described as methodological. In particular, he praises 'understanding' as something distinct from prediction, a very controversial methodological statement. In advising young economists to give no thought to methodology, he is advising them to ignore this argument!

I have two comments to make by way of conclusion. The first is that it may be, as Hahn argues, that an evolutionary process will select those methodologies fitted for survival. If, however, we were to replace the biological metaphor of natural selection with the economic metaphor of competition, we would immediately be faced with questions about possible market failure. What reason have we to think that market failure is not a problem? The second is that economists will, like it or not, discuss methodological questions. Like the history of economic thought, methodological discussion is irrepressible.[6] If Hahn's remarks encourage those whom he would regard as good theorists to disparage methodology, the field will be left to others – including not only philosophers, historians of economic thought, and dissenters from the mainstream, but also econometricians and applied economists.[7] An important input into methodological discussion will be missing, and the discipline will be poorer as a result.

[5] Cf. Hahn (1991) with Debreu (1991).
[6] Cf. Blaug (1991).
[7] E.g. Hendry, Leamer and Poirier (1990); Summers (1991).

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Chapter 12

Economic laws and economic history*

(*Journal of the History of the Behavioural Sciences,* 1992)

This chapter focuses on two methodological issues raised in one of Kindleberger's many books. The first is the concept of an economic law, a term which has largely passed out of use in economics. Economists, by and large, avoid talking of laws, preferring to talk in terms of theorems, results, predictions and stylized facts. In part this may be due to a recognition that economic laws comparable to the inverse-square law of Newtonian physics simply do not exist. But it might also be due to economists' having adopted mathematical, rather than natural-science, standards by which to judge their work. The other is the value of an eclectic approach to economic enquiry, and of more informal methods than those now fashionable in most of the discipline.

This book contains the text of the Raffaele Mattioli lectures, delivered at the Bocconi University in Milan, in May 1980. Of the book's 191 pages, however, only 92 pages are taken up by the lectures themselves, the rest of the book including 23 pages of comments, by leading Italian economists, on the lectures, the 7 pages of Kindleberger's reply, a 9 page bibliographical note on Charles Kindleberger, and a bibliography of his 205 articles and books. The whole is beautifully produced, but one wonders why it took nine years to be published. There seems nothing in the nature of the book to justify such a long delay.

Kindleberger's main point is a methodological one: that economists should make use of a variety of tools, no single one being suitable for all problems. He argues this by considering, in four successive chapters, four different 'economic laws': Engel's law; the iron law of wages; Gresham's law; and the law of one price. Each of

* A review of Kindleberger (1989).

these, he argues, can be used to explain what happened in a variety of historical episodes, but no single law can explain everything.

Engel's Law is the 'law', derived from budget studies, that as per capita income grows, consumption of food grows proportionately less: in other words, that the share of income spent on food falls as income rises. It can, of course, be generalized to apply to goods other than food, but Kindleberger goes further by arguing that Engel's law is related to the thesis that new products spread slowly at first whilst they are still new, then rapidly as they become mass-consumed, and finally more slowly again as the market becomes saturated – the 'S' or 'Gompertz' curve. He uses this to explain historical phenomena ranging from the late-nineteenth century climacteric in Britain's growth performance to Rostow's concept of a 'take-off' into self-sustained growth.

The second lecture is entitled 'The iron law of wages', but discussion centres on the 'Lewis model', proposed by Nobel laureate Arthur Lewis in the mid-1950s. In this model there is assumed to be surplus labour in agriculture. The result is that the industrial sector can grow by attracting labour from agriculture without having to pay increased wage rates, and because it is surplus labour that is lost, agricultural productivity rises. Competition keeps wage rates low. This situation ends, however, when the pool of surplus labour in agriculture has been exhausted. Kindleberger argues that this 'law' explains many aspects of European industrialization. For example, he argues that the availability of cheap labour explains why Belgium industrialized more rapidly than the Netherlands. In an interesting twist of the argument, he argues that if it is applied to land instead of labour, the model is relevant to US experience during the nineteenth century.

'Gresham's Law' is the law that good money drives out bad: that if good and bad currencies are in circulation simultaneously, people will choose to hoard the good, and pass on the bad. This is used, in the third lecture, as the basis for a wide-ranging examination of monetary history, in which it is applied to present-day financial systems as well as the gold- and silver-based systems that it was originally used to analyse. Finally, in the fourth lecture, Kindleberger turns to the all-important 'law of one price'. He uses this as the basis for claiming that 'a most important tool for observing the course of economic history is to examine the changing – and for the most part growing – size of the market for goods, services, money and factors of production' (p. 67). He examines the importance of transport costs and integration at a variety of levels, finishing up with the issue, so topical in Europe at the moment, of optimum currency areas.

In my opinion, there are two main problems with Kindleberger's overall thesis.

The first of these concerns his use of the term 'laws', a term that, with the possible exception of certain parts of economic history, is rarely used in serious work in economics today (i.e. outside introductory textbooks). There are several reasons for questioning Kindleberger's use of this term. (1) It implies something far more concrete and reliable than is the case with any of the examples Kindleberger discusses. Economics does not have laws comparable with those found in the natural sciences. This is one of the reasons why present-day economists prefer to argue not in terms of 'laws', but in terms of assumptions, models and theorems. (2) Kindleberger's use of the term 'law' enables him to blur the distinctions between these three categories. For example, the Lewis model is a 'model', based on precisely specified assumptions, in a sense in which Engel's law is not. (3) Kindleberger has, in order to cover the four examples he wishes to discuss, to use the term very elastically indeed. Even if we regard Engel's law or Gresham's law as an economic 'law', it is stretching a point to regard the Lewis model as falling into the same category. Methodologically, all three are different. Although theoretical arguments can be introduced to explain it, Engel's law is primarily a generalization from experience. The 'iron law of wages' is a theorem derived from a relatively well-defined set of assumptions (satisfied in the Lewis model only so long as there is excess supply of labour). Gresham's law and the law of one price follow fairly directly from the assumption that people adopt profit-maximizing strategies. Many of the instances cited by Kindleberger, such as factor-price equalization, are certainly in the class of theorems derived from certain theories. They are neither assumptions nor generalizations from experience.

This rather loose use of the term 'law' is connected with my second criticism of his thesis. This is that whether or not we find that there are different theories for different circumstances, or a single all-embracing theory, depends on the level at which we look. This is the point raised by Pasinetti when he argues that 'plurality of tools is all right, provided that this plurality is within a well defined theoretical framework' (pp. 96–7). On this point Kindleberger's position is that of a historian rather than an economic theorist. Whereas the theorist would, typically, seek to explain diverse phenomena in terms of a set of more basic postulates, the historians generally prefer to work with a range of less formal models. Thus where Kindleberger sees four different laws, most economists would see all four as deducible, under appropriate circumstances, from the assumption of maximizing behaviour. The law of one price, so closely linked to the hypothesis of maximizing behaviour is, to quote from Beccatini's comment,

> like a Trojan horse within the group [of four laws], which constitutes a risk for Professor Kindleberger's adroit eclecticism. The view of the world that it embodies and that it mediates in the reading of historical events is heuristically so powerful that it forces every other tool of interpretation with which it is combined into a subordinate role.
>
> (p. 101)[1]

Notwithstanding these criticisms of Kindleberger's methodological conclusions, his lectures beautifully illustrate the insights that can be obtained from approaching economic history in the way he advocates. Many of the conclusions he draws from his analysis of history are very persuasive, despite the fact that the models on which they are based are not formulated with the degree of rigour that has become almost mandatory for many economists. The conventional approach to economic theory has produced numerous insights, finding common structures amongst what would otherwise have been considered very different phenomena. It has, however, important costs, in that the development of economic theory has to a great extent been dictated by what the techniques available to economists enable them to tackle – see, for example, Ingrao and Israel (1990). The 'looser' theoretical approach exemplified in Kindleberger's lectures shows that less 'rigorous' methods have much to offer. What is required, surely, is a 'middle way' between the discipline imposed by neoclassical theorizing and the theoretical eclecticism that is characteristic of much economic history. In so far as economics is today dominated by the former approach, Professor Kindleberger's lectures, reminding us of the value of a more eclectic approach, are to be welcomed.

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[1] The past tense used in the book's reported speech has been changed to the present tense.

Chapter 13

Is there life in contemporary academic economics?

(*Journal of Economic Methodology* 2(1), 1995, pp. 135–44.)

This chapter is a review of two books which could hardly be more different in their conclusions. Apart from being a Keynesian (nowadays an unfashionable label to use) Krugman's methodology is orthodox. He advocates formal modelling based on individuals' optimizing behaviour. His explanation of the recent failures of US economic policy-making lies in the success of policy entrepreneurs, who have peddled what politicians want to hear, not ideas for which a solid academic case can be made. Academic economists, he contends, need to become much better at explaining what they do. In contrast, Ormerod seeks to be a root-and-branch critic of contemporary economic theory, considering the postulates of neoclassical economics a travesty of reality. Economics is not suffering from an image problem, but from a fundamental malaise that renders it incapable to explaining the world.

This chapter, and the two books it reviews, raise a variety of issues. Krugman's book points out the importance of understanding the route by which economic ideas actually feed into the policy-making process. The best economic ideas can be harmful if they become seriously distorted on their way between academic economists and policy-makers. The sociology of the profession, and the wider community within which economists are located, does matter. Ormerod's book is easy to criticize very severely, but it makes several valid criticisms of contemporary economics. Comparison of the two books reinforces the points, made in Chapters 10 and 12, that there is a tension between two things, both of which would appear to be necessary for progress in economics. On the one hand, there must be a supply of new ideas, otherwise the subject stagnates. On the other hand, in order for ideas to be developed it is necessary to have a framework within which to work, and for all its many faults, that of mainstream economics has much in its favour.

1 INTRODUCTION

Where is economics going? Has it been successful? If it has, where do its achievements lie? If it has not, what should be done about it? Such questions are clearly methodological, and have been extensively discussed in the literature on economic methodology. They are, however, also addressed in more popular writing, the subject of discussion outside the sphere of academic economics. Books aimed at a non-academic audience may be philosophically naive, lack rigour and offer an oversimplified picture of economics, but sometimes they raise issues and make points that are overlooked in academic writing. The two books under review fall into this category. They both offer popular accounts of developments in economics, criticizing the way in which economic ideas have been used by policy-makers and offering advice about how the situation could be improved. What makes it revealing to review them together is that though they agree that economic ideas have often had disastrous effects on economic policy, their authors adopt radically different attitudes towards modern economic theory. Ormerod blames the failures of economics on a commitment to formal, abstract theory, whilst Krugman sees the problem as being that policy-makers have failed to learn from economic theory, preferring the unsound advice offered by what he terms 'policy entrepreneurs'. Where Ormerod claims that the core of academic economics is so fundamentally flawed that it should be abandoned, Krugman contends that economists have much to offer, but their message has failed to get through.

2 *THE DEATH OF ECONOMICS*

Paul Ormerod's *The Death of Economics* attracted much attention when it was first published in March 1994. It Britain it was reviewed in many national newspapers, including the *Guardian,* the *Financial Times,* the *Observer* and the *Times Higher Education Supplement.* Its thesis is that economics is so fundamentally flawed that it should be laid to rest and replaced with something very different. Unlike many such books, however, this one offers an alternative, even setting out the relevant equations. One of the reasons why the book attracted so much attention is that its author, Paul Ormerod, was for many years one of Britain's leading forecasters, responsible for the macroeconomic model run by the National Institute for Economic and Social Research. He is thus well known to many financial journalists and his criticisms of economics are seen as coming from an insider who knows what it is about. Indeed the dust jacket proffers a quotation from *The Times* in which Ormerod is cited as the only economist known to the reviewer who has shown any remorse for the suffering caused by the policies implemented on the basis of economists' bogus models and inaccurate forecasts.

The book is organized in two parts. Part I describes and accounts for the present state of economics. The basic argument is that economics simply does not work: policy advice misfires – witness the failure of shock therapy in Russia, and the disarray of the European Monetary System (Ormerod, 1994, p. 3); and economists are unable to produce reliable forecasts – in 1992–3 alone, economists failed to predict the depth of the Japanese recession, the strength of the US recovery, German recession or the problems of the European Monetary System (EMS) (ibid., p. 105). These failures, Ormerod contends, are no accident but are the result of economists having become committed to an irrelevant, abstract model that bears no relation to reality.

Economics was led astray, according to Ormerod, in the late nineteenth century when economists, following scientists, began to see the world as a complex machine. This analogy implied that the world was in harmony and equilibrium, with no room for shocks and catastrophes. The foundations were laid by Jevons and Walras, with subsequent generations applying more sophisticated mathematics to the methodological framework they set up (ibid., pp. 39–41). This model, centred on perfect competition, was inappropriate almost from the start, for this was the time that big business was transforming the face of the US economy: 'Almost from its inception, the theoretical postulates of marginal economics concerning the nature of companies have been a travesty of reality' (ibid., pp. 55–6). The success of marginal economics stemmed from three factors: it supported the ideology of the free market; the emphasis on harmony and equilibrium was in keeping with the scientific spirit of the times; and it represented 'a formidable intellectual achievement' (ibid., p. 46).

As Ormerod points out, economists have long been aware of the immense weight of evidence against the theory of competitive equilibrium. In addition to the empirical evidence against its assumptions, there are numerous internal problems with the theory.

- Prices cannot convey all the information needed to prevent exhaustion of exhaustible resources (Dasgupta and Heal).
- In an uncertain world, competitive equilibria are not in general Pareto efficient (Newbery and Stiglitz).
- Futures markets do not exist, which means that equilibrium may not exist (Arrow).
- Negligible violations of the assumption that agents have no market power can lead to outcomes far from the competitive equilibrium (Silvestre).

- The theory of the second best (Lancaster and Lipsey).
- The problem confronting the Walrasian auctioneer may be impossible to solve (Day).
- Agents are unable to compute optimal strategies, because to do so would require infinite computing power (Radner).

Why, then, is there such great confidence within economics? How can a culture that 'positively sustains extols esoteric irrelevance' (Ormerod, 1994, p. 20) survive? Ormerod's answer is that economists are behaving like many other groups do in such situations.

> Sociologists and psychologists have documented many case studies concerning the reactions of groups when views which they hold about the world are shown to be false. In such situations, far from recognising the problem, a common reaction of individuals is to intensify the fervour of their belief.
>
> (ibid., pp. 4–5)

Economists are able to pursue successful careers and as long as they can do so, there is no pressure to change the situation.

Part II of the book outlines Ormerod's ideas on how economics should be reconstructed. There is much discussion of inflation, unemployment and growth. Using simple graphical methods he concludes that the significant relationship is not between inflation and unemployment, but between *changes* in these two variables. He then proceeds to analyse the dynamics of unemployment in terms of attractor points in a diagram relating unemployment to the previous year's unemployment. To provide a theoretical explanation of such dynamics he turns to the Lotka–Volterra system put forward by Goodwin (1967). This is a non-linear system in which employment interacts with the share of profits in national income: high profits promote growth, but at the same time high employment reduces the profit share.

3 *PEDDLING PROSPERITY*

For a generation after World War II, the US had a 'magic economy' – it was an age of affluence and optimism – but in 1973 the magic went away. Krugman's *Peddling Prosperity* is about the search for ways to put this right.

> So what do you do when the magic isn't working? You look for a new set of magicians.
>
> This is a book about that search. Or, to be more exact, it is a book about the

> interplay between economists and politicians, about how the politicians try to find economists with ideas that they can package, and how economists both develop ideas and try to translate ideas into political influence.
>
> (Krugman, 1994a, p. 5)

Like Ormerod, Krugman accepts both that much academic work is of little value, and that economic ideas have led to disastrous economic policies being pursued. Thus we find him making remarks such as the following.

