The Economic Journal, **105** (*May*), 715–734 © Royal Economic Society 1995. Published by Blackwell Publishers, 108 Cowley Road, Oxford OX4 1JF, UK and 238 Main Street, Cambridge, MA 02142, USA.

REVIEW ARTICLE

WHY DOES METHODOLOGY MATTER FOR ECONOMICS?*

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In the context of reviewing some of the important recent literature on the methodology of economics, this article argues for the continuity of economics, economic methodology, and philosophy, against the proposition maintained by some so-called pragmatists and the rhetoric program that economics is sui generis and should be immune to criticism rooted in related disciplines. It is argued that since methodological considerations are unavoidable, effort should be directed to good methodology. The credentials of economics as a somewhat successful, empirical science are defended.

> In the same spirit, therefore, should each of our statements be *received*; for it is the mark of an educated man to look for precision in each class of things just as far as the nature of the subject admits; it is evidently equally foolish to accept probable reasoning from a mathematician and to demand from a rhetorician demonstrative proofs.
>
> Aristotle, *Nicomachean Ethics* (I, 2)

I. IRREPRESSIBLE QUESTIONS

The question 'why does methodology matter for economics?' presumes that methodology does in fact matter. The instinct of many economists would be to reject such a presumption. Recently many philosophers, historians and sociologists of science, as well as economic methodologists, have argued that different disciplines are autonomous with their own internal standards. This view buttresses the practising economists' unexamined instinct that the methodologist is an outsider who cannot add anything materially to the detailed practices of economics or, for that matter, of any other science. Four recent books, in different ways, implicitly and explicitly, reaffirm the importance of methodology or philosophy of science applied to economics against this post-modern current: the second edition of Mark Blaug's widely known *The Methodology of Economics: Or How Economists Explain*,[1] Daniel Hausman's *The Inexact and Separate Science of Economics*[2], Alexander Rosenberg's *Economics: Mathematical Politics or Science of Diminishing Returns*,[3] Lawrence A. Boland's *The Principles of Economics: Some Lies My Teachers Told Me*.[4]

* I am grateful to Roger Backhouse, Thomas Mayer, Steven Sheffrin and Nancy Wulwick for comments on an earlier draft.

[1] Cambridge: Cambridge University Press, 1992. ISBN 0 521 43678 8, £13.95 paperback, US$17.95 paperback.

[2] Cambridge: Cambridge University Press, 1992. ISBN 0 521 41501 2, 0 521 42523 9, £37.50, US$59.95 hardback, £13.95, US $16.95 paperback.

[3] Chicago and London: University of Chicago Press, 1992. ISBN 0 226 72723 8, £25.95, US$37.50 hardback.

[4] London and New York: Routledge, 1992. ISBN 0 415 06433, £35.00 hardback.

The purpose of this article is not to review in detail every aspect of these books, but to consider the connection between methodology and economics and to ask whether such books as these serve any purpose at all. Such a review is inevitably unbalanced. Each book bears on the question of the relevance of methodology, but their explicit aims differ considerably, and many aspects of their rich variety must be ignored.

Most economists recognise methodology or methodological argument when they see it, but few – even few professed methodologists – have a credible definition at their fingertips. A good place to start is Blaug's (p. xii) definition:[5] methodology is

> ... a study of the relationship between theoretical concepts and warranted conclusions about the real world; in particular, methodology is that branch of economics where we examine the ways in which economists justify their theories and the reasons they offer for preferring one theory over another; methodology is both a descriptive discipline – "this *is* what most economists do" – and a prescriptive one – "this is what economists *should* do to advance economics"...

The most common response of economists to methodology is to dismiss it as practically irrelevant. Frank Hahn (1992*a*) has, in a recent number of the *Royal Economic Society Newsletter*, forcibly restated this view, and, subsequently, set off a flurry of replies, letters and rejoinders (Backhouse, 1992; Hahn, 1992*b*; King, 1992; Lawson, 1992). Hahn's attitude towards methodology is an old one, however. Mervyn King's (1992) contribution to this exchange is to quote the opening paragraph of Irving Fisher's presidential address to the American Statistical Association:

> It has long seemed to me that students of the social sciences, especially sociology and economics, have spent too much time in discussing what they call methodology. I have usually felt that the man who essays to tell the rest of us how to solve knotty problems would be more convincing if first he proved out his alleged method by solving a few himself. Apparently those would-be authorities who are forever telling others how to get results do not get any important results themselves. [Fisher, 1932, p. 1].

[5] References to the four books under review are by name and page number without date. Blaug's book is a good place to start in the discussion of methodology in any case. Despite his own strongly held methodological (Popperian/Lakatosian) position, Blaug gives the single best overview of the history and range of methodological issues in economics. The fact that Blaug is primarily an economist is helpful in that the detailed practical application of methodological considerations to live issues in economics is stressed. Blaug's second edition is, as he himself notes in the new preface, an only slightly modified version of the first edition, first published in 1980. A number of topics are brought up to date, and a new chapter on the rationality postulate has been added. The new preface in which Blaug argues for his own methodological position against the fashions of the decade following the first edition is of as much interest as the substantial changes. Here he argues against Bruce Caldwell (1982) and other advocates of methodological pluralism in favour of an old-fashioned monism. He argues for the relevance of philosophical standards against McCloskey and the rhetoric project; and he forcibly restates the case for a prescriptive, Popperian empiricism, deploring the descent of economics into technical problem-solving at the expense of substantial knowledge. Many methodologists would violently disagree with Blaug; but it is impossible to read him and not be aware of where the fault lines are that divide methodologists. Of the other books under review, Hausman's book, especially the appendices, provides an excellent overview of the philosophical issues, while Rosenberg's and Boland's books are more narrowly focused on particular methodological issues.

Even most methodologists would recognise the kind of work that Hahn and Fisher animadvert. All the ink spilled in trying to determine whether economics is or is not a science has, for example, never advanced economics in any practical sense. (It is to his credit that, even though this demarcation issue has been a central one in Popper's methology, of which he is an articulate advocate, Blaug's main recommendations for economics are virtually independent of it.) In his book, Boland (pp. 3–5) accepts the Hahn/Fisher view inasmuch as he argues that the only successful criticisms of neoclassical economics are internal, setting up his argument in a manner as easily criticised by a practising microeconomist as by a methodologist. But Boland is clear that his commitment to internal criticism is a practical judgment; he does not suggest that only internal criticisms could matter as a point of principle. Even Rosenberg (p. 21), a philosopher, recognises the risk – especially to heterodox economists – of being drawn away from the substantial problems of economics itself by the siren song of philosophy.

Practical problems inexorably raise methodological issues nonetheless. At the trivial level, it is not unusual for economists to make logical mistakes, such as mistaking necessary and sufficient conditions. And methodology appears to be unavoidable in more profound contexts as well (cf. Lawson, 1992, 1994). A policy-maker might, for example, wish to know what caused the last recession or whether cutting the deficit will cause another one. Here clarifying the relevant sense of 'cause,' and determining what would count as evidence of causal connection – clearly methodological issues – are essential to answering the policy-maker's questions. On these issues, philosophers may well have as much useful to say as economists. Tests of Granger-causality, for example, are well established in econometrics, and they are technically well-understood. Any remaining technical problems pale by comparison to the methodological issues of whether and to what degree these tests relate to the various senses in which the policy-maker might be interested in causality.[6]

II. WHO SPEAKS TO ECONOMICS?

The belief that methodology is practically irrelevant is motivated by the same sort of sentiment expressed in the old saw, 'those who know do; those who don't know teach.' It would appear to be just as easily dismissed. An individual teacher may be incapable of advancing his subject, but teaching remains essential. A professional methodologist may not say anything of use to

[6] A referee suggests that the fact that Granger-causality deals only with predictability is well understood, and that the relationship of predictability to causality is a philosophical question beyond the specialised interest of economic methodology. The first point is, from my practical experience as a reader and a referee of macroeconometric papers, false: the misuse and misunderstanding of the status of Granger-causality remains rampant, despite the fact that the best informed economists understand the point. The second point reinforces my view: general questions of a philosophical nature, which might be investigated by economic methodologists, by philosophers, or by the practitioners of some other discipline, sometimes assume crucial importance in economic arguments.

practising economists, but methodological questions are irrepressible, and must somehow be answered. Just as the need for teaching, however, says nothing in itself about the organisation of instruction, the need for methodology says nothing about the need for methodologists. It may be that all the methodology that needs to be done can be done by those at the sharp end of the discipline, for whom the stakes are large and the problems palpable. No class of professional kibbitzers is wanted or even useful. The argument about the irrelevance of methodology has shifted and become socialised in that it no longer claims that the issues raised by methodologists are irrelevant, but rather that some people do not have the social standing to raise them: the question has become who is a kibbitzer and who is a player in economists' debates?

Donald McCloskey (1985, p. 139) puts this socialised objection to methodology in a similar spirit to the quotation from Irving Fisher: 'It would be arrogant to suppose that one knew better that thousands of intelligent and honest economic scholars what the proper form of argument was.' McCloskey's work, embodied in the 'rhetoric project,' has been well received by practising economists, because it supports their instinct to dismiss methodology. Blaug (esp. pp. xvii–xx), Hausman (esp. pp. 263–8), and Rosenberg (esp. ch. 2) each squarely and explicitly confronts McCloskey's challenge to methodology.

McCloskey denies the relevance of methodology as a point of principle. Since it is implausible to deny the importance of the sorts of thing that we argued earlier were irrepressible in economic practice, McCloskey is forced to distinguish between methodology in the small, and Grand Methodology. Grand Methodology is for McCloskey a product of 'modernism', which, in his usage, seems to refer to the philosophical school of logical positivism. It is now common to refer to many of the disparate schools of thought that reject logical positivism as 'post-modernist'.

Logical positivism was a doctrine originating in pre-war Vienna that held that scientific knowledge could be fully accounted for as either direct empirical observation or as deductions from a closed logical system. The grand vision of the logical positivists was to construct a purified language for expressing scientific claims that would permit a formal and algorithmic approach to science (see Carnap, 1928/1967). Truths that belonged to the language itself were called 'analytic', those that required reference to sensory experience were called 'synthetic'. All scientific 'sense', the logical positivists hoped, would be expressible in a refined language that would allow them to ground science completely in sensory experience. What could not be so expressed was literally 'nonsense'. 'Science' in this way could be demarcated from 'metaphysics'. Among some proponents, there was an implication in drawing this distinction that modern philosophers and scientists should follow David Hume's (1777/1902, p. 165) advice concerning metaphysics: 'Commit it then to the flames: for it can contain nothing but sophistry and illusion.'

The logical positivist vision of science is foundationalist: science is constructed on the bedrock of logic and sense experience. The vision of logical positivism began to unravel shortly after the Second World War. And, although many of the positivists survive and many of the same concerns for rigorous empirical science continue to animate them and their intellectual progeny, the ideal vision of a unified, formal science died long ago. What sets post modernists apart from those like Popper (1959, esp. chs. 1, 2, 5), who retain the spirit of logical positivism while denying foundationalism, is the view that knowledge is not as much a transaction with the world as it is a subjective, social construction.[7]

McCloskey is at pains to identify current economic methodology with logical positivism. But logical positivism is a straw man. Not only does logical positivism have few adherents among philosophers of science, but, as Rosenberg (pp. 24–5) points out, economics was less affected by logical positivism than any other social science. Logical positivism gave a big boost to behaviourism in psychology. The closest analogue in economics is the theory of revealed preference. But in this case the equivalence of revealed preference to ordinary ordinal utility analysis was quickly established (see Boland, p. 13). In any case, there are no economists or methodologists of note who are classical logical positivists.