> The most obvious thing about professors is, of course, that they are professors – a species that, like penguins or ostriches, is inherently faintly ridiculous. In America's academic system, professors of economics get tenure and build the reputations that give them other academic perks by publishing, and so they publish immense amounts – thousands of papers each year, in scores of obscure journals. Most of those papers aren't worth reading, and many of them are pretty much impossible to read in any case, because they are loaded with dense mathematics and denser jargon. The most popular economic theories among the professors tend to be those that best allow for ingenious elaboration without fundamental innovation – ways to show that you are smart by putting old wine into new bottles, usually with fancier mathematical labels.
>
> (ibid., p. 8)

In the same vein, he remarks that, as with deconstructionist literary theory, 'the technicality and difficulty of Lucas's theory ... was, in the world of academic economics, an asset rather than a liability' (ibid., p. 52). But Krugman goes on to argue that all is not academic games, and that similar remarks could be made about disciplines that exhibit great progress, such as physics and medicine.

Similarly, where Ormerod links policy failures with failures of economic orthodoxy, Krugman makes no such links. Rather, for him, policy failures stem from failure to take orthodox economics sufficiently seriously. Central to his book is the distinction between two types of economist that he terms 'professors' and 'policy entrepreneurs'. The crucial feature of 'professors' is that even when writing for a lay audience, they are continually looking over their shoulder to see what their colleagues think of their ideas. This will inhibit them from saying things that sound good but which are known to be wrong (ibid., p. 11). In contrast, policy entrepreneurs are not subject to this constraint, which makes them free to tell the public and politicians what they want to hear.

> And their rapport with their audience isn't inhibited by an underlying awareness of facts and concepts that do not appear in what they say. What the public

> sees in them is what it get; and what it gets generally plays to its preconceptions.
> (ibid., p. 12)

Policy entrepreneurs may have academic positions (his examples include John Kenneth Galbraith, Lester Thurow), or they may be journalists (Robert Bartley of the *Wall Street Journal,* and Irving Kristol of *The Public Interest*). They cover both the Supply-siders, who dominated Reagan's administration, and the strategic traders, important under Clinton. Both groups, Krugman points out, are made up of outsiders to academic economics (even though a few hold academic posts there is no supplysider at any major department). Some can be described as cranks, isolated from the usual channels of discussion: they do not send their work to recognized journals; they speak to organizations they have founded; and they publish in journals they themselves edit.

Krugman's argument is that up to the 1960s, politicians turned to serious economists (professors) for advice on economic policy, but that after 1973 they turned to policy entrepreneurs.

> It was only in the 1970s, faced with a desperate need to offer the public magical solutions, that politicians began to take policy entrepreneurs seriously
> (ibid., p. 15)

The result was a series of policy failures, notably the package of measures known as Reaganomics. They arose because policy-makers failed to listen to orthodox economists. The real answer to the question of why the magic went out of the US economy in 1973 is that *'we don't know'* (ibid., p. 5; emphasis in original), but this did not provide politicians with vote-winning policies. For these politicians turned to the policy entrepreneurs.

Krugman's argument is based on the claim that orthodox economics has been successful. Economists have, contrary to what Ormerod claims, learned a lot. On the negative side they know why the policies of the supply-siders and the strategic-traders cannot work. Three types of evidence are adduced: logical errors in their arguments; rough calculations to show that the causal mechanisms invoked are orders of magnitude too small to explain what they are claimed to explain; and historical experience. On the positive side are a range of facts, notably concerning the effects of macroeconomic policies, particular emphasis being placed on the power of monetary policy to affect employment. Keynesianism, Krugman argues, is basically right (ibid., p. 215).

4 HOW SUCCESSFUL IS ACADEMIC ECONOMICS?

Ormerod and Krugman reach diametrically opposed conclusions about the success of academic economics. There are several reasons for this.

(1) As one would expect from an economist with his background, Ormerod places great emphasis on the failure of economists to provide accurate forecasts. In contrast, Krugman is concerned with broader, generic predictions about the consequences of certain actions. Thus though economists may be unable to predict the percentage growth rate of GDP a year ahead, they may be able successfully to predict that monetary contractions will produce recessions, or that the provision of deposit insurance will lead to banks' acquiring excessively risky portfolios of assets. Krugman's argument is that economists have been successful in making such predictions.

(2) Krugman focuses on the best available theory, which is frequently overlooked by Ormerod (cf. Dixon, 1994). A good example is the savings and loan (S&L) crisis in the US. Ormerod (1994, pp. 69–70) uses this, alongside the examples of *le franc fort* and the sudden removal of capital controls in Sweden, to support the point that free market economics fails to promote efficiency. He concludes that 'the real world is far more complex than is allowed for in the economic model of Rational Man and competitive equilibrium' (ibid., p. 70). Krugman, on the other hand, sees the S&L scandal as vindicating orthodox economics: 'Banking experts have long been aware that the American system of banks with deposit insurance poses problems of moral hazard' (Krugman, 1994a, pp. 162–3).

(3) Krugman, like his hero, Keynes, appears to adopt a Marshallian attitude towards economic theory. He makes no claim that economic theory can provide a single theory of everything, but regards theories to be used as tools to analyse specific problems. His analysis (partly, no doubt, because he is writing for a popular audience) remains firmly in touch with the real world, never ascending to the abstractions of general equilibrium theory. Theories about limited information, incomplete contracts, uncertainty and so on, that Ormerod sees as destroying the orthodox story about the virtues of free markets, are seen by Krugman as providing insights into real-world problems.

(4) Ormerod makes numerous explicit methodological statements. The method of science, he contents, involves starting with observation and only then going on to analysis (Ormerod, 1994, p. 12). He criticizes economists for failing to allow empirical reality to affect their theories (ibid., p. 21). Thus a major component of his critique is that the assumptions of orthodox theory are unrealistic (ibid., pp. 17, 55–6). Yet he also makes the following claim.

> The more situations in which the model can be applied successfully, the greater is the confidence in the theory, and the greater the respect in which it is held. Newton's theory of gravity is seen as so outstandingly brilliant because it can explain such an enormously wide range of events.
>
> (ibid., p. 21)

This is the criterion by which Krugman, implicitly, judges orthodox theory. The theory, when properly applied, and with appropriate qualifications, works. He places less emphasis on realism of assumptions than does Ormerod. Arguably this follows from his Marshallian approach to theory as providing insights only into certain aspects of reality: he is not looking for a universal, comprehensive theory, which implies that some assumptions will always be unrealistic.

On all four of these issues it is arguable that Krugman is right. Economic theory has made important contributions to our understanding of real-world problems. To this extent economics has not failed, but has been very successful.

And yet, doubts remain. Ormerod's book is over-sold (it is not about the death of economics, but about changing the content of economic theory); it is littered with mistakes that, even if not particularly important, are very annoying and indicative of great carelessness (Richard Kahn's name is always spelled 'Khan', Paul Romer's 'Roemer', and of the two equations that make up the Goodwin model, one is wrong); the book takes inadequate account of very recent developments in economic theory; and virtually all its criticisms of economics are well-known (Dixon, 1994). In addition, the empirical work on which he attempts to base his alternative to orthodox economics is naive and fails to meet the standards that are expected nowadays (Hall, 1994). The book will, therefore, be taken seriously by very few academic economists. Thus amongst reviewers of the book, the most favourable are those by journalists (Brittan, 1994; Hutton, 1994; Keegan, 1994) and the least favourable by academics (Dixon, 1994; Hall, 1994). Such a reaction would, however, be a mistake. For all its failings, the book raises some fundamental issues that Krugman's work (very understandably, given its goal) passes by.

(1) Many major economic problems remain, as Krugman admits, unsolved. In particular, we simply do not know why growth rates differ – why, for example, the 'magic' went out of the US economy after 1973. It is quite conceivable that orthodox economic theory, though successful in tackling some other problems, will never explain this, and that alternative paradigms should be explored. To focus too strongly on the successes of orthodox theory and to overlook its clear and important failures is inappropriate.

(2) Ormerod's claim that economists need to turn away from mechanistic analogies and embrace the notion of the economy as an organism may be important. As it stands the idea is merely suggestive, with much further research being needed to establish whether it will lead anywhere. At present we simply cannot tell. Inadequacies in Ormerod's attempt to provide an alternative to mechanistic theories do not mean that such theories will never be found.

(3) Orthodox economics does suffer from a thinness of vision as regards the importance of the social fabric and the possible importance of sociological, psychological and political issues for understanding economic phenomena (Ormerod, 1994, pp. 45–6). Economics has derived its power to tackle certain problems by systematically neglecting such issues. It seems highly plausible, however, that there are other problems (for example, problems of transition from planned to market economies) for which this is an inappropriate strategy.

(4) The influence of free market policies based on simple competitive models is perhaps not entirely the fault of policy entrepreneurs. 'Professors' should arguably share some of the responsibility. Arguments about market failure that provide a rationale for Keynesian and interventionist policies are well-known to specialists in the relevant fields, but much of our teaching focuses on simpler, often competitive, models where the presumption appears to be that *laissez faire* is an appropriate policy.

(5) Krugman perhaps exaggerates the extent to which economists actually anticipated some of the disasters he discusses. There were theories that could have been used to predict the S&L débâcle and the collapse of the EMS after German unification, but is it fair to portray the 'professors' as on the side of the angels in either case? There have been instances where economists got it right (Friedman and stagflation is the clearest example) but the frequency of such cases should not be exaggerated. In many cases economists are explaining things after the event.

(6) Ormerod's main target is highly abstract, mathematical theorizing. Whilst Krugman is undoubtedly more sympathetic towards such work than is Ormerod, it is far from clear how far the insights on which Krugman draws when he shows the power of academic economics to explain important economic phenomena, are dependent on such theories. Ideas about the significance of moral hazard, adverse selection, path dependence and so on may have been developed by economists committed to formal, rigorous analysis. But if the values of the profession had not been such that formal, mathematical analysis based on the assumption of individuals' optimizing behaviour was a precondition for an idea to be taken seriously by the

profession, such insights might have been obtained via different routes. Indeed, had the values of the profession been different, such ideas (many of which were known to economists such as Adam Smith, John Stuart Mill and Alfred Marshall) might not have remained lost for so long.

5 EPILOGUE

> A theoretical science unaware that those of its constructs considered relevant and momentous are destined eventually to be framed in concepts and words that have a grip on the educated community and become part and parcel of the general world picture ... in the long run is bound to atrophy and ossify, however virulently esoteric chat may continue within its joyfully isolated groups of experts.
>
> (Erwin Schrödinger, quoted by Ormerod, 1994, p. 67)

Ormerod uses this quotation to castigate economists for becoming too engrossed in 'esoteric chat'. However, where Ormerod believes that the process of atrophy and ossification has already happened, Krugman is concerned to make sure that it does not. His book explains and analyses the process whereby economics ideas have affected the educated community in order to influence that process for the better. In the words used at the head of an article in which Krugman summarized the main points of his book, 'Academic economists suffer from an image problem that could be overcome ... if only they explained better what they do' (Krugman, 1994b). *Peddling Prosperity* may be a popular book, even a 'more or less partisan tract' (Krugman, 1994a, p. xiv) but, like Ormerod's *The Death of Economics,* it raises important methodological issues.

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Chapter 14

Vision and progress in economic thought

Schumpeter after Kuhn

(*Joseph A. Schumpeter: Historian of Economics,* edited by L. Moss. London: Routledge, 1996, pp. 21–32.)

This chapter considers the methodology proposed by one of the most important economists of the twentieth century in his, frustratingly incomplete, History of Economic Analysis. *It is argued that Schumpeter's methodology has much in common with that proposed by Kuhn in his* Structure of Scientific Revolutions, *but that it is different in crucial respects. Whilst it can be argued that these differences reflect what would now be considered a naive methodology, it can equally be argued that they reflect a close awareness of modern economics. For example, he emphasizes analytical rigour rather than empirical testing, and he stresses the role of the synthsizer rather than the developer of new ideas as the basis for 'classical situations. Schumpeter's methodology could perhaps be seen as an early example of 'empirical' philosophy of science.*

1 SCHUMPETER'S PERSPECTIVE

Schumpeter's *History of Economic Analysis* (1954) is written from a distinctive perspective, outlined in Book I. There is great emphasis on economics being a science, where science involves going beyond everyday explanations of economic phenomena. Many of the phrases Schumpeter uses to describe science reflect the influence of logical positivism, then developing into the dominant approach to the philosophy of science. Thus Schumpeter writes that the rules of '"modern" or "empirical" or "positive" science ... reduce the facts we are invited to accept on *scientific grounds* to the narrower category of "facts verifiable by observation or experiment"; and they reduce the range of admissible methods to "logical inference from verifiable facts"' (p. 8; emphasis in original). Such philosophy of science now seems somewhat dated. So too does Schumpeter's historiography.

> Economic analysis has not been shaped at any time by the philosophical opinions that economists happened to have... even those economists who held very definite philosophical views, such as Locke, Hume, Quesnay, and above all Marx, were *as a matter of fact* not influenced by them when doing their work of analysis.
>
> (pp. 31–2; emphasis in original)

It thus becomes possible for him to focus on the filiation of ideas. This perspective is given added significance when combined with Schumpeter's view that interdependence, seen in Walrasian terms, is the central economic problem.

> [T]his all-pervading influence interdependence is the fundamental fact, the analysis of which is the chief source of the additions that the specifically scientific attitude has to make to the practical man's knowledge of economic phenomena; and that the most fundamental of all specifically scientific questions is the question whether analysis of that interdependence will yield relations sufficient to determine ... all the prices and quantities of products and productive services that constitute the economic 'system'. ... The discovery [of this fundamental problem] was not fully made until Walras, whose system of equations, defining (static) equilibrium in a system of interdependent quantities, is the Magna Carta of economic theory ... The history of economic analysis or, at any rate, of its 'pure' kernel, from Child to Walras might be written in terms of this conception's gradual emergence into the light of consciousness.
>
> (p. 242)

Today such a perspective seems naive, both as philosophy of science and as historiography: we have learned much since the mid-1950s.

The main reason why Schumpeter's perspective seems dated today is the influence of Kuhn's *Structure of Scientific Revolutions* (1962/70). In the 1960s economists began to interpret the history of economic thought in terms of Kuhn's categories of paradigms, normal science and scientific revolutions.[1] Progress was defined only within paradigms, not across them, which meant that unless one argued that the whole of economic thought from Child to Walras constituted a single paradigm, Schumpeter's account must be flawed. Science could, after Kuhn,

[1] See, for example, Backhouse (1994a).

be understood only with reference to its sociology and its history. Though it is arguable exactly how far Kuhn himself went in this direction, the 'rules' of scientific procedure were to be found not in philosophy but in scientists' practices. Also important has been the influence of the Popperian school, notably Popper, Feyerabend and Lakatos. Popper's falsificationism makes it impossible to see scientific method as 'logical inference from verifiable facts'.[2] Feyerabend's methodological anarchism has made fun of the notion that there are absolute standards in science, dispelling the air of confidence that pervades histories such as Schumpeter's. Lakatos's methodology of scientific research programmes provided a framework, seemingly more rigorous than Kuhn's, through which the history of economics could be interpreted, whilst his methodology of historical research programmes, involving the idea that philosophy and history could inform each other through the method of rational reconstructions, provided a way to write philosophically informed history.