Having failed to tar economics with the logical positivist brush, McCloskey's case against methodology remains the socialised one: methodologists are outsiders to economics, and outsiders cannot matter to the development of any discipline. But the socialised argument against methodology is barely coherent, and it no more rests on deep principle than did Irving Fisher's similar argument fifty years earlier. For McCloskey advocates rhetoric as a replacement for methodology. He argues that by attending to how economists actually argue and by studying persuasion in the small, the irrepressible methodological problems of economics can be adequately addressed *sans* methodologists or Grand Methodology. In pushing the case for rhetoric, however, McCloskey routinely appeals to poets, ancient authorities, and modern literary critics. With these appeals to outsiders to economics, it is puzzling why philosophers and, especially, economists of a philosophical bent should be deemed singularly unable to inform economic practice. What is more, the best explanation for the undeniable fact that *bona fide* economists sometimes appeal to philosophers (Samuelson's revealed preference theory, for example, was justified in part by an appeal to Percy Bridgman's methodology of operationalism) is that philosophical and methodological arguments in fact persuade economists.[8] This, in turn, might suggest that philosophical and methodological persuasion should be proper objects of the rhetorician's attention. Indeed, the literary critic Stanley Fish (1988) argues that precisely the same considerations that persuade McCloskey that methodology cannot matter to economics also imply that rhetoric cannot matter either: outsiders to any discipline cannot matter. One message of this essay is that Fish's view is incorrect. Nonetheless, given his adoption of the socialised objection to methodology, McCloskey is not well placed to controvert it.

[7] See, e.g. Weintraub (1990); also see Backhouse (1994), ch. 1, and Rosenberg (ch. 2).

[8] The list of those who contributed both to philosophy and economics and who found the subjects mutually enriching includes John Locke, David Hume, Adam Smith, John Stuart Mill, Henry Sidgwick, Karl Marx, William Stanley Jevons and Frank Ramsey.

McCloskey's arguments against methodologists misfire, because few methodologists are logical positivists and his attempt to place them beyond the Pale of economics are implausible. It is true that Blaug, Hausman and Rosenberg share the empirical orientation of the logical positivists, and Blaug and Rosenberg also share their aspiration for a unified science. Yet, all three recognise, nonetheless, that the logical positivist programme was not successful. Rosenberg (p. 2), for example, argues that McCloskey and other critics of methodology draw the wrong lesson from the collapse of logical positivism. They are correct that logical positivists cannot draw absolute distinctions between sense and nonsense, between science and metaphysics, and between the analytic and the synthetic, independent of context. One should not conclude says Rosenberg, that economics and other disciplines set their own standards; rather that the borders between disciplines are not firm and well-defined. He writes

> If theory adjudicates the rules of science, then so does philosophy. In the absence of demarcation, philosophy is just very general, very abstract science and has the same kind of prescriptive force for the practice of science as any scientific theory. Because of its generality and abstractness it will have less detailed bearing on day-to-day science than, say, prescriptions about the calibration of pH meters, but it must have the same kind of bearing. [Rosenberg, p. 11]

This is what we might call Rosenberg's 'continuity thesis'.

The continuity thesis supports a somewhat different definition of 'methodology' from the working one that we adopted from Blaug. A methodology for Rosenberg is a set of rules constructed in relation to a particular theory. At the most workaday level, the methodology governing the use of a pH-meter for measuring ion concentration is dictated by a maintained theory of acids and bases, as well as by secondary theories of chemical and electrical functioning that support the belief that what the meter shows is truly connected to the theoretical entity intended to be measured. Rosenberg (p. 10) represents a methodology by its rules (R) as a function of a theory (t) as $R(t)$. The application of these rules may provide grounds for reconsidering a theory. Thus, for Rosenberg, methodologies may develop as theories themselves develop. If t' replaces t, $R(t') \neq R(t)$.

Blaug also maintains that methodology evolves, which explains in part his attraction to Lakatos's methodology of scientific research programs with its stress on progress and degeneration. Blaug's definition of methodology cited earlier is different from Rosenberg's. For Blaug, methodology is the study of the relationship of rules to theory, where for Rosenberg it is the rules that the theory generates. On Blaug's view of methodology, Rosenberg's book is an instance of methodology; on Rosenberg's view, it is in large part a metamethodological discussion.[9] This difference has an ironic twist; for Rosenberg advocates a view in which the fine details matter most, where Blaug's view at least permits a

[9] This is not, however, entirely so; Rosenberg's discussion of economics and psychology (ch. 5) aims to restrict the aspirations of microfoundations.

more detached discussion; yet it is Blaug who discusses economic theories in detail, while Rosenberg's discussion is carried on at a much higher level of abstraction. The explanation, of course, is that Blaug is the practising economist and Rosenberg the philosopher. Despite the fact that Rosenberg's book does not concern itself with those fine details of economic practice that his own understanding of methodology singles out as most important, the continuity thesis saves his claim to relevance, because some of the irrepressible problems of methodology are only answerable at the higher level of abstraction at which he typically writes.[10]

The way in which Rosenberg's continuity thesis answers the post-modernist's socialised objection without appealing to logical positivism can be seen more clearly against the background of Willard V. O. Quine's essay 'Two Dogmas of Empiricism'. The publication of this essay in 1951 was one of the key events in the collapse of logical positivism. In it, Quine denied the general validity of the distinction between analytic and synthetic statements upon which the logical positivists had partly based their distinction between science and non-science. Quine also denied that statements could be judged true or false purely on the basis of sense experience. Theoretical statements in Quine's view were part of a vast web of interlocking and mutually supporting beliefs. Any one of these beliefs could be maintained in the face of any recalcitrant experience provided ample enough adjustments were made elsewhere in the web of beliefs. Quine's view that one can hold to any belief in the face of recalcitrant experience was anticipated by the French physicist and philosopher Pierre Duhem, and is well-known as the Quine/Duhem thesis. The problem posed by the Quine/Duhem thesis and the metaphor of the web of belief stands behind much of the recent methodological debate.

All four of the authors under review understand the need to address the problem posed by the Quine/Duhem thesis. Blaug (p. 18) notes Popper's recognition of the problem and endorses his solution: avoid immunising strategies in the face of recalcitrant experience. For example, suppose theory says demand curves slope down; but empirically the demand for potatoes in Ireland slopes up. An immunising strategy would be to say that the Irish are uniquely irrational, preferring expensive goods to less expensive goods. A non-immunising strategy might be to argue, given Irish poverty and the central role

[10] It will not escape the reader that the irony extends to this review as well; for in advocating the cogency of Rosenberg's continuity thesis, it nevertheless practises metamethodology, not methodology. It does not attempt to make the methodological arguments that should change the practice of economists. This cannot, I think, be helped in the present context. In other contexts, however, I have attempted to apply methodological lessons to genuine problems in empirical economics (see e.g. Hoover, 1991; Hoover and Perez, 1994*a*, *b*; Hoover and Sheffrin, 1992). This ironic twist also explains why Boland's book is dealt with only episodically throughout this review. Of the four books under review, it is the one that takes the call to engage practising economists on their own ground most to heart. The essence of Boland's book is the conviction that detailed consideration of the structure and underlying presuppositions of neoclassical microeconomics can improve the practice of microeconomics. Boland considers such issues as the roles of maximisation and of time, the conceptions of equilibrium and disequilibrium, and assumptions about knowledge in shaping microeconomic theory. Boland thus is the embodiment of the continuity thesis. That, however, implies that a comprehensive treatment of his argument in its own terms would carry us away from the metamethodological theme of this essay, into microeconomic theory. This, of course, needs to be done; but not here.

of the potato in the Irish diet, that income effects overwhelm substitution effects, so that potatoes are a Giffen good. An immunising strategy is again one of those things that we recognise, in egregious instances, but would be hard pressed to define consistently with some operational principle.

Both Boland (p. 20, fn. 12) and Hausman (pp. 187, 188, 200) recognise that not everything can be tested at once, that temporarily at least some beliefs are fixed in the face of all experience, and the recalcitrance of experience for other beliefs is defined against this fixed background. Boland (pp. 17, 18) refers to these beliefs as metaphysics, and shrewdly observes that 'Every model or theory is merely another attempted test of the "robustness" of a given metaphysics'. 'Metaphysics' for the logical positivists had been a term of abuse, a synonym for 'nonsense' or 'moonshine'; for Boland it is simply the name of the least questionable assumptions of a research program, and is therefore a matter of practical importance.

For McCloskey and other post-modernists, the collapse of logical positivism meant that economists need not heed philosophers or methodologists. Henceforth, only economists would judge economics. Post-modernists raise the social standing of economists by casting the philosophers and methodologists into the outer darkness. In terms of the web metaphor, each discipline is a web spun by a different spider. For Rosenberg, the collapse of logical positivism means that questions of social standing no longer make sense: there is only one web.

Rosenberg's goal is to understand the cognitive status of economics, to decide what sort of discipline it is in relation to other disciplines, and to determine how one should appraise its success. There is a tension between this goal and the continuity thesis; for, on the one hand, Rosenberg needs to parse knowledge into different disciplines, if those disciplines are to have different cognitive statuses; and, on the other hand, he asserts the unity of knowledge. The tension is, however, inevitable. Quine's metaphor of the web with experience impinging on the periphery requires that the web have structure, that there be an outside and inside. Various disciplines may correspond to different parts of the structure – related to each other in various ways, some closer, some further from the periphery. The continuity thesis at once asserts the relevance of every part for every other part and denies foundationalism: there is only one web, and there is nothing besides the web. The post-modernists are correct that no one has standing to judge economics from the outside, but only because there is, in the end, no outside.

III. ECONOMICS IS INEXACT, BUT IS IT AN EMPIRICAL SCIENCE?

The cognitive status of economics is important for Rosenberg partly because he wants to know how economics should be appraised. This raises a question: what purpose does the appraisal of economics serve? Rosenberg's answer is exactly right: appraisal serves policy. Economics is above all an advice-giving discipline – even if that advice, as it so often is in classical and neoclassical economics, is to do nothing. Economists profess to understand better than other people how the economy works, and therefore how policy actions might affect it. Theories need to be judged on their ability to support such advice.

Before we can appraise economics however, it would be helpful to know what economics is as a substantial discipline. The question is especially apt, because under Rosenberg's definition (see Section II above), the substance of economics (i.e. theory) determines methodology and, one presumes, the standards of appraisal.

So, what is economics? The classic definitions can be summarized in a word: *plutology*, the science of wealth. John Stuart Mill (1848/1911, p. 1) writes:

> Writers on Political Economy profess to teach, or to investigate, the nature of Wealth, and the laws of its production and distribution: including, directly or remotely, the operation of all the causes which the condition of mankind, or of any society of human beings, in respect to this universal object of human desire, is made prosperous or the reverse.

Alfred Marshall (1920, p. 1) writes:

> Political Economy or Economics is a study of mankind in the ordinary business of life; it examines that part of the individual and social action which is most closely connected with the attainment and with the use of the material requisites of the wellbeing.
>
> Thus it is on the one side a study of wealth; and on the other, and more important side, a part of the study of man.

Modern economists almost all follow the much different definition of Lionel Robbins (1935, p. 16):

> Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses.

Economics, in Robbins's view, is the science of choice. Economics is, in modern terminology, microeconomics.

Of the four authors under review, all but Blaug treat economics as if its essence were microeconomics. For Hausman and for Boland this may simply follow from the desire to keep their projects within workable if somewhat arbitrarily narrow bounds. Nonetheless, Hausman (pp. 1, 30) like Rosenberg (pp. 129 ff.) gives reasons for attending to microeconomics almost exclusively – suggesting that principle as well as convenience is involved. Macroeconomics in their view must be parasitic on microeconomics, since the economy is clearly made up of individual economic agents. Empirical aggregate relationships – market demand curves, as well as macroeconomic curves – are atheoretical and not fundamental. And, of course, they are right that microeconomic theory is a central characteristic of economics as it is practised. Economics is distinguished from other social sciences, as Nancy Cartwright (1989, p. 14) observes, because it is 'a discipline with a theory'. For Hausman (p. 55) and Boland (p. 22) the core of economics is equilibrium theory, that is the theory of the successful optimisation of economic agents subject to constraints. For Rosenberg (ch. 7 *passim*) the core is general equilibrium theory, the theory of the coordination of such optimizing agents (cf. Weintraub, 1985). Although exceedingly narrow in contrast to the notion of economics as plutology, such Robbinsian notions of economics are central to the modern conception of economics.

But there is something else; economists are typically, at least in their asseverations, empiricists – in fact, Popperians, asking for testable hypotheses and acknowledging the preeminent importance of falsifications. All four authors under review support, in their different ways, the spirit of empiricism. Robbins, the apriorist, and Popper, the empiricist, are strange intellectual bedfellows.[11] Many of the interpretive problems and methodological difficulties in economics arise from the tensions of this dual allegiance.