But how much have we learned? There is now considerable scepticism about the relevance of falsificationism to economics. It is hard to identify the components of Lakatosian research programmes, and his method of rational reconstructions is seen by many as distorting history.[3] Though it may none the less be important in altering our perspective (see Hausman, 1994), Kuhn's framework of paradigms and normal science does not take us very far in analysing history.[4] In so far as it is possible to speak of a trend in methodological-historiographical thinking in the 1990s it is probably an emphasis on what has been called 'recovering practice' – away from some of the questions that dominated the subject in the 1970s and 1980s, the answers to which made Schumpeter's position unacceptable.[5]

Given this trend, Schumpeter's *History of Economic Analysis* needs to be reconsidered, for the methodological statements it contains are those of one of the

[2] Popper's work, of course not the only reason for this, but his work has, for good or ill, dominated economist's discussions of such issues.

[3] See de Marchi and Blaug (1991), Chapters 3 and 4 above.

[4] The main reason for this is that it is hard to identify paradigms unambiguously. At one level, the whole of economics since Adam Smith can be seen as a single paradigm, but this fails to tell us much about the many fundamental changes that have taken place in economics since Smith's time. Alternatively we can plausibly identify competing paradigms on a much smaller scale (classical economics, Keynesianism, monetarism, neoclassical microeconomics, game theory), but there are problems with this approach too: it fails to take account of the immense amount that such paradigms have in common with each other; and it is inconsistent with Kuhn's view that at any time there is normally one ruling paradigm, not a range of competing ones.

[5] This is the sub-title of de Marchi (1993). Though there were earlier discussions of Kuhn's relevance to economics, a particularly important contribution was Latsis (1976). From this

leading economists of his generation. As is argued in the rest of this chapter, Schumpeter's view of the history of science is in some respects very similar to Kuhn's, but there are crucial differences. These differences should be seen not as reflecting positivist influences on Schumpeter, but as being closely linked to the nature of economics as the subject has developed in the nineteenth and twentieth centuries.

2 SCIENTIFIC STANDARDS AND THE EMERGENCE OF SCIENCE

Science, for Kuhn, is an activity carried out by an identifiable body of professionals who share both problems and methods of enquiry. The precondition for the emergence of science, therefore, is the emergence of a universally accepted framework or paradigm. Prior to the emergence of any such framework, professionalization is impossible. Instead one finds a variety of approaches – by competing schools, each based on different a different metaphysics. The absence of common belief, Kuhn argued, meant that there could be no progress, for each writer was compelled to build the subject from foundations, there being no agreed knowledge on which to build. Fact-gathering, therefore, was a random activity and there was no clear demarcation between scientist and non-scientist. This period was the 'prehistory' of a science. Transition from prehistory to 'science proper', though not sudden, took place during an identifiable time period – in electricity, for example, it was some time between 1740 and 1780.[6]

Much of this can be found in Schumpeter's *History of Economic Analysis*. Standards and the possibility of progress are associated with professionalization.

> Now our ability to speak of progress ... is obviously due to the fact that there is a widely accepted standard, confined, of course, to a group of professionals, that enables us to array different theories ... in a series, each member of which can be unambiguously labelled superior to the preceding one.
>
> (pp. 39–40)

(Continued from previous page)
point the emphasis shifted away from Kuhn towards Lakatos's methodology of scientific research programmes, perceived (whether correctly or not) by many economists as similar to Kuhn's methodology. See Backhouse (1994a).

[6] Kuhn (1962/70, chapter II).

Professional standards were, he claimed, absent before the end of the eighteenth century (p. 155).

Schumpeter recognized, as clearly as Kuhn or Feyerabend, that there are no absolute standards. ‘The exclusion of any kind of tooled knowledge,’ he wrote, ‘would amount to declaring our own standards to be absolutely valid for all times and places. But this we cannot do’ (p. 8). Even the magic practised in a primitive tribe should be considered science, provided that ‘it uses techniques that are not generally accessible and are being developed and handed on within a circle of professional magicians’ (p. 7). However, where Feyerabend (1988) argued that the values of modern, Western science should be dethroned from their privileged position in relation to other forms of knowledge, Schumpeter adopted the position that knowledge has to be interpreted ‘in the light of our standards, since we have no others’ (p. 8). Thus when Schumpeter deferred to the values of modern science (as seen by the philosophy of science of his day) this was the result of a deliberate choice – he did not deny that other perspective. The success of this attempt to reconcile absolute standards with a recognition of the historical contingency of ideas is debatable. Perlman (1994, p. xxxv), for example, contends that it was a failure.

The most substantial difference between Schumpeter’s treatment of the prehistory of science and Kuhn’s lies in the former’s emphasis on *analysis* as the distinguishing mark of science. Though he later defines science to include not only analysis but also specialist techniques of fact-finding (p. 7), the emphasis is overwhelmingly on analysis as the characteristic of science. In the opening sentence of the *History of Economic Analysis* he goes so far as to equate the ‘analytic’ with the ‘scientific’ aspects of economic thought. More significantly, however, his account of the period from Ancient Greece and Rome to the late eighteenth century he is continually looking for signs of analysis – the instances are too numerous to cite. Schumpeter emphasizes that a science must be the result of ‘conscious efforts to improve it’ (p. 7). For example, in his account of ‘Dearness and Plenty versus Cheapness and Plenty’ he concludes that ‘In important respects, the victory of the Cheapness-and-Plenty advocates spelled analytic advance’ (p. 286). The Cheapness-and-Plenty school saw that it was relative prices that mattered; that cheapness should be measured in terms of effort, not money; and that falling money prices were a natural way, in a growing economy, ‘giving effect to the increasing cheapness of things in terms of effort’.

Schumpeter claimed that science had to be the result of conscious intention: ‘a science is any kind of knowledge that has been the object of conscious efforts to improve it’ (p. 7). Where arguments were based on analytic principles, but in the context of specific industrial or commercial policy programmes, without the analytic principles necessarily being explicit, he referred to ‘quasi-systems’ (pp. 194–9). These had some features of science, but were not science. Schumpeter argues, for example, that the work of Justi was pre-scientific because, in addition to not using tools not at the layman’s command, ‘was not alive to the necessity of proving propositions’ (p. 173). In contrast, Schumpeter *did* see Cantillon as engaged in scientific analysis: ‘Cantillon no doubt felt the scientific need for some such tool [Quesnay’s *Tableau*], had the idea of how to construct one, *and* actually pointed the way toward doing so’ (p. 240; emphasis in original).

This emphasis on analysis as what distinguishes science from prescience is significantly different from what we find in Kuhn. Though Kuhn’s treatment of pre-science is, as he himself admits, ‘much too schematic’ (1962/70, p. ix) it is fair to say that he places much greater stress on facts. In the absence of a common body of belief, people were free to choose what to observe and what experiments to perform, and fact gathering was ‘a far more random activity than the one that subsequent scientific activity makes familiar’, the pool of facts often containing ‘those accessible to casual observation and experiment together with some of the more esoteric data retrievable from established crafts like medicine, calendar making, and metallurgy’ (ibid., p. 15).

3 VISION, PARADIGMS AND THE STRUCTURE OF SCIENCE

The most important aspect of Kuhn’s *Structure of Scientific Revolutions* is his claim that science is characterized by periods of normal science separated by scientific revolutions. His account of the way science developed was explosive because it suggested that shared presuppositions and practices, previously thought peripheral, might in fact be central to the whole process. To quote Hausman,

> Before the publication of Kuhn’s *Structure of Scientific Revolutions,* philosophers paid little attention to the web of commitments that bind together co-workers in a common research enterprise. ... [T]heir ambition was to use formal logic and conceptual analysis to provide abstract characterizations of central features of science, such as confirmation or explanation. They were inclined to regard the context-sensitive shared presuppositions that constitute distinct subdisciplines as obstacles in the way of appreciating the uniform underlying ‘logic’ of explanation, confirmation, theory structure and so forth. ... Kuhn’s *Structure of Scientific Revolutions* was published against this intellectual background, and its effect was explosive. Not only did it throw a spotlight on fascinating features of science that had been ignored by previous

> philosophy, but it offered a way of avoiding the dead-end to which logical empiricism apparently had led.
>
> (Hausman, 1994, pp. 195–6)

Kuhn, in other words, turned philosophers' attention to the structure of science.

Though some commentators have focused on scientific revolutions, the central concept in Kuhn's account of the structure of science is 'normal science'.

> 'normal science' means research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice.
>
> (Kuhn, 1962/70, p. 10)

Though he uses the phrase 'one *or more',* the examples he cites are instances of single achievements laying the foundations for subsequent work. These achievements provide what he later called a 'disciplinary matrix' that is unquestioned within the subsequent period of normal science. Within normal science research involves filling in the gaps, extending and applying the theory, and sorting out anomalies – dealing with pieces of empirical evidence that do not fit and polishing the theory. During any period of normal science, however, there will arise anomalies which cannot satisfactorily be explained away. If theses become too serious, confidence in the ruling disciplinary matrix will be shaken and the science will enter a state of crisis. During a crisis, the rules constraining research break down and scientists try almost anything— Kuhn calls this 'extraordinary research'. Many attempts will be made to resolve anomalies, but out of them one will eventually dominate, becoming recognized as resolving the anomaly and providing the basis for future research. Scientists switch their allegiance to the new paradigm or simply die. A new period of normal science emerges.

A period of crisis has much in common with the prehistory of science, discussed in section 2 above. There is no agreement on the framework within which scientific enquiry is to be carried out, with the result that the choice of methods and the facts that are sought are in a sense random. Thus Kuhn associates the beginnings of science with the emergence of a scientific achievement that has the characteristics needed to form the basis for a period of normal science. The emergence of science is thus simultaneous with the emergence of normal science.

If we focus on Schumpeter's emphasis on the emergence of the analytical tools of general equilibrium analysis, and on what he termed the 'filiation of economic ideas', his perspective appears to be clearly pre-Kuhnian. The *History of Economic*

Analysis, however, also contains in its discussion of 'classical situations' a picture of the structure of economics which has much in common with Kuhn's picture of the structure of science. Schumpeter never completed the section in which he was to define the concept of a classical situation,[7] but his use of the term makes clear its similarity with Kuhn's concept of normal science. Consider his description of the 'second' classical situation, which emerged from the two decades of struggle following the innovations of Jevons, Menger, Walras and the historical school.

> And from these again emerged, in the nineties, a typical classical situation in our sense, the leading works of which exhibited a large expanse of common ground and suggest a feeling of repose, both of which created, in the superficial observer, an impression of finality – the finality of a Greek temple that spreads its perfect lines against a cloudless sky.
>
> (p. 754)

Referring to the monetary theory of the 'first' classical situation, he wrote,

> Adam Smith substantially ratified it. And for more than a century to come it was almost universally accepted ... so much so, in fact, that the majority of economists came to suspect not only unsoundness of reasoning but something very like obliquity of purpose behind every expression of antimetallist views.
>
> (p. 290)

Classical situations are characterized by consensus over fundamental issues and by a refusal to question basic assumptions – to regard dissenters as illogical or prejudiced.

Like Kuhn's paradigms or disciplinary matrices, Schumpeter's classical situations may eventually enter a period of decay. Indeed, his description of the 'first' classical situation as it existed after J. S. Mill comes close to equating decay with the settling down of the subject.

> Then followed stagnation – a state that was universally felt to be one of maturity of the science, if not one of decay; a state in which 'those who knew' were substantially in agreement; a state in which, 'the great work having being done,' most people thought that, barring minor points, only elaboration and application remained to be done.
>
> (p. 380)

[7] Schumpeter (1954), p. 51, n. 1.

Out of decay comes revolution, in this instance associated with Jevons, Menger and Walras (p. 825).

Schumpeter provides a detailed account of how new ideas might emerge if, as we hardly ever do, we had to start from scratch. Three stages are involved. The first stage is 'vision' – 'to visualize a distinct set of coherent phenomena as a worth-while object of our analytic efforts' (p. 41). This is a 'preanalytic' act, inseparable from ideology (p. 43). He makes the point, however, that such a preanalytic act comes in not only at the beginning of analysis, but also every time the subject is transformed.

> It is interesting to note that vision of this kind not only must precede historically the emergence of analytic effort in any field but also may re-enter the history of every established science each time somebody teaches us to *see* things in a light of which the source is not to be found in the facts, methods, and results of the pre-existing state of the science.
>
> (p. 41; emphasis in original)

We have a clear parallel here with Kuhn's notion that new metaphysical presuppositions are the basis for every new paradigm – switching from one paradigm or classical situation to another involves seeing things in a new light. The second stage is to 'verbalize' or 'conceptualize' the vision. The elements of a vision are given names that 'facilitate recognition and manipulation, in a more or less orderly schema or picture' (p. 42). Conceptualizing the vision will lead 'almost automatically' to further fact-gathering and to the addition and deletion of concepts. The final stage is the emergence of 'scientific models'.

> Factual work and 'theoretical' work, in an endless relation of give and take, naturally testing one another and setting new tasks for each other, will eventually produce *scientific models,* the provisional joint products of their interaction with the surviving elements of the original vision, to which

> increasingly more rigorous standards of consistency and adequacy will be applied.
>
> (p. 42; emphasis in original)

Three comments are worth making here. (1) Though the nature of the testing is left unanalysed in a way that would nowadays be difficult, the emergence of a scientific model involves interaction between theoretical and empirical work. Empirical testing of theories appears almost to be unproblematic. (2) Schumpeter sees the elements of the original vision as being modified, possibly substantially, during the process whereby a vision is transformed into a scientific model, a perspective similar to that of Cohen (1977). It may, therefore, be impossible to define an invariant Lakatosian 'hard core' that describes the emerging science.[8] (3) An important aspect of the process appears to be increasing rigour.

Most, if not all, of this is compatible with Kuhn's account of the emergence of paradigms. They differ in that Schumpeter focuses on the processes of discovery and analytical refinement, whilst Kuhn focuses on the way in which new ideas emerge from the crisis in the previous period of normal science. Kuhn thus tells us more about the structure of science in that he provides a much fuller account of how one paradigm succeeds another. There is, however, an even more important difference. For Kuhn, a new paradigm is a path-breaking work which sets an agenda for future research. Revolutionary science is characterized by a proliferation of theories and methods, one of which one eventually emerges triumphant. This becomes the paradigm in the sense of the 'exemplar' defining the way research is to be undertaken. Schumpeter, however, sees the emergence of classical situations rather differently. For him the works that define classical situations are ones that *consolidate* previous knowledge.

> But every classical situation summarizes or consolidates the work— the really original work—that leads up to it, and cannot be understood by itself.
>
> (p. 52)

Schumpeter's 'classic achievements'[9] are not Kuhnian exemplars but works of synthesis, such as J. S. Mill's *Principles of Political Economy* (1848) or Marshall's *Principles of Economics* (1890).

[8] The difficulties involved in finding such 'hard cores' have been one of the major problems found with applying Lakatos's methodology of scientific research programmes to economics.
[9] The phrase 'again, in our sense of the term' (p. 380) suggests that this is being used as a technical term alongside 'classical situation'.

> The breaks with tradition around 1870 were meant to be breaks by the men whose names are associated with them. ... Upon these ‘revolutions’ followed two decades of struggle and more or less heated discussions. And from these again emerged, in the nineties, a typical classical situation in our sense.
>
> (pp. 753–4)

Revolutionary works, based on new visions, shatter existing consensus, initiating periods of struggle, or revolutionary science. For Schumpeter, however, classical situations are based not on these revolutionary works but on the subsequent works of consolidation and synthesis. Such ‘classic achievements’ are frequently textbooks (Mill’s *Principles of Political Economy* or Marshall’s *Principles of Economics* are obvious examples) but they are none the less important creative acts, going beyond the textbooks of Kuhnian normal science.