All four authors under review confront this tension. Blaug embraces the Popperian pole. He describes himself as a Popperian, and he interprets Lakatos as an extension of Popper rather than as an alternative. He offers a free interpretation of both Popper and Lakatos, displaying little interest in the troubling details of their analyses. He cares relatively little for Popper's demarcation criteria or measures of verisimilitude or for Lakatos's hard cores or positive and negative heuristics of a research program. Instead, Blaug's Popper and Lakatos are mainly icons of an uncompromisingly tough empiricism, one that requires that theories be stated forthrightly, subjected to severe trial, and that the evidence against them not be shirked. Blaug recognises the lip-service that economists pay to Popperian methodology, but bemoans their practice of 'innocuous falsification' – the apparent testing of theory that in fact places the theory at no real risk.

In Hausman's view, Blaug's concerns are misplaced. Innocuous falsification is all that one can hope for. Hausman revives John Stuart Mill's idea that economics is a 'separate and inexact science'. For Mill, economics is plutology; its mode of explanation is to understand the implications of the preference of more wealth to less. While Mill's definition of economics is unlike Robbins's his methodological position is closely related. Thus, in contrast to Blaug, Hausman, in embracing Mill, appears to embrace the Robbinsian pole of the methodological tension. Economics is separate because, conditional on its initial assumptions, it generates a complete set of distinct laws. While the explanation of those assumptions is not part of economics itself, they are nevertheless not based on psychology or sociology. On this view economics is the study of man as a utility maximiser. Economics is inexact partly because it is separate. Because psychology, sociology and other approaches retain some purchase on the explanation of human behaviour, economics is incomplete; only the most important causes in the economic domain are known and understood. Causal accounts must then be at most accounts of the tendency of events in fact to conform to economic laws. Theories are necessarily hedged with *ceteris paribus* clauses. And these *ceteris paribus* clauses cannot be easily released. Theories cannot be falsified on this view; for any recalcitrant experience can be attributed to the failure of *ceteris paribus*. Empirical

[11] They were colleagues at the London School of Economics, however.

investigation should nonetheless be pursued. A theory that failed to account for the main empirical facts would not be falsified, because the blame could be laid on the doorstep of extratheoretical assumptions; but it would nonetheless be of little practical use.

Hausman adapts Mill's notion of a separate and inexact science to neoclassical economics letting modern choice theory (Robbins's adaptation of scarce means to independent ends) take the place of wealth as the core of the discipline. The domain of economics is now wider in scope, for the calculus of choice can be applied – as it is, for example, in the works of Gary Becker – to a wider class of phenomena than getting and spending. And its theoretical assumptions – the axioms of the theory of choice – are susceptible to more detailed theoretical elaboration. Still, for Hausman, Mill's essential point remains: economic theory cannot yield tight predictions of the behaviour of individuals.

Although Hausman accepts the practical implications of the method *a priori* that lies behind Mill's account of economics as a separate and inexact science, he does not fully embrace that method nor does he believe that economists generally embrace it. He still feels the tug of the Popperian pole of the methodological tension in economics. Hausman contrasts Mill's method *a priori* with what he calls the 'economist's deductive method'. Where the method *a priori* begins its deductions with 'proven (*ceteris paribus*) laws concerning the operation of relevant causal factors', the deductive method is content to begin with 'credible (*ceteris paribus*) and pragmatically useful generalisations concerning the operation of relevant causal factors' (Hausman, p. 222).

When the method *a priori* faces recalcitrant experience, it looks, first, for deductive errors, then for interfering factors (violations of *ceteris paribus*) and, finally, for the omission or inclusion of inappropriate causal factors. The initial assumptions are not themselves at stake. In the face of recalcitrant experience, the deductive method chooses among alternative accounts of the failure on the basis of explanatory success, empirical progress, and pragmatic usefulness' (Hausman, p. 222).

It would be easy to stigmatise Hausman's defence of the economists' deductive method as wet in contrast to Blaug's tough-minded falsificationism – as a recipe for innocuous falsificationism. But Hausman is adamant. He advocates retention of some of Popper's and Lakatos's slogans (p. 202), and he deplores dogmatism in the face of empirical evidence against one's theory (ch. 13): he is an empiricist. Nevertheless, he maintains that the Quine/Duhem thesis makes strict falsificationism impossible in principle and in practice He writes of the economist's deductive method:

> Economists are committed to equilibrium theory because they regard its basic laws as credible and as possessing heuristic and pragmatic virtues. Their response to anomalous market data, which mimics the inexact method *a priori*, is not illegitimately dogmatic. It is, on the contrary, fully consistent with standard views of theory assessment, once one takes account of how bad these data are. The problem is not a moral failing among economists – their inability to live up to their Popperian

> convictions – but a reflection of how hard it is to learn about complex phenomena if one does not know a great deal already and cannot do controlled experiments. [Hausman, p. 226]

Given Blaug's loose interpretation, in which any serious empiricist is likely to qualify as a follower of Popper and Lakatos, and given Hausman's stand against dogmatism in the face of empirical evidence, less appears to separate Blaug and Hausman than meets the eye. But Blaug clearly protests the status quo in economics, while Hausman, to a large degree, defends it. Where Hausman is attracted to the Robbinsian pole and Blaug to the Popperian pole, Rosenberg explores the tension between them directly. This tension is encapsulated in the two principal views of economics that Rosenberg investigates: is economics an empirical discipline widely interpreted? or is it a kind of applied mathematics – in particular, applied mathematical politics?

IV. DOES ECONOMICS IMPROVE?

In considering economics as an empirical discipline, Rosenberg agrees with Hausman that, because of its irreducible *ceteris paribus* clauses, it is inexact. But is it a science? Rosenberg (p. 18; cf. pp. 56, 238) asserts that any science should show a long-term pattern of improved predictive, explanatory, and technological success. The ratio of its correct to incorrect predictions should rise over time, and the precision of those predictions should become higher. Rosenberg (p. 112) states baldly that, unlike other inexact disciplines, economics is as inexact as ever. He doubts therefore, that economics is a separate science in Mill's or Hausman's sense, because the empirical failure of economics suggests that the major causes of economic phenomena cannot be identified and isolated. Indeed, Rosenberg doubts that economics is an empirical science at all.

Consider three responses to Rosenberg's analysis. First, is it even true that economics does not show a pattern of rising empirical success? Rosenberg does not attempt to document his assertion. Rosenberg compares economics unfavourably with meteorology, which has become a better predictor in our lifetimes. He attributes the success of meteorology to improved data providing better initial conditions for the predictive theoretical models. In any case, against what baseline should we judge improvements? Both economic data and empirical predictions are better now than in Adam Smith's day, better now than at the turn of the century, and, indeed, better now than in 1950. National income accounts and large-scale macroeconomic models were developed in the main after the Second World War. Whatever their faults, and they are many, they represent improvements in measurement and forecasting compared to the pre-war period.[12]

Similarly, the quantitative evaluation of demand, price conditions, risk and return and other aspects of the economy important to businesses is much

[12] Hutchison (1994, esp pp. 30–2) makes a similar argument in response to Rosenberg.

improved over the post-war years. It is not for nothing that large corporations maintain staffs of economists, and corporations at all levels hire legions of economic consultants.

Some areas of corporate finance would not exist but for the advances made in economic theory and in the collection of data. For example, before the path-breaking work of Fischer Black and Myron Scholes, the options market was thin because offering firms did not know how to price them correctly – i.e. they could not predict the behaviour of the market profitably – now they can.[13]

Furthermore, economics exhibits technological improvements. Linear programming, originally formulated in the context of economic analysis, has become a tool in a new discipline – operations research – that, in many ways, might be thought of as 'economic engineering'. Economics parallels philosophy as a nursemaid to areas of research that mature into independent disciplines (e.g. game theory) or move on into practical applications (e.g. corporate finance). The focus on academic microeconomics diverts attention from the considerable success of economics in the quotidian worlds of government and business. Blaug (p. 237, fn. 1) observes that government economists are far more cheerful about the state of economics than are academic economists or methodologists.

Is economics any worse as an empirically improving science than meteorology? I am not a meteorologist, but my impression is that the recent improvements in meteorology are largely the result of satellites permitting the direct observation of weather systems. Weather systems show enough local stability and recurrent patterns that extrapolations based on a fairly crude understanding of atmospheric physics provide reasonably good weather forecasts. Such forecasts are not direct empirical tests of deep underlying physical models. Indeed, such models do rather poorly. Deep models that are used to predict global warming, for example, cannot accurately match the historical climate record. It is not clear that economics is worse off.

Where Hausman (pp. 253–4) calls for more and better economic data, Rosenberg (p. 112) believes that improvements in relevantly better data are impossible. His argument suggests a second response to his belief that economics is an empirical failure. Rosenberg is misled into underplaying the empirical success of economics by an identification of economics with equilibrium microeconomic theory. With the exception of experimental economics, most empirical economics is aggregate economics.[14] Studies at the level of the single economic actor are found in, for example, case studies of firms; but these are the province of business schools, not economics

[13] Prediction has many senses as Rosenberg (pp. 44 ff.) indicates. Options theory is derivative of efficient markets theory. If the efficient markets theory is correct, then it predicts that there will be no unexploited arbitrage opportunities. That prediction itself rules out profitable predictions of specific price movements.

[14] This is not to deny the importance of microeconometrics. But most microeconometrics is conducted with data that are aggregated to some degree. And even in the case of data in cross-sectional or panel-data studies in which the level of observation is truly individual, the desideratum of the study is almost always a *market* relationship: e.g. in a study using panel data we are typically concerned with the average demand for food or health services and not with John Jones's demand for apples or appendectomies.

departments. The applied microeconomist is concerned about the behaviour of individual markets consisting of many actors or the average or typical behaviour of many actors: e.g., the market for wheat or the female-labour supply elasticity. Macroeconomics is even more explicitly about economy-wide aggregates. Rosenberg (pp. 129–30) joins the refrain of many economists in stating, as if it were an obvious fact, that macroeconomics stands in need of microfoundations, because the economy is composed of individual economic actors. While it is true that the behaviour of the individual ultimately determines the behaviour of the macroeconomy, it is not any more obvious that a reduction of macroeconomics to microeconomics is essential than is a reduction of hydrology to the micro-behaviour of water molecules. Relevant underlying micro theories and facts may inform both hydrology and macroeconomics, but the objects of analysis remain at the macro level, and there may be no systematic reduction of that macro level to the level of the molecules or agents.

Rosenberg (p. 132) stigmatises aggregate economics without microeconomic theory as mere curve-fitting. Theoretical understanding can help to transform curve fitting from an atheoretical, arbitrary enterprise into a useful source of empirical knowledge without the curves that are fit being tight deductive consequences of the theory. Rosenberg (pp. 159–60) wonders whether the view that microeconomics is really about aggregates and not individuals does not reduce microeconomic theory to a mere *façon de parler* for downward-sloping demand curves, adding that the Slutsky equation does after all seem to be part of a real explanation. As if in answer, Blaug (p. 145) notices that microeconomic theory, because of the Slutsky equation, does not in fact predict downward-sloping demand curves at all. Which way demand curves slope depends upon the income elasticity, which is an empirical fact, not a theoretical presumption. The power of the theory is *pace* Rosenberg precisely to systematise economic behaviour, to make a curve-fitting exercise relevant to economic understanding.

Economics is really no different from other sciences in this regard. Empirical research in horticulture, medicine, engineering and many other disciplines consists in determining the factual properties of the subjects of study in a manner that does not provide a direct test of underlying theory, because these properties are not directly deducible from the relevant theory. One might assume that they would be deducible *ceteris paribus*; but this is simply faith; for just as in economics, no one knows how to release the *ceteris paribus* clauses. If a horticulturalist, for example, wants to know the effect of a fertiliser on plant growth, he may have some theoretical understanding of the underlying chemistry that may help in the design of his experiments; but, in the final analysis, he must simply do the experiment. Physics is often held up as the model science; but, as Nancy Cartwright (1989, p. 8) points out, the laws of physics are 'hard to find... in nature and we are always having to make excuses for them: why they have exceptions – big or little; why they only work for models in the head; why it takes an engineer with a special knowledge of materials and a not too literal mind to apply [them] to reality'. Rosenberg is, of course, quite right: economics has made almost no progress to date in improving the predictability of the individual economic agent. A richer conception of what economics is might have spared him the trouble of looking for rising empirical precision in the quarter where it is least likely to be found.