4 REASSESSING THE HISTORY OF ECONOMIC ANALYSIS

It is tempting to argue that Kuhnian, and later Lakatosian, ideas about the evolution of science caught on so rapidly in economics because Schumpeter had paved the way. Indeed, Coats, in one of the earliest essays on the relevance of Kuhn to the history of economic thought, noted that Kuhn’s model ‘adds precision to Schumpeter’s conception of the “classical situation”’ (Coats, 1969, p. 61). There is certainly much in favour of such an interpretation. There are strong similarities between Kuhn and Schumpeter.

1. Kuhn’s normal science is very similar to Schumpeter’s classical situation.
2. Science progresses through alternating periods of revolution and stability.
3. The transition from pre-science to science involves the emergence of a dominant framework.
4. Metaphysical presuppositions do matter.
5. Science makes sense only as a professional activity.

In addition, Kuhn does, in crucial respects, go much further than Schumpeter – he ‘adds precision’.

1. He provides an explanation of why paradigms break down.
2. He analyses the very different roles played by empirical evidence in periods of normal and revolutionary science.
3. He shows how normal science may be (indeed, normally is) established on the basis of an exemplar.

To this extent Coats is right.

There are, however, significant differences between the two. The obvious ones are that, in talking about science, Schumpeter retained more of the language of logical positivism than did Kuhn,[10] and that held a clear (Walrasian) view of the nature of the fundamental economic problem. These differences explain why he was able to write his history in a way that is very different from what one might expect of someone whose perspective was close to Kuhn's. They explain, as was pointed out in section 1 above, why Schumpeter's *History of Economic Analysis* now appears somewhat dated. There is, however, a much more fundamental difference between Kuhn and Schumpeter.

Though one of his basic insights was the observation that much of science is uncritical, taking the disciplinary matrix as fixed, Kuhn never doubted that it was the interaction of theories with empirical evidence that provided the fundamental explanation of the growth of scientific knowledge. Much of normal science involves fact-gathering and the application of the ruling paradigm to new areas. Arguably the main reason why crises develop is empirical failures of the paradigm – anomalies that need to be resolved. Though the process is far removed from Popper's falsificationism, Kuhn is concerned with the way in which theories are tested against empirical evidence.

In contrast, though Schumpeter would never have denied the importance of testing theories, the process is unanalysed. The reason is that (at least in the *History of Economic Analysis*) he sees economics as primarily analytical – as akin to mathematics. Analytic progress is associated with increased rigor. He criticizes economists for inadequate logic and for not seeing the need for proofs of important propositions, and he does not question basic assumptions (knowledge of the *meanings* of economic actions, and interdependence as the central economic problem). What drives the *History of Economic Analysis* is the development of the economist's 'box of tools', not the results that can be achieved with those tools. Using McCloskey's phrase, Schumpeter, unlike Kuhn, appears to have 'adopted the intellectual values of the Math Department' (McCloskey, 1991, p. 8). This accounts, amongst other things, for why Schumpeter emphasizes consolidation as the key to a classical situation, where Kuhn sees a pioneering contribution as critical. In the sciences with which Kuhn is concerned, the resolution of anomalous

[10] Kuhn's break with contemporary philosophy of science must not be exaggerated. *The Structure of Scientific Revolutions* was after all published as a volume of the *International Encyclopaedia of Unified Science,* edited by Otto Neurath.

empirical evidence provides a criterion that causes scientists to choose one theory and to abandon others. Because economic theory is more like mathematics, the nature of the consolidation process is different, with the result that the nature of paradigms, both as exemplars and as disciplinary matrices, is different.

Writers on economic methodology have, in recent years, expressed interest in ‘recovering practice’ – in seeking to understand what it is that economists are actually doing. For many this has followed from a realization that the history of economic thought exhibits many features that models taken from the philosophy of natural science cannot explain. For example, Hausman (1991) has sought to do this by going back to, and developing, J. S. Mill’s notion of an inexact science, whilst Rosenberg (1992) has argued that economics should be seen either as mathematics or as a branch of contractarian political philosophy. What these have in common is that they emphasize the importance of mathematical, logical progress in the development of economic theory, whilst minimizing the role of empirical testing (they are both critical of this, but that is a different matter). One of Schumpeter’s merits *as a historian of economic thought* is that he shares this emphasis on logic and mathematics, for the result is that when viewing the history of economic thought he focuses on analytical progress. This means that his account of the history of economic thought reflects the centrality of theory that philosophers such as Hausman and Rosenberg have sought to understand. Schumpeter’s vision of how economics develops offers more than merely a vague anticipation of Kuhn’s *Structure of Scientific Revolutions:* it offers a perspective on the history of economic thought that reflects the nature of the subject as seen by many leading economists and philosophers in a way that Kuhn’s does not.

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Part IV

Pragmatism and empirical philosophy of science

Chapter 15

The fixation of economic beliefs[*]

(*Journal of Economic Methodology* 1(1), 1994, pp. 33–42.)

This chapter uses Peirce's discussion of how beliefs become settled to consider why economists disagree, showing that his four methods – tenacity, authority, the a priori *method and the method of science – can be used as a framework for addressing the question. More important, it suggests that, though talking in terms of Peirce's 'the method of science' may raise as many questions as it answers, these questions lead into a discussion of some of the key issues concerning the relationship between economic theory and empirical evidence. In short, it shows that pragmatism provides a useful way of thinking about the process of economic enquiry.*

1 DOUBT, BELIEF AND ENQUIRY

The origin of enquiry, argued C. S. Peirce, the founder of American pragmatism, is doubt.

> Doubt is an uneasy and dissatisfied state from which we struggle to free ourselves and pass into the state of belief; while the latter is a calm and satisfactory state which we do not wish to avoid, or to change to a belief in anything else. ... The irritation of doubt causes a struggle to attain a state of belief. I shall term this struggle *inquiry.*
>
> (Peirce, 1877, pp. 66–7)

This perspective is a useful one from which to approach the problem of disagreement in economics. If we ask simply 'Why do economists disagree?' we obtain a myriad of answers. Instead, Peirce's perspective focuses attention on the process of enquiry

[*] I am grateful to Tom Mayer for valuable comments on an earlier draft and to Chris Hookway for helping me to understand Peirce.

– on the way in which doubts are removed and beliefs established, a process he terms 'the fixation of belief'. Three main questions arise.

- By what methods do economists move from doubt to belief?
- Are these methods successful?
- Do these methods converge on anything that might be termed the truth?

The second and third questions here are not the same. Beliefs may successfully be stabilized by the imposition of authority or because they are effective in achieving aims other than reaching the truth. As McCloskey (1986) has pointed out, economists' ideas may be persuasive for many reasons. More specifically, Ravetz (1971, pp. 386–9; 1995) has argued that economics should be seen as a folk-science which provides a sense of security for its adherents, with abstraction and formalism serving to deflect attention from politically sensitive issues.

Peirce discussed four methods for fixing beliefs: tenacity, authority, the *a priori* method and what he termed 'the method of science'. What the first three have in common is that beliefs that are settled in these ways are liable to be unstable. People will have reason to question such beliefs, and when they do so the result will be disagreement. Only 'the method of science', he contended, provides a stable method for fixing beliefs. The reason is that scientists (or anyone following the method) observe a common reality.

> There are real things, whose characters are entirely independent of our opinions about them; those realities affect our senses according to regular laws, and, though our senses are as different as our relations to the objects, yet, by taking advantage of the laws of perception, we can ascertain by reasoning how things really are, and any man, if he have sufficient experience and reason enough about it, will be led to the one true conclusion.
>
> (Peirce, 1877, p. 74)

Peirce's conceptions of truth and science raise so many philosophical questions that to focus on them here would be a distraction (see Hoover, 1994, and Hookway, 1985, for more thorough accounts of his ideas). For our purposes it is enough to note the crucial role of empirical evidence in constraining belief, thereby eliminating disagreement.

2 TENACITY

When outsiders criticize economists for constantly disagreeing with each other they probably have in mind a world where economists simply cling to their beliefs in the face of contrary evidence. In some cases economists will be seen as motivated by ideology (as when they are forcing underdeveloped countries to implement tough stabilization programmes or when they are arguing for privatization); in other cases their motives are left unexplained. In so far as beliefs are fixed by tenacity, the result is likely to be disagreement. Economists will represent different interests and different ideologies, and this will lead them to hold different beliefs. The method of tenacity provides no means whereby disagreements can be resolved.

3 AUTHORITY

Heterodox critics of economics often argue that orthodox economics is sustained by authority. The discipline is hierarchical, great power residing with editors of leading journals, who exclude work that does not conform to certain standards. These standards involve standards of mathematical rigour and acceptance of basic assumptions, such as rationality. Because appointments, tenure and promotion require publications in these journals, academic economists are forced to accept these standards.

The use of authority to impose standards and settle beliefs is of long standing. The professionalization of economics in the US in the second half of the nineteenth century was accompanied by a professional conservatism as academics sought to reconcile the conflicting demands of having to publish on topical issues and of avoiding controversy (Coats, 1980). In other countries, too, authority proved important. In England Marshall used his authority to minimize dissent in an attempt to create a scientific economics (Coats, 1964, 1967). In Germany, Schmoller and the historical school wielded great authority.

Though editors of leading journals clearly exercise considerable authority, it is not clear how far they set standards rather than simply reflecting a professional consensus. The list of top-ranked journals depends on where people in the leading departments choose to send their work, and on factors such as citation and rejection rates. Furthermore, given the immense technological changes affecting the publishing industry, it is not clear that the current situation is stable – indeed, proliferation of journals and other publications has already had a significant impact on the way ideas are communicated and codified, and electronic publishing has the potential to introduce even more radical changes. Clearly editorial policies do push dissent outside the ‘major’ journals, into ‘lesser’ journals, books and conference proceedings, but the outlets for heterodox ideas are more numerous than ever before. Dissent has not been suppressed, but has, to some extent, become institutionalized, through the existence of separate journals, conferences and so on.

4 THE *A PRIORI* METHOD

The *a priori* method settles disputes by reasoning from assumptions that are 'agreeable to reason'. The classic statement of such a position is that of Robbins who argued that the central propositions of economics could be derived from the assumption of scarcity. He described the assumption that individuals have preferences as 'an essential constituent of our conception of conduct with an economic aspect' (Robbins, 1932, p. 76). It was such a reasonable assumption that no one would question it.

> No one will really question the universal applicability of such assumption[s] as to the existence of scales of relative valuation, or of different factors of production, or of different degrees of uncertainty regarding the future.
>
> (ibid., p. 81)

Mainstream economists have clearly defined views about the assumptions that it is reasonable to make.

- Economic agents should be modelled as individuals.
- Agents have preferences.
- Given their preferences, agents optimize subject to constraints imposed by markets, the assumed behaviour of other agents and technology.

In so far as there is consensus within economics, it is arguable that it arises from a common acceptance of assumptions such as these.

The problem with such an approach is that assumptions that seem reasonable to one person are unlikely to seem reasonable to everyone else. As Peirce put it, the *a priori* method 'makes of inquiry something similar to the development of taste; but taste, unfortunately, is always more or less a matter of fashion' (Peirce, 1877, p. 73). It does not eliminate disagreement. In economics there are further problems, for the assumption that preferences and technology are given results in economics becoming focused on resource issues of resource allocation, with the result that there is considerable consensus on problems that clearly fall within such a framework (the effects of price ceilings, rationing, tax incidence, and so on). There are some dissenters even here (Austrians, for example, argue that more attention should be paid to the dynamic effects of competition than to static resource allocation) but these are kept to one side. Where problems fit uncomfortably within this framework, on the other hand, disagreement is endemic – as with economic development, unemployment and the business cycle.

5 ECONOMETRICS

There was once considerable optimism about the ability of econometrics to settle disputes in economics. If theorists would formulate testable theories, econometrics would test one against the other. This is not quite how it has turned out, many (if not most) economists having become sceptical about how much econometrics can contribute. At one level there are the arguments of well-established figures such as Summers (1991), Friedman and Schwartz (1991) and Mayer (1993). Summers, for example, has written that 'formal empirical work which ... tries to 'take models seriously econometrically' has had almost no influence on serious thinking about substantive as opposed to methodological questions' (Summers, 1991, p. 129). Mayer has likened much econometric work to 'driving a Mercedes down a cow track' – it is using tools which are too sophisticated for the job in hand. At another level there is the cynicism shown by many graduate students. In response to a series of interviews with graduate students, Colander wrote,

> Students are presented with a contradiction. Many, if not most, of the interesting questions in economics are not empirically testable with econometrics. ... Students resolve the contradictions by (1) doing abstract theoretical work that will never be empirically tested at a later date (which often never comes); (2) empirically testing what can't be empirically tested and coming up with results that convince few, but are formally impressive; (3) becoming cynical and leaving the economics profession; and (4) developing their own reasonable test criteria and learning on their own.
>
> (Klamer and Colander, 1990, p. 190)

There are several ways in which this can be understood.

1. Economists practise what Blaug (1992, p. 244) has called 'innocuous falsificationism'. They proclaim that theories should be falsifiable, but make no serious attempt to falsify them. Such practice could be explained in terms of economists' commitment, on non-empirical grounds, to their theories. Alternatively it could be explained by journals wanting positive results. Significant test statistics are required as part of the process of quality control.
2. Less unfavourable is the argument that when testing comes out against a theory, the weakest link is frequently not the theory, but the quality of the data

or the way the theory was tested (Hausman, 1991). There are many theories for which the data needed to test them are simply not available, with the result that tests are undertaken using data that are inadequate for the task (such as unemployment statistics that measure the number receiving benefit, not the number seeking work, or accounting measures of profit that do not correspond to the economist's definition of profit). It may, therefore, be rational for economists to retain a theory and to blame the apparent refutation on other factors.

3. It is only specific models, not economic theories, that can be tested. Economic theories are normally fairly imprecise and can support a large variety of models. If a model is rejected as a result of econometric testing, the theory can frequently be retained. To develop a testable model it is normally necessary to specify functional forms and lag structures, on which theory has very little, if anything, to say. Thus in much of Hendry's work, for example, economic theory does no more than provide constraints on long run equilibrium conditions – all the rest is driven by the data.

For all these reasons, econometric tests rarely lead, on their own, to decisive tests of economic theories. Such testing is not, however, without importance. As one of the graduate students interviewed by Klamer and Colander remarked, econometrics gives you 'suggestive results' (Klamer and Colander, 1990, p. 70).

6 EMPIRICAL EVIDENCE AND ECONOMIC THEORY

Economists have at their disposal an immense quantity of empirical evidence. In part this comes from formal econometric studies, but arguably more important are (1) informal generalizations made on the basis of statistical data; (2) knowledge of economic institutions; (3) lessons learned from historical-events. Thus economists theorize on the basis of 'stylized facts' about the cyclical behaviour of real wages; they observe that the nature of labour contracts differs between Europe and the US in various ways; and they have observed the way wages responded to supply shocks during the 1970s. Even a cursory glance at intermediate texts such as Dornbusch and Fischer *Macroeconomics* (1994) or Varian *Intermediate Microeconomics* (1990) shows the immense amount of empirical evidence – the three types of evidence just mentioned together with, especially in macro, econometric results – that is woven into the story.

More significantly, economists do take some empirical evidence seriously. (1)

Even though no single study may be decisive, evidence does build up to the point where theorists take note of it. One example is the breakdown of the Phillips curve in the early 1970s – though no individual regression equation was convincing, it was, by around 1972, clear on the basis of both econometric and other evidence, that the idea of a stable trade-off was untenable. (2) Much theorizing is a direct response to empirical puzzles. Efficiency wages, long-term contracts and theories of hysteresis are clear examples of theories driven by empirical problems. (3) Though theory dominates advanced teaching and the leading journals, much applied economics goes on with only passing reference to theory.