Finally, why should rising empirical precision be a standard of the cognitive status of any science? It may be the nature of the economy is such that only a limited degree of precision is possible. Suppose that there were no further advances in meteorology. The knowledge already attained would still be knowledge and would still be useful – tomorrow or in a century – as it is today in understanding and predicting the weather. Would we wish to stigmatise that knowledge as unscientific? Rosenberg's demand for rising empirical precision appears to be Lakatosian in spirit. But one should recall that Lakatos thought in terms of competing research programs. He stigmatised a program as degenerating only when it became illicitly defensive with respect to a more successful program. The scientific status of neoclassical microeconomics might be called into question if there were a progressive alternative economics. But what is it? Thinking only of core microeconomic theory, Rosenberg (p. 252) calls for a successor discipline to guide policy. To want such a discipline is not to have it. And, even if we had such a discipline, it would have to show a superior power to neoclassical microeconomics, including those elements of economics far from the core, before it could be judged a plausible competitor.

Rosenberg (ch. 5) himself makes a powerful case that the precision of individual microeconomics is constrained – not just in practice, but in principle as well. Economic variables have an intentional character – that is, they involve states that are constituted by propositions expressing beliefs, desires, preferences, attitudes and so forth. Because of their intentional character, Rosenberg (p. 129) argues that economic 'theory's prediction and explanation of the choices of individuals [cannot] exceed the precision and accuracy of commonsense explanations and predictions with which we have all been familiar since prehistory'. While this seems exactly right, Rosenberg (p. 129) draws an odd conclusion from it: 'It seems improbable that we can improve on the accuracy of claims about the aggregation of human choices without improvements in our accuracy about individual choice'. He then claims that the history of science supports the 'rational expectationist' view of the necessity of microfoundations for macro-phenomena.[15]

The claim of limited precision at the individual level and the claim of the necessity of microfoundations of macroeconomics are separate. And, except for the analogy with other sciences, there is no argument for microfoundations. If both claims can be sustained, however, they constitute a powerful argument against macroeconomics: advancing precision in macroeconomics requires improved microfoundations; improved microfoundations cannot be had in principle; therefore macroeconomics cannot advance.

But is the microfoundational claim universally true? On the contrary, to take an example from physics, it was the increasingly accurate account of the actual (aggregate or macro) behaviour of gases – the ideal gas law and deviations

[15] For reasons made clear in Hoover (1988, 1992*a*, *b*) I prefer the term 'new classical macroeconomics' for those economists Rosenberg calls 'rational expectationists'.

from it – that supported the advancement and refinement of the microphysical theory of molecular behaviour, not the other way round.

Examples of analogous social phenomena are well known. For precisely the reasons Rosenberg cites, it is difficult to predict individual actions and choices. The electricity supplier could not say when Mary Smith will switch on her oven, but it may know pretty precisely how many total kilowatts it must supply at a given time, based on an aggregate analysis of past behaviour. Similarly, insurance companies could not function without aggregate predictability that far exceeded individual predictability. That is not to say that better understanding of individual behaviour cannot be helpful. Insurance companies know that whether an individual is, say, a smoker or obese matters probabilistically to their chances of dying. But the company would go broke trying to predict individual's precise dates of death. Instead, they use this information to improve the classification systems for their policy rating, relying on aggregation to lend precision to their predictions, now restricted to the aggregate level.

Rosenberg should have drawn a different conclusion: the intentional character of economic behaviour, because it limits the precision of prediction and explanation at the individual level, demonstrates the microfoundations of macroeconomics – construed as a direct reduction of aggregate to individual behaviour – is impossible; macroeconomics is autonomous and must seek improvements through its own development. Contrary to Rosenberg, the new classical school (Rosenberg's 'rational expectationists') implicitly recognises the autonomy of macroeconomics. Lucas, Sargent and company call for microfoundations, and employ the machinery of microeconomics (constrained optimisation of utility and profit functions), yet they routinely model only a single agent (or at most a small number of agents), who takes aggregate constraints to be his own. This is not microfoundations; the optimisation problems solved in these models are but the simulacrum of microeconomics; but Rosenberg's intentionality argument suggests that this is the best that a direct reduction can be expected to achieve.

This alternative conclusion also helps to clarify a point that puzzles Rosenberg. He argues that economics needs to strengthen the realism of its description of individual choice, but has, in fact, weakened it to the point of vacuity (p. 117; cf. Hausman, pp. 243–4). Added realism would make sense if the desideratum were individual prediction and explanation. But, it is not; it is aggregated market or macroeconomic behaviour. If, as I want to conclude, Rosenberg's own argument rules our direct reduction of aggregate to individual behaviour, then the microeconomics of the individual is relevant only to the extent that an individual is representative of large numbers of other individuals. Increasing realism – at least on some dimensions – undermines the representativeness of the individual by increasing his particularity. Some of the most powerful and precise aggregative theories in economics gain their power and precision just because they depend only on very weak (but not vacuous) individual foundations. The theory of finance makes strong predictions. For example, the efficient markets hypothesis states that changes in the price of an asset are unpredictable from publicly available knowledge.[16] The theory, which despite a large literature on anomalies has proved nearly impossible to refute empirically, relies only on the assumption of arbitrage, that people typically exploit profitable opportunities, without any further realistic specification of their characteristics or choices. Its predictions are aggregative. The theory is consistent with – indeed requires – the existence of trading profits for some individuals, but cannot make any accurate prediction about the success of particular individuals. New classical macroeconomic models have not enjoyed the same empirical success as the theory of finance, because it attempts a direct reduction of macroeconomics to microeconomics. New classical models posit a representative agent and attempt to use his particular characteristics to deduce aggregate results. Because his characteristics are unrealistic for any one agent, and unrepresentative of the wide variety of real agents, the deductive consequences have little bearing on reality.