Why, then, is empirical evidence not more successful in settling disputes in economics? The poor quality of much empirical evidence has already been mentioned as one explanation. Two further reasons can be added. The first is that, because of the complexity of economic phenomena, economic theories of necessity abstract from many features of the real-world in the sense that they have implications that are false. Economists, therefore, routinely dismiss evidence against their theories. Evidence that individual firms exercise market power, for example, is not taken to be a problem for macroeconomic theories based on perfect competition even though the theory of perfect competition predicts that such activities will not occur. It may be seen as an anomaly, and some economists may explore alternative theories, but there is no presumption that such problems are serious.

The second is that, despite the vast amount of applied economics, facts about the world are not central to the discipline. Though economists claim to attach great importance to empirical work, they regard the discipline as being centred on what Marshall called the 'engine of economic theory' and Joan Robinson the 'box of tools'. This was revealed very neatly by two linguists who analysed the way economists modify propositions (saying, for example, 'It seems likely that money causes inflation' or 'I wish to suggest that money causes inflation' rather than 'Money causes inflation') in a randomly selected issue of the *Economic Journal* (Bloor and Bloor, 1993). Evidence from biology suggests that field-central propositions are normally modified (hedged) whereas propositions that are not field-central are not. In economics they found that theoretical propositions, whether the theory was economic or econometric, were invariably hedged, but statements about the world were not.

Evidence for the centrality of theory to economics is abundant. (1) Though textbooks contain much empirical material, it is rarely indispensable – theory provides the structure. Thus European students use US textbooks, skipping all the empirical material on the grounds that, being US oriented, it is irrelevant to them. (2) In their survey of graduate students, Klamer and Colander found that only 3 per cent considered 'a thorough knowledge of the economy' very important for success, with 68 per cent considering it unimportant. With problem-solving ability and mathematical excellence these proportions were almost exactly reversed: problem-solving ability was thought very important by 65 per cent, and unimportant by 3 per cent, and mathematical excellence very important by 57 per cent and unimportant by 2 per cent (Klamer and Colander, 1990, p. 18).

The centrality of theory, and the peripheral nature of facts about the world, is reinforced by the increasing use of what Fisher (1989) has termed 'exemplifying theory'. The distinguishing feature of exemplifying theory is that it analyses what *might* happen. Fisher contrasts it with 'generalizing theory', which describes what *must* happen if the theory is correct. The problem is that generalizing theory cannot be tested, because it does not rule out any state of the world - if it is inconsistent with empirical evidence, one simply concludes that it is not applicable. It is well illustrated by the new literature on industrial organization in which game theory provides the unifying framework and in which almost anything can happen. Empirical evidence provides a series of puzzles to which theorists provide solutions, the main constraints on which are mathematical rigour and the assumption of fully rational behaviour. Exemplifying theory is, therefore, a set of tools. However, where Marshall was worried about the dangers of economists regarding abstract or theoretical economics as economics 'proper', contemporary economists appear to be less worried about such dangers.

A side effect of the centrality of theory to economics is that the mechanisms for consolidating empirical knowledge and structuring the discipline around empirical evidence are weak. One aspect of this process, for example, is the role of textbooks, some writers on the sociology of scientific knowledge going so far as to say that it is because they appear in textbooks that scientific facts are facts, not the other way round. Given the peripheral role of facts about the economy in micro and macro texts, it is hard to see facts being 'established' in this way. Arguably more important, in an experimental science the process of replication provides a means whereby the existence of phenomena is established. Replication of non-experimental results (say of macroeconomic time-series regressions) is much more problematic (see Backhouse, 1992).

7 THE CONVERGENCE OF ECONOMIC BELIEFS

Peirce believed that the method of science would, for the reasons outlined above, eventually eliminate disagreement: in the long run there would be agreement amongst all enquirers. The previous two sections have suggested several reasons for thinking that the weakness of the constraints imposed by empirical evidence on economists' beliefs make such a scenario implausible. Further reasons for doubting the ability of economists to reach a consensus arise from the nature of the material with which they are forced to deal.

1. The economic world is continually changing. Thus, even if beliefs would, given sufficient time, converge, sufficient time may not be available. The world may have changed first. Even more fundamental, the world may change in response to economists' discoveries. If an empirical regularity is discovered, whether the Phillips curve or a stable bank reserve ratio, governments may seek to exploit it and as a result the relationship may disappear.
2. Economists have, compared with those working in experimental sciences, little control over what they observe. They are dependent on statistical offices, government departments and the like for much of their empirical information. Concepts are redefined, and statistics do not correspond to what economists would wish to measure. There are thus severe constraints on the extent to which economists can improve their discipline by improving the quality of their data.
3. To a great extent, the agenda economists face is set by noneconomists. The avenue of reaching agreement by redefining the questions in such a way that they can be answered is to this extent closed

8 WHY DO ECONOMISTS DISAGREE?

This question can be rephrased as, 'Why are the methods by which economists settle disagreements (fix beliefs) so ineffective that they are perceived to disagree more than one might reasonably expect?' The arguments surveyed here suggest a number of answers. Tenacity and authority probably play some role, and the *a priori* method is undoubtedly important. As Peirce noted, none of these methods is effective in settling disputes except within limited domains and for short periods: the result is perennial disagreement. In the long run only empirical evidence can effectively stabilize beliefs, but in economics empirical evidence provides only very weak constraints on theorizing – the discipline is centred on theory, procedures for consolidating empirical knowledge being weaker than those for empirical results;

there is little control over the questions that need answering or certain types of empirical data; and great problems are posed by the changing nature of the economic world.

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Chapter 16

An empirical philosophy of economic theory

(*British Journal of the Philosophy of Science* 46, 1995, pp. 111–21.)

This chapter and the following one discuss Hausman's The Inexact and Separate Science of Economics (1992a), *perhaps the most important book to have been published on economic methodology since Blaug's* The Methodology of Economics (1980/92). *This chapter outlines the book's arguments, and argues that although it succeeds in providing a convincing portrait of much contemporary economics, it is only a very partial one, focusing excessively on the core of the subject, and that Hausman ends up less critical of the status quo in economics than is perhaps appropriate (or than the book's closing chapters suggest he wishes to be).*

1 INTRODUCTION

From its origins in the mid-1970s till some time during the 1980s, discussions of economic methodology were dominated by falsificationism, the most frequently discussed philosophers being Popper and Lakatos, with Kuhn close behind.[1] During this period one of the leading critics of falsificationism has been Daniel Hausman, who has argued that John Stuart Mill's methodology is, with certain modifications, basically right. *The Inexact and Separate Science of Economics* (1992a) provides the most developed statement of his position to date and is the more important of the two books under review. *Essays on Philosophy and Economic Methodology* (1992b) brings together seventeen essays published during the period 1980–91. Some of these contain material incorporated into *The Inexact and Separate Science*

[1] See the introduction and chapter 1 of Hausman (1992b). My own interpretation can be found in Backhouse (1994a, chapter 1).

of Economics, but others explore themes that receive little attention there. Essays that should not be neglected are his survey of the field (chapter 1), the essays on causality (chapters 9–11) and explanatory progress (chapter 14) and his general reflections on how the subject should be pursued (chapters 16–17). Because it is the clearly the more important of these two books, this review will focus on *The Inexact and Separate Science of Economics,* with some reference to Hausman's *Essays* where this is appropriate.

2 MILL MODIFIED

Though other themes are pursued as well, *The Inexact and Separate Science of Economics* is concerned primarily with analysing what neoclassical economists actually do. This follows from Hausman's credo that 'The philosophy of science is itself an empirical science' – that conclusions about science must be defended in the same way that scientific results are themselves defended (Hausman, 1992b, p. 221). He argues that current practice is best described in terms of a modified version of John Stuart Mill's *a priori* method, and (crucially) that there are good reasons why economists have adopted such a methodology.

Hausman's modified *a priori* method involves four stages:

1. *Formulate* credible (*ceteris paribus*) and pragmatically convenient generalizations concerning the operation of relevant causal factors.
2. *Deduce* from these generalizations, and statements of initial conditions, simplifications, etc., predictions concerning relevant phenomena.
3. *Test* the predictions.
4. If the predictions are correct, then regard the whole amalgam as confirmed. If the predictions are not correct, then *compare* alternative accounts of the failure on the basis of explanatory success, empirical progress, and pragmatic usefulness.

(Hausman, 1992a, p. 222; emphasis in original).

This differs from Mill's method in the first and last stages. In the first stage, deduction is based on 'credible and pragmatically convenient generalizations', not on proven laws; and in the last stage, a more flexible range of responses to predictive failure is allowed for. This is all consistent with the hypothetico-deductive method (ibid., pp. 222–3, 304).

Such a method, Hausman claims, describes what economists actually do, and, equally important, it is a method that can, unlike falsificationism, be defended.

When theories fail empirical tests, economists have to decide whether to modify the theory or to conclude that something else was wrong (for example, that the *ceteris paribus* condition was violated). Given the poor quality of most economic data[2] and the strength of the arguments for the assumptions underlying much economic theory, it is frequently sensible for economists to have more confidence in their theories than in conflicting empirical evidence. As a result they are not prepared to modify or abandon the theories. What Blaug (1980/92) has termed 'innocuous falsificationism' arises not because economists are methodologically misguided, but because it is a rational response in the face of inadequate empirical evidence. This does not mean, however, that Hausman is uncritical of economics. He argues that if research effort were directed differently, the quality of empirical data could be improved and there would arise more circumstances where economists would have reason to believe their data.

3 ECONOMICS AS A SEPARATE SCIENCE

Hausman's second main argument is that economists are concerned that economics be a *separate* science – not dependent on psychology, sociology or any other science for its behavioural postulates. The main evidence for this proposition is provided in a fascinating discussion of preference reversals (1992b, chapter 13).[3] The phenomenon of preference reversal arises when subjects are faced with a choice between two lottery tickets, usually referred to as the $-bet and the P-bet. The $-bet offers a small prospect of a very high monetary reward (say a 10 per cent chance of $100) whereas the P-bet offers a high chance of obtaining a much lower reward (say a 90 per cent chance of $10). Under experimental conditions a significant proportion of subjects places a higher value on the $-bet than on the P-bet, but when asked to choose between them chooses the P-bet (on the grounds that with the $-bet they are very likely to end up with nothing). This phenomenon was discovered by psychologists who wished to test the hypothesis that choice and valuation were two distinct operations. Their experimental evidence thus confirmed

[2] Most economic statistics are compiled by government departments and other organizations. Sources (for example, tax returns) are often inaccurate and statistics do not always correspond to the corresponding concepts in economic theory (for example, unemployment statistics measure those receiving benefit, not those seeking work; profits are measured using accounting concepts).
[3] A previously pubhshed version can be found in Hausman (1992b, chapter 15).

their theory. It is, however, inconsistent with the theory of rational choice and utility maximization that underlies most contemporary economics.[4]

Hausman argues that economists resorted to all sorts of manoeuvres in order to explain away experimental evidence. In brief, the story is as follows:

1. Attempts were made to explain away the evidence by finding fault with the experimental design.
2. New experiments were conducted, to test whether the reference reversals disappeared when these faults were eliminated, but the phenomenon was found to persist.
3. Attempts were made to explain the results by dropping some of the axioms underlying expected utility theory, and by postulating *ad hoc* utility functions.
4. Psychological theories were ignored – it was frequently claimed that preference reversals were an unexplained phenomenon, even though psychologists had explanations they regarded as acceptable (the phenomenon had even been predicted before the experiments were undertaken).

Hausman draws two conclusions from this. The first is that the economists involved exhibited no dogmatism as regards theory appraisal: utility maximization was clearly not being treated as a unquestionable law. Utility maximization could, therefore, not be seen as part of the hard core of a Lakatosian research programme. The second is that they were exhibiting enormous dogmatism as regards their commitment to economics as a *separate* science. Any solution to the problem of preference reversals which might force economists to rely on psychology for behavioural assumptions, was rejected, for it would undermine the separateness of economics from other sciences. Any solution which retained the 'separateness' of economics was explored.

4 THE HEGEMONY OF EQUILIBRIUM THEORY

Economists' commitment to economics as a separate science is linked to a particular theoretical strategy to which they are equally committed – to the search for a general, unifying theory capable of explaining economic phenomena. This unifying theory is equilibrium theory.

[4] Economists generally take preferences as given. Behaviour is rational if preferences are consistent (and usually transitive) and agents choose their preferred action from the set of actions available to them. Throughout this review, the term rational is understood in this sense. For a more thorough discussion see Hausman (1992a, chapter 1).

> Neoclassical economics *is* the articulation, elaboration and the application of equilibrium theory. ... Equilibrium theory consists of the theory of consumer choice, the theory of the firm and the thesis that equilibrium obtains.
>
> (Hausman, 1992a, p. 272; emphasis in original)

Consumer choice in turn comprises rationality, consumerism and diminishing marginal rates of substitution, whilst the theory of the firm comprises diminishing returns, constant returns to scale and profit maximization. Equilibrium, however, is not necessarily either competitive or general.

Hausman denies that equilibrium theory, defined in this way, can be thought of as a Lakatosian hard core.

> Commitment to equilibrium theory is only commitment to some subset of its components. Some of the laws that constitute equilibrium theory are more central than others, but, if one attempts to say what theoretical economics is by identifying some common core of propositions that are shared by every model or theory, one will not be able to give an informative characterization. So what Lakatosians might be inclined to call the 'negative heuristic' does not forbid tampering with equilibrium theory. It effectively forbids removing rational greed and the possibility of equilibrium from their central places, but the characterization of the 'pseudo-hard-core' is left open: non-satiation can be replaced with satiation, but claims about cognitive dissonance are not allowed. Incompleteness or intransitivities can be explored, but psychological generalizations about procedure variance are probably forbidden.
>
> (ibid., p. 272)

This conclusion matches that of Hoover (1991) who has argued that the new classical macroeconomics, though a research programme in the everyday sense, is not a Lakatosian 'scientific research programme', for it has no 'hard core' common to all theories. Yet every theory within the new classical macroeconomics is closely related to some other theory within the programme, enabling one to speak of family resemblances.[5]

Hausman provides a number of reasons for this 'hegemony of equilibrium theory'. Rational choice theory provides a theory of how people *ought* to behave – if people exhibit preference reversals an experimenter could systematically extract money from them.[6] The most important reason, however, is that rational choice theory

[5] Despite Hausman's rejection of Lakatos, Hausman's conclusion that neoclassical economics is characterized primarily by its research strategy fits will with the conclusion that can be reached via a Lakatosian route. See Chapters 2, 3 and 5 above; Weintraub (1985).

provides the simple, unifying theory required for economics to be a separate science covering a uniquely economic domain. It is only a slight exaggeration to say that economics is *defined* as the science of rational choice.[7] Other explanations are not regarded as part of economics.

5 THE SCOPE OF ECONOMICS

Hausman makes it very clear, right from the start, that he is concerned *only* with contemporary neoclassical microeconomic and general equilibrium theory (1992a, p. 1). Though Hausman makes light of it, this point is vital, for it affects his whole argument. He defends it on the grounds that it is simply a terminological convenience.

> *Indeed, to avoid unnecessary repetition, I shall usually omit the adjective 'neoclassical' and just speak of 'economics' when I am discussing neoclassical economics.* This is merely a convenience, not a covert attempt to denigrate other schools or to define them out of existence.
>
> (ibid., p, 3; emphasis in original)

The problem, however, will not go away as easily as this, for two reasons. The first is that if he fails to define of the domain to which his thesis applies, it loses content. His thesis about neoclassical economics being characterized by a particular research strategy may be true simply because that is how neoclassical economics is defined. The second, and much more important, reason is that the power of Hausman's thesis depends on its characterizing (albeit with exceptions) *economics as a whole.* It is this that gives his charge of methodological narrowness its significance.

Hausman could have greatly strengthened his argument by showing how equilibrium theory pervaded other fields of economics. Such a claim would, to many economists, have appeared uncontroversial. The story of post-war macroeconomics, for example, can be told in terms of the progressive extension of optimizing models to explain more and more phenomena that had previously been explained using *ad*

[6] Consider the example of the P-bet and the $-bet given above, and suppose a subject values the P-bet at $7 and the $-bet at $8, but prefers the P-bet to the S-bet. The subject will be willing to undertake the following series of transactions.

1 Purchase the $-bet for $7.95.
2 Exchange the $-bet for the P-bet.
3 Sell the P-bet for $7.05.

The net result is that the subject has lost 80¢ and gained nothing. Such behaviour can be found under experimental conditions, though most subjects cease to exhibit it when it is explained to them what is happening.

[7] Cf. the definitions offered by Menger (1871) and Robbins (1932).

hoc assumptions (such as Keynes's consumption function). In development economics, once seen as a subject separate from the economics of industrial countries, rational choice models have, since the 1960s, been more and more widely used. Similar remarks can be made about industrial economics since the 1970s. The list is endless. Hausman has brilliantly characterized the type of economics that has increasingly dominated the subject. The imperialist ambitions of neoclassical economics appear to be unbounded.

Despite all this, however, there are grounds for unease. For all the extension of neoclassical theory, there is much that does not fit. There are too many recalcitrant empirical facts. Keynesian economics, for example, has long resisted assimilation into the neoclassical scheme. New classical explanations of unemployment as voluntary were, for many economists, simply implausible. Even now, with the proliferation of 'new Keynesian' explanations of unemployment, the subject remains wide open. Thus whilst economists have wished to construct economics along the lines Hausman describes, they have not been able to do so.

The main reason for unease is that much (most?) economics involves empirical work. This includes not only formal econometrics, but the analysis of statistical data and information on institutions. It is debatable how far equilibrium theory is really central to such work. Weintraub (1988) made a bold attempt to argue that it was,[8] but his argument is not conclusive. In econometric work, estimated equations are frequently very different from the equations suggested by pure theory – data availability, the requirements of available estimation techniques and the 'inexactness' (in the Mill–Hausman sense) of theories mean that the results are driven by data as much as by theory.[9] Applied economics frequently has to take account of factors about which theory has little to say. Whatever economists might wish, the relevance of equilibrium theory to such work is problematic.

By adopting such a narrow definition of the subject, Hausman has provided a distorted picture of economics. He is, of course, not alone in this. Many histories of economics are centred, somewhat Whiggishly, on the theory of value, an approach which leads to exactly the same view of economics as Hausman is offering. Schumpeter (1954, p. 309) was probably typical in holding the theory of price to be

[8] He argued in Lakatosian terms, but as pointed out in note 5, his perspective is not as different from Hausman's as this would imply.
[9] This is not, of course, to say that results are robust, or predictively successful. Random data-mining with a stopping rule determined by test statistics is data-driven, but does not produce robust results.

but another name for economic logic, thus justifying its central place in his history. The history of economic thought can, however, be told rather differently, with problems and their solutions replacing the theory of value as the unifying principle.[10] Two consequences of the standard perspective, shared by Hausman, are that the role of equilibrium theory is maximized and, more important, problems of empirical evidence and the testing of economic theories are minimized.

6 EMPIRICAL METHODOLOGY AND DEFENDING THE STATUS QUO

Hausman is emphatically *not* a defender of the status quo in economics – his criticisms of the attempt to keep economics separate from other social sciences and his plea for more investment in data are too strong for that. However, two aspects of his work tend (in Mill's sense) to support the status quo. The first is the imprecision of his modified Millian methodology. The second is his claim that the failings of economics do not arise from the adoption of an inappropriate methodology.

If the goal of economic enquiry is seen as the construction of a separate science of an economic realm, an *a priori* method is clearly appropriate. Defining an economic realm provides a starting point for analysis through defining a set of economic phenomena to which logic can be derived. This is the approach of Menger (1871) and Robbins (1932) amongst others. However, Hausman advises economists to abandon this conception of economic science. The consequence is that the inexact *a priori* method needs to be reconsidered in the light of what economics is meant to be doing. To see the significance of this we need to consider Hausman's modified Millian method in more detail.

The two main modifications Hausman makes to Mill's inexact *a priori* method are (a) that the starting point is 'credible' and 'pragmatically convenient' generalizations, and (b) that in the event of predictive failure alternative accounts should be compared 'on the basis of explanatory success, empirical progress, and pragmatic usefulness' (1992a, p. 222). The main characteristic of these criteria is that they are imprecise and, more significant, highly dependent on prior beliefs. Take, for example, the theory that unemployment is caused by workers' deciding to take more leisure now on the grounds that they expect the real wage (which measures

[10] For example, Rostow (1990), Backhouse (1994b).

the cost of leisure) to be higher in the future.[11] To someone committed to the assumptions of rationality and competitive markets, the theory's assumptions are plausible, and it provides a satisfactory explanation of unemployment. In contrast, fix-price disequilibrium models in which people are unemployed because they cannot find work, whatever the wage they are prepared to accept, are seen by such economists as being based on assumptions that are not credible, and as providing no explanation of unemployment. Hausman's other criteria of 'pragmatic convenience' and 'pragmatic usefulness' depend even more crucially on the aims of enquiry.

One way to harden Hausman's inexact *a priori* method would be to relate explanation to prediction (for example, in the manner of the covering law model), and to define the pragmatically useful in terms of usefulness in making predictions (on the grounds, perhaps, that prediction is required by policy makers). Predictive success could then be used as a criterion by which to appraise economic theories.[12] Such an approach would be consistent with Hausman's critique of economists' commitment to the separateness of their science, but it would fit less well with his critical attitude towards falsificationism. Thus he does not follow this route.

The consequence of Hausman's 'soft' interpretation of explanatory success is the implication that, whether he intends this or not, it results in a tendency for theory appraisal to reflect values prevailing in the discipline. The status quo is privileged. We thus have the excessive focus on the core of neoclassical microeconomics and general equilibrium theory. Hausman's failure to adopt 'harder' appraisal criteria also means that his criticisms of economics are in a sense hanging in the air, for they do not follow from a methodological critique.

7 CHANGING THE AGENDA FOR ECONOMIC METHODOLOGY

Since the publication of Hutchison's *The Significance and Basic Postulates of Economic Theory* (1938), falsificationism in various forms has dominated discussions of economic methodology. Hutchison drew on logical positivism; Friedman (arguably) on Dewey's pragmatism; Samuelson on Bridgman's operationalism; Lipsey and his colleagues at LSE in the 1960s on Popper; and more recent scholars on Popper and Lakatos. Hausman is critical of all brands of falsificationism, arguing

[11] This is chosen as an example of a theory that commands widespread support amongst economists, incredible though the theory sounds to most non-economists.

that it is philosophically flawed, that it is inconsistent with what economists do, and that it is unworkable. Economic methodologists such as Blaug (1980/92) who urge economists to take falsificationism more seriously are, Hausman contends, mistaken. Kuhn, he claims, has more to offer than Lakatos.[13]

Hausman wishes to change the agenda facing workers in the field of economic methodology away from falsificationism and the set of questions addressed by Popper and Lakatos. Thus he has sought to address issues such as causality, explanatory progress and theory structure. Broadening the agenda in this way is an important goal, probably shared by most of those currently working on economic methodology.[14] Beyond this, however, things are more controversial.

Hausman has succeeded, brilliantly, in characterizing contemporary neoclassical economics. To a great extent this has involved re-discovering the insights of Robbins, Schumpeter and others, but his presentation and analysis of these ideas has taken debates about the nature of economics a step further. However, what he has created is a philosophy of economic *theory,* not economics as a whole. More important, he has not, I contend, gone sufficiently far in deriving a normative philosophy that can be used critically. The Mill-inspired inexact deductive method fits well with the theoretical strategy of building a separate science of a purely economic domain, but serves much less well as a foundation for the criticisms of economics that Hausman wishes to make. If, instead, we start by accepting his criticisms of the theoretical strategy of neoclassical economics, we are led naturally into the notion that the failings of economics *may,* contrary to what Hausman argues, be the result of economists' following an inappropriate, insufficiently empirical, methodology.[15] It may be that falsificationism of whatever variety cannot provide the basis for such a critical position,[16] but we should none the less be careful before endorsing an alternative that serves, even if unintentionally, to defend the status quo.

[12] See, for example, Rosenberg (1992).

[13] In addition to the books under review, see Hausman (1994).

[14] See, for example, Mäki's (1990) critique of what he terms 'the Popperian dominance'.

[15] An interesting comparison is with Rosenberg (1992). Like Hausman, Rosenberg is also critical of Popper and Lakatos. They also see the theoretical strategy of contemporary neoclassical economics in terms that are not too dissimilar. Where they part company is that Rosenberg still regards prediction as an important scientific goal, with the result that he has a much stronger basis on which to construct a critique of economists' commitment to rational choice models than does Hausman.

[16] Though a position very close to Hausman's can, as was suggested in note 5, arguably be reached equally well from a Lakatosian starting point.

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Chapter 17

An 'inexact' philosophy of economics?*

(*Economics and Philosophy* 13(1), 1997, pp. 25–37.)

This chapter develops the two criticisms of Hausman's Inexact and Separate Science of Economics *that were sketched in Chapter 16. It suggests that though his empirical approach to economic methodology and his overall thesis are right, the book's distance from Lakatos is in practice less than his explicit criticisms would suggest. More important than this is the criticism that Hausman has paid too much attention to economic theory, and too little to econometrics and economists' empirical practices, the only sustained treatment of which is his discussion of experimental economics and the problem of preference reversal. The reason why this matters is that when he turns to criticism, a significant fraction of his proposals are concern the empirical side of the discipline.*

1 INTRODUCTION

The Inexact and Separate Science of Economics (ISSE) (Hausman, 1992) represents the most ambitious attempt to provide a systematic account of economic methodology since the first edition of Blaug's *The Methodology of Economics* (1980/92). As such, it has been the subject of extensive critical commentary (for example, Chapter 16 above; Blaug, 1992; Miller, 1996; Hahn, 1996; Mäki, 1996). For all the attention it has received, however, some important aspects of the book's thesis have not been developed properly. Two important ones are (1) what might be called, following the terminology used in the experimental economics literature, the

* I am indebted to Philippe Mongin and Denis O'Brien for invaluable detailed comments on earlier drafts of this chapter. Neither should be held responsible for the use I have made of their advice.

'framing effect' of Hausman's definition of economics,[1] and (2) the significance of Hausman's claim that economists are committed to developing economics as a 'separate' science. To understand these points it is important to make explicit the position from which Hausman approaches the philosophy of science.

2 ISSE AS EMPIRICAL PHILOSOPHY OF SCIENCE

One key to understanding ISSE is that Hausman is engaging in what he would term 'empirical philosophy of science'. The most succinct statement of his approach is to be found, not in ISSE, but in an article written a decade earlier, 'How to do philosophy of economics'.

> The credo of the empirical approach may be stated trenchantly and simplistically as follows:
>
> The philosophy of science is itself an empirical science.
>
> All conclusions about the scientific enterprise that the philosopher of science draws are, or should be, scientific conclusions and must be defended in the same way or ways that the results of the sciences are defended. When the philosopher of science makes pronouncements about the goals of science or the basis or bases upon which scientists accept various theories or about any other feature of science, we should regard these pronouncements as scientific claims and assess them as we would assess the various assertions the sciences make.
>
> (Hausman, 1980/92, p. 221)

He argued that such an approach should be applied to economics, even though there are greater obstacles in the way of deriving normative conclusions than there are in physics – there is less agreement on what can uncontroversially be regarded as 'good science'.[2]

3 THE FRAMING EFFECT

On the opening page of ISSE, Hausman defines his subject matter:

[1] This point is made, but too concisely for its significance to be apparent, in Chapter 16. It is the basis for the title of that chapter, 'An empirical philosophy *of economic theory*' (emphasis added).

[2] Perhaps because he felt that it could, by 1992, be taken for granted, or because the point had already been made, there is little explicit discussion of this in ISSE, though, significantly, the last sentence of the book reads, 'One must address the problems of economic methodology by studying economics' (p. 329).

> This book will be concerned only with contemporary microeconomic theory and general equilibrium theory. These theories are the best known of economic theories, the theories that have most influenced work in the other social sciences, and the theories which have been most discussed by philosophers, economists, and other social scientists.
>
> (ISSE, p. 1)

The book deals only with ‘neoclassical’ economics, or ‘equilibrium theory’, where this is defined in the following way.

> It consists in my view of seven laws: those of the theory of consumer choice, those of the theory of the firm, and the assertion that markets ‘clear’ or come quickly to equilibrium.
>
> (ibid., p. 51)

The laws that make up the theory of consumer choice are rationality,[3] consumerism[4] and diminishing marginal rates of substitution. Those making up the theory of the firm are constant returns to scale, positive but diminishing marginal products, and profit maximization. Individual optimization, a particular form of rationality, is thus a part of Hausman’s definition of equilibrium theory.

For most of the book, however, Hausman drops the adjective ‘neoclassical’, describing this as no more than a convenience.

> *Indeed, to avoid unnecessary repetition, I shall usually omit the adjective ‘neoclassical’ and just speak of ‘economics’ when I am discussing neoclassical economics.* This is merely a convenience, not a covert attempt to denigrate other schools of economics, or to define them out of existence.
>
> (ibid., p. 3; emphasis in original)

With these remarks, he minimizes the importance of his choice of subject matter, for most readers will take these omitted schools to be Austrian economics, Post Keynesian economics, Marxist economics, institutionalism, and so on. Though it would be desirable to say something about these as well, it is clear that to do so would be a distraction. The focus on neoclassical economics is made to appear

[3] Rationality can be broken down into its components – completeness, continuity and transitivity of preferences, plus utility maximization. If this is done, equilibrium theory comprises ten laws rather than seven.

[4] Individuals preferences relate only to their own consumption, different individuals’ preferences are not interdependent, and up to some point individuals prefer larger commodity bundles to smaller ones.

uncontentious. On the other hand, had he said he was ignoring macroeconomics, development economics, the provision of policy advice, and most empirical work in economics, except in so far as these rest on foundations of equilibrium theory, questions might immediately have been asked about the grounds for his choice.

What reasons does Hausman give for confining attention to neoclassical microeconomics? In the passage just quoted, three are provided: (1) it is well known; (2) it has influenced *non-economist* social scientists; (3) philosophers have paid more attention to it than to other parts of economics. These, however, cannot be the real reasons, for once presented in this way, they are clearly inadequate. The real reason must be the belief that equilibrium theory is either the most important part of economics (the justification, perhaps, for ignoring heterodoxies), or that it is the foundation for those parts of economics that he does not discuss. This case might be argued, but Hausman fails to tackle it systematically. In the remainder of this section I wish to argue that though such a case can be presented, it is not the whole story. In other words, Hausman's methodology is 'inexact' in the sense that it takes account of only *one* of many strands in contemporary economics. That, to use his Millian terminology, there are other, disturbing, causes to be taken into account.[5] Furthermore, as will be argued in the concluding section, even if Hausman has isolated the 'major' element in contemporary economics, the omission of other strands causes very serious problems for the appraisal of economics that Hausman wishes to make.