V. IS ECONOMICS REALLY NOT EMPIRICAL AFTER ALL?

Having convinced himself that aggregate economics requires microfoundations to progress and that the precision of microeconomics is limited in principle, Rosenberg sinks into a sort of empirical despair. Repelled by the apparent failure of the Popperian pole, Rosenberg is drawn to the Robbinsian pole for the lack of any apparent alternative. He explores the possibility that economics is really mathematical political philosophy (Rosenberg chs. 7, 8). That economics might be applied mathematics is a reasonable inference supported by observing economic theorists and the amount of intellectual effort that has gone into the elaboration of axiomatic theories distant from empirical application. That mathematical economics might be a branch of political philosophy stems from the observation that so much of it appears to be the drawing of blueprints for ideal political arrangements – the mathematisation of social contract theory. That such activities are widely observed is the result of the basic distinction between the positive and the normative. Economics has always been both a descriptive discipline and an advice-giving discipline. More than that, its characteristic mode of explanation blurs this distinction: people are positively assumed to be behave economically as they normatively should. This blurring can be misleading with respect to the true function of microeconomics within the discipline. Microeconomics services other areas of economics, providing them with classification systems and schematic accounts of typical individual behaviour. These other areas stand on the empirical periphery, while individual microeconomics stands far to the interior and bears only indirectly on empirical reality. It is only the commitment of the profession and of critics to the Popper/Robbins marriage from hell that suggests the direct test of microeconomics at the individual level that Rosenberg calls for.

If economics is politics, it is of an oddly neutral kind. The normative elements are not placed in the service of any particular political philosophy. Economists intend their claims to have universal appeal. The statement of the

[16] N.B: again, two senses of prediction relevant here (cf. fn. 13).

head of the Russian central bank, Viktor Gersashchenko, '[i]t is impossible to apply economic theory to Russia' (Rossett, 1993) rightly strikes most economists of widely differing political persuasions as absurd: whatever theory of the relationship of central bank behaviour to hyperinflation that they subscribe to, they think it applies as surely to Russia as to the United States, Germany, or Papua New Guinea. Even Marx, who would place his economic analysis at the service of a particular political philosophy, does not see the truth of his analysis as depending on the success of that philosophy.

Boland (p. 18) sees the role of individual microeconomics clearly when he calls it 'metaphysics'. Microeconomics is not refutable in practice, because it is the underlying presupposition of empirically relevant economics. But to call it metaphysics and irrefutable is simply to recognise it as a *temporarily* fixed point, and not to claim that it is immutable. Game theory or psychology might conceivably replace constrained optimisation as the core description of individual behaviour in economics. That they have not yet to done so testifies to the strength of microeconomics. This strength derives from its universalisability (see Boland, pp. 93–4). Both psychology and game theory demand realistic detail even to begin to make definite predictions. Either founder on Rosenberg's intentionality argument at least as surely as does standard microeconomics. Microeconomics works sometimes because of its very weak assumptions, as with the arbitrage arguments in finance. Sometimes it works because ideal cases can set a lower bound on economic behaviour. This helps explain the appeal of existence proofs. For example, many people today still believe that a market populated by greedy people could not possibly achieve a coordinated outcome. The economic theorist then postulates the limiting case of perfectly self-interested agents, shows that there exists an equilibrium in principle, so *a fortiori* real people could coordinate in principle. The existence proof is a way of obtaining theoretical reassurance (Hausman, p. 101). This happens in other fields as well. A series of articles by Werner von Braun and Fred Whipple in *Colliers* magazine (Braun, 1952*a*, *b*; Whipple and Braun, 1952) in the early 1950s mapped out a blueprint for space travel to the Moon and Mars. The point of the articles was to establish the feasibility in principle of space travel, to provide reassurance that it would be worth the effort to attempt it in reality.

Even in the course of the limited discussion of economics as mathematical politics, I have pointed to a wide variety of activities that are collectively known as economics: e.g. giving policy advice, developing and testing theories of human behaviour in aggregate, providing theoretical reassurance, formalising political philosophy. Economics is many things. Even if many of the concerns of methodologists unsettle academic economists, many economists work for government or business in practical environments, and are on the whole more complacent about the state of economics and its claims to utility than are their academic counterparts. Rosenberg's concentration on the question of its cognitive status itself seems implausibly narrow. Why should the rich variety of activities called 'economics' each have the same cognitive status? Progress in general equilibrium theory or empirical industrial organisation or applied macroeconomics or development economics should be judged relative to intentions and goals of the practitioners of those subdisciplines. This is not relativism nor is it the assertion of the independence of research programs or interpretive communities. Rather it is the more mundane observation that a good hammer would be a bad wrench, that success as a landscape painter differs from success as a housepainter, that textbook theories of thermodynamics are of little help to plumbers in designing heating systems. In the case of economics, it is the legacy of Robbins – economics *is* choice subject to constraints – that misleads us. The richness of economics would be clearer, if we would recognise in principle, what we indeed recognise in practice, that economics must be broadly defined, that Mill's and Marshall's definitions of economics as plutology are much closer to the mark than Robbins's definition.

VI. METHODOLOGY MATTERS

So, we have come full circle. The reason methodology and philosophy matter to economics is simply that Rosenberg's continuity thesis is correct. Some arguments are closer to the ground, as it were, more obviously economic; but there is no boundary between these and higher-level arguments made by methodologists and philosophers. Higher-level arguments do not always appeal to practising economists. Sometimes this is because the methodologists or philosophers do not appreciate what really matters to the economist, so the arguments fail to connect. Sometimes this is because the economists refuse to trace out the general consequences of their subject. Sometimes it is because the economists and the philosophers do not understand each other's language. The four books under review go a long way to bridging the gap. Each raises deep and genuine issues that economists must confront in their own work whether or not they recognise that confrontation as methodology, philosophy, or something else.

Methodology is indeed unavoidable. What Keynes (1936, p. 383) thought about the relationship between economists and politicians is *mutatis mutandis* also true of the relationship between economists and methodologists: Practical economists, who believe themselves to be quite exempt from any methodological influences, are usually slaves of some defunct methodologist.

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Date of receipt of final typescript: September 1994

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