The evidence that neoclassical microeconomic theory, which I will, following Hausman, refer to as 'equilibrium theory', is *one* of the fundamental elements in contemporary economics is overwhelming. Provided that we define it sufficiently broadly, emphasizing rationality and equilibrium rather than profit-maximization and perfect competition, allowing for game theory, incomplete markets, limited information and the like, equilibrium theory has increasingly provided the organizing principle for much of modern economics. It is the basis for 'economic imperialism' – the encroachment of economics into spheres previously thought of as lying outside its domain. Macroeconomics, since the advent of the new classical macroeconomics

[5] Hausman admits as much, not simply in the passage quoted above, but in passing remarks such as, after arguing that the consumption-loan model should be seen as equilibrium economics, 'This is, to be sure, only one sort of economics – indeed only one sort of orthodox neoclassical economics, which consists of a great variety of different kinds of work' (ISSE, p. 119). Or, 'Economics is a diverse enterprise, ... My focus is only on theoretical economics' (ibid., p. 255). He does not, however, pursue the implications of these remarks.

in the 1970s, has become so dominated by equilibrium theorizing that some economists cannot conceive any other way to understand the causes of macroeconomic phenomena. Development economics, once a much more distinctive field, containing 'grand theories' of its own, has now succumbed to equilibrium analysis, losing much of its distinctiveness (see Krugman, 1995). 'Applied economics' is routinely (perhaps too routinely, some might argue) seen as applying equilibrium analysis to various economic problems, whether concerning the labour market, innovation, finance, international trade, and so on.

Why, then, do I quibble with Hausman's confining attention to equilibrium theory?

(1) The dominance of equilibrium theory is very recent. It is customary to think of equilibrium theory as going back at least to Jevons and Walras – to the early 1870s. Yet this is an exaggeration. Marshallian economics, which dominated much of the profession till the 1920s, was not purely equilibrium theory. Equilibrium theorizing was used, but it was only part of the story. It was not till the late 1920s, with economists such as Robbins, Joan Robinson, Lange, Hicks, Samuelson and others, that equilibrium theorizing began to be the dominant type of theorizing.[6] In international economics the change, described by Krugman as the 'rout of institutional economists by modelers' (1994, p: 275), did not take place till the 1940s. In many areas (macroeconomics, development, industrial economics) the changes is even more recent (since the mid-1970s). Because the dominance of equilibrium theory is so recent, earlier approaches still survive in many areas of the discipline.

(2) Even where equilibrium theorizing is important, the dynamics of the discipline are determined by the interaction of equilibrium theory with empirical problems. For example, economists have sought, since at least the 1930s, to reconcile unemployment with equilibrium theory. Yet to understand the way in which economists chose to modify the standard theory, it is necessary to take into account the empirical problems with which they were dealing, such as the breakdown in the Phillips curve in the 1970s or the persistence of high unemployment rates in Europe during the 1980s.

(3) Much work still ignores equilibrium theory, either ignoring formal theory, or using only very basic theory,[7] such as opportunity cost, the concept of the margin, externalities or cost-benefit analysis.[8] Measurement problems frequently dominate other issues, with the need to use fairly crude proxy variables rendering theoretical

[6] See, for example, Backhouse (1985, chapter 14, especially pp. 140–2) and Mongin (1992).
[7] In part, of course, this is the other side of point (1) above.
[8] Cf. Rhoads (1985).

subtleties irrelevant. In other areas, historical research is the only available method. Sometimes equilibrium theory has nothing to say about an important problem. For example, it has nothing to say about whether the tight monetary and fiscal policies pursued under Mrs Thatcher's government after 1979 and the resulting high unemployment were a necessary price to be paid for higher productivity growth, or about the supposed dynamic effects on productivity of closer integration within the European Union.

(4) Even where equilibrium theory is the language in which theorizing is undertaken, its centrality is sometimes very problematic. in the sense that most, if not all, the results that are important for empirical work and for understanding what is going on in the world, can be derived without it.[9] The main reason is the gulf that frequently exists between theoretical and empirical models. As an example, take the inter-temporal optimization and life-cycle model of consumption, which underlie the overlapping-generations model discussed by Hausman (ISSE, chapter 7) and much contemporary economic theory. This has become a standard framework within which to think about consumption and saving. Yet how important is it when it comes to empirical models of consumption? To model consumption, for example, in the UK during the 1980s, it is arguable that factors about which life-cycle theory is silent are as important as those on which it has something to say.[10] Financial deregulation, links between saving and the housing market, and expectations of inflation and unemployment all have to be taken into account. At a deeper level, our understanding of income, consumption and saving depends as much, if not more, on measurement techniques – on National Income accounting techniques – which owe nothing to equilibrium theory. Though models of inter-temporal optimization may provide an incredibly elegant way to analyse consumption and saving behaviour, much could be done with simpler, more *ad hoc* theories (one does not need a model of inter-temporal optimization to work out that working households will typically save more than retired ones). It might even be argued that, far from providing additional insights, models of inter-temporal optimization have, by

[9] The problems are virtually the same as those facing Weintraub when he sought to argue that general equilibrium analysis (the hard core of his Lakatosian research programme) was fundamental to developments in other fields (in the protective belt). Though he sought to make his case with a detailed case study (Weintraub, 1988) few people appear to have been convinced and central to such arguments is the attitude of the relevant scientific community.

[10] Hausman goes so far as to say of Samuelson's consumption-loan model, 'These models do not answer empirical questions, and it is hard to see how they could do so' (ISSE, p. 255).

assuming a representative agent, *distracted* attention from possibly more important issues such as aggregation problems.

To take another example, it is not clear that the rigorous application of equilibrium theory to the business cycle has increased our knowledge of the phenomenon beyond what had been learned through generalizations from statistical evidence (such as the tendency of some sectors to exhibit more pronounced cycles than others, or the failure of real wages to show any marked cyclical pattern) guided by relatively simple theories (such as the accelerator, or the transactions demand for money). The clearest examples of concepts that it is hard to imagine having arisen without a commitment to theories based on intertemporal optimization are Ricardian equivalence and the dependence of aggregate supply on the interest rate via workers' intertemporal substitution between current and future leisure. The evidence for these propositions is, however, at best, inconclusive.[11]

Finance might be cited as an area where equilibrium theory has been central, and where great progress has been made. Here, however, the central concepts are efficient markets and arbitrage. These are quite compatible with equilibrium theory – with rationality and equilibrium – yet they do not require it. Some of the most successful developments in the field of finance, such as the Black–Scholes option pricing theory, are based on the law of one price, or the no-arbitrage condition. To quote Stephen Ross, a leading finance theorist, 'To make a parrot into a learned financial economist, he only needs to learn the single word "arbitrage"' (Ross, 1987a, p. 30).[12] The efficient market hypothesis is not derived from rigorous microeconomic foundations – indeed the task of deriving it is very difficult, 'the principal difficulty' being, according to Ross, 'that models with fully rational investors tend to break down' (Ross, 1987b, p. 7). The notion of rationality in Hausman's characterization of equilibrium theory, and hence of economics, cannot encompass rationality as it

[11] Seater (1993) surveys the literature on Ricardian equivalence (RE). Though he concludes that RE holds 'as a close approximation' (p. 160) it is hard to avoid reading the evidence he presents as inconclusive. Even if one were to accept his conclusion that aggregate consumption data 'almost always fail to reject' RE (p. 174), his survey reveals major problems with the theory. (1) Indirect evidence (such as microeconomic evidence that policy announcements do not cause people to change their spending as RE predicts they should) provides evidence for 'non-negligible inadequacies' in the life-cycle model on which RE is based. (2) There are highly plausible alternative explanations of the failure of aggregate consumption data to reject RE.

[12] See the discussion in Campbell (1994), where Summers is also quoted making the same point. One response, of course, is simply to deny that finance should be regarded as a part of economics (Ross is sympathetic to this line), but this would, for Hausman, be a dangerous strategy. His thesis would become true by excluding the exceptions.

is understood in the finance literature (i.e. as taking account of all available information).

The assumptions of rationality and equilibrium have been fundamental to recent developments in industrial economics, where the application of game theory has transformed the subject since the 1970s. And yet, though theory has been a fertile source of insights into mechanisms that *might* be operating, it has provided precious little guidance for empirical work (Fisher, 1989). The range of possible outcomes is too large (multiple solutions are a common problem) and too sensitive to assumptions which are essentially arbitrary (such as the precise order of moves in a game, or the type of solution concept used).

The reason why these points are significant is not that Hausman is wrong to argue that equilibrium theory is very important in contemporary economics – it is clearly fundamental to much of the most prominent work in the subject. It is that by defining economics as neoclassical microeconomics (with some incursions into related macro), he is biasing his results. The broader his definition of economics, the greater the bias. To claim that equilibrium theory exercises a hegemony within neoclassical microeconomics (without an independent definition of neoclassical) is a much weaker claim than that it is hegemonic within economics as a whole. Had Hausman started, instead, with a series of applied (perhaps policy) problems (perhaps research and development (R&D) policy, European integration, the business cycle, tax reform, the design of benefits), starting with the theories and evidence that is brought to bear on these problems, he might have come up with a very different characterization of economics. Sure enough, equilibrium theory would have been there (the optimal tax literature is pure equilibrium theory) but other types of analysis would have been brought in as well. Hausman would at least have had to make the case that equilibrium theory was the primary cause, and the other arguments the disturbing ones, rather than letting it go by default.

4 SEPARATENESS, PROGRESS AND THE APPRAISAL OF ECONOMICS

The idea that the theoretical strategy of economics (as understood in ISSE) is driven by the attempt to keep economics a 'separate' science is a brilliant way of characterizing the attitude of economists to other social sciences. It is the type of insight that, even if one regards it only as a fact to be explained, more than justifies doing empirical philosophy of science. For Hausman, however, the notion of separateness as an objective does much more than this. It is the basis for his critique of contemporary economics. As was the case for Mill (see Blaug, 1980/92), treating economics as an inexact science is inevitably conservative, rendering it easy to defend the theory, which becomes irrefutable.[13] It is Hausman’s belief that the strategy of separateness is indefensible that is the basis for his powerful, and to my mind convincing, critique of economics in chapters 14 and 15 of ISSE.

And yet, there are problems. Hausman’s case against separateness is an empirical one: ‘Given the limited predictive power of equilibrium theory, there should be no presumption that alternative theories can be dismissed on general methodological grounds’ (ISSE, p. 253). He is judging economics on the basis of its predictive power and finding it wanting. Hausman’s presumption is that, unless evidence can be produced to the contrary, diversity is good: ‘There is absolutely no reason why all economists should employ the same styles and strategies of reasoning’ (ibid., p. 255). Though the goal is prediction, the means by which predictions are obtained and improved may be diverse and cannot be captured within definite methodological rules.[14]

His response is a detailed list of proposals, four concerned with data, three with the use of theory: (1) to commit more resources to experimental economics; (2) to make greater use of observational data of all types; (3) to engage more actively in the process of data gathering; (4) further work on improved statistical techniques for data analysis; (5) work on alternatives to standard choice theory; (6) paying more attention to other social scientists; (7) encouraging different styles of theorizing, such as that ‘exemplified by the institutionalists’ (ISSE, p. 254). But though I have no difficulty in accepting virtually all these recommendations, I am not convinced that they follow from Hausman’s analysis of economics. He has examples, notably Akerlof’s apparently successful drawing on sociology (ibid., pp. 257–62), that suggest that cooperation with other disciplines would yield fruit, but these

[13] Hausman (1997) has since tried to put some teeth into his account of the methods of confirmation that economists employ.
[14] Stage 4 in Hausman’s version of the inexact deductive method – ‘*compare* alternative accounts of the failure on the basis of explanatory success, empirical progress, and pragmatic usefulness’ (ISSE, p. 222; emphasis in original) – is too vague to count as a methodological rule.

conclusions can stand up independently of his more general arguments about the nature of the discipline.[15]

To establish the first four of these points he should have considered the *empirical* practices of economics in more detail, to see how they relate to the objectives economists are trying to achieve. Without such enquiries it is hard to be confident that the marginal returns to such activities are greater than the marginal returns to what economists are currently doing. (1) Many economists, including ones with no commitment to equilibrium theory, are much more sceptical about whether the data produced by experimental economics are of any value in interpreting behaviour outside the laboratory than Hausman appears to be, certainly when it comes to applying it to fields such as industrial organization.[16] (2) Economists do already use a variety of data, ranging from descriptions of institutions to accounting data produced by governments.[17] What evidence is there that the supply of such data is constrained by resources (whether intellectual or financial) rather than by the inability of economists to be sufficiently imaginative in working out what new data would be worth collecting?[18] (3) Becoming involved in the data-gathering process has large opportunity costs, and some case studies of instances where it has happened (contrasted with ones where it has not) would be useful. (4) Improved statistical techniques are already being developed rapidly, and on an enormous scale, by econometric theorists. It is far from obvious that lack of suitable statistical techniques is a significant barrier, or that this is an activity where resources are scarce. There are clear instances of economists ignoring much potentially relevant empirical evidence, suggesting that a major part of the problem lies not with the availability of such evidence, but with the unwillingness of economists to use it.[19] In contrast, Hausman's case for his theoretical prescriptions is much more strongly

[15] It is in this vein that Blaug (1992, p. 6) wrote, 'Hausman's closing chapters are filled with cogent proposals to improve economics. ... All this is music to my ear. What a pity these conclusions do not follow from the previous chapters expounding the problem of theory assessment in economics.'

[16] O'Brien (1994, p. xiii) has remarked, 'industrial economics ... has fallen to game theory, in which, to put it unkindly, the difficulties of confrontation with real world data are such that practitioners are forced to make up their own data under the guise of "experimental" economics'.

[17] An example of imaginatively using a variety of data sources is Diamond (1994), discussed in detail in Backhouse (1997).

[18] This question could be answered, to some extent, by pointing to the numerous problems with official statistics that have resulted from the British government's attempts to reduce expenditures (continually changing definitions of unemployment, the lack of continuous earnings series, and so on). But my point is that Hausman does not discuss the question, not that his conclusion is wrong.

made, a crucial element reason being the examples he is able to cite. A possible exception is his appeal for greater regard to 'institutionalism' (does he mean the old or the new institutionalism?), which he fails to support with detailed examples. It is hard to believe that the predictive track record of institutionalism (and prediction is, at bottom, Hausman's appraisal criterion) is any better, if as good, as that of neoclassical economics.

This is where the 'framing effect' and Hausman's argument about the separateness of economics as it is currently practised come together. Had Hausman not focused purely on neoclassical microeconomics, considering the empirical practices of economists in more detail, not only might he have come up with a very different portrait of the discipline, but he might have had to soften, slightly, his view on separateness. Economics would have seemed less pure.

5 HAUSMAN AND FALSIFICATIONISM

Hausman criticizes economics from the point of view of an empiricist, committed to the view that both science and economic methodology are empirical endeavours. In this attitude he is close to the falsificationists of whom he is so critical. In Blaug's hands, for example, falsificationism, whether Popperian or Lakatosian (he slips between the two), is mainly a way of packaging a tough-minded empiricism (cf. Blaug, 1994, especially p. 131). Rather than emphasizing those aspects of Popper and Lakatos with which he agrees (perhaps because they are, to a philosopher, not the most original aspects of those philosophers' work) he criticizes them ruthlessly, thereby covering up his substantial agreement with methodologists such as Blaug.

To see this, consider the end of his chapter on Lakatos, where Hausman points out several points on which he believes Popper and Lakatos to have been correct (ISSE, p. 203).

1. The importance to science of empirical criticism and of theories being open to empirical criticism.
2. The most important evidence in support of scientific theories comes from hard tests and similar explanatory achievements.
3. Scientific knowledge is corrigible, and theories may have to be abandoned.
4. The importance of heuristics in the development of science.

[19] Mongin (1992) provides one illustration of this.

5. Epistemology should be concerned with *changes* in knowledge.

To this he might have added Lakatos's espousal of empirical philosophy of science (in the methodology of *historical* research programmes) and Lakatos's facing up to the tensions implicit in drawing normative Conclusions on the basis of an empirical philosophy of science. Though he rejects this, at one point he even asks whether his critique of Popper and Lakatos may be 'just semantics' (ibid., p. 203). Rather than adopt an overwhelmingly negative stance, therefore, Hausman could easily have presented falsificationism as an imperfect methodology from which important lessons could none the less be drawn.[20] He provides two reasons why he chose not to do this. The first is that he regards the doctrine as flawed, and the second is that to correct these flaws (for example by acknowledging a modicum of justificationism) would 'eviscerate' their philosophies (ibid., p. 204).

Compare this with the way Hausman treats Mill's methodology. He achieves rigour in his characterization of economics as following an inexact deductive method only through weakening the criteria by which predictive failure is judged, making his methodology so elastic as to provide a very weak basis for mounting a critique of economics. Recall that Mill's inexact deductive method involved testing propositions deduced from a set of *proven* laws, and that in the event of predictive failure, the scientist had to judge, not whether the laws were correct, but simply (1) what sort of interferences occurred, (2) how central the laws were, and (3) whether the set of laws should be expanded or contracted.[21] This is a very clear-cut and arguably dangerous philosophy.[22] Hausman modifies it *very* significantly when he argues that the starting point is not proven laws, but 'credible ... and pragmatically convenient generalizations', and that in the event of predictive failure the scientist should '*compare* alternative accounts of the failure on the basis of explanatory success, empirical progress and pragmatic usefulness' (ibid., p. 222, emphasis in original). This makes the method so elastic that it fits much of what economists do, and it becomes unobjectionable from a normative standpoint. But, even if Hausman has reached a methodology that fits modern economics well, one might wonder whether Hausman has not 'eviscerated' Mill in precisely the way that he objected

[20] This is the perspective adopted in Chapter 5 above.
[21] See the table in ISSE, p. 222. I leave aside the clear-cut case of the scientist discovering a mistake in the deductions.
[22] See Blaug (1992).

to doing with Popper and Lakatos.[23] There are technical problems with both Popperian and Lakatosian falsificationism, and there are also problems with the way falsificationism has been applied to economics. It is, however, arguable that the case Blaug makes for greater falsificationism in economics is as well supported, especially with case studies, as the case Hausman makes for the changes in economists' practices that he supports.

6 CONCLUSIONS

Hausman has offered an outstanding characterization of the most dominant, most prestigious type of academic economics being undertaken at the moment, the influence of which is felt throughout the discipline. To this extent his main thesis about economics is correct. His methodology is rigorous and captures much of what is going on in contemporary economics. Because of this it is undoubtedly an important contribution to our understanding of the discipline. Hausman's view of economic methodology, however, is inexact. Partly because of his choice of subject matter, he neglects (as he would probably be the first to admit) many other aspects of the discipline, particularly many aspects of economists' empirical practices.[24] In other words, his portrait of economics is incomplete. This matters for two reasons. The first is that Hausman wishes to provide a methodological critique of economics from an empiricist standpoint. This aim requires that he pay attention to those aspects of the discipline he has neglected, notably economists' empirical practices. The second reason why the inexactness of Hausman's portrait matters is that it causes problems for his characterization of economics. If he defines economics as those fields dominated by equilibrium theory, his argument about the dominance of equilibrium theory is vacuous, and the argument about separateness loses much of its force. If, on the other hand, if he broadens his definition of the economics, then the broader is his definition, the greater the importance of the 'disturbing causes' that he has neglected, and the weaker the evidence for his characterization of the subject.

[23] It is also worth noting that some of the issues that arise in connection with Hausman's characterization of economics also arose when the subject was viewed from a Lakatosian perspective. The claim that equilibrium theory is central to economics bears a family resemblance to Weintraub's claim that economics can be seen as a neo-Walrasian research programme.
[24] The big exception is his brilliant discussion of experimental economics (ISSE, chapter 13).

Hausman criticizes economists for their commitment to keeping economics a separate science. In other words, he is criticizing economists for their commitment to equilibrium theory. It can be argued that had economists not paid so much attention to equilibrium theory, they might have been able to offer a wider range of insights into some of the most fundamental economic problems, such as, 'Why some countries grow faster than others?' or 'What is the appropriate mix between public and private enterprise?' However, in succumbing to the fascination of equilibrium theory, Hausman has been led into neglecting some of the most important aspects of economists' empirical practices, as a result of which his normative conclusions are left hanging in the air. Hausman appears to have fallen into the same trap as the profession whose practices he is seeking to analyse.[25]

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[25] He is, of course, not alone in this. Historians of economic thought, for example, usually focus on the theory of value and distribution, which has a profound effect on the way the subject looks (see Backhouse, 1994).

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Chapter 18

Philosophical foundations of the social sciences*

(*History of Economic Thought Newsletter,* 1996, pp. 23–6.)

Kincaid, like Hausman (1992), adopts an empirical approach to the philosophy of science, supporting his arguments with detailed case studies. Unfortunately, however, his main examples are all taken from social sciences other than economics. He has failed to take his empirical approach far enough. This chapter argues that, though Philosophical Foundations of the Social Sciences *contains many insights, Kincaid, like Hausman, pays insufficient attention to empirical progress in economics.*

This book defends the theses of naturalism (the claim that the methods of the natural sciences can also be applied to the social sciences) and holism (the claim that explanations of what goes on in society have to involve more than simply concepts relating to individuals) in social science. Kincaid summarizes his thesis in the following way:

> After removing conceptual obstacles [*a priori* arguments that no science of society is possible], I defend a more interesting and controversial thesis: that specific pieces of social research meet basic standards of scientific adequacy and/or support the holist conception. The moral is that the only obstacles to a science of society are practical and eliminable ones.
>
> (p. 9)

Removing conceptual obstacles starts with a discussion of Quine and the demise of positivism, a discussion of the varieties of scientific rationality and Thomas

* A review of Kincaid (1996).

Kuhn. After this it turns to ‘social constructivism and postmodernist rhetoric’. However, whilst Kincaid is opposed to foundationalism – ‘the idea that philosophers can describe on *a priori* grounds the standards for real scientific knowledge’ (p. 20) – he is equally opposed to what he calls ‘currently trendy forms of irrationalism’ (p. 8) such as those of Bloor, Rorty and McCloskey. He finds a route between these extremes through empirical philosophy of science. This involves drawing conclusions about what makes for good science from observing good science: ‘we learn the methods of good science from experience’ (p. 43).

For the social sciences this process involves two stages. The first is to draw, on the basis of our experience of natural science, conclusions about what makes for good science. Kincaid’s list of evidential virtues includes: falsifiability, empirical adequacy, scope, coherence, fruitfulness and objectivity. To achieve these virtues, science requires ‘fair tests, independent tests and cross tests’ (pp. 50–1). However, whilst such a view might seem uncomfortably close to the foundationalism he rejects, Kincaid argues that these criteria will seldom resolve scientific disputes on their own. They admit of multiple interpretations, they cannot be measured and offset against each other, and they are simplifications that apply only *ceteris paribus.* The second stage is to argue, on the basis of specific examples, that social research can conform to such scientific standards. He draws, in particular, on two case studies: research on the causes of agrarian political behaviour in particular in developing countries, linking revolutionary activity to the class structure; and research on organizational ecology, seeking to find links between organizational forms and the environment in which organizations operate. He argues that *ceteris paribus* problems are equally significant in biology, and that social scientists deal with them as well as do some biologists.

Kincaid then proceeds to defend functionalism and to criticize individualism. Though economists perhaps ought to be interested in both of these, it is the chapter on individualism that will in practice be of most interest. Here, Kincaid argues convincingly that social explanations cannot be purely individualistic. The qualification ‘purely’ is important here, for he concedes the desirability of being able to relate the behaviour of social wholes to that of the individuals of which they are composed. He then discusses the idea that social science should be a science of interpretation, or hermeneutics.

Philosophical Foundations of the Social Sciences is a very useful contribution to the debate on the methodology of social science, with much to say that is relevant to economic methodology. The line he takes between foundationalism and postmodernism is, in my view, basically the right one. I would start from Peirce, Lakatos and even Kuhn rather than from Quine, and I would not wish to place so much emphasis on the concept of naturalism, but I am convinced that Kincaid has reached the correct conclusion. The chapters on causes and *ceteris paribus* explanations, functionalism, individualism and hermeneutics all contain many valuable insights. The concluding chapter contains a very useful discussion of obstacles to good social science.

One criticism of this work is that Kincaid fails to present some of his targets in the best possible light (as his criteria for good science require him to do). The discussion of Kuhn, for example, focuses purely on the first edition of *The Structure of Scientific Revolutions* (1962/70), finding the argument about incommensurability 'implausible'. Perhaps this is why Kuhn 'backed away' from its radical implications when he wrote the Postscript in 1970. Though it may be defensible for Kincaid to take what he wants from the literature, he does not give a balanced reading of Kuhn. Similar remarks could be made about Popper and Lakatos: Kincaid could have made exactly the same points, but presented them as drawing positive conclusions from the work of these currently unfashionable philosophers.

Works on the methodology of social science are frequently perceived by economists as irrelevant to their work. Economics may be social science, but its methods are often seen as very different from those of the other social sciences. Kincaid's discussion of economics means that his book should clearly not be dismissed on those grounds. Economics is taken seriously. And yet, one is left with the feeling that Kincaid's perspective remains one from outside economics. The naturalism thesis is argued on the basis of case studies of good practice, yet all the crucial ones are from other social sciences, not economics. In addition to those mentioned above we have discussions of esquimo hunting behaviour and the Hindu refusal to eat beef. In discussing functionalism, Kincaid fails to make the point, obvious to an economist, that when thinking about purpose, it is vital to distinguish between the purposes of individual agents within the theory, and purposes that are 'read into' social phenomena (perhaps entirely legitimately) by the social scientist. In addition some of Kincaid's conclusions clearly do not apply to economics: it is hard to see economists as failing to produce clearly formulated causal claims or lacking rigorous training in available methods. Maybe most economics students dislike quantitative methods, but that is not true of those who successfully complete a doctorate, the group that is crucial to the subject's evolution. In addition, Kincaid fails to realize that the key figure in GE theory was Walras, not 'Walrus' (Sir James Steuart's name appears also to be wrong, but as all one is provided with is a surname, it may be that Kincaid refers to someone else).

A further reason for thinking that Kincaid's thesis has been developed primarily for other social sciences, with economics being brought in, is his penultimate chapter, not discussed so far: 'Economics: a test case'. In this chapter he treats economics in a way that is very different from the way any other social science has been treated – rather than discuss isolated, successful, case studies, he treats the discipline as a whole. He refers to economics as 'the best of the social sciences' but I was left unclear as to whether he believes this, is describing the views of others, or is using the words ironically.

Kincaid's chapter begins with a discussion of the way economics has been viewed by three economists (Friedman, Roy Weintraub, vintage 1985, and McCloskey) and two philosophers (Rosenberg and Hausman). He points out that the economists defend their enterprise, whereas the philosophers are more critical. The neatness of this observation, however, would have been upset had he included in his list examples of economists who are critical of economics, Blaug being the most important example. All except Hausman, Kincaid claims, 'tend to treat economics as a seamless whole that we can evaluate in one fell swoop', for they 'assume that the highly unrealistic models of equilibrium theory exhaust or best represent modern economics' (p. 231). This is a criticism I would readily endorse (though Kincaid partially excepts Hausman from this criticism, I would be less inclined to do so). However, Kincaid does not, I suggest, take this criticism nearly far enough.

Kincaid's own reading of economics is that the subject is dominated by supply and demand analysis, where supply and demand refer to 'aggregate', market phenomena, not individual behaviour. He has no difficulty in arguing that this is a better characterization of economics than GE (Weintraub and Rosenberg), but is less successful in differentiating his position from Hausman's. Hausman sees equilibrium theory (not general equilibrium) as central to economics, this assuming supply and demand *plus* optimization. But it is no criticism of Hausman's thesis to point out that modern theories of the firm do not assume profit maximization, but optimizing behaviour on the part of the agents that control the firm. Maybe speaking of profit maximization is an illegitimate short-cut, but equilibrium is still fundamental. In addition, though defending the demand curve as a market phenomenon, Kincaid's exposition draws on the Slutsky equation, which is an equilibrium concept.

Kincaid is right in saying that general competitive equilibrium, or even competitive equilibrium, does not exhaust modern economics, and he is right in pointing out that much work in economics requires no more than market supply and demand curves, but he ignores the mass of work that goes beyond supply and demand, whilst retaining the concept of equilibrium. After a list of postulates underlying competitive equilibrium, Kincaid observes that they ‘obviously leave no or little room for mistakes, limited information, non-economic causes, groping towards acceptable solutions, incomplete markets, cheating on contracts, collusion, continuous and rapidly changing economic environments, and so on’ (p. 223). But this observation should have been the cue for observing that these are precisely the issues that concern contemporary economic theorists, and that they tackle them using equilibrium methods, often abandoning supply and demand analysis (which requires price taking behaviour).

It is crucial to Kincaid’s case that economics is empirically successful. He defends this claim by arguing: (1) Supply and demand are central. (2) The theory of demand has been ‘reasonably well-confirmed’ (p. 237). His evidence for this is that Stone estimated demand functions for 48 commodities, 46 of which exhibited negative own-price elasticities of demand, and that these results have repeatedly been replicated and extended. (3) ‘Literally hundreds’ of studies on agricultural goods confirm that price changes cause supply changes. Kincaid points to the inclusion of numerous variables to control for variables that are not constant, arguing that this shows that economics is ‘no threat to naturalism’. I would have been more convinced had he taken one or two studies and examined them in detail, in the way that he did with his other case studies. I have no difficulty in accepting that demand curves generally slope downwards but, as with McCloskey, this belief has little to do with the ability of Stone *et al.* to fit demand systems to the data. Thus whilst I agree with Kincaid, *contra* Rosenberg, that there has been empirical progress in economics, I would have used very different evidence, notably the development of new sources of data such as national income accounting (a point argued recently by Terence Hutchison).

It is impossible to resist pointing out one very enigmatic footnote. In this, Kincaid claims, without providing any support for the claim, that ‘work like Mirowski’s (1989) attempt to track down the influence of physics metaphors on neo-classical economics can contribute much to understanding what a better economics might look like’ (p. 230). For someone who cites estimates of demand systems as demonstrating that economics can meet high scientific standards, this is a surprising claim to make. If only Kincaid had given some hint as to what this better economics might be like!

Though written by an outsider, more at home in social sciences other than economics, and addressing issues that have often been either taken for granted or ignored in economics, Kincaid has valuable points to make about economics and economic methodology. The weaknesses in his argument, I would contend, stem primarily from his tendency to do just what he criticizes others for doing: to treat economics as a seamless whole that can be evaluated in one fell swoop. He resisted the temptation elsewhere in the social sciences. It would have been better to avoid doing so when turning to economics, and to have focused on case studies instead. On the other hand, maybe economics *is* different from other social sciences in being amenable to such treatment, but that would raise a different set of issues.

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